

## Let's Try and Fix the Current Publishing System Before Making Dramatic Changes

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Brian A. Nosek and Yoav Bar-Anan (this issue) have produced a forward-looking treatise on what the future of scientific publishing in psychology might look like if some fairly dramatic changes were made to the current system. When I first heard Brian make some of these suggestions a year or so ago in a talk, many of the ideas struck me as quite radical, problematic, and/or impractical. However, on reexposure to them in this extended article, they seem less wild and crazy. Indeed, some are even starting to seem normal and inevitable. In agreement with the authors, I don't know anyone who is completely happy with the current publishing system we have. Some of this has to do with structural issues such as those Nosek and Bar-Anan raise, but much of it has to do with the specific behavior of individual editors and reviewers. Some of the structural changes they suggest (e.g., eliminating anonymous reviews) could have a positive effect on the behavior of the major players in the system, but some ideas (e.g., making producers of research rather than consumers of science bear more of the cost) seem irrelevant to current dissatisfaction and might even have a negative effect. In this brief article I comment on some of the suggestions Nosek and Bar-Anan made that I reacted to most strongly.

The article begins with an illustrative case study of Nosek and colleagues' publication history. This is interesting and provides idiosyncratic data that social psychologists may enjoy comparing with their own experiences. Is my success rate higher? Does it take my papers longer to be published? But it is critical for everyone to remember that although there are many articles in Nosek's analysis, the appropriate  $N$  for the case study is 1. Thus, we can't know how representative Nosek's experience is for the field of social psychology as a whole. He works within a particular area and therefore will tend to get a particular set of decision editors and reviewers. Are editors and reviewers in other areas similar in their practices? We can't know. It helps that Nosek and his collaborators attempted to publish in a variety of different journals over a period that would encompass more than one editorial board for each journal, but given the number of scientists and reviewers in the field, one has to extrapolate from the data presented with caution.

The bulk of the article deals with changes recommended to the entire publishing system. Before getting to these, Nosek and Bar-Anan note several problems with the current system, which I have boiled down to three. The first is that there often is either no or rather slow communication of research results. Indeed, a lot of data are never published, and data that are can take a long time to reach the intended audience. This is certainly true if one focuses only on the traditional journal publication process. But research results are disseminated in many ways. For example, one can wonder how many of Nosek's papers cited in his Table 1 were presented at professional conferences or meetings years before the final journal publication. I can't know if my own experience is similar to others, but virtually every published paper that I and my collaborators have done has been preceded by a presentation at a conference in some form and well before the journal article appeared. Thus, if societies would make searchable online archives available of these presentations, it would help to solve this problem. Further, it would set a minimal bar to placing material in the professional database. Researchers would have to think their results (whether supportive or nonsupportive of the null hypothesis) were important enough to present to others, even if they were not yet deemed worthy of writing up in a formal paper.

An archive of such conference papers in each discipline would be extremely helpful in speeding access to at least the topics and core findings people have, and researchers interested in common problems could communicate with each other. Some major societies (e.g., SPSP) are already considering such a practice. As in the current system, a finding presented at a conference would not "count" in the scientific race to be the first to publish a finding because conference papers are not publications. Nevertheless, scholars aware of such prior work could still easily cite it. The major barrier to communicating one's results, then, would remain with the individual scientist. Do they submit their work to conferences? In my own lab, results of individual studies are not sent to conferences at the first opportunity if there is some doubt about their replicability or concern about whether an effect might be "scooped" if known too widely. But in nearly every case, there is a presentation of the results that precedes actual publication

by at least a year. If conference findings were more easily accessible, it would help to fix the no or slow communication problem.

The second problem mentioned is that of incomplete communication. For example, researchers do not report everything that they did because they often don't even know what critical aspects of the procedure were necessary to get an effect. Nosek and Bar-Anan do not address this problem, but it seems like at least some of this can be solved by linking raw materials for a study (in a common format) with articles once they are published. Publishers and societies are working on this issue currently (e.g., see APA's new Archives of Scientific Psychology).

The third problem is inaccurate or unmodifiable communication. In the current system, once a paper is published, it remains that way for all time. So, what can one do when errors appear in an article (or an article needs to be retracted) or some important feature of the procedure that was missing is now deemed worthy of inclusion? With virtually all journals moving to online publication and availability, corrections and addendums are ever easier to make and link to the original article. For example, although I do not think that authors should be able to go back and modify their articles as if they never wrote what they did (it is important to keep the historical record clean), errors or new developments can be reported as linked additions. Within the current digital universe, wouldn't it be possible for publishers of journals to allow an errata or addendum or comments area to follow each article published? Societies who own journals (e.g., APS, SPSP) and contract with private publishers could lead the way in negotiating this feature into new contracts. Having been part of several negotiations for societies with publishers, I suspect that many will embrace this notion. Some have even suggested linking articles to podcasts or interviews with the authors. In addition, following publication, authors could post links to their new conference papers that follow up on the work they have published. Publishers want to draw traffic to the articles they publish, and anything that increases the value of the publication and their websites without adding too much cost would surely be welcomed. Furthermore, there is nothing preventing societies and publishers from linking a final accepted article to earlier versions of the paper along with the reviews so interested readers can see how the paper developed.

The important point is that if these are the core problems with the current system, there are steps that can be taken that address them at least in part without having to wait for all of the major changes proposed to take place. This is notable because some of the six changes proposed to address these problems have problems of their own, at least in my view. The more modest changes just noted could be made within the context of the current system and might be sufficient to address

the core concerns or at least serve as stepping stones to the more useful of the dramatic changes that Nosek and Bar-Anan suggest.

The first major change suggested is moving to full digital communication. I agree that this is already happening, inevitable, and highly desirable. One touted benefit mentioned is that this effectively removes page limits. However, even if true, page or word limits serve other purposes such as protecting readers from unnecessarily wordy papers. Just because one can write as much as one wants doesn't mean one should. Nosek and Bar-Anan argue that in the current system, the number of articles a journal publishes remains the same whether it gets 100 or 10,000 submissions. I disagree. Journals routinely increase the number of pages they publish to accommodate increased submissions (assuming submission quality remains constant). During my time as editor of *PSPB* and when serving on the SPSP publication committee, society members seemed more concerned with holding constant the rejection rate than the number of pages. If a journal is getting more and more papers, it means it is getting more and more popular, implying more and more people will want their library to subscribe providing the resources necessary to expand. Also, is digital publication really unlimited? The costs for such things as proofing papers and giving them a standardized look goes up with more papers published. And if the papers undergo even a modicum of review prior to publication, the cost of providing stipends to a greater number of associate editors goes up as well. The costs may be lower with digital than hard copy, but the relationship of costs to amount published remains positive.

Nosek and Bar-Anan are surely correct that journal editors often note in rejecting a paper that the journal gets more "good" papers than it can accommodate. In many cases, this is a polite way of saying that the journal aspires to publish "very good" or "extremely good" papers rather than merely good ones. The prestige of a journal and everyone associated with it depends on how high the quality of the papers it publishes is perceived to be. Removing page limits would not change some journals from being a repository for extremely important and exceedingly well-done papers and others for being a repository for somewhat important and generally well-done papers. Would a journal not cost more to a library or individual subscriber if it produced 10,000 articles a year rather than 100—whether print or digital? If so, journal publishers would still determine how much they would charge by the anticipated number of articles (or words) it would publish. There are other methods, of course. For example, publishers could charge based on the impact rating of the journal. If so, it would put pressure on the journal's editors to publish higher and higher quality papers (and thus fewer of them), the opposite of Nosek and Bar-Anan's

intent. The point is that digital publication involves some of the same considerations and limitations as print publication.

Second, the authors advocate open access to all published research. They deal with the current high cost of subscribing to journals by having authors and the universities that employ them (producers) rather than readers (consumers) pay for the journal's cost in the form of publication fees. Of course, there are far more consumers than producers when it comes to science as with any other valued commodity. Imagine if producers in any other enterprise were charged for producing their product so they could give it away free to consumers. One doesn't have to think about that model too long to realize some potential concerns. Should our top researchers and universities have the most cost so that their research product can be given away? One can still cut out the for-profit publishers (if that saves money) but still have the consumers pay for what the producers produce. Of course, some of the producers are also consumers, but they pay when they find research to consume rather than produce. Journal editors have the job of selecting products (articles) that they believe consumers will want (pay for). In science, the best product is high-quality research.

One can imagine in an increasingly decentralized university-funding model, the university would pass the costs of publishing on to departments, which would then either need to impose a limit on the amount that its faculty members could publish or pass the costs on to the faculty member himself or herself. Because university budgets are not unlimited, instead of cutting back on the number of journals it consumes (in the current system), cutbacks would occur in the number of articles faculty could publish. This might not be a bad thing, but it does seem a bit odd to have a system that restricts the publication of research (which prevents everyone from learning about it) rather than the local consumption of it. Ultimately, it may depend on how much is saved. Nosek and Bar-Anan claim that 1.3 million articles per year are produced and at least \$1.6 billion would be saved by going to digital access. To put this in perspective, if these savings were distributed to each article producer, each would receive \$1,231 in savings, not quite enough to afford publishing one article in PLoS ONE (cost of \$1,350).<sup>1</sup>

One innovative suggestion that Nosek and Bar-Anan make is that editorial boards of journals can create their own brand-new open access journals by

<sup>1</sup>If any change were made, a fee for reviewing a manuscript rather than publishing it could be considered. It spreads the cost over more people and would also serve as an impediment, of sorts, on submitting work that is not really ready for review. Indeed, the journal could provide a rebate for articles that were accepted, rewarding those who produced the highest quality work. Journals with fees that are too high would be checked by those upstarts operating at lower cost.

simply adding "open" to the name of the journal and carrying on as before. I look forward to publishing in the *Open Journal of Experimental Social Psychology*, but I suspect that once the Elsevier lawyers get involved, I will be publishing in that journal at about the same time as I am drinking my Open Coca-Cola and taking calls on my Open Apple iPhone. I am all for open access if the financial model is worked out, but so far, I am not convinced it has been.

In Stage 3, Nosek and Bar-Anan disentangle research publication from evaluation. This would allow authors themselves to decide when to post their papers to a huge article repository. Furthermore, they can keep posts of all papers leading up to the final one. As noted earlier, the latter can be accomplished in the current system and the former can be accomplished to some degree by creating a database of conference papers. If the proposed open database repository were established, one wonders if scholars would wait until they have some confidence in their findings before posting. Some might reason, why wait? If a finding doesn't pan out (stand the test of time) there is little loss, because authors can honestly claim that they only ran and archived a very small tentative study (and will surely include cautionary language in the discussion). On the other hand, if the preliminary result does pan out, then the author of the early small study gets the scientific credit that comes with publishing first.

Researchers would get access to data much sooner than now if they read the archived papers, but how does one deal with such data in one's own work? Is it appropriate to treat such self-determined archived data as if they are peer-reviewed findings worthy of citing, or is it more appropriate to treat them as we now treat unpublished data? In the current system, scholars are not expected to know about everyone's unpublished data. If the social psychology archives had every high school, undergraduate, or layperson project on the planet (along with graduate students and faculty), that could be a huge archive to mine (both good and bad). This might not be an issue for physics (where research is expensive to produce) but could be for some areas of social psychology. One can imagine the many exciting ideas as well as junk that such an archive would contain. And one has to wonder, if another person already has received credit for some idea (because it is in the archive), will there be enough people willing to formally test this (other person's) idea? One can hope so, but current practice suggests this is debatable.

In Stage 4, Nosek and Bar-Anan suggest a major modification to the current article evaluation system. Authors could submit their articles to as many journals as they like and "dozens of journals could promote the same article." How would journals decide which articles to promote? Would they send them to reviewers? If so, there is much time that could be wasted if the same article is reviewed by a dozen journals. Or the

archive could collect ratings from volunteers much as TripAdvisor collects ratings of hotels. But Nosek and Bar-Anan presumably would allow all consumers to do the ratings, and nonexperts would surely outweigh experts in some areas of social psychology (though probably not in any area of physics). The authors suggest “review services” to which scholars would send their articles. Who runs these services and who pays for them? Researchers are allowed to send to multiple services. Will reviewers seek multiple grades from different services, or will they shop sequentially or keep going back to the same review service to get a better grade? These are interesting questions, but regardless, it doesn’t sound like this will save reviewers time over the current system unless submitters get an A on the first try. If not, aren’t we back to the same system of scholars getting a B or C on the first round and then revising to seek a higher grade (to enhance one’s tenure and promotion prospects, salary raises in one’s department, offers from other universities, etc.). One can see different review services developing, and universities would then want to know which review service gave you that A (was it one that gives lots of As or one that is stingy with As), just as they currently want to know what journal your published paper. Furthermore, given the large number of papers to review, wouldn’t the most prestigious grading systems develop a triage procedure similar to the most prestigious journals today? And wouldn’t authors then seek the grading system that was most suited to their paper (i.e., weaker papers going to services that were “easier” graders)?

In Stages 5 and 6, the call is to publish all peer reviews and to open up the review system to all who wish to comment. This could be a good idea, but as

Nosek and Bar-Anan recognize, if scholars are to be able to locate and profit from the best reviews, then the reviews themselves would need to be submitted to a grading system so all can profit from the A+ reviews and ignore the D– reviews. And of course, the reviews of these reviews should presumably also be published along with their grades. Nosek and Bar-Anan envision a world in which scholars can earn tenure by being “renowned evaluators of research.” This is not likely to occur unless, as just mentioned, the reviews are graded in some way. Grading reviews will surely lead to more work for those review services, and makes one wonder if some people will become renowned reviewers of reviews. Some of the possible advantages of publishing reviews (e.g., transparency) are good but can be accomplished by requiring signed reviews in current practice so that all other reviewers and authors know who reviewed their papers. And as noted earlier, the current system can accommodate publication of reviews.

In sum, improving the review and publishing process is a noble goal. I’d encourage societies and publishers to begin implementing some of the ideas on which general agreement can be secured and not wait until the wholesale changes Nosek and Bar-Anan advocate are implemented.

#### Note

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