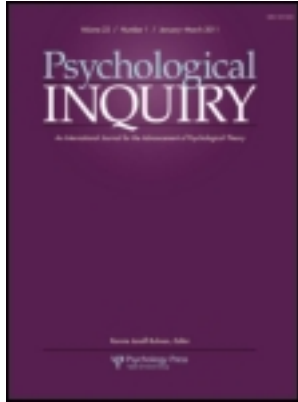


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A Dinosaur Comments on the Coming Apocalypse: Does Anybody Else See That Asteroid?

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A Dinosaur Comments on the Coming Apocalypse: Does Anybody Else See That Asteroid?

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Brian A. Nosek and Yoav Bar-Anan (this issue) present an expansive vision of the future of scientific communication in psychology. In principle, I agree with much of what they suggest and find true wisdom and innovation in their approach. It is easy to look at the status quo and sigh, “There must be a better way.” It takes courage, imagination, and energy to propose a new way forward. For this the authors deserve appreciation and praise. Yet, thinking about this article while lugging around a few manuscripts from the *Journal of Personality and Social Psychology: Personality and Individual Differences (JPSP:PPID)* in my backpack, I found myself a bit queasy, contemplating my own apparently imminent obsolescence. This editor’s accoutrement (hard copies of manuscripts and reviews; reading glasses; a stiff Americano; and, of course, a can-do attitude and open mind) feel like John Henry’s hammer meeting up with the steam drill. Is the role that has occupied so much of my time and energy, and generated no small amount of professional satisfaction, about to go the way of the dinosaur? Am I to find myself walking not so much in the footsteps of Gordon Allport (King, 2010) but those of whoever it is that runs *Rotten Tomatoes*? As onerous as journal editing is widely perceived, I really don’t want the job of Internet message board moderator.

Is my reluctance to jump with both feet into the utopia described by Nosek and Bar-Anan a product of vanity? Dissonance? Some deeply ingrained narcissistic need to edit? Simple inertia? For whatever reason, I feel compelled to pause and consider what we might be leaving behind on our journey to utopia. In this commentary, then, I expound upon the editorial role and seek to flesh out aspects of that role that influence the publication process in ways that are not given sufficient care in the target article. My point is simply that many of the changes the authors seek can be accomplished by editors within the current system. Active editors have the capacity to change the status quo in ways that authors have not acknowledged. Such efforts can improve (and have improved) scientific communication, apart from the kind of revolution the authors envision. I hope, also, to highlight that there are aspects of the editorial role that are valuable to science that would be difficult to replace and might be missed, even in utopia.

Editorial Innovation

Nosek and Bar-Anan underestimate the current ability of editors to institute changes in the logistics of publishing. Through my experiences as the editor or associate editor for four different journals in personality and social psychology, I have come to appreciate that editors have a great deal of power to make changes in the day-to-day operations of a journal in ways that might not be obvious from the outside. Currently, at *JPSP:PPID*, my associate editors and I abide by one hard and fast rule: No more than two rounds of review are permitted for any paper (without special dispensation that has never been received because it has never been requested). I am lucky to work with very talented associate editors, but it is clear that if the context is in place, people can make decisions, without consensus among reviewers or the need for round after round of feedback.

A far more impressive example is provided by the streamline review mechanism at the *Journal of Research in Personality (JRP)*. As editor-in-chief of *JRP*, Lynne Cooper instituted this brilliant innovation by which authors of papers rejected by any APA or APS journal could revise the paper and submit it, along with the previous action letter, reviews, and a revision letter, to *JRP*. The hard work of reviewers is not wasted or repeated. Further, decisions can be rendered quite quickly: After inheriting *JRP* from Lynne, I once accepted such a paper within hours of its submission. The authors admitted to riding an emotional roller coaster, having scarcely digested a rejection from *Psychological Science* that morning, but were happy with the eventual outcome (just before lunch). It seems to me that this inspired policy might be instituted at any number of journals and would greatly reduce many of the logistical issues that plague our science.

Editors can remedy many problems on the quality side as well, taking active steps to institute the good advice that emerges in the field (e.g., Francis, in press; LeBel & Peters, 2011; Simmons, Nelson, & Simonsohn, 2011). Editors can demand, for instance, replications of effects that are counterintuitive or that seem too good to be true. They can prod and probe in directions reviewers might not have considered.

Editors and Null Results

Clearly, the issue of how to deal with null results is a serious one for our science. Nosek and Bar-Anan propose that publishing everything will help to alleviate this problem. From my perspective, the real problem is that, because of our reliance on null hypothesis significance testing, we have come to think of studies with null results as truly qualitatively different from those that report significant results, as if never the twain shall meet in the same paper. Editors can change that. Editors can accept papers that present null results, and can call on authors to use the appropriate statistical techniques to evaluate the truth of the null hypothesis. More important, within articles, editors might begin to recognize that really good papers are not “too good to be true” papers. The existence of null results *alongside* significant ones within the same program of studies would not only provide a more realistic portrait of science but add confidence to assertions about what is really going on in any study (e.g., Francis, in press).

Editors Are Not Calculators

Nosek and Bar-Anan’s very strong reliance on average “grades” for papers far underestimates the editor’s role, at least as I see it. At the very least one would wish for the range and standard deviation! Editors are not calculators. They do not average reviewer ratings. I think it is good for science that editors are not prisoners of reviews or ratings. With all due respect to reviewers, I have accepted papers that they all hated, I have rejected papers they mostly liked, and I have found that often it is the most exciting papers that provoke very extreme reactions. Would a paper floating around online with a reviewer rating of 50% be read? What if that paper was carrying around a 1% and 100% rating and just might have the capacity to greatly irk and greatly inspire? Although there may be papers the merits of which are well represented by the average reviewer rating, in my experience it is often papers that inspire stark disagreement that have the greatest promise. When one reviewer admonishes me for not rejecting a paper without review while another suggests acceptance without revision, I am glad that I am in the position of making a decision on that paper and am not limited to knowing it got a 50% “fresh” rating.

Who Will Do the Teaching?

A final aspect of the editor role is pedagogical. Part of editing, in my view, is shepherding scholars through the process of figuring out what their papers are about and finding out what their data are telling us about human behavior. That a great deal of that conversation

occurs in private is not, in my view, wholly problematic. Nosek and Bar-Anan’s perspective is one from a perch of great success in producing science. This perspective may miss some of the nuances of science as it is practiced by those whose efforts have not been so successful. I certainly think that transparency is valuable, but I am not convinced that either science or scientists would be well served by publicizing everything that comes across my desk or all of the editorial back and forth I have had with authors about their work. What I have learned from these “conversations” is that authors very often do not know when a manuscript is “ready for prime time.” I can appreciate the idea of separating publication from evaluation, but I fear that without the stakes of publication “on the line” the same level of attention would never be brought to submissions.

A lot of people submit papers to journals. Not all of these individuals are scientists. I have handled submissions from unlikely individuals, high school students, renegade undergraduates, and a variety of other walks of life. Such individuals might be prohibited from submitting to open access portals, according to Nosek and Bar-Anan, without, for instance, membership in a professional society. I understand that this might work for other sciences, but for me, even the most wrongheaded of these efforts suggests an enthusiasm for science that ought not to be squelched. If the next William James wants to submit a paper to *JPSP*, shouldn’t he (or she) at least be allowed to try?

Even among scientists, many submissions are simply not sound. Claims are made that far exceed the data presented. Analyses are outdated, potentially misleading, or simply wrong. Nosek and Bar-Anan far underestimate the sheer enormity of papers that are submitted to journals by scientists that are not even in the ballpark. In such cases, I still believe the authors deserve some kind of information about what is strong science and how their work falls short of that standard. That these conversations occur in private is, in my view, appropriate. I believe that this process is a bit more than the minimal screening the authors describe.

Yes, if authors choose to publish their papers online prior to receiving feedback, who is to stop them? But having such papers in the public domain of science seems to me to be fraught with potential disaster. Can we expect a reader of “science” to disregard a paper that supports his or her own prejudices because it has a reviewer rating of 15% or because no one has commented upon it? The controversy over autism and vaccinations tells us that even when scientific findings have been debunked thoroughly, once published they continue to possess gravity in people’s lives. Getting good science out the door as fast as possible, to me, is simply not as important as ensuring that science is, in fact, good.

What I Will Miss

Nosek and Bar-Anan gleefully announce the death of the whole idea of journal *issues*. Apparently, utopia has no use for tables of content, either. I would miss them: At what other table will you find these authors sitting right next to each other (and both of them smiling)? In addition to shedding a tear over the loss of tables of content, I will miss issues. Perhaps I am dispositionally prone to inertia, but I have found myself, more than once, reading an article in a journal and, having nothing else around, going ahead and reading the next one, and maybe the next one after that.

Maybe here is where I have landed on something that deeply worries me about utopia: Its efficiency seems to rule out accidents and “wasted” time. How dreadful to read an article that is not specific to one’s research area—or to even know that it exists. I hope science never becomes so efficient that we miss completely those accidental moments when we are exposed to what we didn’t know we didn’t know. While racing toward utopia, I hope we don’t find a way to rule out the stumbling that might provide inspiration for even more creative and interesting scientific directions. That I find value in these accidental wastes of time is, no doubt, mere dissonance, and I can hear the authors as-

suring me that I can still live in an accidental world, just by setting my filters appropriately. I’ll get on that as soon as I find my way out of this blasted tar pit.

Note

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