Let's Publish Fewer Papers

Leif D. Nelson [a] , Joseph P. Simmons [b] & Uri Simonsohn [b]

[a] Haas School of Business, University of California, Berkeley, California

[b] The Wharton School, University of Pennsylvania, Philadelphia, Pennsylvania

PLEASE SCROLL DOWN FOR ARTICLE

Let's Publish *Fewer* Papers

Leif D. Nelson

Haas School of Business, University of California, Berkeley, California

Joseph P. Simmons and Uri Simonsohn

The Wharton School, University of Pennsylvania, Philadelphia, Pennsylvania

Brian A. Nosek and Yoav Bar-Anan's (this issue) scientific utopia is quite appealing. The narrative of Hari Seldon's productive morning, which concludes the article, depicts many of the ideals at the core of science. Ideas are celebrated, arguments are pursued to increase collective knowledge, and clarity of thought is rewarded in all corners of the academic community. Indeed, there is not much to dislike about this vision of the future (though one might quibble with the lack of nonacademic conversation in Seldon's life). Like many utopian narratives, Seldon's life is also tidy to the point of austerity. Reviewers are thoughtful even when disagreeable, discourse is sensible and productive, and insight is seemingly encumbered only by the speed of light. The ugly detritus of imperfect science has been eliminated by the procedural and incentive-based efficiencies of modern life. This future sounds amazing.

But maybe it is not as amazing as it sounds. Nosek and Bar-Anan outline a series of inefficiencies in current scientific communication and then lay out their utopian solution. The existence of those inefficiencies is inarguable, as is the sensibleness of many of Nosek and Bar-Anan's proposed solutions. For example, we agree that there is no longer a need for page limits, long lags between acceptance and publication, and prohibitive journal subscription fees. Separating research production from for-profit publishing houses seems like a goal well worth the adjustment costs. But we worry that certain features of Hari Seldon's world will lead to more ugly detritus and less pristine knowledge. In particular, we focus on two concerns. When all findings are made available, (a) it is harder to discriminate the true findings from the false findings, and (b) there will be more false findings.

Neat File Drawers versus Cluttered Offices

A critical inefficiency identified by Nosek and Bar-Anan is "no communication," the idea that the current system withholds key insights from the academic community. They accurately point out that the "file-drawer effect," the fact that authors file away their less suc-

cessful findings, inevitably leads to publication bias. If we hide all the bad stuff, then whatever is left is almost certainly too good to be true.

When we think through the alternative, however, we find just as much to worry about. We worry about the "cluttered office effect" that may follow if we do away with file drawers and publish every study we run. In an office full of papers, it is hard to tell the good ones from the bad ones.

Nosek and Bar-Anan suggest that this will be remedied by rating systems and dynamic reviewing and updating. The problem is that when people are confronted with a vast amount of information, they are vastly less likely to read it, and they are especially unlikely to read it all carefully.

People are lazy and busy. With such constraints, in this system, not all researchers would receive equal (or sufficient) consideration. Already established researchers will get the initial benefit of more readers, the subsequent benefit of more comments, and the continued reification of high status. The less established researcher is unlikely to even be noticed and less likely to be lauded. This is especially true in a system that rewards identifiable commenters. A researcher seeking a top job would do better to comment (positively) on a paper by a famous, higher status researcher. That comment will be more likely to be read, and more likely to be viewed favorably by an influential person.

This may all possibly be worth it in order to advance scientific knowledge, but whether that advancement occurs ultimately depends on the value of those papers being rescued from the file drawer. There is a notion that the file drawer contains key ingredients for a complete understanding. We tend to picture crucial failures to replicate widely accepted findings and fascinating new insights that did not overcome the blight of an unfortunate "$p = .081$." What is less often pictured is the paper that landed in the file drawer, not because of the vagaries of the publication process but because it reports a study that was ill-conceived, poorly run, or generally uninteresting. That study serves the intellectual community best by staying in the drawer.

Once it enters the academic market it clutters our knowledge. Perhaps the field quickly identifies it as

dead weight and relegates it to a set of discarded mediocrity. Just as likely, though, it has already done some harm. When every paper is available, it becomes increasingly burdensome to find the good papers, and even harder to find the diamond in the rough—the paper that is not by a famous person, not from a famous school, and not in a popular research area. That paper goes from having a chance to be noticed in the pages of a prestigious journal to simply being entirely unread.

All of us know that for every good idea we have, we needed to consider many bad ones (sometimes for embarrassingly long periods of time). The same, however, does not apply to papers. We don't need to publish many bad papers in order to generate a good one. It is a good idea to drop bad ideas before they grow up to be bad papers. Bad papers are easy to write, but in the current system they are at least somewhat difficult to publish. When we make it easier to publish papers, we do not introduce good papers into the market (those are already going to be out there); we introduce disproportionately more bad papers.

Easy Publication Could *Increase* False-Positive Findings

We don't yet know for sure, but our educated guess is that some published papers present false-positives: They erroneously present evidence for effects that do not actually exist. The occasional publication of a false-positive is bad; lots of false-positives are catastrophic. False-positives are hard to identify and correct, and they impose severe costs on the scientific community, causing it to misallocate resources and to lose credibility. Crucially, those costs are felt much more by the field as a whole than by the individual researcher, especially when it takes years to diagnose a false-positive as such. Indeed, individual researchers rarely pay a price for publishing false-positives; they instead benefit from adding a novel finding to the published record. Nosek and Bar-Anan's utopia would not change that. It could actually make it worse by stimulating paper-count inflation and increasing the need for researchers to constantly be putting out new "findings."

It would also not change how easy it is for researchers to generate false-positives when details of data collection and analysis remained undisclosed. The file drawer would still contain all of the failures the author *prefers* not to make public. Moreover, a system in which "everything is published" would make it even easier for the job-seeking (or tenure-seeking or grant-seeking or reputation-seeking) researcher to publish a theoretically novel, attention-getting false-positive. Decreasing barriers to publication will do little to increase the reporting of the critical methodological details that might otherwise keep the false-positive safely quarantined from scientific discourse. In this utopia it is *easier* to publish that people are younger after listening to "When I'm Sixty-Four" but not any harder to quietly drop the failed "Hot Potato" condition (Simmons, Nelson, & Simonsohn 2011).

Under the current system, researchers are heavily rewarded for having new and exciting ideas and only vaguely rewarded for being accurate. Researchers are trained to defeat the review process and conquer the publisher. Uncovering a new and true insight is quite helpful in that process, but it is hardly necessary. As we have discussed elsewhere (Simmons et al., 2011), small flexibilities in data analysis give researchers a potent weapon against the reviewing process, but one that does not directly increase truth value. In a world where researchers are rewarded for how many papers they publish, this can lead to a decrease in the truth value of our shared knowledge. We think that the truth value of published work has been at least partially compromised. Decreasing the barriers to publication will certainly not help.

A Different Utopia: One-Paper-Per-Year[TM]

As a thought experiment, consider a different utopia. In this one, researchers are allowed to publish only one paper per year. Publication quantity is no longer a relevant dimension. This system incentivizes researchers to demonstrate that an effect is robust and generalizable, and hence true and important. Rather than the community of researchers being forced to wade through a mountain of papers to discern, with extreme difficulty, the true ones from the false ones, it is the researcher herself who chooses among all of the effects she would have attempted to publish in order to focus on the one that she can obtain most reliably.[1]

In this One-Paper-Per-Year[TM] utopia, if Hari Seldon arrived at work and the published literature was slimmer and more digestible, would he be worse off? Furthermore, rather than wondering about how to evaluate two job candidates who differ in quality and quantity, Seldon would instead see candidates who were matched on the latter, allowing him to entirely focus on the former. Finally, Hari can pursue his own work with improved clarity and focus. There is only one paper to write this year. He had better make it count.

Acknowledgments

---

[1]We agree that it is impractical, but it is just a thought experiment. Still, we stand behind the notion that the ideal is much closer to "a paper a year" than to "publish as many papers as you can."

Note

Address correspondence to Leif D. Nelson, Haas School of Business, University of California, 2220 Piedmont Avenue, Berkeley, CA 94720. E-mail leif_nelson@haas.berkeley.edu

Reference

Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2011). False-positive psychology: Undisclosed flexibility in data collection and analysis allows presenting anything as significant. *Psychological Science*, *22*, 1359–1366.