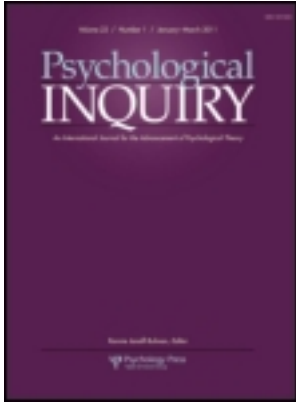


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Does Open Scientific Communication Increase the Quality of Knowledge?

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Does Open Scientific Communication Increase the Quality of Knowledge?

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It was a pleasure to read the rich, sometimes even breathtaking target article by Brian A. Nosek and Yoav Bar-Anan (this issue) on a utopia of open scientific communication. The authors start from the premise that the purpose of public science is knowledge accumulation, and propose a sequence of six steps that would increasingly take us away from the current model of scientific communication with its mixture of public and private communication channels to a utopia of rapid, open, and extensive communication. The sequence of the steps makes sense, but does each step serve the purpose of public science?

In my view the most urgent problems of today's psychological science are not slow speed of dissemination, nontransparent decision processes, or restricted communication options. Instead, psychological research accumulates a vast body of bits and pieces of knowledge that are produced by scientists, institutions, and journal editors that increasingly strive to maximize impact instead of quality by focusing on jazzy "unique contributions" instead of contributing to a common cumulative progress based on rock-solid findings. Consequently the disseminated knowledge is to a large degree nonreplicable and lacks serious theoretical foundation and integration.

Therefore let's look at the proposed sequence from the somewhat different perspective that the purpose of public science is the accumulation of *high-quality* knowledge.

Step 1: Speed-Quality Trade-Off

There are many advantages of dismissing print journals, including faster and less costly publication. A good friend of mine once puzzled me with his view that a complex bureaucracy can be healthy because it slows down decision processes, leaving time for revisions—certainly not a majority view among scientists who like to complain about inefficient administrations. But there is some wisdom in this view. I am aware of cases when authors were informed about mistakes in an accepted article by readers of a preprint or an electronic early view publication, or recognized the mistake by themselves and had a chance to correct the mistake because of the delay between acceptance and

print. Later in the target article the suggestion is made that authors can and should revise publications based on the feedback they receive. One idea would be that after its acceptance a paper has to survive a fixed try-out phase of 3 months or so before it becomes flagged as final. Peers may then, depending on their expertise, either engage in the tryout phase or only consider final versions for their work. Quality needs time.

Step 2: Open Access—But for Everyone

I fully agree that some large journal publishing companies abuse scientists in order to make excessive profit. This needs to be changed, and physics show that this can be accomplished. There are, however, also publishers that currently ask for excessive publication fees for an open access option, and a matter of great concern are dubious open access journals that encourage their editors to attract manuscripts from Big Names without requiring a serious peer review. A critical issue is whether publication costs are low enough for young scientists not involved in well-funded projects or colleagues from developing countries. If costs are too high for them, open access is in fact closed access.

Step 3: Disentangling Publication From Evaluation—After a First Quality Check

At a first glance, disentangling publication from evaluation will necessarily lead to poorer quality of public knowledge because everyone may enter methodologically unsound studies into a repository. However, if viewed within the context of the later proposed review systems, Step 3 makes more sense. Nevertheless I wonder why repositories should not have a gatekeeper function by requiring minimal scientific standards that can be easily checked with high interrater agreement (e.g., sufficient power of the design for the main research questions). In these days of putting, for good reasons, the finger on false-positive findings (Fanelli, 2011; Simmons, Nelson, & Simonsohn, 2011) we should not forget the many false-negative findings based on poor methodology. To my experience, roughly half of the manuscripts submitted today to journals with a serious peer review would not pass

such an initial check. This would enormously help the proposed review systems and avoid the abuse of repositories for placing poor-quality studies. Note that such gatekeeping does not proliferate a file-drawer problem because not everything drawn from a filer is useful; serious meta-analyses include such initial gatekeeping for methodological quality, and if the repository gatekeeping can be trusted, this would also make life easier for meta-analysts.

Step 4: Graded Open Evaluation and Diversified Dissemination

I greatly like the idea of a graded evaluation that would strongly change the current practice where it more depends on the author's frustration tolerance or stubbornness than on the paper's quality whether a paper is finally accepted somewhere, after involving many people's time and effort. I vividly remember the case of an author with hundreds of publications in his CV who two times resubmitted an initially rejected manuscript to the same journal without doing any revision because he lost track of the long rejection history of that paper. Along with the ability to revise a paper, graded evaluation would better reflect its quality, and thus help to improve the mean quality in the field. Also it would assist the dissertation option of many universities of submitting a cumulative dissertation (based on a bundle of published or accepted papers plus an integrative text). It would avoid unduly delay of the dissertation when papers in the bundle are rejected, would counteract the tendency to submit such papers to journals with low rejection rates, and would assist reviewers of such dissertations because they can use the open evaluations of the papers for their own evaluation of the whole bundle.

Of course, everything depends then on the quality of the reviewing systems. Having multiple systems compete with each other is a good idea if it is competition for quality. However, there is an uncanny similarity with the current grading system of banks and countries. After a certain time of competition only a very limited number of reviewing systems may survive the competition—the Standard & Poor's and Fitches of scientific grading—that will have an enormous power of directing research and the career of researchers. Unless they are controlled by the scientific community (e.g., via an international organization), they may grade in ways that serve their own interests rather than increase the quality of research.

Concerning diversified dissemination, I wonder why journals are needed anymore. Wouldn't it suffice to maintain reading lists of recommended articles in repositories? Today's reading lists by publishers are composed with the intention to increase the impact of articles that promise high impact. Diversified dissemination

driven by research interests, not by expected impact, would counteract this unhealthy preference for mainstream research and thereby increase the quality of public knowledge.

Step 5: Publishing Peer Review

In general I like the idea to publish reviews because it will increase transparency of grading, provides alternative views on controversial issues, disseminates the review to a much broader audience, and can serve as a real incentive for doing a serious job. Because a broader audience can access the reviews, an overall increase in quality of knowledge can be expected. I also agree with the point made in the following step that good reviewers are not necessarily creative scientists, and that creative scientists are often bad reviewers because they do not care so much, or are unable to care, for others' views on the same topics. New members of editorial boards are often acquired because of their reputation as scientists, not because of their reviewing skills. Publishing peer reviews will shift the burden of reviewing from good scientists to good reviewers such that both groups will profit, which in turn will increase the quality of public knowledge.

Step 6: Open, Continuous Peer Review—if the Stakes Are High

This is the proposal I found least convincing. Good reviews are based on real expertise and successful and efficient communication of the points made. As an editor, I introduced a yearly special issue with target articles, open peer commentaries, and authors' responses in order to increase the transparency of controversial issues. However, Step 5 of publishing peer review would already largely fulfill this goal. Open peer commentary on everything in a repository may proliferate gossip, serving the ego of the self-selected reviewers more than serving science. More important, impact in terms of the number of attracted reviews unavoidably becomes a goal in its own, which will increase even further today's unhealthy tendency to attract impact rather than to provide quality. Should we allow every member of a professional society (the minimum competence for reviewing required by the authors) to press a "like it" button as on Facebook? In the authors' own words, "Work that is disinteresting will not be reviewed. Work that is interesting will get reviewed a lot. Reviews become the life blood of evaluating, improving and making the research have impact." Yes, but open peer review will undermine the quality of public knowledge unless the stakes for reviewing are set much higher than proposed. Why not restrict reviewers to the

reviewing systems proposed in Step 4 if these reviewing systems are properly controlled by the scientific community?

The target article may mark a turning point in the development of scientific communication in psychological science. The authors have offered us an impressive sequence of proposals how the current landscape of disseminating psychological findings can be changed. Looking at the proposals from a quality of knowledge perspective, some will be helpful for building rock-solid mountains, but others may increase the danger of getting lost in fashionable, shifting sand dunes. It is our responsibility as scientists to assist the former and prevent the latter.

Note

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