

International Edition: DOI: 10.1002/anie.201510303  
German Edition: DOI: 10.1002/ange.201510303

# Better Practices in Scientific Publishing\*\*

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Everyone strives to do or be the best, but the state of better, meaning to improve that which has been done before, may be all that can be reasonably expected or hoped for.<sup>[1]</sup> Publishing is extremely important to the science profession because in the scientific world, the assessment of the value of an individual's achievements depends on reputation, and wide recognition of the worth of scientific work is most effectively achieved through publications (see my previous Editorial on assessing academic researchers: *Angew. Chem. Int. Ed.* **2012**, *51*, 7338; *Angew. Chem.* **2012**, *124*, 7452). No wonder then that pressure to publish manuscripts in journals with large readerships and highly cited papers has greatly intensified. This problem is further aggravated by the practice of some institutions and countries to base promotions of researchers on the numbers of papers published and the impact factors of the journals in which they were published. I suggest two possible changes that might make better practices in scientific publishing, one involving the review of manuscripts, and another related to making the author contributions to published papers more transparent.

The review process and subsequent revision often adds much value to the final manuscript, as well as ensuring that high standards are met. Consequently, a premium is put on carefully reviewed articles, especially those appearing in journals with an established record of publishing important papers. In addition, because of the flood of papers appearing in so many different places, comprehensive and critical reviews assume ever-increasing importance. I want to consider the difficulty that publishers and editors face in finding qualified reviewers. I have been told many times that this lack of good reviewers represents an important obstacle in maintaining a high quality of publications.<sup>[2]</sup>

What has made this problem more pressing today than previously? Certainly, a major part of the answer is the greater number of papers. Another part is that the life of the researcher has become, if anything, more stressed, with increased bureaucracy.


## With the concept of double-blind reviewing we are moving in totally the wrong direction

Why do reviewers accept this task? Presently, it seems to be a combination of 1) if they review others' papers, others will review theirs; 2) they learn about what work their colleagues are engaged in, and this may help them in their own research; and 3) they believe that such service helps to maintain the scholarly community (see the comment "The Problem(s) With Credit for Peer Review" by David Crotty<sup>[3]</sup>). Clearly, these motives are not enough for some.

At present, referees who review a paper are anonymous to the author. There is even a move afoot that the author or authors of a manuscript should also remain anonymous in a double-blind review process (see "Publishing: Is Double-Blind Review Better?": S. Paulus, *APS News*, July 2015). At the 9th Forum of the Alexander von Humboldt Foundation on the Internationalization of Sciences and Humanities, both the Editor-in-Chief of *Nature* (Philip Campbell) and the Editor-in-Chief of *Science* (Marcia McNutt) advocated the use of double-blind reviewing. The argument was made that this makes a level playing field with as much opportunity for a lesser-known author as a better-known author to get a favorable review. Today, *Nature* and some of its sister journals offer a double-blind option to authors. The idea is to remove all identifying materials from a manuscript. Interestingly, a double-blind experiment carried out about two decades ago by journals of the American Physical Society did not go well. From 1993 to 2001, the use of this option was requested for only 0.06% of the submitted papers, and of these manuscripts, only about 6% were accepted for publication, which is an acceptance rate about ten times lower than that for other submitted papers. There have even been efforts (by philosophy journals) to introduce a triple-blind system in which even the editor is anonymous. While some argue that double-blind reviewing overcomes bias, for example against women and members of minority groups, the results to date do not seem to support that contention. Moreover, most research work builds on previous research work done

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[\*\*] This article is based on a keynote address for Panel 1 of the 9th Forum of the Alexander von Humboldt Foundation, held October 20, 2015 in Berlin (Germany; "Identifying the Best—Theory, Method, and Practice").

 The ORCID identification number for the author of this article can be found under <http://dx.doi.org/10.1002/anie.201510303>.

by the same author. Consequently, it would not take much detective work to guess the identity of an author. Another objection is that by removing all identifiers, the reviewer is not able to judge appropriately how trustworthy or significant the claimed results are based on previous work from the same laboratory or research group (see *Nature* **2015**, 520, 623). Indeed, I believe that with the concept of double-blind reviewing, we are moving in totally the wrong direction!

I think the reason why it is difficult to obtain reviews from outstanding experts is that there is insufficient reward or incentive for the reviewer to undertake this important task. A financial reward seems to me to be out of the question, and neglects the fact that reputation is the real currency of the scientific realm. What is needed is a mechanism to recognize the valuable task that reviewers do perform in examining submitted manuscripts. I would like to see each journal publish annually a list of the reviewers used and how many papers each reviewer examined. To me, that would be the first step. A few journals, such as the journals of the American Economic Association, explicitly acknowledge reviewers but most journals do not. I also suggest that journals indicate in some manner the amount of reviewing done. I realize that some journals do not want to let it be known who their most reliable or best reviewers are, but the number of reviews done per year could also be listed in some way, such as one, two, or many. This approach might overcome the objection of Crotty that producing incentives for reviewing will detrimentally distort the review system.

As a further suggestion, I advocate giving reviewers the option to reveal their identities, not only to the authors, but also publicly, that is, they would be named in the published article. I could imagine this would provide benefits to the reviewers and the authors. It is common for authors to acknowledge by name the contributions of reviewers of book chapters.

What else can be improved? For every journal article with multiple authors, I would like to have a brief explanation of the individual author contributions to the manuscript. Presently, in many institutions and countries, credit seems to

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### Public recognition of reviewers' efforts is needed

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be awarded to the first author of a manuscript and to a lesser extent to the corresponding author (if different from the first author). This practice hinders collaborations, especially between research groups, as positions in the author line can cause great rivalries and really works against cooperation. So many advances require interdisciplinary efforts because so many problems do not come "prepackaged" with the names of departments and subspecialties written on them. The present system impedes such collaborations, whereas they should be nurtured to advance scientific progress.

Moreover, it is important to know the exact individual contributions when judging the worth of researchers in exercises such as promotions and the awarding of prizes. What I am suggest-

ing might or might not cut down the number of what some call "honorary authors" that I so often see in the literature, but it ought to make their "contributions" much clearer to everyone. A few journals now follow this practice of detailing author contributions, but I would like to see this more universally adopted. Papers that have tens to hundreds of authors may pose a unique problem (see the comment "How Many Scientists Does It Take to Write a Paper? Apparently, Thousands" by R. L. Hotz<sup>[4]</sup>) but certainly that can be handled in the supporting information. I think that a move to describe in more detail the contributions of individual authors to a project would make the research process more transparent and it would add a more human dimension to publications. It is my experience that anything that makes it clearer to others how research is really done benefits the reputation of science.

#### How to cite:

*Angew. Chem. Int. Ed.* **2016**, 55, 2606–2607  
*Angew. Chem.* **2016**, 128, 2652–2653

- [1] I recommend reading the book *Better* by Atul Gawande (Picador, **2007**), in which a surgeon describes what it takes to be good at something in which failure is so easy and so effortless.
- [2] From 1992–1995 I was Vice-Chair of the Board of Directors of Annual Reviews, Inc., and since 1995 its Chair. The Annual Reviews offer collections of critical reviews written by leading scientists in 46 disciplines. It is this experience that I am borrowing from in writing this article and I wish to thank Editor-in-Chief Emeritus Sam Gubins for discussions on this topic.
- [3] <http://scholarlykitchen.sspnet.org/2015/06/17/the-problems-with-credit-for-peer-review/>.
- [4] <http://www.wsj.com/articles/how-many-scientists-does-it-take-to-write-a-paper-apparently-thousands-1439169200>.