Beyond the IF boycott: Let's think about counter-incentives against illegitimate co-authorship

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Comment on: **D Shaw** (June 2014)

The troublesome issue that illegitimate (guest, honorary, ghost) authorship poses a significant challenge for the integrity of science and that the contemporary usage of journals' Impact Factors (IFs) has a corrupting effect on publication practices are adequately emphasized by David Shaw's paper in this journal [1]. However, his suggested remedy is not a cure, in my view, but just a partial treatment of symptoms.

Throughout his discussion, Shaw fails to differentiate the problem of authorship from the problem of IF usage. He obscures how IF is calculated and fails to fully understand why authorship guidelines are not followed. The inappropriate usage of IF is not the only factor that inhibits researchers from following authorship guidelines. As a conclusion, Shaw suggests that if universities, funding agencies and researchers could reach a global consensus to avoid using IFs to evaluate individual or institutional performance, scientists could escape this situation and hence could do the right thing when following authorship guidelines. Although the suggested boycott of the IF would be an advantageous development for many other reasons, I believe it would have very little effect on decreasing the prevalence of illegitimate authorship practices in science.

The premise of boycotting the use of IFs for the evaluation of individual scientists is itself sensible, as the IF was originally developed to roughly compare the

'influence' of different scientific journals. As such, the IF is a valuable tool for librarians or administrators who must decide which journals to order for the library, for example. However, even the use of the IF for this limited purpose is riddled with problems, as the citation database itself and the algorithm used to make the calculation suffer from numerous biases [2], including a preference for English language journals, specificities of the algorithm, distribution of citations among journals, online availability of journals, citations on retracted publications, negative citations, publication delay, editorial pressure on journal self-citation and disciplinary variation in citation customs [3].

In Shaw's boycott scenario, it is certainly true that scientists would be liberated from caring about the IFs of the journals in which they publish. Research institutions would not focus on IF when evaluating their employees' performances, and grant agencies would not require applicants to provide data about their IF scores, and so on. Would this significantly change the current situation? In my view, it would have only a limited effect because authors would still be evaluated on their publication merits—albeit in a more granular fashion—specifically on the quality and quantity of papers they produce.

As such, even if we replace the IF with one of the many measures proposed elsewhere to evaluate the impact of the work of an individual scientist—in this case, let's call it the Q factor—scientists will still be

driven to enhance their Q score, which would be based on both the quality and quantity of publications, rather than their IF. But how would this new measure have any effect on decisions about who should be listed as an author in a given publication? Given that a long author list has little impact on the prestige attributed to the first and corresponding authors on a paper, the first and corresponding authors would still permit researchers who have not made a significant contribution to the work to become co-authors on their high-Q papers, if it proved politically convenient or unavoidable. Other than uneasy feelings, the presence of excess baggage on the author list is of little detriment to the real authors.

The solution to this particular problem, therefore, is not to do away with IF—though we should do that for other very good reasons entirely—but rather to divide the prestige equally between all the authors on a paper—so a longer list results in less prestige per person—or to explicitly capture how much prestige each author should receive. As others have suggested, one way of doing the former would be to divide a given paper's IF by the number of authors who published the paper, while achieving the latter would require authors to agree to a percentage-based distribution of credit based on their actual contributions [4].

At present, if researchers decide to involve many authors who did not make a real contribution to the research or its publication, there is no real downside for them:

everyone gets credit. It is more or less similar to a win-win scenario, except that some of the authors might feel uncomfortable that they failed to follow the guidelines that some journals provide, but rarely police. Thus, the current system neither really inhibits the inclusion of inappropriate authors, nor rewards their exclusion. This is coupled with increasing competition to get one's name on as many papers as possible, resulting in inappropriate requests to be included and tricky situations where one is obliged to do favours in terms of authorship. Thus, we need to find ways to discourage researchers from limiting authorship lists on papers (by dividing credit equally among all authors), without unfairly penalizing those papers that are the result of genuine large-scale,

multi-discipline collaboration (perhaps by optionally including author-agreed credit apportioning).

Some readers might be sceptical about the feasibility of dividing credit equally, or of ever being able to achieve an agreed split for a paper. Some others might feel disadvantaged that in their own field, multi-author research is the norm (like high-energy physics), whereas in others, single authors are the rule (like mathematics). We must find rules that are fair to everyone. We are desperately in need of appropriate incentives and counter-incentives to tidy up the authorship on scientific research papers. It's time to think seriously about the feasibility of changing the culture of authorship in the life sciences.

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