

Clash of Confidence and Responsibility in Scientific Publishing

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Abstract

Scientific publishing is a highly responsible enterprise that involves shared responsibilities between the authors and the publisher. It is based on mutual trust and on the principles of respect of freedom of expression of ideas. The author is responsible for the content of the article and for the truthfulness of the affirmations while the publisher verifies the formal coherency of the articles and is seldom engaged in the verification of the truthfulness of the original content. Publishing bad science is damaging to the scientific community and society as a whole. It has been shown that there are scientific journals that publish without much control over the form and content of the papers. Such journals usually have low impact on the scientific community. Damage that is made by publishing bad papers and bad research in an unimportant journal is small in comparison to the damage that may be and is often produced if the article is well written but contains trivial results and unsound conclusions and yet is published in a journal of high reputation. Some measures are proposed that could, by improving the reviewing procedure, also affect the quality of the publishing of science. It would also help if, when judging the scientific value of an article, the scientific community were to pay less attention to the fame of a journal or to various quality indicators, but consider more directly the quality of the research itself.

From immoral means to moral ends - Should we accept to use immoral means to achieve moral ends? Depends how high the stakes are.

Scientific publishing is an adventurous and highly responsible enterprise. The scientists who publish frequently have been aware for some time that there are scientific journals that publish without much control over the content of the papers they publish.¹ We think that the clash between confidence and responsibility in scientific publishing has its roots deep in our concept of science and scientific practice.

We shall first refer to three important principles that are relevant to the publishing of scientific matter and then refer to the mentioned paper¹ to elaborate more on some related issues. The *first principle* is: the author is responsible for the content of the article. What this means can be seen from the example of the procedure that is normally employed by the top journals. The article may be routinely and meticulously examined and still that which would not be really verified is whether the data presented are true. Let us insist on this: no scientific journal practically ever verifies whether the most essential new data are true. A *second important principle* must be added: in scientific publishing the whole enterprise is based on trust, on confidence. Otherwise it would have been impossible to “produce” science with such intensity as has been the case during the last and the preceding century. The

promotion of science reposes on confidence and shared responsibility between author and publisher. Finally, the *third principle* that would determine how we go about scientific publication is the general principle of the respect of freedom. This would imply freedom to speak, to write and publish almost all that one pleases as long as this does not reach into the domain of the rights and freedoms of other people. This is so general a rule that the law sanctions the acts that would limit these freedoms. There is no need to give examples here of the publications which, in spite of our not approving of them, in order to satisfy the principles of freedom, are permitted to be published. It is similar with some “scientific” publications, which we may consider to be useless or not at the highest scientific level.

And, before we come back to the issue raised by Bohannon¹, let us introduce another “category” of scientific writing: scientific fraud. It will be of relevance for this discussion to distinguish levels of scientific fraud, such as “harmless” minimal interventions, sometimes even justified, to data manipulation, or even the invention of data and, lastly, massive fraudulent manipulations of various kinds. What also damages science even more often, is bad reviewing since *publish or perish* fever pushes numbers of published journals to astronomic heights, resulting in a serious shortage of good reviewers, which also affects “big” journals. Such articles will in all probability be well presented, published - and then believed. This has been

demonstrated by the case of Boldt^{2,3} the "affaire" Sokal⁴, and by a number of other similar scandals.

Bohannon's procedure consisted in both, bad presentation and fraudulent data. What the "sting" procedure tested was in fact for negligence. Indeed, it demonstrated the bad quality of some journals and faulty routines in journal editing. But the study falls short of its aim, since that which is most harmful in scientific publications is not shabbily presented results. The sting-like papers, i.e. badly written papers, with methodological flaws, with incorrect presentations and conclusions that do not follow from the investigation, are often not so hard to spot and although harmful in a way are a banality. On the contrary, well written papers, with correct methodology, good presentation and intelligent interpretation of the results, if based on insincere or fraudulent data (which will never be verified) are not a banality. Damage that is made by publishing bad papers and bad research in an unimportant journal is small in comparison to the gigantic damage (exceeding millions of dollars) that is produced if the article is well written but contains trivial results and unsound conclusions and yet is published in a journal of high reputation.

Many journals, and in particular the open access journals (OA), have defective reviewing and in the above mentioned text Bohannon described his "study", in *Science*, classifying it as "news", where he tried to prove this. It was more or less known that many journals have defective reviewing procedures and we think that the "study" was not justified. We admire the perseverance and well elaborated systematic "sting procedure" of the author who revealed his experiences with a number of such journals. Yet the author's method was at the very least bizarre, in spite of its pointing out that Pandora's box has been opened. His method consisted in sending to the "victim" journals (OA journals) variations of an typical article (described as "bait") that all contained serious flaws, as he writes: bad controls for the experimental groups, wrong legend for one graph, and non idiomatic English. In addition, he continues, the conclusion contained unjustified and therefore "fallacious" recommendations.

What Bohannon did by sending fictitious articles to the 304 OA journals that remained in the false belief of having received genuine reports by dedicated researchers about their many months of scientific efforts, was strange because such acts by Bohannon just did not belong to science, being simply offensive. The intention was fine, the question that was asked was good, the method to look for the answer, appalling. In spite of the withdrawal at the last moment, to

send them was morally wrong. It would be certainly impossible not to condemn, on deontological grounds, this act or to try to defend it by employing utilitarian arguments, by using the principle of double effect or some other similar justification, simply because the effects of this observational report will certainly be null. And, nevertheless, it was long known that a number of journals had problematic reviewing procedure and other morally acceptable methods could have been employed to warn such journals.

Astonishingly, if we examined the very article by Bohannon published in *Science*, what we find in fact is that Bohannon's article published in *Science* has itself similar weaknesses as the "bait" article, i.e. there is no randomization of his "experimental group" (the OA journals analyzed were not taken randomly but chosen by him), there are no controls at all (he could have compared the OA journals to the ordinary big journals, for example), there was elimination of non-responders and there was no application of the intention to treat principle in the analysis. Then, even worse: except for some descriptive statistics, there were no inferential statistics at all and no references at all. Finally, the objections of Bohannon about conclusions in his "bait" articles are simply not true. He characterized his "bait" as follows:

"The paper's final statement should chill any reviewer who reads that far. 'In the next step, we will prove that molecule X is effective against cancer in animal and human. We conclude that molecule X is a promising new drug for the combined-modality treatment of cancer. If the scientific errors aren't motivation enough to reject the paper, its apparent advocacy of bypassing clinical trials certainly should be."

Well, as we understand this, the fictitious "author" (Bohannon) in reality mentioned the studies that had to be conducted, should be done "in animal and human", which suggests only implicitly though nevertheless, clinical studies! Why should he then expect a "good" reviewer to object to the conclusion? We do not know a reviewer who would take such a conclusion to be a sufficient reason for a rejection of a paper. Instead, the authors would normally be asked to make some reservations in the conclusion.

The "bait" article of Bohannon was withdrawn from OA journals ("victims"), so unfortunately we do not know what type of article it would have been if published by the "victim" journal. Maybe some journals would have published the text as "news" or even as a pilot study,

short report, special report, just because of interesting findings or something else. Interestingly, Science was sufficiently clever to have published Bohannon's article as "news", which apparently liberated it from the obligation to apply scientific method. And, what a surprise, in a discussions that followed its publication in Science - it IS interpreted not as a simple interesting news item but as a scientific study!

Imposing draconian restrictions on some journals, what some colleagues suggested in discussions that followed this Science publication, to forbid publishing or introduce sanctions, to sanction even the scientists that publish in these journals, is of course inappropriate. Improving the reviewing procedure would certainly improve on the quality of the published science. It is possible that the measures that would introduce the lists of certified reviewers or some mechanisms of feedback that would evaluate the reviewing quality, could improve the reviewing procedure. It would also help if, when judging the scientific value of an article, the scientific community would pay less attention to the fame of a journal, impact factor, h-index or similar, but consider more directly the quality of the research itself. It is an obligation of the scientific community itself to correctly evaluate scientific papers when they are published. It is also true that before the articles were printed, the journals could demand to see the raw data. This could introduce some security against scientific misconduct. Nevertheless, to look for the ways that will permit exhaustive and complete evaluation of all scientific papers remains a romantic dream far away from reality.

The first Macedonian to be an Egyptian pharaoh, Ptolemy I Soter (c. 367 BC – c. 283 BC) asked for a fast way to learn geometry, however the famous mathematician Euclid apparently replied that, "there was no royal road to geometry."⁵ A similar response to the romantic hope about achieving any kind of perfection in evaluating scientific papers seems to be appropriate. Some grey zones will always remain.

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