

RELEVANCE IN SCIENCE

Writing research proposals recently, I came across the familiar elements that the national funding agencies in the UK (and probably everywhere else) ask to be included in the application: relevance, expected results and possible risks. We understand why they are there and generally agree that it is not altogether bad. However, I believe that the trend to give increasing importance to such demands when providing funds for research may be detrimental to science and produce *less relevant* information. If we are asked to provide an outline of the results of our research before we can do it, will we not move into well-trodden paths, where we can safely predict the outcomes and where the project application reviewers will easily agree with us? Obviously, both the risks and the information provided by the project are reduced. The need to study relevant issues produces the same effect, as it is easier to find a link with a social problem in scientific questions that have already been well studied.

We recently celebrated the 50th anniversary of the discovery of DNA. I wonder what Lawrence Bragg, then Head of the Cavendish Laboratory, would have needed to write in a modern project proposal to obtain funds for the projects in which Watson and Crick would work. For example, "Risks: the postdoctoral research associate (Watson) or PhD student (Crick) may neglect their respective research projects and engage in some other study of their fancy (as they did). Expected results: they may end up discovering something of far greater importance. Relevance: ..." The relevance of their discovery needs no comment. I suppose that a modern funding agency would have banned Bragg from receiving further funds as he proved to be such a poor project manager.

This is only one example, but it is a paradigm of how science works. Science is a mental activity more than a commercial operation, and scientific discovery is not driven by relevance but by curiosity. Scientists are stimulated to pursue answers to scientific questions by the desire to know, and not by the desire to solve socio-economic problems. All of us have colleagues who admit that they engage in some scientific projects so as to have the resources to work on the projects that really interest them. I believe that funding bodies should maintain a strong financial support of curiosity-driven science, as this type of research has produced and will continue to produce the important scientific quantum leaps and the *most relevant* information.

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ABOUT THE REVIEW PROCESS

I have just finished reading your thoughtful "Triple Point" editorial in the October issue of *Elements*. I am now retired from the Smithsonian, but I would like to tell you how I dealt with the problem of anonymity in the peer-review process for over 40 years. Early in my scientific career, whenever I was given the option, I always signed my reviews. My reasoning was that if I hid behind the veil of anonymity I could easily make "pot-shot" criticisms. However, if I signed my review I would have to keep it fair and balanced...

I have made it my personal policy always to write a separate letter to every author whose paper I review. I tell the author that I am writing simply to inform him/her that I have been asked by the editors to referee the paper. I further state that if he/she did not receive a copy of my signed review from the editors, or wished to discuss some aspect of it with me, they should feel free to contact me. This has allowed me to be as objective and respectful as possible, whether critical or not, and it has avoided the usual speculation on the part of the author as to who the critic might be. After hundreds of reviews I have found this approach to be very satisfying.

As a former editor or associate editor myself (*Clays and Clay Minerals*; *Journal of Foraminiferal Research*; *American Mineralogist*), I was able to see how authors and referees treated one another. I concluded that the signed reviews were the best and most respectful, but I also noted that usually they were signed only when the paper was deemed worthy or there were only minor suggestions and criticisms. Referees recommending outright rejection rarely wanted their names added.

Clearly, there is no simple solution to the problem. For me, however, making myself known to the author has forced me to tone it down on occasion and be sure of my position. I consider open review "vital to the integrity" of my system. The scientific enterprise is not seriously threatened by the open review. It is dogmatists who don't have the courage of their convictions to stand behind their criticisms who are threatened!

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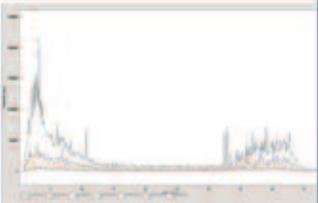
The review process is best when done openly, as a conversation amongst the author, reviewers and editor. The goal of this conversation is to create the best possible science, written to the best possible standards of clarity and quality. Obviously, this conversation must examine the merits of the science and the presentation in a thorough and, yes, critical fashion. I looked up "criticism," and found that only one of the six definitions is a thoroughly negative act. If one denies that both the capacity to evaluate a piece of science critically and an ability to respond to such criticism are part of the scientific enterprise, one doesn't fully understand science... Discourse has been a part of science since Aristotle, but effective discourse cannot take place when everyone is hiding behind a mask!

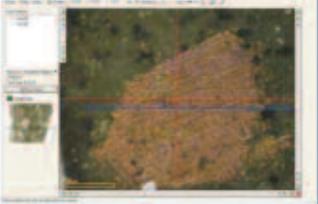
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