Ethics and Peer Review

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Throughout most of its recent history, science was constrained only by the limits of imagination and creativity of its participants. In the past couple of decades that state of affairs has changed dramatically. Science is now constrained primarily by the number of research posts and the amount of research funds available. What had always previously been a purely intellectual competition has now become an intense competition for scarce resources. This change, which is permanent and irreversible, is likely to have an undesirable effect, in the long run, on ethical behavior among scientists. Instances of scientific fraud are almost sure to become more common, but so are other forms of scientific misbehavior. For example, the institution of peer review is now in critical danger.

Peer review is used by scientific journals to decide what to publish, and by granting agencies to decide what research to support. Obviously, sound decisions on what to publish and what research to support are crucially important to the proper functioning of science. Journal editors usually send manuscripts submitted to them to referees who will remain anonymous to the authors of the manuscript. Funding agencies sometimes do the same, especially for small projects, and sometimes instead assemble panels of referees to judge proposals for large projects.

Peer review is quite a good way to identify valid science. It was wonderfully well suited to an earlier era when progress in science was limited only by the number of good ideas available. Peer review is not at all well suited, however, to adjudicate an intense competition for scarce resources, such as research funds or pages in prestigious journals. The reason is obvious enough. The referee, who is always among the few genuine experts in the field, has an obvious conflict of interest. It would take impossibly high ethical standards for referees to fail to use their privileged anonymity to their own advantage. But as time goes on, more and more referees have their ethical standards eroded by receiving unfair reviews when they are authors. Thus the whole system of peer review is in peril.

Editors of scientific journals and program officers at the funding agencies have the most to gain from peer review, and they steadfastly refuse to believe that anything might be wrong with the system. Their jobs are made easier because they never have to take responsibility for decisions. They are also never called to account for their choice of referees, who in any case always have the proper credentials. Since the referees perform a professional service, almost always without pay, the primary responsibility of the editor or program officer is to protect the referee. Thus referees are never called to account for what they write in their reviews. As a result, referees are able, with relative impunity, to delay or deny funding or publication to their rivals. When misconduct of this kind occurs, it is the referee who is guilty, but it is the editors and program officers who are responsible for propagating a corrupt system that makes misconduct almost inevitable.

This kind of misconduct, I fear, is rampant in all fields of science. Recently, as part of a talk to a large audience of mostly young biomedical researchers at an extremely prestigious university, I outlined this analysis of the crisis of peer review. The moderator, a famous senior scientist, was incredulous. He asked the audience how many disagreed with my heresy. No one responded. Then he asked how many agreed. Every hand in the house went up. Many of us, in my generation, wish to believe that nothing important has changed in the way we conduct the business of doing science. We are wrong. Business as usual is no longer a real option for how we conduct the enterprise.

I think we scientists are also guilty of promoting, or at least tolerating, a false popular image of ourselves that may be flattering but that, in the long run, leads to real difficulties when the public finds out that our behavior doesn't match that image. I like to call it "the myth of the noble scientist." It arises, I think, out of the long-discredited Baconian view of the scientist as a disinterested seeker of truth, gathering facts with mind cleansed of prejudices and preconceptions. Thus the ideal scientist would be more honest than ordinary mortals, certainly immune to such common human failings as pride or personal ambition. When it turns out, as invariably it does, that scientists are not at all like that, the public that we have misled may react with understandable anger or disappointment.

The fact is that scientists are usually rigorously honest about the things that really matter to them, such as the accurate reporting of procedures and data. In other arenas, such as disputes over priority or credit, they tend to behave like the ordinary mortals they are. Furthermore, scientists are not disinterested truth-seekers, they are more like players in an intense, winner-take-all competition for scientific prestige, or perhaps merchants in a no-holds barred marketplace of ideas. The sooner we learn to admit to those facts, and to distinguish carefully between serious scientific misconduct and common human conduct by scientists, the better off we'll all be. ///