NON-OBJECTIVITY IN EVALUATING SCIENCE AND ITS IMPLICATIONS FOR OBJECTIVITY IN SCIENCE

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Peer review has a prominent position in modern science: it serves as the filter for which ideas are allowed to enter the international discussion forum, and which ideas are not. Peer review is supposed to be an objective evaluation of the scientific quality of a work, but unfortunately, that is not always the case: with the advent of peer review, two new forms of charlatanry have silently crept into modern science. In the first place, this concerns passing off fabricated negative conclusions about someone else's work as the genuine findings of a serious evaluation of the scientific quality of the work. In the second place, this concerns abusing one's discretion to officially decide on a formal complaint about such fabrications by dismissing it as baseless in the face of material evidence to the contrary. Recently (*Sci. Eng. Ethics* **20**, 41-45, 2014) it has been proposed to start viewing these two forms of charlatanry as forms of *type II scientific misconduct*, where the qualification 'type II' indicates that the misconduct leads to falsely negative conclusions about someone else's work—as opposed to the qualification 'type I', which indicates that the misconduct leads to falsely positive conclusions about one's own work.

In this talk[1] I would like to focus on this first-mentioned form of type II scientific misconduct, concerning the fabrication of negative conclusions about someone else's work, and in particular I would like to focus on the implications for objectivity in science. The purpose is to create awareness for the magnitude of the problem posed by the existence of this misconduct.

Of course, when evaluating a work there can be good reasons to conclude that the work is of insufficient scientific quality. For example in theoretical physics, any new theory should *at the very least* meet criteria of conceptual coherence, logical consistency, mathematical rigor, and experimental testability: so if a work describes a new concept both as a substance and as a property, then that signals a conceptual incoherence that *genuinely* renders the work of insufficient quality. So the question is: how to discern type II scientific misconduct from genuine negative conclusions in peer review? For the criteria of demarcation we have to look at the Radbuch formula in the philosophy of law: any law, in which not even an attempt is made to respect basic human rights, is not a law at all. In analogy to that, we get the following rule: *an evaluation of a work, in which not even an attempt is made to respect basic principles of good scientific practice, is not an evaluation of the scientific quality of the work at all—it is type II scientific misconduct.* This rule doesn't qualify an honest mistake as misconduct, and leaves enough room for fierce scientific discussion.

An implication of this form of type II scientific misconduct is that valuable output of innovative developments doesn't get past peer review. Of particular importance in this context is the difference between *pseudoscience* and *unorthodox protoscience*, the latter meeting all criteria of scientific quality but lacking experimental back up. All too often, a non-objective evaluation of unorthodox protoscientific work results in a dismissal as pseudoscience; certainly in physics, peer review has deteriorated from a *quality check* to a *conformity check*. Suppressing fundamentally new developments unduly enlarges the *perceived* importance of the dominant research programs: alternative ideas then exist but remain unseen because they are not published. There is then a real danger of a global scientific inbreeding, that is, a real danger that generations of researchers remain locked within a preexisting research program.

[1] The talk has never been held: this paper is merely the submitted conference abstract.

An equally obvious implication of this form of type II scientific misconduct is that valuable criticism is not allowed to the international discussion forum. Of particular importance in this context is criticism of the biggest claims that virtually shape the future of research in an area. All too often, the result of a non-objective evaluation of a critical work is that it is dismissed as “not interesting”, which indicates that what is widely held is a Berkelian idealism with respect to criticism: a criticism not published is a criticism that doesn’t exist. Consequently, as the degree of exaggeration in overstated claims is withheld from the community, a breeding ground is formed for the development of pseudoknowledge. But not only that: there is also the real danger that what prevails in the community is a collective delusion about the successes of the dominant research program(s). Such developments seriously comprise objectivity in science.

A third implication of the widespread abundance of this form of type II scientific misconduct is the non-possibility for self-realization in the academic world for anyone whose intended research is not in line with the dominant research program. When one’s unorthodox or critical papers are impossible or more difficult to publish, then one’s chances for an academic career in the present ‘publish-or-perish!’ world are slim to none. The consequence thereof for the community at large is that the previous effects are enlarged: of course, the position of the dominant research program is further reinforced and its output is less likely to be publicly criticized, when there are less and less unorthodox or critical ideas produced.

Concluding, non-objectivity in evaluating science certainly has implications for objectivity in science. This may also have repercussion for science policy. The point is, namely, that science policy decisions are ultimately based on a perception of importance by the policy makers: if a widespread abundance of type II scientific misconduct in the back doors of institutions yields a distorted picture of the scientific landscape, then of course that has an effect on the decisions made. To tackle the problem, it is not enough to give a course in ethics: the entire scientific attitude has to change. Ideally, both unorthodox and critical ideas should be evaluated *objectively*; a concrete measure might be to change the standard peer review process by expanding it into one full round of discussion **with** input of the author. To be effective, that demands, however, brutal honesty from referee, editor, *and author*.