Sci Eng Ethics
DOI 10.1007/s11948-013-9433-8

ORIGINAL PAPER

Scientific Misconduct: Three Forms that Directly Harm Others as the Modus Operandi of Mill's Tyranny of the Prevailing Opinion

Marcoen J. T. F. Cabbolet

Received: 11 September 2012 / Accepted: 3 February 2013

Abstract Scientific misconduct is usually assumed to be self-serving. This paper, however, proposes to distinguish between two types of scientific misconduct: 'type one scientific misconduct' is self-serving and leads to falsely positive conclusions about one's own work, while 'type two scientific misconduct' is other-harming and leads to falsely negative conclusions about someone else's work. The focus is then on the latter type, and three known issues are identified as specific forms of such scientific misconduct: biased quality assessment, smear, and officially condoning scientific misconduct. These concern the improper ways how challenges of the prevailing opinion are thwarted in the modern world. The central issue is pseudo-skepticism: uttering negative conclusions about someone else's work that are downright false. It is argued that this may be an emotional response, rather than a calculated strategic action. Recommendations for educative and punitive measures are given to prevent and to deal with these three forms of scientific misconduct.

Keywords Scientific misconduct · Discourse ethics · Pseudoskepticism · Biased refereeing · Smear · Integrity committees

Introduction

In the discussion on scientific misconduct, the emphasis has thus far always been on those forms of scientific misconduct that benefit the perpetrator. This concerns malpractices like plagiarism, the fabrication of data, and the falsification of credentials. Below some (locally) well-known examples are given to illustrate the scope of the discussion.

M. J. T. F. Cabbolet (✉)
Center for Logic and Philosophy of Science, Vrije Universiteit Brussel, Pleinlaan 2, 1050 Brussels, Belgium
e-mail: marcoen.cabbolet@vub.ac.be

Published online: 19 February 2013

Example 1 In the Netherlands, it surfaced in 2011 that publications of the psychologist Diederik Stapel contained data that were fabricated. His work was then put under scrutiny, which yielded the conclusion that 55 out of his 130 journal papers and 10 out of his 24 book chapters were certain to be fraudulent, while on another 10 journal papers a strong suspicion of fraud rested (ANP 2012); Stapel admitted to scientific fraud, renounced his PhD degree and resigned as a professor at Tilburg University.

Example 2 In Germany, it became known in 2011 that Karl-Theodor zu Guttenberg, at the time Minister of Defense, had copied large parts of existing publications into his 2007 dissertation without giving proper references; although he didn't admit to intentional deceit, Zu Gutenberg was stripped of his PhD degree because of this violation of the principle of carefulness (Löwenstein and Müller 2011). He then resigned within weeks from his post as Minister of Defense.

Example 3 In 2007, it turned out in the USA that the Dean of Admissions of Massachussets Institute of Technology, Marilee Jones, had misrepresented her academic degrees when she first applied to MIT years ago. The affaire led to her resignation (Mytelka 2007).

It is, however, not at all the case that nothing is wrong in science circles if we leave out the cases of self-serving misconduct such as the above examples. The point is, namely, that those who come up with results that are critical of the prevailing paradigm or that introduce an altogether entirely new way of thinking are not exactly greeted with open arms by the science community. It is emphasized that this is not a complaint about the inherently conservative nature of science: healthy skepticism plays an important role in science in that it may point out in which aspects a new idea is not convincing; an attempt will then be made to solve the issue, but of course the problem may also turn out to be unsolvable—eventually, science advances by terminating degenerating research programs and adopting the views laid down in the progressive ones. But as the following examples show, there are some recurring issues in science that have nothing to do with finding out the truth about something.

Example 4 The widely used physics preprint server arxiv.org is not peer-reviewed, but there is some moderation. The (anonymous) moderators require some papers to be endorsed in writing by others than the authors. In 2004, Paul LaViolette uploaded a paper with a new idea on the solar system to arxiv.org, but he received a reply that an endorsement was required. However, even after LaViolette got an endorsement from another author who was registered as an endorser, his paper still was not published on arxiv.org—even though it was already accepted for publication in a journal, cf. (LaViolette 2005).

Example 5 In 2011, the physicist Joy Christian uploaded a paper to the preprint server arxiv.org, in which he believed he had found a disproof of the famous theorem of Bell, cf. (Christian 2011). Within weeks, he was called a "crackpot" in the media by numerous professional physicists, amongst whom were Vongehr (2011), Aaronson (2012), and Motl (2012).

Example 6 In 2008, Eindhoven University of Technology (TU/e) canceled the defense of this author's unorthodox thesis, after the Dean of the faculty in question, Kees Van Hee, had lobbied the administration with his conclusions of a self-initiated *reevaluation*: he had asked four close colleagues to review the already approved work, but omitted any discussion of the negative outcome. In the aftermath, numerous scientists like Lambert (2008), Post (2008), and 't Hooft (2008a, b, c) attacked the work and those involved in the media, and the TU/e maintained the PhD cancelation *even after* its Committee for Scientific Integrity had established that 11 scientists of the TU/e had violated scientific ethics in the process[1] (ANP 2008). The entire affair was then brought before the National Committee for Scientific Integrity (LOWI), which by contrast ruled that the complaint about the PhD cancelation and the attacks in the media was baseless (2009). Yet by 2011, virtually the same thesis had been defended at the Vrije Universiteit Brussel and the main result had appeared in a reputable journal, cf. (Cabbolet 2010, 2011).

The above Examples 4–6 add to the many historical cases involving famous scientists and philosophers, such as Descartes' work being declared an *anathema* by successively Leiden University (1642) and Utrecht University (1647); the theological hate campaign at the end of the seventeenth century following the publication of Spinoza's work; the 1931 book "100 authors against Einstein", the inadequate response of the academic community to Einstein's theory of relativity. These cases highlight issues that concern what John Stuart Mill in his 1859 book *On Liberty* called "the tyranny of the prevailing opinion". These issues are well known. A substantial amount of research on the various forms of attacks on dissenters has already been done, most notably by Brian Martin; in his 2004 paper, coauthored with Juan Miguel Campanario, the following conclusion is reached, cf. (Campanario and Martin 2004):

> A proponent of an unorthodox idea is likely to encounter several types of difficulties. First, it is difficult to obtain funding. (...) Second, it is difficult to publish in mainstream journals. Third, [they] may come under attack; their colleagues may shun them, they may be blocked from jobs or promotions, lab space may be withdrawn and malicious rumors spread about them.

This gives a clear picture of the *effects* of the tyranny of the prevailing opinion that will be observed *at the receiving end*. This essay doesn't purport to add anything to that: the main purpose is to lay bare the behavior *at the perpetrator's end*, so that the difficulties reported by Campanario and Martin are but a result thereof. Of course, an author does not have the right to publish in a particular journal, nor should the fact that an idea is new guarantee any funding: there can be sound arguments to deny publication and funding, but the focus is here on the denying of such as a result of behavior that has nothing to do with healthy skepticism or a conservative attitude, the same way pseudoscience has nothing to do with science. The general idea is then to start viewing this kind of behavior henceforth as *scientific misconduct*. For that

[1] The TU/e thus stood by the conclusions of Van Hee's quality assessment, even after it had been independently established that this was unsound.

matter, it is necessary to introduce a distinction between two types of scientific misconduct: *type one scientific misconduct* leads to falsely positive conclusions about one's own research; *type two scientific misconduct* leads to falsely negative conclusions about someone else's research. This terminology is derived from the concepts of a 'type one error' and a 'type two error' that are commonly associated with false positives and false negatives in statistics and other fields of science; e.g. in a 1989 colloquium, Truzzi (1990) mentioned two types of scientific mistakes: a "type one error" occurs when one thinks one has discovered something special while nothing special has happened, and a "type two error" is when one thinks nothing special has happened while in fact something special did happen. Of course, the two types of scientific misconduct can also be called 'self-serving' and 'other-harming'.

The remainder of this essay is organized as follows. Section "Identification of Three Forms of Type Two Scientific Misconduct and Their Tell-Tale Signs" identifies the three forms of type two scientific misconduct that are the ways by which the tyranny of the prevailing opinion works—its *modus operandi*; Section "Discussion" discusses how type two scientific misconduct might arise, and what can be done against it.

Identification of Three Forms of Type Two Scientific Misconduct and Their Tell-Tale Signs

Biased Quality Assessment

In science, the quality evaluation process plays a central role. In fact, the trustworthiness of the quality evaluation process is one of the pillars on which the entire notion of 'scientific quality' rests. And this notion of scientific quality serves as a filter in the allocation of research grants and the publication of research results: a work of insufficient scientific quality will not be funded nor be published. Those who get to decide on the scientific quality of a work, i.e. those that participate in a quality evaluation process, are thus in a position of power. And that makes this position open to abuse.

Ideally, a quality assessment is a finite Habermasian discourse in which at least three parties are involved:

- the research group, whose work is subjected to a quality assessment;
- at least one reviewing party, who after investigation puts forward *substantiated* conclusions about the scientific quality of the work;
- a presiding party, who initiated the quality assessment, who moderates the discourse, and who draws the end conclusions.

So ideally, the involved parties (e.g. a journal editor, an independent referee, *and* the author of a paper) *unanimously* reach a conclusion about the scientific quality of the work under consideration. If the conclusion is that the work is of insufficient scientific quality, then the ethics of scientific discourse thus require that clear reasons with appropriate references be provided to justify any claims that impugn

either the methods, data or conclusions of the work under consideration. This author holds that the first form of type two scientific misconduct is then this: *falsely and with blatant disregard for the ethics of scientific discourse reaching the conclusion that a work is of insufficient scientific quality*. As a result, researchers may experience rejection of submitted papers and denial of research funding; these are among the to-be-expected difficulties mentioned in (Campanario and Martin 2004). To indicate how serious this is, it suffices to quote the Nobel laureate Schwinger (1991): "the replacement of impartial reviewing by censorship will be the death of science". An indication that this already in the 1950s occurred on a large scale is from Schweber (1989), who wrote the following about leading theorists in high-energy physics in the 1950s: "in their capacity as reviewers of research proposals, and by virtue of their dominance in the funding process, they tended to reinforce their dominant view".

The point is this: the fact that a work has *formally* undergone a procedure for quality assessment doesn't necessarily make that procedure *really* an evaluation of the quality of the work in question. Compare the theory of law: the fact that a proposal for a law has undergone a procedure for approval doesn't necessarily make it a positive law—there is the Radbruch formula, which asserts that any law, in which not even an attempt is made to adhere to basic human rights, is not a law at all. Now one should always leave room for discussion, but let us, based on the Radbruch formula, agree on this rule: *a procedure for quality evaluation, in which not even an attempt is made to adhere to principles of good scientific practice, is not an evaluation of the quality of a work at all—it is type two scientific misconduct*.

The above rule raises the question *when* it can be said that no such attempt has been made; with regard to that, two certain tell-tale signs can be mentioned that are evidence of a blatant disregard for the ethics of scientific discourse:

1. The first tell-tale sign is such a lack of substance in the report(s) of the reviewing party that the term *pseudoskepticism*, as defined by Truzzi, applies: uttering negative conclusions about someone else's work without satisfying the burden of proof that these conclusions require,[2] cf. (Truzzi 1987). As said, there should always be room for discussion, but the crux is that pseudoskepticism always concerns a gross violation of the standard of scientific discussion, so gross that one doesn't have to be an expert in the relevant field to see that it has nothing to do with scientific argumentation. For instance in the affair of Example 6, Beenakker (2008), who was asked by the LOWI to judge the scientific quality of the thesis, used the fact that the results at the time had not been published in a journal article as an argument for his conclusion that the work was not admissible as a PhD thesis, while such is not a requirement in the Netherlands: that is not a scientific argumentation, and any professional scientist understands that.
2. The second tell-tale sign is strategic action of the presiding party by *consciously* refusing to consider the input of certain participants into the discourse. Examples of such strategic action are refusing to consider positive input of

[2] Note that the term 'pseudoskepticism' here has the same connotation as the term 'pseudoscience'.

reviewing parties as in Examples 4 and 6, flat out denying the research group the possibility of reacting to a negative referee report as in Example 6, or by giving it that possibility only formally, that is, without subsequently taking the reaction into consideration (this tactic was followed by the LOWI in Example 6). Such strategic action is evidence that not even an attempt is made to arrive at an end conclusion about the scientific quality of the work by sound argumentation; as Habermas (1991) put it: every speaker knows intuitively, that an alleged argumentation is not a serious argumentation, when for example certain participants are not admitted.

It is then not important where the pseudoskepticism or the strategic action originates from: the point is that it *is* evidence that not even an attempt is made to adhere to the principles of good scientific practice.

Smear

It lies within reason that a scholar should be able to discuss the scientific work of others in the mass media. But the point is that the general public is not capable of verifying the claims made in such a discussion: although it has been known for centuries that it is a logical fallacy to base one's conclusion on authority, this is precisely what the general public does—the general public will accept conclusions about a work as true if these are put forward by an official such as a university spokesman, or an authority such as a university professor. Those with authoritative power are thus in a position to influence the public opinion by means of the mass media, and that makes this position open to abuse.

The second form of type two scientific misconduct that can be identified is *smear in the public media*. This can take the form of a public pseudoskeptical attack at a work, but also false allegations can be spread about the researcher(s) involved to insinuate that his work cannot possibly be sound. A sophisticated example of the latter is *falsely* claiming in public that the author of a work has compared himself to Newton or Einstein, as Lambert (2008) did in the affair of Example 6: comparing oneself with one of science's greatest is generally seen as a sign of amateurism, so the general public will then immediately get the impression that the work in question is the work of an amateur. Campanario and Martin (2004) already mentioned that anyone who challenges dominant paradigms has to expect that malicious rumors will be spread.

Obviously, smear has no *direct* physical consequences for the one targeted. But Spinoza wrote in the seventeenth century that, following the example set by the Pharisees in the Bible, throughout the ages this publicly discrediting someone's work as despicable has become the number one method to set up people against someone. And more recently, Brown observed that an accumulation of critique can change the rhetorical climate for a work, cf. (Brown 2005). Thus speaking, smear tactics can *indirectly* contribute to all the effects mentioned by Campanario and Martin.

Now, of course, one can have a negative opinion about someone else's work: the fact that one publishes a negative opinion doesn't necessarily make it a smear. There

are, however, a few tell-tale signs that make clear that smear has nothing whatsoever to do with a scientific discourse:

1. it always concerns *false allegations* that more often than not are peppered with strong pejoratives;
2. it always *gravely discredits* someone else's research as despicable;
3. it always involves a *gross violation* of the principle of carefulness, one of the principles of good scientific practice, with regard to checking the correctness of the allegations; more often than not, the allegations are taken straight to the mass media, that is, without any prior discussion with the author(s) of the targeted work.

So, although this form of attack is already known for centuries, from the fact that it is currently rampant it is obvious that this has not been pointed out often enough. Of course one can fight defamation in a court, but the point is that defamation by scientists shouldn't occur at all in the first place. Those that engage in such activity call upon the right to freedom of speech, but they can do so without a job at the university paid by taxpayer's money. As it has nothing whatsoever to do with finding out the truth about anything, the question that the community has to ask itself is this: hasn't the time arrived to flush these charlatans out of academic circles?

Officially Condoning Misconduct

Virtually everywhere, committees for scientific integrity have been set up to decide on complaints about violations of scientific integrity—this includes complaints about type two scientific misconduct. Ideally, the committee critically examines the course of affairs from the perspective of the principles of good scientific practice; e.g. the committee of the TU/e in Example 6 has done so. The problem is, however, that the decision whether or not scientific integrity has been violated in a particular case is *entirely* at the discretion of the committee: metaphorically, such a committee thus has the power to officially decide that a beef is good to eat although it is crawling with maggots. This gives the committees for scientific integrity a means of power that is open to abuse.

The third form of type two scientific misconduct is breach of duty of committees for scientific integrity: *when deciding on a complaint about type two scientific misconduct, officially denying that it has taken place, although it is plain for all to see that this is the case*. It should be realized that these are high-profile cases: if a committee decides that some university professor has committed scientific misconduct, then this is bound to end up in the newspapers—and that will have an effect on the public opinion about the academic world. The integrity committees can thus maintain the illusion that all is well in academic circles by officially denying that misconduct has taken place; interestingly, George Orwell already wrote in his dystopian novel *1984* that with such a means of power the government can maintain an illusion among the general public by officially claiming that an event has not taken place. Martin (1997) called formal channels already "useless" when a proponent of an unorthodox idea files a complaint about being attacked, but

that is an understatement: this third form of type two scientific misconduct is nothing less than an Orwellian abuse of power. When committees for scientific integrity do not appropriately deal with complaints about the two forms of type two scientific misconduct described by the Sects. "Biased Quality Assessment" and "Smear", then these are de facto condoned: that way this third form of type two scientific misconduct creates an academic environment in which the Sadean-libertinistic freedom exists to disregard professional ethics with impunity in the back rooms of academic institutions—and the Examples 4–6 indicate that this situation already exists. One does not need a background in academic ethics to see that this goes against common sense.

As a side note, suppose hypothetically that Dr. Smith knows that his colleague Dr. Williams is thwarting someone else and does nothing about it for fear of hurting the team's reputation; one can then say that Dr. Smith is condoning type two scientific misconduct, and is thus himself committing a form of type two scientific misconduct. But suppose, then, that we apply Kant's categorical imperative and make the following a law: *every scientist is obliged to report scientific misconduct the moment he knows about it*. Such an obligation to report is then difficult, if not impossible, to enforce as it is virtually impossible to prove that a scientist *knew* what was going on. In other words: violations of the law are then virtually impossible to prove. For that reason, the proposal is to limit the discussion to committees for scientific integrity: when deciding on a complaint about type two scientific misconduct, they can hardly say that they knew nothing about it when all the evidence is put right before them.

Discussion

How Type Two Scientific Misconduct Arises

Pseudoskepticism: Martin, who extensively studied the suppression of dissent in modern times, came to the conclusion that there is no such thing as a conscious conspiracy of evil schemers who set out to destroy dissidents: the opposite is the case, in the sense that those who attack dissent sincerely believe that *they* are doing the right thing (Martin 1998). Interestingly, Spinoza made it very clear that people can act in a certain way without realizing *why*, cf. (Beeckman 2009): one is then compelled to accept that scholars can engage in a pseudoskeptical attack without realizing what they are doing.

In his 1998 study, Martin (1998) has established that whenever a scholar is confronted with a new work that challenges the dominant paradigm in which (s)he is an expert, the first emotional reaction is usually to feel contempt for the work or its author. From there it is only one step to pseudoskepticism, and this step can be found in Spinoza's seventeenth century ethics: the scholar then starts to think of bad things that would please him if these could be said about the work or its author, and—regardless whether or not these bad things can indeed be said of the work or its author—this affects him in the sense that he actually starts to mock the new work or its author (Ethics, part III, def. 11). Thus speaking, pseudoskepticism concerns

conclusions that are simply made up—as opposed to being based on a careful evaluation—and as such they have to be treated in the same way as the fabrication of data.

Spinoza made clear that this latter affection, this wanting to mock the work or its author, lasts only temporarily, but the crux is that the pseudoskeptic *gives in to it*. In that context it is interesting to remark that Thomas More wrote in 1516 in *Utopia* that it should be forbidden that a man reacts to a piece on the same day that it is submitted to him, to prevent that he blurts out what first comes to mind and then spends all his energy in defending his own initial reaction under disregard of the common interest.

According to Van Reijen (2009), the Spinozistic remedy for pseudoskepticism is then "not fighting it or talking about it, but understanding it, knowing its cause". Thus, knowledge of the above mechanism might be useful for reducing pseudoskepticism in science: if one recognizes oneself in the mechanism, one might refrain from executing the act of pseudoskepticism.

Manipulation: The scientific documentary "I and others" (Russian title: *ya i drugiye*; USSR, 1971) contains a live recording of a very interesting psychological experiment, which is related to the idea of a self-fulfilling prophecy as defined in (Merton 1968). First, a photograph of a man was shown to a group of randomly selected test persons, who were told he was a convicted criminal and who were then asked to describe the character of the man based on the photo; *all* the test persons responded that certain features of his face (e.g. the eyes) were evidence that this was a very bad man. Second, the same photograph was shown to another group of randomly selected test persons, who were told he was a famous scientist and who were asked the same questions: *all* the test persons then responded that certain features of his face were evidence that this was a very intelligent man. This footage undeniably provides experimental evidence of how people can be manipulated with just a few words, and there is no reason to assume that modern-day scientists cannot be manipulated the same way. That is, in a quality assessment, the presiding party can—affected by emotions as in the preceding paragraph—manipulate the reviewing party by depicting the research group metaphorically as the "convicted criminal" during the first contact: the reviewing party will then start its task with an instigated bad impression of the research group, which by the above mechanism can lead to pseudoskepticism. And this is not just theorizing: Example 6 is a well-documented case.

Group Conformity: The same 1971 scientific documentary "I and others" also contains a live recording of another psychological experiment, similar to the experiments by Solomon Asch reported in (Asch 1951). A single test person was added to a group of ca. ten paid actors, whereafter two similar pyramids, one white and one black, were placed on a table, and the group members were asked one by one which color the pyramids were; all paid actors, who were of course asked first, answered that both pyramids were white. In some (but not all) cases the test person would then respond by saying that both pyramids were white. Upon evaluation, these test persons said that they didn't want to fall outside the group. This footage undeniably provides experimental evidence of how people can make false statements because of group pressure, and there is no reason to assume that this

excludes modern-day scientists. Thus speaking, if several members of a group have already criticized a work, then, even if their criticism is false, this might affect other members of that group in that they also might start criticizing the work out of fear of a conflict with the group.[3] This way, an accumulation of critical material can change the rhetorical climate of a work: group conformity can thus lead to pseudoskepticism and to defamation. The individuals that give in to this group pressure may thus orient their actions more on others, as they choose to conform rather than to report their own opinion.

Condoning Misconduct: Another interesting issue is an observation of Dijkgraaf in the recent Stapel-case of Example 1, widely published in the Netherlands: according to Dijkgraaf (2011), the scientific fraud could continue for years *because* Stapel was seen as a "star researcher" so that it was difficult to publicly doubt him. This one observation might express a reason why integrity committees breach their duty if they are not intentionally protecting a colleague: it might just be the case that what prevails among the members of a committee is the disbelief that the scholar, about whom a complaint is filed, can engage in scientific misconduct. In handling a complaint about type two scientific misconduct, such a disbelief can lead to a "reinterpretation of actions". Here also, Example 6 provides a well-documented case. But the point is this: every member of every ethical committee ought to know that history has already proven that people do not always act in accordance with professional ethics, even if they know what these professional ethics are. In other words: officially deciding that no type two scientific misconduct took place when in fact it did happen *is itself* a form of type two scientific misconduct—regardless whether one is intentionally protecting the reputation of a colleague or acting from the disbelief that the colleague can engage in scientific misconduct.

What Can be Done About Type Two Scientific Misconduct

Broad Education: Of course, scientific misconduct to thwart new developments can only thrive in what Popper (1982) called a "cult of narrowness"—the embarrassing result of narrowing down university educations for decades in a row. There is nothing wrong with efficiently educating students to become good specialists in established research areas, but the university curriculum should nevertheless contain some broad basis. The physicist Lev Landau introduced a "theoretical minimum" in his school of theoretical physics in the 1930s (Landau and Lifschitz 1976), but one can think of a more general such theoretical minimum as a *conditio sine qua non* for a career as a professional scientist. For the technical sciences, this should at least contain some training in formal logic, mathematical logic (the language of mathematics), philosophy of science, as well as a thorough exposition of the historical ideas that have changed human thinking. Moreover, every professional scientist should be aware both of scientific ethics and of its most common violations. The university curriculum should thus provide a course in the basic

[3] From a Spinozistic point of view, scholars—not necessarily group members—might also start criticizing the work in the hope of scoring points among group members, which might yield a better perspective for the future.

principles of good scientific practice, as well as a course in logical fallacies (including an exposition on pseudoskepticism): any professional scientist should be able to recognize the latter immediately, both in other's work and in his own.

Self-reflective Attitude: Furthermore, a preemptive tactic against pseudoskepticism is that scholars practice a self-reflective attitude when judging someone else's work. A broad educational background provides the researcher with the right instruments for Lon Fuller's mental exercise, entailing that the researcher distantiates himself intellectually and emotionally from his work and looks at what he is doing as if he is a complete outsider (Fuller 1981). From such a distant perspective the researcher should be able to discover when he is merely responding emotionally and no longer acting in the interest of science. One can look at one's own comment to someone else's work and ask oneself this question: isn't this pseudoskepticism? Another exercise in self-reflection uses a method by which one can test one's belief by betting against it, cf. (Earman 1992). Now as a rule one should *believe* in the conclusions that one puts forward about someone else's work, so to test that belief one could ask oneself the following question: am I willing to bet my own career that my conclusions about my colleague's work are sound? The ideal here is that every student that graduates from a university should have adopted such a self-reflective attitude. Also Consoli recently made a plea for a more self-reflective attitude among scientists, cf. (Consoli 2006); this could certainly contribute to a rise in the standard of discussion as it lets people look beyond a first emotional response.

Punitive Measures: A third point is that besides these improvements in education and attitude some form of punitive measures have to be taken: history has already proven that the assumption, that people will always do the right thing when they know what that is, is *false*. It is, in other words, an illusion to think that improvements in education and attitude will be sufficient to root out all scientific misconduct. The goal of punitive measures is that scholars adhere to the principles of good scientific practice: it is then not important whether that is done voluntarily or out of fear of the consequences of misconduct. A simple measure that can relatively easily be implemented in the current system is to limit the power of the committees for scientific integrity: there has to be some rule establishing that, in cases that concern type two scientific misconduct, such a committee *has to* conclude that scientific integrity is violated when it is proven that not even an attempt has been made to adhere to the principles of good scientific practice. This immediately raises the question *when* that can considered to be proven; here one might think in terms of tell-tale signs (pseudoskepticism, strategic action, defamation). The point here is this: it may be impossible to reach a general consensus about what good scientific practice means in every detail, but on the other hand it may be easy to reach a general consensus on when it is clear that an effort has not even been made to adhere to the principles of good scientific practice.

Revoking Anonymity: In the vast majority of scientific journals, the peer review process is anonymous: the argument is that the referee should be protected from the author. But on the other hand, authors should be protected from hostile critics that hide behind that anonymity. Van Rooyen et al. (1999) have already concluded that the answer does not lie in open (i.e. non-anonymous) peer review: it "had no important effect on the quality of the review, the recommendation regarding

publication, or the time taken to review, but it significantly increased the likelihood of reviewers declining to review". That being said, a measure that can relatively easily be implemented in the existing anonymous peer review process is then to reveal the identity of an anonymous referee to the author(s) of a submitted manuscript when the review report is nothing but a pseudoskeptical attack. The author has then the possibility of filing a complaint about type two scientific misconduct at the referee's university. This also raises the question *when* it can be considered proven that a review report is a pseudoskeptical attack; the idea is then that the general rule of Sect. 2.1 applies. In other words, there should be ample room for scientific discussion. The measure should thus not be applied when there are sound arguments to reject a paper, or when the referee has made an honest mistake: it is to be applied in blatant cases only. One can think of a list of generic examples of pseudoskeptical attacks (e.g. after 10 months waiting, the referee comes up with an obviously hastily written report that, in abusive language, rejects the submitted paper without uttering a single word about the specific contents of the paper): if the referee report is an instance of such an attack, then that is evidence that not even an attempt has been made to adhere to the principles of good scientific practice. Further discussion might yield a consensus about the contents of the list. The suggested measure then takes away the possibility of hiding behind anonymity, that is, of launching such a pseudoskeptical attack without any danger to oneself: with that measure in place, an anonymous referee might think twice before sending in a hostile review report—this is the desired self-reflection.

Conclusions

First of all, the scope of the concept 'scientific misconduct' has been widened. Thus far, scientific misconduct has almost exclusively been thought of as being self-serving; examples hereof have been given in the text. But in the foregoing a distinction has been made between *type one* or *self-serving scientific misconduct*, and *type two* or *other-harming scientific misconduct*. Examples of recent cases of that latter type have been given; these can be added to the well-known historical cases in which scientists and philosophers have been thwarted by an establishment in a way that has nothing to do with scientific discourse.

Second, all these cases of attacks on scientists and philosophers have been reduced to three forms of type two scientific misconduct: (1) biased quality assessment; (2) smear in the public media; (3) officially condoning misconduct. That is to say: the *modus operandi* of Mill's "tyranny of the prevailing opinion" has been described in terms of forms of type two scientific misconduct, of which there are three. Most forms of attack are already mentioned in the literature, e.g. in (Martin 2010), but the new approach here is to start seeing these as forms of type two scientific misconduct. As the examples show, there has been no social evolution in the scientific community throughout the centuries: the same old dirty tricks are applied time and time again, up until the present day. So calling it scientific misconduct provides an entirely new perspective for preventing and addressing such inappropriate behavior.

Concerning the border between acceptable behavior and type two scientific misconduct, the criterium of demarcation is that one should only speak of the latter in case *not even an effort has been made* to adhere to the basic principles of good scientific practice. The point is that a definition of scientific misconduct should not hinder a (fierce) discussion at an objective level, in which room should be allowed for honest mistakes. Another way to look at it is to view scientific quality as a two-edged sword: it applies to one's own scientific work *and* to one's evaluation of someone else's work. The ethics, *in casu* the principles of good scientific practice, are then in place to guarantee this scientific quality. If then these ethics are so blatantly violated that one's evaluation of someone else's work is obviously not written with the idea of scientific quality in mind, then one can speak of type two scientific misconduct.

Concerning Mill's *Sollsatz* from *On Liberty* that "there needs [to be] protection ... against the tyranny of the prevailing opinion", recommendations have been made, the implementation of which might lead to a raise in the standard of scientific discussion. By broadening education and teaching students a self-reflective attitude, type two scientific misconduct might occur far less frequently in the future. And by putting a rule in place for handling complaints about type two scientific misconduct, the incidents that then nevertheless occur can then be brought before a committee for scientific integrity: which will deal with such matters appropriately. This would create an atmosphere in which, on the one hand, a scientist who believes he has found a new point of view why some widely accepted theorem is incorrect, is able to set forth his reasoning in a publication without the risk of being called a crackpot in public by his "colleagues", or being fired if his reasoning after all turns out to be invalid; and, on the other hand, a scientist who believes that a newly proposed theory is flawed, is free to publish a scientific refutation of that theory, but won't get away with using smear tactics against the author.

The central issue regarding type two scientific misconduct is pseudoskepticism, which in some branches of science—certainly in the foundations of physics—has virtually become the standard of discussion: the bottom line is that it ought to be the duty of every thinker who cares for the traditional quality of scientific discussion to combat this major menace of our times.

Acknowledgments The author wishes to thank Aliaksei Sedzin (NXP Semiconductors, Eindhoven, the Netherlands) for bringing the Soviet documentary "Ya i Drugiye" to his attention, and Brian Martin (University of Wollongong, Australia) for his helpful comments. This research has been facilitated by the Foundation Liberalitas (the Netherlands).

References

Aaronson, S. (2012). I was wrong about Joy Christian. *Shtetl-Optimized blog*, May the 10th.

ANP. (2008). TU/e in de fout bij herbeoordeling promotie. *NRC Handelsblad*, August the 29th (in Dutch).

ANP. (2012). Eindrapport over fraude ex-hoogleraar Stapel. *Volkskrant*, November the 28th (in Dutch).

Asch, S. E. (1951). Effects of group pressure on the modification and distortion of judgements. In H. Guetzkow (Ed.), *Groups, leadership and men* (pp. 177–190). Pittsburgh: PA Carnegie Press.

Beeckman, T. (2009). Blijheid in overvloed. Spinoza over verlangen en macht. In: T. Beeckman (Ed.), *Spinoza: Filosoof van de Blijheid* (pp. 53–72). Brussels: ASP (in Dutch).

Beenakker, C. (2008). Unpublished referee report, conclusions mentioned in: (LOWI, 2009) (in Dutch).

Brown, C. L. (2005). Overcoming barriers to use of promising research among elite middle east policy groups. *Journal of Social Behavior and Personality*, *17*(1), 489–544.
Cabbolet, M. J. T. F. (2010). Elementary process theory: A formal axiomatic system with a potential application as a foundational framework for physics underlying a gravitational repulsion of matter and antimatter. *Annalen der Physik 522*(10), 699–738.
Cabbolet, M. J. T. F. (2011). Addendum to the elementary process theory. *Annalen der Physik 523*(12), 990–994.
Campanario, J. M. & Martin, B. (2004). Challenging dominant physics paradigms. *Journal of Scientific Exploration, 18*(3), 421–438.
Christian, J. (2011). What really sets the upper bound on quantum correlations? arXiv:1101.1958v1 [quant-ph].
Consoli, L. (2006). Scientific misconduct and science ethics: A case study based approach. *Science and Engineering Ethics 12*, 533–541.
Dijkgraaf, R. H. (2011). In *Geschokte Robbert Dijkgraaf: Fraude Stapel veel erger dan gevreesd.* Volkskrant, October the 31st (in Dutch).
Earman, J. (1992). *Bayes or bust?* (p. 38). Cambridge: MIT Press.
Fuller, L. L. (1981). Philosophy for the practicing lawyer. In K. I. Winston (Ed.), *The principles of social order: Selected essays of Lon L. Fuller* (pp. 287–290). Durham, N.C.: Duke University Press.
Habermas, J. (1991). *Erläuterungen zur Diskursethik* (p. 161). Frankfurt a.M.: Suhrkamp Verlag (in German).
Lambert, F. (2008). Lemma "Marcoen Cabbolet" in *Wikipedia*, created by Lambert on January the 18th (in Dutch; title has changed in the meantime).
Landau, L. D. & Lifschitz, E. M. (1976). *Mechanics*, 3rd ed. (p. xii). Oxford: Butterworth-Heinemann.
LaViolette, P. (2005). Solar cycle variations in ice acidity at the end of the last ice age: Possible marker of a climatically significant interstellar dust incursion. *Planetary and Space Science, 53*(4), 385–393.
Löwenstein, S. & Müller, R. (2011). Wir sind einem Betrüger aufgesessen. *Frankfurter Allgemeine Zeitung*, 25 February 2011 (in German).
LOWI. (2009). LOWI-advies 2009, nr. 1 & nr. 2 (in Dutch; anonymized version publicly available).
Martin, B. (1997). *Suppression stories* (pp. 84–103). Wollongong: Fund for Intellectual Dissent.
Martin, B. (1998). Advice for the dissident scholar. *Thought & Action 14*(1), 119–130.
Martin, B. (2010). How to attack a scientific theory and get away with it (usually). *Science as Culture, 19*(2), 215–239.
Merton, R. K. (1968). *Social theory and social structure* (p. 477). New York: Free Press.
Motl, L. (2012). Joy Christian, an entanglement denier. *The reference frame blog*, March the 24th.
Mytelka, A. (2007). High-profile dean of admissions at MIT quits over spurious credentials. *The Chronicle of Higher Education*, April the 26th.
Popper, K. (1982). *Quantum theory and the schism in physics* (p. 100). London: Hutchinson.
Post, R. (2008). Addition to the lemma "Marcoen Cabbolet" in *Wikipedia*, January the 23rd (in Dutch).
Schweber, S. S. (1989). Some reflections on the history of particle physics in the 1950's. In L. M. Brown, M. Dresden & L. Hoddeson (Eds), *Pions to quarks* (p. 681). Cambridge: Cambridge University Press.
Schwinger, J. (1991). Cold fusion—does it have a future? In M. Suzuki & R. Kubo (Eds), *Evolutionary trends in the physical sciences, springer proceedings in physics, Vol. 57* (pp. 171–175). Berlin: Springer.
't Hooft, G. (2008a). In: Nobelprijswinnaar negatief over omstreden promotie. *Volkskrant*, January the 17th (in Dutch).
't Hooft, G. (2008b). In: E. Hardeman, Iets heel erg uit de rails gelopen. *Ublad 16*(39), 12 (in Dutch).
't Hooft, G. (2008c). In: Elementaire Proces-theorie allang achterhaald. *Natuurwetenschap & Techniek, 76*(9), 65 (in Dutch).
Truzzi, M. (1987). On pseudo-skepticism. *Zetetic Scholar, 12–13*, 3–4.
Truzzi, M. (1990). Reflections on the reception of unconventional claims in science. *Frontier Perspectives, 1*(Fall/Winter), 13–25.
Van Reijen, M. (2009). Spinoza's naturalistische filosofie van de affecten. In T. Beeckman (Ed.), *Spinoza: Filosoof van de Blijheid* (pp. 13–30). Brussels: ASP (in Dutch).
Van Rooyen, S., Godlee, F., Evans, S., Black, N., & Smith R. (1999). Effect of open peer review on quality of reviews and on reviewers'recommendations: A randomised trial. *British Medical Journal, 318*, 23–27.
Vongehr, S. (2011). Exchange with the future king of physics. *alpha meme blog*, June the 1st.