

Scientific misconduct and science ethics: a case study based approach

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ABSTRACT: *The Schön misconduct case has been widely publicized in the media and has sparked intense discussions within and outside the scientific community about general issues of science ethics. This paper analyses the Report of the official Committee charged with the investigation in order to show that what at first seems to be a quite uncontroversial case, turns out to be an accumulation of many interesting and non-trivial questions (of both ethical and philosophical interest). In particular, the paper intends to show that daily scientific practices are structurally permeated by chronic problems; this has serious consequences for how practicing scientists assess their work in general, and scientific misconduct in particular. A philosophical approach is proposed that sees scientific method and scientific ethics as inextricably interwoven. Furthermore, the paper intends to show that the definition of co-authorship that the members of the Committee use, although perhaps clear in theory, proves highly problematic in practice and raises more questions than it answers. A final plea is made for a more self-reflecting attitude of scientists as far as the moral and methodological profile of science is concerned as a key element for improving not only their scientific achievements, but also their assessment of problematic cases.*

Introduction

It is widely acknowledged that the public perception of science and scientists has deeply changed during the last fifty years.¹ From a status almost comparable to a religious authority tapping directly into the Truth, science is seen more and more by the general public as a fallible and often dangerous activity. Academic sociological discussions aside, this fact is quite evident in public discussions about the role of scientists in modern society. It is therefore hardly surprising that occurrences of scientific misconduct and, more generally, issues concerning the moral responsibility

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of scientists, have received a lot of attention in recent years.^{2,3} The topic almost begs for ethical and philosophical reflection.

In September 2002 the results of an investigation about a high-profile case of scientific misconduct were published.⁴ It can be quite confidently said that the Jan-Hendrik Schön *affaire* has been one of the most publicized and discussed episodes of misconduct in science during the past few years. Bell Labs (the employer of Schön at the time) established a Committee of senior scientists who, after a very detailed analysis of a number of scientific papers published by Schön and co-authors, concluded that "...of the 24 Final Allegations examined, Hendrik Schön committed scientific misconduct in 16..."^{4(p.3)} At the same time, all the co-authors were cleared of all charges of misconduct: "The Committee finds all co-authors of Hendrik Schön in the work in question completely cleared of scientific misconduct."^{4(p.4)} This remarkable combination is one of the starting points for the reflection carried out in this paper. The analysis will not only concern the case itself, but will also focus on the assumptions and presuppositions that come to light if we read the Report with a proper amount of philosophical attention. The attention will thus gradually shift from the case itself to the reasonings and arguments of the writers of the Report. The underlying motivation is twofold. On the one hand, the goal is to assess the adequacy of the perception of misconduct within the scientific community; on the other hand (and of course related to the first point), the interest is in investigating the degree of awareness of scientists about the presuppositions and assumptions underlying the concept of scientific methodology in general and publishing strategies in particular. The case described in the Report is very helpful in exposing existing ambiguities and problematic aspects of science.

After a short factual history of the case, three questions will be used as *Leitmotiv* in the paper: 1) Is this misconduct episode as extreme and exceptional as most coverage of it seems to imply? 2) Are the conclusions of the Report as unambiguous as they seem? 3) If this turns out not to be the case, and in the reasonable assumption that the writers of the Report, being prominent researchers, are representative of the scientific population, what can be said about the general attitudes of practicing scientists with respect to the methodological and ethical assumptions underlying their scientific activities?

History of the case

Nanotechnology, the field in which Hendrik Schön worked as a physicist, is one of the *hot topics* of modern physics. There are both scientific and economic reasons for this: unsolved mysteries about the fundamental way matter interacts intermingle with the huge commercial perspectives linked to the possibility of producing smaller and smaller devices. The research team of which Hendrik Schön was a member had claimed major breakthroughs in the field of organic crystals, and had published these results in an impressive sequence of papers in (among others) *Nature* and *Science*. The details of the research are not in this context directly relevant, but very good accounts of it can be found (see for example⁵).

After five years of successful research and publications, something happened: theoretical and experimental “impossibilities” were noticed and, at the beginning of 2002, the first unofficial investigation was launched, quickly taking an official character in May 2002. The reasons that brought about the doubts can be grouped in two categories: lack of reproducibility and the excessive publication rate (an almost ironic reason in the *publish or perish* climate to which researchers in the exact sciences are subjected). The doubts grew rapidly when it was noticed that some pictures referring to completely different materials were identical down to the background noise. The media rapidly got hold of the case and in both scientific journals and newspapers a lively albeit rather superficial discussion about misconduct in science arose.^{6,7} The conclusions of the Committee were made public in September 2002 in the form of the Report, leading to discussions about its implications.^{8,9} Hendrik Schön was fired and retractions of the once groundbreaking papers began to appear.

Hendrik Schön never admitted guilt, maintaining that “honest mistake” could explain all the inconsistencies.⁴ (Appendix H-1)

In 2004 another turn took place: Jan-Hendrik Schön was stripped of his PhD degree under German laws that allows a doctoral title to be withdrawn if its holder behaves in an “undignified” manner, even if no proof or suspicion of foul play in that research has been shown.¹⁰

The Report: the task of the Committee

It is perhaps not immediately obvious from the above concise description why this case should merit special philosophical and ethical attention. In fact, the starting point for philosophical reflection does not reside in the case itself, but in the way the Committee builds up, justifies and presents its conclusions. The bulk of the paper will therefore be devoted to the analysis of the Report, notably the first 19 pages, in which the arguments are presented.

Let us begin with the task given to the Committee: “...the management of Bell Labs formed a committee to investigate the possibility of scientific misconduct, the validity of the data and whether or not proper scientific methodology was used in papers by Hendrik Schön, et al., that are being challenged in the scientific community.”^{4(p.2)} Two crucial concepts are introduced here: *scientific misconduct* and *proper scientific methodology*. The *validity of data* is of course also relevant, but we will deal with it only indirectly. Some questions arise immediately: what do the authors mean by these concepts? Can we find definitions somewhere in the text or are they used in an intuitive way, based on some kind of implicit communal knowledge? How do the concepts relate to each other? The charges levelled against the scientists are presented immediately after the mission statement and divided into three categories: “substitution of data...unrealistic precision of data...results that contradict known physics.”^{4(p.2)} This list is in itself not particularly problematic, but the picture is mixed up by an interesting observation in the following page: as a consequence of the fact that the original instruments and data records are no longer available, the conclusion follows that “it is not possible to confirm or refute directly the validity of the claims in

the work in question.”^{4(p.3)} This seems to suggest that some kind of distinction is introduced between the validity of the data used to verify an empirical claim and the validity of the claim itself. In other words, scientific misconduct is not directly related to the “truth” of the scientific claims. We aim to show that this position will lead to serious difficulties for the members of the Committee.

Let us now look at the issues described above in greater detail.

What is scientific misconduct?

In the Report we find an attempt to uphold a definition of scientific misconduct based on a broad consensus within the scientific community. The choice was made to follow the guidelines given by the American *Federal Policy on Research Misconduct*.¹¹ The definition that can be found there is: ‘Research misconduct is defined as fabrication, falsification, or plagiarism in proposing, performing, or reviewing research, or in reporting research results’. Looking now at the conclusions of the Committee, we read: “Hendrik Schön committed scientific misconduct as defined by the falsification or fabrication of data, such that the research is not accurately represented in the research record.”^{4(p.14)} This helps us partly in understanding why the distinction was made between the data as such and the way they are presented in print, noticed above: the Committee has found misconduct exclusively in the way results have been published, and nothing can be said about the way research was conducted. This has two consequences: firstly, the report is not dealing with the performance of the research, but with how the research was published. Secondly, the weight of the charges seem to become less heavy as mis-conducting one’s research seems a graver offence than mis-representing it.

In addition, there is in my opinion a more fundamental problem which arises in the definition of misconduct that has been used and, as a consequence, in the final judgment of the Committee. To elucidate this we need first to go back to the definition of misconduct given in the *Federal Policy* and, in particular to the following observation: “Research misconduct does not include honest error or differences of opinion.” This seems reasonable: science is based on differences of opinion and errors are inevitable, they are in a way even an asset for the scientific process. If we now look further, some criteria are given for the assessment of misconduct. In particular: “A finding of research misconduct requires that the misconduct be committed intentionally, or knowingly, or *recklessly*.” This criterion is used by the Committee: “Hendrik Schön committed scientific misconduct ...he did this intentionally or recklessly....”^{4(p.14)} Now, “reckless” means in common English “marked by lack of proper caution: careless of consequences.”¹² This seems to mean that *sloppy research* (which is surely marked by lack of proper caution) falls within this definition of research misconduct. If this is the case, then either the definition of misconduct becomes so broad as to render its usability questionable, or the exceptionality of a case such as that analysed here must be questioned.

These observations can be further pursued if we shift our attention to the second concept, *proper scientific method*. The degree of “sloppiness” can after all be

determined on the basis of some criteria describing a “good” way of performing scientific research.

Scientific method and scientific ethics

The Report does not include an explicit description of what counts as proper scientific method. This could suggest that an implicit definition is used, that relies either on tacit knowledge within the field or on an intuitive understanding of the concept, or on a combination of both. An attentive reading of the Report produces another picture however, and its consequences bear in my opinion on the whole idea of scientific misconduct. Let us not forget that, in the Report, the focus of the discussion is not on the scientific truth of the claims, but only on the validity of the data that have been published as support for those claims. The Report states about this point: “...Hendrik Schön did not follow generally accepted practices in his field with regard to the maintenance of traceable records...nor did he retain original data in a form with which critical physical claims could be verified, or even examined.”^{4(p.15)} In this excerpt a direct link is established between *proper scientific method, generally accepted practices in the field* and *scientific misconduct*. A key question arises immediately: would the behaviour of Hendrik Schön cease to be misconduct if the generally accepted practices in the field changed? What would that imply for scientific research? That the above quotation is *not* an example of a conventionalist vision of science becomes obvious if we go on reading: “This is unacceptable ...and is at odds with the fundamental tenet of science that results must be presented honestly...”^{4(p.16)}

This is a very interesting passage, where the severity of the judgment is underlined without possibility of misunderstanding. At the same time, if coupled with the first excerpt, it brings to light a rather fundamental theoretical difficulty and at the same time suggests an interesting philosophical perspective. First the difficulty: on the one hand, we have a definition of scientific method that seems to imply a relative element (the “accepted practices”); on the other hand, the verdict is presented as an absolute moral judgment. In other words, a Gordian knot links ethics and method; an offence to the latter becomes an offence to the former. This is furthermore presented as unproblematic, but it obviously is. The perspective that we can use to get out of the impasse is as follows: the fact that the Gordian knot *cannot* be untied is *constitutive* of the way the scientific endeavour works. Scientific ethics and scientific method do not just happen to cross each other’s path, but represent a constant unity. Every time we talk about one of the two, we must talk about the other one. This is why great difficulties arise if we just put them next to each other, instead of recognizing their common character. Within this perspective, the research process is analyzed as one where ethical principles underpin and inform methodological principles, and vice versa.

The analysis so far also underlines a pattern which we maintain is fairly common: researchers act and judge moral questions on the basis of tacit and often implicitly transmitted knowledge, and this does not usually pose problems for daily practices (but see below). When the scientists have to make some tough judgment calls, though, like

in this misconduct case, the intuitive and unreflected character of these ideas can bring about some serious problems.

We can actually identify another potential problem, one that goes to the heart of the concept of ‘misconduct’. It has to do with the relationship between data and their interpretation. In the Report it is quite obvious that misconduct has occurred, because the scientist seems to have ‘tampered’ with the raw data in a spectacular way. But the concept of ‘tampering’ is not unequivocal; a sequence of data has meaning only within an interpretative context. To achieve this interpretation it is a very normal, necessary and accepted practice to throw away ‘abnormal’ (meant here not in a Kuhnian sense),¹³ data after a good deal of double-checking and often intensive statistical analysis and to assume that some kind of ‘glitch’ caused the appearance of the results that do not conform to the theory. But how are we to define the line between ‘data analysis’ and ‘tampering’? Is implicit, non-reflective knowledge a good enough criterion?

That these concerns are not too far-fetched should have become clear from the analysis so far. Furthermore, if we now turn to the way co-authorship is treated in the Report, it will become even clearer that these worries are not misplaced.

Co-authors: authors or spectators?

We have quoted in the beginning the remarkable conclusion of the Committee that it does not consider co-authors guilty of misconduct. If we analyze in detail their contributions, we can on the one hand see why this conclusion was reached. On the other hand, a deeper problem becomes visible. Let us quote from the Report: “The devices used in the work in question were (with a few exceptions) fabricated by Hendrik Schön alone, with no participation by any co-author or colleague...Physical measurements of the significant devices underlying all papers in question were (with one exception) carried out by Hendrik Schön alone, with no participation by any co-author or colleague...no measurement of demonstration of a significant physical effect or device characteristic...was witnessed by any co-author or other colleague.”^{4(pp.9-10)}

A question arises immediately: why were these co-authors listed as such if they did not contribute in any significant way to the research papers in question? The problem is compounded by the statement of the Committee that the definition of co-authorship they used in their assessment is that of the Deutsche Forschungsgemeinschaft;¹⁴ they cite Recommendation 11. This Recommendation states: “Authors of scientific publication are always jointly responsible for their content. A so-called “honorary authorship” is inadmissible.” In this misconduct case, it seems that the co-authors are at least “non-active”, which comes quite close to being “honorary”. Further analysis reveals that some of the co-authors have provided the materials to be used as samples in performing the experiments, which helps to qualify “non –active” as opposed to “honorary”. The problem of joint responsibility, nevertheless, stands. The position that the Committee chooses does not help to clarify matters: as far as co-authors are concerned “...there is no implication here of scientific misconduct; the issue is one of professional responsibility.”¹ At the same time, it is stated that “...co-authors...endorse the validity of the work...co-authors represent the first defence line against

misconduct.”^{4(p.16)} Furthermore, “Collaborative scientific research requires a high level of trust among the participants...Shared credit for the accomplishment must be matched with shared responsibility.”^{4(p.17)} New questions arise: how do we measure the level of responsibility? How can someone who did not provide an active contribution to the contents of a papers be held accountable for it? Why is absence of control by the co-authors merely regarded as lack of professionalism and not scientific misconduct? All these questions are not answered in the Report.

The concluding remark summarizes the point of view of the Committee and indicates a possible perspective: “The Committee does not consider itself qualified to make a specific judgment in this case, in the absence of a broader consensus on the nature of responsibilities of participants in collaborative research endeavours...these are matters appropriately left to the scientific community as a whole...It is hoped that this report will provide a solid foundation on which to base any further considerations.”^{4(pp.18-19)}

In the concluding discussion we shall now try to assess in how far the hope of the Committee is justified.

A plea for self-reflection

At the beginning of this paper we asked three questions. Let us see if our analysis of the Report provides us with plausible answers from which we can derive some more general conclusions and perspectives.

1) As far as the media reaction is concerned we can reasonably conclude that the publicity that this case received was caused mainly by two factors: the number of papers that were actually published in high-impact journals before suspicions arose and the sensitivity of the general public towards scientific misconduct in general. In this sense we can speak of an “exceptional” case. If we look however at the (philosophical and ethical) nature of the charges, the picture is more nuanced; in a sense, we are presented with a case that, although exceptional in certain aspects, points to some type of “chronic pathology” in contemporary science. What I mean by this is: practices that are a normal and sometimes essential part of the scientific enterprise (selection and preselection of data, how to treat anomalies) have been bent and abused to such a degree as to remove themselves from any accepted standard. This contextualization does not in any way mean a toning down of the severity of the judgment or a justification of the way in which the scientist involved acted; what it does do is put us in a situation in which it is possible to analyse the borderline between acceptable and unacceptable behaviour and show that it can be problematic. This was not possible (and maybe not even desirable) for the writers of the Report, since their conclusions needed to be as “objective” as possible.

2) It should have become clear by now that the conclusions of the Report cannot be regarded as unambiguous, neither with respect to misconduct nor with respect to the discussion about co-authors. Misconduct is surely established, but the gravity of it, the relationship with “normal” scientific practices and the link with this specific form of

misconduct (the publishing of results) and the broader context of the scientific practices are barely sketched. As a result, theoretical tensions and contradictions appear throughout the report without being identified as such.

The complexity of the authorship question is recognized in the Report, but the question is treated in a confusing way and does not lead to any concrete propositions.

3) One important consequence that can be attached to these conclusions is that practising scientists can get into trouble when confronted with situations in which ethical and philosophical questions need to be dealt with. The practical relevance of this point can be underlined if we put it within the context of an ongoing discussion about the way in which scientific misconduct should be dealt with. Scientists will always be involved in these kinds of investigations, since understanding of the scientific content is essential to establishing whether misconduct occurred or not. In this sense we cannot be satisfied with the idea that only people with specific competence and knowledge of ethical and philosophical problems are also taking part in this kind of committees. What we saw in our analysis is that the judgment about scientific content (the task that is kept away from the scientist) was often based on implicit and contradictory presuppositions. The link between methodology and ethics was also made in an almost “natural” way, without being aware of the theoretical problems that this implies.

The analysis of this paper can then be used to signal a problem in the way scientists are educated; in particular, there is a lack of necessary attention to the ethical and theoretical foundations of science. Researchers can no longer afford themselves the luxury of neglecting these specific skills, nor can it be taken for granted any more that they will be learned implicitly, for example within the master-pupil dialectic. The sheer complexity of modern scientific practice makes it necessary for universities and scientific institutions to take that task upon themselves. Specific attention to different kinds of skills is needed: a) insights into the foundations of science; b) skills for signalling, addressing and analyzing cases of moral conflict; c) formation of moral values of scientists. This will help scientists among others to deal more adequately with problematic cases – and eventually to become better scientists.

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