Fraud: causes and culprits as perceived by science and the media

Institutional changes, rather than individual motivations, encourage misconduct

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Almost 2 years after Woo Suk Hwang and colleagues published groundbreaking work on the creation of human embryonic stem-cell lines, an investigation committee from Seoul National University, South Korea, announced that much of the research had been fabricated. Human embryos had not been cloned and stem-cell lines had not been derived from patient-specific somatic cells. *Science* retracted the two articles concerned, and Hwang and several members of his research team were later indicted on charges of fraud, embezzlement of research funds and violations of bioethical laws.

In the aftermath of this case, many scientists expressed concerns that the public image of science, and of stem-cell research in particular, had been tarnished. "Scientists fear that the episode will damage not only public perceptions of stem-cell research, but science's image as a whole" (Check & Cyranoski, 2005). Others were more optimistic and hoped that the case would "serve as an antidote to the 'tabloidization' of stem-cell research and ... make the public conscious of the fact that the science is difficult" (Snyder & Loring, 2006). Some simply regarded the race as open again: "At one point, the research community thought that we might have, in Hwang, a technical virtuoso. Now we recognize that we all remain on an equal footing" (Snyder & Loring, 2006).

The Hwang case provides a good example of how the scientific community and the mass media deal with fraudulent behaviour in science. Both give the impression that misconduct is committed mainly by

individuals who have lost—or never had—a scientific ethos, while reassuring the public that fraudulent scientists are ultimately caught and punished. Indeed, agencies dealing with cases of scientific misconduct, such as the US Office of Research Integrity (ORI) in Rockville, Maryland, and the German Research Foundation (DFG) in Bonn, also proceed on the assumption that misconduct is a problem at the level of the individual. Instead, we argue here that there are other factors—emanating from institutional changes within science (Weingart, 2001)—that favour misconduct, and might make it more widespread than is suggested by the scientific community and the media. The public discussion of scientific fraud might act to strengthen the scientific ethos, whereas the underlying conditions that

encourage misconduct work continuously to erode it.

Fraud in science is defined by the *intention to deceive* as compared to error or carelessness, and is commonly classified into three categories: fabrication, falsification or plagiarism (FFP). When investigating cases of suspected scientific misconduct, the DFG usually speaks of dishonesty rather than intentional fraud. This wider definition allows all forms of misconduct to be separated from carelessness and honest mistakes.

It is difficult to compare the incidence of fraud in the USA and Germany, owing to different definitions of misconduct. However, both the ORI and the DFG report no significant rise in the number of fraud cases in the

CHARACTERISTICS OF BIOMEDICAL RESEARCH THAT MIGHT ENCOURAGE MISCONDUCT

In biomedical research, scrutiny—usually by peer review—is complicated by the specificities of biological material. It is not easy to replicate results from another research group without also obtaining the same materials, such as specific cell cultures. More generally, biomedical research depends to a large extent on experimental conditions and researchers' skills; therefore, the methods and techniques developed and refined in one laboratory might be difficult to adopt elsewhere. In fact, exact reproducibility of individual experiments is not even expected and therefore "may provide some apparent cover for a biologist who is tempted to cheat" (Goodstein, 2002). Papers on experimental results are examined for the consistency of the argument rather than for details of the underlying methodology. Even where replication of an experiment, or parts of it, would be feasible, this has little appeal to a peer pressured for time, resources and originality: "A chef cannot make a reputation for himself by demonstrating bad recipes" (Broad & Wade, 1982).

Biomedical research increasingly occurs in large compartmentalized laboratories, such as the sequencing centres of genome projects. This increases efficiency, but the mass production of data and publications with scores of authors dilutes the responsibility for integrity. In addition, co-authorship of a paper with fraudulent data usually has—beyond embarrassment—no major consequences (Wormer, 2006).

Biomedical research is increasingly characterized by its interdisciplinary and transdisciplinary nature. Working in multidisciplinary teams makes it more difficult to assess the quality of colleagues' work. Quality standards might not always be shared across disciplines, and mutual control is complicated.

Biomedical research is a highly competitive field in which everybody is aware of where the 'cutting edge' lies. The importance of publishing initially shapes the research: "Work must be rushed out to minimize the danger of being scooped" (Lawrence, 2003). What started as a competitive intellectual enterprise has turned into an intense competition for scarce resources. In trying to get ahead, individual scientists "invest substantial resources [...] and incur substantial opportunity costs" (Martinson *et al.*, 2006).

In biomedical research, publicly and privately funded endeavours are more intertwined than in most other fields. Scientific results need to satisfy financial interests. Issues of ownership create intense discussions, especially if the intellectual property at stake is worth a great deal. Industry has funded an increasing proportion of frequently cited studies in medical research: "Clinical research is dictated by the need to promote products of industry. In this sense, academics might have indeed lost control of the clinical research agenda" (Patsopoulos *et al.*, 2006).

Additional pressure comes from societal expectations, as in the Hwang case. The pressure to produce positive findings can prevent scientists from publicizing potential adverse effects (Mayntz, 1999). Highlighting diagnostic and therapeutic prospects from basic research is a convenient tool to attract both public attention and funding. Although most biomedical work is basic research, results are often linked to potential clinical applications.

A more general phenomenon is the 'publish or perish' mentality in modern science. The number of publications and the impact factors of the journals in which one publishes have become crucial career factors. "Findings are sliced as thin as salami and submitted to different journals to produce more papers" (Lawrence, 2003). Such 'least publishable units' have caused an explosion of scientific publications in past decades. From 1998 to 2003, the number of new publications increased by 5% worldwide, with medical research and fundamental biology as the most prolific disciplines (Observatoire des Sciences et des Techniques, 2006). This increase is exacerbated by performance measures, which are based on publications and citation scores.

This also creates an intense pressure to publish in the top-tier journals. Having a paper published in *Science* or *Nature* has become an end in itself, the symbolic equivalent of a scientific achievement: "Although there are good reasons for publishing papers where they are more likely to be read, when we give the journal priority over the science we turn ourselves into philistines in our own world" (Lawrence, 2003). *Science* receives 12,000 submissions per year—and the number is rising—of which less than 8% are accepted (McCook, 2006). To capture the increasingly scarce attention of editors, scientists might be tempted to exaggerate results. The deluge of papers submitted to high-impact journals puts reviewers under extraordinary stress and might weaken the quality of the peer review process.

Publishing a paper in a high-impact journal is just the first step towards attaining visibility within both the scientific community and the general public. Journalists who cover science in the mass media almost exclusively rely on leading journals for story ideas (Pahl, 1998). Scientific institutions, individual scientists and even scientific journals join in today's "craze for publicity" (Lawrence, 2003). Given the high levels of research funding from the public sector, there is a need for societal accountability in addition to the traditional legitimization by peers. Media visibility justifies research expenditures and is therefore actively propagated by universities and funding bodies.

past few years. Indeed, FFP seems to be rare: Fuchs & Westervelt (1996) extrapolated from known cases and assumed—perhaps too conservatively—that 0.01% of all publications were fraudulent. Studies of the incidence of plagiarism describe an empirical variance from 0.02% to approximately 25% of all articles (Giles, 2005). This variance illustrates both methodological problems and the fact that any quantitative analysis of scientific fraud is speculative because of the number of unreported cases.

Biomedical research seems particularly affected by fraud (Broad & Wade, 1982; Kreutzberg, 2004); indeed, the 'hall of infamy' in science portrays a number of biologists and clinicians. Several underlying characteristics (see sidebar) could explain why scientific misconduct is more prevalent and more visible in this particular research field than in other disciplines. The characteristics of biomedical research and related fields, in which the permanent pressures for publication, time and competition are exceptionally high, exemplify the fact that institutional conditions might encourage scientists to cut corners. This has led to the diagnosis of an "antiscientific culture in which pushiness and political skills are rewarded too much, and imaginative approaches, high-quality results and logical argument, too little" (Lawrence, 2003).

A recent survey in the USA sheds light on this complex situation and the resulting scientific misbehaviour (Martinson *et al.*, 2005). The authors explicitly emphasized cases that eluded public perception: "Historically, professionals and the public have focused on headline-grabbing cases of scientific misconduct, but we believe that researchers can no longer afford to ignore a wider range of questionable behaviour that threatens the integrity of science." They analysed 3,247 responses from mid-career and early-career scientists funded by the National Institutes of Health (NIH; Bethesda, MD, USA) and found that nearly one-third admitted one or more misbehaviours, such as a lack of critical reflection on one's own findings, or "changing the design, methodology or results of a study in response to pressure from a funding source" (Martinson *et al.*, 2005). Although this behaviour does not constitute FFP, it is nevertheless regarded as misconduct: "It is particularly important to notice that when scientists talk about behaviors that compromise the integrity of their work, they do *not* focus on FFP; rather they mention more mundane (and more common)

transgressions, and *they link these problems to the ambiguities and everyday demands of scientific research*" (De Vries *et al*, 2006; the italics reflect the author's original emphasis).

The authors of the survey concluded that so-called normal misbehaviours "present greater threats to the scientific enterprise" than high-profile cases of fraud (Martinson *et al*, 2005). Their subsequent analysis of the role of organizational justice indicates that self-reporting of misbehaviour is positively correlated with perceived violations of justice in the processes and procedures that lead to the distribution of resources, which confirms concerns about the influence of structural conditions on misconduct (Martinson *et al*, 2006). The pressures and temptations of short-term evaluation schemes that link financial bonuses to publications probably contribute to this situation.

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Although one-third of the surveyed scientists stated that they had been involved in some misbehaviour, there is a tendency to portray misconduct as the result of individuals who have lost their scientific ethos and are therefore not 'true' scientists (Kreutzberg, 2004). Indeed, it is much easier for the scientific community to dismiss Hwang as a 'black sheep' than to face the possibility that he is a product of institutional circumstances. At a congressional hearing on fraud in biomedical research, held in 1981 by the US House Committee on Science and Technology, eminent scientists failed to acknowledge fraud as a problem that goes "beyond the mental imbalance of a few individuals" (Broad & Wade, 1982). Why does the scientific community ascribe misconduct to individual behaviour rather than acknowledging that it is the tip of the iceberg of normal misbehaviour?

One reason is the lack of a clear differentiation in practice between misbehaviour and errors. "The lines separating honest error and mistakes from fraudulent misrepresentation, and the lines separating the proper use of others' ideas and methods from intentional theft,

are notoriously and irremediably fuzzy" (Fuchs & Westervelt, 1996). Gregor Mendel's data on inheritance in garden peas are now considered by statisticians to be too good to be true—the fact that Mendel has not been accused of fabrication is due mainly to the fact that his hypotheses turned out to be correct. When publishing data on the charge of the electron, physicist and Nobel laureate Robert Millikan excluded measurements that did not fit his (correct) assumptions. These well-known cases point to the problematic nature of this fuzzy boundary in practice. In theory, the demarcation is clear-cut: dishonesty and deliberate publication of false data betray a central principle of a scientist's profession and of science as a social system, whereas errors do not (Mayntz, 1999).

Linked to this crucial difference is a second reason for the individualization of misconduct given that trust in, and respect for, peers forms the basis of science: scientists need to be able to count on the honesty of their colleagues. "It is possible, even encouraged, to be distrustful and skeptical of some results some of the time, but it is impossible and impolite to distrust all communications all the time" (Fuchs & Westervelt, 1996). Scientists who are not able to replicate a colleague's work usually assume that either they or their colleague have made a mistake. To err, despite behaving properly and adhering to standards, is an integral part of everyday scientific work (Mayntz, 1999). This is illustrated by the fact that most publications are retracted owing to experimental errors rather than scientific misconduct: of 395 biomedical articles retracted between 1982 and 2002, the majority (61.8%) were retracted because of unintentional errors; only 27.1% were retracted due to scientific misconduct (Nath *et al*, 2006). However, several studies have shown that such articles continue to be cited after their retraction (Gabehart, 2005; Sox & Rennie, 2006). Even worse, journals might fail to retract papers that are known to be fraudulent (Sox & Rennie, 2006).

The fuzzy boundary between error and misconduct is exemplified by a recent case in *Nature*, in which inconsistencies discovered in a 1993 article posed a "dilemma about trust" (Editorial, 2006). After an investigation commissioned by the authors' laboratory cleared them of misconduct, *Nature* settled on a correction rather than a retraction, stating: "After all, if researchers and editors cannot safely assume, even as a starting point, that scientific results are

essentially true as reported, then the advancement of science is in serious trouble" (Editorial, 2006).

If colleagues are treated as possible forgers and students are considered as prospective swindlers, the essential virtues of science—such as openness, free exchange of ideas, sincerity and fairness—are at stake (Kreutzberg, 2004). The community's tendency to restrict fraud to individuals therefore acts to defend the social and communication system of science, which would be instantly destroyed if distrust became global and pervasive (Fuchs & Westervelt, 1996).

The individualization of scientific misconduct must also be analysed against the background of a changing relationship between science, the media and the public. The media increasingly cover research with societal implications, notably in the biomedical field. In doing so, they expose the inner world of science, scientific practices, such as quality assurance, and the norms of scientific competition.

Rather than translating and disseminating knowledge when covering scientific stories, the media follow their own rules and construct reality according to their own logic. Their rules are applied to science in the same way as to any other subject: they select news on the basis of personalities, human-interest stories and breaches of moral standards. Accounts of scientific fraud are therefore prone to being turned into scandals. "The public impression of scientific deviance is based on a few individual cases, dramatized by the media with generous doses of human tragedy and failure" (Fuchs & Westervelt, 1996).

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The role of the media in cases of scientific misconduct has been discussed from both scientific and journalistic perspectives. Whistle blowers often address the media, which then play a decisive role in the revelation of fraud. Some hope that journalists might provide transparency in fraud cases, and thereby help to clarify events and limit

damage (Großmann, 2003). In the highly publicized case of Jan-Henrik Schön, a young physicist who was found to have fabricated results on at least 16 occasions, post-retraction citations of his papers dropped off steeply (Gabehart, 2005). Generally, “publicity reduces citation of fraudulent work” (Sox & Rennie, 2006). In this sense, the media act as a catalyst: when spreading the news of fraud, they enhance the swindlers’ visibility in the public and the scientific community.

External watchdogs—be they the media or federal agencies—might therefore have the adverse effect of destroying trust in the informal networks of the communication system

But acting as watchdogs of a science system gone wrong is probably too much to ask of the media and it is arguable whether there are enough well-trained science journalists to do the job (Wormer, 2006). At the 9th International Conference on Public Communication of Science and Technology in Seoul, South Korea, in 2006, a Korean journalist who covered the Hwang case explained that he relied heavily on an anonymous source who delivered “very accurate information” and whom he, as a Korean language major, considered “very useful to understand the details of the case”.

However, in covering cases of scientific fraud, the media can create a misleading image of deviant behaviour in science among the public. With regard to crime reporting, media coverage does not mirror the real percentages and fluctuations in coverage are not related to actual variations in crime patterns (Media Tenor, 2004). Although the total incidence of criminal offences has declined slightly within the past 10 years in Germany, more than 80% of respondents in a recent survey thought that crime rates have risen markedly (Pfeiffer *et al.*, 2005). This is true particularly for highly publicized serious crimes: although the number of sexual murders in Germany has decreased steadily from 32 in 1993 to 20 in 2003, the average respondent believed that it has risen to 115 cases. No similar data on public opinion about scientific fraud exist, but it is likely that the public would similarly assume an increase in scientific misconduct in recent years.

Increased visibility has another effect. By presenting the rise and fall of Hwang, the media showed how scientific institutions deal with scientists ‘gone bad’. To brand individuals as ‘black sheep’ who are expelled both strengthens the norms of science and illustrates that the scientific community is well equipped to deal with the “bad apples that roll into public view” (Broad & Wade, 1982). Part of this topos of personalization is the contention that scientists are human beings after all. It also underlies the codification of the scientific ethos as rules of good scientific practice by many scientific institutions.

The repercussions of the public visibility of fraud—such as open discussions of fraudulent cases, the creation of oversight agencies and the codification of the scientific ethos—might be useful in regaining public trust in science. However, they are unfit to restore trust within the scientific community. As the basic values of science are well known to all scientists, there is no need to codify them in order to make scientists aware that they exist (Merton, 1942). External watchdogs—be they the media or federal agencies—might therefore have the adverse effect of destroying trust in the informal networks of the communication system. A discursive “spiral of mistrust” (Weingart, 2005) might develop, triggered from the outside and then amplified within science.

The imprinting of normal misbehaviour, especially with regard to future generations of scientists, threatens the foundation of science as an institution

There is empirical evidence for this spiral of mistrust. Data from national committees on scientific dishonesty show that there has been no significant increase in FFP in recent years, whereas the number of allegations is on the increase. The ORI recorded 267 allegations in 2004, which was the highest number since tracking began in 1989; however, there have been, on average, 13 individuals sanctioned per year. Germany displays a similar trend, although the number of allegations is much smaller (Ombudsman der DFG, 2006). Fuchs & Westervelt (1996) saw a

link between intense competition and conflict in frontier or revolutionary fields, and the occurrence of hostility, envy and mutual suspicion. Biomedical research is such a field, and our list of institutional circumstances strengthens this assumption. The increasing numbers of allegations might therefore be an indication of the erosion of trust among scientists.

The logic of science and the media’s logic of news selection work together to portray publicly the problem of scientific misconduct as the fault of individuals. Neither side can be blamed for operating as they do; however, the way that science and the media deal with the issue of scientific fraud detracts from the underlying problem: the institutionally induced deviant behaviour of many scientists. This behavioural pattern, if sufficiently ‘normal’ in frequency, will be much more damaging than the rare sensational case: it will discourage and disillusion researchers and result in the mentality that “everybody does it” (Kreutzberg, 2004). Against this background, educational programmes in research ethics for young scientists might be futile. The imprinting of normal misbehaviour, especially with regard to future generations of scientists, threatens the foundation of science as an institution.

Obviously, mere appeals to scientific bodies or the media to change their perspectives will have little impact. However, without expecting either side to change their perceptions, we suggest that both broaden their views. The first steps are to extend the scope from focusing only on FFP to including normal misbehaviour, and from emphasising misconduct to advocating a research environment that promotes integrity, as suggested by the Institute of Medicine and National Research Council Committee on Assessing Integrity in Research Environments (2002). A second step is to adopt a more thoughtful approach towards recent science policy dogmas. The current hype about evaluation, benchmarking and the creation of excellence in science has assumed the form of self-flagellation induced by the perceived withdrawal of public trust from science. Regardless of whether this perception is justified, we can only speculate on the unintended consequences of short-term evaluations for the culture of science. They will probably be dramatic and irreversible, because the fragile balance of mutual trust

and focused critique—a unique social invention—might be lost. Another equally efficient social mechanism of communication for creating reliable and trustworthy knowledge is not in sight.

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The willingness of society to provide substantial resources for research must be based on trust in scientific integrity, as sanctions—such as those found in the marketplace or in politics—do not exist. Recurrent spot checks of publicly funded research cannot replace internal quality control mechanisms. Not only are they far less effective, but also they aggravate the spiral of distrust. The world of science is not the same as the corporate world, in which reporting to the beat of a quarterly report might make sense. Probing the unintended consequences of external performance measures is therefore a genuine scientific challenge in the institution's own interest, and a report on the results—which might be expected to be counterintuitive—will certainly be fascinating news.

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