Scientific Research: Dilemmas and Temptations

Royal Netherlands Academy of Arts and Sciences

Scientific Research: Dilemmas and Temptations

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Foreword

Scientific misconduct can damage the quality of scientific research and the attitude of the public to scientific endeavour. Excessive pressure to perform, blind ambition, or the pursuit of material gain can tempt researchers to adopt a casual attitude to generally accepted rules. It is therefore important to question what is and is not permissible when carrying out scientific research, i.e. what constitutes “good scientific practice”.

This booklet, *Scientific Research: Dilemmas and Temptations*, is intended mainly as an aid to students and young researchers in developing their own sense of standards, but it is also relevant for more experienced researchers. It is based on actual research practice, in other words the problems and choices that arise during the various phases of a scientific study. This involves designing the experiment, collecting data, analysing and reporting the results, and the way those results are used. The booklet is not intended as a detailed and dogmatic guide to scientific practice. Scientific research is subject to constant change. It demands creativity and a talent for improvisation, and it is too varied and multifaceted to be the subject of a standardised system of rules and guidelines. Rather, this booklet is intended to encourage discussion of various issues so as to contribute to deliberate, responsible decision-making. The key question is always how one should act correctly from the point of view of science and responsibly from the point of view of ethics when designing, carrying out, and reporting on scientific research.

The Royal Netherlands Academy of Arts and Sciences (KNAW) has concerned itself with questions of what is desirable and undesirable in the field of science for a number of years now. In 1995, the Academy – together with the Netherlands Organisation for Scientific Research (NWO) and the Association of Universities in the Netherlands (VSNU) – published a memorandum on scientific misconduct. This led to a more detailed memorandum on scientific integrity (2001) and to the setting up by the Academy, the NWO and the VSNU of the National Board for Scientific Integrity (LOWI). It was in the context of these initiatives than the first version of this booklet was published in 2000. This new edition has been revised, expanded, supplemented, and where necessary corrected, partly in the light of comments and criticism on the first edition. The Academy hopes that the new edition will be used as teaching material in lectures and discussion groups and that readers and users will again pass on their own comments and suggestions to the Academy (knaw@bureau.knaw.nl).

Prof. W.J.M. Levelt
President of the Royal Netherlands Academy of Arts and Sciences

Introduction

Scientists are people of very dissimilar temperaments doing different things in different ways. Among scientists are collectors, classifiers and compulsive tidiers-up; many are detectives by temperament and many are explorers; some are artists and others artisans. There are poet-scientists and philosopher-scientists and even a few mystics.

Peter Medawar (1982: 116)

Scientific research has undergone explosive growth, particularly since the Second World War. Scientific knowledge and its applications have now permeated virtually every facet of our lives. In the past, the role of science was mainly apparent in the technical and biomedical disciplines, but nowadays scientific expertise is called in in the context of all kinds of areas, in politics and administration, industry and services, the law, and the media. There are hardly any aspects of life today that are not directly or indirectly dependent on science and technology. Scientific knowledge is therefore held in high regard and the skills and ingenuity of researchers are greatly valued by the public.

This growing role of scientific research in modern life means, however, that researchers themselves are increasingly held partly responsible for the harmful effects of scientific applications, for example environmental pollution or military technology. The question of whether or not research is acceptable is one that responsible administrators and members of the public no longer wish to leave entirely to academia. There has been a major increase in the amount of contract research, and external financiers increasingly demand a say in the research agenda. The general public too demand information about the opportunities and risks associated with technological innovations. These trends have blurred the distinctions between “basic” and “applied” science and between the developments within the field of science itself and interests outside that field. For researchers, this means that they are more and more required to take account of considerations that place major demands on their individual and collective responsibility.

There have also been changes in the world of science that focus attention on what is desirable and undesirable in the area of research. Pressure to get results and publish has increased enormously. Researchers can no longer be sure that their appointment will be permanent, research is no longer financed unconditionally, and in many cases funding has to be acquired in a competitive context. This has led to fiercer competition between researchers, resulting in a need for clearer rules and tighter checks by scientists on one another. At the same time, increased competition can make it tempting to let personal interests prevail above the interests of science.

In the past, it was uncommon to demand separate attention for rules of conduct and dilemmas in scientific work. The prevailing view was that researchers learned their trade by actually doing it, and in the firm belief that the authority of science was by extension bestowed on those engaged in it. These trends – upscaling, greater dependence on external clients, increased interest on the part of the media and public, fiercer competition between researchers – have led in recent years to a growing need to discuss – more openly and specifically than has hitherto been the case – the standards applying to desirable and undesirable scientific behaviour.

Concern regarding scientific abuses has grown in the last quarter century (Lafollete 1992, Drenth 1999), particularly in the United States. In the light of a number of articles in the press, the US Congress held a number of hearings in 1981 that exposed various questionable practices. One of these, the case of John Darsee, originally had to do with the invention of data, but another concern was soon added, namely the listing of co-authors who had not in fact been involved in the research or only to a very limited extent. This "honorary co-authorship" was said to be extremely prevalent and was seen as a threat to the integrity of researchers and public confidence in science. The media played an active role in the debate, and in their much-discussed book *Betrayers of the Truth* (1983), the science journalists William J. Broad and Nicholas Wade claimed that the revelations during the Congressional hearings were merely the tip of the iceberg. Although that suggestion was flatly contradicted by authoritative researchers, politicians expressed surprise that there were no specific procedures or bodies to deal with scientific misconduct. The ensuing discussion led to the creation in 1989 of the Office of Scientific Integrity within the National Institutes of Health (NIH), with the Office being charged with assessing the level of misconduct in the biomedical sciences. Three years later, in 1992, it was replaced by the Office of Research Integrity (ORI), which forms part of the United States Department of Health. The ORI investigates reports of misconduct, promotes scientific research into this problem, and takes action to prevent it. European countries have followed this American example in a wide variety of different ways.

> Denmark was the first European country to set up a national body (in 1992) to deal with complaints of scientific misconduct, the Danish Committees on Scientific Dishonesty (DCSD). For the past few years, these have been a set of three separate committees dealing with natural sciences, medical and social sciences, and humanities. France set up a national *Comité d'éthique pour les sciences* in 1994. In the United Kingdom and Germany, matters are structured in a more decentralised manner, with primary responsibility lying with the research institutions themselves. The UK's Medical Research Council (MRC) does however have a code of conduct, and since 1997 has had a system of regulations and procedures for dealing with misconduct. Germany's *Deutsche Forschungsgemeinschaft* (DFG) published an extensive report, with recommendations, in 1998. One of the things it recommended was the appointment of a national ombudsman with an advisory role, who would advise and perhaps arbitrate between an accused researcher and the institution concerned. In other countries, for example Poland and Turkey, it is the national academies of sciences that play an important role as regards assessment and the provision of information in cases of scientific misconduct.

In the Netherlands, the debate on this issue was greatly accelerated by the publication in 1996 of *Valse vooruitgang* [*Fake Progress*] by the science journalist Frank van Kolfschooten. In his book, Van Kolfschooten – like Broad and Wade in *Betrayers of the Truth* (1983) – examined a number of cases of scientific deception, this time in the Netherlands. Many of these cases had never been brought to public attention and most of them had not led to any sanctions. More recently, in *De onwelkome boodschap* [The Unwelcome Message] (1999), André Köbben and Henk Tromp dealt with a number of cases in which researchers in a wide variety of disciplines found themselves in conflict with their clients or superiors because their research had not produced the desired results. This had led to attempts to gag the researchers, to tamper with the research results, or to cover up those results. Whereas Van Kolfschooten deals with errors in the science itself, Köbben and Tromp focus on external threats to effective and reliable research.

In 1995, the Royal Netherlands Academy of Arts and Sciences (KNAW), the Netherlands Organisation for

Scientific Research (NWO) and the Association of Universities in the Netherlands (VSNU) published a joint memorandum on scientific misconduct [*Notitie inzake wetenschappelijk wangedrag*]. This recommended that general procedures and guidelines should be drawn up that organisations could fall back on when a case of misconduct was identified. The memorandum also stressed the need for greater clarity regarding the rules to which researchers should be subject. The memorandum on scientific integrity [*Notitie wetenschappelijke integriteit*] published in 2001 went into this in greater detail. With a view to any future infringements of scientific integrity, the universities concerned undertook to appoint confidential counsellors or committees, draw up codes of conduct, and set up a National Board for Scientific Integrity [*Landelijk Orgaan Wetenschappelijke Integriteit, (LOWI)*]. (LOWI was in fact set up in 2003.) Except for the institutes that fall within the remit of the Academy and the NWO, non-university research institutions, commercial research firms, and government research institutes are not covered by this arrangement.

The separate regulations mean that it is necessary to specify more closely what is meant by misconduct. In the United States, the term “scientific misconduct” is used to describe cases of fraud or plagiarism, i.e. the invention or falsification of research data or results, or the copying of words, data, or ideas from other persons or teams without their being properly credited (Rennie & Gunsalus 2001). In Europe, a somewhat different definition sometimes applies. For the Academy – as for the Danish Committees on Scientific Dishonesty, for example – the main concerns are scientific dishonesty and infringement of scientific integrity. This is a somewhat broader definition, which also covers various types of deception, for example, the wrong use of tests or controls, deliberate omission of unwelcome results, deliberate presentation of results in a faulty or tendentious manner, and undeservedly claiming credit as an author or co-author (KNAW, NWO, VSNU 2001). Deception can naturally be taken as a kind of falsification or deception, and thus to a certain extent as fraud, but it is still useful to consider it as a separate category. Deception is a more frequent occurrence and is often more subtle than the invention or falsification of research data or results. In addition to fraud, plagiarism, and deception, one can distinguish a fourth kind of undesirable behaviour, namely inflicting harm on persons or groups who are the subject of scientific research.

If one of these types of undesirable behaviour is reported, it may lead – depending on the prevailing rules and practices – to further investigation. The employer can impose sanctions – depending on the seriousness of the case – if that investigation confirms that undesirable behaviour has indeed occurred. Besides undesirable behaviour in the sense referred to, there is also conduct that may well infringe the rules for what constitutes proper and responsible research but that does not warrant further investigation or sanctions. This include such things as carelessness, negligence, not behaving as a good scientific colleague should do, etc. Shortcomings of this kind will only lead to the imposition of sanctions in extremely serious cases.

This booklet deals with the whole gamut of desirable and undesirable behaviour in the context of scientific research, ranging from actual fraud and plagiarism to less serious types of undesirable behaviour. Relatively little research has been done into the scale of such scientific abuses and how they occur. Some authors believe that fraud and deception in basic scientific research are remarkably rare compared to other areas (Holton, 1995: 108). Someone who falsifies or invents results or plagiarises has to hoodwink his most expert colleagues, and if he does actually manage to do so then he will not be successful for very long. “If someone wants to earn their living by fraud, then they would do better to choose a different occupation than scientific research.” (Borst 1999: 185)

The restricted amount of quantitative data available supports this view. The US National Science

> Foundation finances tens of thousands of projects each year in virtually all scientific disciplines. These result in an annual total of between 30 and 80 reports of misconduct being submitted to the relevant supervisory body, the Office of the Inspector General. Of these complaints, an average of one in ten are determined to be well founded (cf. DFG 1998). The Office of Research Integrity (ORI) monitors some 2200 American institutions carrying out biomedical research, including the well-known National Institutes of Health (NIH), the Food and Drug Administration (FDA), and the Centers for Disease Control (CDC). In the first five years of its existence (1993–1997), the ORI received about a thousand complaints. Of these, 218 were considered further, with 68 not going beyond a preliminary investigation, 150 being investigated in greater detail, and 76 leading to the conclusion that scientific misconduct had indeed taken place, i.e. that there had been falsification or invention of data or plagiarism (ORI 1998). Based on these incomplete figures, the number of officially registered cases of scientific misconduct comes to around twenty a year for the whole of the United States and for all scientific disciplines.

But even if scientific misconduct is rare, when it does take place the consequences are very serious, both for the researchers concerned and for the reputation of scientific institutions. There is also a real likelihood that the volume of such misconduct is in fact increasing due to the trends we have already looked at and through use of the Internet (Drenth 1999). Besides what is considered in the United States to be scientific misconduct, there also less serious types of undesirable behaviour. Köbben, who has carried out the most extensive investigation in this field in the Netherlands, speaks of “venial sins”. He also believes that scientific “mortal sins” are committed relatively rarely but that “venial sins” are frequent and if ignored may become more or less a matter of course (Köbben 2003: 65–69). Borst makes a somewhat similar distinction between actual fraud and doctoring one’s results. Even though scientific fraud is relatively rare, Borst believes that we should pay attention to it. It constitutes cheating, can have all kinds of harmful effects, and should basically lead to the imposition of sanctions, for which sound and precise legal procedures are required. Doctoring results is more frequent than actual fraud and above all demands that there be clear rules and an effective system of social control (Borst 1998, 1999).

If we consider scientific research in a broader sense than merely basic research, then the situation is undoubtedly more problematical. Although the scale at which abuses occur is not precisely known, a large number of problems have been revealed in recent years regarding contract research, sponsoring, and “sidelines” engaged in by researchers. Because their interests may conflict with the results of a study, clients sometimes put pressure on the researcher to alter the design, results or reporting in a way that makes them more favourable from their point of view. Researchers may also do this of their own accord, even if they do not have a direct interest in the results. The greatly increased amount of external financing for scientific research has made these problems extremely pressing in a number of research fields (see Chapter 6).

Like that published by the US National Academy of Sciences (*On Being a Scientist: Responsible Conduct in Research*, 2nd edition 1995), the present booklet considers the problems and dilemmas that researchers may find themselves faced with nowadays. It is not intended to specify with absolute legal precision just what does or does not constitute misconduct, or to determine precisely what should and should not be permissible. The differences between research areas and disciplines are too great for that kind of detailed discussion and the final responsibility for determining whether misconduct has taken place generally lies with a specific body – a disciplinary tribunal, professional association, or institutional committee – for each particular discipline or research institute. If the person who is the subject of a complaint or the person submitting it is dissatisfied with a decision by the competent body, they can submit their complaint in the second instance to the National Board for Scientific Integrity (LOWI), which will then provide recommendations to the employer (with a copy going to all those concerned).

A realistic discussion of the problems that may face today's researchers first of all involves clarifying the basic rules of scientific research and the dilemmas and temptations that may arise. But discussing these basic rules is not really a question of ideals of knowledge or basic principles of the philosophy of science. Science comprises a very wide range of different styles of research (cf. Crombie 1994). Some researchers adopt experimentation as their primary working method, others are more naturalistic and inductive, while yet others make use of a strictly hypothetical-deductive model or devote themselves to theoretical systematisation. This variety means that there is room for a wide range of talents and temperaments, and it also allows for a wide range of different views on matters of epistemology.

Nor does discussing these basic rules primarily mean considering the moral qualities that researchers should display. According to Sir Francis Bacon (1561–1626) and many others like him, those in the service of science should be subject to the highest possible moral standards. They should be disinterested, impartial, indifferent to authority, and solely and exclusively concerned with finding out the truth. But researchers who consider their own work objectively sometimes come to the conclusion that the ideals propounded by Bacon are seldom in line with the realities of actual research. The physicist F.W. Saris, for example, decided on the basis of his own diaries that it is not just disinterested curiosity that plays a role in research but also "belief and emotions, fashion, honour and fame, friendship and envy, fanaticism, and intellectual laziness." (Saris 1991: p. 22) Like many historians and sociologists of science, he therefore favours a more realistic view of science and its practitioners.

An exclusively ethical view of research may unintentionally stand in the way of carrying out effective and responsible scientific work. Researchers hope, for example, that their achievements will receive the appropriate recognition, something that could be construed as contravening the standard of disinterestedness. If one were to apply a strict interpretation of disinterestedness as one's standard, one would be doing a disservice to science. Standards of moral behaviour that sound convincing may therefore be at odds with effective scientific research (Woodward & Goodstein 1996).

Rather than assessing science primarily in the light of philosophical or moral principles, there is more point in doing so on the basis of actual practical research, and thus on the basis of the fact that researchers need to take account of a range of different interests and values. A central role in all this is naturally played by the interests of the research and science itself, but that is not the sole consideration that researchers need to bear in mind:

> "Apart from the interests of science and the research object, one can also consider the interests of society (or segments of society) and also those of other researchers and of clients. It is essential that none of these interests is given absolute priority, and one must also remember that they may – and often will – conflict with one another. Depending on the particular situation, the researcher will need to make a choice, doing so after careful appraisal of all the facts concerned." (Köbben 2003: 44)

This booklet primarily concerns rules and practices on which there is a considerable measure of consensus among scientists. Its focus is on the professional quality of scientific research and the scientific integrity of researchers.

This approach means that, depending on the topic, issues of scientific theory and ethical and social matters may also be considered.

Each chapter deals with a general question, beginning with trust and deception. We then consider various facets of the research process, from the collection of data to publication. Chapters then follow on the influence of such external factors as applications, contract research and the media, and the problems that may arise. Most of the chapters conclude with examples of borderline cases or dilemmas, illustrated using actual cases suggested by members of the Academy. Although these cases are real, they are only described briefly in broad outline and the names of the persons involved have been changed. The point is not to pass judgment on those persons but to discuss the problems that they faced. Separate text boxes deal with a number of striking recent examples, and here actual names and details are given. Here too, the intention is not to pass judgment on those involved but, by using published examples, to provide material for discussion of the issues focused on in this booklet. The final chapter deals with remedies and prevention and is followed by a bibliography.

It is important to consider the temptations and dilemmas involved in scientific research not only so as to make a clearer distinction between what constitutes desirable and undesirable conduct. Doing so can also contribute to the quality of scientific work and to increasing the level of trust between researchers. Focusing on these matters can also have a favourable influence on the attitude of the general public to science.

Deceptive elegance: the graphs of Jan Hendrik Schön

September 2002 saw the publication of a 127-page report by a committee of Bell Labs, the renowned research laboratory of Lucent Technologies, on the 32-year-old German physicist Jan Hendrik Schön. Although known as a brilliant and exceptionally productive researcher – in 2001 he published an article almost every week – doubts had arisen about his work after no other research team had been able to replicate his experiments (Goss Levi 2002).

The report found that Schön had invented and falsified research data. In some of his articles, the same graphs were shown even though they were supposed to represent different measurements. Some of them were not in any way a representation of empirical results but only of mathematical connections. Some data also displayed a degree of statistical precision that was extremely unlikely. In 16 of the 24 cases investigated, the committee found that there had been “scientific misconduct”.

Schön was unable to refute the accusations because his laboratory logbooks had not been kept up to date and most of the measurements, which had been stored in digital form, had been deleted. The equipment set-ups that he had supposedly used to achieve his results were also no longer available. There were also no witnesses to his experiments, because he had almost always taken measurements and processed the data by himself.

The committee also concluded that all the co-authors of the 24 articles investigated could be acquitted of scientific misconduct. The committee had not found that they were guilty of contravening the rules of proper laboratory research. The misconduct that had occurred was attributable solely to Schön.

The report contained a response from Schön in which he admitted making mistakes but in which he also insisted that the results he had reported were based on tests that he had actually carried out. Schön had been in line for appointment as director of one of the Max Planck Institutes in his home country but he was dismissed with immediate effect.

This case led to a great deal of disquiet among physicists. Even though only a young man, Schön had already published an impressive series of articles in the top scientific journals. On a number of occasions, he reported success in experiments in which other researchers had been unsuccessful. In some cases, he then offered to cite the original researcher as a co-author. His behaviour was presumably due to a combination of a desire for prestige and overconfidence. His striving for prestige was accompanied by the conviction that he knew how things worked even without precise investigation (Köbben 2003: 67).

The Schön case is illustrative of another important issue regarding scientific misconduct, namely that of collective responsibility. Should his superiors, in particular his boss and co-author Bertram Batlogg, not share some of the blame? They were not guilty of scientific misconduct in the usual American sense, i.e. actual fraud or plagiarism, but that does not mean that they bore absolutely no responsibility for Schön’s fraudulent practices. One can surely expect the management of a research institute to ensure a working environment in which critical consideration of one’s colleagues’ results is part of normal practice (Service 2002, Borst 2002).

The question of shared responsibility can also be raised in respect of Schön’s co-authors. In a number of cases, he simply reported that experiments had been successful that had not led to the expected results for other researchers. Co-authors should not accept that. It is precisely in cases where a number of researchers work together, each contributing his or her own restricted expertise, that problems may arise regarding responsibility. Researchers whose disciplines, background, or research “culture” are different are not always in a position to properly assess one another's results. Nevertheless, when an article is published under more than one name, co-authors are considered to share responsibility. But just how far does that shared responsibility extend? In some cases, research results are withdrawn because the manner in which the experiments were performed or the way one of the authors reported on them cannot bear the test of serious criticism. Just what should happen in such a case? Is none of the co-authors to blame, or do they share responsibility for the error of one particular colleague?

"Somebody who puts his name to another researcher's article as a co-author should be familiar with the data on which the article is based. If that is not practical, he should at least know how the experiments were carried out, who was also involved, and how the data were processed. A superior should create a web of social control that will catch cheats at an early stage. Anyone who put his name as a co-author to an article with results invented by Schön was not being careful enough and is consequently also responsible." (Borst 2002).

1. Trust, deception, and self-deception

False facts are highly injurious to the progress of science, for they often long endure; but false views, if supported by some evidence, do little harm, as everyone takes a salutary pleasure in proving their falseness.

Charles Darwin (1871: 926)

Researchers must be able to rely on the results reported in professional publications being consistent with the results of the research that has been carried out. Without this basis of trust, scientific communication is impossible, and there can be no scientific discussion or accumulation of knowledge. This basis of trust is not the same as a blind belief in the data or ideas of other people. Trust is provisional and often incomplete. The findings of one research team need to be confirmed by other researchers, but the results, even though accepted, may very well be accompanied by justified doubts as to certain aspects of the findings or disagreement with the proposed interpretation or explanation.

Inventing or falsifying data is a form of deception and it infringes the mutual trust that can be expected between researchers. There are a number of notorious cases of this. One of these involves the eminent and authoritative British psychologist Cyril Burt (1883–1971), who was a lifelong proponent of the theory that intelligence is inherited. In order to support his views, Burt published test results concerning identical twins who had been separated and brought up in different environments. The close correlation of the test results was supposedly convincing evidence of the hereditary nature of intelligence. After his death, Burt's findings were called into doubt, at first during discussions at psychology conferences and later in various publications. According to one of his critics, the American psychologist Leon Kamin, there were serious shortcomings in Burt's accounts and reporting, and many of his claims regarding research he had supposedly carried out were contradictory. Burt had never published his original data but neither his published overviews nor his testing methods were properly accounted for. Kamin also points out that the correlations from various different studies were identical down to three decimal points, an accuracy that is statistically extremely unlikely (Kamin 1974). The biography of Burt published by Leslie Hearnshaw in 1979 revealed various other questionable matters. Burt had, for example, rewritten the history of factor analysis in a way that cast him in a highly favourable light, and had also falsified other data than that concerning research on twins. The British Psychological Society concurred with Hearnshaw's conclusions and the "Grand Old Man" of psychology was toppled from his pedestal.

In 1989 and 1991, however, two books were published that to a significant extent rehabilitated Burt. Their authors, Robert Joynson and Ronald Fletcher, argued that a considerable number of other explanations could be put forward for the shortcomings identified in Burt's work rather than the deception claimed by his critics. The supposed fraud was particularly difficult to prove because a significant amount of Burt's research material had been destroyed after his death on the advice of his critics. It was no longer possible to compare the raw data with the reported results, and Burt could no longer defend himself. The British Psychological Society reversed its position and the controversy came to focus on Burt's research on twins. In 1997, the American psychologist W.F. Tucker reconstructed the circumstances under which Burt had produced his results, comparing them with a large number of other findings of research on twins. Tucker's conclusion was that Burt was guilty of fraud beyond any reasonable doubt (Tucker 1997).

Although opinions differ as to Burt's merits and failings, this case is a good example of a researcher who was so totally convinced of his theory that he neglected his responsibilities as a researcher and made use of improper means to defend his ideas (cf. Köbben 1991). Objections to Burt's ideas were initially not taken seriously because his thinking on the hereditary nature of intelligence was shared by a significant proportion of the British elite (Hearnshaw, 1979). It was only when younger researchers came up with different data and used them as the basis for interpretations different to those that Burt had propounded throughout his career that he was tempted to silence them with results that nobody could ignore. But those results, as Kamin had already noted, were too good to be true.

Burt's actions, just like any other kind of conduct – whether desirable or undesirable – might have arisen from cool calculation. Based on a strategic consideration of the risks and opportunities, the costs and benefits, the researcher may take refuge in misrepresentation or deception, despite that being contrary to the rules of scientific research. Two effective ways of combating this kind of undesirable conduct are to increase the risk of being caught and the penalties available.

Cases of actual calculated fraud are probably outnumbered by those in which the perpetrator first deceives himself by justifying his conduct in his own eyes. If he is then found out, he has his justification or neutralisation ready, and appears entirely convinced that he has not actually done anything wrong. Yes, perhaps he has made mistakes, but is that really his fault or did something go wrong with the lab set-up? Perhaps there are some shortcomings, but everyone knows that "you can't make an omelette without breaking eggs". Perhaps the results are incorrect, but the model is perfectly all right.

This mechanism is also known from other cases in which rules are contravened. People who contravene generally accepted rules very often do not do so because they themselves adhere to entirely different standards or due to cool calculation. Most of the time, they are in agreement with the prevailing standard, but they decide that it does not apply to their own particular situation (Sykes & Matza 1957). In order to justify being an exception, they invoke a higher interest, disavow responsibility, deny that their actions are harmful, or reject criticism by explaining that their critics are hopeless, or that many other scientists – even really eminent ones – do the same kind of thing…

When caught in the act of contravening the rules, the perpetrator becomes entangled in his own web of reasoning and self-justification. Precisely because scientific deception is frequently accompanied by this kind of self-deception (Klotz, 1986), it is important that the rules of conduct should be clear, as well as the background to those rules and the risks attached to contravening them.

> Researchers who defend themselves against accusations of misconduct are often prepared to admit that they have been careless or have made mistakes, but they frequently deny emphatically that they have been guilty of misrepresentation or fraud. In 1996, the Leiden psychologist René Diekstra was accused of plagiarising many of the pages in a popular psychology book. The case was exposed in a series of articles in the weekly news magazine *Vrij Nederland* and was one of the most frequently discussed scandals in the press for several months. Diekstra turned out to have copied at least 30 to 40 pages without acknowledging his sources, or at least without doing so in the proper manner. A committee set up to investigate the case not only confirmed Diekstra's indiscretion but also found that he was involved in plagiarism in a scientific article. He thereupon resigned his position at Leiden University. In one of

> his first public interviews during the course of the scandal, Diekstra emphasised that his writings had a higher purpose: “I actually always set my sights higher in my popular works than merely citing sources and giving references” (quoted in Abrahams 1996). This attitude that the end justifies the means meant that Diekstra lost sight of the accepted rules (quoted in Heuves et al., 1997: 182). Although he admitted having infringed copyright, he denied being guilty of plagiarism. The standard Dutch dictionary defines plagiarism as “copying passages, thoughts or reasoning from others and passing them off as one’s own work”. According to Diekstra, there was no question of the latter. He had never intended to pass off the passages concerned as his own work; he merely wished to circulate the results of others (cf. Diekstra 1998). His attempts to clear his name were in fact partially successful and a few years later he was appointed to the post of *lector* at the Hague University of Professional Education [*Haagse Hogeschool*] and to a professorship at Roosevelt College in Middelburg.

Like inventing or falsifying data, plagiarism can also be seen as a kind of fraud, although the negative effects for science are less clear-cut than those of fraud. Misappropriating the work of others without citing the source in the usual way makes one’s own work appear more independent and original than is actually the case. In the scientific world, that is unacceptable. Unlike administrators and politicians, who can have their staff and advisers write speeches and publications for them, scientific researchers are expected to carry out their work for themselves and publish it under their own name. Plagiarism comprises not just copying someone else’s work under one’s own name but also the unauthorised misappropriation of data, phrasing, or ideas. There are various gradations of plagiarism, running from casual borrowing to systematic copying, and the causes are also very varied, ranging from forgetfulness to actual cheating.

Within the boundaries of the legal provisions on intellectual property, free use can be made in scientific research of all the work ever carried out by other researchers. Researchers do not need to pay money for using the products of all those efforts, nor do they have to do anything in return. There is, however, one condition. Researchers using someone else’s results are not allowed to pretend they have discovered or thought up something for themselves if it is in fact derived from the work of another scientist, unless the insights concerned can be considered to be general knowledge. References and citations are therefore a standard part of the acknowledgements section of every scientific study. This lets the reader know about the work that has gone before, and the citations are also a kind of symbolic reward for previous researchers’ achievements. Omitting to cite one’s sources in the proper way is therefore not only misleading but also intellectually discouraging.

It has sometimes been argued that these strict rules regarding citations should not necessarily apply to non-specialist publications; this was in fact argued in defence of the psychologist René Diekstra. But copyright applies to all publications, as does the principle on which it is based, namely the obligation to recognise and respect other people’s intellectual property. This does not mean that in a popular work one must necessarily provide citations in the same way as in real scientific publications, i.e. by means of footnotes, exact references, etc. But misappropriating someone else’s ideas and passing them off as one’s own is not permitted in the world of science. This also applies to papers, essays, and reports written by students that are not intended for publication. Plagiarism by undergraduates and PhD students also amounts to a kind of examination fraud. The Internet has greatly increased opportunities for making use of someone else’s texts without permission.

Depending on the seriousness of the case, discussions of fraud often concern who is actually responsible and what action should be taken. But it is not merely the person who cheats and the person who is cheated who are concerned: in many cases, co-authors, supervisors, assessors, publishers, academic directors, and editors of periodicals are also involved. Just how far does the responsibility of co-authors and close colleagues or superiors actually extend? What is the appropriate action to take against a researcher who has invented answers from respondents? Should he simply be reprimanded, should he be suspended, or is it sufficient that his misconduct has been exposed? Should a trainee research assistant who is guilty of plagiarism be excluded from the research school or should he be given a second chance?

Editor of a scientific periodical

An interesting book is published about youth culture in the Netherlands during the First World War. The editor of a Dutch history periodical then asks a colleague who is familiar with the material to write a review. While reading the book, the reviewer sees that certain passages are highly reminiscent of an article that she recently read by the sociologist Van der Steen. Closer comparison shows that this is definitely not just coincidence and that the author of the book has copied sections without citing his sources. In her review, the reviewer says that “The author has based his work largely on research carried out by others and in some cases has copied passages literally, without always being careful to follow the rules regarding citing one’s sources. However, his innovative approach and surprising findings more than make up for this.”

Questions: Should the editor publish this review without himself investigating the case? In the final sentence quoted, the reviewer would seem to be condoning the plagiarism by saying that the author has nevertheless produced an innovative and surprising study. Is there anything to say for that reasoning?

2. Care and carelessness

Melodramatic as allegations of fraud can be, most scientists would agree that the major problem in science is sloppiness. In the rush to publish, too many corners are cut too often.

David L. Hull (1998: 30)

Scientific research generally complies with strict requirements regarding the care with which it is carried out. The relevant standards have become more comprehensive and stringent in the course of time. A scientific article published today complies with standards that did not apply a century ago, or only to a lesser extent. A wide range of techniques, instruments, and procedures have gradually increased the reliability of scientific work: the introduction of experimentation, the application of mathematics, and the founding of independent scientific societies that draw up their own procedures for assessing articles. Specialist periodicals and laboratories came into being as early as the eighteenth century. Statistics came to play an important role in many scientific disciplines in the course of the nineteenth century, while in the twentieth century structures were created for large-scale research involving collaboration between many different specialists: “big science”.

The care and precision required in scientific research first of all involve the design of the study and the way it is actually carried out. Hypotheses must be drawn up skilfully and carefully tested; data must be collected and processed meticulously; and reporting on the study must comply with stringent requirements for its precision and consistency. If the researcher fails to comply with one or more of these requirements, then his manuscript or publication will not easily convince other scientists. In fact, they will be the first to realise that something is wrong with the argumentation or the evidence.

Extra requirements regarding care apply in studies that make use of test subjects or patients. Clear rules apply as set out in the Declaration of Helsinki, which specifies how test subjects and patients are to be dealt with; there are also rules of “good clinical practice” (GCP) that apply to this kind of research.

Prior to the start of the study, the test subjects must be informed of its purpose and what the consequences may be of their participating. One can then speak of “informed consent”: it is only the explicit agreement of the test subjects and patients based on that information that can justify their participation in the study. In some cases, the nature of the research requires that the test subjects are not in fact aware of the actual purpose of the study. In such cases, it is important that they are clearly informed afterwards (in a “debriefing”) of what the study was intended to achieve and why it was necessary for this to only be revealed subsequently. Of course, the study still needs to have been justified and carried out with great care, because test subjects can sometimes sustain psychological damage by participating. Where research in history and the social sciences is concerned, it is important to ensure the confidentiality of the material and of personal details (KNAW, Sociaal-Wetenschappelijke Raad, 2003).

When testing drugs, pharmaceutical companies are dependent on patients who are being treated by doctors and university researchers. The fact that drugs research has become a large branch of industry, with major interests at stake, means that the care and independence of the research may become an issue. Great vigilance is necessary in this area, particularly where the financial interests of researchers and their clients are at odds with the interests of patients (cf. Angell 2004).

Over the past few decades, there has been a great deal of public discussion of the use of experimental animals. Research using animals has to be approved in advance by specially appointed committees. Although the number of animals used experimentally has fallen in recent years, animal testing is still an essential part of medical-biological research. The scientific effectiveness of such research has been greatly improved by combining it with in vitro techniques such as cell and tissue culturing, and this has meant that fewer experimental animals are needed. However, reducing the number of animals involves the risk that researchers will carry out experiments on too few animals, meaning that no clear conclusions can be drawn from the study. This amounts to a waste of these animals.

In order to keep up, researchers find themselves forced to work quickly and not to delay in publishing their results. Every scientific discipline has had more or less recent examples of conflicts regarding who was first to make a discovery or arrive at an insight. If pressure of time is greatly increased, then there is a temptation to work more quickly than is really sensible, to hastily round off experiments, and not to be too concerned about the necessary checks and tests. This can lead to errors and carelessness.

If a researcher has made a mistake but it has already been published, then a rectification should be issued. This should preferably be in the same periodical as the original article. If this is done quickly and unambiguously, then the researcher will rarely come in for blame. Errors made in good faith – just like differences of opinion regarding the results produced – are something entirely different to misrepresentation and fraud, and do not constitute any kind of scientific misconduct.

Carelessness is more difficult to correct. If it happens more than once, then the researcher is risking his reputation and may no longer be taken seriously by fellow scientists. Carelessness and negligence detract from the quality of research. They undermine the significance of the results generated by a study and if they are publicised then they also damage the reputation of the researcher. Negligence regarding patients and test subjects damages both them and the good name of science and the institution concerned.

The final experiment

Mark remembered it well and still felt uncomfortable about it. He had almost completed the final chapter of his dissertation and had to check it just one more time. The date of his formal PhD ceremony had already been scheduled, and he had already been awarded a grant for the post-doc position that he had applied for. Everything was just fine. There was just that one last check to do. He repeated it five times and although everything indicated that the result should be negative, it constantly came out as positive. The result was not really clear, but still: after such a check, he could not write the chapter as he had done.

He made one more major effort and this time the result was unarguably negative. The student research assistant in the same lab, who had shared in his euphoria, had objected: “I think you must have used the cell culture that I already showed was infected last week.” No, Mark would have noticed that. He wasn’t open to being convinced. His supervisor was glad that everything now fitted together properly because he had already planned a follow-up project on the basis of the results of Mark’s final chapter.

About a year later, when Mark was in the United States, he received a barrage of e-mails. His supervisor’s follow-up project had been approved, but the student who was acting as his research assistant had been unable to replicate the results in Mark’s final chapter – he always got a positive result. Mark was asked to say precisely what he had done. No doubt the new research assistant had made some kind of error.

Mark made a whole series of suggestions. Was the pH of the medium correct, and had the cells actually been cultured in just that one medium of that particular brand? Because he had already seen that differences in the medium could affect the results. But nothing helped. The research assistant was so frustrated that after about a year’s work he switched to something else, and Mark’s last chapter was never published in any of the scientific journals. His relationship with his old lab has never been very good since then.

Did Mark feel guilty? He did a bit, of course, but on the other hand the pressure to write that final chapter – “the crowning glory of your dissertation, your scientific visiting card”, as his supervisor had so often said – had been really enormous. And naturally, you can’t make an omelette without breaking eggs.

Questions: What is the actual problem here? What responsibility is borne by Mark, his supervisor, and the research assistant who shared the lab with Mark?

Case study

The Baltimore affair

One of the most notorious and controversial examples of supposed scientific fraud is the American Imanishi-Kari/Baltimore case. This case, the subject of a book by David Kevles, is interesting because of the complexity of the affair, the fluctuating assessments of it, and the length of time it lasted.

In 1986, the scientific journal *Cell* published an article by six authors on experiments with transgenic mice. The article was the product of collaboration between two teams, one led by Nobel prize-winner David Baltimore and the other by the immunologist Thereza Imanishi-Kari. Baltimore and his co-workers Waever and Albanese were responsible for the molecular biology component, and Imanishi-Kari and her co-workers Reis and Constantini for the serological analysis. Research carried out by the two teams had shown that modifying the animals' DNA appeared to stimulate the production of antibodies.

A post-doc researcher and close collaborator of Imanishi-Kari, Margot O'Toole, carried out a follow-up study but was unable to replicate certain results published in the *Cell* article. Upon consulting the logbook of Imanishi-Kari's co-worker Moema Reis, O'Toole discovered that the data recorded there were not in line with the published data. A number of the experimental results that formed the basis for the article were not in accordance with the report of the observations. O'Toole complained about her difference of opinion with Imanishi-Kari to an immunologist at Tufts University, where Imanishi-Kari had applied for a position. Tufts appointed an ad-hoc committee to examine the case, which interviewed both Imanishi-Kari and O'Toole. The conclusion was that the article contained two errors, but that these were not such as to require correction, let alone withdrawal of the article. There was then a discussion between the main protagonists, chaired by Herman Eisen, a respected professor at the MIT cancer institute. The results of the *Cell* article were perhaps not fully substantiated, but a closer understanding of them would need to be based on follow-up studies. The authors of the article decided not to write to the editors of *Cell*. Baltimore did, however, suggest that O'Toole should write a letter setting out her views and that he would respond to it. O'Toole decided not to do this for fear that it would prevent publication of her own article.

At the point when all this happened, there was considerable interest in the United States in the topic of scientific misconduct. Hearings were taking place and research involving genetic modification was coming in for very critical attention. In fact, a number of American states introduced legislation prohibiting the use of recombinant DNA technology.

In the light of these discussions of the *Cell* article, which had only involved a small number of people, a former colleague of O'Toole contacted two researchers at the National Institutes of Health (NIH), Walter Stewart and Ned Feder, who were concerned with the topic of scientific misconduct. They carried out an investigation of the seventeen pages of Imanishi-Kari's logbook and concluded that her experiments were not in line with some of the main findings of the *Cell* article. On the basis of an internal recommendation, however, the NIH did not allow Stewart and Feder to publish an article about the matter. They were only permitted to do so after intervention by the American Civil Liberties Union. Their article was rejected by both *Cell* and *Science* on the grounds that it was not a scientific paper and that it would be better for the accusations to be dealt with by a committee of inquiry. This had also been the initial view of the NIH.

In the meantime, Stewart and Feder had contacted a Congressional subcommittee. In 1988, Representative John Dingell was holding hearings on fraud in NIH research programmes. His primary concern was with the squandering of taxpayers' money and manipulation of research results. After the hearings by Dingell's committee, the NIH also set up a committee of inquiry. Talks between that committee and the authors led to the latter correcting a number of errors in a letter published in *Cell* in November 1988. The final report of the committee concluded in January 1989 that the article contained serious inaccuracies and omissions, which the authors should preferably rectify in a letter. The committee found no evidence of deception or of deliberate manipulation or distortion of research results.

Dingell's subcommittee pursued its investigation of the affair independently of the NIH, aided by specialist document examiners of the US Secret Service, who investigated Imanishi-Kari's logbook for evidence of falsification. During one of the subcommittee's hearings, in May 1989, the Secret Service investigators stated that twenty percent of the data in the logbook was questionable.

In March 1989, in response to the investigations and the activities of the Dingell subcommittee, the NIH set up the Office of Research Integrity (OSI). This body would in future investigate cases of misconduct at institutions subsidised by the NIH. The investigation regarding the *Cell* article was then officially reopened. In March 1991, a confidential draft report was produced stating that Imanishi-Kari was guilty of "serious scientific misconduct", including falsifying and inventing data. Three advisers endorsed this conclusion, while two did not. There was also disagreement as to the criterion of proof to be applied. Was "preponderance of evidence" sufficient, as the author of the draft report believed, or should the proof be "beyond reasonable doubt"?

The draft report was leaked to the media via Dingell's subcommittee and a scandal blew up. In the ensuing uproar, four of the six authors withdrew the *Cell* article, but Imanishi-Kari and her co-worker Reis refused to do so. Not long after this, David Baltimore resigned as President of Rockefeller University.

The Office of Scientific Integrity (OSI) came in for fierce criticism, however. It was said to have acted as investigator and judge at the same time, with the researchers who were suspected of fraud not being given the opportunity to defend themselves. It had also supposedly acted arbitrarily in pursuing cases of scientific fraud. The OSI failed to survive all this criticism and in 1992 was replaced by the Office of Research Integrity (ORI), which was made part of the Department of Health rather than the NIH. The ORI was expanded, particularly by recruiting lawyers. It gave those who were the subject of accusations the right to inspect all the documents, to have counterchecks carried out, and to appeal against rulings. In 1994, the ORI finally came to the conclusion that Imanishi-Kari was guilty of manipulating research data and attempting to conceal this by means of further manipulation.

Making use of the new procedures, Imanishi-Kari submitted an appeal. The hearing by the Appeals Board was in fact the first opportunity for both the ORI and Imanishi-Kari's lawyer and expert witnesses to have access to all the documents and to cross-examine one another and one another's witnesses. On 21 June 1996, all nineteen charges against Imanishi-Kari were dismissed. There was no question of scientific misconduct and in 1997 Tufts offered her a permanent position as associate professor. That same year, David Baltimore was appointed President of the California Institute of Technology.

This affair, and its long-term reverberations, is interesting for a number of reasons. As a post-doc researcher, the whistleblower, Margot O'Toole, was not in a position to convince other researchers that Imanishi-Kari was guilty of fraud. Initially, she refused to explain her difference of opinion in a letter to *Cell*. It was only when Stewart and Feder became involved in the case and informed the Dingell subcommittee that O'Toole became part of a much more far-reaching struggle. Although her scientific career fell into decline, her accusations made a bigger impression outside the university, and she even received an award for the courage she had demonstrated in exposing scientific misconduct.

The communication difficulties between David Baltimore and Imanishi-Kari, the two main authors of the original article, were undoubtedly one of the causes of the controversy. They came from different scientific backgrounds, did not work at the same laboratory, and Imanishi-Kari, who was of Japanese origin, spoke only poor English. The actual subject of the research was also complex, concerning two different issues. Are antibodies produced solely by mutated cells or by both mutated and natural cells? Was it possible with the reagent used to make a sufficient distinction between transgenic antibodies and ordinary antibodies?

The growing interest of administrators and politicians had more to do with the prevailing political situation than a concern for scientific accuracy. The case was dealt with by a succession of bodies – ad-hoc committees, the NIH, a Congressional subcommittee, the OSI, and the ORI – with varying motives. Some of those involved acted partly for political reasons. Moreover, the regulations and the criteria for evidence were subject to change during the course of the affair.

3. Completeness and selectiveness

The great tragedy of science – the slaying of a beautiful hypothesis by an ugly fact.

T.H. Huxley (1870: 244)

The results produced by a study are not always in line with the insights or expectations of the researcher. Such results may be ignored so that the study concentrates on the results that are indeed according to expectation. Although this tendency is understandable, going along with it detracts from the quality and significance of the research.

Basically, all outcomes and results must be taken into account when processing and producing the analysis. The frequency of the phenomena identified must also be clear, as well as whether the results are significant and what scope the conclusions have. Testing must make use of the proper statistical techniques. Reporting of the study should always attempt to be complete, although selection is unavoidable. A scientific publication is a reconstruction of the research process, but research results must be subject to checking and replication. Details that may seem unimportant while the research is being carried out may prove relevant for checking or replicating an experiment or observation. It is therefore essential for information regarding the course of events during the original research to be as accurate and complete as possible.

But striving for completeness cannot just take place in isolation. It relates to the problem addressed by the study and that must specifically be selective. The study report generally ignores findings that are unrelated to the research problem. Nevertheless, the required selectiveness in this regard must never lead to results being made to look better than they in fact are.

Conspicuous or exceptional outcomes ("outliers") require special attention, and simply omitting them is not merely improper but also unwise. Outliers can in fact give rise to new insights. Seriously anomalous findings therefore demand special attention, both during the course of the research and in the research report.

> "I advise students and researchers to give specific critical consideration to outliers. That is often where innovations are to be found. The discovery that people can respond differently to medication due to genetic predisposition came about because a researcher found that he himself, as his own test subject, was an outlier; he then went on to investigate that phenomenon." (D.D. Breimer, pharmacologist)

The dividing line between an acceptable "creative" interpretation of data and the dubious "massaging" of data is often blurred. Scientific articles in many disciplines are not constructed as research reports that precisely specify the different steps in such a way that they can be checked. In many cases, it is only the final result that is presented and this is accounted for using only a selection of the data collected and appropriate references. In this procedure, it is normal to omit material that appears irrelevant to the final result or that has not led to the proposed interpretation. But omitting irrelevant material should not be confused with manipulating unwelcome results.

Besides leaving out information, the contrary approach – focusing attention on certain connections at the cost of others – is also a form of selectiveness. Every researcher has the right to give preference to a certain interpretation and to emphasise it in his presentation by force of argument. The condition for this, however, is that it should be clear how that interpretation relates to the data. Other researchers should be able to see from the data presented that the material also allows for other interpretations.

Scientific discussions involve more than merely the observations or results of experiments. The methods used and the theoretical principles behind the study may also become the subject of scientific controversies. Disagreements may lead to the formation of different schools of thought. In some cases, researchers may become convinced that adherents of a rival school of thought are deliberately presenting matters in a misleading manner. However, complaints of misconduct need to be clearly distinguished from differences of opinion regarding theoretical or methodological principles. Such differences should lead to scientific debate, something that is separate from questions of scientific integrity.

If the impression arises that problems of selection or interpretation are due to non-scientific preferences or interests, then a study can quickly become problematical. In such cases, the question of what constitutes scientifically responsible behaviour – or goes beyond it – can lead to major disagreement. This can be illustrated by the reception accorded to Björn Lomborg's book *The Skeptical Environmentalist* in 2001. The pointed conclusions of the book and the way in which it selected and presented information led to its becoming the subject of a complaint against Lomborg to the Danish Committees on Scientific Dishonesty (see text box).

Researchers in the social sciences and humanities are to an extent dependent on people who provide them with information orally. This can lead to specific problems because such information may be sensitive from the point of view of privacy, can cause damage, or can be manipulated to protect or damage certain interests.

Concealing negative results

In the early 1950s, a research team published an article on the development of an inflammation (glomerulonephritis) of the filtration units (glomeruli) in the kidney. New techniques for demonstrating protein precipitates in tissues allowed the team to put forward a plausible case that the precipitation of proteins in the glomeruli was responsible for the inflammation.

The team argued that the antigen-antibody complexes that developed in the blood circulation could precipitate in the glomeruli. They achieved international recognition for this discovery and their theories led to a new way of thinking about certain immunological responses that significantly altered ideas on immune responses.

Their study of the assumed method whereby antigen complexes are precipitated is relatively simple to test in an animal model. Nevertheless, none of the researchers was able to localise injected immune complexes in the glomeruli. This negative result of the experiments was concealed because it did not fit in with the team's theory.

Another research team then showed that glomerulonephritis is not caused by precipitates from the circulation but by the local development of immune complexes. This finding was ignored by other researchers, led by the team that had postulated the circulation theory. Their fame was such that the members of the team even succeeded in preventing publication of an article on the local development of immune complexes.

It was not until three years after the local development of immune complexes had been identified as the cause of the inflammation that the article describing this theory was accepted. This then gave rise to a stream of publications consigning the first theory of pathogenesis to the realm of fiction.

Questions: With hindsight, it is often easy to identify the weaknesses of a theory. But how can we reduce the risk of a correct research result being rejected in the first place because it does not fit in with prevailing ideas? What responsibilities do the editors of a scientific periodical have in a case such as that described above?

Explanation or prejudice?

The economist Smelsoen publishes an extensive study putting forward a theory of economic growth. Working on the basis of a large quantity of statistical material on industrialised countries, he argues that there is a negative link between growth and the degree of unionisation of an economy. The more institutions for consultation and decision-making that exist in a particular country, the lower its rate of economic growth. But the tables included in Smelsoen's study show that this theory does not always hold true, and is incorrect even for such major economies as France and Japan. Smelsoen ignores this in his argumentation.

A review in a professional journal accuses Smelsoen of shoddy research, claiming that he has dealt selectively with the facts and systematically ignored contrary examples. In actual fact, says the reviewer, what Smelsoen is doing is pushing his pet neo-liberal ideas under the guise of science. Smelsoen is given the opportunity to respond but he refuses to do so, saying that the reviewer's comments are merely "slurs on his good name".

Questions: What requirements should Smelsoen's material and argumentation definitely need to meet? Is the reviewer's judgment justified? Can the editorial board of the periodical publish a review of this kind?

Sparing someone's sensitivities

Irene is a historian and is writing a biography of a deceased politician whose career was in the 1950s and 60s. It turns out that he in fact played a questionable role during the Second World War, but that this did not have any negative effect on his later career. Irene naturally wants to deal with his record during the war and she gets the full cooperation of the politician's youngest daughter. The daughter provides Irene with information – in the form of written documents – and puts her in touch with other people who can provide further information.

Irene then discovers in a post-war rehabilitation dossier on the politician, which had supposedly been missing, that the daughter herself had been at a school for social services trainees during the war and had a clear plan to go to work as a youth leader in Germany. The dossier presents this as a clear indication of the political sympathies of both father and daughter.

When Irene confronts the daughter with this discovery, the atmosphere suddenly changes. The daughter refuses any further cooperation and prohibits Irene from quoting from any of the documents that she has made available if Irene's biography of her father refers to her own intentions during the war.

Questions: What should Irene do? Should she continue her study without the daughter's cooperation; should she respect the daughter's wishes and thus deliberately present an incorrect picture of the wartime situation; or should she pretend to respect the daughter's wishes but then simply publish the awkward facts in her biography?

Case study

The Lomborg case and the Danish Committees on Scientific Dishonesty

The year 2001 saw the publication of the English edition of *The Skeptical Environmentalist* by the Danish political scientist and statistician Björn Lomborg. The original Danish version had appeared in 1998. In his study, Lomborg analyses the evidence for a number of frequent conclusions regarding current environmental and population issues. These include the growth of the world population and its future prospects, the impending scarcity of food and drinking water, declining reserves of energy and raw materials, pollution, deforestation, and the greenhouse effect. His conclusion is that in general the threats are significantly less than the environmental movement would have us believe and he accuses environmental organisations of making selective and misleading use of the official statistics produced by United Nations organisations (FAO, ICPP) and the World Bank. In contrast to the "lamentations" of certain environmental groups, he believes that there is no reason for pessimism. Lomborg concludes his 500-page book with the unambiguous final flourish: "Children born today – in both the industrialised world and the developing countries – will live longer and be healthier, they will get more food, a better education, a higher standard of living, more leisure time and far more possibilities – without the global environment being destroyed. And that is a beautiful world." (Lomborg 2001: 352)

As might be expected, publication of the book led to a torrent of reactions in the media and in scientific periodicals. The reception was immediately extremely varied. There were enthusiastic reviews in leading newspapers and magazines such as *The Economist*, but highly critical ones in *Science* and *Nature*, for example (for details of the discussion see, for example, www.lomborg.com). In November 2001, Lomborg was selected as a "Global Leader for Tomorrow" by the World Economic Forum; *Business Week* called him one of the "50 stars of Europe", and in February 2002 he was appointed director of Denmark's Environmental Assessment Institute.

An important role in the criticism levelled at Lomborg's book was played by a special dossier devoted to it by *Scientific American* (January 2002). The four experts who wrote the dossier set out a series of objections to the book. According to them, Lomborg had made far too little use of the specialist scientific literature and had only done so in a selective manner. He had consequently overlooked a great deal of relevant information and analysis, criticised views that enjoyed hardly any support among experts, and made frivolous predictions.

Early in 2002, a number of Danish researchers submitted a complaint against the book to the Danish Committees on Scientific Dishonesty (DCSD). The DCSD thereupon appointed a working party on 11 June 2002 to assess the book for "scientific dishonesty" in the normal way for scientific publications. But is *The Skeptical Environmentalist* in fact a scientific book? In September 2002, the working party published its report, with the conclusion that some of its members considered that the book was in fact a thematic work expressing opinions. Other members of the working party interpreted it as a scientific publication, referring to the fact that Lomborg had written it as an "associate professor" of statistics at the University of Aarhus. Moreover, the university listed it as a "research monograph" in its yearbook.

The complaints were investigated on the basis of the available information, including the views of the experts published in *Scientific American*. In December 2002, the DCSD came to two conclusions. In the first place, Lomborg had – objectively speaking – been guilty of "scientific dishonesty". Because of his "systematic one-sidedness in the choice of data and line of argument", he had clearly acted at variance with good scientific practice. On the other hand, there was no question of "intent or gross negligence".

The DCSD's decision led to a new wave of publicity, protest and debate, this time focusing on the DCSD's criteria and argumentation. A year after the DCSD's decision, in December 2003, the Danish Ministry of Science published a critical evaluation of the contested report. In the words of the Ministry, "the DCSD has not documented where [Lomborg] has allegedly been biased in his choice of data and in his argumentation… It is not sufficient that the criticisms of a researcher's working methods exist; the DCSD must consider the criticisms and take a position on whether or not the criticisms are justified, and why." The DCSD then decided in March 2004 not to reopen the case and to consider it as closed. A thorough new investigation would have taken too much time and money, given the fact that it had already been found that there was no "intent or gross negligence". Lomborg was pleased with this conclusion; the DCSD had finally recognised that its previous judgment had been invalid.

4. Competition and collegiality

The praise of ancient authors proceeds not from the reverence of the dead, but from the competition and the mutual envy of the living.

Thomas Hobbes (1651: 727)

Scientific researchers engage in amicable rivalry. They compete to produce better or quicker results, but that rivalry is reined in by the fact that they are highly dependent on one another. Every researcher depends on many others, for information and data, for comments and advice, for peer review, and then for recognition of the work produced. Where people are mutually dependent to such an extent, collegiality goes without saying. Infringing that spirit of collegiality produces collective disapproval and may also lead to the imposition of sanctions. Those sanctions are generally of an informal nature: someone who acts in an inappropriate manner towards his colleagues runs the risk of himself being treated in the same way and no longer being included in consultations and other shared activities.

The increasing importance of external financing and the fiercer competition between researchers means that they may be tempted to forget about the rules governing how fellow scientists should deal with one another. They may consequently conceal material or results, or make themselves unavailable for providing advice and information in the usual way. This may not only lead to the individual researcher or research team finding themselves isolated but can also hinder progress in a whole area of research.

The tension between collegiality and competition and the associated dangers may also be expressed through the various different kinds of peer review. Proposals for research or publication are assessed by the researcher's peers, i.e. colleagues in the same field. The system of peer review is both unavoidable and imperfect. It is unavoidable because outsiders are not in a position to properly assess specialised research, meaning that assessment must therefore be left to others who are experts in the field. It is imperfect because it involves people's assessments, and people can make mistakes. Passing judgment on one's fellows, who are frequently also one's competitors, may also lead to improper motives playing a role. A research proposal that has been submitted for review may constitute a threat to the work and the position of the reviewer. A manuscript under review may contain data or ideas that may be very useful for the reviewer, or may give him new ideas. The reviewer can then be tempted to protect his own research and his own ideas, rather than accepting and promoting the plans and results of the person who produced the manuscript. A reviewer may also have an interest in providing a positive opinion on something because the author of the article is a member of the same research team or works on the basis of the same paradigms.

There are other less conscious mechanisms at work that serve to undermine the significance of peer review. Women, for example, would appear to be disadvantaged in a system that is dominated by men. A study of the way research proposals are assessed in Sweden showed that external reviewers gave men a significantly higher score for scientific competence than women with the same level of scientific productivity. Women had to publish two and a half times more than men in order to achieve the same assessment (Wennerås & Wold 1997).

There is also a third element in relationships of collegiality and competition, namely hierarchy. Differences in reputation and prestige can also be very important in the field of science. In every research field, there are only a few people who really count. Their work is followed closely and cited frequently. Publications by eminent scholars receive greater recognition than equally important work by less familiar names (Merton 1973, Lawrence 2002).

Loyalty may lead to similar dilemmas as collegiality. Unlike the detached respect due to collegiality, loyalty mainly involves a personal relationship. Considerations of loyalty may mean that a PhD student will accept more criticism from his supervisor then he would do from other researchers. Conversely, a supervisor may play down or brush aside reasonable, well-founded objections to the work of one of his PhD students.

A disciplinary committee

Karsten is a microbiologist who is given a research proposal by his colleague Jan de Vries for review. Karsten has known Jan de Vries for many years and does not think much of his work. Something is often wrong with it: it is either sloppy or not properly thought through, even though De Vries sometimes has remarkably good ideas.

Karsten reads the proposal carefully, for one thing because last year his own team considered working in the same direction as that now proposed by Jan de Vries. Karsten comes to the conclusion that the proposal does not qualify for financing. A Swedish team has already tried something similar and it did not lead to much in the way of results. That was also why his own team abandoned their original plan last year. It is true that De Vries has come up with a clever solution to one of the problems, but Karsten does not consider that enough reason for financing to be provided for his project.

The proposal is rejected, but it continues to occupy Karsten. Should the clever idea that De Vries came up with just be abandoned because the proposal as a whole was not good enough? Karsten brings up the question of the research proposal at an informal meeting of his own team. The others also think that it would be worth pursuing, and a few months later, after the committee meeting, Karsten’s team comes up with a new and significantly better proposal. Because of his contribution, Karsten wants to get Jan de Vries involved, but the latter is furious. He accuses Karsten of stealing his ideas and in the university magazine he calls him a cheat.

Questions: Imagine that a disciplinary committee appointed by the professional association has to consider this matter. What considerations will apply and which will not? What will be the committee’s conclusion?

Case study

The Gallo-Montagnier affair

In 1983, a French research team supervised by Luc Montagnier of the Pasteur Institute in Paris published an article in *Science* reporting the discovery of a new virus, the Lymphadenopathy Associated Virus (LAV), which they thought was the cause of AIDS. According to normal practice, the French team made a sample of the virus available to other researchers, including the American Robert Gallo. A year after the French team's publication, the US Secretary of Health announced at a press conference that Gallo and his team had identified a new virus, the Human T-cell Lymphotropic Virus type 3 (HTLV-3), which was probably the cause of AIDS. The Secretary said that a test would be available for the new virus within six months, and a vaccine probably within two years. All of this would be substantiated in a series of four articles in *Science* (Heilbron & Goudsmit 1986).

When this announcement was made, it was not stated whether – and in what way – the new virus differed from that described by the French team. Tensions between the rival teams led to an open conflict when Gallo and his colleagues persisted in claiming all the credit for the discovery for themselves. The dispute concerned not only which team had been first but also what should be done about the patents applications and the future income from AIDS tests. The US Food and Drug Administration gave permission in March 1985 for the production of an AIDS test based on Gallo's work, with the test developed at the Pasteur Institute being approved in the United States only a year later.

The public controversy became so heated that, under great political pressure, a compromise was reached in 1987 whereby the researchers agreed that they would henceforth share credit for the discovery and also share the income from it equally. The official agreement, signed in the presence of US President Ronald Reagan and the then French Prime Minister, Jacques Chirac, was accompanied by an account of the way the research had been conducted. Since then, Gallo and Montagnier have adhered to the agreement and have regularly issued joint declarations regarding the importance of the worldwide struggle against AIDS.

Montagnier's colleagues were not all convinced of the necessity of reaching such an agreement, and one of them, Jean-Claude Chermann, could only be induced to sign by being officially ordered to do so. Other researchers have pretty well ignored the official version of events, as is shown by the way they have cited their sources. At first, the French article was hardly ever cited and all the attention went to Gallo's publications. This gradually changed, however, and from 1986 on the French team's article was cited more frequently than that of the Gallo team (*New Scientist*, 22 September 1990).

For Gallo, the official agreement did not mean the end of the business. An investigative journalist with the *Chicago Tribune*, John Crewdson, reconstructed the whole of the research process, concluding in 1989 that Gallo's discovery was based either on coincidence or theft. According to Crewdson, Gallo had isolated his first sample of the virus with the aid of the sample previously provided by the French team. The Office of Scientific Integrity (OSI) at the National Institutes of Health (NIH), the main source of grants for research in the life sciences in the United States, then instituted an investigation after all. Initially, a draft report pointed to one of Gallo's colleagues, Mikulas Popovic, as being guilty of misconduct. At a later stage, the same accusation was levelled at Gallo himself for having wrongly claimed that the French virus had not been transmitted to a permanent cell line. Popovic appealed against the ruling and on 5 November 1993 was cleared of all charges. With an appeal from Gallo himself in prospect, the Office or Research Integrity (ORI, the successor to the

OSI) shortly after withdrew the ruling regarding misconduct on his part (Crewdson 2002).

Very unusually, this case led not only to a battle for scientific renown and a political and legal dispute about patent rights but also to far-reaching changes extending beyond the world of science (Epstein 1996). Less unusual was the way in which an established research team initially attached hardly any importance to the findings of a team lower down the scientific hierarchy. When it became clear that the French team's results were in fact valuable, an attempt was made to steal a march on them, but things then began to move so quickly and the competition was so great, that no single research team was in a position to dominate the field.

5. Publishing, authorship, and secrecy

Citations are the currency by which we repay the intellectual debt we owe to our predecessors.

Eugene Garfield (1982: 8)

Scientific knowledge is shared knowledge. One of the reasons why science can develop is that research results are basically available to all in the form of publicly accessible books and articles, which other researchers can make free use of. They can then build on the results achieved by others, checking them, correcting them, or using them to develop a different view.

The first report to be presented is usually an oral one dealing with preliminary results. This gives the researcher the opportunity to inform his immediate colleagues at an early stage and to benefit from their comments. The next step is then often publication on paper or electronically. These different types of publication – books, articles, letters to the editors of scientific periodicals, patents, or maps – record the research results for a broader circle of interested parties. Each of these genres has its own history and its own codes and conventions. The genre of the scientific article came into being in the second half of the seventeenth century, when researchers began to write letters regarding their studies to the publishers of such periodicals as the *Philosophical Transactions* of the Royal Society in London. (The entire body of scientific work by Antoni van Leeuwenhoek consists of such letters.)

In the course of time, a certain standardisation took over. In general, one can say that the structure of scientific articles follows that of articles in the biomedical and exact sciences. The standard order is problem, hypothesis, collection of material and data, discussion, and conclusion. This model is also increasingly becoming standard in psychology and the social sciences. On the other hand, there are many scientific disciplines where this is not the case. In history and literary studies, for example, the structure of an article often tends more towards historical portrayal or the form of an essay and takes little account of the protocols that apply in the natural sciences.

Biography is a recognised historical genre, but remarks regarding the design, method of testing, problems encountered, and changes in the planning of the research are only to be found in the acknowledgements section. A biography does not lay claim to being scholarly because one can follow the research step by step but because the author has created a consistent picture based on verifiable data.

One might think that in the humanities the approach is not "scientific" but rhetorical; that view would not be justified, however. The standards that determine what constitutes properly conducted research vary from one scientific or scholarly discipline to another. And in any case, even strictly structured articles in the natural sciences are not entirely without rhetorical elements. The way the research and its results are described is highly stylised. Circumstances that are considered irrelevant and hopelessly incorrect explanations are omitted. The structure of the exposition is adapted to the ultimate results and to what is normal within the particular discipline concerned. In an article of this kind, we are therefore dealing with a rational reconstruction of the research that was carried out and not with a chronicle or a logbook (even though the latter may well be available on request). Such an article is also not intended to be a faithful account of the course of a scientific study but rather a systematic report of the findings it produced and the significance that the author or authors accord to those findings.

In between the primary data and the laboratory logbooks on the one hand, and the presentation of the results in books and articles on the other, is an area where rhetoric does indeed play a role. Recognising that scientific publications also have a rhetorical element – even in the "hard" sciences – has consequences for the assessment of supposed scientific deception. It is not easy to give fixed guidelines in advance as to what constitute legitimate stylistic devices, but any realistic view of scientific research needs to recognise that all scientific reports contain a rhetorical element. To put it rather rhetorically, one might say that reporting on scientific research does not deal with the question of whether something distorts matters or does not; rather, it deals with what kind of distortion is permissible and what is not.

One standardised feature of scientific publications is the specific order in which the names of the authors are given. All those who carried out the research or under whose responsibility it was done are listed as being authors. The person who contributed most, or who wrote the initial draft of the article, is generally mentioned first, with the name of the leader of the research team generally been given last if he or she was actually involved in designing and implementing the study. Everyone else who made a substantial contribution is listed as a co-author. If the various different contributions were of approximately equal weight, then the authors are usually listed in alphabetical order.

But the fact that researchers increasingly work in the context of a team and their input can vary considerably leads to dilemmas as to the order in which they should be mentioned. In some cases, people are listed as authors purely because they are the director of the institute concerned or the leader of the research team. If they were not actually involved in the study, or only to a negligible extent, then this is wrong. Status or one's position in the hierarchy should also play no role in determining one's place in the list of authors. In some disciplines, the normal procedure is to determine in advance what each researcher's contribution will be and how that will be expressed in the order of contributors. In disciplines in which research methods are less standardised, it is a good idea to wait to determine the order of names until the study has been completed, and to list them on the basis of the work actually carried out.

One special case is when a professor is supervising a PhD student who is just starting out on his research career and both are jointly responsible for a piece of research. In some disciplines it is not unusual when a PhD student carries out a study with a large measure of support from his supervisor for both their names to be given as authors. This is only justified, however, if the professor's involvement goes beyond his normal duties as supervisor of the student's PhD programme. In order to prevent the student being dominated by his supervisor or by another senior researcher, it is desirable for inexperienced researchers to have more than just one mentor.

In many cases, a research project produces more than one publication, and there is sometimes a tendency to describe the research in as many different publications as possible, for example so as to reach a wider readership. The disadvantages of this – fragmentation and redundancy – are generally greater than the advantages. The usual motive for this publication strategy is the desire to increase one's output as much as possible. This is also the reasoning behind what is known as the "salami approach", which involves splitting up the research results and publishing them in the smallest possible units. This too leads to a larger number of publications, but it also has a detrimental effect as regards cohesion and transparency. This kind of undesirable conduct is promoted by the increasing importance of a researcher's "score" of publications and citations where his career and funding are concerned.

Keeping results secret runs counter to standards of collegiality and openness and can only really be justified in exceptional cases, for example where the research concerns defence or national security. Where the research is carried out by a company research centre, then it would seem justifiable for the results to initially be available only to the company. The increasing amount of collaboration with private companies and greater competition between researchers frequently means that material is withheld even when articles or books have been published about it. Withholding material and data has become particularly common in such fields as genetics, where advances follow one another extremely rapidly and where economic interests play a major role (Campbell et al. 2002). This conflicts with the rule that as soon as something is published it belongs in the public domain and should be available to genuine scientific colleagues.

Research that may have far-reaching commercial consequences due to patent rights is a separate matter. In the Netherlands, patent applications are made public after a maximum of eighteen months; they can thus be seen as a form of publication. In the case of researchers whose work is funded by the state (for example universities, NWO, or the Academy) it is not a good idea for them to be involved in research that does not lead to publications, at least in the long-term. The results of publicly financed research should be public.

6. Contract research

No period in history has been more penetrated by and more dependent on the natural sciences than the twentieth century. Yet no period, since Galileo's recantation, has been less at ease with it.

Eric Hobsbawm (1994: 522)

Science is sometimes said to be free of value judgments, in the sense that the validity of scientific statements must not be dependent on the ideology or personal convictions of the researcher. Scientific assertions derive their significance from their claim to be objective. In other words, scientific research attempts to determine as accurately and systematically as possible what occurs, how it occurs, and how this can be explained. All other potential questions – regarding such things as the sense, utility, or desirability of the phenomena being studied – are strictly speaking not scientific questions.

But the fact that science claims to be value-free does not mean that the generation and application of scientific knowledge are also value-free. They are not by any means: the motives for carrying out scientific research, the interest in a particular field, and the researcher's preferences for a particular theory may play a significant and even decisive role.

The applications for which science is used are also not free of value judgments because they are bound up with the needs of society and political priorities. The major expansion of research is itself also the result of non-scientific considerations.

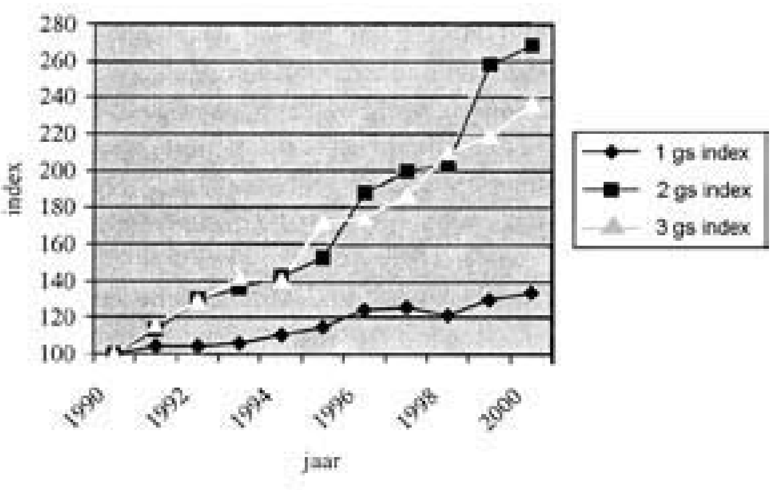
The increased range of applications for scientific knowledge means that research is having an ever-growing impact on basic societal and ethical decisions. This is especially the case with biotechnology, for example. Public concern regarding genetic modification and cloning makes discussion necessary of the ethical aspects of this kind of research. It is important, specifically where applied research is concerned, that researchers consider whether the results of their research may be used for purposes that are legally or ethically unacceptable.

The close-knit links nowadays between scientific research and its practical applications mean that a wide range of research disciplines have developed where it is hardly possible to make a distinction between pure science and applied science. In these sectors, advances in scientific understanding, technological innovation, and applications – including commercial applications – are almost inextricably interwoven.

There has been a similar development in the behavioural and social sciences as in the natural sciences. There too, the basic disciplines (economics, psychology, sociology, anthropology, political science) have led to a large number of applications, sometimes on such a scale that these have developed into separate applied disciplines and sciences, for example public administration, management, educational theory).

Applied research, sponsoring, and contract assignments – for both public bodies and businesses – are becoming increasingly important in financing research. Until about 1980, Dutch universities were predominantly financed from direct public funding, the so-called "first flow of funds", i.e. money provided by the Ministry of Education and Science. For basic research, researchers could also apply to separate organisations (NWO) for funding from the "second flow of funds", i.e. indirect public funding. Contract research used always to be restricted to applied research and represented only a small proportion of the total financing of university research. This changed in the light of the economic recession of the 1970s. Governments in all western countries implemented cutbacks in expenditure, and researchers and universities were forced to look for other sources of income. Government policy also came to be marked by deregulation and market processes, thus encouraging cooperation between universities and both commercial and non-commercial clients, with this “third flow of funds” becoming ever more important (Slaughter and Leslie 1997, Enserink 1996, Bok 2003). Currently, about a quarter of university funding is from the “third flow” of funds. The volume of the “second flow” has also increased, but it represents a much smaller proportion of university funding (3% in 2001).

Indexed development of the three “flows of funds” (1990–2001)



Source: Statistics Netherlands (CBS)

Contract research cannot only be an important source of income but also a way of gaining access to data and contributing to training young researchers and enabling them to gain experience. But becoming dependent in this way can also lead to problems.

It is in the nature of things for clients to have a major say in what should actually be studied. As financiers or co-financiers, clients also influence the scope and duration of the research. However, they should not have any say in the results that research generates or how those results should be interpreted. The old principle of “he who pays the piper calls the tune” is contrary to all concepts of what actually constitutes science.

But because clients have an interest in the results of the research that they finance or co-finance, researchers may find themselves under pressure to take account of that interest when designing and carrying out their research and reporting on it. That pressure can be exercised directly by the client. In some cases, it takes the form of intimidation, the threat that funding will be withdrawn, or damage to the reputation of the researcher or research team. Generally, however, it takes a more subtle form, such as suggestions in the context of supervisory committees or hints that further research assignments may be in the offing (cf. Köbben and Tromp 1999). Even if no pressure is actually exerted, a researcher or his supervisor may still bear in mind the client’s wishes – whether or not they have actually been expressed – and steer the research in the direction of the results that they think the client wants.

If the interests of both researcher and client run parallel, then such problems do not generally arise. If they do, it is generally in the early stages of the research when an exploratory study or a survey study is being carried out. In the course of time, however, the pressure to produce results with practical applications may often increase. In the case of research focusing on applications in the somewhat longer term, it may be possible to collaborate with the relevant sector of industry as a whole. Researchers can then set up an institute financed or subsidised by a number of companies, each of which are provided with the same results of the research that the institute carries out. The autonomy of the research and the public nature of the results can then be more easily guaranteed than if financing were to be dependent on a single company.

If the interests of researcher and client diverge, or if the research results do not comply with expectations, then tensions and conflicts may easily arise. In the case of an evaluation study, for example, to analyse the effects of certain action, it may be difficult for a researcher to have to tell the client that the efforts made by the client's organisation have had no effect, or have even been counterproductive. There may also be major economic interests at stake if the research focuses on the effectiveness and efficiency, the side-effects, and/or environmental effects of a product. Such conflicts of interest may occur at all stages of the research, whether it be when writing the research proposal, designing the study, carrying out the research, and reporting on the results, or when publishing those results.

Moreover, the fact that researchers need to compete for research assignments creates a risk that their research proposals will present matters more favourably than they actually are in reality. Mere assumptions may be presented as if they were actual scientific results; potential events may be presented as if they were genuinely likely; and desirable effects may be presented as being probable successes. If those assessing the research go along with this approach, an improper kind of competition may develop in which researchers bid against one another while what can reasonably be assumed to be possible and feasible becomes less and less clear. This kind of dynamic can be discouraged by applying a system of standard or model contracts, by guaranteeing that at least some of the assessors are independent, and by assessing research not only beforehand but also once it has been completed, so as to determine what has actually transpired as compared to what the researcher had in mind.

In some cases, clients request a specification of the "deliverables" – i.e. the expected results – prior to the study taking place. As a result, researchers are forced to make promises that they may not be able to comply with or that turn out to be no longer relevant. For conscientious researchers, such obligations may create a serious dilemma. Should they get involved in this kind of competition and accept the need for such "paper" obligations, or should they refuse to make irresponsible promises and therefore refrain from participating?

In contract research too, it is necessary to respect the rules for effective and responsible scientific conduct when designing and implementing the study. In addition, the researcher needs to guard against his study being steered too much in an unwanted direction by the client. When reporting and publishing, he also needs to pay close attention to the completeness of the results that are announced and the validity of the conclusions drawn from them. It may well be necessary to resist pressure from the client.

A number of cases have shown in recent years that these basic principles can by no means be taken entirely for granted. In addition to individual lapses and incidents, various consistent problems have come to light. Many of these problems have arisen from various conflicts of interest, including in biomedical disciplines (Bekelman, Li & Gross 2003, Knottnerus 2000, Krimsky 2003, Press & Washburn 2000). The main problem that arises is that collaboration between universities and industry has become so far-reaching that commercial interests in particular, for both clients and researchers, would seem to be influencing research results.

> In 1995 and 1996, there were dozens of articles in international medical journals on the effects of "calcium channel blockers", a type of medication commonly used to treat high blood pressure. Some researchers reported that patients using these drugs had a greater risk of having a heart attack, while others disagreed. Secondary analysis of these publications showed a clear link between the research results and the financial relationship that the researchers had with the pharmaceutical industry. All the researchers who had a positive view of calcium channel blockers turned out to have some kind of relationship with the industry, either with the manufacturer or with other companies within the sector. These relationships varied from actual contracts of employment or advisory positions to appearances at symposiums and contributions to training programmes. Of the researchers who did not have a relationship with the pharmaceutical industry, none had a positive view regarding these drugs, 23% were neutral, and 77% had a negative opinion. Only two out of a total of 70 publications disclosed the relevant links with the industry (Stelfox et al. 1998; Köbben and Tromp 1999: 110–112).
>
> A comparable study that looked at reporting on oncology drugs showed that industry-sponsored studies reported unfavourable conclusions a factor of 8 times less frequently and favourable conclusions 1.4 times more frequently than studies sponsored by non-profit organisations (Friedberg et al. 1999; Knottnerus 2000).

These and other similar findings have led to medical journals and research institutes introducing measures to better guarantee the autonomy of research. The International Committee of Medical Journal Editors (ICMJE) decided in 2001 to tighten up the guidelines for authors. Research in which it is not the researcher but the financier who has the final word will no longer be published in the top medical journals, thus reducing the power of the financier to withhold data or prevent its publication. Editors are increasingly demanding that authors disclose their financial interests in their research. But even if the journal does not require such disclosure, the researcher should disclose such links anyway.

Clients can not only influence the research question and design of a study but also the way the results are reported. For their study *De onwelkome boodschap* [The Unwelcome Message] (1999), Köbben and Tromp analysed 37 cases in which researchers came up with results that were unwelcome from the point of view of their clients or superiors. Although these studies concerned a very wide range of problems and disciplines, from nuclear physics and fisheries biology to political science and anthropology, there was in all cases pressure – subtle or less so – to conceal or at least play down such results. Some researchers stuck to their guns, others gave way to the pressure, and most of them sought a compromise and began to negotiate. Perhaps this is unavoidable in certain cases, but where does one draw the line?

> "How far is looking for a compromise acceptable or even beneficial and at what point do concessions become – to put it bluntly – scientific betrayal? It is impossible to give an abstract judgment on the matter. What would in fact be useful and valuable would be to collect information on a lot more different cases, precedents that could help researchers if the need arose. Too often, they find themselves empty-handed." (Köbben 2003: 54)

In order to counteract unwelcome results or neutralise their significance, clients can also try to tempt researchers to adopt a more accommodating attitude. Some years ago, for example, the international tobacco industry launched a large-scale campaign to play down the health risks associated with passive smoking. Highly renowned scientific authors accepted sponsoring to cast doubt on unwelcome research results in scientific journals. They were then cited as independent and authoritative references in court cases involving tobacco companies.

> Further analysis of the relevant publications produced a revealing picture. In a study of 106 review articles on the harmful effects of passive smoking, Barnes and Bero found that the sole factor that could predict the results reported by the articles was the author's affiliation with the tobacco industry. (Barnes and Bero 1998, Ong and Glanz 2000, Knottnerus 2000). Interestingly enough, those sponsored by the tobacco industry are not all members of the medical profession. In 2002, an e-mail from the eminent British philosopher Roger Scruton was leaked in which he proposed that Japan Tobacco International should increase his monthly fee from £4500 to £5500. In return, Scruton would publish newspaper articles – or arrange for articles by other authors to be published – attacking the anti-tobacco lobby. Up to that point, Scruton was not known to be receiving money from a tobacco company (Van der Heijden 2003: 5).

These cases show that researchers are not always very particular as regards contract research. Distorting and twisting research results is a major risk when there is a conflict of interest. Researchers or supervisors may have an economic interest in the businesses from which they accept assignments or with which they collaborate. This may involve owning shares, having a part-time job or a management or advisory position, or receiving bonuses in the event of results proving favourable. In such cases, the economic interest of the researcher may conflict with that of proper research. Such conflicts of interest are undesirable and need to be avoided. It is therefore essential for researchers to be open about their particular interests.

Contract research, sponsoring, and other kinds of links between universities and private companies or public bodies have become perfectly normal in many fields (Bok 2003, Slaughter and Leslie 1997). In the United States, the authors of a substantial proportion of scientific publications have a financial interest in them. A random survey by Krimsky and Rothenberg in 1992 showed that 34% of the "leading authors" of articles in 14 scientific periodicals had a financial interest in the results reported (Krimsky 2003: 113).

The commercialisation of university research has made some research fields dependent on a small number of external providers of financing. No matter how important some issues are for society in general, if they are commercially unattractive then little or no attention is paid to them. In the field of medicine, for example, this is true of research on ways to reduce the amount of medication prescribed, an issue that is of interest to patients but much less so to pharmaceutical companies. In psychiatry, a great deal of money is available for research into drugs to treat psychiatric problems but very little for research into other kinds of therapy (Healy 2002; Dehue 2003).

The most far-reaching effects of the commercialisation of university research are in research fields where there are no longer enough independent experts. Biomedical journals have found it increasingly difficult in recent years to find independent reviewers because most experts have a sideline as adviser to the company concerned or a competitor (Borst 1999: 223). Even the renowned *New England Journal of Medicine* found it necessary in 2002 to relax its policy on conflicts of interest because it was no longer able to attract enough independent authors and reviewers (Krimsky 2003: 172). Advisory councils in the field of science are struggling with precisely the same problem. A survey of members of the advisory councils of the US Food and Drug Administration (FDA) in 2000 revealed that 54% of them had a “direct financial interest” in the products that they were required to assess (Krimsky 2003: 96). Universities have a duty to reflect on problems of this kind and to take steps to combat them.

Case study

The adventures of the Berkhout Committee

In July 2000, a national advisory committee began work to determine whether the new system of noise level standards for Amsterdam's Schiphol Airport would produce the desired results. Serious doubts had been expressed about this in the Dutch Parliament, partly in the light of complaints by local residents. The "Schiphol dossier", as it is informally referred to by politicians, is an extensive and complicated series of policy documents and measurement and calculation methods intended to comply with a wide range of requirements regarding noise abatement, safety, and the growth of the airport. The major differences of opinion that had developed meant that a great deal was expected of the authoritative committee.

In compliance with the wishes of the Lower House of Parliament, the Minister of Transport, Tineke Netelenbos, emphasised when the committee was appointed that the advice it provided would be independent because its members were independent experts – not civil servants or politicians – and that the advisory process would be entirely open. The six-member committee was chaired by an expert on noise pollution, Prof. A.J. Berkhout, professor of geophysics at Delft University of Technology. The committee's work was to be based on the Dutch Cabinet's aim of allowing "controlled growth" of the airport on condition that this complied with a new system of environmental and safety standards. That new system was to be more transparent – i.e. easier to understand – then the old system and was also intended to provide better protection against noise nuisance and allow for more effective monitoring of violations – i.e. it was to be more enforceable. Another criterion that was to apply was that of "equivalence"; in other words, the new system should not lead to more space for the aviation sector or to stricter environmental and safety requirements.

It seemed when the Berkhout Committee began its deliberations that all the necessary conditions were in place for recommendations that would be both scientifically convincing and socially relevant. Not only was the committee made up of a group of genuine experts who were to carry out their work entirely openly and independently, but it also had a clear specification of its duties and powers. Unlike what sometimes happens in the case of policy studies, the task of the committee was not to determine what was and was not publicly desirable, i.e. more or less growth of the airport and more stringent – or indeed less stringent – standards regarding nuisance. All the committee had to do was determine in a scientifically responsible manner whether the objectives that had been set could actually be achieved. Two years later, however, in November 2002, the committee terminated its assignment prematurely, with Prof. Berkhout announcing that there had been "trickery with figures", "half-truths", "false promises and hidden agendas" (Berkhout 2003).

The proposed system of noise limits differed in a number of respects from the old system. The overall criterion for the total number of passengers and the total volume of cargo had been replaced by a criterion for the "total volume of noise emissions" in the wider area around Schiphol. In addition to the latter criterion, expressed as a single figure, a local criterion had been introduced. This was intended to ensure that a certain assumed noise level would be distributed in such a way that there was less noise in densely populated areas than in areas with a low population. In order to determine that distribution and to influence it, it would be necessary to have a number of monitoring points ("enforcement points") and to specify the maximum permissible noise load for each of them ("limit values").

The Berkhout Committee was highly critical of this new system. It disagreed with the way in which the Ministry calculated the total amount of noise, considering the method used to be "mathematically incorrect" and “misleading”. The committee found the Ministry’s selection of local monitoring points to be “inappropriate” in relation to the basic principles and said that the way the limit values had been determined was “dubious”. All this therefore meant that it would be impossible to achieve the intended results. Moreover, the committee considered the new system to be worse than the old one. Besides this critical analysis, the committee also made alternative proposals and indicated how information could be provided more effectively and more reliably.

The response of the Ministry of Transport was not very sympathetic. The Minister responded to the second set of recommendations with extreme irritation, announcing in the press that “Berkhout has finished the assignment”. The chairman of the committee received an official letter from the Secretary-General in August 2001 thanking him for his work. Under pressure from the Lower House, however, this was withdrawn and the committee recommenced work in October 2001 with a revised assignment. But despite basic objections, the Lower House nevertheless approved the new system of standards at the end of 2001. The Minister promised that she would have the new system thoroughly evaluated.

In April 2002, the Berkhout Committee produced a supplementary action plan. This was drawn up in consultation with the main parties concerned and included a proposal for the evaluation procedure. Six months later, the new person in charge, the State Secretary for Transport Melanie Schultz van Haegen, produced her interpretation of the duties of the committee. The evaluation would only cover locations close to the airport, and the necessary information would be provided via the Ministry. This placed the committee in a difficult position. If its task was to evaluate the protective effect of the new system, then that system ought to be evaluated in the wider area around Schiphol, which was precisely the area that the politicians’ promises concerned. And if the committee really was autonomous, why should it not be able to collect its own information? The committee ultimately came to the conclusion that it could not produce the intended evaluation in a responsible manner, and it terminated its assignment.

Prof. Berkhout published an analysis of all these events in 2003 (Berkhout 2003), emphasising that the assistance of independent experts is increasingly necessary so as to collect, process and analyse information and present it in a comprehensible manner. The trouble is that that kind of autonomy is not always welcome. Berkhout’s committee was treated as a “scientific façade” for policy that already existed. When it turned out that the politicians did not like the advice they were given, political influence on the committee was stepped up. Not only were its results ignored, but it was put under pressure, and its recommendations were misquoted in important documents. In one of the key decision-making documents, the Environmental Impact Report for Schiphol, the committee’s findings were distorted in an improper manner. This was only corrected in the course of the decisive debate in Parliament, in other words when it was already too late.

As yet, Prof. Berkhout’s analysis has not been publicly contradicted by any of the civil servants or senior politicians involved.

7. Publicity and media

The general public can be divided into two parts: those that think science can do everything, and those who are afraid it will.

Dixy Lee Ray (1973: 15)

Scientific research has long aroused the interest of laymen. In the seventeenth and eighteenth centuries, anatomists carried out dissections before fascinated audiences in anatomy theatres, while demonstrations of magnetism and electricity were a standard part of what was referred to as *physique amusante*. Disciplines such as natural history have in fact developed mainly as amateur science. As professional science and amateur curiosity diverged, there was a growing need for explanation and the transfer of knowledge, and popularising science became a separate discipline.

In addition to the thirst for knowledge and diversion, other reasons for public interest in science have become increasingly more important. Various interest groups make great use of scientific research to promote their viewpoints. Patients are better informed about their condition than in the past and often want to be directly involved in their treatment and medication. And in all public discussions, sooner or later experts are brought in to explain the background and comment wisely on it (cf. Lunteren et al. 2002). These are often scientists who are trusted because of their thorough knowledge and independent position. Now and again, one of these experts develops into a media personality, commenting not only on his own discipline but on a whole range of current topics, large and small.

In their contacts with the media and other “users” of their research results, it is important for researchers to realise that non-scientists can interpret the information they receive in an entirely different manner to insiders (cf. Wynne 1995, Lewenstein 1995). Laymen are not familiar with the principles behind the research – or at least only to a very limited extent – and there is less opportunity to make the necessary subtle distinctions. Remarks about probable results and potential consequences can easily lead to exaggerated expectations and speculation. Communication with non-scientists also demands a great deal of care. It is particularly important not to arouse any unjustified expectations if the research concerned can have a major impact on society in general, for example in the case of new drugs. Some scientific institutes employ special spokespersons or make use of special PR committees.

Scientists may also seek publicity so as to stay ahead of competing research teams. Researchers have an interest in publicising their results quickly. Speed and daring may be rewarded, but excessive haste and overconfidence involve major risks. In some cases, researchers try to use the media to trump other teams and to dominate scientific debate with improper means.

In actual research practice, it is becoming increasingly important not only to announce one’s results to other scientists but also to the general public. Publicity increases a research team’s reputation, which improves its chance of acquiring funding.

Publications in scientific journals are assessed by critical, expert fellow scientists, but in the media other standards for success apply, and these may be entirely at odds with the proper scientific criteria. Whereas scientific publications by university researchers have to meet the highest possible standards, statements by the same researchers in more popular periodicals would appear to be subject to much less stringent criteria regarding meticulousness (Van der Heijden 2003).

In some disciplines, the distinction between professional journals and general cultural periodicals or newspaper supplements is much smaller. In these disciplines, the status of practitioners is measured by the articles that they publish in cultural magazines or the science supplements of the newspapers. The boundary between an essay and a scientific article is difficult to determine and the standards for such publications regarding evidence and references are different to those for real scientific journals. The necessary scientific scrupulousness may be at odds with the need for an article to be accessible for readers.

Synthesis leads to problems

A research team carried out a study a few years ago of the excretion of certain metabolites (conversion products) of a drug. Using the extent of excretion of these products, the aim of the study was to acquire information on the rate at which the drug was metabolised (i.e. converted), in other words about the enzyme activity involved in the conversion. Because the metabolite was not available for purchase at that time, a PhD student working with the team synthesised it and the observations and calculations were then carried out using his product. The publicity value of the new discovery meant that the results of the study were quickly announced.

There was great interest both in the Netherlands and elsewhere. The research team therefore provided a number of labs with samples of the metabolite for use as reference material. After a while, it turned out that there were significant discrepancies – by as much as a factor of 2 – between the extent of excretion as measured by the team that had initially synthesised the metabolite and the results achieved by various other labs. Members of the original research team then analysed their own product again and found that it was 50% contaminated with an inorganic salt that had become mixed with the crystals of the metabolite during the crystallisation process.

Questions: What decision would you have taken if you were the person who discovered the contamination? What do you think the leader of the research team should have done?

A hearing

The meteorologist Paul Ligtvoet, an expert on climate and climate change, is invited to a hearing held by a committee of the Lower House of the Dutch Parliament. The committee is attempting to arrive at a standpoint regarding global warming and its causes. During the first, closed session, there is extensive discussion between Ligtvoet and the members of the committee, all of whom are politicians. Ligtvoet explains that some natural phenomena, for example the rise in sea level, cannot be viewed as statistically expected variations. When asked what contribution he thinks is made by human activity, he gives a cautious answer. He says that the consensus among experts is that global warming – one of the consequences of which is the rise in sea level – is probably due for two thirds to natural causes and that one third is the result of human activity.

The committee then holds a second, public hearing on the same subject. Over the three days of the hearing, three different topics are considered, with experts from various different disciplines giving evidence. First of all, the committee talks to Ligtvoet about the causes and effects of climate change. Various possible remedies are then considered, followed by the measures that should actually be taken, from a national, European, and global perspective.

During the public sessions, the media are also present. Ligtvoet is given half an hour to summarise his analysis. As requested, he deals with the relationship between the temperature on earth and carbon dioxide concentrations, changes in sea level, indicators for climate change, and the expected trends. At this open session, the members of the committee ask the same questions as they did during the closed session, but the atmosphere is entirely different. Some members of the committee make use of a wide range of rhetorical devices to emphasise their political views or their own superiority. Partly because of this, a polarised situation is created: the rise in sea level must be due either primarily to human activity or solely to the capriciousness of nature. It is clear that many members of the committee have already determined their own position long ago and are making selective use of the data and arguments presented. Ligtvoet decides not to get involved in the political discussion and restricts himself to explaining his findings. He goes home afterwards with a feeling of disappointment.

Questions: Could Ligtvoet have adopted a different attitude during his discussions with the committee? How far does the responsibility of an expert witness extend when he is called on to participate in political or administrative debate?

Case study

The miracle of cold fusion

In March 1989, the British scientist Martin Fleischmann and his American colleague Stanley Pons announced that they had achieved nuclear fusion at room temperature and under normal pressure. These two chemists had succeeded in doing in a glass of cold water something that had previously only been possible at temperatures in excess of 100 million degrees (Braams 1998; Van Everdingen 1993). Using electrolysis, they had pumped metal electrodes full of deuterium, hydrogen atoms with a nucleus consisting of one proton and one neutron. According to Pons and Fleischmann, cold fusion releases energy in the form of heat, neutrons, and radiation. The method was so simple that chemists all over the world were soon attempting to achieve the same results, sometimes – seemingly – successfully but generally unsuccessfully. After a lot of confusion in the world of science, people soon lost faith in the Pons-Fleischmann method.

The role of the media in this case was crucial (Lewenstein 1992). There had been no publication of the pair's results in a scientific periodical before Pons and Fleischmann made an announcement at a press conference. This was frustrating for other scientists because it made it impossible for them to attempt to replicate the original experiments. The two chemists also gave little away about the method they had used, and information was largely based on what the media published. This meant that the available information was not very precise, soon giving rise to a lot of speculation and rumour.

Pons and Fleischmann reported on their method at various meetings. Their account was supported by a number of laboratories that also confirmed that they had achieved such a high temperature that nuclear fusion must have taken place. Nevertheless, this failed to offset the scepticism of other chemists who had failed to replicate the cold fusion experiment. A year after the initial announcement, the American scientific periodical *Nature* published an article saying that there was insufficient evidence that nuclear fusion had in fact taken place.

Pons and Fleischmann felt so hard done to that they went to court in an attempt to silence their critics. Not long after, a number of nuclear physicists expressed the view that if the pair had actually produced the results they had reported, they would have received such a high dose of radioactivity that they would not have survived (Van Everdingen 1993: 120).

Looking back, it is difficult to determine precisely what went wrong. Was it the two researchers' strategy of communicating via the press; did they make use of the wrong apparatus, thus producing incorrect data; or did they not have a decent theory? The editor-in-chief of *Nature*, John Maddox, suggested that the two researchers found themselves in a distant corner of the scientific world, putting them so far away from the everyday scepticism of their colleagues that they began to believe their own results even though the facts indicated otherwise (Hagen 1991: 172). The fact is that after extensive attempts at replication, other scientists came to the conclusion that what Pons and Fleischmann had discovered was in any case not nuclear fusion and that they had therefore made serious errors in their experiments.

8. Prevention and remedies

Lorsqu'on veut changer les mœurs et les manières, il ne faut pas les changer par les lois.

Montesquieu (1748: 564)

As we have seen, scientific misconduct can have harmful effects on test subjects and patients, on the quality of research, and on the public image of science. Especially where major interests are concerned, there is a serious temptation to adopt a casual attitude to the rules. In science too, ambition or the pursuit of profit can lead to generally accepted rules being broken. There are various things that can be done to prevent or combat misconduct.

Education. Researchers should be introduced during their training to the dilemmas that research may involve. In addition to training in their particular discipline and in methodology, they should be made aware of the applicable rules and standards. Their training should not forget to deal with situations in which they may be tempted to ignore those rules. This can best be done on the basis of practical examples discussed by experienced researchers.

Senior researchers. Senior researchers, team leaders, and scientific directors can set the right example. They are the ones primarily responsible for creating a working environment in which the likelihood of incidents is minimised; in which anyone can make a mistake but where nobody gives way to the temptation to then conceal it; in which mutual monitoring is viewed as a matter of common interest; and in which the most critical questions can be posed and seen as evidence of involvement and collegiality.

Quality assessment and evaluation. The quality of the work of a researcher can best be determined by fellow researchers. That is the normal procedure during work meetings and discussions, selection of publications, assessment of research applications, and external inspections. Quantitative methods have also been introduced as a means of measuring the quality of research teams and scientific journals. These methods include citation analysis, i.e. counting publications and ranking the journals in which they appear. However, one-sided use of such indicators may encourage strategic behaviour and short-term objectives at the cost of the long-term quality of research. In the social sciences and humanities, bibliometric techniques of this kind are not normally an effective means of determining scientific or scholarly quality because the databases of the American Institute for Scientific Information (ISI) contain almost entirely English-language periodicals, making them too one-sided (cf. Dehue 2000, Van Steijn 2001, Dehue 2001).

An increasing number of organisations are working according to standardised, certified procedures. Standards for materials, products, processes and services are drawn up by the United Nations' International Institute for Standardisation (ISO) and in the Netherlands by the Netherlands Standardisation Institute (NNI). Drawing up standards, monitoring compliance, and certifying research institutes can make a positive contribution to the quality of scientific endeavour. Certification can also be beneficial when research grants are being allocated. Where standardisation and certification are concerned, a distinction needs to be made between the actual substance of a research project and the way it is carried out. The latter can be specified with the aid of explicit rules of conduct, although the same does not apply to the former. A good example is the widely used code of “good laboratory practice” (GLP).

Code of conduct. Although scientific research is governed by the normal rules of behaviour, supplementary rules are nevertheless valuable. Formalising these rules in a code of conduct or a code of professional practice plays an important role in an increasing number of disciplines. The aim of such a code is to explicitly determine what is and is not acceptable. These rules provide guidelines for researchers and other parties involved when deciding on disputed issues and other difficult matters. They also increase the level of confidence among researchers themselves and confidence in the quality of science. One condition for an effective code of conduct is that it should be generally recognised and accepted. Two good examples are the detailed research code that applies at the University of Amsterdam’s Academic Medical Centre (AMC) and the code of conduct for the use of personal details drawn up by the Social Sciences Council (KNAW 2003). In 2005, the Association of Universities in the Netherlands (VSNU) also adopted a code of conduct for scientific research.

Confidential counsellors or committees. To enable serious suspicions of infringement of scientific integrity to be reported, the Dutch universities and the research institutes operating under the auspices of the Royal Netherlands Academy of Arts and Sciences and the Netherlands Organisation for Scientific Research have agreed to appoint confidential counsellors or committees (KNAW, NWO, VSNU 2001). Suspicions of misconduct can then be reported in confidence to the counsellor or committee, with an investigation being carried out without immediately harming anyone’s reputation. This is particularly important if the suspicions expressed turn out to be unfounded. The investigation of the suspected misconduct must take place quickly and it must determine whether the report is actually unfounded or whether further investigation is in fact necessary. In the first case, no further action will be taken, but otherwise a small committee will investigate further. The person who is the object of the allegations must be informed of them, and the confidential adviser or committee must have access to all the necessary information. Both the person who reports the misconduct and the alleged perpetrator naturally have a right to be treated with due care and to defend themselves. The confidential adviser or committee will report its findings to the employer, the Executive Board, or the boards of the Academy and NWO. The boards will then determine whether scientific integrity has in fact been infringed. As the employer, they are responsible for imposing any sanctions. This method helps determine precisely what should be deemed to constitute scientific misconduct.

National Board for Scientific Integrity (LOWI). Complainants and those who are the object of a complaint can request the National Board for Scientific Integrity to give a ruling on the manner in which the complaint has been dealt with and on the decision taken by the institution concerned. The ruling rendered by LOWI has the status of a recommendation. LOWI advises the boards of the institutions, and sends a copy to all those concerned. The boards continue to be responsible in their role of employer.

Sanctions. There are three different types of possible sanction (KNAW, NWO, VSNU 1995). The first of these involves measures (a warning or reprimand) applicable to less serious infringements of scientific integrity. The second category comprises measures imposed for a specified period of time. Such measures are imposed if the credibility of research has not been affected and there are no negative consequences for patients, for example.

Permanent sanctions will be imposed in the case of the most serious infringements of scientific integrity.

Scientific study. In line with the example of a number of studies discussed in this booklet, more research needs to take place on the dynamics of scientific misconduct, its consequences, and the way it is dealt with. Sober scientific analysis is a powerful weapon against misrepresentation and fraud, including when they are perpetrated under the guise of science.

There are a number of organisations that are responsible for these various initiatives: research institutions, professional associations, universities, and national institutes such as the Academy, the NWO, and the VSNU. There has only recently been discussion regarding the relationship between existing rules and regulations and the conditions under which they can have the desired effects (cf. Stein 2004). But no matter how effective such initiatives can be, they cannot solve all the problems. Some types of misconduct – no matter how credible they may seem – are difficult to prove, and – whether or not with the aid of a lawyer – the person accused can hit back very hard. One should also not forget that – as Max Weber pointed out almost a century ago in 1919 – scientific endeavour is embedded in a wider social context that is characterised by conflicts of interest and a struggle for power. Many temptations leading to scientific misconduct originate in these social relations, meaning that the behaviour of scientists can never be entirely in accordance with the ideals outlined in this booklet. Even so, that does not mean that one should not strongly defend those rules and ideals.

Confidential counsellors, regulations, and procedures, education and sanctions…all of these can contribute, but ultimately everything is a matter of personal responsibility on the part of researchers and research teams. It is not the fear of sanctions or a high risk of being caught but above all this sense of personal and collective responsibility that will enable researchers to withstand the temptations of misrepresentation and fraud that can have such a deplorable effect on scientific endeavour.

9. In conclusion

> The urge to understand the incomprehensible.... The eagerness to advance daring hypotheses, even if they then need to be repudiated one by one... The satisfaction of discovering links that no one had ever imagined, to find order where chaos reigned, or seemed to reign… The experience of “Eureka!”, and the joy that then seizes you... When Pythagoras discovered his famous theorem, he is said to have been so overjoyed that he sacrificed a hundred oxen to the gods. That’s what it’s all about! Yes, even your struggle with the material when your insights have to be put down on paper. Anyone who has ever got up in the middle of the night to write down that one significant word or that one sentence that explains everything – which you of course cross out next morning – will know what I mean. Others will not.
>
> André Köbben (2003: 26)

In the past century, the extent, variety, and relevance to society of scientific research have grown as never before. There are more scientific researchers alive today than in the whole previous course of history, and their work is made use of by more people than ever before. This booklet has looked at the downside to all this. Pressure to achieve and competition have increased enormously in the scientific world, both because of growing interests outside that world and developments within it, for example pressure to publish, conditional financing, greater international competition, etc. There is therefore a more pressing need nowadays for shared standards and internal controls, but the risk of carelessness, manipulation, fraud, and other types of undesirable conduct has also increased.

Needless to say, not all researchers succumb to those temptations; quite the contrary: the great majority of scientists behave in a careful and honest manner. But there have been enough cases, both in the Netherlands and elsewhere, to justify our being concerned. After all, the repercussions for science are very far-reaching. If colleagues, funding organisations, and clients can no longer be confident of the integrity of scientific researchers and the research they carry out, science will quickly lose its function and usefulness. And in situations where researchers assume the role of a lawyer and act as a representative or manager for third parties, they also forfeit their position as scientific researchers, in other words as those who attempt in a spirit of debate to determine as objectively as possible what occurs, how it occurs, and how this can be explained.

This booklet has attempted to draw attention to a number of dilemmas and temptations that can make themselves felt in the context of research, but it cannot get rid of them. On the other hand, it can perhaps help those who are confronted by such problems to be more clearly aware of them and better able to find a sound solution.

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About this publication

The first edition of this booklet appeared in 2000. After legitimate criticism had been expressed of parts of it, the Board of the Royal Netherlands Academy of Arts and Sciences (KNAW) decided in 2002 to withdraw it and publish a revised version. With that end in mind, a working party was assembled consisting of a number of Academy members: J.H. Koeman (chair), K. van Berkel, C.J.M. Schuyt, W.P.M. van Swaaij and J.D. Schiereck (secretary). Johan Heilbron undertook to write the new text.

This new edition makes grateful use of comments by W.K.B. Hofstee, M.J.M. Hofstede, A.J.F. Köbben and A. Rörsch. The Academy's Science and Ethics Advisory Committee was consulted a number of times during preparation of the booklet.

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