SCIENTIFIC FRAUD AND THE POWER STRUCTURE OF SCIENCE

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In the routine practice of scientific research, there are many types of misrepresentation and bias which could be considered dubious. However, only a few narrowly defined behaviours are singled out and castigated as scientific fraud. A narrow definition of scientific fraud is convenient to the groups in society — scientific elites, and powerful government and corporate interests — that have the dominant influence on priorities in science. Several prominent Australian cases illustrate how the denunciation of fraud helps to paint the rest of scientific behaviour as blameless.

Keywords: Scientific fraud, bias, misrepresentation.

Ask most scientists about scientific fraud and they will readily tell you what it is. The most extreme cases are obvious: manufacturing data and altering experimental results. Then there is plagiarism: using someone else's text or data without acknowledgement. More difficult are the borderline cases: minor fudging of data, reporting only the good results and not citing other people's work that should be given credit. Because obvious fraud is thought to be both rare and extremely serious, the normal idea is that it warrants serious penalties.

That is the usual picture, anyway, for public consumption. Probe a bit more deeply into scientific activities, and you will find that fraud is neither clear-cut nor rare. Stories abound of the stealing of credit for ideas. They range from the PhD supervisor who published his student's work under his own name, to the top scientist who, as a referee, delayed publication of a rival's work in order to obtain full credit for it himself — including a Nobel Prize. There are also stories of various other forms of cheating.

The actual practice of science is a complex business. There are intricate experiments, with continual changes of equipment, protocols and procedural details. There are all sorts of measurements, with much more potential data thrown away than saved. There are pages of theoretical calculations thrown away for every equation published. There are stacks of insufficiently documented data sheets and computer outputs. Next to the desks on which scientific papers are prepared are books, journals, preprints, correspondence and notes. In the heads of scientists are various half-formed ideas, long-held desires, prejudices, and the vague recollections of articles read, seminars attended, conversations with colleagues and discussions with collaborators.

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In this messy process of doing science, there are no red lights which flash when someone does something fraudulent. It is quite an accomplishment for scientists to create a semblance of order in their work, so that they can give the impression of doing proper science. It also takes considerable effort for them to paint a convincing picture of the violation of proper practice, namely what is called fraud, within this semblance of order.

Another way to say this is to say that, out of the many things that scientists do, they attach meaning only to some things, which they call doing science or applying the scientific method.[1] The same applies to fraud. Fraud is what scientists tell each other is fraud. This raises the question, why are certain things called fraud and others not? My general answer is that the social definition of fraud is one which is convenient to most of the powerful groups associated with science. This includes government and corporate sponsors of scientific research, and the scientific community itself, especially scientific elites.

My argument proceeds this way. A host of things go on in scientific research that could be open to suspicion. Some of these are accepted as standard practice, others are tolerated, and some are considered unacceptable. Why? There are a number of reasons, but here the focus is on the power structure of science, namely the interest groups that fund science and reap disproportionate benefits from it. This analysis then is applied to a number of Australian cases.

POTENTIALLY DUBIOUS PRACTICES IN SCIENCE

There are a large number of activities in science that can be called potentially dubious, meaning that they might well be considered unethical or reprehensible if sufficient numbers of scientists decided that they should be. Most of these practices fall into two categories: misrepresentation and bias.

Dictionaries typically define fraud as deceit, trickery or the perversion of truth. Thus, many of these practices could be considered fraudulent, but seldom are they included in discussions of scientific fraud.

Some individuals have tried to raise concern about these practices,[2] but for the most part they are tolerated or treated as standard practice. Here I will describe a number of potentially dubious practices, giving examples in some cases. I do not attempt to rank these practices in terms of seriousness, since it is precisely my point that judgements of seriousness result from social processes, not absolute standards. In the contemporary examples, it is usually impossible to tell the full story due to defamation law.[3] In many cases I will give no names and change certain details to disguise the identity of those involved.

One of the most common misrepresentations in scientific work is the scientific paper itself.[4] It presents a mythical reconstruction of what actually happened. All of what are in retrospect mistaken ideas, badly designed experiments and incorrect calculations are omitted. The paper presents the research as if it had been carefully thought out, planned and executed according to a neat, rigorous process, for example involving testing of a hypothesis. The misrepresentation of the scientific paper is the most formal aspect of the misrepresentation of science as an orderly process based on a clearly defined method.[5]

The misrepresentation inherent in the standard scientific paper is not only acceptable — it is virtually impossible to avoid. Journal editors will seldom accept a more realistic account of how a research project proceeded. Much of the impact of James Watson's book about the discovery of the structure of DNA, *The Double Helix,* derived from its contrast with the antiseptic scientific paper. Watson and Crick's 1953 paper in *Nature,* reporting their discovery, was a misrepresentation of scientific practice, but it was the accepted way of talking about science.

Another misrepresentation occurs in the list of publications cited in any scientific paper. The publications cited serve many purposes, but a principal one is supposed to be giving credit to prior work, in particular work which formed the basis of the present contribution. In practice, citations give a very poor picture in this regard. Citations are often included not because they have been read — Erwin Chargaff refers to slabs of bibliographies "wafted in their entirety from one paper to the next"[6] — or had any impact on the research, but because it is useful to have a long list to impress referees or to enhance one's own work while denigrating competitors or enemies.

Certain types of citations are normally omitted because they do not have a proper status: grant applications, conversations, correspondence, newspaper articles and 'unscholarly' publications generally. These sources may have been key contributors towards the research, but they are not cited.[7] Some journals impose styles which make such citations virtually impossible.

Another dubious practice can be called intellectual exploitation. It occurs when a researcher makes use of work by other people associated with the research, but does not give them proper credit. For example, a wife of a researcher may regularly collect and assemble specimens, write the first draft of a paper, or read the literature and compile the bibliography — and never be acknowledged as a co-author. In addition to wives, other common victims of this treatment are students and research assistants. Many PhD students in science feel obliged to list their supervisor as a co-author of papers, even though the supervisor did little or no work on the project. This practice is another example of misrepresentation. By informal accounts it is more widespread than commonly acknowledged, but is seldom documented.[8]

Intellectual exploitation is not only common: in many situations it is required. In scientific papers it is considered inappropriate to acknowledge typists, secretaries, librarians, lab assistants and others not involved in 'real' science. (In books, these individuals sometimes are mentioned.) By contrast, those in a position of equality or superiority expect generous acknowledgement. The heads of some departments and research labs expect to have their names attached to every paper produced under their aegis, whether or not they had anything directly to do with it.[9] Some would claim such co-authorship is necessary so that they can raise more funds to keep their junior reseachers employed.

Another common misrepresentation of research work is exaggeration of its quality, progress and social importance. This is almost essential for a successful scientific career. A modest and honest grant application stands little chance of success: the applicant, to obtain money, must puff up the quality and importance of previous work and give a highly unrealistic assessment of the likely results of funding future work — or, as is common, request money to carry out research which actually has been completed. Most grant applications are convenient fictions.

The same applies to annual reports, media stories and other material prepared for general distribution. Breakthroughs abound. Research relevant to a cure for cancer covers the gamut of biological science. The quality of research is never honestly assessed. (When did you last read an annual report reporting mediocre research?) Honesty in research grants, annual reports and media reports stands about as much chance as honesty in advertising, because this sort of (mis)representation of science is, indeed, a form of advertising.

Misrepresentation is also common in the curriculum vitae, the formal record of a scholar's career. Creative curriculum vitae writing is a fine art: minor honours are inflated, administrative duties are exaggerated, major credit is claimed for collaborative research, and every possible publication is listed (perhaps including duplicate conference papers and in-press papers that have not yet been accepted or even submitted for publication). Most of all, failings are omitted from the vitae. All this is entirely standard. It is only when non-existent degrees or publications are claimed that anyone even thinks in terms of misrepresentation.

'Shoddy' science or sloppy scholarship is a way of describing research work that does not measure up to a hypothetical set of ideal standards. 'Shoddy' science includes things such as poor experimental design, bungled statistics, incomplete data sheets, improperly tested hypotheses, inaccurate reference to previous work, uncorrected minor mistakes in computer programmes, failure to test alternative hypotheses, and conclusions that do not reflect the body of work. 'Shoddy' science is widespread.[10] Lots of it gets into scientific journals, and much more is rejected. But an occasional rejection is about the only penalty for poor work. More common is the reward of promotion for producing so much of it.

The boundary between 'shoddy' science and what is sometimes called fraud is a fuzzy one. No scientist publishes all the raw data. The raw data must be assessed for quality (discrepant data are thrown out as being due to bad runs), and then suitably processed (transformed theoretically, smoothed, reorganised), and then filtered (only some data are selected to be shown) before publication. Appropriately done, this is standard practice. Inappropriately done (usually according to someone else's assessment), this process can be called cooking, trimming, fiddling, fudging or forging the data.

The available evidence suggests that inappropriate treatment of data is much more common than normally acknowledged. An eminent behavioural scientist, who must remain anonymous, wrote the following:

> I can however assure you that I have had more than one graduate student who has subsequently become eminent and who I know was fudging data. I have also had undergraduate students who have gone on to do their PhD elsewhere and when they went to certain professors reported back to me malpractice that amounted to fudging data (e.g. repeating an experiment ten times until one experiment worked out significant and then publishing just that one without mentioning all the failures). In addition I know of several people in biology and psychology whose results cannot be replicated, who refuse to give access to their raw data and who could not possibly have completed the experiments they claim to have undertaken in the time available. Their immediate colleagues know what is going on but universities tend to protect them because employing fraudulent staff is not good for the image of the university. In a recent case a professor was caught changing figures obtained by a research assistant. All the university did was to reprimand him and tell him that he could never apply for another grant so long as he remained at that university. He is now at another university. I am extremely sorry that the law of libel prevents my being more specific.[11]

Mathematician Alexandre Grothendiek, a professor at Montpelier University, wrote in a letter declining to receive the Crafoord prize that

> during the past two decades, the ethics of the scientific profession (at least among mathematicians) has declined to such a degree that pure and simple plundering among colleagues (especially at the expense of those who are not in a position to defend themselves) has almost become the rule, and in any case is tolerated by all, even in the most flagrant and iniquitous cases.[12]

This sort of behaviour is not exactly standard practice, but it is certainly widely tolerated. The cases that receive extensive publicity are, arguably, only the tip of an iceberg.[13]

Contrary to popular belief, it is not easy to detect most cases of illegitimate manipulation of data. According to one provocative assessment,[14] science is ruled by an oligarchy of mediocrity: in the chaos of fashionable but pointless research done by less-than-competent researchers, cheating can escape unnoticed.

Also tolerated are biased viewpoints, including those linked to powerful vested interests. Many scientists are employed by or receive research funds from companies or government bodies, and both expect and are expected to come up only with results useful to those bodies. Scientists receiving money from chemical companies to study pesticides seldom draw attention to the limitations or dangers of pesticides: they simply do studies within a framework which assumes that using pesticides is the appropriate thing to do. Physicists working on nuclear weapons design do not stray outside their narrow task. Engineers employed by automobile companies do not propose studies looking for safety problems or alternatives to the car.[15] It could be said that the viewpoints of most scientists are not so much biased as limited: they are willing to do narrow research work whose context is set by the powerful patrons of science. The bias comes from the context, not from the conscious intent of the scientist. In any case, this sort of bias is standard practice, or at worst tolerated. Researchers who are funded by the tobacco industry to study the health effects of cigarettes may be frowned upon, but they are not drummed out of science for being corrupt.

The flip side of bias built into the structure of science is suppression of dissent. The few scientists who speak out against dominant interests — such as against pesticides, nuclear power or automobile design — often come under severe attack. They may have their reputations smeared, be demoted, be transferred, have their publications blocked, be dismissed, or be blacklisted.[16]

It can be argued that there is bias in all scientific research. Whether bias is seen as a problem depends on what the bias is. Biases that are no threat to powerful interests are treated as standard or tolerated. Biases that do threaten powerful interests are, often enough, attacked with full fury.

In order to put the allegations of fraud into perspective, it is necessary to understand this point that science as it really happens contains a host of potentially dubious practices, many of which are considered standard and many others widely tolerated. In order to understand why, it is useful to look at the dominant interests served by science.

THE POWER STRUCTURE OF SCIENCE

Contemporary science is a large-scale enterprise, heavily funded and highly directed. The dominant players are governments and large corporations, which provide most of the funding for science, and the community of professional scientists themselves, especially the scientific elites.

For example, a large amount of scientific research is devoted to producing and testing new drugs. Partly this is because of direct funding by pharmaceutical companies. But even the studies of other researchers can be affected, because what are seen as important scientific problems are partly shaped by drug industry priorities. This increases the degree of scientific interest in epidemiology and brain chemistry. The industry funds some pure research in fields of potential interest, knowing that it can reap the benefits of anything useful that turns up. By contrast, some fields languish because they hold no prospects for drug interventions. The same process applies to research in other areas. The dominant influences are from government and large firms, but there are crumbs for others too.[17]

The semi-bureaucratic organisation of scientific research is a crucial factor in this process of shaping scientific goals. A relatively small number of scientists and bureaucrats make the crucial decisions about research: setting up and shutting down research programmes, making key appointments, editing journals, allocating grants, awarding prizes. This group can be called the political scientific elite.[18] They have the dominant influence on priorities within science. More than most other scientists, they have regular interactions with equivalent elites within government and industry, and usually share the same basic concerns. On the other hand, they have an interest in maintaining the autonomy of science, preventing it from becoming solely a servant of external power. They have an interest in maintaining some autonomy for scientists within the general ambit of government and corporate interests.[19]

Within this overall power system in which most scientific research is done, the standards of scientific behaviour are continually negotiated. These standards do not derive from some textbook or eminent authority, but are adaptations to the reality of doing science in a particular social and political context.

Several of the common misrepresentations and biases are natural outgrowths of the hierarchies within scientific organisations: misrepresentation in citations, false pictures of research in grant applications, appointments of cronies and exploitation of subordinates. Many of those who rise within the hierarchy do so by claiming an excess of credit for their own contributions; once somewhat up the hierarchy, it is easier to use the power of position to continue the process. It is easy to see why many of these practices are standard: they serve the interests of the more powerful members of the research community. The main opposition comes from those who lose out or prefer to play the game a different way. By and large, these critics are not influential; they have been unable to do more than occasionally voice concern about the practices, which continue unabated.

Sloppy scholarship and minor cheating are dealt with in a slightly different way. To understand the dynamic here, it is important to remember that there are two types of consumers of science: interest groups such as governments and corporations, and other scientists. Trimming and cooking of data is sometimes a problem for both these types of consumers, since the usefulness of the product is jeopardised. Minor cheating is exactly what the name suggests, namely cheating which does not hurt anyone else so badly that they get too upset. Using this same sort of tautological nomenclature, major fraud is just the sort of fraud that gets others sufficiently concerned to take action.

It is almost always other scientists who are most aware of the cheating that goes on. There are conflicting pressures: some colleagues believe in scientific ideals and hate to see them defiled, or may want to stop the cheater from getting ahead on the basis of shoddy work; but most do not want to undergo the personal confrontation involved in making allegations of fraud. Administrators often are reluctant to raise the matter too widely since that would hurt the reputation of their institution.

In this, science is little different from many other occupations. Take the building trades, for example. Most builders are honest and hard working, taking pride in their work. Some of them, depending on the incentives, take shortcuts. This may be standard or tolerated, so long as it does not put other workers at risk or jeopardise the project as a whole. An electrician who does such shoddy work that other workers are severely inconvenienced or put in danger will not receive further work. Furthermore, if the work is so obviously bad that customers can see the consequences, then that jeopardises the work of other tradespeople. It is to the advantage of builders not to make a big noise about poor work or corruption, but to quietly push out the worst offenders — who are well recognised by other builders — and to tolerate the minor cheating that occurs.[20]

Science basically operates the same way. There are internal audiences and external audiences. The preferred way to handle shoddy research is to quietly deal with the serious offenders and to ignore the widespread minor cheating. In such a situation, cheaters do not bring science into public disrepute whereas, ironically, those who blow the whistle on cheaters are perceived as posing a threat to business as usual.

There remains the category of biased viewpoints linked to powerful interest groups, such as pesticides researchers whose views are convenient to chemical companies. This serves rather than threatens the power structure of science, and is seldom seen as a problem at all. It is only when other scientists voice different views that a problem is noticed.

To return to the example of drugs: research that is directly or potentially useful to the pharmaceutical industry is seldom raised by scientists as a dubious practice. (Members of the consumers movement sometimes do this.) But it is rare indeed for anything more to be done than general criticisms. The refusal by some scientists to participate in recombinant DNA research is conspicuous as an exception. In general, there is no material basis, no alternative source of funding, to sustain an alternative conception of scientific practice. Hence, science in support of powerful interests is usually tolerated.

This has been a very general overview of the exercise of power in science. There are many exceptions to the generalities that I have presented. Nevertheless, the overall picture is quite useful in making sense of the usual responses to what I have called potentially dubious practices in science.

CASES

To illustrate the value and limitations of the general framework outlined above, some Australian cases of what is conventionally called fraud in science and academia are described here. There is little point in presenting examples of standard practices such as misrepresentation of research progress, exploitation of subordinates and bias in appointments, since it is seldom expected that anything will be done about them. Rather, the focus is on cases in which there might be some expectation of action, because the behaviours are officially condemned. This does not mean that these are the intrinsically more 'serious' cases in any absolute sense since, as argued above, standards of scholarly behaviour reflect the interests tied up in the relevant power structure.

In a science department at an Australian university, an honours student was found to have plagiarised, word-for-word, most of the chapters of his thesis from separate published articles. The thesis was ranked as honours second class, second division on the basis of a small portion known not to be plagiarised. Combined with top quality course work, the student received a second class, first division honours degree. This student went on to obtain a PhD and become a lecturer at the same university, with no apparent hindrance to his career. No action was taken to expose the plagiarism or require resubmission of the thesis.

In an arts department at an Australian university, students found evidence of plagiarism in a book, being used as a text, written by one of their lecturers. The students brought this to the attention of other lecturers, who confirmed a pattern of using portions of the text, style of presentation and references of secondary sources apparently without consulting the originals. Nothing was done due to fear of defamation. The author of the suspect text received a promotion.

At an Australian university, an individual was appointed, over well qualified applicants, to a lectureship. The appointee claimed in his application to have nearly completed a PhD thesis at a prestigious overseas university. But the PhD was never completed; later investigation revealed that only a limited amount of work had been done at that university. The appointee received both the sympathy of colleagues and tenure.

In an arts department at an Australian university, a lecturer confronted his professor with evidence of the professor's plagiarism. The lecturer was physically threatened by the professor. The university administration, notified of the evidence and action, transferred the lecturer to another department against his will; it did nothing about the allegations about the professor.

These cases are typical of the cases of fraudulent behaviour that never come to public notice. Their most prominent feature is the reluctance of colleagues or administrators to raise the issues in any public forum. Cases that receive considerable publicity illustrate some of the same processes, as the following Australian examples show.

Ron Wild was professor of sociology at La Trobe University, author of many books and other publications and a prominent figure in the discipline. In 1985 a book of his, *An Introduction to Sociological Perspectives,* was published by Allen and Unwin. It was not long before several academics alleged that extensive passages from the book were taken, without sufficient acknowledgement, directly from other sources. Publicity about this led Allen and Unwin to withdraw the book, and eventually La Trobe set up an inquiry into the apparent plagiarism. In 1986, Wild resigned and hence the incomplete inquiry was disbanded. Wild soon obtained a high-paying job at Hedland College of Technical and Further Education, in a remote location that many would consider to be academic siberia.[21]

Alan Williams was appointed professor of commerce at the University of Newcastle in 1977. About 18 months later, a senior lecturer in the department, Dr. Michael Spautz, raised serious questions about the quality of Williams' PhD thesis. Later Spautz alleged that the thesis contained extensive plagiarised passages, namely that Williams had quoted but not cited secondary sources, giving the false impression of having consulted the primary sources. Spautz, receiving no response to his allegations, broadcast them more and more widely. The university convened an inquiry into Spautz's behaviour, and later dismissed him from his tenured position. The allegations into Williams' thesis were never systematically investigated by the university.[22]

William McBride is one of Australia's best-known scientists, widely noted for his discovery of the link between thalidomide and deformed babies. In 1987, Norman Swan of the Australian Broadcasting Commission published allegations that McBride had falsified data in a paper published in the *Australian Journal of Biological Sciences,* namely changing figures for doses of scopolomine administered to pregnant rabbits and manufacturing data for two nonexistent rabbits. This had occurred in the early 1980s. Two junior researchers under McBride, Phillip Vardy and Jill French, had tried to raise the problems with directors of Foundation 41 where the research was done, but got nowhere and resigned. Seven other junior researchers wrote to Foundation 41's Research Advisory Committee about the allegations; they were retrenched. The *Australian Journal of Biological Sciences* did not publish a letter sent by Vardy and French. The case would never have received public attention but due to the persistence of journalist Norman Swan. Another persistent journalist, Bill Nicol, had written a book about McBride, including this case and other information, but for years was unable to obtain publication due to the risk of defamation. Nicol's book only appeared after Swan's stories and with Swan's help.[23]

After the public revelations about McBride, Foundation 41 set up an inquiry which found that McBride had engaged in scientific fraud. Yet, some time after the inquiry reported, McBride returned to the Board of the Foundation.

Michael Harvey Briggs built his scientific reputation on research into the effectiveness of contraceptives. He worked in universities in Zambia, the United States and New Zealand, and spent four years in England working for the West German pharmaceutical company Schering

Chemicals. In 1976 he joined Deakin University as foundation professor of human biology and dean of science. As a professor and dean, he was one of the university elite. In addition, he attracted sizeable research funding to the university from the pharmaceutical industry, was a consultant to the World Health Organisation and attended numerous overseas scientific conferences each year. Briggs had many supporters at Deakin among both junior scientists and the university elite.

Others were suspicious of him and his work from an early stage, including Deakin professor Mark Wahlqvist, a colleague of Briggs. Prominent Melbourne researchers Bryan Hudson and Henry Burger raised their doubts with Deakin's Vice-Chancellor Professor Fred Jevons in 1983. Jevons put questions from the anonymous scientists to Briggs and conveyed Briggs' responses to them; they decided at that stage not to proceed further.

Dr Jim Rossiter also had doubts about Briggs, and he persisted in raising them. Rossiter, a paediatrician and member of Deakin University Council (representing the community), was also chairperson of the university's Ethics Committee. Rossiter wrote a letter to Briggs querying his method of recruiting women subjects for contraceptive research and questioning his analysis of specimens. Rossiter was not satisfied with Briggs' reply and, in 1984, filed a formal complaint with Jevons.

When Jevons set up a preliminary committee to decide whether formal charges should be laid, Briggs opposed this and succeeded in obtaining the intervention of the University Visitor to halt the preliminary inquiry. In this Briggs had the support of many Deakin staff, the Federation of Australian University Staff Associations and the Chancellor of the University. After Rossiter, joined by Hudson and Burger, made new allegations, a new inquiry was set up. This inquiry was promptly terminated when Briggs resigned.[24]

Let me now summarise some of the common threads illustrated by these cases, and offer explanations in terms of my analysis. The first point is the reluctance of institutions to deal with cases of alleged fraud. This is apparent in most of the less publicised cases where, usually, no formal action is taken at all. It seems that formal inquiries have only been instituted under pressure of media attention, as in the Wild, McBride and Briggs cases. Even then, the inquiry may be quickly disbanded on resignation of the individual concerned. All this suggests that the priority is on limiting not fraud but damage to the reputations of the institutions concerned.

The other side of this reluctance to take formal action is the difficulties faced by those alleging fraud.[25] Without the efforts of Dr Jim Rossiter, the case against Briggs might never have been pursued; for his pains, Rossiter received hundreds of threatening phone calls. Phil Vardy, Jill French and the other seven Foundation 41 staff lost jobs because of their attempts to have McBride's behaviour investigated. Michael Spautz was dismissed as a result of his campaign to expose Alan Williams. In some of the unpublicised cases, individuals obtained advancement on the basis of shoddy work; those passed over in the process obtained no compensation.

That these sorts of experiences are common is attested by Charles McCutchen, a researcher at the US Department of Health, Education, and Welfare who has considerable experience with cases of scientific fraud. McCutchen writes that:

> In protesting scientific fraud, the whistleblower soon realizes that he or she will have few allies. The biomedical science establishment has taken the position that fraud is very rare, and will use almost any means to maintain that illusion. Its response is sufficiently savage to make whistleblowing professionally suicidal if the accused is either important or can involve someone important. This means that one cannot in good conscience ask for support, because one has no right to get the career of an innocent third party destroyed.
>
> The de facto alliance between perpetrators of scientific fraud and the biomedical science establishment is reflected in the response of the scientific journals. I know of only one, *Neurology,* that has published a direct exchange between accuser and accused. *Nature* has been ambivalent, and all other journals I know of have either avoided the issue, or, like *Science,* been captives of the nothing-is-wrong establishment.[26]

These cases demonstrate that the admission of wrongdoing is extremely rare. Ron Wild never admitted to plagiarism; Alan Williams has never publicly responded to Spautz's allegations; William McBride denied any wrong-doing even after he had been pronounced guilty by an inquiry into his behaviour; Briggs only made admissions when he thought they would not be quoted, and he later denied them. In another case, Oxford University Press published a notice acknowledging that a book published by them, written by Helge Kuhse of Monash University's Centre for Human Bioethics, required additional citations to prior work by philosopher Sue Uniacke. Yet Kuhse was reported as denying plagiarism.[27] It seems a fair generalisation to say that no one publicly admits to misrepresentation or bias of a serious sort.

It is worth mentioning here that allegations of fraud are difficult to sustain. This poses difficulties for all parties. In one case, allegations of plagiarism were used to keep a young statistician from obtaining a job. She had no way of countering the allegations, which were made in a referee's report.[28] Because there are few formal mechanisms for dealing with such allegations, it is usually the more powerful individuals who win in confrontations. It is much easier to wreck the career of a PhD student than a world-famous scientist.

The focus on fraud can distract attention from other aspects of the dynamics of science. What is interesting here is that a number of the individuals accused of fraud have also been engaged in other potentially dubious practices which, however, have not been subject to question. Here I will focus on biased viewpoints linked to the interests of powerful groups.

Briggs was considered a successful scientist partly because he was able to obtain large research funding from private industry. In particular, he conveniently found that the contraceptives manufactured by the company that funded his research were more effective than the contraceptives manufactured by competitors. In principle, he could have been accused of scientific bias, or of conflict of interest. Although this may have happened privately, it did not and perhaps could not have formed the basis of a formal complaint against him. Biased viewpoints are normally tolerated: it is standard practice for researchers to be funded by vested interests, and common for the research findings to support those interests. There were critics of Briggs from the time of his appointment at Deakin, but they were unable to challenge him on the basis of his industrial funding. Industrial funding is too common to serve as a strong point of attack.

Nor was anything done about Briggs being a guest author, namely listed as co-author of publications to which he had contributed little scientifically. Guest authorship is too common to serve well as a point of attack. Similarly, nothing was done about Briggs' lack of interest in the activities of his research students and other research nominally under his purview. Inadequate supervision or scientific oversight are also too common to serve as effective points of attack. It took the charge of fraud — manipulation of data — to bring Briggs down, and even that was a tortuous process.

Like Briggs, McBride obtained extensive funds from companies and often made scientific and public stands convenient to them. He received large funding from the lead industry, and in his research and public statements dismissed the role of lead in birth defects. Again, there is the possible presence of scientific bias and conflict of interest. Again, this never formed the basis of action against McBride. His viewpoints on lead and other substances were tolerated. Only the publicity about fraud was enough to bring him down.

Spautz initially made criticisms about Williams' thesis that had nothing to do with fraud. Spautz claimed that Williams had confused cause and effect in claiming that owner-managers of small businesses failed because of psychological shortcomings, rather than the psychological problems resulting from the stress of a failing business. Spautz's sober rebuttals of Williams' thesis argument were rejected by two journals.

In summary, what I have called potentially dubious practices are widespread in science, and indeed it is virtually impossible to survive as a scientist without participating in some of them. Because many of them are considered standard practice or tolerated, they seldom become a focus for concern. My argument is that the most important reason why a practice is tolerated or castigated is its relation to the dominant groups affecting science: government, industry and scientific elites. Raising the issues of misrepresentation of achievement, bias in appointments and biased viewpoints linked to powerful groups threatens one or more of these groups in a systematic way. Hence there is no severe stigma attached to these practices.

Fiddling with scientific data in a major way, on the other hand, is of no particular benefit to any of these groups. It is still risky for an institution to expose this behaviour, because of bad publicity. Nevertheless, most of the blame can be put on individual scientists. It is this process to which I next turn.

FRAUD EXPOSURE AS RITUAL

It is difficult indeed to publicly expose a scientist for fraud, but it sometimes happens, as the Briggs and McBride cases show. These few cases serve as a ritual cleaning of the house of science.[29] In the morality play of storybook science, all are honest except for a few bad apples. When these are exposed, they suffer a severe, yet just, penalty.

The cases of Briggs and McBride did indeed lead to an outpouring of denunciations of fraud in science. The inquiries into their actions, however belated, formalised the process. The message was that the erring scientists had been exposed and penalised, that the system of quality control worked (eventually), and that other scientists were in the clear. The inquiry into the work of Briggs' collaborators served to make this quite explicit: only Briggs was at fault, and the others should be excluded from the taint. $120,000 was provided by Deakin to help their research recover from the episode.

Scientists can be quite righteous about honesty in their profession. They typically claim that fraud is very rare, much less common than in other occupations. This belief is made possible initially by the definition of corrupt behaviour, limiting it to particular extreme cases of misrepresentation such as blatant and detectable altering or manufacturing of data. Such behaviour is defined as terrible and punishable. It is conveniently defined as being quite distinct from the wide range of other misrepresentations and biases that pervade scientific practice

The focus on a few individual violators serves two important purposes. First, it divides the scientific community into the guilty and the innocent by heaping large amounts of contempt on the few singled out as violators. In this way it binds together the majority of members of the community, reaffirming their essential virtue. Second, it isolates a few behaviours as corrupt, and implicitly stamps others as blameless. In this way the interests of corporate and government patrons of science, and of scientific elites themselves, are less likely to come under attack. They benefit from the perception that corruption has to do with what is called scientific fraud and not with obvious misrepresentations and biases which serve their interests.

Briggs would have maintained his successful career had he continued to do research benefiting his pharmaceutical company patron. His weakness was not service to a vested interest or even a cavalier attitude to duties expected of a dean of science, but in failing to follow some of the technical niceties in his research. Of course, one of the prime reasons for fraud is to obtain results that are convenient for a preconceived result, which is often tied to a vested interest such as a corporate patron.[30] Most scientists realise that doing research tied to the interests of particular groups causes no problems. Briggs' failure was to not back up his bias with appropriately careful scientific work.

The usual remedies proposed for scientific fraud are codes of ethics and imposition of penalties for violators. From the perspective presented here, these approaches are largely useless, because they focus only on a narrow subset of problems with scientific practice and leave unchanged the power structures which are centrally important in causing the problems. Furthermore, supposed crackdowns on fraud may have undesirable consequences, such as slowing or inhibiting the publication of unorthodox ideas, since unwelcome papers can be given extra scrutiny under the guise of ensuring quality control.[31]

Structural changes that would affect the level of misrepresentation and bias in science include reducing the power of scientific elites, untying the link between quantity of publication and career advancement and reducing the impact of government and industry funding on science. Specific examples include flat salary structures and anonymous publication. In this paper, it is impossible to deal with the ramifications of such drastic changes, not to mention strategies to bring them about. Suffice it to say that scientific fraud, whether defined as usual in narrow terms or broadly conceived as a range of types of misrepresentation and bias, cannot be seriously affected by tinkering with a few policies. Fraud is an integral part of the way science is organised today. It is safe to predict that official concern about fraud will continue to be triggered mainly by bad publicity rather than by fearless and dispassionate investigations into systemic problems in the practice of science.

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