

Research Ethics and Integrity for Social Scientists

EIGHT

Integrity and Misconduct

Introduction

Researchers owe a professional obligation to their colleagues to handle themselves honestly and with integrity. They need to maintain intellectual honesty in proposing, performing and reporting research, accuracy in representing contributions to research proposals and reports, fairness in peer review, and collegiality in scientific interactions, including communications and sharing of resources. Such matters may not appear in codes or guidelines relating to research ethics, as many national and institutional governance arrangements distinguish between research ethics and research integrity. The former invites scrutiny before research takes place, the latter after. The former concerns relationships with research participants, the latter with the host institution, sponsors and other stakeholders. The regulators, investigatory powers and penalties for breach of ethics and integrity may also be quite different. So, the United States has separate Federal legislation (42CFR93), Australia has the Australian Code for integrity, and Canada has the Tri-Agency Statement. Work by the Global Science Forum of the Office of Economic Cooperation and Development, the European Science Forum and the World Conferences on Research Integrity has stimulated greater international and interdisciplinary consideration of research integrity and misconduct. It has also spurred the creation of further international codes and guidelines as well as national structures that remain separate to the research ethics sector.

And yet, the distinctions are often more apparent than real. In some languages, the same word covers both ethics and integrity. From the point of view of a researcher, both ethics and integrity involve working through a series of possible actions and reaching a conclusion about what might or might not be defensible. For a researcher, determining whether or not to deceive a participant, accept credit as an author or declare an interest in a decision, all look

like questions that relate to the appropriate conduct of research. Conceptually, research ethics does not stop once a proposal has been reviewed and matters of integrity may arise well before data is collected. In practice, conflicts of interest appear in both kinds of codes and some statements on integrity regard breaches of research ethics as constituting a breach of integrity.

Research integrity

Some codes and guidelines such as the Canadian statements issued by the Research Councils United Kingdom (2011), the Canadian Tri-Council (2011), the National Health and Medical Research Council, Australia (NHMRC) (2007a), the Association of Universities in the Netherlands (2004/2012) and the German Office of Ombudsperson describe their remit in positive tones and emphasize integrity and the concepts of honesty, carefulness, independence and fair recognition. However, the dominant position in many other jurisdictions dwells on misconduct. In 2000, the United States Office of Science and Technology Policy published the Federal Policy on Research Misconduct. The policy applied to all research funded by Federal agencies, including work in the social sciences. The policy defined research misconduct in the fairly tight terms of fabrication, falsification and plagiarism. *Fabrication* is 'making up data or results and recording or reporting them'. *Falsification* is 'manipulating research materials, equipment, or processes, or changing or omitting data or results such that the research is not accurately represented in the research record'. *Plagiarism* is the 'appropriation of another person's ideas, processes, results, or words without giving appropriate credit'.

The threefold definition of misconduct as fabrication, falsification and plagiarism (or 'ffp') has become part (albeit in varying forms) of research codes for Australia, China, Denmark, Estonia, Finland, Germany, Hungary, India, Japan, Norway, the United Kingdom, The Singapore Statement on Research Integrity (Wager and Kleinert, 2011) and the European Science Foundation. However, the definition of integrity varies over time and space, or in Cossette's terms, is a 'spatio-temporal intersubjective construct' (2004, p. 214), depending in part on the role played by the definition. Where the definition has legal status and is meant to hold researchers and institutions accountable, the acts and degree of intention associated with misconduct may be tightly demarcated. Where definitions are intended to promote broader research or social values, the field may be conceived more broadly (Fanelli, 2011).

So, several codes extended their definition to include other matters (as did the original Office of Science and Technology Policy code). These are sometimes known collectively as 'questionable research practices'. In the case of

the Australian Code for the Responsible Conduct of Research (NHMRC, 2007a), for example, research misconduct also includes conflict of interest and 'deception in proposing, carrying out or reporting the results of research' (s. 10.1). The remit used by the Research Councils in the UK encompasses: undisclosed duplicate publication; misrepresentation of data, interests, qualifications, experiences, and involvement; mismanagement of data; and breach of duty of care (including improper conduct in peer review) (Research Councils United Kingdom, 2011, pp. 7–8). The Canadian Tri-Agency Framework (2011) also covered destruction of records, redundant publication, invalid authorship, inadequate acknowledgement and mismanagement of conflict of interest. The Norwegian Research Ethics Act of 2007 defined academic dishonesty as including 'other serious violations of good scientific practice that is committed willfully or with gross negligence in the planning, implementation, or reporting of research' a definition also adopted in Danish legislation (Danish Committees on Scientific Dishonesty, 2009). Behaviours addressed by the Chinese Association for Science and Technology (Xinhuanet, 2008, cited in Zeng and Resnik, 2010) included: duplicate publication; 'unethically interfering with other people's research'; 'conspiring with other people's misconduct or retaliating against whistle-blowers and giving unfair review due to a conflict of interest'.

A review of international practice for the Canadian Research Integrity Committee (Hickling Arthurs Low, 2009) distinguished between three kinds of national regulatory systems. The first type, typified by the United States, Norway and Denmark, had a narrow legal definition of integrity and a central regulatory agency with powers of investigation. In Norway, the legislation was administered by the National Commission for the Investigation of Scientific Misconduct and encompassed all research institutions, including the private sector.

The second type – which would include Australia, Canada, Germany, the Netherlands, the United Kingdom and Sweden – had no national legislation, but research councils had created model guidelines and devolved responsibility to research institutions to develop their own policies as a condition of funding, and advice or investigation is provided by an independent body. In the United Kingdom, the United Kingdom Research Integrity Office had no statutory powers, could not investigate independently. It is no longer funded by Universities UK, but it does still maintain an advisory service supported by voluntary contributions. Instead, funding agencies switched their attention in 2012 to a Concordat to Support Research Integrity to which institutions had to subscribe if they wanted grant funding. Having rejected an earlier draft, the Academy of Social Sciences (2013) eventually supported the Concordat, though the Academy remained wary the agreement might turn into a cumbersome, risk-averse, bureaucratic exercise that did little to encourage scholars to grapple with issues

of professional integrity. In China, Croatia and Poland, the state established an independent commission to investigate misconduct.

The third group, comprising countries such as Japan, Eire, France, Spain and Portugal, as well as most middle- and low-income countries had neither national legislation nor independent oversight, and responsibility was at best diffuse and at worst ambiguous (Ana et al., 2013). It may have rested with individual institutions or was left to peer review. In Eire, guidelines developed by research funders were applied by institutions, although the closure of the Irish Council for Bioethics in 2010 probably hindered further policy development. In the case of Japan (Masui, 2011) and France (Alix, 2011), systems were still being developed.

At a supranational level, several organizations have drafted their own statements on research integrity. The European Commission funded a European Network of Research Integrity Offices, and the European Science Foundation set up a Member Organisation Forum on Research Integrity. The Forum supported the establishment of national institutions in its member countries. It drafted a European Code of Conduct for Research Integrity in association with the European Federation of National Academies of Science and Humanities – ALL European Academies (ALLEA) (European Science Foundation, 2011). In 2010, the Second World Conference on Research Integrity completed The Singapore Statement on Research Integrity (Wager and Kleinert, 2011) as a guide for future regulations. The authors of the Statement argued that it contained fundamental principles relating to honesty, accountability, professional courtesy and fairness, and good stewardship of research, as well as 14 professional responsibilities that together ought to transcend legitimate national and disciplinary differences. The Statement was augmented in 2013 by the Montreal Statement on Research Integrity in Cross-Boundary Research Collaborations.

Commentators have been divided on the extent of research misconduct. In defending the withdrawal of funding from the United Kingdom Research Integrity Office, the Chair of the Higher Education Funding Council for England (HEFCE) pointed to the very small number of cases of significant, proven research misconduct. In reply, the editor-in-chief of the *British Medical Journal* and the Chair of the Committee on Publication Ethics (COPE) suggested the lack of examples had 'more to do with a closed, competitive, and fearful academic culture than with Britain's researchers being uniquely honest' (Godlee and Wager, 2012). Godlee's survey of 2,700 British-based scientists and doctors found that over ten per cent had witnessed colleagues altering or fabricating data.

Closer to social science, Cossette (2004) received 136 responses to a survey of administrative science researchers in Quebec. Researchers reported that fragmented publication, plagiarism and self-plagiarism, guest authorship and poor citation practices were all moderately frequent. Bedeian and his colleagues (2010) asked faculty based in 104 management departments in United States business schools whether they had observed or heard about colleagues engaging

in various forms of research misconduct in the previous academic year. Among 438 usable responses, over 70 per cent reported knowledge of colleagues who had 'withheld methodological details or results' (79.2%), 'selected only those data that support a hypothesis' (77.6%) or plagiarized (72.1%), all matters that fell within the United States Federal Policy on Research Misconduct. In addition, over three-quarters reported knowledge of faculty who had engaged in post-facto hypothesizing (91.9%), duplicate publication (86.2%), or had accepted or assigned ghost or gift authorship (78.9%).

Several factors that might encourage misconduct are likely to pose an even greater threat in the future. Many academics are under increasing pressure to publish (and to do so in English irrespective of their competence in that language) as their nation or institution seeks to establish or defend its placing in international research rankings. So, individuals are forced to meet publication targets in order to obtain jobs and grants. In other cases, such as the United Kingdom, Australia, New Zealand and Hong Kong, research infrastructure is funded according to the results of a national research performance evaluation. A fear these pressures will corrode research integrity has been voiced in many countries including Brazil (Vasconcelos et al., 2009), Canada (Hickling Arthurs Low, 2009), China (Postiglione, 2007; Zeng and Resnik, 2010), Hungary (Hungarian Academy of Sciences, 2010), Iran (Ardalan et al., 2009) and Malaysia (Poon and Ainuddin, 2011).

One factor propelling plagiarism in particular into the public eye is the tension between norms that govern academic conduct and those that appear to have been operating in other spheres. There is, for example, a tendency within government and corporate bureaucracies to adopt authorship practices that would constitute misconduct in an academic setting. In 2011, several German politicians were found either to have plagiarized their doctorates or to have relied rather heavily on work undertaken by parliamentary researchers. By May 2012, the website VroniPlag had used crowd sourcing to identify misconduct in the case of 23 research higher degrees, and had triggered the proceedings that had led to many being rescinded (Schuetze, 2011; Vogel, 2012). Similar websites have been established in Russia, Romania and Spain. In some countries, a university undergraduate or postgraduate degree is either required or highly desired among those seeking public office, and politicians and bureaucrats have taken short cuts in order to overcome this hurdle. Scandals relating to higher degree theses have brought down cabinet ministers in Germany, the president of Hungary and a vice-president of the European Parliament, and have troubled the Prime Minister of Romania. Vladimir Putin was undisturbed by claims that a part of his economics thesis was lifted from a 20-year-old American management text. Nevertheless, in 2013 when the Russian Prime Minister announced a campaign against plagiarism by school students, a news website dared him to open investigations into Putin, the Ministers of Defence and Culture, and the leader of Chechnya.

Within most countries, there are some clear gaps in the domains covered by regulatory regimes. Institutions funded by some research councils may be covered by national codes but, in Australia and Canada for example, not all government departments or research agencies, private research organizations, corporations or independent professional practitioners have been subject to the national codes (Hickling Arthurs Low, 2009). As a result, researchers have faced an uneven mix of policies and practices. This has caused difficulties when research or researchers operate across disciplinary, sectoral or national borders (European Science Foundation, 2011).

Fabrication and falsification

In the United States, the Office of Research Integrity has considered allegations of misconduct, although its remit extended only to publicly funded biomedical research. In 2011, 240 allegations were reported and research misconduct was found to have occurred in 13 of the 29 closed cases (Office of Research Integrity, 2011). In each case, the matter involved falsification and/or fabrication. The National Natural Sciences Foundation of China investigated 542 allegations of misconduct involving government-funded scientists between 1998 and 2005. It found misconduct in 60 cases – 40 per cent involved falsification, seven per cent fabrication or theft and 34 per cent plagiarism (Gong, 2005). Following this, the Ministry of Science and Technology established the Office of Scientific Research Integrity Construction to investigate misconduct.

Working within psychology, Sterba (2006) classified misconduct in data analysis and reporting, distinguishing between overt and covert forms of activity and providing examples of each (see Table 8.1). The different forms reflected various distortions of the scientific data. As a Chicago statistician observed, 'The more you torture your data, the more likely they are to confess, but confessions obtained under duress may not be admissible in the court of scientific opinion' (Stigler, 1987, p. 148, cited in Bedeian et al., 2010).

Table 8.1 Overt and covert misconduct

Overt misconduct	
Dichotomizing continuous data	reduces variability and can create significant results
Cross validating exploratory data procedures with confirmatory data procedures on the same dataset	capitalizes on chance variation in the dataset

Overt misconduct

Not testing alternative models that equivalently fit the data but imply different theoretical conclusions	prematurely thwarts the consideration of competing theories
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Covert misconduct

Trimming data in a systematic way	omits outlier data points to sway the significance level in the direction of stated hypotheses
Capitalization on chance variations in datasets	predictors reported as if theoretically conceived a priori, with no mention of other predictors tried and eliminated
Selective reporting	of background literature to inflate the importance of a proposed project; of model fit criteria on the basis of a skim for acceptable values; of the parameters of the model that was fit to the data

Source: Sterba, 2006

For example, Leung (2011) criticized the practice within management research of presenting post hoc hypotheses as if they were a priori (capitalization on chance variations in datasets). Leung found none of the 47 quantitative studies published in the *Academy of Management Journal* in 2009 had rejected more than half of their hypotheses. Over the year, only 16.4 per cent of 251 reported hypotheses were not supported. Although this may reflect ‘impressive foresight’ on the part of researchers or, indeed, bias in the peer review process towards suppressing ‘loser hypotheses’ in favour of positive results, Leung concluded some degree of shift away from the formal hypothetico-deductive model may have contributed. While Kerr (1998) rejected all forms of what he termed ‘HARKing’ (Hypothesizing After the Results are Known), Leung was prepared to accept there might be reasons not to publish hypotheses that detracted from the coherence of a study. Equally, Leung accepted a researcher, inspired by empirical findings, might modify hypotheses ‘to provide an insightful, coherent theoretical basis for the research, but not to artificially inflate the positive findings’ (p. 475).

Although investigations of falsification and fabrication have progressed further in biomedicine, there is some recognition these practices may be rife in other disciplines, such as management and economics, areas that have otherwise not developed much of a literature on research ethics or integrity. In a study by Bedeian et al. (2010), over one-quarter of respondents in departments of management reported knowledge of colleagues falsifying data. Bailey et al. (2001) undertook a self-report study of American academics using a randomized response technique and concluded about 3.7 per cent of articles in the

top accounting journals and four per cent of articles in leading economics periodicals were seriously tainted by falsification. List et al. (2001) surveyed 20 per cent of attendees of the 1998 meetings of the American Economic Association. From a 23 per cent response rate, they estimated more than four per cent of respondents had falsified data at least once, though not necessarily for publication. While some might argue falsification of data in disciplines such as economics may have a less harmful impact than in biomedicine, List and his colleagues noted such practices could lead to the adoption of harmful economic policies.

There are documented examples of falsification or fabrication in history, management and psychology. The language of the American Historical Association's Statement on Standards of Professional Conduct (2011) has been particularly scathing when it comes to forgery and fraud as an 'undetected counterfeit undermines not just the historical arguments of the forger, but all subsequent scholarship that relies on the forger's work' (p. 3). Yet, in 2002, Michael Bellesiles was forced to resign as professor of history at Emory University in the United States after an investigating committee questioned his scholarly integrity and veracity, finding evidence of falsification and misrepresentation of historical evidence or the sources of that evidence in his book on the origins of American gun culture (Katz et al., 2002). Cramer (2006) meticulously detailed Bellesiles' falsification and misrepresentation of original sources, finding serious errors 'on almost any page, picked at random' (p. 168).

In 2006, an internal investigation by the University of Colorado found lack of integrity in the research of Ward Churchill, a professor of ethnic studies, and subsequently fired him. The investigative committee determined he had published an article under another professor's name and then cited that article to support his subsequent claims, misrepresented two pieces of legislation relating to Native Americans, and falsified and fabricated claims the military deliberately infected Indians with smallpox at Fort Clark in 1837. Having reviewed three pages of data drawn from seven published versions of Churchill's smallpox tale, Thomas Brown concluded: 'Churchill has fabricated incidents that never occurred and individuals who never existed. Churchill falsified the sources that he cited in support of his tale, and repeatedly concealed evidence in his possession that disconfirms his version of events' (2006, p. 100). The University's findings were rejected by the Colorado Conference of the American Association of University Professors (2011).

Ulrich Lichtenthaler, a management professor at the University of Mannheim in Germany, had 13 papers retracted by eight different journals, after readers noticed irregularities in his statistical analysis in 2012. Dubbed a 'serial salami slicer' (West, 2013), Lichtenthaler submitted similar papers to different journals without declaring the overlap and, bizarrely, offered diverging interpretation of the same dataset in different publications.

In 2011, Marc Hauser resigned his position in the psychology department at Harvard University, after an internal investigation found him solely responsible for eight counts of scientific misconduct. With a background as an evolutionary biologist, he was working in cognitive neuroscience on the issues of cognition and morality in research comparing monkeys, tamarins and human babies. Hauser had, among other things, fabricated data relating to control experiments. Hauser's Dean later identified that Hauser's experiments had problems involving data acquisition, data analysis, data retention as well as the reporting of research methodologies and results (Smith, 2010). In 2012, the Federal Office of Research Integrity concluded Hauser had engaged in six cases of research misconduct in work supported by the National Institutes of Health.

The reputation of social psychology in the Netherlands took a battering when, in three separate cases, academics were found to have fabricated data. Erasmus University Rotterdam (2012) withdrew three articles published by Dirk Smeesters, professor of consumer behaviour and society, and accepted his resignation. Smeesters' 'massaging' of data was uncovered by Uri Simonsohn, an academic at the University of Pennsylvania, who found the data too good to be true (Enserink, 2012a). A more serious case concerned Diederik Stapel, a social psychologist working at Tilburg University. In 2011, he was found to have fabricated the data for 55 peer-reviewed journal articles and ten PhD theses that he supervised. Other work contained serious methodological flaws. For some of the articles, Stapel took responsibility for gathering data and then provided his co-researchers with a fictitious dataset that fit their hypotheses. An investigation (Universiteit van Tilburg, 2011) into Stapel's work concluded that 'effects were large; missing data and outliers were rare; and hypotheses were rarely refuted' (translated from the original Dutch in Callaway, 2011). Stapel (2012) finally admitted 'The truth would have been better off without me' (translated from the original Dutch in Enserink, 2012b). Finally, in 2014 an internal investigation by the University of Amsterdam called for the retraction of an article co-authored by Jens Förster, one of its experimental psychologists (van Kolschooten, 2014). The Dutch psychologists' fabrications might have been more easily spotted if they had documented their work in such a way that others might quickly check their findings. However, as Wicherts and his colleagues discovered (Wicherts et al., 2006), 73 per cent of researchers who had published in one of four high-impact American Psychological Association journals failed to share their data (in breach of APA Ethical Principles): 'Several data sets, authors said, had been misplaced, whereas others were kept secret because they were part of ongoing work, or because of ethical rules meant to protect participants' privacy' (Wicherts, 2011). These results were particularly disturbing as Bakker and Wicherts (2011) found a high incidence of reporting errors in relation to null-hypothesis significance testing among a representative sample of 281 published papers in

psychology journals (around half had reporting errors and 15 per cent contained at least one result that was erroneously presented as statistically significant) and that some errors were predictive of researchers' unwillingness to share data (Wicherts et al., 2011).

Some of the most serious allegations in psychology involved Cyril Burt, a prominent British psychologist. After his death in 1971, he was accused of fabricating data obtained when studying pairs of twins for his work on the inheritance of intelligence. Hearnshaw (1979) argued Burt added to his original data by inventing results from new sets of twins. Even now, the argument has not been settled. Hearnshaw's claims were initially accepted by the British Psychological Society. However, the Society later withdrew its statement and no longer has a position on the matter (British Psychological Society, 1992). Unfortunately, many of Burt's papers were destroyed after his death and, because of inadequate description of his methodology, it proved impossible to replicate his work. Joynson (2003) suggested: 'Either Burt had committed the most serious scientific fraud since Piltdown man, or he was guilty of no more than carelessness and muddle' (p. 410). One reason for the sloppiness in Burt's research and writing was that he seems to have had few scruples about how he promoted his own work. For example, he was the founding editor of the *British Journal of Statistical Psychology*, in which he published 63 of his own articles. Apart from taking short-cuts in reviewing his own work, he altered the work of others without their permission, often to his own advantage, and attacked colleagues under pseudonyms.

Unfortunately, the Dutch scandals were not limited to social psychology. Mart Bax, a retired professor of political anthropology from the Free University of Amsterdam, was found by his former institution to have engaged in 'serious scientific misconduct', 'deception' and 'unethical scientific behaviour'. He padded his curriculum vitae by adding 64 non-existent papers, inventing awards and roles at prestigious universities, and may well have fabricated his fieldwork in both Brabant and Bosnia (Baud et al., 2013). In 2014, the first of his papers was retracted by *Ethnic and Racial Studies*.

Plagiarism

Plagiarism is one of the more prevalent forms of academic misconduct outside the biomedical field, particularly in qualitative social science research. The Office of Research Integrity (1994, p. 5) in the United States defined it as:

both the theft or misappropriation of intellectual property and the substantial ... unattributed verbatim or nearly verbatim copying of sentences and paragraphs which materially mislead the ordinary reader regarding the contributions of the author.

Working within their own discipline, two American economists surveyed 127 journal editors (Enders and Hoover, 2004) and 1,208 economists (Enders and Hoover, 2006) around the world. The editors reported that collectively they encountered at least 42 instances of plagiarism each year, while 24.4 per cent of the researchers reported they had been plagiarized – 87 by having their original idea or methodology used without attribution, 42 their model, proof or derivation, and 15 their privately collected dataset. The rest involved infringement of copyright as chunks of text (up to and including 90 pages) were reproduced as if original. Levin (2011) noted that, over one year in the mid-2000s, editors of 20 major primary research journals connected to the American Psychological Association on average reported one case of plagiarism, mostly relating to incidents of self-plagiarism.

Plagiarism is one of the more difficult forms of academic misconduct to prove. In his detailed examination of the process of detecting, analysing, assessing, reporting and preventing plagiarism, Decoo (2002, p. 120) identified various ways people who have been accused of plagiarism have attempted to neutralize the accusation:

The wording is quite different from that of the alleged source. The overlap is minimal and accidental. The sources used were properly cited, but in a different place. Every competent reader would know what the obvious source was. The sentence is a truism that many people would write the same way. The copying of that part was inadvertent.

Accusations of plagiarism have been levelled against senior researchers on every continent, including in Australia, Brazil, China, Croatia, Egypt, India, Iran, Korea, Latvia, Pakistan, Peru, Serbia, South Africa, Vietnam, the United Kingdom and the United States (several incidents were discussed in Heitman and Litewka, 2011). Academics whose native language is not English may find it more difficult to express their findings in English in ways that are accepted as original and, as a result, scientists in both Brazil (Vasconcelos et al., 2009) and Turkey (Yilmaz, 2007) have reported seeing either a lack of understanding of the nature of plagiarism or a lack of appreciation that it breaches acceptable conduct.

Findings against academics in social science are rarer. In 2002, David Robinson, the vice-chancellor (president) of Australia's largest university, Monash University, was forced to resign after the university discovered he had been found guilty of plagiarism on at least two separate occasions while working as a sociologist in the United Kingdom. Robinson had not told Monash of his misconduct when the university appointed him vice-chancellor. One of Robinson's critics commented: 'Having a plagiarist as head of a university is like having an embezzler running an accounting firm' (Bigelow, quoted in Madden, 2002).

Abebe Zegeye, a professor of sociology, was dismissed from his research directorship at the University of the Witwatersrand in 2010 and then forced to resign from the University of South Australia early the following year. The South African university initiated an inquiry as a result of complaints from three senior international academics, Kwame Appiah, Stuart Hall and David Goldberg, that Zegeye had 'blatantly, repeatedly and extensively misrepresented published work of a range of authors [including themselves] as his own' (from the report of the arbitrator, quoted in Maslen, 2011). The institution appointed an arbitrator who reportedly uncovered 140 instances of plagiarism in nine publications for work that was undertaken over eight years, with material being recycled from 30 scholars (MacFarlane, 2011).

The head of the Beijing University Anthropology and Folklore Centre, Cai Hua, was accused of plagiarizing and misrepresenting the work of earlier Chinese anthropologists after his work on the Na received strong reviews from Western anthropologists of the calibre of Clifford Geertz and Claude Lévi-Strauss. Xiaoxing Liu criticized Cai: 'He plagiarizes his data, ideas and other sources from his predecessors, Chinese researchers of the 1960s, selects only data that support his arguments, and maligns these researchers to cover up his misconduct' (2008, p. 298). Cai published his doctorate in French in 1997 and it was only after it was republished in English in 2001 that Norwegian and Chinese anthropologists began to question its integrity. Cai was able to take credit for the work of his compatriots by translating their material at a time when few works by Chinese scholars had been translated into Western languages and few Western anthropologists had studied Chinese. Cai was the second plagiarism case at Beijing University in a short period of time. In 2002 another director of the Folklore Centre, anthropology professor Wang Mingming, was demoted after plagiarizing almost 100,000 words of William A. Haviland's book *Cultural Anthropology* (Xueqin, 2002).

There have been cases elsewhere. A professor of psychology and education at Columbia University's Teachers College in New York was dismissed for 'two dozen instances of plagiarism' (Bartlett, 2008). A political scientist and former head of the Middle East Center at the University of Utah was sacked in 2011 for engaging in a 'pattern of plagiarism' that included his doctoral thesis and five book chapters (Maffly, 2011). In 2008 and 2009, the director of the Centre for the Study of Ethnicity and Culture at the University of Birmingham in the United Kingdom, was found guilty on multiple counts of plagiarism in relation to a book, an edited collection and an article (Newman, 2009). Ten per cent of English-language articles in Serbian social science journals by Serbian authors published between 2000 and 2009 involved plagiarism, according to a study by the Centre for Evaluation in Education and Science in Belgrade (Šipka, 2010).

When a group of Russian academics reviewed 25 history postgraduate theses written by public officials graduating from Moscow Pedagogical State

University, they discovered 24 relied on plagiarism for at least half their contents and that the same number had fabricated references (Shuster, 2013; Sonin, 2013). Vroniplag found three authors from the University of Münster had plagiarized large parts of their book on legal writing. It is hard to resist reproducing Wikipedia's comment: 'even the chapter on plagiarism was plagiarized. And although the book told students not to use Wikipedia, the book itself contained 18 fragments taken from the German Wikipedia' (Wikipedia, 2012).

An Indonesian professor in international relations at the Parahyangan Catholic University resigned after publishing an opinion piece in *The Jakarta Post* that reproduced material from an Australian academic (Fitzpatrick, 2010). In 2011, the vice-chancellor of the University of Peshawar was found by a committee of Pakistan's Higher Education Commission to have plagiarized work on Afghan-Pakistani relations (Aftab, 2011). A more complex set of affairs emerged in Vietnam when a professor at the University of Economics Ho Chi Minh City, whose textbook had been copied by another, was in turn found to have translated and published under his own name a book by an American author on international financial management (*Vietnam News Brief Service*, 2010).

Authorship

Leaders of research teams may also be in a position to exploit the labour of their colleagues. Researchers face enormous pressures to publish or, at least, look like they are publishing as they struggle to obtain grants or jobs. Quantity may take precedence over quality in publication. Different disciplines maintain incompatible conventions and different countries face distinct pressures to determine authorship in particular ways. An inevitable consequence has been that tensions have arisen over the attribution of authorship.

Some were the result of blatant research misconduct. In 1990, Carolyn Phinney, a psychologist at the University of Michigan, was awarded US\$1.67 million in damages after a court found another psychologist, Marion Perlmutter, had stolen her research. Perlmutter had falsely claimed the work belonged to her, had sacked Phinney from the laboratory, and then stolen the data from Phinney's office (Charatan, 1997). However, most researchers engaged in misconduct are less brazen. Health researchers have had long-standing concerns (Mowatt et al., 2002; Sheikh, 2000) that the names that appear at the top of an article in their field do not reflect the true authorship, either because someone who has insignificant involvement has been added – gift, honorary or prestige authorship – or because junior staff who made significant contributions have been omitted – ghost authorship. Marušič et al. (2011) conducted a meta-analysis

of 14 studies in various disciplines that surveyed researchers' own experiences or knowledge of others' problems with authorship. On average, 29 per cent of respondents had encountered such difficulties. For example, Martinson et al.'s (2005) study of 3,000 scientists funded by the United States National Institutes of Health found ten per cent of respondents admitted to 'inappropriately assigning authorship credit'. Indeed, drug companies have cross-bred research with marketing by using the names of senior academics to launder the source of the data and legitimate the process (Grassley, 2010):

In extreme cases, drug companies pay for trials by contract research organizations (CROs), analyze the data in-house, have professionals write manuscripts, ask academics to serve as authors of those manuscripts, and pay communication companies to shepherd them through publication in the best journals. (Sismondo, 2007, p. 1429)

In one of the few investigations of authorship decisions adjacent to the social sciences, Geelhoed et al. (2007) surveyed 109 authors who had published in major clinical psychology journals in 2001. Twenty-seven per cent reported having encountered ethical violations in relation to assignation of credit with respect to their 2001 article, 18 per cent in relation to gift and nine per cent in relation to ghost authors. In work I undertook with colleagues in health sciences, we argued:

The implications of ghost and gift authorship can be serious. First, concerns about authorship may bring the integrity of the research into question. Quite simply, the people who are putting their names to the research might not be able to attest to a lack of fabrication or falsification of results. If these forms of misconduct were rife, such misrepresentation might be sufficiently high to bias the evidence base ... Second, it is difficult to respect academic work that is based on a falsehood or the exploitation of more junior members of a research team. Finally, any failure to tackle dubious practices may entrench a culture that rewards with funding, promotion and prestige those researchers who, at best, make questionable decisions about attributing authorship ... (Street et al., 2010, pp. 1458–1459)

Other conflicts arise from varying disciplinary traditions and national priorities. Roger Jeffery explored how difficulties in international collaboration involving researchers from both developing and developed countries could emerge from 'differences in capacity and ability to engage with the requirements of international peer-reviewed journals or book chapters; varying and temporary engagements with the project; and different priorities for academic and research careers' (2013, p. 16). These could cause tensions between approaches that build capacity in the global South and enhanced collaboration and those likely to yield an output valued by research assessment exercises conducted in the global North.

In the Singapore Statement for authors, Wager and Kleinert (2011) stated what should be blindingly obvious: 'authorship of research publications should ... accurately reflect individuals' contributions to the work and its reporting' (s.6.1). The International Committee of Medical Journal Editors (ICMJE) released Recommendations for the Conduct, Reporting, Editing, and Publication of Scholarly Work in Medical Journals in 2013. This recommended four conditions be met before someone be included as an author (s.II.2):

1. Substantial contributions to the conception or design of the work; or the acquisition, analysis, or interpretation of data for the work; AND
2. Drafting the work or revising it critically for important intellectual content; AND
3. Final approval of the version to be published; AND
4. Agreement to be accountable for all aspects of the work in ensuring that questions related to the accuracy or integrity of any part of the work are appropriately investigated and resolved.

The Australian Code for the Responsible Conduct of Research drew on the earlier Vancouver Protocol which only included the first three conditions. Consequently, elements from this standard are applied to all social science researchers in that country. However, the Australian Code has not required all authors to meet all conditions. This more inclusive position mirrored the codes adopted by professional associations associated with psychology, sociology and education in the United States (Bebeau and Monson, 2011).

The British-based Committee on Publication Ethics (COPE) is a voluntary body for scientific journal editors. Its *Guidelines on Good Publication Practice* (2003), no longer available on its website, acknowledged 'there is no universally agreed definition of authorship' but required 'as a minimum, authors should take responsibility for a particular section of the study' (section 3). Where authors could only take responsibility for specialist disciplinary contributions, this could be indicated in the article. COPE required the names of any professional writers be disclosed and urged researchers to be 'vigilant' in ensuring their names were not added to a paper simply to 'add credibility' (section 3(6)). COPE also counselled researchers to reach early agreement on what was expected of each contributor and collaborator and how this would be reflected in decisions about authorship.

Outside the biomedical field, academics have been found putting their name to the work of others. In 1996 Julius Kirshner, a history professor from the University of Chicago, was found guilty of plagiarism by his own institution. A book review written by Kirshner's research assistant had been published under Kirshner's name in a journal he co-edited. Kirshner claimed, implausibly, he believed he owned the ideas in the review because the research assistant was employed by him (Cage, 1996). Somewhat bizarrely, one research assistant appropriated the name of an academic psychologist, Mitchell J. Prinstein, albeit in a slightly disguised form. The junior researcher sought to

create a fictional co-author in the hope the real academic's standing in the field might improve the chances journals would accept his manuscripts. A bemused Prinstein (2011) reported how he first came across a manuscript bearing the name Mitch Prinstein and ultimately discovered a graduate with a history of felony arrests had been appointed to a job on the basis of a fraudulently inflated curriculum vitae. Once in post, he was laundering money through the lab and running a 'fraudulent practice using the lab resources' (p. 180).

Even in the unlikely event that the issues of guest, gift and ghost authorship do not arise regularly in social science journals, social scientists collaborating with colleagues from medical backgrounds may have to confront such practices. In the medical field, Albert and Wager (2003) urged researchers being pushed towards actions they regarded as unethical to do two things. First, they should explain calmly to their collaborators that they thought the action might constitute academic misconduct and that an editor might decline to publish if he or she found out. Second, they should document the discussion. Many social scientists may be pleasantly surprised to find that, despite its origin in medical science, ICMJE's work might support their own views of what does not constitute authorship.

Indeed, if interpreted in the way favoured by the Australian Code, the Vancouver Protocol would recognize the roles played by indigenous communities in community-based participatory research and scholars from the global South engaged in multinational projects (Jeffery, 2013). Castleden and her colleagues (Castleden et al., 2010) surveyed Canadian scholars engaged in collaborative work with Indigenous groups. They found a variety of views on what constituted authorship. Some researchers required Indigenous community members to contribute to the writing process itself, others saw authorship as requiring broader intellectual input into a project. The latter position reflected a collaborative process negotiated throughout the research relationship (see Chapter 5) and aimed at maximizing benefits for the community (see Chapter 7). Unfortunately, the 2013 ICMJE recommendations have closed down this possibility for medical journals.

Duplicate and redundant publication

Other difficulties arise when researchers engage in duplicate or redundant publication, publishing papers that share exactly the same data, discussion and conclusions, or present little or no new material and test what Doherty (1998) described as the "minimal publishable unit" to achieve the maximum number of publications'. In his discussion of 'the dirty dozen habits of highly masochistic authors' (2005, p. 326) Daniel Feldman, the editor of the *Journal of Management*, railed against submissions 'Skating on thin ice in terms of

idioplagerism, simultaneous submissions, slicing the salami too thinly on data sets, and not providing full disclosure to the editor of potential manuscript overlaps' (p. 327).

Wager et al. (2009) investigated the views and practices of 231 editors-in-chief of the academic journals published by Wiley-Blackwell (three per cent of whom were editors of social science journals). Editors were particularly concerned about duplicate and piecemeal publication. In their role as associate editors of the *Academy of Management Journal* and the *Journal of Applied Psychology*, Kirkman and Chen (2011) recalled receiving submissions that used, without acknowledgement, the same data-set as another paper that they had encountered: 'While we cannot ascertain the intent of authors who did this, the attribution made by such behavior is almost always negative and can really hurt the reputation of authors' (p. 442). Similar stories were repeated to Schminke (2009) by editors of other management journals, and by Eden (2010), the editor-in-chief of the *Journal of International Business Studies*. Bretag and Carapiet (2007) used electronic text comparison software to uncover unattributed textual reuse among ten, randomly selected and high-publishing, Australian scholars working in the social sciences, arts and humanities. Having examined 269 digitally available articles published between 2003 and 2006, Bretag and Carapiet found evidence of substantial self-plagiarism among four academics and lesser self-plagiarism among two more. In one case, two articles published virtually simultaneously contained a 55 per cent overlap of text.

It may be appropriate to publish similar articles in different journals in order to ask different research questions, link to different literatures or reach new and different audiences. However, many editors expect authors to identify and justify such a strategy, declaring whether they had published or were preparing to publish papers closely related to the manuscript that had been submitted, thereby warning later researchers that sought to develop a meta-analysis (ICMJE Recommendations, 2013, s.III.D). Without such transparency, it is easy to interpret overlapping publication as poor academic practice and an attempt to pad a researcher's curriculum vitae. Nevertheless, sometimes it is a difficult line to draw and editors may be reluctant to be punitive: 'It can be difficult to know how finely to slice and dice the ideas in a given research program and still maintain some originality of publishable importance in each further morsel that is produced' (Rosser, 2010, p. 15).

On the other hand, some cases are more obvious. In Korea, a professor of education asked for his appointment as presidential senior secretary for education, science and culture to be withheld, in the face of allegations of duplicate publication (*The Hankyoreh*, 2008). In 2011, Bruno Frey, an economics professor at the University of Zürich was censured by the *Journal of Economic Perspectives* and blacklisted by the *Journal of Economic Behavior and Organization*. Working with two Australian-based co-authors, Frey had published four papers about passenger behaviour during the sinking of the *RMS Titanic*. The

four papers presented the same research without citing each other and were described by one editor as 'substantively identical' (Autor, 2011, p. 239). Frey took full responsibility and promised the journal it would never happen again. Unfortunately, the blogging community quickly identified Frey had a long history of publishing closely related pairs of papers (Storbeck, 2011b). In addition, Autor recognized 'considerable overlap' between the papers and an article published by another Queensland academic in 1986 (Storbeck, 2011a).

Editorial ethics

How editors allocate limited space in their journals has been a source of debate. Very little has been written about editorial misconduct, and what little exists has been about medical research. However, two examples offer some indication of possible problems within the social sciences. In 2004, Donald Light and Rebecca Warburton, an economic sociologist and a health economist respectively, submitted a paper to the *Journal of Health Economics*. This led to what the former editor of the *British Medical Journal*, described as a 'ripping yarn of editorial misconduct' (Smith, 2008). Light and Warburton were critical of an analysis by academics from the Tufts Center for the Study of Drug Development in Boston of expenditure on research and development by big pharmaceutical companies, a paper that had appeared in 2003 also in the *Journal of Health Economics*. Among other things, the two social scientists were concerned the Tufts authors had inflated the amount spent on research, used data that remained confidential and so could not be independently verified, and had failed to declare to journal readers that the Center at which the work was done had received funding from the drugs industry. The editors, all professors in health economics at Harvard, accepted the critique for publication but demanded major changes and retractions of information about industry funding. In addition, the editors allowed the authors of the 2003 article several months to write a rejoinder that was longer than the original critique. Light and Warburton were allowed two weeks to deliver a brief reply. The editors then pulled all papers out of production. Following threats of legal action by Light and Warburton, the editors reinstated the papers but subjected the reply to heavy editing and published it in 2005 together with a second rejoinder from the Tufts team. Light and Warburton (2008) wrote an account of the episode for a health policy journal edited by Harvard students in which they detailed authoritarian, unethical and unaccountable practices employed by the editorial team.

Teixeira and da Costa, two Portuguese economists, submitted work to a special issue of a journal specializing in 'innovation studies'. Their paper was accepted by the guest editor and proofs were sent out, corrected and returned.

The authors were surprised to discover their paper was to appear in a different journal, albeit one with the same editor-in-chief and publisher. According to Teixeira and da Costa (2010), the shift in journals occurred at the behest of the editor-in-chief and without the agreement of the authors nor the approval of the guest editor. When challenged, the editor-in-chief told the authors that they were attempting to apply pressure on the publisher to accept a paper that did not meet the requirements of the first journal.

Peer review is used by editors of journals and books to seek advice from experts on the quality of manuscripts submitted for publication. The majority of English-language social science journals use a double-blind process where the identity of the reviewer and author are withheld from the other party (British Academy, 2007). The process confers legitimacy on both the publication and authors. In 2013, COPE published guidelines for peer review. Most editors believe reviewer misconduct is not a serious issue for their journal (Wager et al., 2009). Yet, there has been some evidence in the biomedical field that it fails to operate as a reliable mechanism for quality control. Stung by criticism of their discipline in the wake of the Smeesters and Stapel retractions, social psychologists pointed to a bias against uncovering fraud unless there were already reasons to suspect it:

There are several reasons for such reviewer blindness. Because fraud is relatively rare, its possibility is not generally contemplated. Science is based on trust, and scientists find it difficult even to consider that members of the club might be cheating ... There is a rich social psychological literature on biases in human reasoning and decision-making, including both the 'hindsight bias' – explaining why people are always cleverer after the fact – and the confirmatory bias in hypothesis testing, whereby researchers seek information that confirms their hypothesis and ignore data that contradict it. (Stroebe and Hewstone, 2013, p. 34)

Indeed, the peer review process may be open to bias on grounds that include the author or the reviewer's nationality, language, specialism and gender (House of Commons Science and Technology Committee, 2011). Richard Smith described peer review as 'slow, expensive, ineffective, something of a lottery, prone to bias and abuse, and hopeless at spotting errors and fraud' (2006, p. 116).

In smaller research communities defined by geography, language or academic specialism, or where authors' self-citation cannot be excised, it can be difficult to secure anonymity for authors or reviewers. As a result, the 'temptation to find fault with a rival's efforts must sometimes be irresistible' (Goodstein, 2002, p. 31). Some researchers have used their position as peer reviewers to block publication of an article that might threaten their own interests (World Association of Medical Editors, 2002). Evidence of bias in the peer review process has been less obvious in social science. Indeed, in the British Academy (2007) review of the practices of 96 journals in the humanities and

the social sciences, only six per cent of respondents required referees to complete a conflict of interest declaration.

Instead, in the social sciences, conflicts of interest in peer review are more often conceived as political than ethical. There have been criticisms of the long-term cumulative effect of editorial decisions and editorial appointments. Joe Feagin (1999), as president of the American Sociological Association, noted mainstream editors of journals such as his own Association's *American Sociological Review* rarely published qualitative or theoretical pieces. He was particularly troubled when his Association's Council rejected its own publications committee's first two choices for editor of the *Review* and instead chose two other candidates. The Council's rejection of the committee's recommendations was unprecedented and was condemned by members at the Association's annual conference and by the executive committee of the Association of Black Sociologists.

It is also possible that the peer review process can be unkind to interdisciplinary scholarship. A professor of healthcare organization warned the House of Commons Science and Technology Committee in the United Kingdom that 'a paper which seeks to bring disciplines or ideas together ... can be sent to academics who work in narrow silos who will reject work as wrong, inappropriate or not relevant ...' (Mark, 2011). On the other hand, there have been suggestions that authors have attempted to subvert peer review. Xiao-Ping Chen (2011), editor of *Organizational Behavior and Human Decision Processes*, indicated authors might send out manuscripts to potential reviewers, note the names of any that were critical in their acknowledgements, and thereby ensure journals excluded these academics from the list they consulted. And yet, despite these failings, most journals still use peer review because, in the words of a *Times Higher Education* Editorial it 'remains the worst system we have, except for all the others' (Gill, 2013).

There are also examples of 'coercive citation practices' where editors have required authors to add citations from their own journals into submitted articles without any indication why the addition might improve the quality of the papers. Wilhite and Fong (2012) analysed responses from 6,672 researchers and 832 journals in business, economics, sociology and psychology and concluded such 'coercion is uncomfortably common' (p. 542). In its most extreme form, some editors have produced review articles that, by excessively citing their own journals, inflate a journal's impact factor. This can be relatively easily identified when authors or journals self-cite, and Thomson Reuters has suppressed metrics for some journals as a result. However, it could be tough to spot if a cartel of journals engaged in such practices (Davis, 2012). In 2012, 88 journal editors mostly working in the areas of psychology and management endorsed a code of conduct that condemned 'tactics more focused on engorging impact factors than the advancement of science' (Ethical Practices of Journal Editors, 2012).

Conclusion

This chapter has reviewed issues relating to integrity that rarely find a place in social science research commentary. In the biomedical field, many of these matters – such as the issues of fabrication, falsification and plagiarism – are considered in terms of research integrity and scientific misconduct, and important work has been completed on defining, describing, assessing and preventing misconduct. The United States Office of Research Integrity has identified problems in relation to research integrity in the biomedical research in that country. There are, of course, examples of misconduct in other countries and in other disciplines. However, the extent of the problem is largely uncharted, and there are reasons to believe that the underlying causes of research misconduct might be intensifying as scholars and institutions cut corners in efforts to chase greater output. On the other hand, crowdsourcing the monitoring of plagiarism offers a very promising avenue for curtailing the freedom past plagiarists have enjoyed, by overcoming physical or linguistic barriers to accessing source or copied material.

Few jurisdictions have bothered to investigate the nature and extent of research misconduct in the social sciences even if they have adopted national guidelines. The evidence from surveys and from a few high-profile investigations indicates we cannot be complacent. Those disciplines and jurisdictions that have been slow to attend to research ethics need to respond more quickly to threats to research integrity. Some will inevitably follow the narrower 'fabrication, falsification and plagiarism' definition and more punitive approach of the United States. However, other questionable research practices may offer an equally important challenge to research integrity, and these may be better tackled by placing integrity within the broader and more constructive discourses of honesty and fairness favoured by Australian, British, Canadian, Dutch and German documents. After all, the lines between acceptable and unacceptable academic conduct are not always easy to draw.