Examples of fraud are well documented (see Broad and Wade, 1985 and Judson, 2004). The case of Piltdown Man, in 1912, is perhaps the most well-known instance of scientific fraud (Goodstein, 1991). An alleged fraud within psychology, said to have been perpetrated in later publications by Sir Cyril Burt on the relationship between genetics and the Intelligence Quotient (Hearnshaw, 1979), has been the subject of much controversy (Fletcher, 1991). Goodhart (1999) expresses what appears to be a widely accepted view that

‘much of what Burt published after he had retired was muddled and badly presented, and would never have been printed had it not appeared in a journal of which he was editor, and with no proper reviewing system. But that is far from saying that there is evidence here of fraud. Some of the inconsistencies are so obvious that the data could scarcely have been intentionally falsified: even in old age, Burt could have made a better job than that had he intended to deceive!’

The system of peer review, missing in Burt’s case, although by no means foolproof, provides a safeguard against fraud when operated conscientiously. Similarly, the requirement of open access to full details of results operated by many journals, together with an expectation that important findings will face replication (perhaps leading to the dreaded ‘failure to replicate’) should provide reassurance. Note however that such standard ‘scientific’ procedures are difficult to apply in much qualitative research.

The mainstream view within scientific communities appears to be that outright scientific fraud probably occurs with a relatively low frequency (Goodstein, 1996). However, as Beutler and Harwood (2001) point out the public perception, and the perception of some in the media, are at variance with this view. Surveys of US graduate students and university faculty (Swazey, Anderson and Lewis, 1993) revealed a belief that the problems involving misconduct in scientific research are more common than insiders believe.
Starbuck (2006; pp. 20-21) provides anecdotal support for this view that the system may not work as effectively as researchers would like to believe. In an instructive and entertaining autobiographical account of his career as a leading management researcher he discusses two incidents strongly suggestive of academic fraud. One case involved a prize-winning dissertation describing the thought-processes of a man who managed investments showing a very close correspondence between the behaviour of a simulation model and the behaviour of the person himself. Starbuck sought to follow this up for his own research and after extensive re-reading of the thesis found many contradictions; the theory would not generate the decisions which the thesis attributed to them. As a result he contacted the decision-maker who said that he had eagerly read the dissertation but found little relation between the theory and his thought processes, and hence was surprised that the theory produced decision so similar to his.

While, at a stretch, one might view this as poor, rather than fraudulent, research a second case is unequivocal. This involved a manuscript sent to a prestigious journal where he was a reviewer. In a reaction to the original manuscript, additional information about interviews was requested. Two months later a revision included surprisingly tidy data from 700 interviews in 50 companies. Suspicions alerted, the reviewer read the dissertation on which the article was based, where no interviewers were mentioned. The only legitimate explanation was that the author had rapidly conducted the interviews after receiving the request for revision. The author on hearing that the paper was not to be published demanded a hearing but claimed that he had destroyed interview notes after recording details on punched cards. These turned out to be multiple copies of the same card from each of the 50 companies. The author had previously published an article in the American Sociological Review on the dissertation data. However, a subsequent request to the American Sociological Association, to publish a warning about it was turned down on legal advice on the grounds that this would be picking out one article as an exception!

Given the current pressure on researchers to publish it would be too much to hope that these are isolated examples. Vigilance on the part of dissertation examiners, and journal reviewers and editors, is crucial.

References


Cannella and Lincoln (2007) provide an editorial introduction to a special issue of the journal ‘Qualitative Inquiry’ where the articles are devoted to articulating different aspects of this concern. Tolich and Fitzgerald (2006), and several other commentators, suggest that the ‘ethics-review process is based on epistemological assumptions aligned with positivistic research, and does not fit the qualitative research process’ (p. 71). In similar vein, Janovicek (2006), writing from the perspective of oral history, asserts that ‘Like qualitative researchers in other fields, many historians have had frustrating experiences applying for ethics approval because REBs prefer research methods and ethical practices that work best for an experimental research model designed to produce generalizable data from large data bases’ (p. 157).

Johnson (2008) narrates her personal experience of obtaining institutional review board (IRB) approval for a dissertation study. Although her research topic was particularly sensitive, she claims that it ‘is illustrative of the increasing level of difficulty qualitative researchers are facing in conducting not only risky research but also work that is not aligned with the National Research Council’s conception of “scientific” methodology’ (p. 212). Cummins (2006) also provides a personal account of her gruelling experiences getting a face-to-face study of children on the farm, and their use of space in rural southwestern Ontario, off the ground from an ethics point of view. Holland (2007) investigated the concerns of a university research ethics committee in rejecting an application to interview people diagnosed with a mental illness. The committee’s concerns included the safety of participants and the author as the researcher, the author’s lack of training and clinical expertise, her disclosure of a past diagnosis of mental illness, the need for provisions to handle any emergencies, and the need to screen potential participants to ensure they were well enough to give informed consent. She regards these as ‘paternalistic and medically derived concerns [which] reflect assumptions about mental illness that are challenged by first-person perspectives, social movements in mental health, and newly emerging work within postpsychiatry’ (p. 895).

Difficulties and misunderstandings are also reported in the context of action research (Brydon-Miller and Greenwood, 2006) and other types of participatory research (Boser, 2006). Another common source of frustration is the amount of time and effort that can be required to satisfy an ethical board or committee. For example, Green, Lowery, Kowalski and Wyszewianski (2006) in a
study of the impact of Institutional Review Board practices on observational health services research found that approximately 4,680 hours of staff time over a 19-month period were devoted solely to the IRB process!

It is worth stressing that the various concerns discussed above appear to be an international phenomenon. The examples cited come from a wide range of countries including the US and Canada, the UK, and Australia. Hemmenki (2005) makes a rather different point, looking at the unintended effects that ethics committees may have on research. She is worried that tedious and bureaucratic rules arising from Finnish law and European directives on clinical trials may result in poor designs with unreliable results and increased costs; that in effect ethics committees are fostering poor research for legal reasons.

References


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