BMJ

LONDON, SATURDAY 17 JUNE 1995

Lessons from the Pearce affair: handling scientific fraud

Belatedly, Britain should abandon its lax approach to scientific fraud

See p 1554

Last week Malcolm Pearce, a British gynaecologist, was removed from the medical register for fraud: he had published two papers in the *British Journal of Obstetrics and Gynaecology* describing work that had never taken place (p 1554).[1] Less than nine months had elapsed between the whistle being blown on Pearce and his removal from the register. Outside observers might therefore conclude that, like other countries, Britain has established methods of preventing, detecting, and managing misconduct in research. They would be wrong. That the Pearce affair was handled well was unusual: the principal of Pearce's medical school knew what to do and was determined to do it—speedily and while protecting the rights of both the accused and the whistleblower. In most other medical institutions in Britain nothing would have happened[2]; the affair would have been brushed under the carpet, and the whistleblower would probably have been hounded out of his or her job.

Despite a report from the Royal College of Physicians,[3] Britain has learnt little about handling fraud since the Darsee affair in the United States first brought the subject into prominence in 1983.[4] This is despite a succession of other major scientific frauds in biomedicine. For example, the Office of Research Integrity, a branch of the US Public Health Service set up to investigate fraud, considered 73 cases in 1994.[5] One particular abuse has indeed been tackled in Britain. Several general practitioners who engaged in fraud during drug trials have been struck off by the General Medical Council,[6] but this has been largely because their frauds emerged through pharmaceutical companies' thorough auditing procedures and because the companies have taken an aggressive approach towards tackling fraud. The same has not applied within academia or the NHS. Until the Pearce case I knew of no other academic who had been investigated thoroughly. Within the NHS the postgraduate deans, nominated by the Royal College of Physicians' report to implement its procedure, know little about their responsibilities—or, indeed, about the report itself.

The Pearce affair has implications not only for the conduct of science and for how allegations of fraud should be tackled but also for medical journals. Commendably, the Royal College of Obstetricians and Gynaecologists commissioned a report into the role of its journal in this affair, and it has implemented the recommendations[7]: if followed more widely they should spell the end of amateurism in journals.

It is often unrealistic to expect a journal to detect fraud, but in this case the practices of the *British Journal of Obstetrics and Gynaecology* did not put as many barriers up to the publication of fraud as they might. Firstly, Pearce was an editor of the journal and the editor in chief was his head of department: this case shows how important it is for editors in that position to hand over consideration of the paper to someone else. Secondly, the journal did not review case reports at all, and, thirdly, the review of the clinical trial was clearly inadequate. Even with hindsight, the credulity in publishing the trial is reminiscent of that regarding Darsee, who claimed to have assayed 10 different hormone concentrations twice weekly in blood obtained from rats' tail beginning at 1 week of age and continuing until death,[8] or regarding Slutsky, who at one time was producing a paper once every 10 days.[9] In this case a more disinterested editor might have questioned the fact that over three years Pearce purported to have collected 191 women with a syndrome so uncommon that a major referral centre was seeing only one or two new cases a month. Moreover, all of them had had a battery of complex tests, including karyotyping of both partners.

But, as the royal college's report makes clear, any journal can be the victim of fraud. Six years ago Drummond Rennie, deputy editor of *JAMA*, proposed a simple editorial audit on one in every 1000 papers submitted[10]: do the records exist, were the laboratory tests done, and what was the role of each "author"? Increasingly I warm to that idea. The cries that monitoring would discourage scientists from starting research have already been answered by Congressman John Dingell, the man responsible for making the American biomedical establishment take fraud seriously. "Scientists need to understand," Dingell said, "that the best way, perhaps the only way, to avoid the threat of 'science police' is for scientists to show that they have the ability and will to police themselves. It is a matter of morality but also of self interest."[11]

Another important aspect of the Pearce affair is the light it throws on gift authorship—the practice of treating authorship as something that is conferred as a benefit rather than earned through taking responsibility. For, as they were reminded in letters from the General Medical Council, coauthors have responsibilities to have done enough of the work to be called to account over it. Here none was qualified to be a coauthor: indeed, coauthorship was impossible since the work had not been done. Nevertheless, there were explanations why their names were included: the two junior authors had already been rebuked for asking questions about details of the work and, as one of the them said at the council's hearing, they were "made to feel small." The most senior author, Professor Geoffrey

Chamberlain, who was also Pearce's head of department and editor of the journal, had twice asked for his name to be removed. Of all the abuses of scientific research, gift authorship is the most common and the most lightly regarded. Even the royal college's report, in comments that I disagree with, states that "Mrs Hamid's contributions...in the way of literature searches and writing of introduction and discussion components...justified her acceptance of coauthorship" and "Mr Manyonda's contribution . . . was at an intellectual level with significant contribution to the discussion . . . there is no ground for criticising Mr Manyonda for being a coauthor of the paper. He had accepted the existence of the case on trust from Mr Pearce." This is an unusual attitude to authorship, at variance with accepted recommendations, which if followed will set the clock back.

Many people accept or confer gift authorship, detection is unlikely, and the rewards are obvious: tenure, promotion, research grants, and fame, especially in a society that measures worth by the weight of papers produced rather than their quality. Another reason why gift authorship is so common may be because the recommendations produced by the Vancouver group, an international group of medical journal editors, are difficult to understand[12]: the group should simplify them and also print the masterly table of legitimate and non-legitimate grounds for authorship produced by Ed Huth, a former editor of the *Annals of Internal Medicine* and member of the Vancouver group.[13] Most importantly, however, we should revise our criterion of worth. As recommended by other bodies,[14 15] appointment committees in Britain should follow the longstanding example of Harvard (requiring candidates for a full professorship, for example, to submit copies of only their 10 best articles).

Crucially, however, the Pearce affair raises questions of management. Firstly, we must accept that fraud exists, though with an unknown prevalence: estimates vary from 27% of scientists encountering 2·5 episodes over 10 years[16] through 0·28% in audits of cancer trials[17] to one new case per million population every year (P Riis, personal communication, 1995). Next, the universal lesson is that institutions are not good at policing themselves, so several countries have set up bodies specifically to do this for them, ranging from the Office of Research Integrity in the United States to central committees on scientific dishonesty in the Nordic countries and Austria. The latter committees teach good research practice, advise whistleblowers, are notified of all cases, and may undertake investigations themselves: moreover, they monitor every case and publish annual reports.

A central committee would also seem the most suitable pattern for Britain, particularly as a single body could acquire the necessary experience and skills that more peripheral bodies would lack. On Danish experience, three quarters of the work could probably be handled by the secretariat (disputes about who owns data and authorship, for example), but some would need "due process," and for this reason the presence of a judge on the committee, as in the Nordic countries, would be important. Some link with the General Medical Council, which has statutory powers over doctors, and the statutory bodies would be inevitable. For this time the public outrage that patients might have been put at risk by Pearce's medical frauds means that the subject will not go away. Given its pioneering work, the Royal College of Physicians should seize the initiative again, convene another meeting of interested parties, and implement a workable solution. The time has come for Britain to abandon its lax approach to scientific fraud.

STEPHEN LOCK
Former editor, *BMJ*

Aldeburgh,
Suffolk IP15 5EE

1 Dyer O. Consultant struck off for fraudulent claims. *BMJ* 1995;310:1554.
2 Lock S. Misconduct in medical research. *BMJ* 1988;297:1531-5.
3 Royal College of Physicians of London. *Fraud and misconduct in medical research.* London: RCP, 1991.
4 Relman A. Lessons from the Darsee affair. *N Engl J Med* 1983;308:1415-7.
5 Office of Research Integrity. *Annual report 1994.* Rockville, MD: ORI, 1995.
6 Wells F. The British pharmaceutical industry's response. in: Lock S, Wells F, eds. *Fraud and misconduct in medical research.* London: BMJ, 1993.
7 Royal College of Obstetricians and Gynaecologists. *Report of the independent committee of inquiry into the circumstances surrounding the publication of two articles in the British Journal of Obstetrics and Gynaecology in August 1994.* London: RCOG, 1995.
8 Knox R. The Harvard fraud case: where does the problem lie? *JAMA* 1983;249:1797-807.
9 Friedman PJ. Correcting the literature following fraudulent publication. *JAMA* 1990;263:1416-9.
10 Rennie D. Editors and auditors. *JAMA* 1989:266:2543-5.
11 Dingell JD. Shattuck lecture—misconduct in medical research. *N Engl J Med* 1993;328:1610-5.
12 International Committee of Medical Journal Editors. Uniform requirements for manuscripts submitted to biomedical journals. *JAMA* 1993;269:2282-6.
13 Huth EJ. *How to write and publish a paper in the medical sciences.* Philadelphia: ISI Press, 1982.
14 Thompson C. Publication quality not quantity. *Lancet* 1994;344:118.
15 Evered D, Lazar P. Misconduct in medical research. *Lancet* 1995;345:1161-2.
16 Hamilton DP. In the trenches, doubts about scientific integrity. *Science* 1994;255:1636.
17 Cancer and Leukaemia Group B. A successful system of scientific data audits for clinical trials. *JAMA* 1993;270:459-64.

Vitamin C and vascular disease

Be cautious about the association until large randomised trials have been done

See pp 1559, 1563

Stroke, coronary heart disease, and peripheral vascular disease have many risk factors, or risk indicators, in common, yet some factors are more important for one vascular bed than another. Cigarette smoking is a stronger determinant of peripheral vascular disease than of stroke, high blood pressure is more important for stroke than for coronary artery disease, and a high serum cholesterol concentration has a greater effect on coronary heart disease than on stroke. Other factors may be equally important in all these conditions, and Meade has argued that this is the case for a high plasma concentration of fibrinogen.[1] In this week's *BMJ* Khaw and Woodhouse examine the association between a low vitamin C concentration in elderly people and a high fibrinogen concentration (p 1559)[2] and Gale and colleagues report cardiovascular mortality according to vitamin C intake (p 1563).[3]

Khaw and Woodhouse followed up 96 men and women every two months for over a year.[2] They measured serum ascorbate concentration and plasma concentrations of fibrinogen, factor VIIC, and acute phase proteins at each visit and obtained a dietary history. There was an association between a low vitamin C intake and a high plasma fibrinogen concentration. There are, however, two difficulties in accepting these findings as causally related. Factors that vary with season will be associated for this reason alone, and the low response rate of 45%[4] may have excluded those with more representative dietary patterns. The authors do not present data on either blood lipids or blood pressure, which also undergo seasonal variation.[4] They speculate that vitamin C may protect against cardiovascular disease through an effect on haemostatic factors at least partly through the response to infection. Vitamin C may reduce the incidence of infections and thus lower plasma fibrinogen concentrations, and there is experimental evidence that a large dose of vitamin C increases fibrinolytic activity.[5]