although their success probably has more to do with their natural ability as empathic teachers than with the various theoretical frameworks underlying their treatment.[13]

We do our patients no service by treating clumsiness as if it were a disease. With only rare exceptions clumsiness is a talent deficit and, like other learning disabilities, is primarily an educational problem.[14]

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Fraud in medicine

Since the *BMJ* last considered fraud enough evidence has accumulated to show that the examples discussed of intentional misrepresentation in the late 1970s and early '80s were not isolated aberrations.[1] There are now at least six known cases of major fraud (whether piracy, plagiarism, or forgery) in clinical science and 13 cases in basic science between 1970 and 1985,[2 3] to which we can add the recently discovered instance of Stephen Breuning, who published some 50 articles based on fraudulent data on the use of psychoactive drugs in mentally retarded patients,[4] and the disclosure that since 1975 the Food and Drug Administration has submitted to the US Attorney 20 cases of fraud and other clinical violations in clinical research into new drugs, with convictions in 13 cases.[5] Nor is the problem exclusive to the United States: in Britain work by Gullis and Purves has had to be withdrawn,[2] while recently a psychiatrist has been removed from the medical register because he supplied non-existent data for a drug company trial.[6] On the other hand, the evidence suggests that the problem is not as widespread as had been claimed, certainly not as high as the 100000 concealed frauds (major and minor) for every major case in science that becomes public.[2]

The issue is important. Not only does fraud represent a waste (of money, manpower, and laboratory resources), in itself unethical,[7] but its continuation might suggest inadequacies in the traditional self regulation of science. And in seeking prevention rather than cure we might find that a major cause of fraud was inherent in the scientific community's false values: the pressure for newness at all costs, the dislike of negative results, and the publish or perish syndrome, whereby excellence is equated with quantity rather than quality, leading to inadequate peer review because of the pressure on the referee.

After a slow start the scientific community has acted. Formal inquiries have been held and their results publicised; agreement has been reached on how to investigate new examples; follow up analyses of two prominent cases (Darsee[9-11] and Slutsky[12]) have been published; and mechanisms for retracting published erroneous work (whether described in good faith or otherwise) have been established—both in *Index Medicus* and on Medline[13] and in journals. Last year the Vancouver group of editors prepared draft guidelines for handling retractions in journals (p 400), emphasising the pivotal role of the authors' institution in the investigations and the need for a standardised format.

The two important analyses have emphasised some of the factors underlying fraud as well as the issues the scientific community needs to address. The first, by two scientists at the National Institutes of Health, Walter Stewart and Ned Feder, was based on 18 full length research papers and about 100 abstracts published between 1978 and 1981 with John Darsee as an author or coauthor.[9] In 1981 Darsee was detected falsifying data in a laboratory study at Harvard. The subsequent finding that his results in another, collaborative study did not tally with those produced at three other centres led to an investigation showing that he had systematically falsified data in research at Harvard and elsewhere. Virtually all of his articles and abstracts were formally retracted. Stewart and Feder's article describes disturbing lapses from accepted scientific standards—whether those explicable by carelessness or haste (numerical errors, inconsistency with previous data, and "gift" authorship) or more serious ones (misleading statements or citations, duplicate publication, and failure to acknowledge others' data).

An analysis from the University of California, San Diego, of another major series of frauds, by Robert Slutsky, reiterates many of the features highlighted by Stewart and Feder and points to some important conclusions.[12] Between 1978 and 1985, while he was engaged in research or training in cardiology, nuclear medicine, or radiology, Slutsky was the author or coauthor of 137 articles. The possibility of fraud was raised by an astute referee who queried apparently duplicated data in two articles read in quick succession when Slutsky applied for promotion. The investigation found experiments and measurements that had never been done, incorrect procedures, and reports of statistical analyses that had never been performed. In all, 77 articles were classified as valid, 48 as questionable, and 12 as fraudulent.

Vast amounts of time and money have to be spent in the full investigation of a case such as Slutsky's. The committee had to examine his entire bibliography, review the laboratory records, and interview laboratory technicians and coworkers. The last might find their reputations hazarded, having to remove articles from their curricula vitae and to live with the onus of having worked with somebody who had produced fraudulent work.

Two sets of lessons may be learnt from the many accounts of fraud. The first is practical: young investigators must be closely supervised not primarily to prevent their committing fraud but to teach them good practice and support their daily work; laboratory data books must be retained for several years; and each institute and granting agency must have procedures for investigating suspected fraud, based on putting the onus on the authors of a paper reasonably suspected of being fraudulent to establish that their results are valid. Again, the tendency to frown on the "whistle-blower" should be reversed; rather, as Engler and his colleagues suggest, "collegiality" entails an obligation on research workers to tell superiors about any reservations they have about a colleague's work.

Secondly, we need to rethink the emphasis placed on the number of publications rather than on their quality; most appointment committees still count articles rather than read them. Woolf has documented the high level of productivity in the departments where three of the instances of fraud occurred, with the heads of departments having their name on a total of 29, 31, and 68 articles a year; the productivity for an average "publishing scientist" was one article a year with a range at distinguished universities from 1·8 a year in physics to 2·7 in biochemistry. Stewart and Feder quote a memorandum from the director of one of the world's leading research institutions.

> "Upon proper completion and submission of [two] manuscripts, [a technician]'s appointment will be extended.... During that time it is expected that an additional manuscript... will be completed and submitted. If so, the period of employment will be extended an additional three months and again an additional manuscript... is an anticipated result of the extended employment."

Many of the reports of the Darsee affair speak of his energy and his high productivity. At one stage Slutsky was producing one article every 10 days, at the same time as he was a busy resident in radiology—yet his colleagues' comments about him were praise for his industry rather than questions about the desirability or even feasibility of such productivity.

To be fair, Slutsky's productivity was partly masked by his tendency to bury his name amid those of colleagues awarded gift authorship, with or without their connivance. Here the journal editors could have had a decisive role, by asking each author to justify his or her inclusion (on the basis of guidelines such as those in the Vancouver style (p 401)) and requiring a signed statement that each had seen and approved the final manuscript.

In conclusion, then, we probably have to accept that fraud is an inevitable, if tiny, concomitant of research. Nevertheless, we can do several things to minimise it and its effects. Firstly, by prevention by good laboratory practice. Secondly, if editors and referees were more ready to challenge suspicious elements in articles, an apparently excessive number of authors, duplicate or "salami" publication (writing up a single study in a series of minor papers), and particularly data that are inconsistent, some false publications might be prevented. Thirdly, once suspected or detected, fraud needs intensive investigation with publicity given to the results and retraction in the journals concerned and in the bibliographical databases. Finally, we need to re-emphasise that there is a whole gradation of practices, ranging from honest errors through bias and self interest to forgery of results (figure). Which, of us for instance, has not quoted a reference in good faith only to find on rereading the original that it said nothing of the sort? As has been remarked, in their way Darsee and Slutsky may have achieved more for the good of science with their shady practices than they would have had they stuck to legitimate research.

Good faith	*"Trimming," "cooking"*	*Fraud*
Wrong observations Wrong analysis Wrong references	Manipulating data Suppressing inconvenient facts	Piracy Plagiarism Forgery
Bias Self delusion		
	Gift authorship Duplicate publication Salami publication	Undeclared interest

Some elements in poor science. ("Trimming" and "cooking" were terms introduced by Charles Babbage.[15])

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Rediscovering the diaphragm

In 1959 about 12% of British couples used the diaphragm for contraception.[1] By 1982 it was used by only about 4%—largely because of the rise in the popularity of the contraceptive pill.[2] Family planning clinics were supplying diaphragms to 8% of their clients in 1982, and 1·2% of general practice contraceptive services were for diaphragms.[3] Individual surveys in general practice show that between 0·5% and 4% of the sexually active population are using diaphragms.[4,5] Putting all these data together, the Family Planning Information Service estimates that about 150 000-200 000 British couples now use diaphragms—1-2% of the sexually active population.[3] The diaphragm is thus in decline, but might it return to favour like the condom? What are the risks and benefits of diaphragms in 1988?

Two factors stop more couples using the diaphragm—the relatively high failure rate, and the fact that some couples find them premeditated and messy. The large Oxford study showed that the higher the motivation to avoid pregnancy then the lower the failure rate—for example, in women who had completed their families it was only 0·7 pregnancies for every 100 woman years.[6-8] In younger women with incomplete families, who were using diaphragms to space their children, the failure rate was 5·3 pregnancies for every 100 woman years. Failure became less likely the longer the method was used; the average failure rate for women of all ages and experience was 5-6 pregnancies for every 100 woman years in the first year of use and up to 10 pregnancies for every 100 woman years for young unmarried women of average motivation. Some data from the United States have