reflux nephropathy can result in a progressive decline in function this has worrying implications for living kidney donors.

Everyone has been waiting for the results of controlled trials comparing surgical correction with conservative management of vesicouretic reflux. The preliminary results suggest that there is no compelling case for surgical correction. The Birmingham study set up in 1975 includes 149 children under the age of 15 with newly diagnosed vesicouretic reflux (grade II with scarring and grade III with or without scarring). Children were allocated at random either to surgical correction of vesicouretic reflux or to conservative management. Both groups received continuous prophylactic chemotherapy. Analysis of the results in 49 surgically treated (69 ureters) and 47 conservatively treated children (66 ureters) showed no essential differences. Progression of radiographic scarring as shown by Ecklöf's measurements[16] was recorded in seven of those treated surgically and in 11 of the controls. No significant difference was seen in renal length in the two groups, and new scars appeared in two patients in each group (two over the age of 5 and two with no documented urinary tract infection). Renal function evaluated by edetic acid (EDTA) glomerular filtration rates showed no difference between the groups, but in those aged over 6 there was a difference in concentrating capacity. The rate of "breakthrough" urinary tract infection did not differ in the two groups, but the kidneys of the 28 who developed urinary tract infections grew less than the others.

P G Ransley reported on the important trial of surgery in infants under the age of 1 year with gross vesicouretic reflux which has been conducted at the Hospital for Sick Children in London over the past five years. There are 12 patients in the conservative group and 11 in whom vesicouretic reflux has been repaired surgically. Three of the latter still show vesicouretic reflux; thus, even in the best hands, surgery in infants is not always successful. Five year follow up in seven in the conservatively managed group and six in the treated group showed no difference in glomerular filtration rate estimated by EDTA labelled with ^{51}Cr.

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Regular Review

Fraud in science

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Two cardinal rules of biomedical research are that scientists pursue absolute truthfulness and objectivity and that they report only honest data. Yet in recent years there has been evidence that these rules have been broken, as admirably summarised in Hamblin's review in the Christmas *BMJ* of 1981.[1] Of the three main types of malpractice, perhaps the most dramatic is simple plagiarism, as exemplified by Dr Elias Alsabti, a Jordanian who spent time in postgraduate training in the United States, who simply copied over 60 articles, publishing them under his own name in Japanese and European journals.[2] Second comes plagiarism together with forgery of data. For example, Dr Vijay Soman, a research associate working with Dr Philip Felig at Yale, copied part of the text of an article sent to his chief to referee and added some imaginary data.[1 3] The discovery led to the finding of more papers with data fudged, faked, or missing written by Soman and to the resignation of Dr Felig from his recent appointment to the chair of medicine at Columbia. The final type of fake is concocting false data, as in the case of Dr John Darsee, a research worker in Dr Eugene Braunwald's department at Harvard Medical School, who forged haemodynamic data in a study of the action of drugs on dog myocardium.[4]

Such instances, and others, raise several questions that need answering. Are the cases of piracy-plagiarism-forgery

that we know about only the tip of an iceberg? Why do such cases happen? Is the practice on the increase? And what role should the biomedical establishment, including the editors of medical journals and of information retrieval systems, have in this?

How big a problem?

To answer the first question an informal poll was taken among those attending a meeting of the Council of Biology Editors in Boston in April 1981. Over a third of the members of the audience at a session on the subject raised their hands in answer to a question of whether they knew of a recent unpublicised example of fraud. What did those raised hands represent? Did all the responders know about just one example? Or did each responder know about more than one? And was everyone thinking of the same, or different, examples? Did many others in the audience decline to signal that they knew about additional examples? No one could gather any precise data from that informal poll, but those raised hands represented another indication that the problem is larger and more important than scientists have been willing to recognise.

One reason why the problem looms so large is that for decades scientists have insisted that science was honest and virtually fraud proof. Now, with the wide publicity devoted to scientific fraud, those past denials have created the impression that fraud is a major new problem, even if the percentage of such cases is really no larger than the true hidden proportion in the past. Nevertheless, as further evidence to confirm that, however few their number, every newly disclosed case does raise important points of principle, at least three more instances have been reported since Hamblin's article.

In Britain, after an investigation at the University of Bristol, Dr M J Purves, the reader at the department of physiology there, wrote to *Nature* stating that the data published in the proceedings of the 28th International Congress of Physiological Sciences concerning recent techniques for investigating the function of the mammalian (sheep) brain in utero were false.[5] In his 1982 presidential address to the American Society for Clinical Investigation Dr Philip Majerus said that he knew of two cases of fraud among the 6000 papers he had published as editor of the *Journal of Clinical Investigation*, but he gave no specific details of either.[6] And in 1981 Sir Douglas Black[7] said at a symposium that he had learnt of "two cases where data were not merely misinterpreted but actually fabricated." Since neither Dr Majerus nor Sir Douglas gave references to these cases, we do not know whether they represented four new cases of fraud or whether they were included in the numbers already known.

Publish or perish

To answer the question why there has been the recent wave of incidents one popular explanation is the sense of increased competition and pressure. That view was expressed by a former dean of Harvard Medical School, Dr Robert H Ebert,[8] who, in a letter referring to the disclosure of Dr John Long's fabrication of data about the size of immune complexes in Hodgkin's disease, said: "Medical schools and academic research centers have inadvertently fostered a spirit of intense, often fierce competition which begins during the premedical experience and is encouraged thereafter. Stories of cheating among premedical students are common and the race for high grades so as to ensure admission to medical school is hardly designed to encourage ethical and humanitarian behaviour." Although many have attributed the cases of fraud to human weaknesses, Dr Ebert disagreed: "It would be a mistake to consider this an example of human frailty and nothing more," he said.

Indeed, there is pressure for researchers to publish or else perish because long lists of publications in their curricula vitae tend to help scientists gain grants, promotions, and tenured positions. At a time when funding for scientific research is being cut and jobs are scarce there is an emphasis on getting results and on producing them quickly. Younger investigators are under pressure to turn out papers reporting positive results as fast as possible, since promotions tend not to be based on confirming research done elsewhere or in proving that it was wrong. Further pressure comes because, in the United States at least, medical schools are now economically dependent on the grants generated by researchers.

An additional problem, which Dollery has emphasised, is the common practice of keeping sloppy and illegible laboratory notebooks and hospital records, which leads to dishonest and biased interpretation of scientific data.[9] Finally, there is a widespread feeling that the supervision by many leading researchers of their juniors' work is often lax. Senior investigators often have other administrative activities or research projects and they are not well informed about each stage of every project's development. A technician or staff member may feel that the data have to be manipulated to please his superiors.[10]

There are no mechanisms built into the scientific process to record data about the frequency of fraud. *Index Medicus* contains no headings listing frauds or correcting false information, and at least one medical journal editor (Dr Philip Majerus, of the *Journal of Clinical Investigation*) has declined to publish letters of retraction of frauds.[3] In his presidential address Majerus gave no further explanation for that decision and he rejected as impracticable suggestions that scientific journals should take a greater role in ensuring against publication of false data.[6] Scientists, not medical journal editors, were most responsible for that step, he thought, and he also pointed out that "We are the *JCI* not the FBI."

Society's concern

Nevertheless, to ignore the problem means that the profession has not been honest with itself or the public, thereby doing both medicine and the public a disservice. The Association of American Medical Colleges tacitly acknowledged this in June 1982, when it adopted a committee report, *The Maintenance of High Ethical Standards in the Conduct of Research*.[11] Recommending that institutions should adopt policies that define misrepresentation of research data as a major breach of contract, the association's report suggested that there should be a two tiered system for investigating allegations of fraud. The first should be confidential and carried out by the departmental chairman. The second should be carried out by a committee outside the department concerned—made up, for example, of members from another faculty—and, once the decision had been taken to pass to this stage, the accused's collaborators and sponsors should be informed. If a person was found guilty then institutions and sponsoring agencies with which he had been associated in the past should also be told "if there is any reason to believe

that the validity of previous research might be questionable." If a person was exonerated then full efforts should be made to restore his reputation and that of others under investigation.

This report brought an editorial response from the *New York Times*, which criticised "the naive assumptions on which it is based"—that science is self correcting.[12] "Had the reports' authors looked more carefully at the cases that prompted their inquiry, they would have noticed that none of the frauds was originally brought to light through the standard mechanisms by which scientists check each other's work." A year earlier a subcommittee of the United States House of Representatives, the Science and Technology Subcommittee on Investigations and Oversight, had been concerned at the reluctance of the scientific community to face the problem of fraud in scientific research until it was forced to because of public pressure over new disclosures. Its recently published report is instructive for the disparity it discloses between the attitudes of the legislators and those of the scientists.[10]

The late Dr Philip Handler, former president of the National Academy of Sciences, told the subcommittee that the issue of falsification of data had been "grossly exaggerated." In fact, the problem was too minor to merit congressional attention. Though he agreed that the magnitude of the problem was unknown, it was "remarkable" that fraud occurred so infrequently given the amount of research that took place. Legislators at the hearings said that they were astonished by what they perceived as "arrogance" within the scientific community or the attitude of "we know best, and so therefore we have asked the questions and if we don't ask the questions, no one else should." Disturbed by Dr Handler's statement that the falsification of data need not be "a matter of social concern," one legislator, Mr Robert Walker, argued that there should be some admission within the scientific community that a problem existed, needed to be dealt with, and should be a matter of general societal concern. Mr Albert Gore, Jr, chairman of the committee, added that one reason why the problem of fraud existed was the apparent reluctance among scientists seriously to consider the matter. In citing the Felig case, he said, "If instead those in higher positions of responsibility in science had reacted with a sense of outrage at the first indication of fraud, then this thing wouldn't have dragged on the way it did."

Ways of correcting the problem

What means are there for medicine to correct the problem and restore whatever faith the public has lost in the research community? Some have recommended regular random audits of laboratory notebooks. Boston University has adopted such a measure in response to the incident involving Dr Marc Strauss (who had an alleged role in falsifying records of patients to make them eligible for research projects at the university hospital) (personal communication, public information officer, Boston University, October 1982). Similarly, the United States Food and Drug Administration has increased the number of field inspectors in laboratories,[13] though such a policing measure is impracticable for laboratories in medical centres. Others have suggested that it should be a requirement of publication that laboratory bench books should be available for a minimum of five years afterwards. But, perhaps most important, research institutions (both academic and governmental) must investigate complaints of fraud more promptly and forget the scientific community's traditional reluctance to accept the problem. There is need to establish a system to deal with complaints about fraud and to determine what disciplinary measures are needed once the facts are understood.

Ascertaining the facts about cases of fraud may be difficult and slow, and once established the facts may have astonishing ramifications. Take the case of Dr John Darsee, which is still unfolding. So far it has led all the way back to his undergraduate days at Notre Dame University; prompted federal investigations by the National Institutes of Health of two leading medical schools; involved the authors of two leading textbooks of cardiology; and caused bitter disputes in academic medicine.

Darsee, at Harvard Medical School, was seen to falsify data in May 1981 and when challenged admitted it as an isolated lapse. Dr Eugene Braunwald immediately terminated Darsee's NIH fellowship, informed the dean of the medical school, but did not tell the National Heart, Lung, and Blood Institute, on whose study Darsee was working, and decided instead to scrutinise his work closely. Braunwald concluded that the incident in May had been an isolated one. In October, however, the National Heart, Lung, and Blood Institute released data produced at Braunwald's laboratory and three others elsewhere taking part in a trial to assess treatments designed to protect the ischaemic myocardium. The Harvard data were clearly different from those produced at the other three centres. This revelation prompted the Harvard Medical School to set up an investigative committee to look at Darsee's work. The committee's report, which concluded that Braunwald's own investigations had disclosed the extent of Darsee's fraud, was later strongly criticised by the NIH, which by this time had decided to conduct its own investigation. Darsee was found to have systematically falsified data in at least five animal research studies at Harvard. Braunwald had to write letters retracting all or part of the work in nine published papers in which Darsee had been a coauthor. The NIH report agreed with the Harvard committee that an investigative panel of professors should have been set up once the evidence of fraud had come to light and that other people who had worked with Darsee should have been told of the suspicions. It criticised the committee, however, for not conducting a thorough investigation. In one of its harshest judgments it barred Darsee from receiving NIH funds for 10 years, asked Harvard to return the $122 371 it had received for the ischaemic myocardium study, and placed Braunwald's laboratory under probation for a year to ensure that it maintained proper procedures and a high standard of supervision.[4]

Darsee's fraud, originally thought to have been confined to just one incident, now seems to have begun during his undergraduate days at Notre Dame University. There he published two articles in the student run *Notre Dame Science Quarterly*, which one professor, Dr Julian R Pleasants, believes were fabricated.[14]

Evidence that the fraud continued when Darsee was a resident and fellow at Emory was disclosed only recently, as a result of another investigation. This one, by the Emory faculty, found "overwhelming direct and circumstantial evidence of flagrant and extensive fraud in his research at Emory University and of fabrication of data published in the name of the University even after he was at Harvard."[14]

Specifically, the committee found that of the 10 scientific papers on which Darsee's name appeared, eight could not be substantiated. The data either were fabricated or could not be verified. Also, the committee found that the data in 32 of the 33 known abstracts that listed Darsee as a coauthor could not be confirmed and in many cases are believed not to have been collected. "Furthermore, for 23 of the 33 known abstracts,

the Emory faculty listed by Darsee as coauthors did not participate in the studies described and did not know about the abstracts," the committee said.[15] One coauthor listed by Darsee appears not to exist. Thus, of the total of 43 papers and abstracts on which Darsee was named as an author, 40 are now considered frauds. As a result, Emory is retracting all the papers and abstracts in question. In addition, the committee recommended that corrections appear in the next edition of *The Heart* by Professor J Willis Hurst because the legends of three pictures written by Darsee were inaccurate.[14]

Thus Darsee's case has shown flaws in the supervision of research in academic centres, lax attitudes over authorship in published articles, and weaknesses in the peer review processes that govern the contents of medical journals and textbooks.

Moreover, Darsee's case illustrates that what is needed first and foremost is a change of attitude; as one thoughtful writer has put it: "Profound human costs and considerable financial costs make it impossible to take at face value the kind of official statement that recently issued from the Massachusetts General Hospital—that falsified data published by an investigator on their staff in the *Journal of the National Cancer Institute* had caused no harm even in the field of science. In such cases, the greatest damage appears to be to other scientists and to scientific institutions, although costs to the public (and, in one case, to the patients who were research subjects) have not been evaluated."[16]

Even with these measures, however, the problems of dealing with fraud in science are not nearly so simple as might first appear. For example, one key problem will be to draw a clearer distinction between deliberate and non-deliberate fraud or self deception. Where is the line drawn, for example, between researchers who do not look at data as objectively as they might because they are convinced that their theory is correct and those investigators who deliberately change the data to make these fit their theory? Scientists have long been accustomed to rounding off numbers to make smooth lines in graphs, tables, and the like. But when does that become cheating? Again, researchers often submit abstracts for presentation at medical meetings long before the experiments are completed. As a result, when the data are available, investigators may be tempted to make them fit the preconceived ideas presented in the abstract rather than analyse the data cleanly. This practice may result in more data being fudged than is generally recognised. Not only might the abstracts themselves be less than accurate, but also the data may find a permanent place in the scientific literature.

One critical factor not mentioned in the recommendations issued so far is what, if any, punishment guilty parties should face beyond resigning from the institution where the misdeed took place, reimbursing grants already issued, and losing future research awards. Among the unanswered questions are: Should officials of the institution report the misdeed to the body that governs medical licensure? Should physicians who commit fraud in the laboratory or in clinical trials automatically be allowed to practise medicine without review by licensing officials? What, if any, disciplinary action should the head of the laboratory be subject to? By whom?

Much more attention needs to be given to an unrecognised aspect of the problem of cheating in science: the methodology of clinical trials. Biomedical statisticians contend that physicians and others have cheated by assigning patients to receive their favoured treatment in supposedly randomised clinical trials.

All these features are continually being evaluated routinely by editors of medical journals and their teams of referees. Thus, despite Majerus's objections, we believe that editors could do more to heighten the profession's consciousness of the problem of fraud. Moreover, they also have an obligation to publish retractions in some form when cases of fraud are discovered. It is ironic that, by awakening public and professional concern about fraud, Dr William Summerlin (who falsified a transplantation study by inking in some black patches on white mice[17]), Dr John Darsee, Dr Vijay Soman, and their colleagues in scientific fraud have probably contributed more to science than they would have done had they spent a lifetime doing honest research.

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[Since the authors passed the proofs of this Regular Review the *New England Journal of Medicine* has published a leading article on fraud in science by its editor, Arnold Relman; a formal and prominent retraction of two articles of which J A Darsee was a coauthor; and a letter to the editor by Dr Darsee (1983;**308**:1415-7;1400;1419). The role of the editor in the problem of fraud is also being considered by the International Committee of Medical Journal Editors (the Vancouver group), which will discuss a working paper at its next meeting. —ED, *BMJ*.]