Editors' responsibility in defeating fraud

Sir — As editor-in-chief of *Nutrition*, an international medical journal, and as director of a research laboratory, I found your Briefing on science and fraud most interesting, because I am both a producer and a consumer of science (*Nature* **398,** 13–17; 1999).

My editorial colleagues and I have a high state of awareness of 'fabrication, falsification and plagiarism (FFP)'. As reviewers of manuscripts, we have a difficult time detecting the two Fs, but allegations of the P have come to our attention several times.

I believe that editors have an obligation to the scientific community to pass such concerns to the authors and to their institutes' research dean or administrative supervisor in a confidential manner for investigation according to the guidelines of the US Office of Research Integrity. In doing so, we do not act as "secret police", as the editor of the *Journal of the Norwegian Medical Association* maintains. Instead, we align ourselves with the UK Committee on Publication Ethics and the World Association of Medical Editors, whose recommendations are in my view appropriate.

It does untold harm to the scientific community to be betrayed, deceived and defrauded. Such harm ranges from the squandering of limited research resources to the undermining of confidence and trust in the reporting of scientific findings. A journal should not be used to validate misconduct by publishing fraudulent data submitted knowingly by the author. If this occurs, editors bear an obligation to retract the paper.

Our journal asks authors to sign a declaration of scientific integrity in their letter of transmittal.

To avoid scientific misconduct in my laboratory, each new research fellow's attention is drawn to this potential problem via policy and procedure material given to them on arrival, and the consequences of such temptations are clearly spelled out. Each new fellow also repeats a portion of their predecessor's work to confirm the results, as an internal control standard. This has not dampened the lust for data among the 'young and hungry'. But, ultimately, solid, reliable laboratory habits and supervision and mentoring are critical components to prevent misconduct.

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Inequities in German research system

Sir — You report that the German government, which has been making efforts to rebuild the science base in east Germany, is changing its funding policy, because research in the east is now considered ready to compete with the west (*Nature* **398,** 7; 1999). We would like to add a point that is often forgotten: salaries in east German government-funded research labs are generally only at 86 per cent of the level of their west German counterparts.

This difference was introduced after the country's reunification to take account of the former lower productivity of companies, administrations and labs in the east. Curious effects can be observed as a result. In Berlin, for example, moving to a job across the street can bounce you from 100 per cent to 86 per cent payment (city versus federal funding). This makes it a tough job to hire scientists in the east.

We wonder whether the new east–west competition for federal research funds will in this respect be on a fair and equal footing, and hope that the government will find a way to solve this problem.

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Sir — You report Germany's junior minister for research as saying that east German researchers have been so well supported in recent years that they are now considered good enough to compete with west German scientists for conventional funds. Having worked in research institutes in west Germany, Britain and now in east Germany, I can assure you that it is not the standard of the scientists that has been the limiting factor, but the administration with which they have had to contend.

We have good and bad scientists, like everywhere else, and it says much for the spirit of the good ones that they show every sign of succeeding in the face of adverse conditions and patronizing attitudes.

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Cloning claims challenged

Sir — Karl Illmensee[1] has suggested that the data on mouse cloning he and Hoppe[2] published in 1981 were unjustly neglected or rejected. He opines that, following the recent cloning of mice[3], "the time has come for the *correct* evaluation of our earlier results on the *first* cloning of a mammal" [my italics].

Illmensee and Hoppe[2] reported the birth of live mice following the transfer of nuclei from inner cell mass (ICM), but not trophectoderm, cells into an enucleated zygote. Their method involved denuding the donor nucleus, injection of the donor nucleus into the zygote cytoplasm, and using the same pipette to remove the zygotic pronuclei. To my knowledge, neither this method nor these results were ever repeated[4], though not for want of trying.

Tsunoda and Kato[3] recently reported the birth of live mice following the transfer of ICM or trophectoderm nuclei into an enucleated oocyte. Their method was as follows: metaphase chromosomes were removed without penetrating the egg membrane; a single ICM or trophectoderm cell was introduced under the zona pellucida and fused with an enucleated oocyte using inactivated Sendai virus; and the nuclei of reconstituted eggs that developed to the two-cell stage were fused, again using Sendai virus, with enucleated blastomeres of normal two-cell stage embryos.

We described this method in 1983 (ref. 5), and it has been used by everybody doing nuclear transfer ever since. We do not know all of the parameters necessary for successful nuclear transfer, but it seems that oocytes are much better recipients than zygotes, and it may be that cloning cannot be achieved by the transfer of somatic nuclei into zygotes.

Comparison of Illmensee and Hoppe's methods and results[2] with those of Tsunoda and Kato[3] does not show that their early data should be re-evaluated and rescued from neglect. One could equally well argue that the success of the Apollo missions confirms Jules Verne's or Cyrano de Bergerac's descriptions of voyages to the Moon.

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