

Should camp-followers be policemen?

Journals are bound to be concerned with and by the published products of scientific misconduct, but there must be doubts about their ability to keep a diverse profession's conscience.

Bethesda

THAT the scientific literature is occasionally contaminated with fraudulent or otherwise misleading documents has regrettably become a fact of life. To what extent are journals and, specifically, their editors, able to prevent that by the elaboration of suitable procedures? And are they willing? These were among the questions raised by the Office of Scientific Integrity (OSI) of the National Institutes of Health (NIH) at a small and private conference of a score of journal editors here last week. What follows is not so much a report of the proceedings (of which nothing is attributable to anybody), but an impression of them.

Journals naturally have a vivid self-interest in the avoidance of being stung; too many *errata*, some of which may amount to retractions of published claims, are bad for a journal's reputation, leading would-be contributors to send their manuscripts elsewhere. But is there a wider responsibility, perhaps to the cause of "scientific integrity" as such? And can it be exercised effectively?

Some principles are generally accepted, although not universally practised. Journals should, for example, offer means by which others than the original authors can comment on published research. Otherwise, misleading claims to priority (arising perhaps from oversight) or errors of fact and interpretation cannot be corrected. But that is a bare minimum.

Many US-based journals seem anxious to go — and seem to have gone — a lot further. One objective is to stamp out "honorary co-authorship", the name given by Feder and Stewart (*Nature* **325**, 207–214; 1987) to the practice in which senior researchers add their own names to research reports which are largely the work of lesser fry. The Darsee case shows how honorary co-authorship can land everybody in the soup.

Now, guidelines developed by what is known as the Vancouver group, latterly the International Committee of Medical Editors, are being widely accepted. Among other things, they require all the authors of a research report to attest that they have "participated sufficiently" to "take public responsibility" for its content. Authors must affirm that they have been engaged in the conception and design of an investigation and/or in the analysis and interpretation of data, that they have helped in drafting the report

and that they have seen the final version.

Only to have helped recruit funds or collect data is not sufficient, nor is "general supervision".

These requirements are sensible, yet tougher than they may seem. Physicians who have collected blood samples for a genetic study, for example, or researchers who have contributed to a successful study only DNA clones or specific monoclonal antibodies, would not qualify as authors. And what would happen to the long lists of authors heading reports of experiments in high-energy physics, some of whom may have been responsible only for the design of some crucial electronic component? No recognition?

The trouble with rules is the seemingly recurrent need to break them. Of course, the names of laboratory chiefs should not be added *ex officio* to a list of authors in a research report, nor should those concerned wish that to happen. (The now-recognized dangers may help warn them off.) And people who contribute only materials to other peoples' studies should be thanked for their generosity, not made into fictitious authors.

But what should happen if a scientist, say an amateur, has gathered a mass of empirical data he is not technically competent to analyse? Should credit lie solely with the academic who finally makes sense of it, as these rules would require?

Why should authorship matter so much? Sadly, there are two distinct answers. On the idealistic view of the scientific process, which is the progressive but tentative deepening of understanding, it may be a practitioner's only lasting reward.

But in the world of immediate rewards — promotions, appointments and research grants — authorship has become an indicator of performance. One of the few jokes at an otherwise over-solemn meeting was the dictum that "every editor should have a pimp as a brother so that he will have somebody to look up to". Only a few at last week's meeting seemed concerned (to continue the image) that the scientific literature is in danger of being prostituted to the preferment business.

On the contrary, most speakers seemed content with the guidelines, even to wish that there were more of them. (In the merciful absence of working researchers, one speaker went so far as to say that "medical editors are the keepers of the conscience...") But a brief discussion of

whether it might be possible to list authors' names so that the relative importance of their contributions would be apparent by inspection ran into the sands of infeasibility.

The Vancouver group has now added the requirement that it should be a precondition of publication that authors should be prepared to share research materials with others, and has won the adherence of the Society of General Microbiology and its nine powerful journals. (Others appear already to have consulted attorneys on the problems of enforcement.) There is general applause for data-sharing, through data banks and otherwise, tempered by an acknowledgement that it is rarely as simple as it seems.

Why all the fuss? At least in the United States, there is a genuine difficulty: creeping litigiousness. Presumably it is only a matter of time before some journal is sued by somebody inconvenienced by a mistaken claim. But, for the time being, legal action swirls around researchers, at least one of whom has been sentenced to six months in jail for falsifying a grant application to a federal agency by citing his own earlier work, afterwards shown to be fraudulent.

The growing apparatus of investigative committees (not to mention OSI's own existence) is how institutions must demonstrate their own vigilance, and thus their continued eligibility for federal grants, but there are problems of due process (for accused researchers) still to be ironed out. Meanwhile, it appears that journals would be legally well-advised to agree on "industry standards" so as to be less vulnerable to defamation suits when, for example, reporting what the committees have to say.

Litigiousness evokes a normative response. That, at least, is the charitable explanation of US institutions' rule-making tendencies. This journal, while no less eager than the rest to encourage seemly behaviour, will rather rely on exhortation and the occasional admonitory illustrative example. As in the past, it will look into suspicious circumstances arising from its own postbag. But rules run the risk of burying what remains the chief function of the literature, that of assisting the communication of information among astonishingly creative people — and between them and their successors.

John Maddox