

## Do scientific journals need a code of practice?

THE Scientific Information Committee of the Royal Society has recently put forward a set of guidelines for the refereeing of papers for publication. The status of the guidelines is unclear in the context in which they have been presented (as part of the programme for a conference of editors), but it seems that for the present they are open for discussion, and that even after such discussion they will only represent an informal set of rules to which editors may or may not choose to conform. We believe that such a voluntary code of practice is unworkable, will cause more trouble than it is worth and should be quietly dropped.

The committee is right in one respect: there is a growing feeling that the processes by which scientists regulate each other's activities, be it through grant-giving, refereeing and editing, or even control of membership of learned societies, could with profit be made somewhat more open. There are still, we think, strong objections to a system in which all is revealed and the names of referees and advisers are made public. But there are obvious intermediate positions in which the supplicant at least knows what sort of treatment to expect and can thus choose his own strategy accordingly. Unfortunately the issuing of guidelines suggests a move to uniformity, whereas what is actually needed is a preservation of diversity to allow individual scientists in individual cases to make individual choices.

Furthermore, once a set of guidelines is published there will be a tendency for some aggrieved authors to look to the guidelines for redress, even if the journal has never claimed to conform to them.

In matters of dispute it is clear that the drafting of the document leaves much to be desired, as loopholes abound. Three of the guidelines with which we disagree strongly are:

- Every paper submitted to a journal for publication should be refereed.
- No paper should be rejected on the adverse report of a single referee.
- A definite procedure should be established for editorial decision within a stated period.

However worthy these statements are, they are unworkable—indeed undesirable—in particular instances; and it is often in these very instances

(admittedly rare) that the author is looking for a way to prolong the battle. What is one to do about the author who scatters his paper with unsubstantiated statements, demands that not one but two referees should see it and insists that the journal should decide within a stated period? Supporters of the guidelines say that this is where editorial discretion comes in and of course “every” doesn’t mean “every”. But this leaves us with an additional guideline—‘the editor may choose to ignore these guidelines’. From a straw poll it is clear that many editors by no means conform and have no intention of doing so. It will surely bring the guidelines into discredit if they are widely ignored.

An alternative response to calls for more openness should be considered. If journals were to state regularly what their policy was in handling manuscripts, and the extent to which editorial discretion entered, prospective authors would actually have a clearer impression of how they stood.

With this in mind we have compiled the following brief statement of our editorial procedures.

*Nature* aims to publish scientific reports which are suited to a wider readership than a specialised journal can provide; manuscripts concerned with any branch of science will be considered. The spectrum of papers published broadly represents that of papers submitted.

All papers are assessed by one of the three manuscript editors (two, a biologist and a physical scientist, based in London; one, a biologist, based in Washington). These three editors may consult with other editorial staff and will return roughly a quarter of all manuscripts at an early stage, chiefly on the basis that they do not have any obvious broad appeal. The remainder are sent to one referee (occasionally two) and the referee's recommendation is generally followed. On rare occasions a negative report is overruled when there seems to be a pressing need for a particular paper to be made widely and rapidly available. Somewhat more often a paper that has received only a lukewarm recommendation from a referee is declined for reasons of limited space. Specific comments from a referee are usually passed on to authors.

The objective is to make a decision in principle on papers within a month of receipt, although authors are often asked at that stage to revise or shorten manuscripts. The present acceptance rate for submissions to *Nature* is around 35%, and following acceptance we aim to publish papers within six weeks.

We propose to publish an updated version annually.