am also aware of articles with correct statistical methods that have been rejected because, in the referees' opinion, the statistical methods were not appropriate. Submitting a manuscript again, together with a letter explaining and justifying the statistical methods, is not acceptable to authors. The data of Ernst *et al.* reinforce the need for reputable learned journals to refer all statistical methods (including interpretation of statistical analyses) to a reputable statistician.

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SIR — The system that Ernst *et al.* tested is flawed from the start. To ask reviewers to judge a submitted paper merely numerically is to invite the disparity these authors found. Worse, it provides the editor with no basis for making an independent judgement (except for calculating an average), and provides the author with no basis for improving the paper. Editors of the journals published by the Entomological Society of America (whose *Annals* I have co-edited for 20 years) require reviewers to write — in words — what they think of a paper, and why. The editor can then evaluate both the reviews and the paper; and the author then knows what needs to be improved, or why the paper will not be published. We editors have found this system useful and reliable, and authors have found it beneficial.

The peer-review system has been challenged by many people over many years. Yet no-one has devised a better method for judging submitted papers. The peer-review system is rather like democracy, about which one Anglo-American remarked that it is the worst of political systems — except for all the others.

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SIR — The aims of Ernst *et al.* may be noble (to improve the peer-review system) but their conclusion (that the system is unreliable and lacking in reproducibility) cannot be justified by their mini-experiment. The data can have another interpretation.

The bottom line for many academics who submit manuscripts for publication is whether they are accepted. Whether a paper is considered acceptable, fair, good or excellent by the referees is irrelevant; that will be decided over time by the academic community as a whole.

Once these categories are combined into a single 'acceptable for publication', the data from Ernst *et al.* show that 29 out of the 31 referees thought the paper acceptable while two did not, implying 94 per cent agreement and 6 per cent dissention. Surely that indicates clearly that the peer-review system does work? It certainly does not indicate that the system is unreliable. The consideration of most manuscripts submitted to peer-reviewed journals by more than one referee only strengthens this argument.

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SIR — Ernst *et al.* express concern about the reproducibility of the peer-review system. On the basis of their data, I conclude that peer review is actually quite reliable. Most journals routinely solicit two reviews for each manuscript. When two reviewers disagree substantially on the merit of a manuscript, the paper is often sent to a third reviewer. In the test case presented by Ernst *et al.*, choosing any two reviews randomly from the 31 available would probably yield two generally favourable, reasonably consistent reviews. If an unfavourable review was chosen initially, the probability that a third review, again chosen at random from this group, would be favourable is quite high. The probability that this manuscript would have been viewed very unfavourably (two overall judgement scores of 1 or 2) based on two randomly selected reviewers from this group is obviously not zero; it is 0.017. Do we expect better reproducibility than this from any living system?

Of course, there is always an element of subjectivity in any human judgement, and so there is a chance that a scientifically sound manuscript will be rejected inappropriately under the existing peer-review system. The exercise undertaken by Ernst *et al.* suggests that the chance is surprisingly and reassuringly small. For all its human failings, peer review seems to be alive and quite well.

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SIR — Ernst *et al.* show clearly that, even when a paper is of high quality, getting it accepted is like driving on a bumpy road: one may finish in the ditch. If we want a better system of evaluation, we must replace the present lottery.

We think that an open system would help. A few lines at the end of the paper, saying who accepted the paper (as is often done already), who reviewed it and the main questions raised, would benefit the referee, the author and the journal, as well as inform the reader. Referee work would thus gain kudos and readers could compare their opinions with those of the authors and referees, giving the reading of a paper an additional attraction: the possibility of judging the referees.

Indeed, a few journals already ask authors to suggest potential referees or publish author/referee discussions of invited papers, thus going in the direction we are suggesting.

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SIR — The communication by Ernst *et al.* proves that the peer-review system is gravely flawed. But I have the solution. In January 1984, I submitted a proposal to the US National Science Foundation (NSF) requesting funds; five months later, the foundation sent me my own proposal for review. I naturally jumped at the opportunity to break new ground and sent back a totally objective review. In fact, my review was so objective that the proposal was rejected.

Since then, I have been conducting an in-depth study of the peer-review problem, a problem so vexing that it has even attracted the congressional eye (Stone, R. *Science* **256**, 959; 1992). This study has led me to conclude that not only proposals but also articles and books should be reviewed by their own authors. Recognizing that the author is of course the person most qualified to understand what he (or she) is doing, has done or plans to do, I hereby propose the adoption of the Absolute Review System (ARS), a system in which the authors themselves (the Absolute Reviewers) do the reviewing.

The advantages are several. First, the material will be reviewed, for a change, by a person really qualified to do the reviewing. Second, there will be no spread of opinions, instantly eliminating the problem that so pains Ernst *et al.* Third, the state of total nervous collapse in which programme directors and journal editors find themselves each time they try to get reviews back in time will simply vanish, because the authors will be jumping at the opportunity to review their own work. And fourth, peers everywhere will be freed from the demoralizing task of having to review works they do not understand but which they review anyway just to show that they actually understand them.

My review of my own proposal has been published in the *Journal of Irreproducible Results* (**37** (6), 12; 1992) and my review of my latest book has been published in *EOS* (**74**, 258; 1993). Anxious authors, journal editors, programme directors and peers are referred to these publications for guidance, solace and a glimpse at the freedom that the future promises.

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