All Correspondence this week arises from recent News and Editorial coverage of fraud and quality control.

Why negatives should be viewed as positives

SIR — Your News story "Journals submit to scrutiny of their peer-review process" (*Nature* 439, 252; 2006) reports findings of no bias against manuscripts presenting negative results. But the journals examined in this study are at the top of their field, and top journals are only likely to receive submissions reporting negative results if these are of clear 'positive' interest. This is certainly the case in ecology: Julia Koricheva (*Oikos* 102, 397–401; 2003) showed that non-significant results in ecology tend to be published in lower-impact journals.

This filtering of results undoubtedly biases the information available to scientists (see, for example "Null and void" *Nature* 422, 554–555; 2003). And communication is at the heart of science.

If non-significant results remain unpublished, we will be left with only half the picture. We encourage scientists to submit the negative results of their rigorous research to journals such as the *Journal of Negative Results — Ecology and Evolutionary Biology* (www.jnr-eeb.org), the *Journal of Negative Results in BioMedicine* (www.jnrbm.com), the *Journal of Negative Observations in Genetic Oncology* (www.path.jhu.edu/NOGO), and the *Journal of Negative Results in Speech and Audio Sciences* (journal.speech.cs.cmu.edu).

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A simple system of checks and balances to cut fraud

SIR — As your Editorial "Standards for papers on cloning" (*Nature* 439, 243; 2006) demonstrates, the fraudulent Hwang stem-cell research papers will have consequences for future research in this and related biomedical fields. As you point out, this does not justify imposing more rigorous standards for reviewing manuscripts in this field than others. Enforcing the deposition of samples with independent laboratories or repositories would be inappropriate, and deposition could also be done fraudulently. Although deposition might allow another layer of supervision, it would also create another layer of complexity and cost to stymie further research in a field already encumbered by restrictions.

As researchers in different parts of this field, we would like to make a joint response. We believe a simpler system of checks and balances could reduce incidents of scientific fraud and increase our confidence in published reports.

First, all co-authors should indicate the scope of their involvement and declare their understanding of the data in, for example, an author contribution statement such as that recommended by *Nature* (www.nature.com/nature/authors/gta). Surprisingly, it seems clear in retrospect that many of the 26 authors on Hwang's report (*Science* 308, 1777–1783; 2005), could not have attested to the veracity of the human nuclear-transfer embryonic stem cells (ntESC) presented. A requirement for personal accountability might have encouraged greater communication between authors and uncovered the deception before publication.

Second, all journals should, like *Nature* (www.nature.com/nature/authors/policy), require that all published reagents and cell lines be made available to other laboratories.

Finally, peer reviewers should be encouraged to demand that authors provide clear and strong evidence that the data presented support the claims made — including the request for mitochondrial DNA fingerprints if appropriate.

Of course, the best way to ensure integrity in any field is independent replication of results, which requires multiple investigators to be free to do the research. Current limits on US federal funding make independent verification of results especially challenging in the case of human ESC or ntESC research, and undoubtedly contributed to the difficulties in uncovering the misconduct of Hwang and his colleagues .

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Peer review could be improved by market forces

SIR — Although peer review seems the best system for quality control of scientific publications and grant proposals ("Three cheers for peers" *Nature* 439, 118; 2006), we might try to improve it. Market forces are known to optimize complex systems where multiple players have conflicting interests. Economic principles and internet technology could be applied to a peer-review system in the following way. First, a central digital repository receives a paper for a fee 'x'. Potential referees then bid to review the paper, and, if approved by the author, receive a fee 'y < x'. Payments are made at the end of each month, allowing for exchanges where an author pays by reviewing other papers.

Referees who can recommend an appropriate journal for the paper and provide the required reference are given due credit and might eventually raise their fee. Authors wanting additional improvements to their work might also pay a higher fee.

Soon, an active exchange could take off where referees quote their position in the peer-market as eagerly as authors quote their citation impact. This system could diminish the workload of referees, by reducing the need to review the same paper for different journals. Eventually this system might be run as an independent peer-review exchange for a profit.

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Bureaucracy won't change the character of a cheat

SIR — Your Editorial "Standards for papers on cloning" (*Nature* 439, 243; 2006) invites comment on current peer-review procedures and fraud detection. The use of deception for personal gain is neither new nor restricted to human beings. To my mind, the three relevant new things in contemporary society are intense media attention, piles of bureaucracy through which even the lowest-ranking staff have to wade and a culture dominated by lawsuits where redirecting blame seems increasingly prevalent.

During my undergraduate studies, the idea of reporting fictitious data never crossed my mind. And yet I witnessed friends regularly 'massaging' graphs, in spite of being taught the proper scientific technique. Is this not, therefore, a problem rooted in personal character? Would the introduction of yet more bureaucracy really solve the problem?

Science and fraud have coexisted for millennia, throughout which time progress was made without computers or armies of administrators. In the case of Hwang and, more recently, Jon Sudbø — who invented test subjects and published his results in *The Lancet* (*Nature* 439, 248–249; 2006) — the open scientific process of peer review, publication and further study revealed the falsehood, and the only people who should be held accountable are those committing the

deception. I believe the scientific method as it exists now is all we need as a community. Indeed, given the current power of the media, the quantity of academic fraud may well decrease, as potential fraudsters witness in full colour the disintegration of their dishonest colleagues.

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Pressure also leads to worthless publications

SIR — Your Editorial "Ethics and fraud" (*Nature* 439, 117–118; 2006) does not address the problem of 'publish or perish.'

Researchers are increasingly put under pressure to publish papers to further their career and access resources. But the fact that there are millions of pages published every month, only a few percent of which are worth reading, seems as much a fraud as the Hwang case. Are you wasting your time any more reading something fraudulent than reading something worthless? Neither helps the student or researcher wanting to do something concrete. It seems we have to read ten papers to get the one that really gives us something. The information is fragmented — distributed across hundreds of publications, around the world, many of them inaccessible.

I suggest slowing down the paper-publishing machine by limiting the number of journals that publish original research, asking more peer-reviewers to read preprints and opening up preprint manuscripts for public discussion.

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It's difficult to publish contradictory findings

SIR — Your recent Editorial (*Nature* 439, 117–118; 2006) bemoans the recurring subject of ethics and fraud in scientific research. I contend that many journals contribute to the prevalence of bad science, because, when the fundamental observation that led to the original publication cannot be reproduced, it is nearly impossible to publish a paper documenting this. Hence, controversies persist in the literature over many years, simply because the corrected story either is never published, or is not published as prominently as the initial paper.

True, there is an extensive specialist literature where ambiguous or conflicting results can be addressed in detail, but the readership is limited. Some journals, such as *Nature*, have mechanisms for publishing technical comments on published research (Brief Communications Arising: online only; see www.nature.com/nature/authors/gta/briefcomms.html#a2), but these are few in number and must adhere to strict criteria.

Reviewers of contradictory results often ask that the authors explain how the original authors could have obtained their results. To quote a recent rejection letter, "an adequate explanation for the apparent contradictory findings is not provided". Certainly, speculative explanations can be offered for some kinds of experimental differences. But it is never possible to prove how another lab obtained data that cannot be reproduced. One can only be certain of one's own data. This demand for explanation creates serious problems in the case of scientific fraud. In a minor case, the original authors may have fudged one small set of data to 'prove' their theory. In a more serious case, fundamental observations cannot be reproduced. Whether this irreproducibility is due to outright fraud, scientific incompetence or some combination cannot be determined by the authors who try to reproduce the result and fail.

Another often-made request of reviewers is that the original experiments be reproduced exactly. This sounds reasonable but, in fact, can become an absurd burden. Even if the methods section were complete and accurate, one can never say with certainty that one has reproduced the experimental conditions precisely. Instead, the appropriate approach is to design experiments to test the conclusions of the original paper. If these conclusions are disproved, then the details of how they were arrived at are not relevant.

Of course, a contradictory paper should be held to a higher standard than was the paper it refutes. But all journals must endeavour to correct errors, or those who perpetrate scientific misconduct (not necessarily outright fraud) will be rewarded, and those who try to correct wrong hypotheses in the proper hegelian manner — thesis, antithesis, synthesis — will be punished.

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Data audit would reduce unethical behaviour

SIR — I agree with the point made in your Special Report "Should journals police scientific fraud?" (*Nature* 439, 520–521; 2006) that editorial offices are not the proper place to monitor fraud. This is why, 19 years ago, my colleague Zoltan Aannau and I proposed data audit (*Nature* 327, 550; 1987).

Research subject to data audit could include studies presenting possible risks to public health, or those questioned by a whistleblower or by peer review. Others could be subject to random audits. Up to 1% of all studies could be audited every three to five years, at less than 1% of the cost of the original study (*Accountability Res.* 1, 77–83, 1989). Auditing could be done by an independent body that would certify the validity of published results. Sponsoring institutions could choose to publish a transparent analysis of selected papers on the web.

Although these processes might not eliminate all fraud or misconduct, they could substantially reduce such unethical practices.

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Discourse among referees and editors would help

SIR — In the discussion on enhancing peer review, following publication of the controversial human-cloning papers in *Science* ("Ethics and fraud" *Nature* 439, 117–118; 2006), I would like to highlight one of the limitations of this process. Last year, I was asked to review a manuscript for a high-impact journal. Although I duly reviewed the paper — before the deadline and after extensive reading and research — I have yet to receive information on the paper's status.

I see three possible scenarios regarding the paper's fate: either it has been accepted or it has been rejected or the authors have been advised to revise it. In the first and second cases, a referee likes to know how an editor made their decision in light of, or in spite of, any objections raised. In the third case, each referee likes to know what comments or recommendations other referees and the editor have made, as well as details of the authors' rebuttal. Obviously each referee is an expert in his or her field, but not necessarily in the other sub-fields relevant to a particular manuscript. Sharing the referees' comments is essential to the learning (and in some cases validation/checking) process. It might also help clarify differences of opinion between referees of the same expertise. I believe discourse among the referees and editor would enrich the quality of reviewing and might prevent scandals such as the human stem-cell line cloning debacle.

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