

Recognizing good work

SIR — Few people in research would argue with Maddox¹ about a lot of published research being impenetrable to readers. The question is what to do about it. I spend a lot of my time teaching scientists how to publish 'reader-friendly' papers; one of the tools I use is to get them to review well-written papers, such as Watson and Crick's classic², slices of Einstein's theory of relativity³ and so on.

The problem is that they know the work is good and they often adjust their opinions accordingly, so I recently did something different. I gave 24 scientists a copy of a paper by Pontecorvo⁴ and asked them to read it and to analyse it according to what they thought was good or poor. I gave them no other information about the paper or why I had chosen it, but they were given as much time as they wanted to come to a decision. (The main text is about a thousand words and most were done in 20 minutes.)

I then asked them to assemble along a line ranging from 'great' to 'rubbish' with the mid-point being the dividing line between when an editor should accept it or reject it. Only one person thought it should have been published, and most of the rest clustered near 'rubbish'. We had a good exchange of views from both sides but nobody elected to change camps.

Then I told them that I had chosen it because *Current Contents* had listed it as a citation classic (that is, a paper cited more than 400 times) and because it is a well written paper. They were surprised and confused, and found these facts difficult to accept.

My own interpretation (which I discussed with them at the time) is that their judgement was clouded by a mixture of inexperience and perfectionism. Perfectionism is an occupational hazard in science; scientists can easily get hooked into rejecting anything that has a demonstrable flaw (Pontecorvo writes well, but the perfect paper has yet to be written, so my group could all find plausible — to them — grounds for rejection.)

I think this shows a weakness in the way in which we educate researchers, which is not confined to Australia. Too often, students are left to use osmosis to learn how to publish, but this is turning out a proportion (most?) who cannot recognize good papers when they see them. If they cannot do that, what are they going to model their own papers on?

My suggestion is that academics and anyone else involved in the management of research should spend time with students and less experienced scientists to

analyse important papers in order to understand how the authors made their points, not just what the points were. This should be an integral part of any higher education and my concern is that too few do it.

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1. Maddox, J. *Nature* **305**, 477–478 (1983).
2. Watson, J. D. & Crick, F. H. *Nature* **171**, 737–738 (1953).
3. Einstein, A. *Relativity: the special and general theory*. (Authorised translation by Robert W. Lawson).
4. Pontecorvo, G. *Somatic Cell Genetics* **1**, 397–400 (1975).

Page charges

SIR — Leung and Robson (*Nature* **355**, 760; 1992) and Sharma (*Nature* **355**, 104; 1992) have referred to the problems faced by authors, particularly those in developing countries, when trying to meet page charges. I would like to draw your attention to a practice that essentially amounts to inertia selling of page charges.

In early 1991, *Physics Essays*, published by the University of Toronto Press, accepted an article of mine for publication. About six months later and shortly before the proofs arrived, I received an invoice for page charges based on a rate of \$55 per page.

Such charges were not mentioned in the instructions to authors nor anywhere else in the journal. Although I refused to pay, the article was eventually published. My understanding of English law is that I am not obliged to pay because the charges were not apparent when a contract was made, that is, acceptance for publication. Nevertheless, and despite my protests, I continue to receive regular demands for payment.

I would be interested to know whether any of your readers have had a similar experience.

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Orphan drugs

SIR — F. Allerberger and M. P. Dierich point out (*Nature* **357**, 531; 1992) that niclosamide is a new kind of orphan drug because it has become too cheap to justify marketing in Austria. Niclosamide has also been withdrawn from the market in the United Kingdom, where we have been using praziquantel instead. Praziquantel is an excellent taenicide

with only minor side effects and has the advantage of activity in cysticercosis, schistosomiasis and hydatid disease and is not very expensive. We are told that praziquantel is registered only for veterinary use in Austria — I wonder if the same applies to other veterinary drugs such as albendazole (useful for strongyloidiasis, gnathostomiasis and resistant enterobius) and ivermectin (drug of choice for onchocerciasis)? It is a fact of life that tropical diseases have often had to wait for decent chemotherapeutic agents to emerge from veterinary medicine although they afflict considerably more than 200,000 people.

Although it would be a shame if niclosamide were to be withdrawn from those who cannot afford praziquantel, perhaps our real concern should be not for orphan drugs but for those that are never even conceived through lack of commercial interest.

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Political views

SIR — Paul Cumming (*Nature* **357**, 432; 1992) expresses his dismay on reading a political commentary in *Nature* (**356**, 92; 1992) about the possible establishment of a free trade agreement between Canada and Mexico. We agree that a scientific journal should not be a forum for "unsustainable assertions which seem to gain credibility through repetition".

However, in presenting these arguments, Cumming contradicts himself. First, he makes a political statement about the inconvenience of this agreement between these countries. Second, he makes an unsustainable assertion that Mexico is governed by a dictator — making a political statement about his views on the legitimacy of the Mexican government.

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