

Objectivity in science

SIR—Despite some unfortunate, inaccurate and even outrageous remarks about philosophers of sciences the community of science is greatly indebted to Theocharis and Psimopoulos (*Nature* 329, 595; 1987). They have raised questions that call for urgent and honest answers. Not least, they have brought into the open the problems of objectivity and truth in science and, perhaps, reminded us of what we are in danger of losing. But some cautionary notes need to be sounded.

In suggesting that the practice of science today is affected (adversely) by the prevailing whims and fashions of 'philosophy', are they not in mortal danger of selling the pass to their enemies? Is this not precisely the point of the cultural relativists whom they oppose? According to these "betrayers of the truth", science is always conditioned by cultural values and it cannot be said to deal with unchanging objective realities. The present difficulties vexing the Royal Society, for instance, are a standing demonstration of the critical dependence of science on socially acceptable values. Yet this is to fudge the distinction between public (including government) attitudes to science on the one hand and the attitudes of the scientists on the other. It by no means follows that the one is reproduced in the other. The opposite may well be true.

If that is a distinction between the piper and he who calls the tune, there is another to be drawn between music-maker and critic. Few would deny the latter the right to exist, but it is to be hoped he or she is not tone-deaf. When criticism of science is concerned, however, tone-deafness (or its equivalent) is all too prevalent. The public understanding of science is often formed by those with no experience of doing science, and great harm is done by their guileless and simplistic pronouncements.

A further distinction may further clarify the issues. It is that between scientific method and scientific intention. One reason why scientists are so embarrassingly coy about their methods is that these vary widely over the whole field of science, as witness the different approaches to (say) evolutionary theory and antibiotic therapy. (Another reason is simply that the methods have become second nature and are rarely defined in words.) But this is not to imply a diversity of intention, for there always is a determination to wrest secrets from nature, whether or not this leads to 'practical' applications. Indeed, it could be argued that it is this very uniformity of intention that binds the scientific community together.

It is remarkable that Theocharis and Psimopoulos make little mention of the history of science (apart from a passing reference to an unspecified golden age).

For it is studies in this area that throw into sharp relief yet another distinction, namely that between *actual* and *public* motivations. Over the past 20 years or so, an immense amount of painstaking scholarship has been devoted to private papers, diaries and other manuscript sources, and has produced overwhelming evidence that the practice of science is very culturally dependent. Yet even that overlooks a further distinction which is probably more important than any of the others. It is the difference between approaches to science, which may be conditioned by our culture, and the empirical results determined by nature.

Whether the buzz-words 'objectivity', 'truth' or 'value-transcendence' are actually used or not, common observation suggests that few scientists doubt that there is something 'out there' to be examined, whatever their own circumstances may be. That pragmatic belief today is successor to a deeply held Christian conviction about the created world that attended the rise of modern science. Donald MacKay has argued that "the ideal of objectivity . . . is the main spring behind the success of science, and finds its status reinforced by that success"¹. Such a view is entirely compatible with historical research about the ways in which scientists formulate programmes, examine and select data and interpret their results. It is not that we have to choose one or the other: culturally conditioned methodology or an objective world outside. We need both. Only then can we appreciate the essential humanness of science, but only then can we find any hope of escape from the miasmatic swamps of sociological relativism from which the image of science so urgently needs to be rescued. To set the record straight is a noble agenda for history of science for the rest of this century.

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1. MacKay, D. *Objective Knowledge* (ed. Helm, P.) 43–57 (Inter-Varsity, Leicester, 1987).

• This correspondence is now closed. — Editor, *Nature*.

Editorial policy

SIR—In his article on evidence for an influx of small comets into the Solar System, Paul Feldman (*Nature* 330, 518; 1987) also comments on the editorial policy of *Geophysical Research Letters*. He asks whether the papers of Frank *et al.*¹, which inspired the *Nature* article² whose findings Feldman discusses, should have been published at all. He also disagrees with the policy of *Geophysical*

Research Letters that gave Frank and his co-authors the opportunity to respond to each criticism that their papers drew. I am pleased to outline the editorial policy that governed the publication of the original papers by Frank *et al.* and of the ensuing comments and replies.

Geophysical Research Letters seeks to publish papers that describe interesting, forefront science. These papers occasionally challenge conventional wisdom and thereby engender controversy. Written at the turbulent interface between ignorance and knowledge, such papers are frequently wrong. However, not all controversial ideas are wrong. The importance for scientific progress of the occasional new idea that proves correct is out of all proportion to the number of such ideas. Because it is not possible to tell in advance which new idea is correct, it is best to get them into the open literature where they can be discussed, attacked, tested or supported as the will of the community and the soundness of the idea dictate. This is the way science has advanced, and no better way has been demonstrated.

Exposure of conflict is a proper function for a letters journal that publishes new and interesting research results. Comments and replies are an important part of *Geophysical Research Letters*. They provide a forum for discussion and testing of new ideas. Rather than restricting such debate to private exchanges between authors and referees, the publication of comments and replies allows the broad scientific community to hear both sides of an issue and form an independent judgment.

Finally, I would claim that the publication by *Nature* of the paper by Donahue *et al.* is, in itself, a vindication of the original decision to publish the papers of Frank *et al.* Their papers and the ensuing debate will, no doubt, inspire other useful work. And, although Feldman is perhaps annoyed by the protestations in the replies of Frank *et al.* that their critics have not shown conclusively that the claimed comets do not exist (in the sense that claims are made that the non-existence of the Loch Ness monster has not been conclusively proved), it seems clear from his statements that Feldman himself has not been persuaded. I don't believe many others have been either.

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1. Frank, L.A. *et al.* *Geophys. Res. Lett.* 13, 303–306 (1986); *Geophys. Res. Lett.* 13, 307–310 (1986).

2. Donahue, T.M. *et al.* *Nature* 330, 548–550 (1987).

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