

Publicity on controversial data

SIR — In the light of the enormous publicity that followed the publication by Jacques Benveniste in *Nature* in June 1988 (ref.1), it is highly remarkable that a letter which appeared in *Nature* on 23 November 1989 (ref.2) remained apparently almost unnoticed. In this letter, the author describes results of a test by an unidentified “eminent homoeopathic practitioner, a former President of the Faculty of Homoeopathy” to select from 20 bottles containing either “natrium muriaticum 30°C” or “sulphur 30°C”. The investigator was told that the contents of the bottles had “strikingly different properties” and “were strongly active”. The bottles were blinded. The investigator was allowed to use any method for identifying any “distinction”.

The outcome of the test was as might be expected: no distinctions were found, the results obtained “might just as well have been arrived at by chance alone”.

Not only does the letter of 1989 not mention the name of the person who carried out the homoeopathic test, it does not give any information either on the nature of the tests which have been carried out, which seems to be unique for *Nature*. After some enquiries, I was informed about the actual test: the so-

called emanometer had been applied. In a very diffuse way, this ‘instrument’ has been described by Ritchie McCrae and F.F. Hom³. In this paper the impression is given that with the emanometer the efficacy of homoeopathic products can be proved. The outcome of the study in 1989 is absolutely negative, but the author of the letter is still not convinced of the inactivity of the 30°C dilutions. He suggests that a clinical trial should be carried out: the circle is round indeed.

A few years after the ‘Benveniste story’, Benveniste himself announced: “. . . I will (try to) publish in the months to come indisputable proof”⁴. So far I have not seen such a paper. Scientific journals should stop publishing papers that deal with obscure techniques or nonsense theories, as it is very likely that they will be misused⁵.

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1. Davenas et al. *Nature* **333**, 816–818 (1988).
2. Roberts, T.O.M. *Nature* **342**, 350 (1989).
3. McCrae R. & Hom, F.F. *B.J. Homoeopath.* **50**, 143–164 (1961).
4. Benveniste, J. *The Lancet* **ii**, 944 (1990).
5. Kleynen, J. Knipschild, P. Ter Riet, G. *Br.med.J.* **302**, 316–323 (1991).

Lesson for science

SIR — With the publication of the draft report by the NIH Office of Scientific Integrity¹, the most important lessons for science in the Baltimore controversy threaten to become lost in the ensuing squabbling. These lessons are twofold.

The first is that, protestations to the contrary notwithstanding, the scientific endeavour tends to resemble Kuhn’s description of science as a triumph of expectations over reality². Initially conceived test-hypotheses solidify as dogmatically held paradigms, impervious to refutation. In this instance, David Baltimore states his reliance on the authority of his co-author (“Imanishi-Kari provided the expertise in serology that I lacked”³) over Margot O’Toole’s experiments indicating non-replicability of a co-author’s conclusion. If the most renowned of the co-authors cannot understand the basis on which his own paper’s conclusions rest, what must that imply for those of us who are not Nobel laureates? So much for the vaunted “rigours of the scientific peer review process” to uncover error. Or is O’Toole not a peer?

Second, and more important, is the threat to the vigour of scientific debate. One of our leading scientific institutions, whose adherence to experimental inquiry led Oswald Avery and colleagues to the trail of DNA as the inheritance

factor at the then Rockefeller Institute, a decade before the discoveries of Watson and Crick^{4,5}, may now be headed into an era based on hypothesis as unrefutable dogma.

Should it not now be our concern to focus on the central problems this controversy raises? Why did not O’Toole’s efforts find a publication outlet? Why could not a 1987 paper by Walter Stewart and Ned Feder outlining discrepancies in the original *Cell* paper also find a scientific journal willing to publish their doubts⁶ [see footnote]? As James Lovelock observed in these pages⁷, the evolution of scientists into dogmatic authoritarians poses the danger of an acceptance of a censorship of ideas. As this case reveals, the danger is also to the integrity of the process of scientific inquiry itself, the censorship of validity checking. That should be our central concern in this matter.

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1. *Nature* **350**, 262 (1991).
2. Kuhn, T. S. *The Structure of Scientific Revolutions* (University of Chicago Press, Chicago, 1970) 36–45.
3. Baltimore, D. *Nature* **351**, 94 (1991).
4. McCarty, M. *The Transforming Principle: Discovering that Genes are made of DNA* (W.W. Norton, New York, 1985).
5. Crick, F. *What Mad Pursuit: A Personal View of Scientific Discovery* (Basic Books, New York, 1988) 36–37.
6. Kuznik, F. *The Washington Post Magazine*, 14 April 1991, p.31.
7. Lovelock, J.E. *Nature* **348**, 685 (1990).

■ Since published in *Nature* **351**, 687–691 (1991).

New words

SIR — Friedrich Katscher complains (*Nature* **351**, 179; 1991), and quite rightly too, that scientists who do not grasp the intricacies of Greek and Latin grammar should be more hesitant about creating modern words in which the mysteries of declension render the scientific meaning incoherent to anyone unfortunate enough to be conversant with these two languages.

A ‘mutant’ (properly signifying ‘that which is in the process of mutating’) is clearly a solecism, and if Katscher thinks ‘mutatus’ too ugly, may I suggest a ‘mutate’ in the hope that scientists will still be able to tell the difference between the word’s use as a noun and a verb?

And can anyone enlighten me on how physicists came to imagine that ‘accelerate’ denotes ‘any change in velocity or direction’, when even a passing acquaintance with Latin (or English) would seem to indicate otherwise?

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SIR — Friedrich Katscher’s suggested term ‘recombined’ is undesirable as an alternative to ‘recombinant’. Participial adjectives are formed directly from participles, and the suffix *-ated* is not regularly present where the verbal stem does not itself contain *-at-*. The form ‘recombined’ has apparently been generated as a back-formation from ‘recombination’, but it is in effect a formal derivative of a non-existent verb **recombine*. (Compare, for example, the regular noun ‘computation’ and the unacceptably irregular **computed*).

Although there is no objection to the regular form ‘recombined’, there is another perfectly possible adjective of past participial form; *recombinate*. This represents the common English development from the Latin suffix *-atus*, and has the added advantage of readily forming a corresponding noun, also spelt *recombine*.

The analogous adjective and noun ‘mutate’ (stress on the first syllable) is used in certain restricted contexts, but is awkwardly confusable with the verb; however, Latinate forms such as ‘mutatus’ are not easily assimilated into natural English use, and are perhaps best avoided. I think ‘mutant’ is philologically defensible: there are precedents for words ending in *-ant* which have lost a present participial sense, though they are few (*convenant*, *descendant*, *immigrant*, and *quadrant*, for example).

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