

## Planning of US AIDS research

SIR — Your News report headed “AIDS office opposes outside planning” (*Nature* **381**, 5; 1996) draws a misleading interpretation from our comments to Congressman John Porter during testimony before the US House of Representatives appropriations subcommittee on health, education and labour.

Far from opposing the role of the outside scientific community in planning AIDS research activities, the National Institutes of Health (NIH) and the Office of AIDS Research (OAR) in particular have strongly encouraged such advice. The statement that “the direction of AIDS research should remain firmly in the hands of institute directors and their advisers” is a misinterpretation of the view we expressed, which was that peer review should remain a process devoted to the determination of the scientific merits of a grant proposal and separate from the planning process. Indeed, the inclusion of nongovernment experts from academic institutions, industry and the AIDS community in the planning and evaluation processes has been a hallmark of the OAR.

The NIH is currently preparing an implementation plan based on the recommendations from the National AIDS Research Program Evaluation Working Group led by Arnold Levine.

**Harold Varmus**

(Director)

**William E. Paul**

(Director, Office of AIDS Research)

National Institutes of Health,

Bethesda, Maryland 20892, USA

## IQ and censorship

SIR — We welcome the fact that Christopher Brand's book *The g Factor: General Intelligence and its Applications*, has been reviewed in *Nature*<sup>1</sup>, and we regret its withdrawal by the publishers, John Wiley & Sons. As departmental colleagues of the author we should like to make the following points.

(1) Without commenting on the content of this book, we feel that it deserved publication through the normal channels, not suppression. In an eloquent argument which could apply equally to Brand's book, James Flynn<sup>2</sup> made the case that J. P. Rushton's views — with which Flynn disagreed absolutely — deserved publication and then attempted scientific refutation rather than suppression. Flynn argued that “the truth can never be racist, nor can telling the truth as you see it, assuming

there is no evidence of wilful neglect of evidence”. In summary, Wiley has threatened academic freedom.

(2) The sensational treatment of this affair in the non-scientific press threatens to taint sensible research on intellectual abilities, as Brand may in future be used as a convenient bogeyman to discredit work in a broad and important area. (As a parallel, Sir Cyril Burt's name is used to give a foul smell to research quite unconnected with his own; for instance, by Kitcher in his assessment of ‘pop’ and human socio-biology<sup>3</sup>.)

(3) The data presented in Brand's book may usefully be read alongside the authoritative views recently endorsed unanimously by an American Psychological Association (APA) task force<sup>4</sup>. From Brand's book, your reviewer, N. J. Mackintosh, singles out two details for comment: the *broad* heritability of IQ, and the correlation between IQ and “inspection time” (IT). In the first case, the APA consensus estimate of adult *broad* heritability agrees with Brand rather than Mackintosh. On the second, less controversial issue, the APA experts are with Mackintosh rather than Brand. As this latter disagreement is not about observed values, but rather about how to extrapolate from these to the ‘true’ IQ–IT correlation in the ‘normal population’, this example serves to make the point that determination of truth requires discussion. Withdrawal of the book will not advance our understanding of the truth in the cases where Brand's interpretation of evidence can be disputed.

**P. G. Caryl**

**I. J. Deary**

Department of Psychology,

University of Edinburgh,

7 George Square,

Edinburgh EH8 9JZ, UK

1. Mackintosh, N. J. *Nature* **381**, 33 (1996).

2. Flynn, J. R. *Psychologist* **2**, 363–366 (1989).

3. Kitcher, P. *Vaulting Ambition. Sociobiology and the Quest for Human Nature* (MIT Press, Cambridge, Massachusetts, 1985).

4. Neisser, U. et al. *Am. Psychologist* **51**, 77–101 (1996).

## Global surface temperatures

SIR — Gordon et al.<sup>1</sup> comment, somewhat misleadingly, on the press release issued by the Met Office (jointly for the University of East Anglia) on 1995 global temperatures. We should like to make the following comments.

The December 1995 global surface temperature anomaly was a combination of sea surface temperatures (two-thirds of the globe) and land (near-surface air) temperature, the latter calculated using a strong relationship<sup>2</sup> with (final, not provisional) 500-hPa geopotential fields. This is

rather more than an “educated guess”. The final estimate of the global annual anomaly remained the same as that in the press release. We pointed out that “differences of a few hundredths of a degree between global average temperatures in individual years are not significant”.

The temperature anomaly for the globe was not “determined largely by that for the Northern Hemisphere”. The global mean is a straightforward average, with cosine (latitude) weighting, of anomalies for each 5° × 5° square where sufficient data exist. If, as is sometimes the case, there are more data-sparse squares in the Southern Hemisphere (SH), then a slight Northern Hemisphere (NH) bias can occur. In fact, in 1995, the NH temperature anomaly was 0.55 °C and the SH anomaly was 0.23 °C, giving a global anomaly of 0.39 °C, not significantly different from the 0.40 °C in the press release. We said in the press release that the SH was “relatively less warm”; nevertheless, our record shows it to be (along with 1990, 1991 and 1993) the third warmest since 1860.

The press release clearly referred to the “surface temperature of the earth”, that is, the region that we inhabit. The MSU satellite instrument averages temperatures over a deep layer from the surface through most of the troposphere; it need not agree closely with surface temperature in its decadal trends or shorter-term fluctuations. The relationship between the two is discussed extensively in the forthcoming 1995 report of the Intergovernmental Panel on Climate Change<sup>3</sup> and is the subject of continuing work at the Hadley Centre, the University of Alabama<sup>4,5</sup>, the National Aeronautics and Space Administration<sup>6</sup> and elsewhere.

**David E. Parker**

Hadley Centre for Climate  
Prediction and Research,  
Meteorological Office,  
Bracknell, RG12 2SY, UK

**Philip D. Jones**

Climatic Research Unit,  
University of East Anglia,  
Norwich, NR4 7TJ, UK

1. Gordon, A. H., Bye, A. T. & Byron-Scott, R. A. D. *Nature* **380**, 478 (1996).

2. Wallace, J. M., Zhang, Y. & Bajuk, L. J. *Climate* **9**, 249–259 (1996).

3. IPCC Working Group 1. *Climate Change 1995 — The Science of Climate Change*. Second Assessment Report of the Intergovernmental Panel on Climate Change (eds Houghton, J. T. et al.) (Cambridge Univ. Press, in the press).

4. Christy, J. R. & Spencer, R. W. *Clim. Change* **30**, 97–102 (1995).

5. Christy, J. R. *Clim. Change* **31**, 455–474 (1995).

6. Hansen, J. et al. *Clim. Change* **30**, 103–117 (1995).

### corres@nature.com

Letters submitted for Correspondence should be typed, double-spaced, on one side of the paper only, or e-mailed (with full mailing address) to [corres@nature.com](mailto:corres@nature.com)