

to occur in a sub-population has a long history in medicine and we do not feel it should necessarily be dismissed as ageist or sexist.

GODDARD, E. (1991) *Drinking in England and Wales in the late 1980s*. London: HMSO/OPCS.

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Sir: We agree with Vanstraelen and Duffett that substance use occurs at different levels in different population subgroups. We also agree that it is clinically important to recognise such differences. However, in emergency clinic psychiatry we think that to let information about populations lead us into assumptions about individuals is clinically dangerous. One cannot exclude a diagnosis just because it is unlikely.

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"Peering into review"

Sir: Peer reviewed publication is the *sine qua non* of academe. Those of us wearied by incessant demands to justify our existence to funding quangos should welcome Dr Persaud's (*Psychiatric Bulletin*, September 1995, 19, 529-531) informative article on the pitfalls of peer review and its adverse consequences. Having been at the receiving end of reviews which could be described euphemistically as impolite, I blame reviewer anonymity. Rejected authors play 'spot the reviewer': first, round up the usual suspects. Friends may have spied the paper on the suspect's desk; intimate knowledge of suspect's word processors is also useful, as is approaching

them at meetings and observing their behaviour. Scrape at any 'tippex' on the report, sometimes they forget to leave their name off.

Either we should know who they are, or they should not know who we are. Open reviews may be less inclined to insults and destructiveness. If both sides were anonymous it could discourage bias on the grounds of who the author is, or who they are not.

Dr Persaud suggests the review should be an encouraging tutorial despite poor quality material. The focus of my comments as a reviewer is what is needed for publication (most researchers send their efforts to colleagues for informal advice prior to submission). It is infuriating to perform a significant re-write only to have the Editor reject the paper a second, final time: such practices cause interminable delay. I suggest that the convention of submitting to one journal at a time be abandoned. This would dispense with most of the waiting, generate a large variety of peer comments and even give the author a choice of journal.

Finally, I propose a radical solution to the peer review problem. Why not dispense with it and publish on the Internet? My husband, a network specialist, remarks that the current Internet "is the biggest waste of time ever invented by man". However, worldwide web pages and usenet groups have vast capacity. Research, reviews and teaching material could be accessed much more easily than hard copy. The Internet costs less than subscribing to journals, purchasers could invest in it alongside ordinary library facilities.

The funding quangos would have to assess academics more imaginatively: peer reviewed papers are a tiny part of what I, and I suspect many other academics, are here for. Best of all, there is no censorship on the Internet, which is what suppression of material by a disapproving handful of peers amounts to. We deserve to be allowed to make up our own minds on the merits of new research.

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