

Correspondence

EDITED BY MATTHEW HOTOPF

Contents ■ Declarations of interest ■ Comparative cannabis use data ■ Prognosis of depression and generalised anxiety in primary care ■ The stigma of suicide ■ Who is politicising psychiatry in China? ■ Lest we forget, again ■ An alternative to interruption of treatment in recurrent clozapine-induced severe neutropenia

Declarations of interest

In a recent editorial, Thompson (2001) argues strongly against the findings of a systematic review in the same issue of the *Journal* (Barbui & Hotopf, 2001). The *Journal* requires authors of original papers, but not of editorials, to declare financial interests. In this case your editorial writer did not record his consultancies (past or present) to companies that manufacture selective serotonin reuptake inhibitors. Neither did he indicate that the recent study he cited to support his case (Thompson *et al*, 2000) was, in fact, carried out by a pharmaceutical company. The company (Eli Lilly) manufactures one of the antidepressants, the benefits of which are questioned by Barbui & Hotopf's systematic review.

The *Journal* began to publish Declarations of Interest in 1999, but only for original papers not editorials. We urge extension of the Declarations to include editorials. We would support more stringent criteria for editorials and similarly for reviews than for original papers – in line with the *New England Journal of Medicine* which for 10 years has “had a policy that prohibits editorialists and authors of review articles from having any financial connection with a company that benefits from a drug or device discussed in the editorial or review article” (Angell & Kassirer, 1996).

Angell, M. & Kassirer, J. P. (1996) Editorials and conflicts of interest. *New England Journal of Medicine*, **335**, 1055–1056.

Barbui, C. & Hotopf, M. (2001) Amitriptyline v. the rest: still the leading antidepressant after 40 years of randomised controlled trials. *British Journal of Psychiatry*, **178**, 129–144.

Thompson, C. (2001) Amitriptyline: still efficacious, but at what cost? *British Journal of Psychiatry*, **178**, 99–100.

—, **Peveler, R. C., Stephenson, D., et al (2000)** Compliance with antidepressant medication in the treatment of major depressive disorder in primary care: a randomised comparison of fluoxetine and a tricyclic antidepressant. *American Journal of Psychiatry*, **157**, 338–343.

D. Owens, A. House Academic Unit of Psychiatry & Behavioural Sciences, University of Leeds School of Medicine, 15 Hyde Terrace, Leeds LS2 9LT, UK

Author's reply: I am pleased to be able to respond to the letter from Drs Owens & House with which I am in partial agreement. I am happy to confirm that the cited study (Thompson *et al*, 2000) was designed by me, carried out by Eli Lilly in a UK primary care context, and was analysed and written up by my colleagues and I. These facts are acknowledged in the primary research publication in the *American Journal of Psychiatry*.

I am also happy to confirm that, along with occasional paid lectures, I currently hold a consultancy with Organon UK and have recently held a similar consultancy with Janssen and with Philips, but not with Eli Lilly. Readers of the *Journal* might also wish to know that these interests have been declared to the Royal College of Psychiatrists and are known to my employing University.

I completely agree that conflicts of interest should be declared. However, I wonder whether the editors of the *New England Journal of Medicine* would include in their censorship policy the authors of editorials in which the objective is solely to comment on the methodological adequacy of an original article. If so, that appears to me to be more a prohibition on freedom of speech than anything related to evidence-based medicine, and would have precluded my criticisms of the Barbui & Hotopf article. If that is the objective of Drs Owens & House, then I cannot agree with them on that point.

Finally, I have not been able to find any scientific points in Owens & House's letter to which I can respond and I therefore assume that they are in full agreement with my criticisms. Otherwise, I am sure that they would have presented rational arguments

against my analysis instead of taking their argument *ad hominem*.

C. Thompson Department of Mental Health, 1st Floor Department of Psychiatry, University of Southampton, Royal South Hants Hospital, Brintons Terrace, Southampton SO14 0YG, UK

Editor's response: In response to concerns brought to my attention in recent months, including those presented by Drs Owens and House, the Editorial Board now requires that authors of editorials and items of correspondence submit a Declaration of Interest, as authors of papers have been required to do for some time now. Similarly, assessors participating in peer review will in future be instructed not to assess material in which they have an interest. This change in policy is reflected in the Instructions for Authors, published in the July issue of the *Journal*, available on-line at <http://bjprcpsych.org/misc/ifora.shtml> and available upon request from the publishers. Declarations of Interest for papers and editorials will be published as a matter of course. In the interests of space, Declarations of Interest for items of correspondence will be published at the Correspondence Editor's discretion.

I have acted as proxy Correspondence Editor in the acceptance of the above letter and its response, in view of Dr Hotopf's interest in the matter.

G. Wilkinson Editor, *British Journal of Psychiatry*, 17 Belgrave Square, London SW1X 8PG, UK

Comparative cannabis use data

MacCoun & Reuter (2001) examine alternative legal regimes for controlling cannabis availability and use. They claim that the Dutch experience (the coffee shop system with decriminalisation of purchase, followed by “commercial promotion”) significantly increases cannabis use prevalence. They conclude, however, that primary harm comes more from criminalisation than from decriminalisation. They base their conclusions on the comparison of cannabis use data from The Netherlands and from other countries. Rightly, they warn that “meaningful cross-sectional comparisons of drug use should be matched for survey year, measure of prevalence . . . and age groups covered in the estimate”. They forget that