

## 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.

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**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*.

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**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.

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### 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

the comparisons should also be matched for type of geographical area. Comparing Flatbush to New York City would have limited relevance, even if correctly matched for age group etc. To compare cannabis use in New York City with somewhere else, one would have to look for a similar area, both in address density and in variation of population and lifestyle. Amsterdam could be compared to San Francisco, because these cities are very similar in size and cultural characteristics, but not to New York City, a metropolis over 10 times as large, or to the USA as a whole. Such comparisons are wrong and without meaning.

We agree with MacCoun & Reuter that decriminalising cannabis merits serious consideration. But we disagree with their observations on “commercialisation”. In this letter we will turn most of our attention to the epidemiological material the authors base their conclusions on.

MacCoun & Reuter focus entirely on cannabis prevalence (assuming that a lower prevalence is better than a higher one) without considering whether this is the most relevant issue; the social and legal consequences of the use of cannabis could be considered at least as important. But given that a comparison of prevalence figures is a useful first step towards informed comparisons, we propose that the conclusion of MacCoun & Reuter that the commercial type of Dutch coffee shop system increases cannabis prevalence is based on statistically ill-founded comparisons of Dutch prevalence figures with those in other Western nations.

MacCoun & Reuter compare cannabis prevalence figures of a Dutch city or nationwide with prevalence figures from the USA or other Western nations. Differences are summed and averaged, resulting in (among others) a mean Dutch–US difference and a mean Dutch–European difference. This is statistically erroneous for reasons we supply below.

First, in 16 cases a Dutch city is compared with a nation (UK, USA, Sweden, etc). By doing this, MacCoun & Reuter presuppose that prevalence rates are the same all over The Netherlands. This is incorrect: in our 1997 national survey we found large geographical differences between locations with different address densities, a measure of urbanisation. For example, lifetime prevalence of cannabis use in Amsterdam (address density >3000/km<sup>2</sup>) was 36.7%, the average national prevalence was 15.6% and average prevalence in rural

areas (address density <500/km<sup>2</sup>) 10.5%. Correct international comparisons can be made, but have to be between comparable geographical or urban areas. Despite the sensitivity MacCoun & Reuter demand for correct comparisons, nationwide US figures (260 million inhabitants, including major metropolitan areas) are compared with the small Dutch city of Tilburg (165 000 inhabitants).

Second, comparisons are arbitrarily selected. For example, replacing prevalence figures for Amsterdam (the city most often chosen in MacCoun & Reuter’s comparisons) with figures for Rotterdam changes the outcomes of the average difference in cannabis prevalence between the Dutch and other systems.

Third, MacCoun & Reuter state that the lifetime prevalence of cannabis in The Netherlands has increased consistently and sharply in the age group 18–20, stating: “the increases . . . provide the strongest evidence that the Dutch regime might have increased cannabis use among the young”. This finding is based on school survey data (lifetime cannabis use in 1984: 15%, in 1996: 44%). Again, the choice of figures that are compared is crucial. Moreover, the Dutch school survey data of the age group 18–20 is an extremely biased selection of this age cohort. The school survey takes place in some primary schools, but mostly in secondary educational institutions, that are designed for 12- to 18-year-olds. However, some persons remain much longer in this system for a variety of reasons but they are atypical for the age group in general. They bias the school survey estimate for this age group.

More suitable figures are given by Statistics Netherlands (CBS) and by the Centre for Drug Research (CEDRO), and reflect a much more moderate increase or no increase at all. Statistics Netherlands measures cannabis use prevalence in a national representative sample. For the age group 18–20 lifetime cannabis use remains at the same level over time (17% in 1989, 19% in 1990, 18% in 1991, 20% in 1992 and 14% in 1993; data from D. J. B.). Using CEDRO data, we are able to produce trend data for the city of Amsterdam for the same age group 18–20: in 1987 lifetime cannabis use was 34%, rising to 44% in 1997. This is a rather modest increase in cannabis use, very similar to the slowly rising consumption levels of other European and US measurements. The 18- to 20-year-olds in the samples from Amsterdam

are randomly selected from the citizen registry, and represent the age group much better than 18- to 20-year-olds still attending school. The ‘dramatic’ increase that MacCoun & Reuter hypothesise in Dutch cannabis use in the period 1984–1996 (as reflected in the same age group) does not exist.

Finally, the most serious flaw develops by creating a series of ‘absolute’ differences between Dutch and other data, and averaging them. MacCoun & Reuter create the suggestion that too large or too small differences will be averaged and thereby, in the form of an ‘average’ difference, become more reliable. The opposite is true. If pears can not be compared to apples, their ‘differences’ can not be used for normal mathematical computations.

#### Declaration of interest

None. The views expressed by D. J. B. are the author’s own and do not necessarily reflect the policies of CBS.

**Abraham, M. D., Cohen, P. D. A., van Til, R. J., et al (1999)** *Licit and Illicit Drug Use in The Netherlands*. Amsterdam: Centre for Drug Research (CEDRO).

**MacCoun, R. & Reuter, P. (2001)** Evaluating alternative cannabis regimes. *British Journal of Psychiatry*, **178**, 123–128.

**Substance Abuse and Mental Health Services Administration (1997)** *National Household Survey on Drug Abuse: Population Estimates 1997*. Rockville, MD: US Department of Health and Human Services.

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**Authors’ reply:** We thank Abraham *et al* for their comments, but they have misrepresented our paper, and we find their arguments either misleading or unconvincing.

Abraham *et al* complain that 16 of our 28 statistical comparisons contrast a Dutch city with a national estimate from the USA or another nation, suggesting that we “presuppose that prevalence rates are the same all over The Netherlands”. We made no such presupposition. As we clearly stated in our article: “American surveys indicate little difference, on average, between large metropolitan samples and the USA as a whole . . . but the estimates in Table 1 suggest that Amsterdam has a higher fraction of