Correspondence

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Antidepressant-related deaths

Cheeta *et al* (2004) present data on antidepressant-related deaths in England and Wales, 1998–2000. They report information about antidepressant deaths collected by the National Programme of Substance Abuse Deaths (np-SAD database). The aims of their paper were: (a) to investigate relative toxicities of major classes of antidepressant (compare accidental/intentional deaths); and (b) to analyse deaths where other drugs were also present at post-mortem examination. We are concerned that the data-set used for this analysis is unable to address these objectives.

The Office for National Statistics (ONS) database of drug-poisoning deaths holds information about all such deaths since 1993. This includes textual information from the coroners' reports about the types of substances taken. We have recently conducted an analysis of antidepressant-related poisoning deaths for a 10-year period from 1993 to 2002 (Morgan *et al*, 2004).

Between 1998 and 2000, Cheeta *et al* reported 468 deaths involving antidepressants recorded in the np-SAD database. Our analysis of the ONS database found 1452 deaths involving antidepressants for the same period. Clearly a large number of antidepressant-related deaths are missing from the np-SAD database. This is probably because about 80% of antidepressant-related deaths are due to suicide and not substance misuse (the data collected by np-SAD). Furthermore, Cheeta *et al* found that 93% of deaths involving selective serotonin reuptake inhibitors (SSRIs) also involved other drugs. In our study, this figure was 75%, suggesting that the np-SAD database is less likely to contain deaths involving SSRIs taken alone. This is likely to introduce bias into Cheeta *et al*'s study, leading to an underestimation of the relative toxicity of SSRIs compared with tricyclic antidepressants (TCAs). This is borne out when death rates per million prescriptions are compared between the np-SAD study and our study. The np-SAD study gives the overall death rate per million prescriptions for SSRIs as 1.4. In our study this was 5.1. Our study showed TCA rates to be around 8 times greater than rates for SSRIs; in the np-SAD study TCA rates were 10 times greater.

Cheeta, S., Schifano, F., Oyefeso, A., *et al* (2004) Antidepressant-related deaths and antidepressant prescriptions in England and Wales, 1998–2000. *British Journal of Psychiatry*, **184**, 41–47.

Morgan, O., Griffiths, C., Baker, A., *et al* (2004) Fatal toxicity of antidepressants in England and Wales, 1993–2002. *Health Statistics Quarterly*, no. 23 (autumn).

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Authors' reply: We are very pleased to hear of the interest of Griffiths and Morgan in our paper on antidepressant toxicity using the np-SAD database. It is intriguing that even though our paper was published earlier this year and prior to the Office for National Statistics report (Morgan *et al*, 2004), there is no mention in their letter of the similarities between the two studies. For example, the proportion of deaths from TCAs (amitriptyline and dothiepin being the most frequently implicated compounds) were very similar (85% and 89%), and both studies found that approximately 80% of deaths from antidepressants were suicides. Furthermore, one of the main implications of our study for clinical practice (and a result not previously reported in the UK) was the risk of SSRI-related fatality when these drugs are ingested in combination with TCAs, with or without other illicit drugs. Griffiths and Morgan appear erroneously to assume that np-SAD only collects information on illicit drugs. Rather to the contrary, the programme protocol, which is published bi-annually with surveillance reports (Ghodse *et al*, 2003), defines a case as one where psychoactive substances are directly implicated in the fatality, and this includes antidepressant-related deaths. Consequently, Griffiths and Morgan's findings *de facto* suggest that some of the 'culpable' antidepressants in their data-set might not have been prescribed. It is unlikely that the Office for National Statistics data-set will contain information on prescriptions written as this is often not required on death certificates.

Ghodse, A. H., Schifano, F., Oyefeso, A., *et al* (2003) *Drug-Related Deaths as Reported by Coroners in England, Wales, Scotland, N. Ireland and Channel Islands. Annual Review 2002 and npSAD Report no. 11*. London: European Centre for Addiction Studies, St George's Hospital Medical School.

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Depression and the CIDI

Vicente *et al* (2004) and Weich & Araya (2004) have made important observations regarding the reporting of substantially different rates of mental disorders, particularly major depression, in two well-designed studies in Chile. The lower prevalence of major depression of 3.4% was determined by using the Composite International Diagnostic Interview (CIDI), and Vicente *et al* noted that diagnoses were based on an algorithm. However, they did not describe the nature of the exclusion criteria for the diagnosis of major depression contained within that. They are perhaps unexpected, and may at least partly explain the different results.

The CIDI has a number of probe or stem questions that determine the presumed clinical significance, thereby excluding a number of conditions. For example, it excludes those persons whose symptoms were considered to be due to medication, drugs or alcohol, physical illness or injury; those who considered their symptoms to be trivial or who had not consulted a doctor; those who considered that their symptoms did not interfere 'a lot' (determined by the respondent) with their everyday life and activity; and those who

had not taken medication for their symptoms on more than one occasion.

The validity of these exclusions warrants further consideration. It is acknowledged that the exclusion of those whose depressive disorder is associated with alcohol and/or drugs, or with concomitant physical illness and injury, is consistent with DSM–IV guidelines, but we agree with Paykel (2002) that the DSM–IV 'assigns separate unjustified categories of medical and substance-induced mood disorders'. At the very least the exclusion of persons with such comorbidity, which is common in clinical practice, would result in an appreciable underestimate of depression. In this regard it is of interest that the CIDI even excludes pregnancy as a 'physical condition that can cause symptoms', although it is reassuring that the probe guidelines acknowledge that 'pregnancy is not a physical illness'!

The exclusion of those who considered their symptoms to be trivial risks the omission of those who tend to deny the significance of their symptomatology and who have poor mental health literacy. Indeed, there are data that have demonstrated that the mental health literacy of those in the community who have major depression is no more conducive to identifying depression and recommending its treatment than it is in those without depression (Goldney *et al*, 2001). Therefore, the exclusion of those who believe their symptoms are trivial is not necessarily supported by existing evidence.

Exclusion of those who sought treatment but who had not taken medication more than once is also liable to underestimate the prevalence of depression. Poor mental health literacy and the presence of side-effects which may militate against medication use are but two reasons why those with major depression would be excluded by this criterion.

Each of these exclusion criteria is open to interpretation and we doubt whether many researchers, let alone the average clinician, would be aware of this potential for the CIDI to underestimate the prevalence of depression. Weich & Araya noted correctly that prevalence surveys were designed to provide data for local health planners, but Vicente *et al* observed that planners may well distrust studies when there are marked differences in results.

We have expressed concern about the use of CIDI-derived prevalence figures for depression in Australia, as they could underestimate by at least half both the financial burden on the community and potential service requirements (Goldney *et al*, 2004). It is probable that these exclusion criteria explain the majority of the difference in the results of the two Chilean studies. We trust that health planners in Chile and elsewhere are aware of the potential for underestimation of depression in studies using the CIDI.

Declaration of interest

R.D.G. has received honoraria, been on advisory boards, and has received grants from Bristol-Myers Squibb, Janssen-Cilag, Lundbeck, Organon, Pfizer Australia, Sanofi Synthélabo and Wyeth Australia. G.H. received financial support from Pfizer Australia, Bristol-Myers Squibb Australia and Wyeth Australia.

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Goldney, R., Hawthorne, G. & Fisher, L. (2004) Is the Australian National Survey of Mental Health and Wellbeing a reliable guide for health planners? A methodological note on the prevalence of depression. *Australian and New Zealand Journal of Psychiatry*, **38**, 635–638.

Paykel, E. S. (2002) Mood disorders: review of current diagnostic systems. *Psychopathology*, **35**, 94–99.

Vicente, B., Kohn, R., Rioseco, P., *et al* (2004) Population prevalence of psychiatric disorders in Chile: 6-month and 1-month rates. *British Journal of Psychiatry*, **184**, 299–305.

Weich, S. & Araya, R. (2004) International and regional variation in the prevalence of common mental disorders: do we need more surveys? *British Journal of Psychiatry*, **184**, 289–290.

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Compulsory community treatment and admission rates

We fully agree with Kisley *et al* (2004) that the patients receiving compulsory community treatment are often relatively young, male, single, Black or from a minority ethnic group, unemployed and with a history of schizophrenia, drug use, previous admissions and forensic contact. They obviously are more severely unwell and more liable to be readmitted than are those who are treated without compulsory treatment orders (CTOs). Therefore, it would have been more appropriate to compare the patients on CTOs with individuals whose applications for CTOs were not granted by the family courts (as in New Zealand), or who were discharged by the Mental Health Review Boards (as in Australia).

In our experience, a patient's non-adherence with treatment is a common reason for the psychiatrist to consider compulsory treatment in the community. In this respect, the clinical experience of psychiatrists in New Zealand has been satisfactory as 69.2% reported that CTOs were a useful tool for promoting community treatment for people with mental illnesses (Currier, 1997). On the other hand, there is a paucity of conclusive findings and qualitative research into the experience of patients, carers and professionals regarding compulsory community treatment, with respect to how it may impact upon civil liberties and, in particular, future engagement with mental health services (Moncrieff & Smyth, 1999), which is of concern.

Currier, G. W. (1997) A survey of New Zealand psychiatrists' clinical experience with the Mental Health (Compulsory Assessment and Treatment) Act of 1992. *New Zealand Medical Journal*, **110**, 6–9.

Kisley, S. R., Xiao, J. & Preston, N. J. (2004) Impact of compulsory community treatment on admission rates. Survival analysis using linked mental health and offender databases. *British Journal of Psychiatry*, **184**, 432–438.

Moncrieff, J. & Smyth, M. (1999) Community treatment orders – a bridge too far? *Psychiatric Bulletin*, **23**, 644–646.

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Author's reply: As Robinson & Mahmood point out the crucial issue in our paper is the comparability of those patients who were on community treatment orders (CTOs) and those who were not. Although we controlled for sociodemographic variables, clinical features, case complexity and psychiatric history, we fully acknowledged in our paper that there may have been additional factors that we could not control for in the analysis. These might include social disability, aggression not resulting in a forensic history, medication