

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

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R. D. Goldney, L. J. Fisher Department of Psychiatry, University of Adelaide, The Adelaide Clinic, Gilberton, South Australia 5081, Australia. E-mail: robert.goldney@adelaide.edu.au

G. Hawthorne Department of Psychiatry, Australian Centre for Posttraumatic Mental Health, University of Melbourne, Melbourne, Victoria, Australia

Compulsory community treatment and admission rates

We fully agree with Kiskey *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.

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J. Robinson, T. Mahmood Leeds Mental Health Teaching Trust, Malham House, 25 Hyde Terrace, Leeds LS2 9LN, UK

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

type (including the use of depot preparations) and characteristics of the clinician, treating team or service. Inevitably, a study that took these factors into account would be restricted to one or two services with consequent loss of statistical power and the dangers of selection or referral bias. Furthermore, our study was able to adjust for more service use confounders than others that have shown positive effects of compulsory community treatment (Bindman, 2002).

However, we disagree that patients who had been discharged from a CTO by a Mental Health Review Board would be a more appropriate control group. Even with careful matching, there would be a reason why the intervention group remained on a CTO while the controls were discharged from their order. For instance, patients who remained on compulsory community treatment could have been less insightful about their illness or more likely to have a history of aggressive behaviour. Neither can we accept that surveys of psychiatrists' views on CTOs have any place in an era of evidence-based practice. This would not be accepted as a reason to introduce any other psychiatric intervention. Why should CTOs with their attendant implications for the civil liberties of patients be treated differently?

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S. Kisely Department of Psychiatry, Dalhousie University, Abbie J. Lane Memorial Building, 5909 Veterans Memorial Lane, Suite 9211, Halifax, Nova Scotia B3H 2E2, Canada

Learning disability services

Bouras & Holt (2004) propose a bold solution to a frustrating problem: in a socially inclusive post-institutional society, how should the mental health needs of people with learning disabilities be met? *Valuing People* (Department of Health, 2001) encourages learning disability services to support access to mainstream services, and only provide specialist services to a minority with particularly complex needs. Their idea of a tertiary level service within adult mental health is, therefore, attractive, although probably more so for people with mild learning disability. Individuals could initially use the same service as everyone else and only be 'referred on' if clinically necessary.

But what would this service look like? What, in fact, *are* the specialist mental health needs of adults with learning disabilities? When do these needs require a specialist learning disability mental health service? If you have a mild learning disability and a new psychotic illness should you go to the 'first-episode psychosis' team, the 'home treatment' team, the 'assertive outreach' team, the 'long-term intervention' team or the 'specialist learning disability' team? What would be 'special' about the specialist learning disability service? It is not only about being 'secondary' or 'tertiary' but finding a way to participate in a new mixed economy of 'mainstream' services, where the number of potential interfaces has grown considerably in recent years.

General psychiatrists often look after mental illnesses in people with mild learning disability and do so extremely well. However, if learning disability psychiatry aspires to tertiary status it will be important to respond to those who will rely on it. This is not just the potential patients, but also staff of the mainstream services who will refer them. In my local service, colleagues want recognition that although some of their patients fall outside traditional eligibility criteria for learning disability services, they would none the less benefit from such services and should have equity of access the other way.

Bouras & Holt propose a new, probably rebranded, subspecialty within adult mental health. This has significant implications not just for the National Health Service, but also for the local authorities and other agencies with which it will work. I hope their views will stimulate wider debate.

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A. Flynn Oxleas NHS Trust, 183 Lodge Hill, Goldie Leigh, London SE2 0AY, UK

Psychosocial factors in the pathogenesis of mental disorders

In an interesting Editorial, Leon Eisenberg (2004) discussed the possible impact of the recent advances in genetics and genomics on social psychiatry. He suggested that these advances, instead of diminishing

the importance of social psychiatry, will instead enhance it.

In this context, psychosocial factors may be important environmental factors in the pathogenesis of primary (idiopathic) mental disorders. Several lines of evidence suggest that the primary mental disorders are a product of the evolution of the human brain and mind (Abed, 2000; Peedicayil, 2001). Among the many hypotheses proposed to explain this evolution, the most plausible is the social brain hypothesis, which has also been referred to as the Machiavellian intelligence hypothesis (Dunbar, 1998). According to this hypothesis, the human brain, especially the neocortex, evolved to the relatively large size it has because of the computational demands of the complex social system of primates.

Epigenetics (heritable changes in gene expression that occur without a change in DNA sequence) is thought to have played a major role in the evolution of the human brain (Rakic, 1995), and it is known to involve marked environmental inputs (Strohman, 1997). Hence, by extension, psychosocial factors may be important environmental factors in the pathogenesis of the primary mental disorders.

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J. Peedicayil Department of Pharmacology, Christian Medical College, Vellore 632 002, India

Form–content dichotomy in psychopathology

We read the article on *dbat* syndrome (Sumathipala *et al*, 2004) with interest. The apparent disappearance of the syndrome in the Western world and its persistence in the East can be explained by the form–content dichotomy related to psychopathology. Typically, patients with the