Correspondence

EDITED BY KHALIDA ISMAIL

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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 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.


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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 (amitrtryptline 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 fatalitity 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.


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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. 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.

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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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.


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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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 not then benefit from such services and should have equity of access in 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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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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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
syndrome present with a variety of ‘neurotic’ symptoms. The patients also offer ‘loss of semen’ as the explanation for these disabling symptoms. Such patients are diagnosed as having dhat syndrome if the physician is aware of the label and the explanation, and if he or she focuses on the content. These patients could also receive a label of anxiety, depression or somatisation if the physician emphasises the form of the presentation. The patient perspective of ‘loss of semen’ as the cause of the symptoms would then be perceived as the patient’s explanatory model of his illness.

It has long been recognised that contemporary themes are often incorporated into psychopathology. The culture in south Asia tends to highlight sexual causes for a variety of neurotic phenomena. These explanations generate more acceptance and understanding for the patient than anxiety, depression or somatic symptoms would. Such beliefs are reinforced by traditional Indian systems of medicine which subscribe to these concepts and whose physicians and healers are often the first contact in the pathway to care. Thus, such beliefs are reinforced and perpetuated.

Sexual misconceptions related to dhat are also observed among patients with schizophrenia, substance dependence, bipolar disorders, delusional disorders and major depression.

The focus on form allows psychiatrists to differentiate the different syndromes (Sims, 1988). International classifications have emphasised form over content as a response to the various treatment modalities, based on the recognition and treatment of the clinical syndrome. This does not imply reduced importance being placed on the person’s culture and beliefs. It would mandate the management of the patient’s explanatory model. This is also true for other culture-bound syndromes such as koro.

Clinicians focusing on content make such presentations appear exotic. Physicians emphasising form are able to recognise behavioural syndromes across cultures. The management of patients with such presentations is the same, irrespective of the diagnostic labels employed.


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Author’s reply: Drs Rajesh and Jacob’s suggestion of a dichotomy between form and content is an interesting one. We acknowledge that some patients’ explanations for their distress may be linked to their perception of semen loss or dhat. Drs Rajesh and Jacob highlight that contemporary themes are incorporated into psychopathology, and we agree. However, it is interesting to note that patients with dhat appear to latch on to a more traditional explanation. The ‘disappearance’ of similar complaints in the West may be related to changes in socio-economic conditions. The distinction between form and content of a number of psychopathological symptoms is well worth studying and ripe for further research. We think the suggestion that management is the same, irrespective of the diagnostic labels is simplistic – the cultural explanations of distress and their understanding is paramount in delivering services that will be acceptable and in providing treatments that will be adhered to by patients.

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One hundred years ago

The problem of the feeble-minded

Dr. Tredgold’s paper read at the Guildhall Conference on the Feeble-minded has now been printed, and will be found to contain a very succinct discussion of the sociological bearings of the efforts now being widely made to improve the condition of the mentally defective class. He points out “that with the exigencies of civilized life, the demands upon the intellectual faculties of the individual become daily more exacting”, and consequently that it is highly important to the community to devise means to prevent those who by original infirmity are incapable of meeting them from swelling the ranks of pauperism, crime and insanity. Defining amentia, or mental deficiency, as the manifestation of an imperfect or arrested development of certain cells of the brain, in the great majority of cases due to a defect inherent in the germinal plasm, Dr. Tredgold deduces from the teachings of experience, as well as of etiology, that cure is out of the question; yet training can do much, at any rate for the milder grades of defect, though effective training involves a large expenditure of time and money. Such an expenditure, however, he argues, is not wasted if thereby such unfortunates may be prevented from drifting into pauper vagrancy, into crime, and into insanity, a course which entails upon the community a charge far in excess of that of even specialized education. He does not agree with the views held by some that “all patients suffering from whatever degree of mental defect should be subject to permanent detention”, and contends that “so long as the feeble-minded, or any other section of society, are capable of earning an honest living, it appears unjustifiable to interfere with their liberty”. At the same time, he recognizes the necessity of judicious after-care even for them, and alludes to the scope there is for philanthropic persons to form associations for this purpose, and perhaps also to provide
industrial colonies or boarding-houses of a voluntary character. But training alone does not solve the whole problem, “since it fails in one-third, and is only partially successful in another third of the cases”; and so, in addition to (1) adequate training and (2) after-care associations, Dr. Tredgold advocates (3) the establishment of industrial colonies for the permanent detention of those who are only partially self-supporting, and (4) cheaper institutions for the permanent detention of the failures. The last-mentioned class, he thinks, might fittingly be dealt with under the provisions of the “Idiots Act” so far as detention is concerned: but the difficulty everywhere, except, perhaps, in the metropolitan area, is the lack of sufficient accommodation for unimprovable imbeciles, which Dr. Tredgold thinks the county councils, either singly or in combination, should be required to provide.

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C. Griffiths and O. Morgan
BJP 2004, 185:518.
Access the most recent version at DOI: 10.1192/bjp.185.6.518

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