Correspondence

EDITED BY MATTHEW HOTOPF

Contents ■ Continuing stigmatisation by psychiatrists ■ Common mental disorders in urban v. rural Pakistan ■ Prevalence of depression in old age ■ Antidepressants and suicide risk ■ Cognitive therapy and social functioning in chronic depression ■ Long-term psychotherapeutic relationships in schizophrenia ■ Psychodynamic thinking and the community mental health team ■ What came first: dimensions or categories? ■ Practical application of structured risk assessment ■ Classic text still accessible

Continuing stigmatisation by psychiatrists

In response to the Royal College of Psychiatrists’ anti-stigma campaign, Chaplin (2000) emphasises the role psychiatrists can have in both creating and perpetuating stigma. Chaplin particularly focuses on people with alcohol problems and on those with learning disabilities. However, she omits a third and important group: those with personality disorders.

Lewis & Appleby (1988) showed that such patients are readily stigmatised by psychiatrists. They found that the introduction of the term personality disorder produced marked levels of negative attributions in psychiatrists when comparing otherwise similar clinical vignettes. The reasons for this are complex but there is little evidence that such attitudes have changed. Hinsonwood (1999) has discussed some of the problems this group of patients can present.

The Government’s proposed reforms of the Mental Health Act (Department of Health, 2001) present major challenges to forensic psychiatry in relation to the proposals concerning those with ‘dangerous severe personality disorder’. In the controversy surrounding these proposals it should not be forgotten that they redefine ‘mental disorder’ in its broadest sense, specifically including personality disorder within its scope. Any clinical separation that existed between personality disorder and mental illness may thus be consigned to history. In the light of psychiatry’s stigmatisation of those with personality disorders, the proposals present a very clear challenge to present services.

The Government’s proposed reforms are also contributing to further stigmatisation of those with personality disorders, via the routine newspaper equation of ‘dangerous severe personality disorder’ with personality disorder and the false generalisation of risks from one group to the other that this entails (Gillian & Campbell, 1998). This demonstrates again that stigmatisation is an active and continuing process. The profession of psychiatry itself needs to recognise when it contributes to this process, as well as addressing the contributions of others.


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Common mental disorders in urban v. rural Pakistan

I read with interest Mumford et al’s (2000) paper on stress and psychiatric disorder in urban Rawalpindi. Their findings and explanations of a lower prevalence of common mental disorders in an urban area compared with a rural area of Pakistan (Mumford et al, 1997) need to be treated with caution. Their study population is unrepresentative of the city as a whole. Although they studied an urban slum, strictly speaking, it is a relatively ‘prosperous’ urban slum. The assets and income of this population lie between the fourth and the richest quintile for the Pakistani population (Gwatkin et al, 2000). The use of only male interviewers for female subjects in an orthodox society is also a source of potential bias and cannot be ignored. This was not the case in the rural study. Thus, their findings are unlikely to be generalisable to the urban population of Rawalpindi or other cities of Pakistan. A more plausible explanation for their findings is that financial prosperity together with strong and varied social networks might be associated with a lower prevalence of common mental disorders. Their study attempts to address one aspect of urbanisation due to rural migration, rather than looking at stress and psychiatric disorder in urban Rawalpindi.


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Author’s reply: It is difficult to find a truly representative area of any city, since its districts vary greatly in socio-economic terms. Nevertheless, we made a careful selection in Rawalpindi of a recently established housing area, with poor public utilities, of middle to lower socio-economic status. As presented in our paper, the socio-economic findings confirmed our choice and revealed a wide social spectrum. For example, among men, 31% had had no formal education yet 38% had been educated to tertiary college level. Over half the households had an income of less than 5000 rupees (currently worth £55) per month.

As it happens, the socio-economic status of the nearby rural population in our Gujar Khan study (Mumford et al, 1997) was quite similar to that in Rawalpindi in terms of education and income, and in fact they reported greater ownership of most electrical appliances. So financial prosperity alone is not a plausible explanation for the very striking difference we found in psychiatric morbidity (i.e. less than half) in urban Rawalpindi compared with a rural village in the Punjab.

Whether urban populations in Pakistan indeed have more “strong and varied social networks” than rural populations, as Dr Mirza suggests, remains to be investigated, but this is doubtful. We are planning
Further studies to determine to what extent the quality of life in the city, as opposed to selective migration, can account for the enormous rural–urban differences in psychiatric morbidity. Replication of our study in other cities in Pakistan would be very useful.

However, the truly remarkable finding is not the prevalence of common mental disorders in urban Rawalpindi, which is more in line with rates reported elsewhere in the world. It is the exceptionally high rate of psychiatric morbidity in rural villages in Pakistan, recently confirmed by other investigators in another village near Gujar Khan (Hussain et al., 2000); this cries out for further research.

Regarding the use of male doctors to conduct the second-stage interviews of female subjects, we were obliged to do the same in the previous study in rural Chitral (Mumford et al., 1996). However, we have found no psychometric inconsistencies between the three epidemiological surveys (in Chitral, Gujar Khan and Rawalpindi) to suggest that this was a source of bias while making psychiatric diagnoses according to ICD–10 criteria for research.


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Prevalence of depression in old age

Korten & Henderson (2000) described findings from a 1997 national survey, in which a “household sample of 10,641 individuals representative of the adult population of Australia” was interviewed. They reported that “the prevalence of a diagnosis of any ICD–10 anxiety or depressive disorder . . . declines for both men and women after the age of 55 years”, and noted a trend for psychological symptoms to be fewer among the older age groups. Before accepting the findings as evidence that depression is less prevalent in old age (a conclusion that might affect decisions about allocation of resources), the following points should be noted (see Snowdon et al., 1998).

First, the (approximately) 1600 subjects aged over 65 years were not truly representative of the older population. The survey excluded the 10% of older Australians who were temporarily or permanently residing in institutes (e.g. hospitals, nursing homes, boarding houses), or homeless at the time of the survey. It also excluded those with moderate or severe dementia (Mini-Mental State Examination score <18). The prevalence of depression is considerably higher among those with physical disability, those in residential care and those with dementia.

Second, the instrument forming the core of the interview was the automated version of the Composite International Diagnostic Interview (CIDI), which discounts symptoms attributable to physical illness (Jorm, 2000). Studies that rely on subject-reported symptoms may underestimate the severity of depression in old age, since older patients with depression are less likely than younger patients to acknowledge having affective symptoms (Lyness et al., 1995).

Third, the response rate in this survey was 78%, but the response rate of different age-groups was not known. In other surveys (e.g. Kramer et al., 1985), older subjects have been twice as likely as younger adults to decline involvement. Refusers are more likely to be depressed.

Finally, the report did not differentiate prevalence rates in ‘young-old’ and ‘old-old’ individuals, yet various researchers have found a progressive increase in rate from 55 to 85 years. Jorm (2000) commented on the lack of consistency between researchers regarding whether or not depression becomes less prevalent in old age.


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Authors’ reply: Professor Snowdon questions the validity of the results on the elderly from the Australian National Survey of Mental Health and Well-Being. He rightly points out that the survey failed adequately to cover the population living in institutional care, which was 9% of Australians over 65 in 1998. This is clearly acknowledged in earlier publications. Indeed, the indigenous people of Australia, people in prison, the homeless, the armed forces and the migrant population were also not included in numbers large enough to give stable prevalence estimates, mainly for the sake of economy in what was already a very large undertaking. We used “an unweighted sample with no group represented in a proportion greater than its frequency in the population” (Henderson et al., 2000).

The lack of information concerning the 22% non-responders is indeed regrettable, but does not detract from the finding, consistent with many of the studies cited in Jorm (2000), that the community-dwelling elderly displayed significantly lower levels of depressive symptomatology than younger cohorts. This was reflected in the prevalence rates and in all the scales of psychological distress measured in the survey: the 12-item General Health Questionnaire (GHQ–12), the 12-item Short-Form General Health Survey (SF–12), the Kessler-10 scale, the CIDI screen items for depression and finally the neuroticism items from the Eysenck personality questionnaire considered to reflect vulnerability to psychological symptoms. Each of these scales handles symptoms associated with physical disability in a different way. In all cases, the lower levels of symptomatology observed among 65- to 70-year-olds were maintained into the oldest age group (75 years and above), although the pattern is less stable than for younger age groups because of smaller numbers. The interested reader is referred to Jorm (2000) for a discussion of the possible mechanisms involved.

Information on mental disorders among the oldest old and institutional elderly are of crucial importance for advocacy. But this needs to be addressed in ways other than in large community surveys. This was made explicit from the beginning, where we stated that information on “some of the most significant elements in our society” would need special studies (Henderson et al., 2000). Any concern that our findings might affect decisions about allocation of resources is unlikely to be justified, because it assumes that administrators and policy-makers will
make the grave error of extrapolating from community estimates to the special population of the elderly in hostels and nursing homes.


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Antidepressants and suicide risk

Donovan et al (2000) make interesting points about deliberate self-harm (DSH) and antidepressant drugs, but their report as written is open to grave misinterpretation. Indeed, a reporter brought the article to my attention wanting to know why selective serotonin reuptake inhibitors (SSRIs) increased suicide risk relative to tricyclic antidepressants (TCAs).

A key problem with this cross-sectional, naturalistic study of DSH and antidepressant medications at emergency department presentation is that patients were not diagnosed. The authors write as if antidepressant medications are almost invariably prescribed to treat depression, yet clearly this is not always true. Even within mood disorders, patients may differ greatly in suicide risk. The authors found fragmentary evidence that patients on SSRIs may have been relatively treatment-resistant.

Moreover, SSRIs are prescribed for a growing spectrum of psychiatric illnesses beyond depression. The authors hint at the multiplicity of indications, mentioning enuresis as an indication (presumably for TCAs). Astounding, however, they never mention borderline personality disorder (BPD). Patients with BPD, known for their frequent parasuicidal gestures (Davis et al, 1999), are more likely to receive SSRIs than TCAs: partly because of their safety in overdose, partly for their benefit for impulsivity independent of mood disorder. Hence BPD and other patients at higher risk for DSH may have received SSRIs rather than TCAs. The authors mention this briefly (“... the question of whether patients prescribed TCAs were similar in terms of DSH risk to those prescribed SSRIs”, p. 553) but fail to emphasise how crucial this issue is. (Neither do they mention substance misuse, a further risk factor for self-destructive behaviour.) Given this likely diagnostic and prescriptive imbalance, it is unsurprising that more suicidal patients presenting at emergency departments were taking SSRIs.

In summary, without knowing that equivalent patient populations were receiving the two classes of medications, we cannot compare their effect on suicide risk.


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Cognitive therapy and social functioning in chronic depression

We clinicians constantly encounter patients with major depression in partial remission. They are no longer acutely depressed but continue to present with substantial functional impairment (Paykel et al, 1995). For treatment-resistant depression, only one pharmacological intervention can be recommended today with reasonable evidence, namely lithium augmentation (Austin et al, 1991; Aronson et al, 1996), but this may not be the answer for those with low-grade residual depression.

Scott et al (2000) demonstrated that cognitive therapy can help these people. Critically appraising their article in our evidence-based psychiatry case conference, however, it was very difficult for us to appreciate the substantive significance of this improvement, because only means and standard deviations of scores on the Social Adjustment Scale were reported. Analyses based on these data can show whether or not the treatment is better than the control condition, but cannot show how much better it is – a crucial piece of information for both patients and clinicians. We therefore resorted to the normative data for this scale (Bothwell & Weissman, 1977).

Calculation based on the means and standard deviations under the assumption of a normal distribution showed that, at week 20, 68% of patients with residual depression reached the 95% range of the control subjects when treated with clinical management plus cognitive therapy, whereas only 45% did so when treated with clinical management only. This translates into a ‘number needed to treat’ of 4.4 (95% CI 2.6–12.6).

This is an impressive figure. By adding 16 sessions of cognitive therapy to usual care, we can achieve social remission in one additional patient out of four, compared with continued standard care only. The original authors had concluded, “In patients showing only partial response to antidepressants, the addition of CT produced modest improvement in social and psychological functioning”. We find that the improvement was more than modest and would be clinically meaningful.


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Author’s reply: I am a strong advocate of the use of cognitive therapy in chronic and residual depressive disorders. I am therefore the last to disagree with the comments of Ito and colleagues that there is real benefit in providing psychosocial treatments to individuals with residual depressive symptoms. My comment on social functioning was not meant to underestimate the benefits, but paid heed to two factors. First, although individuals who received cognitive therapy undoubtedly
showed significant improvements in social functioning, there were still obvious impairments within this population. Second, and very importantly, the differences between the cognitive therapy group and the control group were only apparent during the active phase of treatment— the control group continued to make modest gains during the follow-up period so that at 1 year after cognitive therapy there was no difference in social functioning between the two groups. One conclusion from this result is that individuals who receive 16 sessions of cognitive therapy for chronic or residual depressive symptoms may benefit from additional but less-frequent maintenance cognitive therapy sessions.

Lastly, Ito et al are right to point out that calculations of numbers needed to treat from this study are indeed indicative of substantial benefits from using cognitive therapy. For the record, using data from our study and other recent studies, only four to six additional patients need be treated with cognitive therapy to prevent one relapse.

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Long-term psychotherapeutic relationships in schizophrenia

I would like to add what I believe is an important factor not mentioned by Thornicroft & Susser (2001) in their editorial on evidence-based psychotherapies in the community care of schizophrenia. It is a factor that I think is missing from a great deal of psychiatric literature on what helps patients get better and what makes us human. People with schizophrenia have withdrawn from being able to relate to others. They need somebody who is able to provide a long-term therapeutic relationship and is not frightened off by those who say ‘beware of dependency’ or seduced by the culture of brief interventions; a person who can stand up to the ‘package culture’ and stay with the patient and family over a long period of time. This sort of work does not make headlines. I think it is the role of psychodynamic psychotherapists to champion dependency in order that the patient can find something of his or her own from the shattered fragments of self; a mature dependence, within the constraints of illness. This work is not easy, requires support, supervision, time and resources. Perhaps the paucity of evidence is because this apparently simplistic viewpoint meets great resistance and is culturally dystonic.


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Psychodynamic thinking and the community mental health team

I read with great interest Thornicroft & Susser’s (2001) editorial on evidence-based psychotherapeutic care in schizophrenia. It called for evidence-based interventions being implemented in the face of resource limitations and a remedy to the absence of implementation plans for well-established effective interventions such as family therapy. However, Thornicroft & Susser dismiss psychodynamic approaches. Although the general view is that people with schizophrenia do not benefit from intensive psychodynamic psychotherapy, there are some heroic efforts by analysts such as Herbert Rosenfeld (1987). In particular, such approaches do address the imperfection of our models of mental disorder.

One thing the psychodynamic way of thinking can offer members of the community mental health team (CMHT) is understanding of complex mental states from the patient’s perspective, and new ways of understanding those that fall outside of our management strategies. There is no doubt that the delivery of psychoanalytic psychotherapy to people with schizophrenia, on an intensive basis, will not be resourced, nor will the symptom outcomes necessarily be better. Therefore, the cost cannot be justified. However, part of the problem that faces CMHTs is the long-term nature of their work with little reward in terms of symptom improvement and recovery for those with enduring severe mental illness. This can be frustrating and de-skilling for staff, particularly if they have a limited range of therapeutic models. I have worked in an assertive treatment team for the people with severe mental illnesses and one for homeless people with mental illnesses. Staff retain curiosity and capacity to think and question their formulations about patients in a psychodynamic way. Their work continues to be fresh and motivating. This is particularly welcome in light of Wykes et al’s (1997) finding that CMHT staff are not uncommonly depersonalised and therefore unable to empathise with their patients. At a time when there is a movement to ensure good human relationships as well as therapeutic relationships with patients, dismissal of the relevance of psychodynamic thinking in the healthy functioning of a CMHT is premature. This is one area of CMHT functioning that warrants further research, as suggested by Thornicroft & Susser.


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What came first: dimensions or categories?

We read with much interest the paper by O’Dwyer & Marks (2000), and think that the case vignettes reported by the authors fit perfectly with Insel & Akiskal’s (1986) model that considers obsessive-compulsive disorder as a disorder that can develop along a continuum of insight. Therefore, the primary problem is not the boundaries between obsessive-compulsive disorder (or anorexia, or body dysmorphic disorder) and psychosis, but rather at which point insight is lost and the disorder under consideration becomes a frankly psychotic one. If one considers insight as a dimension spanning from normality to the most severe psychiatric conditions, then it will not be difficult to posit several psychiatric disorders along it, with all possible heterogenous combinations. The model becomes even more comprehensive if we add the ‘uncertainty/
certainty’ dimension, so that prevalent ideations, and thus ‘normal’ conditions, can also be accommodated (Marazziti et al., 1999). Thus, insight can be considered to be intertwined with several other dimensions, yet to be identified, and can become disturbed when these other dimensions are altered. In our opinion, therefore, insight is a phenomenon that is only apparently heterogeneous, and in fact is strictly related to other variables and/or clinical core features, so that it may well respond to serotonergic drugs and behavioural therapy, as underlined by O’Dwyer & Marks. The response of insight to various drugs may depend on how close are the links with the other dimension primarily disturbed (e.g. affect; certainty/uncertainty; anxiety). Naturally, these considerations demand new operational criteria that should complete, if not replace, current diagnostic criteria.


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Practical application of structured risk assessment

I was disappointed by Szumukler’s (2001) negative response to Dolan & Doyle’s (2000) excellent review of attempts to measure the risk of violence in psychiatric patients. His pessimism about the practical application of structured risk assessment results from a misunderstanding of the way in which these instruments may be used. First, he emphasises the low baseline. Of course, we do not know the baseline, as the information has never been collected accurately in this country. The 6% in 6 months cited in the letter derives from retrospective ratings by clinicians. Studies in the USA show that rates of violence by psychiatric patients may rise threefold if self-report is supplemented by official records and by the account of a key informant. In any case, the low baseline is irrelevant. Most risk assessment tools were developed on high-risk populations, usually people who had already committed serious offences. They were not designed to be applied to all patients. If these instruments were applied to those 6% of patients with a record of recent violence, would they assist future management by identifying those who were at highest risk of a repetition? I do not know the answer, but it is a sensible and important question.

The second problem is an unrealistic expectation of what such instruments can do. They are a supplement to good clinical practice, not a substitute for it. In many parts of Canada, for example, the Violence Risk Appraisal Guide (Harris et al., 1993) is administered to all patients admitted to a medium secure unit. The results do not dictate future management but, like the results of any other investigation, they inform it. I have no good answer to the question of why we are not evaluating this practice in the UK. When one concentrates on small, high-risk populations, the resource arguments lose this force. With medium secure admissions in this country averaging >18 months in duration, at £100 000 per patient per annum, a few hours of a psychologist’s time is neither here nor there.

It is unfortunate that British psychiatry has been slow to look at the application of these instruments, on which much basic research has already been done. The situation is even more depressing because National Health Service trusts, fearful of lawsuits, are insisting on the introduction of standardised risk assessments. In most cases, they are not of proven value. Trusts in different areas use different instruments, making evaluation more difficult. The measures are likely to be employed indiscriminately, becoming just another form to be filled in. Would it not be better if this initiative were driven by clinicians, and underpinned by a sound methodology for evaluation?

Psychiatry must not persist in assuming that violence, an uncommon complication of mental disorder, is unimportant because of its rarity. Reforming the Mental Health Act (Department of Health, 2001) illustrates that concern about violence dominates the thinking of politicians in this area. It is unlikely that they are going to lose votes by overstating the level of risk associated with psychiatric patients, so the profession is going to have to come up with something better than bland reassurance. We are in a weak position, so long as we lag behind North America and parts of Europe, both in our use of existing risk-assessment technology, and in research into violence. The paper by Dolan & Doyle should stimulate us to make up some of that lost ground.


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Classic text still accessible

Professor Goldberg’s (2001) excellent retrospective demonstrates the breadth of reading that used to be part of psychiatric training. His statement that General Psychopathology by Karl Jaspers is “out of print” is happily not the case. It was republished by Johns Hopkins University Press in 1997 in two volumes and is readily available via internet booksellers.

As Goldberg states, the work influenced a whole generation of psychiatrists and remains a very readable text, full of detail and close observation, which harks back to a time when meticulous detail and careful attention to language were essentials of good psychiatric practice. Any student of psychiatry, at any level, would do well to take a look.


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One hundred years ago

Loss of memory and of sense of personal identity

Cases of this sort, though decidedly uncommon in occurrence, are of considerable medical and legal interest. The record of a specially interesting case is published in the Philadelphia Medical Journal (May 19th, 1900) and recalls the occurrence of a similar case recently at Cambridge where a young undergraduate disappeared from his quarters and was not heard of for several days till he was discovered staying at an inn in the country some distance from Cambridge. He seemed to have had no recollection of leaving his rooms in Cambridge or of any of the incidents subsequent to that event and until his arrival at the inn, except that he was feeling tired and fatigued and that his boots and clothes were wayworn and dusty. The undergraduate was a youth subject occasionally to epileptic fits at infrequent intervals, and it appears that after such a fit he lost his memory and his sense of personal identity and wandered about until he came to the inn where, wearied and worn out, he took lodgings. He had lost all recollection of his former life and condition and stayed in the inn for some days. Then a second fit occurred after which the memory of his former self returned and he was able to realise his position and find his way back to the University. The case reported in the Philadelphia Medical Journal is that of a law student, aged 19 years, living in New York, who suddenly lost his memory and sense of personal identity and found himself wandering in the street of what he considered a strange city. He was so perplexed that he asked people in the street if they could tell him who he was and went to the libraries and hotels to search in the newspapers for stories of missing persons in order that he might get some clue to his identity. He lived at a hotel and after spending five days in fruitless wanderings and inquiries he finally entered a police station and inquired of the sergeant on duty whether he could inform him in what city he was and requested that search might be made in the record of missing persons. When this was done it was found that there was a description closely tallying with his appearance. A detective was sent with the youth to the address given in the description, where the wanderer was received with great joy by his mother and sisters. To their great surprise, however, he thanked them very politely, but assured them that he did not know them or the place. The mother told the police that he was a somnambulist and had left home previously under similar conditions. In the youth’s pocket there was found a diary in which he had entered the details of his daily experience since the time he left home and forgot who or where he was. “His physicians state that his attack of amnesia is gradually passing off, and that while he shows memory of other events in his past life any reference to himself seems to be the signal for another lapse of his memory.” The import of cases such as the above is evident both from a medical and medico-legal standpoint, and it is interesting to note their affinities with such neuropathic conditions as epilepsy and somnambulism.

REFERENCE

Lancet, 9 June 1900, 1670.
Researched by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey

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Corrigendum

Common mental disorders in Santiago, Chile. Prevalence and socio-demographic correlates. BJIP, 178, 228–233. Rows 2 and 3 of Table 2 (p. 231; ‘One-week prevalence of Clinical Interview Schedule–Revised (CIS–R) ‘cases’ by gender . . .’) should read:

<table>
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<th>Gender</th>
<th>Sample size</th>
<th>% Prevalence (95% CI)</th>
<th>Crude odds ratio (95% CI)</th>
<th>Adjusted odds ratio (95% CI)</th>
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<td>Male</td>
<td>1538</td>
<td>15.7 (15.6–15.7)</td>
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<tr>
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<td>2332</td>
<td>33.6 (33.5–33.7)</td>
<td>2.72 (2.19–3.38)</td>
<td>2.37 (1.84–3.07)</td>
</tr>
</tbody>
</table>

1. Weighted sample.
2. Adjusted by age, marital status, education level, social class, employment status, family type and household size.
Continuing stigmatisation by psychiatrists
D. Beales
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