

ECT is 1.20, which is not significant but which the authors refer to as ‘a marginally significant trend’, and ‘significantly increased suicide rate’. The finding that the risk from suicide is highest in the first 7 days after discharge and ECT is based on a small sample ($n=6$). Although the authors concede that admission status and time since discharge are important confounders in the analysis of suicide in patients with affective disorders, the statistical analysis does not consider these factors when calculating the relative risk of suicide after ECT. The authors discuss in some length the lack of a selection bias of patients with poor physical health. However, it is likely that patients with very poor physical health are not given ECT and this introduces a selection bias. Also, given the bias that occurs as patients at high risk for suicide are given ECT preferentially, this calls into question the validity of the conclusions. Further, it would have been very useful if the authors could have compared the death rates with those in the general population. This study provides several good research questions which need to be pursued further.

Munk-Olsen, T., Laursen, T. M., Videbech, P., et al (2007) All-cause mortality among recipients of electroconvulsive therapy. Register-based cohort study. *British Journal of Psychiatry*, **190**, 435–439.

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Authors’ reply: Both Le Strat & Gorwood and Bharadwaj & Grover comment on the finding of a decrease in mortality in ECT-treated patients. In Denmark, all psychiatric patients are given a thorough medical assessment prior to any somatic treatment. This is partly because of the well-known cardiac contraindications for the use of tricyclic antidepressants which were widely used during the study period from 1976 to 2000, as the selective serotonin reuptake inhibitors (SSRIs) were only available in the latter part of the period described. Furthermore, SSRIs were generally considered less effective than tricyclic antidepressants or ECT in patients with

severe depression. Accordingly, ECT was often used in patients with contraindications for tricyclic antidepressants. We are aware that this notion is at variance with several British guidelines (e.g. National Institute for Clinical Excellence, 2003) but it is in accordance with Danish and American Psychiatric Association guidelines, which state that the only contraindications to ECT are cerebral and other aneurysms. In Denmark, a preponderance of patients with medical illness is thus found among ECT-treated patients compared with those treated with tricyclic antidepressants and we therefore maintain our conclusion.

Drs Bharadwaj and Grover point out that admission status and time since discharge are important confounders. We fully agree and have hence adjusted for these variables in the analysis. The variables in Table 3 on risk of suicide in ECT recipients were mutually adjusted but this was not mentioned specifically in the footnote.

The number of patients dying by suicide in the first week after ECT discontinuation was small, and therefore our results should be interpreted with caution, as we mention in the discussion. Electroconvulsive therapy is often administered to patients who are assessed to be suicidal and we acknowledge that this could introduce selection bias (confounding by indication), which we also mention in our paper. These are the reasons why we concluded that: ‘the increased suicide rate among ECT patients shortly after treatment is probably a result of bias’ and we therefore disagree that the validity of the study is questionable regarding suicide rates after ECT.

A more in-depth description of the ECT patients can be found in a paper based on the same data (Munk-Olsen *et al*, 2006).

Munk-Olsen, T., Laursen, T. M., Videbech, P., et al (2006) Electroconvulsive therapy: predictors and trends in utilization from 1976 to 2000. *Journal of ECT*, **22**, 127–132.

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Measuring stigma

King *et al* (2007) frequently state that their stigma scale is measuring ‘the stigma of mental illness’ but, when closely scrutinised, it measures nothing other than stigmatisation perceived by users in out-patient, in-patient and crisis settings. There is no evidence that this is an objective assessment of stigmatisation. Users’ perception of stigma is affected by their mental state, depression, persecutory delusions or hallucinations. These symptoms can help to exaggerate the estimate of social stigmatisation (including rejection and discrimination) and hence the assessment is by no means an accurate measure. Measurements of more objective perceptions of stigmatisation can only be obtained from users in remission.

The reported negative correlation between self-esteem and perceived stigma can be confounded by high rates of both low self-esteem (e.g. Axford & Jerrom, 1986; Barrowclough *et al*, 2003; Blairy *et al*, 2004) and persecutory ideation and depressive cognition, including ‘self-stigmatisation’ in people with mental illness. Indeed, low self-esteem is a common symptom in psychiatric conditions such as depressive disorders, in which people can perceive more rejection and discrimination than warranted. Overemphasis on this correlation can divert attention from the fact that the correlation has to do more with people’s mental state than objective level of social stigmatisation.

An instrument can only be called ‘standardised’ if it is shown to be both reliable and valid. This instrument is not validated and so cannot be called standardised, on the basis of mere test–retest reliability. The correlation between the stigma scale and self-esteem scale is not an indication of validity of the instrument and although King *et al* admit this, they end up referring to their instrument as ‘standardised’ and to the correlation as ‘concurrent validity’.

A wide range of people with diverging diagnoses and mental states were recruited by King *et al* but there was no randomisation and no exclusion criteria. Even the ‘perceived stigmatisation’ cannot be attributed to a particular category of patients with a given diagnosis, or at least to psychiatric users in general, owing to lack of randomisation and inclusion of arbitrary proportions of participants with different diagnoses. This is likely to cause problems

in comparative studies. Also, stigma by definition excludes 'positive aspects of mental illness'. This is why the authors decided to reverse the scores of the 'positive aspects of mental illness' factor. For this reason, they should have also called the factor 'negative aspects of mental illness', as a high score on this new factor then represents stigmatisation and its negative influence on the person.

In brief, a scale which partly measures people's mental state and partly objective social reality is neither valid nor standardisable because it cannot measure what it is supposed to measure (i.e. it cannot satisfy the fundamental condition of validity).

Axford, S. & Jerrom, D. W. (1986) Self-esteem in depression: a controlled repertory grid investigation. *British Journal of Medical Psychology*, **59**, 61–68.

Barrowclough, C., Tarrier, N., Humphreys, L., et al (2003) Self-esteem in schizophrenia: relationships between self-evaluation, family attitudes, and symptomatology. *Journal of Abnormal Psychology*, **112**, 92–99.

Blairy, S., Linotte, S., Souery, D., et al (2004) Social adjustment and self-esteem of bipolar patients: a multicentric study. *Journal of Affective Disorders*, **79**, 97–103.

King, M., Dinos, S., Shaw, J., et al (2007) The Stigma Scale: development of a standardised measure of the stigma of mental illness. *British Journal of Psychiatry*, **190**, 248–254.

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Authors' reply We were puzzled by Dr Haghghat's criticism of our development of a stigma scale and would like to respond to his points. First, ours is a self-report measure of perceived stigma and we do not claim otherwise. Perceived stigma is a valuable construct that may have a greater impact on mental and social well-being (including relationships and occupation) than so-called objective acts of discrimination. This is also true of social support. Second, we agree that the relationship between perceived stigma and low self-esteem is potentially confounded by low mood. However, our sample contained a heterogeneous group of participants from a range of settings and thus it is unlikely that a sizeable proportion were depressed at the time

of the study. In addition, Dr Haghghat overlooks the complexity of any putative association between stigma and depressive symptoms. Perceived stigma may cause or maintain depressive episodes.

Third, it is important to avoid invalidating reports of perceived stigma by dismissing them as depressive or paranoid epiphenomena. Fourth, Dr Haghghat claims that our instrument has no validity. In fact, as we made clear in our paper, it is based firmly on the views and experiences of people with mental illness who were interviewed in depth in a previous study (Dinos *et al*, 2004), and thus it has greater validity than many scales used in the field of mental health. Fifth, we do not understand Dr Haghghat's reference to randomisation, which has no role here. If he means random selection of people to participate, then our method closely approximates to this in that potential participants were not selected on any pre-determined basis. Naturally, participation depends to some degree on participants' abilities and personal inclinations but that is true whether selected randomly or not.

Finally, participants in our earlier qualitative study (Dinos *et al*, 2004) emphasised that positive outcomes may arise from experiencing mental illness and thus such items were included in our scale. We reversed their scores to indicate that stigma might be greater when such positive aspects were lacking. This is not the same thing as assuming mental illness has only negative aspects. In parallel fashion the opposite of risk is not protection, it is lack of risk.

Dinos, S., Stevens, S., Serfaty, M., et al (2004) Stigma: the feelings and experiences of 46 people with mental illness. Qualitative study. *British Journal of Psychiatry*, **184**, 176–181.

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Metabolic syndrome and intellectual disability

Mackin *et al* (2007) highlight the importance of screening and management of metabolic syndrome in patients with severe mental illness. This is particularly important in patients with intellectual disability as they have high rates of both physical and psychiatric comorbidities compared with the general population (Welsh Office, 1996). In addition, considerable evidence points to a disparity between the health of people with learning disability and the general population, and this was also highlighted in two Mencap reports (Mencap, 2004, 2007).

Suggested causes for this disparity include specific patterns of complex health needs associated with the aetiology of their intellectual disability, sensory and communication difficulties, reliance on carers to communicate their health needs, and barriers to healthcare accessibility due to poor professional knowledge and attitudes.

The Government White Paper *Valuing People* (Department of Health, 2001) acknowledges this disparity and identifies the improved healthcare of people with intellectual disability as a key outcome. However, the document is a little unclear on how these aims will be achieved.

As Mackin *et al* point out few studies specifically examine the impact of different models of care on physical well-being and comorbidities in people with severe mental illness, and this is also the case for people with intellectual disability. There is a pressing need for evidence-based integrated models of care for delivering high standards of care for this patient group.

Department of Health (2001) *Valuing People: A New Strategy for Learning Disability for the 21st Century*. TSO (The Stationery Office).

Mackin, P., Bishop, D., Watkinson, H., et al (2007) Metabolic disease and cardiovascular risk in people treated with antipsychotics in the community. *British Journal of Psychiatry*, **191**, 23–29.

Mencap (2004) *Treat Me Right*. Mencap.

Mencap (2007) *Death by Indifference*. Mencap.

Welsh Office (1996) *Welsh Health Survey 1995*. Welsh Office.

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