Correspondence

EDITED BY TOM FAHY

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Treatment of severe personality disorder

Sir: I was interested to read the paper by Dolan *et al* (1997) as I am involved in developing guidelines for the treatment of patients with severe personality disorder. I was, however, concerned that the conclusions and recommendations of the paper were inconsistent with the data provided. There are three areas of concern.

First is the nature of the controls used in the study. The authors highlight the 22 controls who were refused funding for admission and their similarity to patients admitted to Henderson Hospital. Less emphasis is given to the remaining two-thirds (n=45) who were not admitted because of non-attendance, clinical unsuitability or admission elsewhere. This is hardly a random sample but an often self-selected group with a particularly poor prognosis.

Second, in discussing the limitations of their study, the authors fail to stress that only 25% of the original sample was successfully followed-up (137 out of 598). It is, of course, difficult to undertake a study of this type in this group of patients, but such a degree of follow-up bias must limit the generalisability of Dolan *et al*'s results.

Third, the authors fail to mention that the outcome measure used, the Borderline Syndrome Index, could not be validated against another standardised psychiatric instrument of borderline personality disorder (Marlowe *et al*, 1996).

Given these methodological problems it is hardly surprising that health authorities are reluctant to fund expensive in-patient treatment at Henderson Hospital. Rather than berating health authorities, it might be more appropriate to undertake further research into what still remains an unproven treatment.

Dolan, B., Warren, F. & Norton, K. (1997) Change in borderline symptoms one year after therapeutic community treatment for severe personality disorder. *British Journal of Psychiatry*, **171**, 274–279.

Marlowe, M. J., O'Neill-Byrne, K., Lowe-Ponsford, F., *et al* (1996) The Borderline Syndrome Index: a validation study using the Personality Assessment Schedule. *British Journal of Psychiatry*, **168**, 72–75.

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Authors' reply: We support some of Dr Kisely's reservations about the methodology we employed. However, in our view, these limitations were clearly stated and openly acknowledged in the original paper.

At no point did we claim that the non-admitted group was a control group, and the extent to which they are comparison groups, with all the accompanying disadvantages, is clearly stated in the paper. Dr Kisely is correct that the whole non-admitted group is not a random sample and this was our point in mentioning the group whose funding was refused by health authorities. Since we made no attempt to obtain a randomly allocated sample, on ethical and clinical grounds, and no claim for such methodology was made, the criticism that the sample was not random is irrelevant. Dr Kisely provides no evidence for his assertion that non-admitted patients form a "self-selected group with a particularly poor prognosis". Research data which may have informed any clinical differences between the non-admitted (self-selected or otherwise) and the admitted groups are reported in our paper.

The low response rate was fully acknowledged, and the potential limitations of this were highlighted (Dolan *et al*, 1997, p. 275).

We acknowledge Marlowe *et al*'s (1996) criticism of the Borderline Syndrome Index (BSI), and we cited his paper for the reader. It is important in interpreting Marlowe *et al*'s findings to note that they compared the BSI with the Personality Assessment Schedule, which was devised prior to publication of the DSM system and, although it is compatible, is derived from ICD–10 categories. Furthermore, Marlowe *et al* did not control for multiple personality disorder diagnoses. Although the subjects in that study were diagnosed with borderline personality disorder, we do not know for how many other diagnoses they also met criteria. Furthermore, Marlowe *et al*'s study addressed the question of whether the BSI could help to identify personality-disordered patients in a heterogeneous group, whereas the study of changes following specialist treatment addresses the severity of personality disorder in a population already known to be personality-disordered.

Health authorities have been reluctant to fund patients with severe personality disorder long before the publication of our prospective outcome study, which could clearly not have influenced their decisions, positively or negatively, in retrospect. Previously (Dolan *et al*, 1994) it was reported that only one in three patients with severe personality disorder, referred as extra-contractual referrals, received funding. Such decisions were made on a financial rather than a clinical basis.

Dolan, B. M., Evans, C. & Norton, K. (1994) Funding treatment of offender patients with severe personality disorder: do financial considerations trump clinical need? *Journal of Forensic Psychiatry*, **5**, 263–274.

——, Warren, F. & Norton, K. (1997) Change in borderline symptoms one year after therapeutic community treatment for severe personality disorder. *British Journal of Psychiatry*, **171**, 274–279.

Marlowe, M. J., O'Neill-Byrne, K., Lowe-Ponsford, F., *et al* (1996) The Borderline Syndrome Index: a validation study using the Personality Assessment Schedule. *British Journal of Psychiatry*, **168**, 72–75.

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Economics of attachment disorders

Sir: Adshead (1998) eloquently describes the impact on staff of those who have disturbed patterns of attachment, but her reasoning can be taken a little further and point to service-level considerations and a link between cost of treatment, which is often inappropriate, and severity of attachment disorder.

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If we start from the well-researched position that severe disturbances of emotional development are likely to be enacted in later life, and add Adshead's observation that this is often manifest in care-seeking behaviour with psychiatric services, it is a small step to propose that resources are used in some proportion to that disturbance. Conversely, disturbance of attachment can be 'weighed' like patients' case notes, by the input from the services that they receive.

So perhaps the cost-offset studies for personality disorder, such as the Henderson Hospital work (Menzies *et al*, 1993) are measuring improvement in dysfunctional attachment patterns as well as counting money. Clinicians in therapeutic communities and other settings for treating emotionally unstable personality disorder are likely to be more comfortable thinking that they are making a fundamental difference to the way patients relate to others, than that they are doing it to save money, but perhaps they are the same thing.

Adshead, G. (1998) Psychiatric staff as attachment figures. Understanding management problems in psychiatric services in the light of attachment theory. *British Journal of Psychiatry*, **172**, 64–69.

Menzies, D., Dolan, B. M. & Norton K. (1993) Are short term savings worth long term costs? Funding treatment for personality disorders. *Psychiatric Bulletin*, **17**, 517–519.

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PTSD and victims of torture

Sir: I read with interest the article by Gorst-Unsworth & Goldenberg (1998) on psychiatric morbidity in Iraqi victims of torture. The authors speculate on the causes of the relatively low level of post-traumatic stress disorder (PTSD) within their sample compared with the rate found among Vietnam veterans.

While it is of interest to compare the rates of PTSD in various high-risk groups, it is important not to lose sight of the specific features of each of these groups. Torture is, by and large, a highly selective act employed by tyrannical states against certain individuals for a complex range of political purposes. It is rarely an indiscriminate act. In the Iraqi context, torture is used as a routine form of social control which is meant to terrorise the individual concerned (and/or extract information from the victim) as well as to act as a deterrent to others. The targets of the torture are carefully selected from suspected activists or other independently minded individuals who appear to stand out among their peers and who are judged to pose a short- or long-term threat.

From the point of view of the tyrannical state, torture is a finite resource (limited by the availability of 'skilled torturers') that must be deployed to best effect. The non-random nature of the targets of torture is in clear contrast to other victims of disaster, where either minimal or no selection applies (e.g. victims of road traffic accidents or the veterans of a conscript army).

An understanding of the interaction beween the victims' characteristics and the characteristics of the trauma may offer us a better chance of predicting the level of risk of developing a particular psychiatric syndrome following traumatic and stressful events.

Gorst-Unsworth, C. & Goldenberg, E. (1998) Psychological sequence of torture and organised violence suffered by refugees from Iraq. Trauma-related factors compared with social factors in exile. *British Journal of Psychiatry*, **172**, 90–94.

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Cost-effective community psychiatry

Sir: There is good evidence to support Dr Tyrer's (1998) point that fragmenting community care for serious mental illness leads to longer hospital admissions. In a natural unplanned experiment during the Daily Living Programme of home-based care in south-east London, removing the community care team's responsibility for any crisis admissions of their patients led to a trebling in duration of those crisis admissions (Marks *et al*, 1994). Having the same staff responsible for the patient's community care and any crisis admissions seemed critical if care was to remain cost-effective.

Marks, I. M., Connolly, J., Muijen, M., *et al* (1994) Home-based versus hospital-based care for people with serious mental illness. *British Journal of Psychiatry*, **165**, 179–194.

Tyrer, P. (1998) Cost-effective or profligate community psychiatry? *British Journal of Psychiatry*, **168**, 1.

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Sulpiride augmentation on schizophrenia

Sir: I would like to comment on the article by Shiloh *et al* (1997) which suggests an augmenting effect of sulpiride in patients who failed to respond satisfactorily to clozapine.

The population studied comprised patients with schizophrenia who, after receiving clozapine for 12 weeks at daily doses ranging from 350 to 600 mg, showed a partial clinical response. Most British psychiatrists would not consider these patients as resistant to clozapine and would increase the daily dose, side-effects allowing, up to 900 mg.

Recent clinical plasma levels studies on clozapine support this practice, pointing to the importance of achieving values in excess of 370 ng/ml (Buckley, 1996). When Miller *et al* (1994) raised the dose in seven patients not responding to clozapine who had plasma concentrations below 370 ng/ml, five of them improved. It has to be added that Kane *et al* (1988), in their seminal study on the effectiveness of clozapine in treatment-resistant patients, prescribed daily doses of up to 900 mg.

It is therefore possible that the clinical improvement obtained in the study was not due to any specific pharmacodynamic effect, but was caused by an aspecific increase in the dopamine blockade in a population which was receiving clozapine at a subtherapeutic dosage. The same effect would probably have been achieved by adding any of the other major tranquillisers or, better still, increasing the dose of clozapine. Bone marrow depression, which is the most dangerous adverse effect of clozapine, is not dose dependent. It would, therefore, be unwise to advocate a polypharmaceutical regimen, which introduces the very unpleasant side-effects secondary to the sulpiride-induced hyperprolactinaemia, instead of just increasing the dose of clozapine.

As a last point it is unlikely that, as suggested in the article, the augmenting effect of sulpiride could be due to a pharmacokinetic interference at the level of the P450 liver metabolism of clozapine. The hepatic metabolism of sulpiride is, in fact, negligible, more than 95% of the compound being excreted unchanged in the urine (Imondi *et al*, 1978).

Buckley P. F. (1996) Treatment of schizophrenia: advances during the decade of the brain. *British Journal of Hospital Medicine*, **11**, 574–580.

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