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Clozapine treatment following blood dyscrasia

Dunk *et al* (2006) investigated 53 patients who were rechallenged with clozapine following leucopenia or neutropenia during previous therapy and found that 33 did not experience a second episode of blood dyscrasia and were able to continue drug treatment. This result is of considerable clinical relevance because it suggests that some patients with leucopenia or neutropenia may unnecessarily be denied effective clozapine treatment.

We agree that there may be two types of clozapine-associated neutropenia: an early sign of incipient agranulocytosis and a more common transient and harmless phenomenon, not necessitating the discontinuation of drug treatment. Transient neutropenia (defined as a return of the neutrophil count to normal values without changing the clozapine dosage) was found in 22% of 68 patients treated with clozapine for the first time (Hummer *et al*, 1994). Neutropenia of short duration (2–5 days) and weekly benign variations of the neutrophil count have been reported. Marked circadian variations in the number of circulating neutrophils (morning pseudoneutropenia) have also been described in several clozapine-treated patients (Ahokas & Elonen, 1999; Esposito *et al*, 2004).

The actual issue might therefore not be which patients could be rechallenged with clozapine following drug-associated neutropenia but which could be maintained on clozapine despite this side-effect. Laboratory screening tests, including the use of a hydrocortisone test, are being devised to determine whether clozapine-associated neutropenia is transient or malignant (Murry & Laurent, 2001). Until these tests become available for routine use, it is necessary to increase the frequency with which white blood cell counts are determined. As first suggested by Ahokas & Elonen (1999), when the absolute neutrophil count is below the normal range in the morning, the test should be repeated in the afternoon of the same day before a decision to stop clozapine treatment is made. This might be the basis for further clarification of the significance of transient neutropenia.

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Authors' reply: We agree that attempts should be made to continue patients on clozapine if at all possible in order to give them an adequate trial of the drug. The Clozaril Patient Monitoring Service (CPMS) routinely advises that samples should not be taken first thing in the morning for patients with borderline white cell/neutrophil counts to avoid the problem of morning pseudoneutropenia. Taking samples after a brisk walk has also been suggested in such patients.

Although Hummer *et al* (1994) reported transient neutropenia in 22% of 68 patients they defined leucopenia as a white cell count $<3.5\times10^9$/l and neutropenia as a neutrophil count $<2.0\times10^9$/l. The mean neutrophil count at the time of the transient neutropenia was 1.78×10^9/l. In the UK and generally, the cut-off points for leucopenia and neutropenia used in clozapine monitoring are lower (3.0×10^9/l and 1.5×10^9/l respectively) and patients with counts higher than this are not required to stop clozapine but are monitored more frequently. The relevance of the findings of the study to all clozapine-treated patients must therefore be considered with this point in mind.

The use of a hydrocortisone test to distinguish between benign and malignant neutropenia is of great interest but findings must be interpreted with caution as the study involved only three patients (Murry & Laurent, 2001). Furthermore, the risk of further stressing a compromised bone marrow must be borne in mind with such interventions. Whether it is possible to distinguish between transient neutropenia and the prelude to agranulocytosis in clozapine-treated patients remains to be determined.

Declaration of interest

L.D. has undertaken consultancy for Novartis UK and Novartis Australia and received a fee from Novartis Australia for the preparation of the paper; she was formerly employed by Novartis UK. L.A. and C.A. are employed by Novartis UK.

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Personality disorder and outcome in depression

Newton-Howes *et al* (2006) attempted to definitively answer the question of whether comorbid personality disorder affects outcome in people with major depression. Their search strategy, study selection, data summary and analysis are clearly described.

However, the heterogeneous nature of their data does not allow such definitive answers as they claim. As has been noted previously (Charney *et al*, 1981; Black *et al*, 1988; Mulder, 2002) people with depression and comorbid personality disorders are less likely to receive drugs or electroconvulsive therapy (ECT), precisely the treatments (as this meta-analysis reports) that they are more likely to respond to. Therefore, the only fair assessment of the effect of personality disorders on outcome is the randomised controlled trial (RCT). When the meta-analysis was confined to such trials the effect size was smaller but was still significant. A recent meta-analysis that restricted itself to RCTs of drug treatment reported no effect of recent comorbid personality disorder on outcome in people with depression (Kool *et al*, 2005). This suggests that better studies with more effective treatments will report less effect of comorbid personality disorder on outcome.

What does this mean clinically? Less than the authors claim, I would suggest. The sample size required to detect the difference between the outcome of patients with depression and personality disorders and similar patients but without personality disorder exceeds 1000 (and this by using all trials rather than just RCTs), suggesting minimal effect in normal clinical practice. Although it seems like a good idea, there is no evidence that targeting comorbid personality pathology is necessary and will result in better outcomes for those with depression. The numbers needed to show an effect of personality disorder on outcome suggest that a treatment trial specifically designed to look for a treatment effect would require such large numbers that it will never be performed.

What the meta-analysis suggests, along with many recent studies, is that good treatment of depression, particularly using drugs and ECT if indicated, will result for the most part in a similar outcome for people with and without personality disorders. Such treatments may in fact be effective for the comorbid personality disorder. Clinicians should be encouraged that aggressive treatment of mood disorder is likely to lead to a positive outcome in those with depression and comorbid personality disorder.

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Authors' reply: We did not set out to provide a definitive answer to a specific question; our objective was to provide a comprehensive synthesis of all available studies and, by using a systematic approach to data collection with limited exclusion criteria and a robust statistical analysis, we have produced the best summary available to date. Although data from RCTs are valuable, they are not the sole arbiters of association and so our information covers much more than the necessarily short-term span of an RCT. Even if we confine our analysis to the 14 RCTs in our review, we obtain an odds ratio of 1.60 (95% CI 1.25–2.06), indicating better resolution of a depressive episode without comorbid personality disorder. Both cohort studies and case series support this finding, with all groups identifying a poorer outcome in those with a personality disorder.

The overview by Kool *et al* (2005) included just six RCTs, all of which involved drug treatment with antidepressants and none of which extended beyond 24 weeks. The judgement that these were the only trials of 'high quality' may be suspect, as it is difficult to assess quality from published papers (Soares *et al*, 2004). In addition, despite their claim that studies were excluded when 'they presented reanalyses of a study population that was already included', we believe that their two largest studies (Hirschfeld *et al*, 1998; Russell *et al*, 2003) both stem from the same trial (albeit with different outcomes) first reported by Rush *et al* (1998). Excluding Russell *et al* (2003), from their meta-analysis slightly widens the 95% CI for the reported (inverted) odds ratio of 1.14 from 0.93–1.39 to 0.88–1.45, neither of which are inconsistent with our own estimate above.

Our review also suggested that there may be a better response to the treatment of comorbid depression and personality disorder with antidepressant drugs than with other treatments, which is consistent with Kool *et al* (2005). We remain optimistic about treating personality pathology successfully in this group, and think that newer treatments which focus on personality should be compared with aggressive pharmacotherapy for those who are regarded as having 'resistant' depression.

Declaration of interest

P.T. and T.J. belong to a UK Medical Research Council Cooperative Group (Mencog) evaluating mental health interventions. P.T. is Editor of the *British Journal of Psychiatry* but had no part in the evaluation of this letter.

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