restraint. The reality is probably somewhere between these extremes.

As a general psychiatrist with a special interest in psychological treatments (especially cognitive–behavioural therapy) I am not proposing that medication has all the answers or is even the preferred choice in all cases. However, I have to persuade many patients on a regular basis to take antidepressant medication before improvement can occur. The 'chemical imbalance theory' is a useful working hypothesis for one cause for depression. There is a current climate of opinion among those who regularly surf the internet that medication is all bad, dangerous and addictive. Clinical psychiatrists like me have an uphill battle to persuade patients to take life-saving medication which articles such as those by Moncrieff, and the websites she directs us to, make even harder.

Moncrieff, J. (2006) Psychiatric drug promotion and the politics of neoliberalism. *British Journal of Psychiatry*, **188**, 301–302.

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Author's reply: By mentioning the use of steroids in asthma, Dr Stern highlights an important contrast between our understanding of how drugs work in general medicine and how drugs work in psychiatric conditions. In general medicine the effects of drugs can usually be understood by their actions on some level of the pathological process that generates the symptoms. Thus, steroids reduce the inflammatory response that gives rise to some of the symptoms of asthma. In contrast there is no evidence that drugs used in psychiatric conditions act on specific neuropathological processes. No specific physical pathology has been established for any major psychiatric condition and other evidence that drugs might be specific is lacking. Instead I have suggested elsewhere the alternative hypothesis that psychiatric drugs do not correct pathological brain states or chemical imbalances but create them (Moncrieff & Cohen, 2005, 2006). These drug-induced states might sometimes prove useful in psychiatric conditions, but the negative aspects of such states are often likely to outweigh the benefits that can be gained. However, drug companies and the psychiatric profession have presented psychiatric drugs as disease-specific treatments that correct chemical imbalances. This view helps to downplay the disadvantages of long-term drug use and may help to create the context for the expansion of markets for psychiatric drugs.

As far as antidepressants are concerned, there is little evidence that they have specific antidepressant effects (Moncrieff & Cohen, 2006) or that they are 'life-saving' in terms of reducing suicide (Moncrieff & Kirsch, 2005). There is no evidence that there is a chemical imbalance in people with depression, and I do not understand how we can be justified in persuading patients to see their problems in this way. Doing so runs the risk of undermining patients' own coping mechanisms and thereby increasing chronicity, dependence on services and use of prescribed drugs.

Declaration of interest

I am co-chairperson of the Critical Psychiatry Network.

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Initial rate of improvement in major depression

Dr Mitchell (2006) suggests that it may be pertinent to re-examine another commonly quoted recommendation – that an antidepressant trial must be at least 6 to 8 weeks before switching drugs. The evidence on which switch guidelines are based is weak but these guidelines are applied frequently in daily clinical practice. In previous studies symptom improvement at earlier time points in relation to *response* has been investigated (e.g. Koran *et al*, 1995) but the ultimate goal of depression treatment is complete *remission*. Remission takes longer than 4–6 weeks to achieve but substantial improvement is unlikely after 10–12 weeks (Trivedi *et al*, 2006). Quitkin *et al* (2003) investigated the relationship between initial change in symptoms and remission by week 12 and demonstrated that even when there was no improvement after 6 weeks of treatment, an antidepressant trial should be continued because the proportion of patients attaining remission by week 12 was still considerable (i.e. greater than 30%). They argued that a switch of antidepressant medication would be unlikely to have resulted in higher remission rates. Furthermore, large studies are required in which change in symptoms is frequently measured at uniform time-points and dimensions other than those measured by conventional questionnaires for depression are assessed. These might be more sensitive to early change following the initiation of antidepressant treatment (Harmer *et al*, 2004), and therefore might better predict which patients will attain remission. Calculation of the sensitivity, specificity, area under the receiver operating characteristic curve, and positive and negative predictive power to assess the likelihood of remission for various levels of symptom change at different time-points would help clinicians to decide on clinical applicability. Results from such studies will improve the evidence on which switch guidelines are based.

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Author's reply: I agree that the evidence base for strategies for treatment-resistant