

Correspondence

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General psychiatry in no-man's land

Sir: I think Deahl & Turner (1997) have been courageous in expressing their views on the current state of general psychiatry, while the majority of the profession seems to have passively, if reluctantly, acquiesced to the recent changes. I would like to add some comments regarding these developments.

The supervision register seems to be a good idea in theory, but to succeed at least two issues need to be clarified, which the Department of Health (Heywood, 1996) have avoided doing. These are, exclusion criteria for the register and what happens if patients are unsupervisable. The most obvious exclusion seems to be people with personality disorders and frequent self-harmers in whom the crux of therapy is the patient acknowledging responsibility for his actions and the task for the therapist is developing a relationship in which he or she enables the patient to take this responsibility. Putting a patient on a register gives a conflicting message regarding responsibility and probably represents an insecure therapist acting in an over-controlling manner, which may ultimately prove counter-productive. The most common scenario of a patient being unsupervisable is a young person who has recently recovered from a psychotic illness in which drugs were involved and is now non-compliant and associates with a criminal/drug subculture. In such cases patients should be assessed for detention under the Mental Health Act, and if not detainable, relevant persons in his or her care informed. Maintaining these patients on a register would seem to be a futile and anxiety-provoking exercise.

I would also like to comment on the recent preoccupations with risk assessment and Health of the Nation Outcome Scales (HoNOS) (Wing *et al.*, 1996), which in

themselves are useful concepts, but are being used uncritically and indiscriminately. Some services have used these criteria to make priorities, resulting in curious anomalies. Young persons who self-harm, are violent, misuse drugs or have relationship difficulties and have high HoNOS scores are being targeted for services; precisely the group who do not benefit from interventions (Van der Sande *et al.*, 1997). On the other hand, patients who have stable homes and supportive relationships are put on long waiting lists and have restricted access to services.

In any system the public, politicians and purchasers have their own agenda on the provision of services and it is the duty of professionals to inform the debate to ensure that these expectations are realistic and achievable. However, if unreasonable demands are made on services, they should be opposed to ensure that patients receive appropriate and effective care.

Deahl, M. & Turner, T. (1997) General psychiatry in no man's land. *British Journal of Psychiatry*, **171**, 6–8.

Heywood, P. (1994) *Introduction of Supervision Register for Mentally Ill People*. London: Department of Health.

Van der Sande, R., van Rooijen, L., Buskens, E., et al (1997) Intensive in-patient and community interventions versus routine care after attempted suicide. A randomised controlled intervention study. *British Journal of Psychiatry*, **171**, 35–41.

Wing, J. K., Curtis, R. H. & Beevor, A. (1996) HoNOS: Health of the Nation Outcome Scales. *Report on Research and Development July 1993–December 1995*. London: Royal College of Psychiatrists Research Unit.

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Evidence-based psychiatry: which evidence to believe?

Sir: The recent article on lithium and the evidence of its efficacy (Moncrieff, 1997;

Cookson, 1997) shows the fallibility of the very process of seeking evidence in medicine, especially in psychiatry. It is apparent from the two reviews that the final conclusion not only depends upon who is doing the review and what type of evidence one is looking for, but also in what way one is looking at it. My point is proven by the fact that while looking for lithium's efficacy, Moncrieff (1997) quoted 93 references, Cookson (1997) quoted 72, only 14 of them being common, and they came to completely different conclusions. The obvious question that springs to mind is whose evidence do we believe in? This dilemma is further proven by Post *et al.*'s recent re-evaluation of carbamazepine prophylaxis in bipolar disorder (Post *et al.*, 1997).

Since it is now known that certain conscious or unconscious biases of researchers who tend to want a new treatment to work are more likely to produce misleading results than any research process-related factors (Robert & Kennedy, 1997), a similar argument can be extended to researchers who are reviewing these works. Two other important factors are worth mentioning when one is looking at evidence of efficacy of any intervention. First, it is increasingly being recognised from meta-analytic studies that the more controlled the trials are, the lower are the chances of their results being generalisable to routine clinical practice. Second, one needs to remember that prior to the recognition of the importance of using confidence intervals in published research, many researchers might have unpublished their negative results, the influence of which may have altered the balance of overall efficacy of an intervention.

Contradictory reviews like these show that though most clinicians are being urged to base their clinical practice on the best available evidence and more and more facilities are being made available to the clinician to find that evidence (i.e. Cochrane databases) the final decisions remain on the individual clinicians' strategies of appraising and using the evidence, *irrespective* of what the true evidence may be. Does this then pose a question of the necessity of such an elaborate and time-consuming process? Or is it merely an indication of an inherent human weakness in pursuit of the ultimate truth?

Cookson, J. (1997) Lithium: balancing risks and benefits. *British Journal of Psychiatry*, **171**, 120–124.

Moncrieff, J. (1997) Lithium: evidence reconsidered. *British Journal of Psychiatry*, **171**, 113–119.

Post, R. M., Denicoff, K. D., Frye, M. A., et al (1997) Re-evaluating carbamazepine prophylaxis in bipolar disorder. *British Journal of Psychiatry*, **170**, 202–204.

Robert, G. & Kennedy, P. (1997) Establishing cost-effectiveness of atypical neuroleptics. *British Journal of Psychiatry*, **171**, 103–104.

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Lithium: evidence reconsidered

Sir: In re-evaluating lithium augmentation studies Joanna Moncrieff (1997) quotes our study, but appears not to have read it. She writes “The largest and longest of them [lithium augmentation studies], which followed up 34 patients for three weeks, was negative (Stein & Barnadt, 1988)”, whereas we actually reported on 24 patients followed up for nine weeks and that there were significant lithium augmentation effects at weeks three, six and nine.

The reason we then reported on 24 patients, which was three-quarters of the way through the trial, was the occurrence of the Second British Lithium Congress, which we regarded as an important occasion. Our final and definitive report (Stein & Bernadt, 1993) which Dr Moncrieff did not quote was of 34 patients who over a nine-week trial showed significant benefit from lithium augmentation for treatment-resistant depression. We found that when added to existing tricyclic medication, which was taken in maximally tolerated doses, lithium 750 mg per day had a greater antidepressant effect than low-dose lithium (250 mg per day), which was no better than placebo. Dr Moncrieff also states that our earlier study reported low compliance rates, but neither of our publications mentioned compliance and our impression was of good compliance. The review is also inaccurate in stating that ours is the largest lithium augmentation study in that Katona *et al* (1995) had 61 patients. Another study that should have been quoted is Dinan & Barry (1989) who showed that lithium augmentation was as effective as electroconvulsive therapy in tricyclic non-responders.

Dr Moncrieff is right to draw attention to methodological weaknesses in lithium studies. Because lithium treatment is cheap, research about it has never attracted funding of the scale lavished on the newer

antidepressants and there remain important uncertainties about its clinical use.

Dinan, T. G. & Barry, S. (1989) A comparison of electroconvulsive therapy with a combined lithium and tricyclic combination among depressed tricyclic non-responders. *Acta Psychiatrica Scandinavica*, **80**, 97–100.

Katona, C. L. E., Abou-Saleh, M. T., Harrison, D. A., et al (1995) Placebo-controlled trial of lithium augmentation of fluoxetine and lofepramine. *British Journal of Psychiatry*, **166**, 80–86.

Moncrieff, J. (1997) Lithium: evidence reconsidered. *British Journal of Psychiatry*, **171**, 113–119.

Stein, G. & Bernadt, M. (1988) Double blind trial of lithium carbonate in tricyclic resistant depression. In *Lithium: Inorganic Pharmacology and Psychiatric Use* (ed. N. J. Birch). Oxford: IRL Press.

— & — (1993) Lithium augmentation therapy in tricyclic-resistant depression. A controlled trial using lithium in low and normal doses. *British Journal of Psychiatry*, **162**, 634–640.

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Grey matter correlates of syndromes in schizophrenia

Sir: In a sample of people with schizophrenia Chua *et al* (1997) reported the interesting finding that there was a significant and positive correlation between the disorganisation score and relative volumes of grey matter in medial temporal lobe structures. They mention that this finding “may seem at odds with the reportedly reduced size of this region in schizophrenia. However, our observations were made within a group of schizophrenics, and reflected differences among schizophrenics rather than differences between schizophrenics and controls”. They proceeded to discuss reasons why increased volume of grey matter in medial temporal lobe structures might result from some pathological process in schizophrenia, for example, lack of neuronal pruning.

There is another possible interpretation which may be somewhat more parsimonious. It is possible that the finding reflects normal volume of medial temporal lobe grey matter in patients with the disorganisation syndrome, and abnormal reductions of temporal lobe grey matter in other people with schizophrenia. This interpretation is consistent with many studies which have found that the volume of medial temporal lobe grey matter is abnormally reduced in schizophrenia (Suddath *et al*, 1990; Breier *et al*,

1992; Rossi *et al*, 1994; Fukuzako *et al*, 1996). It is also consistent with a previous report of an association between reduced mesiotemporal tissue volume and higher ratings of the Brief Psychiatric Rating Scale psychosis factor (Bogerts *et al*, 1993).

Hypotheses based on these alternative interpretations could be tested in future studies by including a normal comparison group.

Bogerts, B., Lieberman, J. A., Ashtari, M., et al (1993) Hippocampus–amygdala volumes and psychopathology in chronic schizophrenia. *Biological Psychiatry*, **33**, 236–246.

Breier, A., Buchanan, R., Elkashef, A., et al (1992) Brain morphology and schizophrenia. A magnetic resonance imaging study of limbic, prefrontal cortex, and caudate structures. *Archives of General Psychiatry*, **49**, 921–926.

Chua, S. E., Wright, I. C. & Poline, J. B. (1997) Grey matter correlates of syndromes in schizophrenia. A semi-automated analysis of structural magnetic resonance images. *British Journal of Psychiatry*, **170**, 406–410.

Fukuzako, H., Fukuzako, T., Hashiguchi, T., et al (1996) Reduction in hippocampal formation volume is caused mainly by its shortening in chronic schizophrenia: assessment by MRI. *Biological Psychiatry*, **39**, 398–945.

Rossi, A., Stratta, P., Mancini, F., et al (1994) Magnetic resonance imaging findings of amygdala–anterior hippocampus shrinkage in male patients with schizophrenia. *Psychiatry Research*, **52**, 43–53.

Suddath, R. L., Christison, G. W., Torrey, E. F., et al (1990) Anatomical abnormalities in the brains of monozygotic twins discordant for schizophrenia. *New England Journal of Medicine*, **322**, 789–794.

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Comorbidity of mental disorders with substance misuse

Sir: A recent editorial in the *Journal* on comorbidity of mental disorders with substance misuse proposed that staff in addiction services should be trained to identify anxiety and affective disorders in their clientele (Hall & Farrell, 1997). A third of persons with an alcohol use disorder (37%) have another mental disorder and half of those with other drug use disorders have comorbid mental disorder (Regier *et al*, 1990). Purchasers are increasingly using services provided by non-statutory agencies, which are often cheaper than those provided by psychiatric addiction services, in part because there is less emphasis on nursing and psychiatric training within the staff skills mix.