these straitened financial times I am of the opinion that neither private health-insurance funds nor public health budgets should be used for dynamic psychotherapy. They should be used for the other, more effective treatments.

I consider that all psychological treatments should be evaluated as to their efficacy, safety, and cost, then registered, exactly as drug treatments are at present, before being provided by a health service. My position is, therefore, very similar to that proposed by Cochrane (1971), that no new treatments should be introduced into medicine unless they have been shown to be more effective, or as effective but safer and cheaper, than existing treatments. It is, therefore, important to use shrinking health-budgets to support the proven remedies to comfort, relieve, and cure the maximum number of patients, and not to use them to support a sectarian battle between the psychotherapies.

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GAVIN ANDREWS

Clinical Research Unit for Anxiety Disorders
University of New South Wales at St Vincent's Hospital
Sydney, Australia

Treatment resistance in schizophrenia

SIR: Despite major advances in our understanding of the neurobiology of schizophrenia, treatment resistance remains a challenge to the clinicians. Treatments using psychological principles have rarely been subjected to the rigours of controlled trials. Hence we read with interest Tarrier *et al*'s study (*Journal*, April 1993, **162,** 524–532). They reported a significant reduction in psychotic symptoms in drug-resistant schizophrenics treated with cognitive–behavioural methods. They also reported that, at six-month follow-up, five patients (22%) were in complete remission. The method is sound and the findings are highly impressive. However, a few issues are worth considering.

(a) The patient sample was drawn from community settings; it could be argued that they were well enough to be in the community and hence not representative of the truly resistant sample who usually remain in the hospital.

(b) There is a high drop-out and refusal rate. Out of the 48 suitable patients, only 56% continued to post-treatment and 48% to six-month follow-up. Of those who dropped out, two were incoherent and six were readmitted to hospital on account of violent behaviour or affective symptoms. Again, this would suggest that those who remained in the study were less intractable.

(c) Perhaps it would have been interesting to obtain some information about those who dropped out of the study, although we do appreciate the Herculean task involved in such an endeavour.

(d) It has been reported that positive symptoms showed substantial fluctuations despite the patients being on stable psycho-pharmacotherapeutic regimes (Johnston *et al*, *Journal*, January 1987, **150,** 60–64). Hence remission of psychotic symptoms in 22% of patients in this study should be viewed with caution.

(e) The authors mention that the patients were resistant to regular and stable neuroleptic medication, but unfortunately there is no mention of the groups of drugs tried, the dosage, duration, compliance, blood levels, and so on. The definition of drug resistance itself would otherwise be open to criticism.

(f) It would have been interesting to know whether the patients were on medication during the study period.

However, despite these finer points, this study offers hope from therapeutic nihilism and emphasises the need for conducting controlled trials of psychological treatments in schizophrenia.

S. MIRZA

Naas General Hospital
Co. Kildare,
Eire

V. SUNIL BABU

Warley Hospital
Brentwood
Essex
CM14 5HQ

E. VITHAYATHIL

Shenley Hospital
Shenley, Radlett
Hertfordshire
WD7 9HB

AUTHORS' REPLY: We were pleased to note Mirza *et al*'s positive comments about our recent report and we welcome this opportunity to respond to the points that they make, we will respond to these in the order that they are presented.

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(a) Hospital or community tenure is probably more a function of policy than an index of illness severity except in extreme cases.

(b) This is true but it is a general problem with this patient group. Other successful psychosocial interventions, such as family management, also suffer from the difficulty of high attrition rates (see Leff *et al*, *Journal*, January 1989, **154,** 58–66; Tarrier, *Journal*, October 1991, **159,** 475–480), and it is a persistent difficulty with pharmacological and other forms of care. It is quite plausible that the group who remained in treatment were less intractable. However, the important point that Mirza *et al* appear to ignore is that these patients show significant improvements over and above those they were benefiting from medication alone. We were surprised by the magnitude of improvement made by some patients in response to the psychological intervention. This was especially so given the short duration of the intervention – ten sessions over five weeks – and we noted the potential of a longer intervention to produce a greater and more robust improvement.

(c) For obvious reasons extensive information about non-compliers is not easy to obtain, but we are attempting to address the questions of predictors of treatment response and attrition in a current study.

(d) Although fluctuations do occur in psychotic symptoms, there are a number of indications that random fluctuation was not responsible for sustained improvement of the magnitude reported in some of our patients: no such changes were observed in patients in the waiting-period group; patients were recruited into the study because their symptoms had not shown further improvement over the six months before inclusion, that is their symptoms were stable over a significant period of time before the intervention; improvement was generally maintained from post-treatment to follow-up, suggesting a treatment effect rather than random fluctuation; and improvement in symptoms was found to be associated with improved coping (Tarrier *et al*, 1993), again suggesting a treatment effect.

(e) While it is true that extensive data on the medication history of the patients were not collected, we are unconvinced that these data would have added more to our confidence that these patients were experiencing psychotic symptoms despite receiving the optimum pharmacological treatment available.

(f) The patients continued to receive medication over the period of the study and this point is stated in the text.

We have now embarked on a five-year study, funded by The Wellcome Trust, to examine an extended psychological intervention over three months. The intervention includes coping-training, problem-solving, and relapse-prevention strategies, and will be compared with supportive counselling. We intend to recruit patients who experience residual symptoms in the community and patients admitted to hospital for an acute episode. We are especially interested in investigating predictors of response to treatment and drop out.

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NICHOLAS TARRIER
School of Psychiatry and Behavioural Sciences
University Hospital of South Manchester
Manchester M20 8LR

LAWRENCE YUSUPOFF
Hope Hospital
Manchester

Lithium neurotoxicity at normal therapeutic levels

SIR: I read with interest the case reports on lithium neurotoxicity at normal therapeutic levels by Bell *et al* (*Journal*, May 1993, **162,** 689–692).

The authors have described the difficulties related to lithium therapy where (as it is recommended in clinical textbooks) we deal with patients and not laboratory results. As the authors have wisely shown, when we treat individual patients we should always be aware of the possibility of idiosyncratic reactions or 'lithium supersensitivity', as in those described cases of neurotoxicity.

There is, however, one issue I would like to point out: in their discussion the authors state that

> "As mentioned above, a high starting dose is a common factor in previously reported cases. Brain concentrations of lithium have been shown to rise higher than serum levels in both animals and man, reaching their peak 22–26 hours after lithium treatment, and a non-uniform uptake of lithium has been observed in the brain .... It is conceivable that with rapid dosage the 12-hour lithium level bears little relation to the lithium levels in the brain, which continue to rise for the next 12–14 hours, producing neurotoxicity. Intra-erythrocyte levels correlate better with cerebral levels ... ".

Lithium is known to be a highly hydrophilic agent, does not bind to plasma proteins, and crosses the blood–brain barrier with difficulty. The rate of absorption into the brain is apparently proportional to serum lithium concentrations. Brain lithium levels were shown to be even lower than serum concentrations (Gyulai *et al*, 1991; Kato *et al*, 1992).

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