

## Invited commentary on: Cost-effectiveness of current and optimal treatment for schizophrenia<sup>†</sup>

As always, one is lost in admiration over the sheer *chutzpah* with which Gavin Andrews and his team set about tackling unanswerable questions (Andrews *et al*, 2003, this issue). As they admit themselves, they can only do so by making numerous assumptions, and some of these assumptions are highly questionable.

Among the less probable of these are that waiting-list controls can be used as proxies for the untreated course of schizophrenia, and that efficacy reflects effectiveness provided that two corrections are made to the data. These are both false for broadly the same reason: we are assuming comparability where none exists. Patients with acute, florid psychoses are never assigned to waiting lists yet they are more responsive to treatment than patients with psychoses of insidious onset, who may well find themselves on waiting lists if other beds are full. Similarly, all the randomised controlled trials of hospital *v.* community care that have produced efficacy data have excluded many patients – usually those who are suicidal or homicidal, those with psychoses complicated by drug misuse and those with organic features. There is no known way for correcting for either of these severe biases to efficacy data.

Next, is it desirable to impose evidence-based medicine on 100% of the patients we see? It is clearly right that clinicians be aware of the recommendations given for the average patient by evidence-based medicine, but many of our patients are very far from the average. Patients consult clinicians for *patient-based evidence* rather than the authoritarian insistence on the diktats of evidence-based medicine.

The reader of the paper is taken through a complex argument and

impressed by the sophisticated statistical procedures used, but it is not clear to what extent the margins of error of the various assumptions have been taken into account in arriving at the final conclusions.

If recommendations are made that are in fact inaccurate, the risk is that they might be believed by gullible officials in the Department of Health in Canberra (or, worse still, in Quarry House) and used as a stick to beat the mental health services by closing down beds and failing to provide extra resources for the undoubtedly greater burden that would fall on community mental health teams.

How would such a gullible official take action on the basis of the article? By studying Table 3, he might possibly conclude that by doubling the use of atypical antipsychotics and reducing the amount spent on psychiatrists and psychologists – but mainly by the simple expedient of further reductions in short-stay patients and discharging still more long-stay patients (more than a 50% reduction in expenditure here) – it would be possible to have a six-fold increase in expenditure on mental health teams.

The evidence that greater use of atypical antipsychotics would bring about economies in hospital expenditure is partial (Sernyak *et al*, 2001), and so far only applied to clozapine and risperidone (Rosenheck *et al*, 1999; Czernansky *et al*, 2002). It remains arguable that such a great reduction in short-stay care can be made safely in view of the increased prevalence of drug-induced psychoses in many places; also, many of those still undergoing long-stay care are where they are because of the danger they pose to others. Australia has a good record in closing its mental

hospitals and recently was said to have only 1.8 beds/10 000 at risk in mental hospitals: a 50% further reduction would be difficult, but perhaps not impossible, to achieve. The total number of beds in mental hospitals (all lengths of stay) is also less than that in the UK (4.1 *v.* 5.8 per 10 000; World Health Organization, 2001).

One assumption that the authors do not make, but if true would help their case, is that it is quite possible that a policy of non-institutional care may itself greatly reduce the overall disability burden associated with schizophrenia. There is some suggestive evidence that this might be the case but it would be dangerous to assume that it is so until it is demonstrated conclusively (Jones *et al*, 1981; Thornicroft *et al*, 1998).

Of course, Utopian papers spelling out the unthinkable should not be muzzled, but it would be unfortunate if important policy decisions were to be based on quite such slender evidence.

**Andrews, G., Sanderson, K., Corry, J., et al (2003)**  
Cost-effectiveness of current and optimal treatment for schizophrenia. *British Journal of Psychiatry*, **183**, 427–435.

**Czernansky, J. G., Mahmoud, R., Brenner, R. (2002)**  
A comparison of risperidone and haloperidol for the prevention of relapse in patients with schizophrenia. *New England Journal of Medicine*, **346**, 16–22.

**Jones, R., Goldberg, D. P. & Hughes, B. (1981)** A comparison of two different services treating schizophrenia: a cost–benefit approach. *Psychological Medicine*, **10**, 493–505.

**Rosenheck, R., Cramer, J., Allan, E., et al (1999)**  
Cost-effectiveness of clozapine in patients with high and low levels of hospital use. Department of Veterans Affairs Cooperative Study Group on Clozapine in Refractory Schizophrenia. *Archives of General Psychiatry*, **56**, 565–572.

**Sernyak, M. J., Rosenheck, R., Desai, R., et al (2001)**  
Impact of clozapine prescription on inpatient resource utilization. *Journal of Nervous and Mental Disease*, **189**, 766–773.

**Thornicroft, G., Wykes, T., Holloway, F., et al (1998)**  
From efficacy to effectiveness in community mental health services. PRISM Psychosis Study 10. *British Journal of Psychiatry*, **173**, 423–427.

**World Health Organization (2001)** *Atlas: Country Profiles on Mental Health Resources*. Geneva: WHO.

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<sup>†</sup>See pp. 427–435 and editorial, pp. 375–376, this issue.