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EDITED BY LOUISE HOWARD

Comments on Jerusalem syndrome

As the authors of several articles on Jerusalem syndrome (Bar El *et al*, 1991; Witztum & Kalian, 1999), we would like to add our comments to the paper by Bar-El *et al* (2000). If epidemiological data supporting Bar-El *et al*'s typology exist, it is regrettable that they were not presented in their article. To our knowledge, such data have not been found in previous studies (Bar El *et al*, 1991). The psychiatric hospitalisation of tourists in Jerusalem is uncommon (around 50 patients per year, from among almost two million tourists). The condition is much less prominent than problems faced by local services in other major cities (Parshall, 1995; Tannock & Turner, 1995). Contrary to some 'doomsday' predictions, so far, there has been no significant increase in the rate of tourist hospitalisations due to the new millennium. In our view, perhaps Jerusalem syndrome should be regarded as a unique cultural phenomenon because of its overwhelming theatrical characteristics (Witztum & Kalian, 2000). Such dramatic qualities have been reported by various biographers since the establishment of pilgrimage and tourism to the Holy City (Witztum & Kalian, 1999). In view of our accumulated data, Jerusalem should not be regarded as a pathogenic factor, since the morbid ideation of the affected travellers started elsewhere. Jerusalem syndrome should be regarded as an aggravation of a chronic mental illness, and not a transient psychotic episode. The eccentric conduct and bizarre behaviour of these colourful yet mainly psychotic visitors became dramatically overt once they reached the Holy City – a geographical locus containing the *axis mundi* of their religious belief (Turner, 1973).

We would also like to comment on another inaccurate interpretation, relating to Gogol's pilgrimage. It had nothing to do with Jerusalem syndrome. Nikolai Gogol suffered from manic depression, severe hypochondriasis and physical ailments, and he set out to Jerusalem (acts of pilgrimage were widely encouraged in tsarist Russia) hoping to alleviate his long-standing suffering (Witztum *et al*, 2000).

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Effectiveness of intensive treatment in severe mental illness

The criticism of the PRiSM Psychosis Study (Marshall *et al*, 1999; Sashidharan *et al*, 1999) betrays several misconceptions about the nature and philosophy of community mental health teams in the UK. Unlike in the USA, where assertive community treatment (ACT) teams were set up in a desert of community care, any similar teams in the UK have to adjust to working in collaboration with other teams in the area and never aspire to providing a service for an entire catchment area, as Thornicroft *et al* (1999) have emphasised. Both Marshall *et al* and Sashidharan *et al* have failed to note that standard community care has improved enormously in the past 20 years and therefore can compete successfully with formal assertive approaches, including both ACT and intensive case management. Unlike drug/placebo comparisons, in which the effects of placebo are roughly similar whatever the year, complex psychosocial interventions such as those in a mental health service are changing constantly. I can predict with some confidence that the Cochrane review showing such excellent findings with regard to superiority of ACT in randomised controlled trials (Marshall *et al*, 1998) will show steadily decreasing benefits of ACT in future revisions. This is not because ACT has suddenly lost its effectiveness; standard treatments have caught up immensely in the past few years and have done so often by using different approaches to those of the original ACT programmes. The statement of Sashidharan *et al* (1999) that contemporary psychiatric care "continues to be dominated by thinking and practices which have their origin in the last century" is a travesty of the current position and a slur on the reputation and performance of many dedicated community mental health teams across the country. Such teams have cause for congratulation. Even though they are deprived of the resources that are allotted to ACT, particularly the requirement of a case-load of only 8–12 clients per worker, they are undoubtedly effective and may even have a positive effect on reducing suicide and other causes of undetermined death (Tyrer *et al*, 1999). But there is a limit to these benefits and some of those treated assertively may be better cared for in hospital. Sashidharan *et al* find it hard to conceive that intensive case management might increase violence in community settings. Unfortunately, antisocial behaviour in all its forms has been shown to be more prevalent in those with some personality disorders in this type of service than in one in which hospital treatment is given more readily (Gandhi *et al*, 2000) and this could undermine progress towards better community care unless it is acknowledged as a problem.

It is time for the programme of assertive community treatment (PACT) model of

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community treatment to be judged in contemporary settings where, in most developed countries, there is reasonable state-funded community care. Len Stein, Mary Test and their colleagues in Wisconsin have made a great contribution to community care by the introduction of PACT but they should not adopt the reflex argument that all studies that show ACT or intensive care programmes to be less effective in other settings must be failing because they do not apply the PACT model properly. The fact is that PACT is primarily a care philosophy backed by a secondary compendium of interventions that have rarely been tested individually. One of its core features, the case-load of only 8–12 per worker, has recently been shown in a much larger trial than any others to be unimportant in influencing outcome (UK700 Group, 1999). There may be many other elements of PACT that are also unimportant. Stein & Santos (1998) quote our own work (Merson *et al*, 1992) as indicating that ACT works outside the USA. Our service had case-loads of 20–25 per worker, did not have 24-hour cover and had psychiatrists working full-time in the team, all of which invalidates its description as an ACT model.

The other issue that must be taken account of by ACT enthusiasts is the need for ACT teams to have much closer liaison with existing teams when there is already well-established community care. This was never a problem at the time ACT was originally introduced as there was no competition. Now that there is a backbone of community care present in the UK and many other countries, it is really inappropriate for a new assertive team to come along and indulge in its autonomous activities with a small number of clients without establishing formal links with other teams, both in-patient and out-patient, in the relevant areas. I have drawn attention (Tyrer, 1999) to the similarity between ACT and plant succession in alien habitats; ACT is like a specialised plant that does extremely well in conditions that are alien to community care, but as it improves the circumstances for good care it gradually becomes redundant and can be replaced. This does not mean the principles of ACT are abandoned; it is just that the philosophy of seamless transfer between hospital and community is rarely supported by small teams based in the community with no responsibility for services beyond their immediate clients. Such teams and their evangelists should really be more humble before they advise others on how to run a comprehensive mental health service.

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Antidepressant choice to minimise treatment resistance

Malhi & Farmer (1999) comment that in their clinical experience effective therapy for treatment-resistant depression necessitates enhancement of noradrenergic neurotransmission because of the effect this has on the typical symptoms of severe depression, which they speculate is more likely to lead to treatment resistance. It could seem from their letter that they advocate holding noradrenergic antidepressants in reserve for treatment-resistant depression. Such an interpretation would be unfortunate as it is probable that much of what is called treatment resistance results from the use of the wrong antidepressant. Roose *et al* (1994) showed a substantial superiority for nortriptyline over fluoxetine in patients with melancholia. Melancholia is linked to abnormal response to the dexamethasone suppression test (DST) (Carroll *et al*, 1981), and some studies have shown a preferential response to noradrenergic antidepressants in patients with an abnormal DST (Fraser, 1983; Kin *et al*, 1997). The failure of many other studies to replicate that finding is likely to be due to a closer link between DST non-suppression and weight loss or sleep disturbance, than melancholia (Mullen *et al*, 1986). Nevertheless, no study has shown an advantage for a serotonergic antidepressant over a noradrenergic antidepressant in patients with melancholia, psychosis, or DST non-suppression.

In recent years, psychiatrists have been exhorted to avoid the dangers of the older (especially tricyclic) antidepressants in favour of the safer selective serotonin reuptake inhibitors (SSRIs). Their greater safety arises from an absence of cardiotoxicity, a lack of cognitive slowing, and minimal effect on blood pressure. Preferential prescription of an SSRI is justified on the basis that there is no evidence that any antidepressant is consistently any more effective than any other antidepressant in double-blind controlled trials.

Although it has not been conclusively demonstrated that noradrenergic drugs are better than serotonergic drugs for severe or melancholic major depression, there is a definite possibility that they are. More importantly, there is no evidence that they are worse than serotonergic drugs. Clinicians should preferentially prescribe a noradrenergic antidepressant for melancholic depression. Those who do so are likely to experience a decreased frequency of treatment-resistant depression among their patients, just as I have over the past 18 years.

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