Correspondence

EDITED BY LOUISE HOWARD

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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 30 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 idea- tion 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).


M. Kalian, E. Witztum PO Box 53/199, Jerusalem 91531, Israel

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 (Tyers 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
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 outpatient, in the relevant areas. I have drawn attention (Tyre, 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.


P. Tyre Division of Neuroscience and Psychological Medicine, Imperial College School of Medicine, St Mary’s Campus, Paterson Centre, 20 South Wharf Road, London W2 IPD

Antidepressant choice to minimise treatment resistance

Mali & 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.


Kin, N. M., Nair, N. P., Amin, M., et al. (1997) The dexamethasone suppression test and treatment outcome in elderly depressed patients participating in a placebo-controlled multicentre trial involving
Depression and interferon-alpha therapy

In their case report, McAllister-Williams et al (2000) hypothesised that recurrence of major depression following treatment with interferon-alpha (IFNz) is related to its capacity to impair serotonin synthesis by inducing enzymes that degrade tryptophan and they cite in vitro evidence in support of this. We suggest that there are other in vivo biological effects of this treatment, which may explain the association of IFNz with depression.

First, it is possible that the pathogenesis of depressive symptoms following treatment with IFNz is related to disturbance of the hypothalamic–pituitary–adrenal (HPA) axis. Overactivity of the HPA axis occurs commonly in people with major depressive disorder (Dinan, 1994), the rates of over-activity increasing with growing severity of depression. There is evidence to suggest that the effects of antidepressants on mood may be brought about by re-equilibration of the HPA axis (Barden et al, 1995). Exogenous IFNz therapy has been found to increase plasma adrenocorticotropic hormone (ACTH) and serum cortisol in humans (Shimizu et al, 1995). The mechanism, however, does not appear to be a direct one as exogenous IFNz is a polypeptide that does not cross the blood–brain barrier and direct application of IFNz to cultured pituitary cells does not release ACTH. Indirect effects of exogenous IFNz on the HPA axis may occur through activation of endogenous cytokines, specifically interleukin-6 (IL-6) which is known to stimulate release of corticotrophin-releasing factor from the hypothalamus in vitro. Furthermore, increase in serum IL-6 following in vivo IFNz is positively correlated with the IFNz-induced changes in serum cortisol (Shimizu et al, 1995).

Second, the possible effects of IFNz on tryptophan availability to which the authors refer may be a secondary effect of immune system activation. Major depression is associated with an activation of the immune-inflammatory response system, with cell-mediated increases in serum levels of pro-inflammatory cytokines including IL-6. Reduced availability of tryptophan in depression may be a result of this inflammatory response activation (Song et al, 1997). Exogenous IFNz also activates pro-inflammatory cytokines.

Paradigms about the aetiology of major depressive disorder are expanding beyond a narrow monoamine-centred concept. Clearly, stress, either medical or psychological, is important in the aetiology of depression. The major stress axis, the HPA, which is overactive in major depression, is potentily activated by both exogenous and endogenous cytokines.

We suggest, therefore, that these biological pathways are important in the pathophysiology of depression during treatment with IFNz.


E. M. Cassidy, V. O’Keane Beaumont Hospital, PO Box 1297, Beaumont Road, Dublin 9, Republic of Ireland
Assessment and discharge following deliberate self-harm

The paper by Hurry & Storey (2000) raises some important points pertaining to the psychosocial assessment of young people presenting with deliberate self-harm (DSH) to accident and emergency (A&E) departments. It is disconcerting that only 54% of children in the 12–15 age group received a specialist assessment prior to discharge from A&E. Department of Health and Social Security guidelines (1984) state that admission to hospital is desirable in most cases in this age group. It is interesting that the rate of specialist assessment was not dependent upon the existence of on-site psychiatric facilities, which in many cases will be based in the community child and adolescent mental health services (CAMHS). The finding that although senior clinicians believe that admission and subsequent specialist assessment is the rule, in practice nearly half the young people in the 12–15 age group are discharged, highlights the need for good liaison between CAMHS and A&E. As minors, most 12- to 15-year-olds will be accompanied by carers, and will be discharged to their care. It is difficult to envisage a situation where a casualty officer would consider discharging a minor following DSH without the involvement of a responsible carer. In the absence of on-site specialist assessment, and with a favourable short-term risk assessment, a casualty officer may be justified in discharging a young person if he or she can be confident that rapid specialist follow-up has been arranged, and that the carer has given an undertaking to ensure that the young person attends. It is, therefore, important that casualty officers receive training in the assessment of short-term risk following DSH, and in communicating with the families of young people.

Such training, regularly undertaken, is the responsibility of senior clinicians in A&E and their psychiatric colleagues. It should ensure awareness of DSH guidelines and the route to fail-safe follow-up, and address the situation reported by Hurry & Storey (2000) of junior doctors who are believed to be “...ill-equipped to make such assessments adequately ...”, owing to “...lack of experience or lack of concern with the psychological aspects of treatment”.


Hurry & Storey (2000) highlight the relatively low rates of specialist assessment for patients who present at hospital following deliberate self-harm (DSH). One contributing factor not commented upon by the authors may be those patients who leave the accident and emergency (A&E) department prematurely. We surveyed psychiatric presentations to an inner-London A&E department over a four-month period and found that premature discharge was taken by 32% of adult patients following an overdose and 7% of those following other forms of DSH. The majority left before assessment by a casualty officer. A survey of premature discharges from Glasgow Royal Infirmary raised a similar problem (Pennycook et al, 1992).

Identifying the reasons for premature discharge will form the basis of a future audit. Possible factors include ambivalence about seeking help, long waiting times and adverse interactions between staff and patients.

Premature discharge may have repercussions for patients, as well as medico-legal implications for A&E. Local guidelines for A&E staff are being drawn up, to minimise the rate of premature discharge by these patients. For those who do leave, there should be careful documentation of the attendance and an attempt to organise follow-up. This should at least include telephone contact with the general practitioner.


Ambient iodine and lithium-associated clinical hypothyroidism

Johnston & Eagles (1999) report a prevalence of hypothyroidism, in terms of the indication for thyroxine treatment, under long-term lithium therapy which by far exceeds the estimated prevalence of clinical hypothyroidism in the local population (Aberdeen area). Like Kirov (1998), who also retrospectively found a similar prevalence in a lithium-treated cohort from London, they compare their findings with results reported from North America and Sardinia. Whereas the Italian researchers (Bocchetta et al, 1996) did not find an excess of hypothyroidism under long-term lithium therapy, results of studies from the USA and, above all, from Canada are close to those from the UK, suggesting a considerable excess of clinical hypothyroidism under lithium treatment.

The well-known discrepancies in results among studies of lithium’s anti-thyroid effects that have emerged frequently in different parts of the world over 30 years may not only be due to different study designs. In Canada, there is an overabundance of nutritional iodine (Dussault, 1993); Italy is an iodine-deficient country. As in the general population (Laurberg et al, 1998), in patients treated with lithium, ambient iodine seems to play a major role in the manifestation of thyroid failure. Conversely, iodine deficiency may act as a protective factor under lithium therapy. In iodine-deficient Germany, Italy and Spain, an excess of clinical hypothyroidism in patients taking lithium has never been reported, whereas in Canada, six papers from different clinics consistently reporting high prevalence of hypothyroidism under lithium therapy have been published (Leutgeb, 1999). Sorting the studies published on this topic geographically provides a confirmation of the early (case-report-based) assumption by Shopsin et al (1973) of a synergism between iodine and lithium in the manifestation of thyroid failure.

Consequently, in those countries where the World Health Organization’s iodisation programme is gaining ground (Dunn,
1998), psychiatrists should be aware of an increased risk of clinical hypothyroidism in their patients taking lithium.


U. Leutgeb The Practice “Rotmantl” for the Treatment of Affective and Anxiety Disorders, Bayreuther Strasse 15, 95500 Heinersreuth b. Bayreuth, Germany

Consent in mandatory homicide inquiries

Since 1994, an independent inquiry has been required in all cases of homicide by discharged psychiatric patients (Department of Health, 1994) and health authorities have needed to develop local procedures for terms of reference for inquiry teams. Methodological inconsistencies have been highlighted (Buchanan, 1999) but the definition of the process of obtaining consent from the patient involved (to allow the inquiry team access to their medical and other relevant case notes) has been neglected. Issues regarding consent and capacity are assuming ever-increasing importance in clinical practice. Psychiatrists routinely assess this with respect to consent to medication, and rigorous safeguards exist to ensure patients understand their right to withhold or refuse consent. However in the case of homicide inquiries there are neither guidelines nor consensus. According to the terms of reference for homicide inquiries (Department of Health, 1994) it is the responsibility of the health authority to obtain consent. In order do identify current practice, we wrote to 22 health authorities that had commissioned homicide inquiries. Details of the procedures/process used by inquiry teams to obtain consent (and a copy of the actual consent form used) were requested.

Only 11 responses were received, seven providing a copy of the consent form used. These were broadly similar, requesting consent for access to all records (health, social services, probation and housing). Only one reply explicitly stated assessment of capacity. Two authorities did not know how consent was obtained and suggested we contact the psychiatrist on the inquiry team. All respondents included the terms of reference and procedures issued to the inquiry team. None of these mentioned how consent was obtained. A variation in practice for obtaining consent was evident; consent forms were directed through solicitors, prison medical officers and inquiry psychiatrists. Only two consent forms explained that reports would be compiled and published.

Our limited study demonstrates that the important issue of consent appears to have been neglected, which is surprising as inquiry reports rely on full access to medical notes. It is of concern that none of the health authorities could demonstrate adequate procedures for obtaining valid consent. This raises the issue of what patients understand are consenting to when they sign consent forms to release their records to an inquiry. Understanding fully the consequences of an inquiry (some of which is inevitably negative) is a difficult conceptual task. It is, therefore, most important that patients are presented with clear and comprehensive detail (e.g. with sufficient time allowed to consider the information, explanation of the right subsequently to withdraw consent and that the report will be published). The procedure should be conducted in accordance with the British Medical Association (BMA) guidelines; thus, patients must be able to understand and retain the main benefits and possible risks, be shown to believe that information and be capable of weighing-up the information in order to make a choice (BMA & The Law Society, 1995). We recommend that health authorities adopt and expand the BMA guidelines to ensure they obtain informed valid consent.

Although it is not the responsibility of the patient’s current responsible medical officer to assess the capacity of the patient to give consent, we believe it should be good practice to do so. If this procedure is followed, there is a risk of an increasing proportion of patients refusing to consent to the release of confidential information. If no guidelines exist for health authorities in such circumstances, the whole inquiry process might grind to a halt. Finally, we raise the legal spectre that if valid consent cannot be obtained by health authorities, that they may subsequently be accused of breaching patient confidentiality and be open to a legal challenge from patients who have been subjects of homicide inquiries.

The value of continuing mandatory local inquiries is an important debate but before further inquiries are commissioned we propose that issues surrounding the process and extent of consent be better clarified in the interests of both patients and health professionals.


H. Rees Yate Health Centre, 21 West Walk, Yate, Bristol BS37 4AX

A. Lillywhite Fromeside Clinic, Blackberry Hill, Stapleton, Bristol BS16 1ED

Medical roles in mental health review tribunals

Richardson & Machin (2000) draw attention to the dual role imposed on the medical member of mental health review tribunals (MHRTs), and to the fact that, having made a preliminary examination, they are unlikely to come to a tribunal hearing with an open mind as to whether or not the patient should continue to be detained. Having served on a great many tribunals, I can say that tribunal members understand that they must reach their decision on what they read in the reports presented to them,
and what is said, in front of them, by the witnesses (including the patient) at the tribunal hearing. Anything that the patient may have said to the medical member at the preliminary medical examination is not evidence, unless it is reproduced at the hearing, and must not be taken into account.

Medical members, not having heard the views of the responsible medical officer and social worker expressed, have not necessarily formed a view of what the tribunal outcome should be before they arrive at the hearing. Moreover, there is nothing in the tribunal rules that says the medical member should discuss or even reveal what he discovered at the time of his examination of the patient, and on occasion I have refused to do so, as such discussion would clearly influence the other members’ final decision.

The advantages of the preliminary examination are that it assists the medical member to ask the most appropriate questions at the hearing, and gives him or her the opportunity to peruse the clinical notes, which may contain important information not available in the reports. The disadvantage is that the preliminary examination is time-consuming, particularly if there is much travelling involved, and there are insufficient medical members of MHRTs comfortably to cover the work.

Why, 40 years ago, it was thought necessary for the medical member to make a preliminary examination is not clear, but I imagine it was primarily to allow a tribunal member to look at the hospital notes, without ruffling medical feathers. If it were possible for the notes to be made available to the whole tribunal in the half-hour before the hearing, I would have thought that we could dispense with the preliminary medical examination.


A. C. Gibson 73 Canford Cliffs Road, Poole, Dorset BH13 7AH

Possible neuroleptic malignant syndrome with quetiapine

A 20-year-old man with treatment-resistant schizophrenia developed autonomic instability, hyperpyrexia and clouding of consciousness while on quetiapine. At the time he was maintained on 2.4 g sulpiride. The young man had been unwell for four years, initially in prison, the last 18 months in hospital. He suffered from a schizophrenia illness which was both severe and refractory. The situation was complicated by severe extrapyramidal side-effects with many antipsychotics and benign idioleptic neutropenia.

Sulpiride was started in March 1999, the dose in July increased to 2.4 g daily. Quetiapine was added in July 1999, to a dosage of 150 mg b.d. This was increased to 200 mg b.d. at the end of October. Compliance was assured.

In early November the patient developed a tachycardia; therefore, quetiapine was reduced to 150 mg b.d. In late November he was noted to be confused, flushed, tachycardiac (130 beats per minute) and pyrexial (37.4°C). His creatine phosphokinase was 723 IU/l (range 55–120). There had been no other pharmacological interventions for 20 days.

A diagnosis of early neuroleptic malignant syndrome (NMS) was made. All antipsychotic medication was stopped and his physical symptoms resolved over 72 hours.

All antipsychotics can cause NMS (Bazire, 1999). Sulpiride was introduced in the UK in 1983. Twenty-eight cases of NMS with sulpiride have been reported to the Committee on Safety of Medicines, seven cases with sulpiride alone have been published. Quetiapine was introduced in the UK in 1997. Four cases of NMS have been reported to the Committee on Safety of Medicines, one has been published to date (Whalley et al., 1999).

In this case, quetiapine is the more likely causative agent as the patient had been maintained on sulpiride for many months and the onset of symptoms was preceded by a recent change of quetiapine dosage.


A. K. Stanley The Norvic Clinic, St Andrew’s Business Park, Thorpe, Norwich NR7 0HT

J. Hunter Kingfisher House, Hellesdon Hospital, Wensum Meadows, off Hospital Lane, Hellesdon, Norwich NR6 5NB

One hundred years ago

The Medico-Psychological Association

The next general meeting of the Medico-Psychological Association of Great Britain and Ireland has been fixed for Thursday, May 10th, and will be held under the presidency of Dr. J. B. Spence, at 11, Chandos-street, Cavendish-square, W., at 4 P.M. Much interest attaches to Dr. H. Maudsley’s paper on “The New Psychology,” which will take the form of critical remarks on the methods and aims of the new psychology, especially in reference to children and psycho-physical research, for not only is the subject an important one, but it is intimated that several psychologists and others interested in metaphysics will take part in the discussion. Dr. A. W. Campbell has promised to give a microscopic demonstration illustrating the arrangement of nerve fibres and nerve cells in the cerebral cortex of a series of idiots’ brains, and Dr. W. J. Koenig will contribute a paper in English on “Pupillary Anomalies in Paralysed and Non-paralysed Idiot Children and their Relation to Hereditary Syphilis.” On the evening of the day
previous to the general meeting the Educational Committee will meet at 36, Queen Anne-street, while in the forenoon and early afternoon of May 10th other committee meetings will be held at Chandos-street.

In the evening members will dine together at the Café Royal, Regent-street, at 7 p.m. Tickets for the dinner can be obtained (price 7s. 6d., exclusive of wine) from the honorary general secretary.

**Corrigendum**

Psychological model of post-stroke depression – author’s reply, *BJP*, 176, 295–296. Co-authors’ names were omitted from the manuscript of this letter. The authors are: G. Gainotti, Institute of Neurology, Catholic University of Rome, Largo Agostino Gemelli 8, 00168 Rome, Italy; A. Azzoni, Psychiatric Service, Ospedale S. Spirito, Rome, Italy; C. Marra, Institute of Neurology, Catholic University of Rome, Rome, Italy.

**REFERENCE**

*Lancet*, 5 May 1900, i296.

Researched by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey
Antidepressant choice to minimise treatment resistance
A. R. Fraser
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