Correspondence

EDITED BY MATTHEW HOTOFP

Contents ■ NICE guidelines and the treatment of Alzheimer’s disease: evidence-based medicine may be discriminatory ■ PTSD and stillbirth ■ Psychiatric services in developing countries ■ Need for paediatric–psychiatric liaison ■ Cannabis regimes – a response ■ Monthly variation in suicide is still strong in the USA ■ Evolution, biological reductionism and closed minds

NICE guidelines and the treatment of Alzheimer’s disease: evidence-based medicine may be discriminatory

Arshad et al (2001) raise important concerns that UK guidelines for the treatment of Alzheimer’s disease (National Institute for Clinical Excellence (NICE), 2001) may be counterproductive for patients with learning disabilities. Potential for discrimination does not by any means stop here. A particular difficulty they highlight is the central role of the Mini-Mental State Examination (MMSE) instrument in determining treatment ‘eligibility’ and response. Scores on the MMSE are strongly influenced by previous education and cross-cultural validity is poor. The guidelines are, therefore, unhelpful for people with lower educational attainment or for growing numbers of older people from minority ethnic groups in the UK. Comorbid cerebrovascular disease will also be more frequent in people from more disadvantaged backgrounds and, in particular, minority ethnic groups such as African–Caribbean populations (Stewart et al, 1999). This reduces the likelihood of a diagnosis of Alzheimer’s disease and therefore eligibility for anti-cholinesterase treatment according to standard diagnostic criteria (McKhann et al, 1984), despite growing evidence for overlapping pathological processes in dementia (Holmes et al, 1999).

For sub-populations who are under-represented in clinical trial samples (minority ethnic groups, people with lower educational attainment, people with learning disability, people with comorbid cerebrovascular disease), the best that can be hoped for is that a considerably weaker evidence base might emerge some years in the future. By this time large numbers of people may have failed to receive potentially beneficial treatment. The problem does not lie with treatment guidelines themselves but with how they are applied at the level of individuals and services – in particular regarding groups with Alzheimer’s disease for whom a 26-year-old cognitive screen and/or 17-year-old diagnostic criteria are unhelpful. Evidence-based medicine is a noble ideal. However, clinical practice that is restricted to the evidence base may amount to institutionalised discrimination.


R. Stewart Institute of Psychiatry, De Crespigny Park, London SE5 8AF, UK

PTSD and stillbirth

The study by Turton et al (2001) on post-traumatic stress disorder (PTSD) in the pregnancy after stillbirth represents ground-breaking research in this area. It is a welcome addition to the world literature in a hitherto neglected field of enquiry. It is of serious concern, however, that they present their results in such a way as to imply the practice of seeing and holding the dead infant as being related to the development of PTSD in subsequent pregnancies. Of those who had not seen the infant, one (17%) of 14 developed PTSD compared with 12 (26%) of 47 who had (P = 0.26). This is a statistically non-significant correlation and as such no relationship can be assumed.

The current practice of encouraging mothers to see and hold their dead babies was initiated by Lewis’s seminal work (Lewis, 1976, 1979; Lewis & Page, 1978) on the special difficulties of mourning a loss that frequently mothers had never seen and that often led to later psychological difficulties. Although in practice most maternity departments have developed protocols which give parents this opportunity, the nature of this service is extremely variable. Some units have specially trained bereavement midwives who offer support at the time of death and during subsequent pregnancies. Units may provide special suites to allow parents to spend time privately with their dead child. In other units a brief time in a delivery suite may be all the contact they are allowed. Staff may have little or no training in psychological care. Turton et al “presumed supportive management of the stillbirth itself” but do not discuss the nature of the service provided by any of the three centres included in the study. In future studies this is an important confounding variable that should be considered in examining the hypothesis that holding the dead infant following stillbirth is a risk factor for developing PTSD in subsequent pregnancies. What Turton et al assert as a clinical implication is nothing more than an interesting but, as yet, untested hypothesis. It would be a pity if policy-makers gave this research undue emphasis and abandoned current practice hastily. In establishing evidence-based best practice, longer-term outcome, morbidity in partners and views of maternity service users will be important areas of enquiry. It is disappointing that Turton et al have been tempted to emphasise a relationship between clinical practice and outcome that their own results did not demonstrate.


K. F. Lovett Plymouth NHS Primary Care Trust, Westbourne, Scott Hospital, Beacon Park Road, Plymouth PL2 2PF, UK
Turton et al (2001) describe substantial morbidity in pregnant women whose previous pregnancy (or pregnancies) had ended in spontaneous loss after 18 weeks’ gestation. She found that 21% of women reporting stillbirth as a criterion A stressor had post-traumatic stress disorder (PTSD) symptoms at caseness level in the third trimester of the next pregnancy. Turton et al conclude that women are vulnerable to PTSD in the pregnancy following stillbirth.

I take issue with these findings. There are several methodological problems with the study. First, stillbirth is not defined as pregnancy loss after 18 weeks’ gestation. An infant born after the 28th week of gestation who does not breathe at birth or show any other sign of life is termed a stillbirth (Beischer & Mackay, 1988). Hence, by definition, Turton et al have included 41 women (out of their total number of 66 subjects) who have had miscarriages. It would have been better to report foetal loss figures on babies with a birth-weight of <300g, which is current widespread practice. Second, the authors state that 14 out of 66 women did not see their stillborn infants. No reason is given for this. Was this because of the gestational age of the infant (<28 weeks’ gestation)? Third, the use of the term PTSD must be questioned. The authors describe stillbirth as a criterion A stressor. One would therefore expect the onset of PTSD within 6 months of the stillbirth. The authors appear to have ignored this time criterion in making a diagnosis of PTSD (World Health Organization, 1993). Similarly, it is difficult to see how the persistent avoidance criterion (criterion C) was met. None of the subjects avoided pregnancy but became pregnant following stillbirth. What the authors describe are symptoms precipitated by the subsequent pregnancy, with the previous ‘stillbirth’ as a vulnerability or predisposing factor. Perhaps the diagnosis of adjustment disorder would be more appropriate.


J. D. Sheehan University Department of Adult Psychiatry, Mater Misericordiae Hospital, 62–63 Eccles Street, Dublin 7, Republic of Ireland

Author’s response: Legal and operational definitions of stillbirth have changed over time, partly in response to medical advances in pre-term baby care which continue to push back the technical age of viability. UK law currently defines stillbirth as the birth of a dead child after 24 weeks’ gestation (the legal age of viability). Definitions also vary across nations; in Australia for example, the birth of a dead child after the 20th week of pregnancy is described as stillbirth. Clinicians in every country tend to use parents’ experience of their baby’s maturity as a guide and generally recognise a loss in the second half of pregnancy as representing a lost child to the parents. As 18 weeks is the gestational age when mothers typically detect foetal movement, and because mothers deliver in the labour ward rather than the gynaecology ward after this date, we operationally defined any infant born without sign of life after 18 weeks’ gestation as stillborn.

Women whose pregnancy had reached 28 weeks (n=25) were significantly more likely to have seen their dead baby than those whose pregnancy ended before 28 weeks (n=41). However, as the paper reports, there was no significant association between gestational age and PTSD.

We reported both current and lifetime prevalence rates for PTSD. Lifetime diagnosis rates were higher than for current diagnosis, presumably reflecting Dr Sheehan’s point regarding the time criterion. None the less, the high rate of PTSD in the pregnancy following stillbirth compared with 1 year postpartum (birth of healthy baby) does suggest that pregnancy may act as a reactivating stressor, as the paper suggests.

Section C of the PTSD interview identifies seven items associated with ‘persistent avoidance’; subjects are required to have above-threshold scores on four or more to qualify. Avoidance of another pregnancy would be a singularly harsh and absolute criterion to apply, as well as being unrelated to the terms of the assessment interview, which shows close correspondence with DSM–III–R standards (American Psychiatric Association, 1987).


P. Turton University of London Parent Child Research Group, Jenner Wing, St George’s Hospital Medical School, London SW17 0RE, UK

Psychiatric services in developing countries

We read with interest the editorial on community psychiatry in developing countries (Jacob, 2001). Historically, in the West, community psychiatry arose in the context of the deinstitutionalisation movement and anti-psychiatry. In developing countries, however, the impetus for developing community-based care was the lack of universally accessible services. Thus, without any ideological baggage to contend with, the emphasis should be on integrated services rather than an artificial schism between hospital and community psychiatry.

We agree with Jacob that psychiatrists should concentrate on what they know best – the identification and treatment of mental illness. The mental health programmes in many developing countries set lofty goals of primary prevention that cannot succeed unless backed by overall social and economic development. But we take issue with his inclusion of epilepsy as a potential target of community psychiatry. It is the authors’ experience, while working at the Community Psychiatry Unit at Bangalore, India, that this results in the programme becoming a glorified antiepileptic medication clinic.

Jacob’s criticism of vertical mental health programmes ignores the practical reality that there is a limit to what generic health workers can deliver given their commitments to other public health programmes such as immunisation. A practical way of getting around this would be to have mental health workers, based at primary health centres, whose skills are intermediate between community psychiatric nurses and generic health workers. There is also a need to develop simple psycho-social interventions which can be delivered by these workers and draw from the strengths of the family or the local community. Community-based rehabilitation is also a priority area as the prevalent concept of good prognosis of mental disorders in developing countries is being challenged (Mojtabai et al, 2001).

One of the stated goals of community psychiatry is to deliver evidence-based treatments to people with mental disorders (Szmukler & Thornicroft, 2001). It may be heartening for psychiatrists in developing countries to know that the conventional psychotropic medications still remain first-line treatments (Geddes et al, 2001; Barbu & Hotopf, 2001). The challenge is to ensure
that all primary health centres stock essential psychotropic medications and that primary care physicians are trained in the detection and management of common disorders.


G. Ranjith Affective Disorders Unit, Bethlehem Royal Hospital, Beckenham BR3 3BX, UK
Y. Duddu Beechhurst Unit, District General Hospital, Chorley PR7 4PP, UK

Author’s response: Drs Ranjith and Duddu argue that primary health care workers, because of their commitments to physical health needs, are not able to deliver mental health care. While this is partly true, I believe that the accomplishment of programmes that have been successfully integrated into primary care depends upon empowerment of the primary care staff to manage these problems. Physicians, nurses and community health workers in many developing countries, with their limited training, are not confident in managing mental disorders. Changes in the basic curriculum, training of trainers within primary care and ongoing support in fieldwork are necessary for skills to be transferred. The empowerment of primary care staff to tackle mental health problems is mandatory for the success of such programmes. Obstetric and immunisation services in many parts of the developing world have succeeded because of such empowerment and consequent integration into primary care.

The successful treatment of epilepsy in many mental health programmes is because the primary care staff are confident and competent in managing these disorders. The lack of these components in the management of psychoses and depression has resulted in programmes mainly treating subjects with epilepsy. The absence of other programmes for treating seizure disorders in the community would argue for retention of this component within mental health initiatives.

The problems of mental illness are complex, with implications for health care, the economy, and social and cultural practices. The current approaches have not delivered reasonable health care in many parts of the developing world. There are no simple solutions. There is a need for debate to generate new and different initiatives in order to overcome the present inertia. A combination of approaches, which harness the available resources, may be more successful than a single strategy.

K. S. Jacob Department of Psychiatry, Christian Medical College, Vellore 632002, India

Need for paediatric—psychiatric liaison

Bass et al (2001) have recently drawn attention to the insufficient recognition given by clinical services to somatoform disorders. Although the authors explicitly exclude children and adolescents, most of the issues they raise apply equally to the younger age groups.

It has long been known that impairing functional aches and pains unexplained by medical disorders are common in children (Garralda, 1999). As in adults, those associated with chronic widespread pain and persistent fatigue have been shown to be associated with marked functional impairment including school non-attendance, which is substantially higher than in serious chronic paediatric conditions (Rangel et al, 2000). There is considerable continuity with functional symptoms in adulthood and family aggregation of health problems (Garralda, 2000). Although less extensive than in the adult literature, there is evidence for the effectiveness of psychological treatments in children (Garralda, 1999). However, the development of dedicated psychiatric–paediatric liaison services often has low priority, is poorly coordinated and monitored, and the training of paediatric staff in this area is clearly limited.

In line with Bass et al I support the view that young patients with severe forms of somatoform disorders require specialised multi-disciplinary treatment which is not appropriately administered in either a psychiatric or paediatric ward. I would echo the need for a serious joint business case between paediatric and psychiatric providers and general practitioners. Although in itself not sufficient, it might help to increase awareness and action if the Royal College of Psychiatrists were to issue guidelines on the number of paediatric liaison psychiatrists required for a given population and on job specifications.


E. Garralda Imperial College School of Medicine, St Mary’s Campus, Norfolk Place, London W2 IPG, UK

Cannabis regimes – a response

de Zwart & van Laar (2001) provide a thoughtful discussion of our recent article comparing alternative legal regimes for cannabis (MacCoun & Reuter, 2001a). We quite agree that any correlation between a rise in cannabis-selling coffee shops and a rise in cannabis prevalence might be coincidental rather than causal; we said so in our article and highlighted this point in its ‘Limitations’. Our purpose was not to evaluate the Dutch model on its own terms, but to highlight potential risks and benefits of alternative strategies for the USA.

However, we take issue with several points made by de Zwart & van Laar. First, they question the plausibility of our term ‘commercialisation’, noting that since 1991 coffee shops have been subject to criminal prosecution for violations of regulations against advertising. But our article explicitly stated that changes in coffee shop regulation probably reduced commercialisation during the 1990s, and for this reason we explicitly argued that our commercialisation hypothesis was limited to the period 1984–1992. At any rate, this argument confuses formal regulations with their implementation; tourists can attest that cannabis is openly promoted in Amsterdam and other cities, with not-so-veiled
references in newspaper advertisements, posters, postcards and shop signs. (Indeed, one can readily verify this by searching for Dutch coffee shop websites on the World Wide Web.)

Second, de Zwart & van Laar claim that “less than half of cannabis consumers purchase the drug in a coffee shop – the majority obtains it elsewhere . . .”. This statement is apparently based on the Trimbos survey of students. The rule banning minors from coffee shops is difficult to enforce, but one would expect adolescent users to rely less heavily on coffee shops than adult users do. In his intensive longitudinal study of the Amsterdam cannabis market, Jansen (1994: p. 172) claims that the shops account for over 95% of cannabis sales in Amsterdam. In their more recent study of 216 experienced cannabis users in Amsterdam, Cohen & Sas (1998: p. 63) report that 75% of those still using cannabis reported one or more coffee shops as their primary source of cannabis. Given the accessibility of coffee shops in cities and the fact that one can buy enough for a few days (or weeks) each time, there is hardly more reason to make street purchases of cannabis than of instant coffee. But the $5 purchase limit surely facilitates secondary transactions in which coffee shop clients share or provide cannabis for their (sometimes younger) friends.

Third, we agree that coffee shops are much more common in Amsterdam than in small Dutch towns, although various estimates in the 1980s suggest that more than half of all coffee shops were located outside Amsterdam. But the concentration of coffee shop sales in Amsterdam actually strengthens our inference that commercialisation might promote cannabis use. Urbanicity has not been shown to be an important correlate of prevalence rates in the USA. Yet the recent national survey by CEDRO (Abraham et al., 1999) shows that cannabis use was much more prevalent in Amsterdam than in The Netherlands as a whole.

Fourth, de Zwart & van Laar correctly note that school surveys tend to yield higher prevalence estimates than household-based population surveys. Unfortunately, an omitted footnote to our Table 1 obscured the fact that our US source for the “approximately age 18” comparisons was the Monitoring the Future school survey. Trimbos contends that their school survey was specifically designed to facilitate comparisons with that US survey (see Plomp et al., 1991: p. 11).

What may be obscured in this exchange is that we hold a mostly enthusiastic view of Dutch drug policy (MacCoun & Reuter, 2001b). Indeed, we argue that the coffee shop system has produced few measurable social harms and possibly some benefits by reducing the excessive use of police sanctions and by weakening the link to hard drug markets. Still, an alternative model that might meet the same goals with less risk of promotion is the South Australian system that permits home cultivation of small quantities of cannabis.


R. MacCoun Goldman School of Public Policy, University of California at Berkeley, 2607 Hearst Avenue, Berkeley, CA 94720-7320, and RAND Drug Policy Research Center, USA. P. Reuter School of Public Affairs, University of Maryland, and RAND Drug Policy Research Center, USA.

Table I Summary by decade of suicides in USA

<table>
<thead>
<tr>
<th>Year</th>
<th>Mean number of suicides per day</th>
</tr>
</thead>
<tbody>
<tr>
<td>1960s</td>
<td>1970s</td>
</tr>
<tr>
<td>January</td>
<td>54.59</td>
</tr>
<tr>
<td>February</td>
<td>55.61</td>
</tr>
<tr>
<td>March</td>
<td>58.75</td>
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<td>April</td>
<td>60.62</td>
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<tr>
<td>May</td>
<td>59.69</td>
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<tr>
<td>June</td>
<td>57.26</td>
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<tr>
<td>July</td>
<td>56.36</td>
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<tr>
<td>August</td>
<td>56.76</td>
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<tr>
<td>September</td>
<td>56.47</td>
</tr>
<tr>
<td>October</td>
<td>56.47</td>
</tr>
<tr>
<td>November</td>
<td>55.33</td>
</tr>
<tr>
<td>December</td>
<td>53.72</td>
</tr>
</tbody>
</table>

Control for the number of suicides each year. There was no linear trend in the contingency coefficients over the 39-year period (Pearson r = 0.033). The month with the peak number of suicides varied over the 39-year period, but not in any linear fashion (see Table 1 for a summary by decade).


D. Lester The Richard Stockton College of New Jersey, PO Box 195, Jim Leeds Road, Pomona, NJ 08240-0195, USA.

Evolution, biological reductionism and closed minds

Since recently familiarising myself with evolutionary psychology literature, I have been amazed by how frequently scathing criticism is thrown at authors by way of the words ‘biological reductionism’. In a series of letters in the Journal, Rose & Lucas (2001) criticise Abed (2001) for, sure enough, using the words “it is not ‘biologically deterministic’ to claim that . . ..”. This made me chuckle, as Rose declared himself to be a neuroscientist. I fully accept the importance of understanding the neuro-physiology of the brain. However, among the amazing revelations of recent years I
now know that serotonin is involved in depression, obsessive-compulsive disorder, anxiety disorders, aggression, schizophrenia and goodness knows how many other psychological disorders, yet we proudly tell our patients with depression that their illness involves a problem with serotonin. To complicate matters further we now have to tell them that there are countless different versions of serotonin.

‘Biological reductionism’ occurs in all spheres of biology. It is time that this reality was accepted and ceased to be used for devaluing the arguments of those who profess what some others may deem politically incorrect theories. I make a special plea to journal editors, as the frequency of this retort is such that free (and potentially legitimate) speech is at times being compromised.


C. H. Cantor Noosa Junction Specialist Centre, Suite 5, Cnr Eugari Street and Sunshine Beach Road, Noosa Junction, Queensland 4567, Australia

One hundred years ago

General hospitals and mental diseases

Out-patients suffering from mental disorders have been for long treated at St. Thomas’s Hospital and elsewhere. The results of experience have shown that good work has been done in relieving many sufferers and in avoiding the necessity for asylum care in not a few cases. The quarterly court of governors of the Newcastle Infirmary has decided to institute such a department, as reported by the Newcastle Evening Chronicle of Nov. 1st. Dr. G. H. Hume, in bringing forward the motion, pleaded for the fundamental ideas of prevention and cure, and it has been decided that a physician, qualified as the rules require and occupying an appointment in a public asylum in Northumberland or Durham, should attend at the infirmary one day in each week and prescribe for the out-patients placed under his care.

REFERENCE

Lancet, 24 November 1900, 1515.

Researched by Henry Rollin, Emeritus Consultant Psychiatrist, Horton Hospital, Epsom, Surrey

Corrigendum

Specificity and heterogeneity in children’s responses to profound institutional privation. BJP, 179, 97–103. Authorship of this paper should read: M. L. Rutter, J. M. Kreppner and T. G. O’Connor and the English and Romanian Adoptees (ERA) study team.
PTSD and stillbirth
J. D. Sheehan
BJP 2001, 179:368.
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