

culture-bound syndromes should be separate, which is an assertion we disagree with.

Drs Painuly & Chakrabarti's suggestion that there are cases of 'pure' *dhat* also reflects the possibility that there are cases of 'pure' depression. To argue that treatments should reflect the diagnosis is putting the horse before the cart. It is not true to say that neurasthenia does not exist any more. Neurasthenia as a diagnosis exists not only in China but also in France, once again emphasising that idioms of distress do cross cultural boundaries.

Dr Gonjanur misses the point we were making. The semen loss anxiety which led to Kellogg and Graham marketing corn flakes and Graham crackers, respectively, as treatment (for semen loss) has disappeared from the West because of changes in the social, political and economic climate. Why have the symptoms that were widely prevalent and described in the UK, USA and Australia in the 19th century disappeared over time? Dr Shankar seems to argue that Ayurveda is a culture; it is a system of medicine developed at a specific time. It should be left to historians to discern whether Ayurveda reflects the culture or the culture is influenced by Ayurvedic concepts in exactly the same way as Western medical systems reflect or influence Western cultures. We believe that culture-bound syndrome as a nosological category is a colonial invention and deserves to be dumped in the bin of history. We agree that culture plays a key role in how symptoms are allowed and encouraged to be developed and expressed by individuals. However, the role of culture is essential for *all* our patients and not a few selected ones. Everyone has culture.

One of the key factors that the correspondents have chosen not to discuss is the distinction between disease and illness. *Dhat* as a symptom and syndrome reflects illness in the broadest term. The clinicians are trying to place this in a disease category, thereby paying lip service to cultural influences only in the pathological diagnostic sense, not in a broader idiom of distress. Although some acknowledgement is made to the heterogeneity of the syndrome, we believe that cultures themselves are markedly heterogeneous and the clinicians must address not only the cultural values and identity of individuals but also those of the cultural groups to which the individual belongs, and place the expression of distress in its historical and social context. It would appear that our correspondents are arguing

for exemption for a geographical syndrome. It is indeed a pity that Westermeyer & Janca's (1997) argument is not universally accepted in the classificatory and nosological systems as it deserves to be – the exact point we have striven to put across. Culture-bound syndromes have fascinated anthropologists and psychiatrists alike as accounts of strange syndromes, myths and symbols. We urge clinicians to place these symptoms in the context of cultural values and not simply medicalise and pathologise distress that can be dealt with using other models. Another question that deserves to be raised and answered is why *amok* in Malaysia is seen as a culture-bound syndrome but similar behaviour of random shootings and running 'amok' is not seen in this way in the USA? It is time that we gave up the ghost of colonialism and looked at culture-bound syndromes with a new eye. We acknowledge that culture is an important pathogenic and pathoplastic influence but our belief is that culture-bound syndromes are a historical anomaly. *Dhat* as symptom is important but the classification of *dhat* syndrome is problematic.

Sumathipala, A., Siribaddana, S. & Bhugra, D. (2004) Culture bound syndromes: the story of *dhat* syndrome. *British Journal of Psychiatry*, **184**, 200–209.

Westermeyer, J. & Janca, A. (1997) Language, culture and psychopathology: conceptual and methodological issues. *Transcultural Psychiatry*, **34**, 291–311.

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A. Sumathipala Section of Epidemiology, Institute of Psychiatry, London

S. H. Siribaddana Sri Jayawardenapura General Hospital, Nugegoda, Sri Lanka

D. Bhugra Section of Cultural Psychiatry, PO 25, Institute of Psychiatry, London SE5 8AF, UK

Disability after trauma

That post-traumatic stress disorder (PTSD) is not associated with disability and that its clinical importance 'may be questionable' are huge claims and somewhat counter-intuitive. For Neal *et al* (2004) to come to such counterintuitive conclusions would require a very strong piece of research. Unfortunately, since their paper does not seem to offer a sound methodology or results, the force and the validity of their conclusions is debatable.

Six years (on average) after a traumatic event the armed services personnel they study report high levels of disability. From

the paper it *appeared* that the authors thought that the association was not mediated via diagnoses of PTSD, depression or alcohol dependence on statistical grounds (although these disorders were all frequently present in the 70 armed services personnel referred to the PTSD unit). The presence of high scores on the Beck Depression Inventory (BDI) accounted for a 'high proportion of the variability'. In other words, diagnoses did not account for variability but 'depression consequent upon trauma' did. It is difficult to understand how the findings of relatively high disability 6 years (on average) after a trauma associated with depressive scores on the BDI cannot be linked to the diagnoses found at interview, as the conclusion of the abstract section clearly implies.

There are several problems with the study design. There is no control group. All the interviews were conducted by a single individual, a nurse, who was presumably not masked to the origin of the patients. The Structured Clinical Interview for DSM-IV (SCID) is not primarily intended as an instrument to detect PTSD. An easily administered alternative to the SCID might have been the Short Post-traumatic Stress Disorder Rating Interview (SPRINT) (Connor & Davidson, 2001) which has solid psychometric properties. There is perhaps an over-reliance otherwise on self-report questionnaires.

The study also fails to refer to relevant literature. A study published in 2001 by Tucker *et al* recruited 307 patients with PTSD and was both double-blind and placebo-controlled. This found a significant reduction in disability, as measured by the same Sheehan Disability Scale, after 12 weeks of treatment with paroxetine (Tucker *et al*, 2001).

At this point I must state my own interest in that I have written court reports on PTSD, but on joint solicitors' instructions or single solicitor's instructions for claimants or defendants. However, with regard to the authors – although one would like to believe in their independence – surely it is not credible for there to be no declaration of interest stated when all three authors are employed either directly or indirectly by the Ministry of Defence?

Declaration of interest

B.H.G. has written numerous personal injury and clinical negligence medico-legal reports and is Editor of Psychiatry On-Line.

Connor, K. M. & Davidson, J. R. (2001) SPRINT: a brief global assessment of post-traumatic stress disorder. *International Clinical Psychopharmacology*, **16**, 279–284.

Neal, L. A., Green, G. & Turner, M. A. (2004) Post-traumatic stress and disability. *British Journal of Psychiatry*, **184**, 247–250.

Tucker, P., Zaninelli, R., Yehuda, R. et al (2001) Paroxetine in the treatment of chronic posttraumatic stress disorder: results of a placebo-controlled, flexible-dosage trial. *Journal of Clinical Psychiatry*, **62**, 860–868.

B. H. Green University of Liverpool and Cheadle Royal Hospital, 100 Wilmslow Road, Cheadle, Cheshire SK8 3DG, UK

Author's reply: We accept that our findings require confirmation from further studies. However, human intuition has often been shown to be incorrect in the face of scientific research and perhaps ought not to be taken too seriously. A good example of this is the recent history of psychological debriefing to prevent PTSD (Rose *et al*, 2003).

The finding that the categorical measures of depression (according to DSM-IV) did not concur with the continuous measure of depression (according to the BDI), in terms of predicting disability, may be evidence for the unreliability of the way we categorise psychiatric disorder, in terms of individual functioning. This is a possible area for further investigation.

The study was a cross-sectional survey examining the within-subject variability and relationships between variables. It has not been explained how a control group would add anything to the findings. The origin of the subjects was not a variable in the study design and it has not been explained why the assessor should have been masked to this information. The SCID is the most widely used and the most thoroughly researched clinical interview format for PTSD (Wilson & Keane, 1997). The SPRINT is one of numerous other measures of PTSD. A search on the National Center for PTSD database showed 127 hits for the SCID and 3 hits for the SPRINT. The use of self-report questionnaires as continuous variables was integral to study design and was not an 'over-reliance'.

The study by Tucker *et al* (2001) does not tell us anything about the relative contribution of PTSD, depression or alcohol dependence to disability, which was central to our hypothesis. Paroxetine is effective in the treatment of depression as well as PTSD.

Employment by the Ministry of Defence does not introduce an obvious partisan interest in this study. On the one

hand, the Ministry might benefit from showing that PTSD does not cause disability, but on the other hand, if PTSD has little relevance, then the need to employ military psychiatrists may be questionable. Either way, the employing organisation can hardly be said to have been concealed by the authors from Dr Green.

Declaration of interest

At the time of data collection, L.A.N. was employed by the UK Ministry of Defence. At the time of submission of the publication, he had no links with the Ministry.

Rose, S., Bisson, J. I. & Wessely, S. C. (2003) A systematic review of single-session psychological interventions ('debriefing') following trauma. *Psychotherapy and Psychosomatics*, **72**, 176–184.

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Wilson, P.W. & Keane, T. M. (1997) *Assessing Psychological Trauma and PTSD*. London: Guilford Press.

L. A. Neal King's College, London and the Institute of Psychiatry and The Priory Hospital, Heath House Lane, Stapleton, Bristol BS16 1EQ, UK

Analysis of psychiatric in-patient violence

There are two major problems with the study of Gudjonsson *et al* (2004) which render interpretation of the results problematic. First, violent incidents were identified from untoward incident reporting forms. Formally evaluated scales measuring violent behaviour were not used. It is well recognised that nursing staff may underreport violent behaviour on incident forms (Shah *et al*, 1991). Moreover, there is no mention of incident forms other than those for untoward incidents. In some hospitals there are different types of incident forms (including that for untoward incidents) depending how the incident is classified. Furthermore, no data are provided on the exact number of staff completing these forms and reliability between different raters in the reporting of violent incidents. The second concern is with the definition of ethnicity. The authors provide no information on how ethnicity was defined. It was simply ascertained from the record of ethnicity on the untoward incident form. There are many problems with the definition of ethnicity. Unless ethnicity is clearly defined and all those completing the incident forms use the same

definition, this is likely to introduce bias in the findings. Again, no data are provided on how many staff completed the incident forms and the reliability in the reporting of ethnicity between different staff members. These issues are important because findings on psychiatric issues and ethnicity are often considered to be controversial and emotive to all sectors of society.

Gudjonsson, G. H., Rabe-Hesketh, S. & Szmukler, G. (2004) Management of psychiatric in-patient violence: patient ethnicity and use of medication, restraint and seclusion. *British Journal of Psychiatry*, **184**, 258–262.

Shah, A. K., Fineberg, N. A. & James, D. V. (1991) Violence among psychiatric inpatients. *Acta Psychiatrica Scandinavica*, **84**, 304–309.

A. Shah West London Mental Health NHS Trust, John Conolly Wing, Uxbridge Road, Southall UBI 3EU, UK

Authors' reply: The two main concerns of Dr Shah relate to the standard hospital incident form used in the study and lack of definition of ethnicity. We accept the potential methodological problems associated with the use and retrospective analyses of routine 'untoward' incident data.

However, we do have two specific comments on Dr Shah's letter. First, our study was a large-scale investigation involving 1515 'untoward' incidents on 14 general wards within our trust over a 3-year period. In view of the large number of incidents analysed we believe it is unlikely that unrecorded incidents or inaccurately recorded ethnic background of some patients would have significantly influenced the findings. In the statistical analyses the patients were only classified into two groups: 'White' and 'Black' (i.e. 'African-Caribbean' and 'African'). Second, the main findings were broadly similar to those of a previous large-scale study of 165 medium secure unit patients at the Bethlem Royal Hospital (Gudjonsson *et al*, 2000). In that study the ethnic background of the patients was obtained from the patient register rather than from the incident forms (Gudjonsson *et al*, 1999).

Gudjonsson, G. H., Rabe-Hesketh, S. & Wilson, C. (1999) Violent incidents on a medium secure unit over a 17-year period. *Journal of Forensic Psychiatry*, **10**, 249–263.

Gudjonsson, G. H., Rabe-Hesketh, S. & Wilson, C. (2000) Violent incidents in a psychiatric hospital. The target of assault and the management of incidents. *Journal of Forensic Psychiatry*, **11**, 105–118.

G. Gudjonsson, G. Szmukler Department of Psychology, Institute of Psychiatry, De Crespigny Park, Denmark Hill, London SE5 8AF, UK