

Correspondence

EDITED BY KHALIDA ISMAIL

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The continuing story of *dhat* syndrome

Sumathipala *et al* (2004) raise an important argument regarding culture-bound syndromes. We agree that *dhat*, although considered an exotic neurosis of the orient, is a symptom that has been observed worldwide. The use of the term 'syndrome' cannot be justified in this case. We see it more as a mere symptom of common disorders like depression, anxiety and somatisation. Mumford (1996) argued that *dhat* should be primarily regarded not as the focus of a culture-bound syndrome but as a culturally determined symptom associated with depression.

It has been stated by Kuruppuarachchi & Williams (2001) that in Sri Lanka somatic complaints override the more recognised presenting symptoms appearing in the diagnostic criteria for conditions such as depression. The language used facilitates this as the vocabulary does not promote expression of symptoms like low mood.

It is our belief that 'semen loss anxiety' too is a form of communicating distress seen in those with these conditions.

We would also like to highlight the fact that the current classification systems such as ICD-10 (World Health Organization, 1992) and DSM-IV (American Psychiatric Association, 1994) do not give clear operational guidelines to come to a proper diagnosis of culture-bound syndromes. As a result, many clinicians tend to make a diagnosis based on arbitrary guidelines.

In Sri Lanka clinicians come across many patients presenting with '*dhat* syndrome'. However, on further exploration one of the common neurotic and depressive disorders can be recognised as the cause.

The belief that seminal fluid is precious is still largely prevalent. It is considered by many lay people that 100 drops of blood make one drop of semen. Buddhism, while discouraging promiscuity, does not proscribe sexual activity. However, the

erroneous belief that all sexual activity is sinful is widespread among Buddhists. The anxiety regarding loss of semen may be related to this belief.

Dhat is also promoted by practitioners of alternative and complementary medicine. This is obvious in many advertisements appearing in the newspapers boasting different forms of cures for loss of semen.

American Psychiatric Association (1994) *Diagnostic and Statistical Manual of Mental Disorders* (4th edn) (DSM-IV). Washington, DC: APA.

Kuruppuarachchi, K. A. L. A. & Williams, S. S. (2001) Cross-cultural psychiatric interviews and research instruments (letter). *British Journal of Psychiatry*, **179**, 461.

Mumford, D. B. (1996) The 'Dhat syndrome': a culturally determined symptom of depression? *Acta Psychiatrica Scandinavica*, **94**, 163–167.

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

World Health Organization (1992) *ICD-10 Classification of Mental and Behavioural Disorders*. Geneva: WHO.

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In their comprehensive review of *dhat*, Sumathipala and colleagues (2004) have made some interesting observations on the syndrome being culture-related rather than culture-bound. Their suggestion that the label 'culture-bound' may exclude such syndromes from mainstream psychiatric classifications and hamper their understanding is also pertinent. However, we believe there are certain issues beyond the label which are as yet unresolved. As mentioned in the review, such syndromes cut across diagnostic categories, and it may be particularly difficult to classify a high proportion of these cases, for example of 'pure' *dhat* (Chadda & Ahuja, 1990; Bhatia & Malik, 1991). The other problems with

ubiquitous presentations such as *dhat*, which also have a great degree of cultural sanction, is the blurring of boundaries between normal and pathological that complicates the diagnostic process. The authors' contention that multi-axial classifications with due importance to cultural factors will obviate the necessity of such diagnoses has yet to be tested. For example, primary-care physicians are often the first port of call for most of these patients; how familiar can such doctors be expected to be with culturally sensitive diagnostic formulations? Diagnostic issues apart, the nature of treatment to be offered still remains uncertain, given that most do not seem to feel the need for any psychiatric treatment (Malhotra & Wig, 1975). High drop-out rates from psychiatric clinics also indicate dissatisfaction with whatever is done in terms of treatment or causal explanations (Chadda & Ahuja, 1990). Finally, the prediction that with industrialisation/urbanisation *dhat* will vanish from the East as it has done in the West might not turn out to be true. Instead, *dhat* might persist and be labelled differently, as has happened with neurasthenia and chronic fatigue syndrome. Both conditions have been considered medical illnesses, underlying stress being the presumed cause, acting either via depletion of nervous energy (neurasthenia) or via immune dysfunction (chronic fatigue). However, neurasthenia, a very common diagnosis at one time, is hardly encountered any more (Abbey & Garfinkel, 1991).

Thus, although incorporating 'culture-bound' syndromes in mainstream nosology seems to be an ideal solution for the future, abandoning such categories may be premature at present.

Abbey, S. E. & Garfinkel, P. E. (1991) Neurasthenia and chronic fatigue syndrome: the role of culture in the making of diagnosis. *American Journal of Psychiatry*, **148**, 1638–1646.

Bhatia, M. S. & Malik, S. C. (1991) *Dhat* syndrome. A useful diagnostic entity in Indian culture. *British Journal of Psychiatry*, **159**, 691–695.

Chadda, R. K. & Ahuja, N. (1990) *Dhat* syndrome: a sex neurosis of the Indian subcontinent. *British Journal of Psychiatry*, **156**, 577–579.

Malhotra, H. K. & Wig, N. N. (1975) *Dhat* syndrome: a culture bound sex neurosis of the orient. *Archives of Sexual Behaviour*, **4**, 519–528.

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

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I read with interest the article by Sumathipala *et al* (2004) – an excellent review on *dhat* syndrome, a clinical entity highly prevalent in the Asian continent and not considered an entity in the Western world. This article is not free from publication bias. I wish to make the following observations based on our work in different parts of India over a period of 15 years.

Dhat syndrome, a concept developed from Sanskrit literature, is based on a cultural belief in people who live in the Indian subcontinent. The syndrome is highly prevalent not only in India but also in its neighbouring countries such as Pakistan, Nepal, Burma (Myanmar), Sri Lanka and others. It is more prevalent among men in early adulthood, starting in late adolescence. Patients present with multiple somatic and psychological symptoms in the background of loss of semen. Surprisingly, patients have their first contact with departments other than psychiatry, for example urology, dermatology and general medicine, and are then referred to psychiatry.

We presented our first observations from northern India on *dhat* syndrome from patients presenting with weakness, anxiety symptoms with sexual difficulties such as premature ejaculation and impotence (Behere & Nataraj, 1984). In further work by myself and others in the southern part of India, we were able to observe that the belief underlying *dhat* syndrome had a dimensional impact in clinical practice. While it was common to find anxiety and phobic symptoms, it was also extended to encompass hypochondriacal, obsessive and body dysmorphic symptoms. Affective symptoms were also common. Uncommonly, some patients presented with delusional beliefs. Thus, from a clinical perspective, the symptoms in *dhat* syndrome may cluster to give a spectrum of diagnostic possibilities ranging from anxiety to somatoform disorders, affective disorders and, rarely, psychosexual delusional disorder (further details available from the author on request).

This multiplicity of clinical presentation makes it difficult to classify *dhat* syndrome purely as neurotic. We question the validity of *dhat* syndrome being incorporated as a single neurotic disorder in ICD-10, where it is included under 'other specific neurotic disorders' (F48.8; World Health Organization, 1992). No single diagnosis encompasses the clinical presentation of *dhat* syndrome; the presentation

of symptoms needs to be seen from a clinical perspective rather than viewing it as a neurotic disorder alone. This might help to formulate the management comprehensively on a biopsychosocial model depending upon its clinical presentation.

Behere, P. B. & Nataraj, G. S. (1984) *Dhat* syndrome: the phenomenology of a culture bound sex neurosis of the orient. *Indian Journal of Psychiatry*, **26**, 76–78.

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

World Health Organization (1992) *International Classification of Diseases and Related Health Problems* (ICD-10). Geneva: WHO.

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I read Sumathipala *et al*'s (2004) review on *dhat* syndrome with interest. The authors' contention is that *dhat* syndrome is not culture-bound. My argument is although *dhat* is globally prevalent, the specificity of the culture (Ayurvedic concept) and certain psychosocial features being pathogenic in the development of *dhat* syndrome in the south Asian context cannot be ignored and the essence of the cultural perspective of 'semen loss anxiety' in different geographical areas has been misunderstood.

According to the traditional Indian Ayurvedic system of medicine, genital secretions are considered a highly purified form of *dhatu*, or bodily substance, and loss of this precious substance is thought to result in progressive weakness or even death. In south Asia, the complaint of loss of genital secretions is regarded with concern by both men and women. The cultural and biomedical meanings of the complaint of leucorrhoea in south Asian women (Karen, 2001) demonstrate that the complaint of vaginal discharge accompanied by a host of somatic symptoms could not fit a particular biomedical diagnostic category, and is understood within the ethno-medical context of Ayurveda.

As noted by Malhotra & Wig (1975), Asian culture condemns all types of orgasm because they involve semen loss and are therefore 'dangerous'. In contrast, the Judaeo-Christian cultures of the 18th and 19th centuries in Europe considered most types of sexual activities outside marriage to be 'sinful'.

The so-called culture-bound syndromes have been the focus of the debate between adherents of biopsychological universalism (universal human psychopathology) and adherents of an ethnological cultural relativism (typical aspects of a particular culture). Culture-bound syndrome is not always bound (Westermeyer & Janca, 1997) but heavily related to certain cultural traits or cultural factors that can be found in different geographical areas, or across ethnicity or cultural units or systems, which share the common cultural view, attitude or elements attributed to the formation of the specific syndromes. Based on this new understanding, the term should be changed to 'culture-related specific syndrome' to reflect its nature accurately (Tseng & McDermott, 1981).

Karen, T.-K. (2001) Cultural and biomedical meanings of the complaint of leukorrhoea in South Asian women. *Tropical Medicine and International Health*, **6**, 260–266.

Malhotra, H. K. & Wig, N. N. (1975) *Dhat* syndrome: a culture-bound sex neurosis of the orient. *Archives of Sexual Behavior*, **4**, 519–528.

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

Tseng, W. S. & McDermott, J. F., Jr (1981) *Culture, Mind and Therapy: An Introduction to Cultural Psychiatry*. New York: Brunner/Mazel.

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

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Authors' reply: We are delighted to note the varying and huge response to our paper (Sumathipala *et al*, 2004). It is interesting to note that most of the comments are from the Indian subcontinent where the *dhat* syndrome is prevalent.

Drs Kurupparachchi & Wijeratne point out that semen loss anxiety is a form of communicating distress. We agree, but our conjecture is that male preoccupation with semen loss has been universal and we need to place the related depression and anxiety in the specific context. Our contention with which Kurupparachchi and Wijeratne agree is that ICD-10 and DSM-IV-TR are culturally influenced classificatory systems. Wig's (1994) suggestion that culture-bound syndromes should be integrated into existing rubrics of psychiatric classification is an appropriate one. Most of the correspondents feel that

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.

Wig, N. N. (1994) An overview of cross-cultural and national issues in psychiatric classification. In *Psychiatric Diagnosis* (eds J. Mezzich, Y. Honda & M. Kastrup), pp. 3–10. New York: Springer.

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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.

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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.

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.

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

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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 UB1 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.

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Analysing the efficacy of clozapine

It is interesting to note that while all the participants in the debate on clozapine *v.* typical neuroleptics referred to Kristian Wahlbeck's meta-analysis (Wahlbeck *et al.*, 2000), none of them referred to her sub-analysis of her own earlier meta-analysis on this topic (Wahlbeck & Adams, 1999). In this subanalysis, all randomised trials comparing clozapine with typical neuroleptic medication for schizophrenia were divided into sponsored (reporting some kind of connection with manufacturers of clozapine) and non-sponsored trials. Odds ratios and 95% confidence intervals were calculated for the primary outcomes of relapse, clinical improvement, and leaving the study early, separately for sponsored and non-sponsored studies. Odds of relapsing were significantly in favour of clozapine in the sponsored trials (OR=0.5, 95% CI 0.3–0.7). Non-sponsored studies reported equivocal findings (OR=0.4, 95% CI 0.1–1.4). Similarly, sponsored studies showed a significant difference in favour of clozapine on the outcome measure of leaving the study early (OR=0.5, 95% CI 0.4–0.7). Non-sponsored studies showed a non-significant difference (OR=0.6, CI 0.3–1.2). Only on the outcome measure of improvement did both sponsored and non-sponsored studies show a significant benefit of clozapine over older antipsychotics. Wahlbeck suggested that those undertaking meta-analysis of drug treatment should investigate for sponsorship bias by using sensitivity analysis.

Outside of psychiatry, similar associations between sponsorship and outcome of trials has been demonstrated in randomised controlled trials (RCTs) published in five general medical journals (Davidson, 1986; Yaphe *et al.*, 2001), RCTs of non-steroidal anti-inflammatory drugs in the treatment of arthritis (Rochon *et al.*, 1994)

and RCTs published in the *BMJ* over 4½ years (Kjaergard & Als-Nielsen, 2002).

Although RCTs and meta-analyses have contributed greatly to increasing our knowledge base about which treatments work and which do not, maybe it is time we began to consider other factors that might explain the observed difference between two treatments in RCTs and meta-analyses, beyond the standard critical appraisal questions. Maybe we need to ask not only how the efficacy of clozapine (or any other drug for that matter) has been analysed but also who has analysed it.

Davidson, R. A. (1986) Source of funding and outcome of clinical trials. *Journal of General Internal Medicine*, **1**, 155–158.

Kjaergard, L. L. & Als-Nielsen, B. (2002) Association between competing interests and authors' conclusions: epidemiological study of randomised clinical trials published in the *BMJ*. *BMJ*, **325**, 249–253.

Rochon, P. A., Gurwitz, J. H., Simms, R., et al (1994) A study of manufacturer-supported trials of nonsteroidal anti-inflammatory drugs in the treatment of arthritis. *Archives of Internal Medicine*, **154**, 157–163.

Wahlbeck, K. & Adams, C. (1999) Beyond conflict of interest. Sponsored drug trials show more favourable outcomes (letter). *BMJ*, **318**, 465.

Wahlbeck, K., Cheine, M., Essali, A., et al (2000) Clozapine versus typical neuroleptic medication for schizophrenia. Cochrane Library, issue 3. Oxford: Update Software.

Yaphe, J., Richard, E., Knishkowsky, B., et al (2001) The association between funding by commercial interests and study outcome in randomized controlled drug trials. *Family Practice*, **18**, 565–568.

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Case reports still valuable

I read with interest Dr Bourne's claims against the *Journal's* Editor. These claims are only partially right. The reason the *Journal* is considered one of the most prestigious in the field of psychiatry is because

of the editorial policy of encouraging and accepting novel research that strives to the highest scientific and medical levels. Sound research is the basis of all leading medical journals, and this one is no different. This research is the foundation of progress in psychiatry. The fruits of this research are to our benefit as well as that of our patients. Think of the effect of psychotropic drugs in the 1950s and their side-effects and compare them with new, state of the art medication. Psychiatry is a living and developing field which must obtain new and original research at all times in order to be relevant to medicine.

However, the days of case studies are far from over. Every leading medical journal has a section for case studies. The importance of case reports is highlighted by the reporting of a novel mental disorder or medical condition that catches the attention of the medical community, such as concentration camp syndrome (Eitinger, 1961) and severe acute respiratory syndrome (Zambon & Nicholson, 2003) – both good examples of case reports that had an impact on the fields of psychiatry and medicine.

Instead of taking sides in this clash, it would be advisable to introduce a small section for case studies where clinicians could share important insights about patients or unusual cases. This section would also be beneficial to research by stimulating new ideas.

Bourne, H. (2004) A new name for the *Journal*? (letter) *British Journal of Psychiatry*, **184**, 455.

Eitinger, L. (1961) Pathology of the concentration camp syndrome. *Archives of General Psychiatry*, **5**, 371–379.

Zambon, M. & Nicholson, K. G. (2003) Sudden acute respiratory syndrome. *BMJ*, **326**, 669–670.

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One hundred years ago

Asylum reports

London County Asylum, Bexley Heath (Report for the year ending March 31st,

1904). – The average number of patients resident during the year was 2085, comprising 1012 males and 1073 females. The admissions during the year amounted

to 585 – viz., 282 males and 303 females. Of these, 528 were first admissions. Dr. T. E. K. Stansfield, the medical superintendent, states in his report that “the hopeless

character as to the possibility of recovery of the bulk of the admissions during the year is clearly demonstrated.... The axiom the sooner the disease can be brought under treatment the greater hope there is of recovery applies more to mental alienation perhaps than to any other form of disease." Only 30 per cent. of cases came under treatment within three months of the beginning of the attack, owing to which, adds Dr. Stansfield, "not only is the possibility of recovery greatly diminished but when recovery does take place the permanent injury present in every case is proportionately greater. The importance of *habit* in relation to mental diseases is but little appreciated and yet to my mind it is one of the most powerful factors in the development of chronic insanity and it remains after the first causes have passed away, as is well illustrated by the bulk of cases in every asylum. All this points to the urgent necessity of patients being bought [*sic*] under proper observation and treatment at the earliest period of their alienation before

insane habits of thought and action become fixed." Melancholia was present in 23 per cent., paranoia and delusional insanity in 14 per cent., primary dementia in 13.5 per cent., and general paralysis in 6 per cent. of the admissions. Among the causes of insanity were arteriosclerosis, senile decay, alcoholic intemperance, insane parentage, and syphilis, in descending order of frequency. "The important part played by heredity in the causation of mental disease was demonstrated by the fact that insane heredity was made out in 132 cases or 22.5 per cent." The number of cases discharged as recovered during the year amounted to 162 – viz., 56 males and 106 females, or 7.7 per cent. of the average number resident. The deaths during the year amounted to 167, or 8 per cent. as calculated on the same basis. Of the deaths seven were due to renal disease, nine to colitis, 13 to cerebral softening, 18 to cardiac disease, 19 to pulmonary and other forms of tuberculosis, 46 to general paralysis of the insane, and the rest to other

causes. The Commissioners in Lunacy state in their report that the general scheme and management of the asylum deserved favourable commendation, that the patients appeared to be well dressed and well cared for, that the day-rooms and dormitories were bright throughout, and that the medical case books were kept in a highly satisfactory manner. They recommend that in case of fire the high level water-tank should be used. The committee of management states in its report that plans have been prepared and adopted for the erection of a hospital villa to accommodate 50 male patients and the necessary staff. The internal decoration of the asylum is now complete.

REFERENCE

Lancet, 14 January 1905. 112.

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Corrigenda

Cannabis as a psychotropic medication (letter). *BJP*, 185, 78. The correspondent's name should read: S. K. Chaturvedi.

Pituitary volume in psychosis. *BJP*, 185, 5–10. The pituitary gland was indicated incorrectly in Fig. 1 (left, p. 6). The correct figure is reproduced below.

