Correspondence

EDITED BY STANLEY ZAMMIT

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Flashbacks and PTSD

Jones et al (2003) claim to provide evidence that flashbacks in post-traumatic stress disorder (PTSD) are culture-bound because they were reported less frequently following earlier conflicts. They discuss the central issue of whether this was due to an under-reporting bias either because patients declared them less frequently or because doctors did not ask about them. In this discussion they conclude that this was not probable because the veterans were assessed frequently and because they were financially motivated by the prospect of a war pension. They have ignored the most important counter-argument that veterans of recent conflicts are most likely to over-report flashbacks in order to obtain compensation because: (a) the PTSD criteria are now publicised by the media; and (b) enquiry about flashbacks is now included in the routine clinical assessment of veterans.

A systematic study of exaggerating PTSD symptoms for compensation claims (Lees-Haley, 1997) indicates that at least 25% of present-day claimants overreport psychological symptoms. In earlier conflicts the post-trauma flashback symptom was mostly unknown by soldiers, clinicians or the media and there is no evidence of a 'compensation culture' at that time. Therefore, Jones et al's finding probably has more to do with the cultural aspects of compensation and malingering than the cultural aspects of PTSD. In failing to deal with this important issue I do not believe the authors have provided sufficiently strong causal evidence for their conclusion 'that some characteristics of PTSD are culture-bound'.

Jones et al (2003) draw conclusions that I believe are not entirely supported by the results of their study. The results show us that the percentage of flashbacks in post-combat syndromes is as low as 9%, thus challenging the credibility of flashbacks as a diagnostic sign for PTSD. Moreover, the study showed that only 9% of the soldiers with combat syndrome exhibit flashbacks. The argument that PTSD is a culture-bound syndrome is quite overstated. It seems that somatic symptoms are far more widespread in PTSD than are flashbacks. These somatic symptoms stand at the base of traumatic syndromes. The link between PTSD and culture is weaker than we might think. Elbert & Schauer (2002) state that survivors from different cultures (Sudan and Somalia) exhibit psychiatric symptoms of PTSD. Jones et al (2003) state that many historical documents regarding trauma lack a common denominator, and they are right to some extent. However, I have shown (2001, 2002) that the somatic symptoms of nightmares, sleep disturbances and increased anxiety occurring as a response to traumatic events are symptoms that have not changed in 4000 years. There is some connection between trauma and culture, but this connection is mild at most. I do agree with Jones et al that PTSD is an evolving syndrome. In my opinion, the core of PTSD (somatic symptoms) is timeless and not culture-bound. However, other less-common symptoms are prone to some cultural influence.


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Nobody, I think, would doubt that the diagnosis and management of some mental illnesses, perhaps PTSD especially, is culture-bound. However, I think the paper on flashbacks by Jones et al (2003) is misleading.

A flashback is not defined in the glossary of technical terms in either DSM-III (American Psychiatric Association, 1980) or DSM-III-R (American Psychiatric Association, 1987). The only mention of flashbacks in DSM-III is as a complication of hallucinogen hallucinosis. It does appear in the diagnostic criteria (B3) for PTSD in DSM-III-R (in parenthesis) but the reader is referred in the index to post-hallucinogen perception disorder. Thus, while DSM-III refers to dissociative states and DSM-III-R refers to ‘dissociative (flashback) episodes’, both, in the context of the diagnosis, are described as rare. Thus, at the time of publication of these manuals, they were not a ‘core symptom of PTSD’.

DSM-IV (American Psychiatric Association, 1994) retains ‘dissociative flashback episodes’ (without parenthesis) as one of the ways a traumatic event is persistently re-experienced, and in the glossary of technical terms defines a flashback as ‘a recurrence of a memory, feeling, or perceptual experience from the past’. Thus, flashbacks, unless they are qualified as dissociative, have become synonymous with ‘recurrent and intrusive distressing recollections of the events including images, thoughts or perceptions’. They do not even have to be intrusive. Such unpleasant memories are universal in combat veterans of any war. What has changed in this instance is how the term is used – not the phenomenon itself.

That ‘earlier conflicts showed a greater emphasis on somatic symptoms’ (Jones et al, 2003) indicates more clearly the impact of social values on symptomatology. Where a particular manifestation of
distress meets with disapproval – the suggestion in these cases of lack of moral fibre or worse – somatic symptoms could be expected. In the early 1970s a Thai psychiatrist returning to Thailand from training in the USA indicated to me that he had to educate his patients before he could diagnose depression (P. Chawasip, personal communication, 1972). At that time, all his patients with depression presented with somatic complaints.


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The study by Jones et al (2003) adds an interesting perspective on the concept of PTSD. However, there are methodological matters that concern me.

First, why are no subjects included from the Falklands Conflict of 1982? Jones et al cite O’Brien & Hughes (1991), whose work suggests that a much higher incidence of flashbacks might be found among that population.

Second, how many raters were used to confirm the existence of PTSD symptoms in the case records? What were the interrater reliabilities? How was any disagreement resolved?

Third, during my brief sojourn as Medical Member (Psychiatrist) of the War Pensions Appeal Tribunals, I studied in detail some 80 War Pension Agency case records, many for non-psychological cases. My overriding concern was the lack of symptom recording. Frequently, the relevant questions on War Pension Agency medical assessments concerning mental state received one-word answers, or were deleted entirely. How did these researchers deal with such cases?


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Authors’ reply: Leigh Neal has suggested that the increased incidence of flashbacks that we detected for Gulf War veterans is not a genuine observation but simply the result of contemporary overreporting. This effect he attributes to our ‘compensation culture’ and malingerling. While we fully agree that claimants with PTSD may on occasion elaborate psychological symptoms for financial reasons, this factor is hardly novel (Wessely, 2003). There was, for example, an epidemic of war pension claims for shell shock and neurasthenia in the aftermath of the First World War. By March 1921, it was estimated that of the 1.3 million awards, 65 000 were for functional nervous disorders (Jones et al, 2002). So concerned was the Ministry of Pensions that applications were being falsified or exaggerated that they appointed Sir John Collie, an expert in rooting out fraud, to chair their ‘special medical board for neurasthenia and functional nerve disease’. In 1917, Collie had included a chapter on the military in his textbook, in which he observed that ‘the thin line which divides genuine functional nerve disease and shamming is exceedingly difficult to define’ (Collie, 1917: p. 375). In fact, concerns about spurious or exaggerated claims for functional disorders pre-dated this conflict and followed the passing of the Workmen’s Compensation Acts of 1897 and 1906. In the 6 years following the 1906 Act, the sums paid in accident compensation rose by 63.5% – despite the fact that the number of people in employment remained the same (Trimble, 1981). The research in the 1880s by Herbert Page to establish that most cases of railway spine were without organic basis was driven by the large settlements being paid by railway companies to passengers who had exaggerated or falsified symptoms following accidents. Indeed, the term Rentenkampfneurosen (pension struggle neurosis) had been coined following Bismarck’s accident insurance legislation of 1884 and reflected widespread concerns that workers and passengers were defrauding companies through dubious medical claims (Lerner, 2001).

Other than agreeing that these things can and do happen, it is always risky to make statements about the incidence of malingerling, as clinicians have no particular expertise in its measurement. Dr Neal has no more information than we have, or anyone else for that matter, on the true rates of malingerling, let alone whether or not it is increasing. What the above does show is that concern about the phenomenon is certainly not new.

Menachem Ben-Ezra rightly points out that the flashback is a comparatively rare symptom among PTSD sufferers. He argues that other symptoms, such as nightmares, sleep disturbance and elevated anxiety, are common and enduring features, and, therefore, not culture-bound. While we agree that these symptoms were widely reported in the past, their existence per se does not justify the creation of a new and very specific disorder. The complex diagnostic criteria for PTSD in DSM–IV (American Psychiatric Association, 1994) comprise six sub-groups, which extend over three pages. Anxiety, sleep disturbance and nightmares are not disorders in themselves, as most people suffer from them at some time. It is only when they become severe or arise inappropriately that psychiatrists elevate them to psychiatric disorders. With the exception of hallucinogen persisting perception disorder, flashbacks are almost unique to PTSD. As a result, we chose this symptom as a way of trying to evaluate the incidence of this modern diagnosis. It should not be forgotten that PTSD did not enter DSM–III (American Psychiatric Association, 1980) as a result of a series of rigorous epidemiological investigations but in the context of an anti-war movement, which sought to demonstrate that service-men suffered long-term effects from combat. Only after it had been formally recognised by the American Psychiatric Association was PTSD then subject to intense scientific analysis (Young, 1995).

Dr Burges Watson has identified not only the growing significance attached to the flashback but also the disparity between the way that flashbacks are described as part of the diagnostic criteria for PTSD and in the DSM–IV glossary. In the former, they are included within ‘acting or feeling as if the traumatic event were recurring (includes a sense of reliving the experience, illusions, hallucinations, and dissociative flashback episodes . . .)’, while the latter contains a brief definition: ‘a recurrence of a memory, feeling, or perceptual experience
from the past’ (American Psychiatric Association, 1994: pp. 428, 766). Dr Burges Watson infers from this that the flashbacks is a new term for an old phenomenon; what in the past would have been described as a vivid memory of conflict is today called a flashback. The objection to this hypothesis is that we discovered both phenomena in medical records from the First and Second World Wars. We were careful to adopt a rigorous definition of flashback (which included the sense of reliving the traumatic episode) to distinguish it from eidetic memories.

In answer to Dr Hambidge, we were unable to include veterans of the Falklands War because ministerial permission was not granted to study recent war pension files of service personnel still living, and because the Medical Assessment Programme is limited to veterans of the Persian Gulf War. As regards the collection of data, three research assistants recorded symptoms on a standardised form by copying the case notes. In general, were excluded from the study. In general, were excluded from the study. However, this is apparent not only in areas of treatment standards and material standards of the facilities in which people are detained. I would also echo their sentiment that the wording of article 5(1)e of the European Convention on Human Rights is at best unfortunate and at worst deeply stigmatising. That said, I believe that the doctrine of the ‘living instrument’ (Tyrer v. United Kingdom, 1978; Reed & Murdoch, 2002) in Strasbourg jurisprudence is of fundamental importance in interpretation of the Convention and may yet lead to improved protection of the human rights of both patients with mental illnesses and people with learning disabilities.

With respect to patients who are de facto detained, the case of Rierra Blume v. Spain (1999) may improve rights protection. Here, the European Court of Human Rights ruled that the complainants, who had been escorted by the police to receive, among other things, psychiatric treatment, had been de facto detained and that their detention was unlawful. However, many patients for various reasons, especially non-protesting patients as in the Bournewood case (R v. Bournewood Community and Mental Health NHS Trust, 1998), will not take cases to the courts, and the protection of their rights may depend on relatives or voluntary organisations acting on their behalf.

Legal protection with regard to the autonomy of patients with mental illnesses and people with learning disabilities may improve by a back-door means, arising from the debate over privacy protection and article 8 rights (‘right to respect for private and family life’). However, rights can be secured in court only if challenges are brought, and many people with mental illnesses or learning disabilities may not have the awareness or the means to bring such challenges. The importance of ways other than legislation for highlighting and securing rights, such as the Royal College of Psychiatrists’ anti-stigma campaign ‘Changing Minds’, education campaigns about mental illness and the work of numerous voluntary agencies, cannot be underestimated in promoting equal rights and opportunities for these population groups.

**Declaration of interest**

The study was funded by the US Army Research and Material Command under grant number DMD1-98-1-8009. Edgar Jones was supported by a grant from the US Department of Defense.

**American Psychiatric Association (1994)** Diagnostic and Statistical Manual of Mental Disorders (3rd edn) (DSM-III), Washington, DC: APA.


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**Human rights and mental health**

I agree with Bindman et al (2003) that, to date, the jurisprudence of the European Court of Human Rights has not set a ‘high standard’ for modern mental health services. This is apparent not only in areas of the process of detention and its lawfulness, but also in areas of treatment standards and material standards of the facilities in which people are detained. I would also echo their sentiment that the wording of article 5(1)e of the European Convention on Human Rights is at best unfortunate and at worst deeply stigmatising. That said, I believe that the doctrine of the ‘living instrument’ (Tyrer v. United Kingdom, 1978; Reed & Murdoch, 2002) in Strasbourg jurisprudence is of fundamental importance in interpretation of the Convention and may yet lead to improved protection of the human rights of both patients with mental illnesses and people with learning disabilities.

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**Rierra Blume v. Spain (1999)** Judgements of the European Court of Human Rights (October 14). Paras 16–18; Available at http://www.echr.coe.int/

**Tyrer v. United Kingdom (1978)** 2 EHRR 1.

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**Slavery and psychiatry**

Raj Persaud (2003) begins his review of Thomas Szasz’s book Liberation by Oppression: A Comparative Study of Slavery and Psychiatry by asserting that something false is true: ‘Thomas Szasz became famous for being at the vanguard of the anti-psychiatry movement’. First, Szasz has never been part of the anti-psychiatry movement, much less at the vanguard of it. Second, there is as much truth in Persaud’s assertion as there is in asserting that the Nazis were simply practising medicine. Szasz has made it absolutely clear for over 50 years now that he supports psychiatry between consenting adults, that is, he supports contractual psychiatry. Third, Dr Persaud then asserts that Szasz is an ‘ally rather than an enemy of the National Health Service general adult psychiatrist’.
This is another fiction masquerading as fact. Szasz is not an ally of National Health Service psychiatrists, none of whom, to my knowledge, has denounced or renounced the practice of psychiatric slavery. Moreover, Szasz is a classical liberal, not a social list. The two cardinal principles of the classical liberal credo are the affirmation of the right to bodily and mental self-ownership and the prohibition against initiating violence.

These rather serious misrepresentations aside, Persaud ignores the core ideas in Szasz's book. Institutional psychiatry is an extension of law: institutional psychiatrists are agents of the state, not of their patients. Doctors who practise contractual medicine are agents of their patients, not of the state. The importance of this difference cannot be overemphasised.

People labelled by institutional psychiatrists as mentally ill are concurrently defined by the courts as less than human, in much the same way 'Negroes' in America were once defined as three-fifths persons. This is how Black people were, and people with mental illnesses are, deprived of liberty and justice by the state. Labelling of anyone as less than human is legal fiction, something false that is asserted as true, that the courts will not allow to be disproved. Just as defining Negroes as three-fifths persons served to maintain the institution of slavery, defining people as mentally ill serves to maintain the institution of psychiatry.

A person has a right to refuse treatment for cancer. A person does not have a right to refuse treatment for mental illness. If institutional psychiatrists are deprived of their power by the state to deprive mentally ill persons of their liberty, that is, if the state did not allow psychiatrists to enslave their patients in the name of liberating them, institutional psychiatry would go the way of slavery, as well it should.


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Treatment of common mental disorders in general practice: are current guidelines useless?
The paper by Croudace et al (2003) confirms the pattern set by previous studies (Upton et al, 1999; King et al, 2002) in showing little or no effect of educational and treatment initiatives on primary care physicians' practice of psychiatry. The authors provide various explanations for the negative outcome; one of these – 'failures in the content of the guidelines themselves in terms of their evidence base or relevance' – deserves greater prominence. Although psychiatry can claim some credit for advances in the diagnoses and treatment of more-severe disorders seen in secondary care, our interventions for the common mental disorders in primary care are much less securely founded.

The guidelines do not take proper account of the well-established fact that approximately two out of five patients presenting with common mental illnesses in general practice (even when considered ill enough to merit psychiatric input) improve rapidly within a few weeks. These probably merit the often forgotten diagnosis of adjustment disorder (Casey et al, 2001). Thirty per cent pursue a slower course of recovery and a further 30%, mostly with mixed anxiety and depressive disorder, have a worse outcome with frequent relapses (Tyrer et al, 2003), although in the short term a variety of interventions can be effective.

The methodology of Croudace et al's study is to be commended and the results show that even when guidelines lead to greater specificity in identifying illness, this is not accompanied by better outcomes. Pressured general practitioners in the past used to take the approach that if a patient with mental health symptoms presented for treatment, the doctor could listen sympathetically and, unless there was significant risk, would ask them to come back in 4 weeks' time. If the patient returned, he or she might have a more serious problem necessitating formal treatment. Such an approach may have a greater evidence base than any of our guidelines. It nicely separates those with adjustment disorders from the rest, prevents inappropriate therapies that might lead to iatrogenic problems like dependence, and is an excellent predictor of improvement many years later (Seivewright et al, 1998). If we were able to help general practitioners at the time of presentation to diagnose which patients needed intervention and which did not, we might be doing a better service than any of the current guidelines that litter general practice surgeries in this and many other countries.


Note
This letter was submitted before the appointment of P.T. as Editor of the Journal.

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Management of borderline personality disorder
Verheul et al's article (2003) states that dialectical behaviour therapy is an efficacious treatment for high-risk behaviours in patients with borderline personality disorder and suggests that this occurs via four core features (Linehan, 1993): routine monitoring; modification of high-risk behaviours; encouragement of patients to consult therapists before carrying out these behaviours; and prevention of therapist burnout.

We propose a management strategy for these patients delivered via a systemic approach that incorporates these principles and is especially relevant for services without the capacity to provide the skills base or intensity required for effective dialectical behaviour therapy. Such an approach has been developed by our service and is currently the principal method of
working with clients with borderline personality disorder in the lower North Island of New Zealand. It is a service-wide intervention with a long-term perspective, providing stabilisation and containment for both patient and staff. It is encapsulated in a management plan – a behavioural intervention to minimise reinforcement of hazardous behaviours and promote self-responsibility.

The plan defines the treatment system (e.g. psychiatric team, family, police, accident and emergency department staff), contains an acceptance of risk and explains the dangers of risk-averse responses from the service (Maltsberger, 1994). This breaks the cycle of assuming responsibility for the client and replaying a traumatising parent–child dynamic, with subsequent regression, increased risk and institutionalisation. We found that this is achieved by a behavioural intervention to minimise reinforcement of hazardous behaviours and promote self-responsibility.

The review by Marks (2003) of our book Introducing Cognitive Analytic Therapy: Principles and Practice (Ryle & Kerr, 2002) is both rude and misleading. His reminiscences about a visit to Leningrad in 1966 have nothing to do with the book and we certainly do not see ‘Pavlovian therapy’ (with which we are entirely unfamiliar) as ‘part of cognitive analytic therapy (CAT)’. His objection to the fact that our explicitly integrative model draws on a wide range of sources tells us more about the limitations of his own conceptual framework than about CAT. These limitations are also evident in his inability to understand or unwillingness to mention the key features of CAT, which he seriously misrepresents. These include: (a) focus on ‘reciprocal role procedures’, which are formed through the internalisation of socially meaningful, intersubjective experience and subsequently determine both interpersonal behaviours and self-management; and (b) the practical emphasis on the joint creation of descriptions of these, which serve to enlarge patients’ capacity for self-reflection and change and therapists’ ability to provide reparative, non-collusive relationships.

The reviewer’s bias is epitomised in his discussion of one of the case histories in the book (pp. 138–144). While asserting that this ‘patient with obsessive–compulsive rituals’ would have been better served by nine sessions of behavioural therapy or by one session plus computer-aided therapy, he fails to record that the patient was presented precisely to illustrate the limitations of cognitive–behavioural approaches and does not mention that she had previously dropped out of an anxiety-management group and of cognitive–behavioural treatment. Of this she had noted that the more her symptoms were worked on, the ‘more grimly’ she hung onto them. This was not a report of the treatment of obsessive–compulsive rituals, it was a summary of the psychotherapy of a person, an unhappy woman with a history of many years of panic, phobias, obsessive–compulsive behaviours and irritable bowel syndrome. The case was chosen, in part, to demonstrate how focus on presenting symptoms can actually be counterproductive and paradoxically collide with the enactment of underlying reciprocal role procedures in a patient who had come to be regarded as ‘difficult’ and ‘resistant’. This patient’s list of ‘target problem procedures’, as worked out with her, included a pervasive need to control both her feelings and other people’s behaviours. As is usual in CAT, this formulation, and her therapy, focused on intra- and interpersonal attitudes, assumptions and behaviours (procedures) and paid little direct attention to her symptoms. Therapy included, importantly, work on reciprocal enactments with the therapist. Assessment at termination and follow-up showed major improvements in her life, and psychometric testing demonstrated reductions in symptoms at termination with further reductions at 6-month follow-up.

We think it unfortunate that so obviously partisan a reviewer was selected to discuss a book outside his area of expertise and sympathy and that it was considered appropriate to publish so tendentious a review of the work of colleagues.


Cinders, you shall go to the ball

Goodwin has described bipolar disorder as the Cinderella of psychiatry, largely on the basis of his study showing the relative paucity of research studies in bipolar disorder compared with schizophrenia (Goodwin, 2000). This study has been reinforced by Clement et al (2003), who similarly concluded that bipolar disorder is underrepresented compared with schizophrenia and that this disparity is not declining over time. The importance of this discrepancy is demonstrated by the finding that bipolar disorder causes a greater global burden of disease than schizophrenia (Murray & Lopez, 1997) and by the huge financial impact of bipolar disorder on society (Das Gupta & Guest, 2002).

Clement and colleagues appear to lay the responsibility for the relative lack of bipolar research on a national shortage of specialist clinical services and on the lack of interest of researchers. However, clinical services such as our own in the Northern Deanery are flourishing and we suggest that historical difficulties in obtaining public
One hundred years ago

Insanity from hasheesh (extract) by John Warnock, MD, Medical Director, Egyptian Hospital for the Insane, Cairo

In the report for the year 1899 of the Bengal asylums, it is stated that 45 out of 220 cases admitted were due to the use of Cannabis Indica.

In Egypt, statistics are available since the year 1895. During the six years 1896–1891, out of 2364 male cases of insanity admitted to the Egyptian Asylum at Cairo, 689 were attributed to the abuse of hasheesh, i.e., nearly 27 per cent. Very few female patients used hasheesh, and it is noteworthy that insanity is more than three times as common among the hasheesh-using sex as among women, who, comparatively, seldom use the drug.

I think this difference in the insanity rate between the sexes is significant, and goes a long way to prove the importance of hasheesh as a cause of insanity among Egyptian men. Let it also be remembered that in England insanity is more frequent among women than among men (35 to 31).

My experience does not confirm the Indian Commission's belief that Cannabis Indica only sometimes causes insanity. In Egypt it frequently causes insanity. As to whether excessive use of hemp drugs is commoner here than in India I can give no opinion, but many thousands use it daily here. Probably only excessive users, or persons peculiarly susceptible to its toxic effects, become so insane as to need asylum treatment. Whether the moderate use of hasheesh has ill effects I have no means of judging, and this paper is now read to elicit the opinions of my colleagues in Egypt, whose daily practice must give them opportunities of studying the effects of the ordinary use of hasheesh. I should be grateful for information on this question.

REFERENCE

Journal of Mental Science, January 1903, 109–110.

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

Corrigenda


Non-right-handedness and schizophrenia (letter). BJP, 181, 349–350. The first sentence of the last paragraph should read: In schizophrenia, I have suggested that the gene may lose its directional coding and become ‘agnosic’ for right or left.

Long-term outcome of long-stay psychiatric in-patients considered unsuitable to live in the community. TAPS Project 44. BJP, 181, 428–432. Table 1 (p. 430) should read:

Declination of interest

The authors are supported in their research by the Stanley Medical Research Institute.


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<th>5 years</th>
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</table>

1. Patients who died during the 5 years have been excluded.
2. The numbers of patients in these columns differ. Thus, for Present State Examination total score, data were available for 57 subjects at baseline, 70 at 1 year and 56 at 5 years.
3. Only patients with data at the two time points being compared are included in the analyses. Thus, for Present State Examination total score, the comparison between baseline and 1 year involved 54 subjects and that between baseline and 5 years involved 43 subjects. Further details available from the authors upon request.
Human rights and mental health
L. Findlay
BJP 2003, 183:77.
Access the most recent version at DOI: 10.1192/bjp.183.1.77

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