Disclosure of religious beliefs

Surely after more than 200 years, psychiatry has become reasonably sophisticated and we can assume that there is no such thing as an unbiased comment. Professor Cooper’s attack on Professor Casey for being ‘a sincere member of the Roman Catholic Church’ is only justified if he also states, as the Editor does, that the other comment comes from Dr Oates, who is a ‘representative of the pro-choice group’. These senior psychiatrists were asked by the Editor to comment because they had both a special interest and special expertise.

There is a more general issue at stake here. It seems a sad reversion to attitudes in psychiatry of the 1960s when taking a religiously inspired position was seen as being unacceptably prejudiced, whereas taking a non-religious stance, even at the expense of the patient’s discomfort, was regarded as normal practice. Professor Casey has been asked to wear her religious belief publicly, like some yellow Star of David, with the intention to undermine the validity of her professional opinion.

As a former chairman of the Royal College of Psychiatrists’ Spirituality and Psychiatry Special Interest Group, I would hope that we could now give equal value to the viewpoints of psychiatrists with different philosophical and religious backgrounds. Dr Oates should be permitted, even in your august pages, to express a personal position, and so should Professor Casey. Yes, I do express a personal interest.

Cooper states ‘we all start from a position determined in part by personal background, and readers will not fully understand comments unless such things are known’, referring to Casey’s commentary on Fergusson et al and her Catholic faith. This seems to suggest that however sound our reasoning may be, it must be taken with a pinch of salt because one is a Catholic. Perhaps a Black man’s arguments against racism would be similarly invalid. No doubt Professor Cooper would not want an upsurge in anti-Catholic bigotry, but his suggestions may not prevent it.

Author’s reply: I am very pleased to have the opportunity to respond to the comments by Professor Cooper. They are all relevant to the important general issue of whether authors of papers on topics known to be controversial (such as abortion and ethnicity) should always be obliged to state their own background position in full. I suggest that the answer to this must always be ‘Yes, definitely.’

In scientific research, all possible attempts should be made to keep biases to a minimum, but unavoidable human influences can still be there and need to be known by readers if they are to understand both the data and the conclusions. These include the reasons for the research or review, the conclusions of any previous related studies by the same authors, possible biases in the methods of collection and analysis of the data, and possible biases in the conclusions of the authors. Different readers may then interpret the findings in different ways, depending upon their own viewpoint. If authors of papers on controversial topics follow these guidelines, and always state whether their conclusions are based solely upon the data of the study or also upon other background personal reasons, then the question of ‘outing’ will never arise. Similarly, on this line of reasoning, the simple statement of undisputed facts should not be regarded as ‘an attack’. There is wide agreement that financial rewards in the background must always be declared, so surely the same should apply to other potentially biasing influences.

Professor Sims’s reference to the ‘psychiatry of the 1960s’ puzzles me, and without specific examples I cannot comment on this.

The overall point at issue is that readers should be able to make up their own minds, and not be limited only to what the authors believe to be the best interpretation of the study. This may be rather perfectionist advice, but at least it gives a model as a target.

A more specific issue relates directly to the paper by Fergusson et al and to the comments by Professor Casey suggesting that this study constitutes evidence that special emphasis on the potential psychiatric hazards of abortion should be an obligatory part of psychiatric educational programmes.

Drs Rowlands & Guthrie seem to me to give a good summary of this whole problem: ‘Whether abortion causes harm to women’s

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Author’s reply: I am very pleased to have the opportunity to respond to the letters of Drs Blackwell and Aitchison, and Professor Sims. They are all relevant to the important general issue of whether authors of papers on topics known to be controversial (such as abortion and ethnicity) should always be obliged to state their own background position in full. I suggest that the answer to this must always be ‘Yes, definitely.’

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mental health is a question that is not scientifically testable, as women with unwanted pregnancies cannot be randomly assigned to abortion vs. abortion denied groups. It seems inappropriate therefore for Casey to talk of potential litigation against abortion providers for failing to provide information on a possible causal link between abortion and subsequent mental health problems.1

Debates on this topic and others such as racism tend to be endless, so I suggest that if anyone wishes to continue further, they should do so by direct personal emails.


Response to the Editor: We were dismayed and deeply concerned to learn, from the Editor’s note to Professor Cooper’s letter,1 that we had been characterised as holding a pro-choice position in our commentary on Fergusson et al’s paper.2 This was not mentioned in the commissioning process and, if it had been, the invitation would have been declined. Our commentary acknowledged a range of opinions among ourselves. Our arguments were based on an analysis of Fergusson et al’s paper, explicitly eschewing any partisan approach, and stating quite clearly that the debate on the rights and wrongs of abortion is primarily moral, legal and ethical rather than psychiatric or indeed scientific. We hoped we had been very clear in this approach, and most strongly reject any suggestion that our commentary was based in beliefs from either ‘side of the debate’.


Editor’s note: This correspondence is now closed.

Diagnosing chronic fatigue syndrome

In their comparative epidemiological study of chronic fatigue syndrome in Brazil and London, Cho et al.3 conclude that cultural differences affect only the recognition, rather than occurrence, of this condition. Although a reasonable interpretation of the epidemiological data, without complementary consideration of the cultural context this assertion is likely to obscure some of the most salient features and clinical significance of the study. The authors note that ‘both population and healthcare professionals seem unfamiliar with the construct of the syndrome’. Recognition of the community and professional inattentive to and low priority of chronic fatigue syndrome, however, is not necessarily a failing; it may also be regarded as an updated example of Kleinman’s4 formulation of the category fallacy – the imposition of alien diagnostic concepts where they lack local validity. The assertion of underrecognition is incomplete without consideration of alternative formulations of the problems that in some respects resemble the syndrome, but are not diagnosed. Do conditions such as neurasthenia in East Asia and dhat syndrome in South Asia have characteristic patterning of distress or meaning in Brazil?

If one accepts the authors’ tacit premise that the constructs of chronic fatigue syndrome and related UK formulations (encephalomyelitis and fibromyalgia) are unquestionably valid diagnoses for use everywhere, then the conclusion that chronic fatigue syndrome is neglected by professionals but no less important in the Brazilian population is valid. Accepting that premise, however, requires that we ignore the fact that the syndrome is neither in the ICD or DSM, and neurasthenia was rejected after consideration in the draft version of DSM–IV.3 Standard texts in the field of cultural psychiatry regard chronic fatigue syndrome as a North American culture-bound syndrome.4 Earlier research by some of the same Brazilian authors also highlights the social determinants of essential features of chronic fatigue, rather than the categorical diagnosis of the syndrome.5

Culturally sensitive clinical care will benefit from a reconsideration of cultural interpretations of these study data and from additional cross-cultural research. Are other diagnoses or local clinical and cultural formulations used to manage and treat such patients locally? Are other non-medical sources of help and social interventions given higher priority by patients and communities in Brazil?

Findings of Karasz & McKinley6 showing the tendency of North Americans to ‘medicalise’ and South Asians to ‘socialise’ similar clinical vignettes recommend consideration of that point. Among patients studied by Cho et al, one might also ask whether higher rates of associated common mental disorders suggest that these psychiatric conditions are more likely to be the focus of treatment. The emphasis on underrecognition of chronic fatigue syndrome is likely to prove less important for community mental health and culturally sensitive care than questions of how such clinical patterns are understood in the population and explained by professionals.


Authors’ reply: The assertion that chronic fatigue syndrome is a culture-bound syndrome of high-income Western countries may be largely based on the observation that ‘clinical descriptions of chronic fatigue syndrome, also known in some countries as myalgic encephalomyelitis, have arisen from a limited number

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of high-income countries in Northern Europe, North America and Oceania. We aimed to examine the reasons for this particular observation: proving or disproving the above assertion was beyond the scope of our study. Without any pre-assumptions regarding the local validity of the construct of chronic fatigue syndrome, we used this ‘etic’ construct (originating from high-income Western countries) in Brazil in order to examine whether this foreign concept defines a similar proportion of individuals as ‘cases’. We found that, using the current Centers for Disease Control (CDC) case definition of chronic fatigue syndrome, similar proportions of primary care attendees were defined as cases in São Paulo and London. However, Brazilian doctors were unlikely to recognise and/or label such patients as cases.

In a way, we actually used Kleinman’s formulation of the category fallacy as a research method in our study. That is, by imposing an alien diagnostic concept where its local validity is untested and unknown, we examined which component of this alien construct is not sanctioned by the local cultural context: the occurrence itself or the recognition/labelling. In Brazil, although unexplained fatigue as formulated by the Western medical community indeed does occur, it is not sanctioned as a medical condition worthy of medical treatment, sick leave or sickness benefit, and it may be more likely to be considered as part of everyday adversity and less likely to be recognised as a medical disorder.

Furthermore, although Paralikar et al suggest that our paper lacked consideration of the cultural context, we actually discussed and interpreted these findings mostly in light of the sociocultural context. For example, based on previous studies and our own data, we discussed that sociocultural differences such as the degree of medicalisation of the population and awareness of chronic fatigue syndrome among the population and the medical professionals might have contributed to the current findings.

We have not specifically addressed alternative local formulations for the problems resembling chronic fatigue syndrome in Brazil. However, our case vignette study using a typical history of the syndrome according to the CDC definition revealed that the most common diagnoses given by Brazilian doctors were psychological disorders, hence providing some information regarding the question raised by Paralikar et al. In order to address this and other important questions, we have collected qualitative data through in-depth interviews of individuals with chronic fatigue in Brazil and these data are currently being analysed.

We agree with Paralikar et al that the pattern of recognition and labelling observed in Brazil is not a failure, since this pattern is probably due to the sociocultural context rather than to medical incompetence. Indeed, we never suggested it was a failure.

Finally, the study by de Fatima de Marinho de Souza et al actually used the same questionnaire as our study: the Chalder Fatigue Questionnaire. We also used a more inclusive concept of chronic fatigue as operationalised by this questionnaire, namely unexplained chronic fatigue, as we additionally screened for medical causes. The prevalence of unexplained chronic fatigue was similar in the two settings.


Differentiating spiritual from psychotic experiences

Stein1 raises an interesting and important question – that of differential diagnosis between spiritual experiences and psychotic disorders with religious content – when he shows that Ezekiel, as described in the Old Testament, has experiences that might be interpreted as first-rank symptoms. In addition to the religious implications of making such a diagnosis for the prophet (and possibly other spiritual leaders), there are critical implications for the evaluation and conduct of people that seek our clinical care with similar experiences.

We have conducted research on the relationship between spiritual experiences and psychotic and/or dissociative symptoms. A sample of spiritist mediums in São Paulo, Brazil, reported on average four first-rank symptoms, the same number as Ezekiel. However, the number of symptoms was not correlated to other markers of mental disorders such as scores on the Social Adjustment Scale–Self-Report (SAS–SR), Self-Reporting Psychiatric Screening Questionnaire (SRQ), and history of childhood abuse. Despite showing a high level of what could be interpreted as psychotic and/or dissociative symptoms, the total sample of 115 mediums had a high socioeducational level, a low prevalence of mental disorders and were socially well adjusted.2,3

There is an increasing literature showing a high prevalence of psychotic and dissociative symptoms in the general population. However, most of our knowledge of those experiences is based on clinical, often hospitalised, samples. Those and other studies indicate the necessity of being cautious when analysing the clinical significance of anomalous experiences emerging in non-clinical contexts, especially since our knowledge about these experiences is based on clinical samples. It seems that these psychotic or dissociative experiences are not necessarily symptoms of mental disorders. (Similarly, certain medical symptoms such as shortness of breath and tachycardia may be pathological in some circumstances and physiological in others.)

Certain additional features may suggest a non-pathological basis for the experience: lack of suffering or functional impairment, compatibility with the patient’s cultural background, absence of comorbidities, control over the experience, and personal growth over time. These criteria are useful pointers, but there is a lack of well-controlled studies.4

Experiences like those of Ezekiel have had an important role in the Greek, Jewish and Christian roots of Western society, and in our time they are prevalent in spiritual groups such as those related to spiritism, channelling, Pentecostalism and the Catholic charismatic movement.

Research to clarify our understanding of this aspect of human experience will not only enlarge our knowledge of human nature but also improve the cultural sensitivity and effectiveness of our clinical practice.

Author’s reply: Professor Moreira-Almeida’s study showing that Brazilian spiritist mediums who are not suffering from any current mental disorder may have a high prevalence of first-rank symptoms is an important addition to the debate on just how specific/non-specific such symptoms are to the diagnosis of schizophrenia. In this light, making any judgement about a prophet such as Ezekiel who lived more than 2500 years ago and basing it on only a few verses from the Book of Ezekiel (which many scholars believe he wrote himself) would seem at best to be a highly dubious exercise. Nevertheless, in the setting of a mental illness, particularly a psychotic episode, the presence of first-rank symptoms usually does point to a diagnosis of schizophrenia and in this context may have a helpful discriminating function.

However, I believe that in Ezekiel’s case these were genuine first-rank symptoms of schizophrenia. This is because there is other corroborative evidence that he suffered from mental illness. Thus there is good evidence of two catatonic episodes, one lasting for 340 days and a second shorter period lasting 40 days, and also that he had a variety of different types of auditory hallucinations as well as several visions. Although any one of these phenomena taken separately can be explained away as being due to the religious experiences in a devout person, the combination of having first-rank symptoms, catatonia, auditory hallucinations, as well as probable visual hallucinations all of a schizophrenic type, can only really be explained by the individual actually having schizophrenia. Perhaps it would have been more coherent to have written a single article on all aspects of Ezekiel’s illness, but because of the space restrictions of the Journal’s fillers, Ezekiel’s phenomenology cannot be revealed to readers all in one go, only as several smaller items. Interested readers should therefore watch this space and read the forthcoming fillers!

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doi: 10.1192/bjp.195.4.371

Corrections

Dissecting the phenotype in genome-wide association studies of psychiatric illness. BIP, 195, 97–99. The ninth member of the Cross-Disorder Phenotype Group is Thomas G. Schulze (the middle initial was omitted from the original publication).

Impact of childhood exposure to a natural disaster on adult mental health: 20-year longitudinal follow-up study. BIP, 195, 142–148. Table 3 (p. 145), columns 2 and 3: the values for PTSD current among bushfire survivors with PTSD symptoms arising from the 1983 bushfires are: 3 (0.9%). These values were erroneously reported as zero in the original publication.

doi: 10.1192/bjp.195.4.371a
Response to the Editor: Roch Cantwell, Ian Jones and Margaret Oates
Access the most recent version at DOI: 10.1192/bjp.195.4.369

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