

been that the true risks of violence from people with psychosis, at the population level, are exceedingly small.

Professor Persaud's impression might be owing to the space in our paper devoted to discussing the public health impact of alcohol misuse and antisocial personality disorder on violence. In an additional paper published recently in the *American Journal of Epidemiology* we make the point about psychosis more strongly (Coid *et al*, 2006). Researchers with an interest in violence and psychosis often emphasise that relative risks of violence are greater for individuals with psychosis but they ignore the fact that illnesses such as schizophrenia are rare and that persons with psychosis account for an exceptionally small number of violent incidents at the population level. Detaining more persons with psychosis in hospital would have a very small effect in reducing violent crime (Fazel & Grann, 2006).

Misleading impressions based on relative risks are typical for homicides perpetrated by people with psychosis. These are often based on Scandinavian countries where the base rate is exceptionally low (Hodgins & Janson, 2002). In locations where the base rate is very high, for example certain areas in the USA and South American countries, people with psychosis hardly feature in criminal statistics.

Careful reading of our paper will reveal how we dealt with confounding from comorbid conditions. We agree with Professor Persaud's point about residents in violent neighbourhoods entirely, but the sampling frame was intended to exclude bias from factors such as socioeconomic deprivation. We used two-level hierarchical models throughout the analysis to take account of clustering from these areas. We would concede, however, that our study did not adequately explore the important issue of neighbourhood effects.

**Coid, J., Yang, M., Roberts, A., et al (2006)** Violence and psychiatric morbidity in a national household population – a report from the British Household Survey. *American Journal of Epidemiology*, **164**, 1199–1208.

**Fazel, S. & Grann, M. (2006)** The population impact of severe mental illness on violent crime. *American Journal of Psychiatry*, **163**, 1397–1403.

**Hodgins, S. & Janson, C.-G. (2002)** *Criminality and Violence Among the Mentally Disordered: The Stockholm Metropolitan Project*. Cambridge University Press.

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### Assessment of manic symptoms in different cultures

Mackin *et al* (2006) make a laudable attempt to evaluate cultural differences in the perception of psychiatric symptoms. Unfortunately, aspects of their methodology make it difficult to draw definitive conclusions. I will leave it for the statisticians to decide whether the sample sizes for the English and Indian groups ( $n=20$  and 24 respectively) are large enough to allow the findings to be generalised. Given the authors' concerns about the influence of confounding variables on the findings, however, the disparity between the size of these groups and that of the American clinicians ( $n=82$ ) is striking. A demographic breakdown of the various groups might have been useful in allaying these concerns.

A further source of potential bias is introduced by asking the participants to complete rating scales for only two patients of a single nationality. There is a risk that cultural differences between nationalities might influence attitudes as to what can be considered 'normal' behaviour for people of other nationalities. Certainly, an English psychiatrist whose expectations of a 'typical' American have been shaped by stereotyped media images might not be expected to register certain aspects of the patients' behaviour as pathological on the Young Mania Rating Scale. The threshold for recognition of manic symptoms might well have been different had they been asked to rate their own compatriots. More revealing conclusions could perhaps have been drawn had all participants been asked to complete rating scales for patients of a variety of nationalities, including their own.

The authors make a compelling argument about the potential consequences of

cultural differences in the recognition of symptoms of mental illness, and have provided a useful starting point for future discussion and research. Unfortunately, they fall short of proving these differences exist with their preliminary data.

**Mackin, P., Targum, S. D., Kalali, A., et al (2006)** Culture and assessment of manic symptoms. *British Journal of Psychiatry*, **189**, 379–380.

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The study of Mackin *et al* was interesting but so much highly relevant information is missing that it is hard to determine whether the findings have validity. The clinicians are effectively trial participants, yet we are not told the method of selection for doctors in each country. Training and employment structures are so different in the three countries that the clinicians are likely to have had very different degrees of experience and specialisation (the American system in particular favouring greater sub-specialisation). We are required to make the assumption that the groups are similar in all respects except the culture of the country of practice, yet there is no way to tell this without a socio-demographic profile of the participants from each country. There should be an attempt to make them representative of the total population of psychiatrists in their country in terms of ethnicity, gender and other factors which have a strong subcultural influence. There is no unifying 'culture' for psychiatrists in the UK, where at least one-third are trained outside the UK, and in some areas of the country the significant majority of doctors are non-UK-trained. Sampling such a small group from the UK ( $n=20$ ) would be most unlikely to give a representative picture of British psychiatry as a whole. Similarly, India and the USA are also among the most multicultural countries in the world, and the same issues of systematic sampling bias apply.

Furthermore, we do not actually know the ethnic and cultural background of the two videotaped individuals with mania. They are described only as 'American' – but can this be a meaningful term when describing an individual's culture in such a varied society? The authors minimise the

implications of these difficulties by stating that ‘similar variability is likely to be present when ranking patients in routine clinical practice’. Few would debate the existence of inter-observer variability, but the core issue here is whether the authors’ data support culture as being a central factor in this phenomenon. The design of the study simply does not permit this conclusion.

**Mackin, P., Targum, S. D., Kalali, A., et al (2006)**  
Culture and assessment of manic symptoms. *British Journal of Psychiatry*, **189**, 379–380.

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**Author’s reply:** We wholeheartedly agree with Dr Sanderson’s conclusion that this study provides ‘a useful starting point for future discussion and research’. Clearly, the number of assessed patients was small

as was the number of clinician-raters. We acknowledge these points in our discussion and conclude by recommending other large studies using patients from real-life clinical settings. We also agree that perception of ‘normal’ behaviour would vary according to nationality and this might have very real significance when assessing the mental state of an individual. This warrants further research.

Drs Sanderson and Reed both comment on the lack of socio-demographic data on the rating clinicians but unfortunately these data are not available. We disagree with Dr Reed’s assertion that we are required to make the assumption that the groups are similar in all respects except culture. We state clearly that ‘we cannot exclude the possibility that other factors, in addition to cultural background, may have influenced these results’, and we go on to prescribe potential confounding influences, including age, gender, psychiatric training, years of experience, etc. Similarly, Dr Reed’s suggestion that we minimised the

implications of these difficulties is unfounded; in fact, we highlight the possibility that multiple factors, including cultural biases, might affect the accuracy of scores on the Young Mania Rating Scale between clinicians from different countries. It is highly probable that similar variability will be present when this rating scale is used in routine clinical practice by clinicians from diverse cultural backgrounds.

Notwithstanding the preliminary nature of our study and the methodological considerations discussed above, we believe our data support the suggestion that cultural background influences the interpretation of manic symptoms when using the Young Mania Rating Scale.

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## One hundred years ago

***On the Growth of Nails in States of Mania and Mental Depression [l’Accrescimento Ungueale nella Frenosi Maniaco-depressiva]. (Ann. Di Freniat., June, 1906.) Falciola***

Dr. Falciola has collected a large number of papers upon the growth of nails and the changes noted after disease, which he has tested by his own observation. He is not disposed to agree to the assertion of Parisot and Paget that the state of the nails is an index of trophic alterations in the body,

although he admits that their growth is affected by a general disturbance in the economy of the organism. He found that in melancholy the growth of the nails is slower. The increase of the nails is somewhat irregular, being greater at one time than another, and differing in each finger, although there is a general equality in growth, which is more marked in the three middle fingers. The nails of one hand do not grow at exactly the same rate as in the other. He fails to find either marked acceleration or slowness of growth in states of mental depression or mania. In general he

finds that the study of the growth of the nails in insane patients appears to support the views of Kraepelin on the clinical unity of all those types of mental disease which writers generally wish to treat as distinct, but which, in truth, only represent different episodes of one fundamental malady.

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