

of PTSD in low- and middle-income countries. Under-recognition and under-treatment of PTSD, as well as problems with the PTSD construct, may well exist in certain countries; however, these issues are logically distinct and cannot explain the specific pattern of results we obtained. For example, under-treatment cannot explain why prevalence rates based on standardised population surveys are relatively high or low.

Their second concern is about the measurement of exposure to trauma. It is true that we could not distinguish between different exposure types, which we continue to see as an important limitation. But in our analyses – and also when the exposure rates by Benjet *et al*<sup>2</sup> are used – higher rates of trauma exposure were associated with higher prevalence in the expected way. It is not the exposure data but the country vulnerability data that generate the paradox. Vermetten *et al* do not raise concerns about the measurement of vulnerability.

We disagree with their suggestion that ‘drawing strong inferences from this single data-point is not a scientifically sound approach’. Table 2 and Figure 2 in our paper clearly show patterns in the data as a whole that are not reliant on one country. Vermetten *et al* suggest that ‘other analytic approaches do allow rigorous conclusions regarding the cross-national epidemiology of PTSD’. However, the example they give does not involve country-level variables, which are the focus of our analysis. It is not clear to us how their example is relevant to our quite different research question.

So far, we found indications that, regardless of exposure, PTSD and other mental health problems are more often observed in less vulnerable, more affluent countries.<sup>1,3</sup> The analyses we have used are appropriate to the question asked. Rather than ignoring challenging findings, we believe it is scientifically responsible to explore them further. If reliable, they have potentially far-reaching implications from an international mental health perspective.

- 1 Dückers MLA, Alisic E, Brewin CR. A vulnerability paradox in the cross-national prevalence of post-traumatic stress disorder. *Br J Psychiatry* 2016; **209**: 300–5.
- 2 Benjet C, Bromet E, Karam EG, Kessler RC, McLaughlin KA, Ruscio AM, et al. The epidemiology of traumatic event exposure worldwide: results from the World Mental Health Survey Consortium. *Psychol Med* 2016; **46**: 327–43.
- 3 Dückers MLA, Brewin CR. A paradox in individual versus national mental health vulnerability. *J Trauma Stress*, in press.

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### Ethnic density – meaning and implications

The ecological study by Keown *et al*<sup>1</sup> is undoubtedly of value, both methodologically and in relation to the further exposition as to how ‘structural and social issues can shape mental health’, as Burns and Rugkása<sup>2</sup> (p. 97) note in their related editorial. However, some clarification of the authors’ use and operationalisation of the term ‘ethnic density’ is required to more fully understand the study’s implications and limitations.

The study documents ‘a positive association between ethnicity and compulsory in-patient treatment’ in urban areas (p. 158), but as the denominator of population analysis is relatively large (divided by primary care trusts (PCTs) with an average population of 350 000), it is unclear whether ‘ethnic density’ is defined in their study according to the overall prevalence of different

ethnic groups within these relatively large unit PCT populations under study, or whether smaller and more relevant unit neighbourhood-level measures of ethnic density have been used.

An important earlier study using such neighbourhood-level measures, by Das-Munshi *et al*,<sup>3</sup> demonstrated that ‘people resident in neighbourhoods of higher own-group density experience “buffering” effects from the social risk factors for psychosis’ (p. 282). As psychotic presentations are more likely to result in compulsory admission, Das-Munshi *et al*’s findings would be expected to predict, when controlling for other variables highlighted by Keown *et al* – in particular, age and deprivation indices – that higher ethnic density, through ‘buffering effects’, would lead to lower levels of compulsory admission. Although it is possible that the findings of Das-Munshi *et al* and Keown *et al* are therefore in contradiction, it seems more likely that the Keown *et al* study did not measure ethnic density at the more relevant neighbourhood level in which buffering effects are manifest, and therefore that their measure of ‘ethnic density’ is less meaningful.

Ecological studies, by definition, attempt to attend to these more proximal influences on the immediate living environment.<sup>4</sup> Although the data-set used by Keown *et al* no doubt precluded this, the contingent limitations of such data, if this was the case, are therefore important to further acknowledge. Neighbourhood-level ethnic density data would also be needed to confirm the significance of Keown *et al*’s unexpected finding of a lack of association between ethnicity and compulsion in rural areas, where genuine neighbourhood-level ethnic density might be expected to be low, at least in some areas. Nonetheless, Keown *et al*’s study alerts us to the importance of attending to both social and cultural factors influencing the genesis, precipitation and maintenance of mental illness, including psychosis, which may be variously protective or risk-amplifying, and which interact in complex – sometimes counterintuitive – ways, influencing prognosis,<sup>5</sup> hospital admission and compulsion.

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- 2 Burns T, Rugkása J. Hospitalisation and compulsion: the research agenda. *Br J Psychiatry* 2016; **209**: 97–8.
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- 5 Rodger J, Steel Z. *Between Trauma and the Sacred: the Cultural Shaping of Remitting-Relapsing Psychosis in Post-Conflict Timor-Leste*. Springer, 2016.

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