

81% (240/297) of rural residents with a psychotic illness had schizophrenia, so we used 'schizophrenia' as the overarching label for these individuals. We agree that there may be differences in the employment status of individuals with schizophrenia compared with those with other psychotic disorders, but inclusion of this variable (i.e. schizophrenia *v.* other psychotic disorders) as a separate variable in our multivariate analysis did not appreciably affect our final results.

Finally, Gnanavel points to the need to adjust comparison of urban *v.* rural employment rates in persons with schizophrenia for differences in urban *v.* rural rates of employment in the general population. Official unemployment rates – the proportion of individuals 15 to 59 years of age in the workforce – in China are notoriously unreliable. Moreover, comparison of urban *v.* rural employment rates are confounded by the much larger number of young adults in school in urban *v.* rural areas (an issue we have adjusted for in our analysis) and by the huge number of individuals who migrate from rural to urban areas for work. Thus, it was not feasible to adjust for this factor in our analysis. We think it is unlikely that any differences in general employment levels between urban and rural residents would explain the more than threefold difference in employment (94% *v.* 27%) that we identified in rural *v.* urban residents with psychotic disorders. Nonetheless, if reliable data on rural *v.* urban differences in the general population were available, they might enhance interpretation of our results.

These issues raised by Gnanavel, in addition to considering premorbid employment as we indicate in the discussion, certainly merit consideration and suggest directions for future research. We do not, however, believe that they seriously undermine the key finding of our paper. This large, community-based study in China<sup>3,4</sup> found that opportunities for employment for individuals with psychotic illnesses are much greater in rural than in urban areas. The employment available in rural areas is largely restricted to that of agricultural worker and may only be part-time, but it is, nevertheless, an important indicator of social integration. Further studies are needed to unravel the full implications of this rural *v.* urban employment difference for social integration, and to determine the extent to which the increased employment opportunities of rural residents with schizophrenia are related to stigma<sup>5</sup> and to other measures of social integration.

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## Religious service attendance as a protective factor against suicide

We compliment Kleiman & Liu for their important research on suicide.<sup>1</sup> Suicide is the most preventable cause of death among the top 20 leading causes of mortality for all ages.<sup>2</sup> It is a complex and multifaceted problem that requires multidimensional prevention programmes. Therefore, this research effort of Kleiman & Liu is worth praising for demonstrating the protective role of religious service attendance against suicide. This paper has additional merits as it was a prospective study of time to completed suicide as an outcome variable in a large, nationally representative sample that was assessed directly rather than by proxy informants as in most other studies. We would also like to mention several important limitations. First, the numbers of suicides might not be true figures, so the overall findings might not be a fair representation of this major public health problem. Second, the rate of depression appears too low in the overall sample as well as zero in individuals who died by suicide, which might be because of improper evaluation and record-keeping rather than actual low prevalence in this population. Third, the authors addressed religiosity only by attendance of religious services and ignored other measures of religiosity such as practices at home or in other settings. They also make no mention of spiritual beliefs and practices. Fourth, their finding of a protective effect of religious service attendance in a specific population may not be generalisable to other religions across the globe. Fifth, dichotomising religious service attendance (i.e. 24 times per year) has an inherent conceptual problem in defining religiosity and its effects.

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**Authors' reply:** We agree that several of the points raised by Nebhinani merit consideration. Indeed, many are discussed in detail in our paper.<sup>1</sup> However, we further elaborate on some of them below. First, Nebhinani noted that the suicides reported in the study might not be accurate figures. We direct readers to the second paragraph of our method section, where we discuss the accuracy of our mortality data. We also direct readers to several studies that show that the National Death Index, our data source for mortality information, is highly accurate, with sensitivity and specificity nearing or exceeding 99%.<sup>2,3</sup> Moreover, the data-set from which our baseline data were drawn (the National Health and Nutrition Examination Survey) is a nationally representative sample, and so is indeed representative of the USA. Second, we agree that zero suicides among depressed individuals required further thought. This point was mentioned as a limitation in our paper. We speculated that this might be because the individuals became depressed after the baseline assessment, or because of a slightly lower estimate of the prevalence of depression in our sample compared with a similar epidemiological study. Third, we agree that the examination of religious service attendance is only one component of the multifaceted construct of religion, and other constructs (e.g. practising religion at home) may also be important. In fact, in our paper, we highlighted the need for future research to clarify the specific aspects of this

broader construct that are most directly relevant to resilience to suicide. We mentioned social contact or connectedness and religious beliefs as potential factors relevant to resilience to suicide. Finally, we dichotomised religious service attendance in order to be consistent with previous research that used the same variable from this data-set<sup>4</sup> and to allow easy interpretation of the survival analysis. Moreover, as we noted in our paper, using lower cut-offs was overly inclusive and using higher cut-offs was overly exclusive.

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