

conclusions regarding suicide among employed working populations. Last, the finding that suicide is highest among lower educated people in India may also mean a higher rate in lower skilled employed people, as education and occupational status are usually related.

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The rural employment advantage for people with psychosis: is it real?

The population-based study on employment outcome for people with schizophrenia in rural *v.* urban China by Yang *et al*¹ has revived the issue of rural advantage for people with psychoses in terms of functional outcome. However, I would like to point out a few methodological issues and practical considerations in the study that limit the interpretation of its results.

Non-inclusion of premorbid employment as a socio-demographic variable prevents us from gaining insight into the current employment status as a functional outcome marker. In addition, not incorporating elements of total work hours, income status and, most importantly, satisfaction with the current employment and simply considering the dichotomy of employed and unemployed with six subcategories seems too simplistic considering that employment outcome is the primary (and only) outcome that the study deals with. Inclusion of the category of underemployment (in addition to the categories of employed and unemployed), defined as employment not commensurate with one's educational level or premorbid occupational functioning, might have provided further valuable information regarding the employment outcome for these patients.² Not including the type of psychotic illness in the regression model is a major drawback, given that some forms of psychotic illness included in the study (such as delusional disorder and brief psychotic disorder) typically are associated with better functional outcome than others (such as schizophrenia).^{3,4} Further, a basic question that has been left unaddressed in the discussion is whether the differences in rates of employment in patients in rural *v.* urban China is simply reflective of differences in the overall employment/unemployment rates for the general population in the rural and urban regions of the country. Reports have documented higher unemployment in the urban regions of China than in the rural regions.⁵ It would also be important to conceptualise the social integration or social inclusion that the authors have discussed as a composite of employment, community networking and a supportive social environment without undue emphasis only on employment measures.⁶ Last, but not least, the authors could have avoided using the term schizophrenia as a synonym for psychotic illnesses

in the title of their paper, considering the spectrum of psychotic illnesses apart from schizophrenia that the study population covered. Notwithstanding the above methodological issues and practical considerations, I would like to congratulate the authors for undertaking a population-based study addressing the crucial issue of rural advantage in psychotic illnesses and the variables mediating the advantage, which has potential policy implications for this disadvantaged population.

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Authors' reply: Dr Gnanavel's letter has raised several interesting methodological issues related to our paper.¹ He notes that employment is only one measure of social integration and social inclusion. We certainly agree that other measures of social integration beyond employment are needed.² But fundamental differences in urban and rural environments make it extremely difficult to develop instruments that can validly assess social integration in both settings. Employment status is one of a very small set of variables about social functioning that can be readily measured and meaningfully compared (with the caveats noted below) between urban and rural settings. More detailed evaluation of community networking and social support may require the development of rural-specific and urban-specific measures; information from surveys that use these scales could then be used to devise and assess targeted interventions.

He also remarks on the need to consider work hours and work satisfaction when assessing the occupational functioning of persons with schizophrenia. We agree with the general point that more in-depth quantitative and qualitative data would enhance the interpretation of rural *v.* urban differences in employment, and encourage researchers to collect such data in future studies. Separate consideration of part-time *v.* full-time work would provide a more detailed understanding of the work status of persons with schizophrenia. As we indicate in the discussion, we believe that the greater flexibility of work in rural areas (i.e., allowing for part-time and full-time work depending on the individual's condition) may be an important factor in the higher rates of employment in rural areas. Comparisons of work satisfaction could also be useful, but such comparisons would require careful development of measures of work satisfaction that can be meaningfully compared for people with schizophrenia across these settings; to our knowledge, such measures are not yet available.

As regards collapsing all psychotic illnesses under the 'schizophrenia' rubric when assessing work status: in our study 90% (86/96) of urban residents with a psychotic illness and

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^{3,4} 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 stigma⁵ 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.¹ Suicide is the most preventable cause of death among the top 20 leading causes of mortality for all ages.² 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.¹ 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%.^{2,3} 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