Reviewer’s report

Title: The gravitational force of mental health services: Distance decay effects in a rural Swiss service area

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Reviewer: Tessa Roberts

Reviewer's report:

The paper addresses a relevant question from the perspective of mental health service planning and has many strengths. The use of travel time using public transport is arguably a more appropriate measure than that used in some studies. The separate examination of spatial patterns for inpatient and outpatient care, and of initial treatment contact and treatment volume/intensity, as well as investigating differences by disorder, adds to our understanding of distance decay effects with regard to the uptake of mental health services.

The general methodological approach used is appropriate, although there are a few clarifications needed. There is no justification of the choice of variables included in the logistic regression model, which should be explicitly stated. The third research question is vaguely worded, and would be improved by stating specific ecological variables to be investigated as well as offering explicit hypotheses about the expected influence of distance, diagnosis and the ecological characteristics investigated. A brief line to explain the reasons for the choice of a non-parametric test would be helpful, as would a note on the sample size calculation for the minimum number of cases for the analyses by disorder. It's not entirely clear from the authors' description whether travel distance was calculated from individuals' residences or if all calculations were done at the level of municipalities - this could be clarified in the text, and it may be helpful to discuss the amount of variation in travel time within municipalities so that readers can assess the likely accuracy of these measures if they were all at the municipality level. The authors also suggest that some of the variables in the model may be spatially clustered - for instance, proportions of immigrants are likely to be higher in urban areas - which has not been tested, and might warrant the use of spatial regression models. It would be useful to check the spatial distribution of these variables, to inform the choice of spatial or non-spatial regression models.

In terms of controlling for major confounders, the authors recognise that some important factors are missing. Potential confounders include population density (reflecting urban/rural residence) and education levels. Age is a relevant factor in both disorder prevalence and treatment-seeking, although it might be appropriate to treat this as a categorical variable, using age groups, rather than a continuous variable, given previous evidence of an inverse U-shaped relationship between age and treatment-seeking for mental disorders (e.g. Carragher et al., 2010; Issakidis and Andrews, 2002; Mackenzie et al., 2012; Ojeda and McGuire, 2006; Rost et al., 1998; Roy-Byrne et al., 2009; Starkes et al., 2005). The lack of any measure of geographical variations in prevalence is an important limitation, albeit one that the authors themselves acknowledge. Given
this lack of an appropriate denominator, it seems misleading to describe the caseload as "the proportion of mentally ill people receiving any outpatient treatment" (p.14). If any epidemiological studies of the region exist, this limitation could be addressed in part by referencing the extent of spatial variation in prevalence of mental disorders found in previous studies and the proportion of the variation that is explained by the factors controlled for in the current study.

Finally, it would be interesting to discuss potential explanations for the differing patterns of inpatient service use by people with organic mental disorders.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
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Yes

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