Reviewer's report

Title: Associations between residence at birth and mental health disorders: A spatial analysis of retrospective cohort data

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Reviewer: Shuang Wang

Reviewer's report:

General comments:

The authors investigated the spatial associations between residence at birth and odds of depression, bipolar disorder, and post-traumatic stress disorder (PTSD) in a retrospective cohort in Cape Cod, MA. The authors claimed that they observed spatial variation in the crude odds ratios of depression that was still present even after accounting for spatial confounding due to geographic differences in the distribution of known risk factors. The authors then concluded that spatial associations exist between residence at birth and odds of PTSD and depression. They also acknowledged that much of this variation can be explained by the geographic distributions of available risk factors, but these risk factors did not account for all the variation observed with depression, suggesting that other social and environmental factors within our study area need further investigation.

My biggest concern is, I'm not convinced that looking spatial association with depression and PTSD adjusting some known confounding factors is meaningful. It is not informative to conclude that a specific town has a higher odds of depression than another town. It will be more informative to see what may contribute to the increased odds. Although the authors mentioned that different spatial patterns may due to different environments and social economic status in the Introduction, they did not try to quantify the social economic status of different towns, or try to quantify environment (maybe overall pollution levels) or job component of those towns etc.

Other specific comments are:

1. Are the 1,256 subjects in the study independent subjects or included some siblings and family members? If so, did the authors treat them as independent in GAM?

2. The authors mentioned on page 10 “To determine the robustness of our results to including multiple children from the same family, we conducted sensitivity analyses which included one randomly selected participant from each family.” I don't think this procedure serves the purpose. If in the analysis with the full sample, the authors treated everyone as if they were independent, the authors should instead consider the dependence among subjects that are family members. Randomly pick one from the sibling pairs and treat all subjects independent only makes the analysis appropriate, but not to test the robustness
of the results.

3. Other than Table 1, the authors may also want to have another table that summarizes those characteristics within different towns.

4. The authors may also want to have a table that summarizes number of depression, bipolar disorder, and post-traumatic stress disorder (PTSD) in each town.

5. Are there any township-level environmental exposure data other than PCE, township-level income level, etc?

**Level of interest:** An article of insufficient interest to warrant publication in a scientific/medical journal

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.