Author's response to reviews

Title: Does place of residence affect risk of suicide? A spatial epidemiologic investigation in Kentucky from 1999 to 2008

Authors:

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Author's response to reviews: see over
Dear Editor-in-Chief,        December 26, 2011

BMC Public Health

We are pleased to submit the revision for our manuscript numbered MS 147775913619417 titled “Does place of residence affect risk of suicide? A spatial epidemiologic investigation in Kentucky from 1999 to 2008” (Daniel M. Saman, Sabrina Walsh, Anna Borówko and Agricola Odoi).

The paper has been updated in guidance of the reviewers’ comments and we hope the revised version will be up to the editorial standards of publications.

The following changes requested by the editorial office have also been made:
1. Please remove the authors’ qualifications (e.g. PhD / MD) or job titles from the manuscript.
2. Please change the title "Introduction" to "Background".

For your convenience, we have highlighted the major changes made in yellow. The article has been carefully revised and formatted according to the journal requirements. Some of the formatting and style changes including references may not be highlighted.

Below you will find a step-by-step response to all the reviewers’ comments. We have made all the changes that the reviewers have recommended, and believe it is a stronger manuscript because of it.

It has been a great experience to publish with you and we highly appreciate the time and efforts of the editorial team and the reviewers. We believe that this manuscript will be a valuable addition to the literature on suicide, and it highlights the importance of accounting for place of residence when performing injury epidemiologic research related to suicide.

Best regards,
Daniel M. Saman

Sabrina Walsh
Anna Borówko
Agricola Odoi
Response to reviewer 1, Nathalie Huguet

Reviewer: “The study use an innovative approach to examining suicide however the discussion section needs to explain/speculate as to why some of these counties are clustering.”

Response: Thank you for your comments on our manuscript. This study seeks to explore which areas in Kentucky are at a greater risk of suicide, and also describes characteristics of suicide cases inside the identified high risk clusters. We do not attempt to perform regression analysis to determine what factors contribute to the increased risk of suicide in these counties. The study is limited to a narrow and but well-defined objective. Many studies before ours have shown that regional poverty and social deprivation increase the risk of suicide. This study only identifies those counties at greatest risk for suicide. A lot of speculating beyond our results would be misleading, as our results say nothing regarding underlying etiology of county level increased suicide risk. We have identified counties at greatest risk, identified an older age group of suicide cases in one of the clusters, and used various methods to statistically and visually show these results. Without more rigorous regression analyses, we would only be speculating as to what explains high-risk county clusters of suicide in Kentucky. However, in the manuscript, we do hypothesize that socioeconomic deprivation is a likely contributing factor and briefly mention results from an unpublished study. We agree with you that some additional information is needed about what we hypothesize to be the reason behind these clusters. We have added more information showing that most of the counties in the cluster are rural counties and have included this in the manuscript: “We hypothesize that similar results may be found in Kentucky as in Glasgow, UK, and Queensland, Australia, where socioeconomic deprivation has been associated with high-risk suicide clusters [10, 11]. Without more rigorous regression analyses, we would only be speculating as to what explains high-risk county clusters of suicide in Kentucky. Beyond regional social deprivation, several other factors may be contributing to the higher rates of suicide in these relatively rural regions (i.e., all seven counties in the most likely cluster are rural, and three out of eight counties in the secondary cluster are rural) (42). Specifically, rurality has been found to be a likely regional risk factor for suicide in the United States and Australia (43, 44). Rural populations have less access to mental health care facilities, putting those with mental health disorders at a greater risk for suicide than their urban counterparts. Furthermore, data collected from the Kentucky Violent Death Reporting System (KVDRS) from 1999 – 2008 suggest that county-level unemployment may be statistically associated with higher rates of suicide (Sabrina Walsh, unpublished data).”

Reviewer: “Introduction: Given that suicide most recently reached the mark of being the 10th leading cause of death, updating the first paragraph with more recent data would be useful.”

Response: We agree and have added the newest figures we could find for 2009 along with a more recent reference.

Reviewer: “It is not clear why the SEER program was used rather than Census for example, please clarify.”

Response: Only SEER has population by county among gender and age distributions for 1969 to 2009. The spatial scan analysis controlled for age and gender distributions at the county level, so SEER was used. SEER is also used for state cancer registries. Population data is derived from the US Census Bureau's Population Estimates Program. This population data has been obtained from the Surveillance, Epidemiology, and End Results (SEER) Program at the National Cancer Institute: US Population Data 1969-2009, Release date January 21, 2011. A
All analyses were implemented in SatScan and adjusted for age (10–14, 15–19, 20–24,…, >85) and gender distributions at the county level for each individual year from 1999 to 2008 using population estimates derived from the US Census Bureau’s Population Estimates Program obtained from the Surveillance, Epidemiology, and End Results (SEER) Program at the National Cancer Institute [29].

Reviewer: “The description of the spatial and temporal cluster of suicide Line 225-236 could be shorten since the information is available in the table. I suggest you only discuss the RR and not provide details on observe and expect frequencies to make readership easier.”

Response: We have removed the information from lines 225-236 about observed and expected suicide cases inside the clusters. We agree that it improves the way it reads.

Reviewer: “The authors do not discuss how counties with very small numbers of suicide are treated and the effect it may have on the statistical estimates.”

Response: Although we do not discuss this problem, a more pressing problem in spatial epidemiology is small population sizes, and we do discuss this at length: Smoothed cumulative incidence suicide rates were mapped at the county level (n = 120). “The heterogeneity of variances and spatial autocorrelation of suicide rates were adjusted for by smoothing using Spatial Empirical Bayesian (SEB) rate smoothing [16]. This technique is appropriate when population sizes of areas of aggregation vary and there is spatial autocorrelation in the data. Given that population sizes vary by county, suicide rates from counties of small population have greater variance than counties with higher populations [17].”

Essentially, we do not treat counties with small cases any differently than other counties. Although a smaller number of cases would increase the variances of the county level rates, we have performed this analysis from 1999 to 2008 to ensure that small numbers were not a problem. It was also the largest time frame we had, and the small case problem was not really a problem at all for us, as only one county did not have any cases and it wasn’t found to be a high-risk cluster. We didn’t feel it was necessary to discuss this because it really wasn’t a problem for us.

Reviewer: “I am not convinced that the comparison of Exeter et al and the authors’ results is needed (line 277-290). The two studies have different populations making the comparison difficult. The authors suggest that the different rates between Kentucky and Scotland may be explained by other factors such as SES, unemployment, which are related to social deprivation the Exeter suggest. So in other worlds, the authors agrees with Exeter. I suggest that this section be changed. The authors, however, make a good point that (on line 291-293) both studies reveal that suicide is not randomly distributed and thus spatial analyses are useful.”

Response: We think it is interesting noting the differences in suicide in Kentucky and in a European country. In fact, we think it’s fascinating! Although the populations are different, it doesn’t make it a less apt country to compare suicide rates to. We want to emphasize the importance of multiple factors on suicide rates, and discussing this paper was one way of doing that. The Exeter et al study is also very similar to our study, allowing for ease of comparison. Because it is one of the only other studies performing a spatial scan analysis on suicides, we felt it was important to discuss it relative to our results. We also think it’s an important study to discuss relative to ours because it shows dramatically higher suicide rates in Scotland than in Kentucky despite Kentucky having ease of access to firearms where Scotland has very limited firearm availability. This suggests that firearm availability may not be the sole driving force behind increased regional suicide rates. A sentence has been added to emphasize this point:
‘Thus, this comparison suggests that firearm availability may not be the sole driving force behind increased regional suicide rates.’

**Reviewer:** “The authors should discuss what makes these groups of counties at higher risk. There is no need for further analysis. Are these counties more metropolitan or rural than the rest of Kentucky? Is there evidence that these counties have more social deprived? Examining the census data would be enough to provide a discussion of what might link these counties together.”

**Response:** We agree that some information on this should be added to our discussion and we have done so: “Beyond regional social deprivation, several other factors may be contributing to the higher rates of suicide in these relatively rural regions (i.e., all seven counties in the most likely cluster are rural, and three out of eight counties in the secondary cluster are rural) (42). Specifically, rurality has been found to be a likely regional risk factor for suicide in the United States and Australia (43, 44). Rural populations have less access to mental health care facilities, putting those with mental health disorders at a greater risk for suicide than their urban counterparts. Furthermore, data collected from the Kentucky Violent Death Reporting System (KVDRS) from 1999 – 2008 suggest that county-level unemployment may be statistically associated with higher rates of suicide (Sabrina Walsh, unpublished data).”
Response to reviewer 2, DeWayne Derryberry

Reviewer: “i) It would be nice to include medians (perhaps in parentheses) as well as means when reporting average ages.”
Response: Thank you very much for reviewing our manuscript. I checked the medians and because this is such a large dataset (n=5237) both the mean and median are nearly exactly the same for males and females and inside the clusters. To me adding the medians would be redundant because they’re so close to the means; adding medians wouldn’t add any important information to the manuscript. For this reason, only means are included in the results.

Reviewer: “ii) Twice the phrase “t-tests for unequal variances” (lines 146, 177) was used, I assume the authors are testing for means and not variances. Perhaps they should just say: “t-test (unequal variance was assumed)”, or just “t-test”. It does not matter much which test they used, a t-test with or without equal variance, since the tests almost always produce very similar results expect for small sample sizes.”
Response: We agree and have made the appropriate changes in the manuscript. Both places (lines 146, 177) now say the following: “two-tailed unequal variance t-tests”. This phrase is the one most widely used in the literature and it removes some of the confusion. Thank you for the suggestion

Reviewer: “iii) I assume the” ranges” given (lines 252-254) are actually confidence intervals, they should probably be identified as “95% intervals”. If they are not confidence intervals, or if the intervals differ form 95%, they might want to be more specific.”
Response: The range in the tables and the manuscript are just the full range of the dataset (not confidence intervals), from the smallest interval to the largest interval.

Reviewer: “I think this paper does a good job of presenting an example of a complete analysis of clusters. The scan statistics used is superior to many other methods for detecting hot spots. This is a reliable approach to preliminary investigation of differential suicide rates over large regions.”
Response: Thank you very much. We are grateful for your valuable comments.
Response to reviewer 3, Andrew Haddow

Reviewer: “Minor Essential Revisions:
A spatial epidemiologic investigation in Kentucky from 1999 to 2008” by Saman et al. is well written, the methodology employed is appropriate, and the limitations are stated. The spatial distribution of suicide in the United States has received little attention and is important to determine hot-spots in an effort to target prevention efforts. There are a few minor revisions noted below.”
Response: Thank you for reviewing our manuscript. We value your input and greatly appreciate your comments.

Reviewer: “Line 110. “La Cross” should be “La Crosse”
Response: We have made the appropriate change in the paper.

Reviewer: “Lines 343-346. “However, according to Dr. Sabrina Walsh, DrPH, MPH, unpublished data from the Kentucky Violent Death Reporting System (KVDRS) suggests that county-level unemployment is statistically associated with higher rates of suicide (oral communication, August 2011).” This sentence should be revised. This is somewhat speculative without presenting the data in the manuscript. Also, “is statistically associated” should be changed to “may be statistically associated” unless you have tested for statistical significance. Additionally, Dr. Walsh is an author on the manuscript. Therefore, you should just say something to the effect that: “Data collected from the Kentucky Violent Death Reporting System (From?? To When??) suggest that county-level unemployment may be statistically associated with higher rates of suicide (S.W. unpublished data).
Response: We have made the appropriate changes to the paper: Furthermore, data collected from the Kentucky Violent Death Reporting System (KVDRS) from 1999 – 2008 suggest that county-level unemployment may be statistically associated with higher rates of suicide (Sabrina Walsh, unpublished data).

Response: This has been corrected in the manuscript

Reviewer: “Discretionary revisions:
Figure 1. Move the box “Excluded 147 cases where county death was unknown” slightly to the right so it is in line with the box above containing “Excluded 153 cases where underlying cause of death was suicide (ICD 10; X60-X74, Y87.0) but manner was homicide” Make the box containing “5237 Suicide cases included in the study” smaller. There is a lot of wasted space otherwise.
Response: All the changes recommended have been made to the figure.

Reviewer: “The inclusion of the following information would strengthen the manuscript: Concerning suicide, were the clusters located in rural/urban areas, socioeconomically disadvantaged areas, areas with high unemployment during the study period when compared to areas outside of the clusters?”
Response: We agree that some information about the counties is useful and have added a paragraph to incorporate you’re the suggestion in your comment: “Beyond regional social deprivation, several other factors may be contributing to the higher rates of suicide in these relatively rural regions (i.e., all seven counties in the most likely cluster are rural, and three out of eight counties in the secondary cluster are rural) (42). Specifically, rurality has been found to be a likely regional risk factor for suicide in the United States and Australia (43, 44). Rural populations have less access to mental...
health care facilities, putting those with mental health disorders at a greater risk for suicide than their urban counterparts. Furthermore, data collected from the Kentucky Violent Death Reporting System (KVDRS) from 1999 – 2008 suggest that county-level unemployment may be statistically associated with higher rates of suicide (Sabrina Walsh, unpublished data).

Thank you so much for your comments. They’ve made this paper much stronger.

Best Regards,

Daniel Saman

Sabrina Walsh
Anna Borowko
Agricola Odoi