Author's response to reviews

Title: Bowel disorders and its spatial trend in Manitoba, Canada

Authors:

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Author's response to reviews: see over
March 8, 2014

Ms. Natalie Pafitis  
Executive Editor, *BMC Public Health*

Dear Ms. Pafitis:

**RE: Paper “Bowel disorders and its spatial trend in Manitoba, Canada”**

Thank you for the review and comments on the manuscript titled “Bowel disorders and its spatial trend in Manitoba, Canada”. We have provided a second revision of the manuscript based on the comments and suggestions, for your consideration. We address the comments and/or suggestions in turn.

1. Editor

   (a) *Specifically, the authors should acknowledge in the discussion that without the use of simulated data, where the “truth” is actually known, these methods cannot be evaluated for their accuracy of detection. The authors have chosen to not perform the simulations, but in doing so, they must now acknowledge that the performance of the methods cannot be compared in terms of one method being better than the other when applied to real data. The text must be rephrased to only draw comparisons of the results across the methods. I trust that the authors will also make the minor revisions requested.*

   Thank you for your valuable comment. It is known that it would not be easy to implement simulation study for the CSS and FSS methods using FleXScan (or SaTScan) software, however, we have considered the four different scenarios with low and high dense populations to study the performance of these four methods (CSS, FSS, BYM, MLE) *in the context of Manitoba (in terms of spatial structure and population density)*. We have now reflected this fact in the Discussion section of the paper. However, if the Editor does not agree with my argument, I will be happy to remove the corresponding paragraph in the Discussion regarding to compare these four methods.

2. Referee 2

   (a) *Discussion p9. The author concludes “BYM and MLE did good jobs... compared to the methods CSS and FSS”. What constitutes a “good job” in this context and how are the comparisons between methods made? Since the “true” cluster is not known, simply comparing which regions are or are not included in a putative cluster does not differentiate the performance of methodologies.*

   Thank you for your comment. We provided the four different scenarios (low and high dense regions in terms of population) to elevate some regions with relatively high risk. Our approach was then to evaluate the performance of the four methods to detect the “hot spots”. *In this context (spatial structure and population density in Manitoba)*, it would be fair to compare the performance of these four methods.
(b) Related to this, a previous reviewer suggested simulations “to investigate the ability of detecting spatial clusters using those four methods.” Instead the author has designated specific regions as clusters and performed tests of these alternative hypotheses. This does not get to the fundamental question of whether the cluster identification by each method is appropriate. The advantage of using simulated data is that the “truth” is known and thus the methods can be compared on performance and accuracy of detection.

Please see my responses in part (a) of the Editor’s comment and part (a) of the Referee 2.

(c) Spatial analysis using these methods is very complex and poses many challenges. It is somewhat disappointing to see the discussion and limitations treated in so brief a manner. Through multiple rounds of peer review, the reviewers have identified important limitations and discussion points which have been dutifully added to the text by the author. A thoughtful, careful exploration of these issues, instead of a “laundry list” mentioning each one after it has been raised during peer review, should be incorporated into the manuscript text before submission. The unknown “truth” of the underlying disease risk is a major limitation to the current comparison of methods and this must be addressed and acknowledged in the discussion.

Please see my response to part (a) of the Editor’s comment.

MINOR ESSENTIAL REVISIONS:

(d) Results p8. Typo “non” → “none”
Corrected.

(e) Results p9. Typo “The all four methods”
Corrected.

(f) Methods p6. The author has added an explanation of the importance of the deviance residuals to evaluate model fit, and removed the figure of residuals, both at the suggestion of reviewers. However there should be an accompanying explanation in the results which describes the results of the investigation, and in particular the unusual pattern of residuals related to possibly dependent high density urban regions. Could this influence/impact the results? This might also be mentioned as a limitation. If in fact there is something important to discuss here, then perhaps the figure should be included again. However it is problematic to emphasize the importance of this step and then omit the results (with an unusual finding that should be mentioned).

Thank you for your valuable comment. Regarding to the deviance residual (please see Figure 1), it seems that there is no obvious pattern to violate the goodness-of-fit
assumption. Having said that, as always in statistics, one can use other things to improve the model such as incorporating some risk factors, using other spatial components, among other things.

**DISCRETIONARY REVISIONS:**

(g) Results p8. Long lists of regions embedded within the text are not especially helpful for the reader. These data are displayed nicely in Table 1. The text should reference important highlights of the table (and the table itself could be enhanced by highlighting particular sections or rows) without listing the regions as currently presented.

Thank you for your suggestion. We have now only listed the regions of each cluster. The results are now explained by refereeing to the corresponding Figures (1-4).
Thank you again for your consideration and feedback. If you require further information, please contact me by e-mail at Mahmoud.Torabi@med.umanitoba.ca.

Sincerely,

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