Reviewer's report

Title: Quantitative measurements of inequality in geographic accessibility to pediatric care in Oita Prefecture, Japan: Standardization with complete spatial randomness

Version: 7 Date: 23 November 2010

Reviewer: Stefania Bertazzon

Reviewer's report:

I think the authors did a reasonably good job at clarifying the methodology they used. Their correctly numbering and labeling the figures certainly makes helps. I am still not satisfied by some of their responses. Specifically, I have some relatively minor concerns about the presentation, i.e., editing and English (see my comments 3 and 4). More importantly, I have some major concerns with the way they deal with the geography of the region and their response to their measurement of distance (see my comments and).

1. Comment:

I have some concerns with how authors deal with what they call “background geographic distribution pattern”. This treatment appears to be superficial as it does not carefully account for local specificities. This seems to be at odds with the authors’ stated intention of explicitly considering the underlying geography. The authors state that the study region is mostly mountainous, but they make a number of assumptions and generalizations that do not seem to appropriately take this aspect into consideration. These generalizations may seriously impact distance measurements. I strongly recommended computing travel time (i.e. not simple distance), and compute it from a road network. Using Euclidean distance and ignoring elevation, is inappropriate in a region described as “almost entirely covered by mountains”.

Response

We agree with your comments. Unfortunately, there are no data set for calculating travel time in any type of either commercial purchase or open to public. Thus, we need launch a big project by ourselves to survey for the data for travel time. In the revised manuscript, we change Euclidean to road network distance. The all results were refreshed with the network distance. With regard to elevation and road condition, we assumed the speed of car is equal because the condition of road is very nice and traffic jam is not so heavy regardless of mountainous or flat area in Oita Prefecture. We hope the reviewer so kind as to permit us to use travel time for not this paper but the future paper.

Using road network distance is certainly a big improvement over the use of Euclidean distance. I am still surprised that the data they used to measure this distance does not contain speed limits or a road classification (e.g., highway vs.
secondary road) that allows them to infer the speed limit on each segment and hence compute travel time at least based on speed limits.

Even more seriously, I find that their statement “With regard to elevation and road condition, we assumed the speed of car is equal because the condition of road is very nice and traffic jam is not so heavy regardless of mountainous or flat area in Oita Prefecture.” contradicts their stated intention of considering the geography of the place.

If the authors think that there is no difference between driving on a flat and straight road and driving on a steep and winding road, what is the point of explaining that this is a mountainous region, and that they want to explicitly consider geography?

Geography is not considered carefully in measuring distance, and is completely disregarded in their allocation of facilities, as they do include any representation of the topography, i.e., mountains and valleys are ignored.

2. Discussion “In this study, the inequality measure was standardized by using an expected value that was estimated by CSR and Monte Carlo simulation. This made it possible to compare inequality in geographic access to pediatric facilities across various regions in Oita Prefecture, Japan, by adjusting the influence of the background geographic distribution pattern of the location of children.” – I cannot say I agree with this statement.

If the reviewer clearly understand our method with this revision, we believe the reviewer agree with this statement.

I still do not agree, not because I do not understand the method, but because I do not think they are effectively “adjusting the influence of the background geographic distribution pattern of the location of children”. My previous paragraph should clarify this point.

3. The text still needs editing, e.g., in some cases the acronym CSR appears as CRS.

4. Comment:

The language appears to be overall acceptable but I suspect that it is often misleading. My impression is that the manuscript was edited by someone with good command of the English language, but poor understanding of the subject matter. The manuscript must be revised by an expert in the field, whose first language is English.

Response

The manuscript was copy-edited by the commercial copy editors. As you suspect, they are not expert in the field. Nevertheless, we do not have personal connection to any experts in the field whose first language is English. Unfortunately, all of experts we can ask the copy editing are not native speaker of English.
I do appreciate the authors’ difficulties, but I am still concerned with the comprehensibility of the article to an international audience, as is the target of this journal.

**Level of interest:** An article of limited interest

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests