Reviewer's report

Title: Effects of Neighborhood Geodemographic Profiles on Healthcare Service Wait Time: A Case Study on Cardiac Care

Version: 2 Date: 13 October 2012

Reviewer: Nathaniel Bell

Reviewer's report:

Thank you for the opportunity to review this manuscript. The purpose of this work was to assess the direct and indirect effect of a large number of demographic, socio-economic, and geographic determinants on the demand for cardiac care in the province of Ontario, Canada. The authors proposed the use of structured equation models to build on previous research in this area for purposes of illustrating previously under-explored latent variables that are associated with cardiac surgery characteristics. It is my opinion that this work, as it is currently written, largely re-emphasizes current knowledge and does not sufficiently focus on the novel contributions that could be made in this research area using the proposed technique. I hope the following comments will be of use to the authors for revising their manuscript.

Major compulsory revisions pertaining to the logic, structure, and theory of the manuscript

1. The authors propose an inappropriately large number of hypotheses for a single research study. Many of the proposed hypotheses (e.g. H6, H8) simply reaffirm what is already known on the subject. These are redundant and should be removed.

2. A considerable portion of this paper re-affirms what is already known on the subject. The authors need to work on collating the known evidence to better position this piece to stand out as a logical extension of the current research field.

3. The manuscript, as it is currently written, suffers from a lack of organization. For example, the hypothesis section should not also contain the literature review; the methods section does not adequately describe the data; the results section does not contain basic summary data pertaining to the study population, adverse event, and indicator variables (e.g. mean, SD, 95% confidence intervals); and the discussion section introduces new results that were not previously included in the manuscript (e.g. ethno-cultural influence on wait times). This lack of organization makes it very difficult to read the manuscript. It may benefit the authors to employ and independent scientific editor to ensure proper documentation as well as minimal use of jargon and run on sentences.

4. Although I think there are some real gems in this piece, the manuscript suffers from a lack of identity as it is unclear as to whether the primary contribution the
authors are attempting to make is on the value of the proposed SEM technique or the theory as to why direct/indirect models should be used to evaluate access to care. If the former, the authors may wish to expand on the strengths/weakness of current models (e.g. logistic regression, multivariate regression, etc.) and what (specifically) current techniques fail to provide that the SEM can offer. On the other hand, very little information is provided as to why the independent variables are included and what broader contexts they reflect. It would help if the authors propose a single model whereby the pathways that socio-economic, demographic, and geographic variables interact and influence either demand or wait times and introduce new/current theory as to why they order/pair of the variables as they do. This will help to build on previous studies and help readers identify (a) where previous studies fall short and (b) how the proposed analysis fills this gap.

5. The authors are primarily using aggregated (ecological) data for this study. As such, it is not entirely appropriate to infer a cause/effect relationship using path analysis/SEM as it is not possible to infer whether these variables in fact contribute to adverse events/wait times, etc. All that the authors can do is measure an association between the independent and dependent variables, which to me significantly detracts from the value of the proposed technique.

Additional compulsory revisions

Page 4, last paragraph: Is it that the evidence is inconsistent, or that the evidence is consistent to developing and developed countries. It is not appropriate to lump these two contexts together as a rationale for ‘inconsistent evidence’. As such, I question whether the authors are in fact providing anything new with their hypothesis based on previous Canadian studies.

Page 5, paragraph 1: Age can confound the relationship between education and health outcomes when the study population is older/elderly as the significance of education on income is considerably different than it was 50 years ago. In this study, what is the proportion of the population ages 50 and over with education level greater than high school compared to the population ages 50 and under? How does this relationship hold together when correlated against income?

Page 5, paragraph 2: Hypothesis 4 is not new. It is already known that higher-income is associated with medical insurance. Furthermore, the authors do not have the data (i.e. insurance status) to support this claim, so it should be removed as it cannot be substantiated using the proposed rationale.

Page 5, paragraph 3: The authors are in large part repeating work previously conducted by Seidel et al [manuscript reference 35]. How is the author’s work different than what has previously been tested?

Page 5, paragraph 3: The authors suggest that patient’s are dispersed over several hospitals as satisfying their null hypothesis. The authors do not report a sensitivity analysis that tests the null hypothesis nor do the authors report general descriptive statistics that illustrate the average number of persons per
hospital.

Page 5, paragraph 3: Why did the authors use a 60 minute driving time as the cut-off? This may not be an appropriate threshold for their study as the majority of the study population (i.e. all populations within 5 of the 9 LHIN's) are all within 60 minutes from a hospital. The authors also report on using only urban populations for their study. Is it feasible to expect to see large travel distances when only urban populations are included in the analysis?

Page 7, paragraph 4: Why did the authors choose to use a population cut-off of 30,000 in the analysis of travel time when the geodemographic profiles of the LHIN’s used in their study are based on populated areas of 40,000 or more (page 6, methods paragraph). The study should use consistent end points. Furthermore, what method was used to measure travel time?

Page 7, last paragraph: What is your unit of time for wait times? Minute, hour, day, week, month, year? This information is missing from the text as well as from the associated table (table 2).

Page 9, first paragraph: What is the study’s N used to measure wait times? There are three classifications of wait times provided in table 2. Which wait times is the study reporting on? Are the wait times condensed into a single wait time?

Page 10 and 11, discussion section. Many claims are introduced into this section that should be removed. This section should focus on the results that were presented and what they may mean. The authors should refrain from making speculation about relationships that they did not specifically measure or introduce earlier in the manuscript (e.g. immigrant status as an indicator of health).

Page 11, paragraph 3: The authors should refrain from entering new information in the discussion section that was not discussed in the introduction, methods, or results section (i.e. ethnographic comparisons).

Minor revisions

Abstract, background section: ‘ageing’ is misspelled.

Abstract, methods: ‘integrated’ should be deleted. Use ‘linked’ or ‘merged’ or ‘aggregated’.

Abstract, conclusions: use ‘associated’ as opposed to ‘may be due’. It is not appropriate to imply cause/effect using aggregated data.

Please defined ‘geosocioeconomic’ as I am unfamiliar with this term. Do you simply mean ‘socioeconomic’?

Page 2, last paragraph: the last sentence requires a reference.

Page 3, first paragraph: this paragraph largely repeats what was mentioned in the last paragraph on the previous page. These two paragraphs should be merged to remove redundant material.
Page 3, paragraph 1: define ‘geographic level’.

Page 3, last paragraph: I would advise removing the ‘rationale of population growth’ from this work unless your specific focus is to assess *change* in population growth and influence on wait times or to stratify by immigration status and adverse events. How is this paragraph as well as the first paragraph on page 4 supporting this work?

Page 4, paragraph 1: Information on immigrant status is not relevant to the author’s hypothesis. The authors state they are measuring population size without stratifying by immigrant status. This information should be removed.

Figure 2: Is this figure generated from Google Earth/Maps? If so, users are required to reference Google in all publications.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests - NBell