Author's response to reviews

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

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Version: 6 Date: 16 January 2013

Author's response to reviews: see over
Dear Editor-in-Chief and Handling Editor

We would like to thank all reviewers for their constructive comments and suggestions. We have taken great care to ensure that they have been adequately addressed in our major revision.

In order to further enhance the quality of the presentation, we have in addition acquired a professional copyediting service to go through the language.

In what follows, we provide detailed responses to the reviewers’ comments and descriptions of the corresponding changes.

Sincerely,

Prof. Jiming Liu

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Reviewer #1

We thank you for your thoughtful and valuable comments, and submit our responses below.

Comment 1

“Explicit definition and clarification of key concepts would be helpful to head off questions that may arise when reading the manuscript …

(1) What does ‘moderating effect’ mean in the context of the study?
(2) Is there any difference in treating education as a moderating effect of the relationship between age and demand from hypothesizing age to moderate the effects of education and demand?
(3) How is ‘demand’ defined?
(4) And ‘neighbourhood’?”

[Our response]

Thank you for the feedback. We have defined all of the key concepts in this revision and our responses to the above enumerated points are as follows:

(1) The “moderating effect” refers to the effect of a third variable that affects the direction and/or strength of the relation between dependent and independent variables [1]. For example, in the context of our study, the negative moderating effect of educational profile on the relationship between population size and arrival means that the influence of population size on arrival in a well-educated Local Health Integration Network (LHIN), e.g., Mississauga Halton LHIN, is not as strong as its effect on arrival in a less well-educated LHIN, e.g., Hamilton Niagara Haldimand Brant LHIN. Please see the second paragraph on page 2 of this revision for the informal definition of moderating effects, and paragraphs 3-4 of the Discussion section (pages 11-12) for the discussion on the meaning of moderating effects in our research context.

(2) The difference lies in treating education to moderate the relationship between age and demand (please refer to Figure 1(a)), and treating age to moderate the effect of education on demand (please refer to Figure 1(b)).

Regarding the first case (shown in Figure 1(a)), the focal independent and dependent variables are age and demand. Education, as a moderator, means it may affect the direction (e.g., make a positive effect into a negative effect), or mitigate/strengthen the effect of age on demand (i.e., change the path weight). In
other words, the slope that measures the rate of change in the dependent variable (i.e., demand) as the independent variable (i.e., age) changes – is no longer constant – but depends linearly on the level of education.

Unlike the first case, age as a moderator (shown in Figure 1(b)) focuses on how the change in demand (as dependent variable) depends on the change in education (as independent variable) and the level of age (as moderator).

![Diagram](image)

Figure 1: Illustrating the difference in hypothesizing education to moderate the relationship between age and demand (a), and hypothesizing age to moderate the relationship between education and demand (b).

(3) We changed the key term “demand” to “arrival” in response to Comment 2, because “arrival”, which is defined as the number of patients registered in hospitals for a particular healthcare service, is a clearer expression than “demand”. Please refer to the first paragraph on page 4 for the definition of “arrival”.

(4) In our research context, the key concept of “neighborhood” is defined as a Local Health Integration Network (LHIN). An LHIN is a way to represent the spatial divisions among healthcare services in Ontario, Canada. Please refer to the first paragraph in the Literature review and research hypotheses subsection on page 3 for the definition.

Comment 2

“... From an economics perspective, demand is largely decided by needs for goods and services and from a population health perspective, health needs are found to be associated with population geodemographics ... In my view, the “demand”, as conceptualized in the paper, has a closer meaning to “use” or service utilization pattern. Having a demand or need for services does not guarantee one is able to use the service when needed. For example, on p5, Hypothesis 5.1 states that travel time has a direct negative effect on demand. In fact, remote areas with a large number of seniors but low service accessibility could have the same or even a higher demand for cardiac services than “younger” neighbourhoods with good service accessibility and
consisting of individuals of high socioeconomic status.”

[Our response]

Thanks for the valuable suggestions. We have replaced “demand” with “arrival” accordingly. Please refer to the first paragraph of the Background section on page 4, or our response to Comment 1 for the definition of “arrival”.

Comment 3

“The limitations were discussed extremely briefly. I would like to see an expansion of the limitation on the chosen methodology and including a discussion on datasets, conceptualization of neighbourhoods, and even the conceptualization of the study.”

[Our response]

Thank you for the helpful comments. We have added a subsection to discuss the limitations in regard to the following aspects: (1) our conceptualization of “neighborhood”, (2) the data used in our investigation, (3) the data collection and statistical methods used, and (4) the research context.

Please refer to the Limitations and future research subsection on pages 12-14 for detailed information.

Comment 4

“(1) There is no justification as to why a 60-minutes threshold was used despite that the choice of threshold is critically important and it may potentially affect the conclusions drawn. A 60-min driving time threshold is quite large for urban areas.

(2) Also, difference in patient travel times between large urban centres like Toronto and small towns need to be recognized and discussed.”

[Our response]

Thank you for the valuable comments. Our responses to the abovementioned points are as follows:

(1) This revision uses a 30-minute driving time (based on the work of Bosanac et al. [2] and Chan et al. [3], and a recommendation from the Cardiac Care Network of Ontario [4]) as the threshold to measure geographic accessibility to healthcare services. Please refer to the last paragraph on page 7 for the corresponding definition and justification.
Travel time varies between urban centers and small towns and from one Local Health Integration Network (LHIN, the defined neighborhood in this study) to another. While it would be interesting to discuss these differences in service accessibility and its effect on arrival, it is difficult to recognize such differences in this study because our neighborhood is defined, at a relatively coarse granularity, as an LHIN. Our revision notes this limitation in the first paragraph of the Limitations and future research subsection on pages 12-13. We believe that defining “neighborhood” at a finer granularity level (e.g., at the level of city, town or community) in the future may produce further insights into the relationship between geodemographic profiles and the characteristics of healthcare services.

As LHINs are our neighborhoods of concern in this study, determining the differences in service accessibility is also an important and interesting issue. In this regard, we have presented the difference in terms of service accessibility (which is measured by the proportion of population within a 30-minute traveling time from home to healthcare services) in Table 1. The mean value and standard deviation for service accessibility are shown in the new Table 3 and the overall difference is addressed in the Discussion section (please refer to the third paragraph on page 11).

Comment 5

"Partially related to point 4, geographic accessibility perhaps plays a more important role in using cardiac care provided by physicians as one with low service accessibility would likely be more willing travel to a hospital for scheduled surgery than go to a physician for regular checkups (preventive care)."

[Our response]

Thank you for this insight. Geographic accessibility to services has been found to play a role in the use of various healthcare services such as primary care, cardiac angiography and pre-surgery tests [5][6]. Because cardiac surgery is invasive, with relatively scarce resources, the use purposes, risks and service resource distributions of cardiac surgery may be quite different from those of other cardiac services. Thus, the service utilization patterns of cardiac surgery may differ from those of other cardiac care services. It would be interesting to compare the roles that service accessibility plays in various cardiac services such as regular checkups, diagnostic cardiac catheterization and non-invasive percutaneous coronary interventions – once more data about other cardiac care services have been employed in the future. We have indicated this as a direction for future research. Please refer to the last paragraph in the Limitations and future research subsection on pages 13-14 for details.
Comment 6

“...There are 14 LHINs in Ontario; some are quite large geographic units and some have large populations. For example, the entire city of Toronto is one LHIN but it is nearly impossible to view Toronto as one single neighbourhood and in fact most of the literature on neighbourhoods and health use census tracts or communities as proxy of neighbourhoods.

(1) What are the pros and cons of using LHINs as proxy of neighbourhoods in the study?
(2) Also, it would be beneficial to further discuss the importance of examining neighbourhood contextual effects on health, in addition to individual compositional effects by including a stronger literature review in this area.”

[Our response]

Thank you for the valuable comments. Our responses to the abovementioned points are as follows.

(1) The LHIN, as the concerned neighborhood in our study, is a scenario representing the spatial division of healthcare services. In Ontario, LHINs are not strictly divided and aggregated by cities/towns. For example, the large urban city of Toronto has been divided into 4 LHINs. The majority of Toronto belongs to the Toronto Central LHIN (please refer to http://www.torontocentrallhin.on.ca/). The south-western portion of Toronto is part of the Mississauga Halton LHIN (please refer to http://www.mississaugahaltonlhin.on.ca/Page.aspx?id=146). The majority of the residents who use the Central LHIN live north of Toronto (please refer to http://www.centralhin.on.ca/home.aspx), and the Central East LHIN serves the eastern portion of Toronto (please refer to http://www.centraleastlhin.on.ca/home.aspx). Because LHINs are the only organizations that plan, fund and integrate healthcare services in Ontario, it is reasonable to regard them as neighborhoods, and doing so is one of this study’s contributions. In addition, using LHINs to proximate the neighborhoods can help us to understand the effects of the LHINs’ geodemographic profiles and corresponding healthcare service characteristics. Therefore, our study not only uncovers interesting relationships, but also provides further insight for resource planning and allocation in Ontario.

However, treating LHINs as neighborhoods also poses limitations. The LHIN is a relatively coarse granularity that contains several cities/towns/communities with potentially diverse geodemographic profiles. A finer granularity (e.g., at the level of city, town or community) may provide more insight into the relationship between geodemographic profiles and healthcare service characteristics.
This revision includes a discussion of the pros and cons of using LHINs as neighborhoods. Please see the first paragraph in the Limitations and future research subsection on pages 12-13.

(2) We have added a new paragraph to discuss the importance of examining the relationships between neighborhood geodemographic profiles and healthcare service characteristics. Please refer to the first paragraph in the Discussion section on pages 10-11 in the revision.

Comment 7

“In Discussion (p11), the authors mentioned immigration and the effect of population diversity on use of cardiac services. Some statistics and preliminary correlation analysis results were also included. I find this part not well connected with the rest of the paper. There is also no concrete evidence to show that ‘immigration has indeed led to increases in the demand for cardiac surgery services in the recent years’ (p12). I suggest to remove the discussion and analysis related to immigration or include it as a factor when establishing hypotheses in Discussion.”

[Our response]

Thank you for the valuable suggestion. We have removed the discussion of the relationship between immigration and patient arrival. Please refer to the revised Discussion on pages 10-14.

Comment 8

“There is much room to improve the maps that largely lack of essential cartographical elements. The overall presentation/design of the maps could have been improved as well (e.g., in Figure 2, the large symbol representing urban centres could be smaller, especially in the inset, and transparent…In Figure 3, no text and label, no legend, …low resolution for both maps)”.

[Our response]

Thank you very much for the kind comments. We have removed Figures 2 and 3 for the following reasons:

(1) Figure 2 illustrates the distribution of our sampled cities/towns. As we have clearly stated, our sampled cities/towns sustain populations above 40,000 as recorded in the 2006 Canadian Census data (the last paragraph of the Methods section on
Readers who are interested in the sampled cities/towns can easily find them on the Statistics Canada website. Given that the illustration in Figure 2 repeats statements made in the text, we have removed it to avoid redundancy.

Figure 3 illustrates the distribution of our focal 11 hospitals providing cardiac surgery services. However, because Table 2 provides the hospital names, interested reader can access them easily on the Cardiac Care Network (CCN) of Ontario website http://www.ccn.on.ca/ccn_public/FormsPartner/lhin_hospitals.aspx, or via search engine. Therefore, we also removed Figure 3 to avoid redundancy.

Comment 9
“All the figures have no title.”

[Our response]
Thank you for the helpful suggestion. We have titled all of the figures in the revision.

Comment 10
“The level of writing is acceptable but there is room to further improve the writing of the paper. For example, should ‘in prior literature’ be ‘in the literature’ or ‘in the prior literature’? Should ‘geographic accessibility of healthcare services’ be ‘geographic accessibility to healthcare services’…”

[Our response]
Thank you for the feedback. We have improved the writing in this revision and have employed a professional editing service to proofread the language.
Reviewer #2

We thank you for your attentive and beneficial comments, and submit our responses below.

Comment 1

“…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.”

[Our response]

Thank you very much for your valuable suggestion. The hypotheses related to the effects of income profile (H4.1-H4.3 in the previous version) have been removed from the revision. However, we have kept the hypotheses about the inter-relationships between healthcare service characteristics – i.e., the direct positive effects of arrival (i.e., demand in the previous version) on capacity and supply (H6 in the previous version) and the direct negative effect of supply on wait time (H8 in the previous version) for the following reasons:

(1) A comprehensive path diagram, including the hypothesized relationships among geodemographic factors of interest and healthcare service characteristics (i.e., arrival, capacity, supply and wait time), represent how the geodemographic factors directly, indirectly and/or moderately affect healthcare service characteristics, especially wait time.

(2) The research context and the secondary data we used in this study are unlike those in previous studies (e.g., [7][8]). Hence, we must validate whether the hypotheses hold in our specific research context. While we have kept the abovementioned hypotheses in the revision, we have restrained the content for proposing and discussing them to avoid redundancy and present a comprehensive model. Please refer to page 6 for these improvements.

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Please note that in this revision, we have used the term “arrival” to replace the term “demand” because “arrival” (which means the number of patients registered in healthcare services) can better express our intention than “demand” (which could be said to stand for the “needs” of healthcare services, but the “needs” may not equal the number of arrivals). Please also refer to our response to Reviewer 1’s Comment 2 regarding reasons for the improvement and note the Background and Discussion sections for details.
Comment 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."

[Our response]

Thank you for the feedback. We have extensively improved our manuscript to better position our work. The major contributions of this study are:

(1) We have examined the moderating effects of certain geodemographic factors (e.g., geographic accessibility to services and educational profile) on healthcare service characteristics (e.g., arrival, capacity, supply and wait time). To the best of our knowledge, this is the first work to report such findings in this research context.

(2) Because most of the existing work focuses on investigating the pairwise relationships between geodemographic factors and healthcare service inequities (e.g., [9][10]), this work provides a comprehensive and hierarchical conceptual model that uncovers how the geodemographic determinants lead to variations in healthcare service characteristics, especially wait time.

(3) The use of the structural equation modeling (SEM) technique to test our hypotheses is also a valuable reference in the related research field.

Comment 3

"The manuscript, as it is currently written, suffers from a lack of organization. For example,

(1) the hypothesis section should not also contain the literature review;
(2) the methods section does not adequately describe the data;
(3) 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);
(4) 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 an independent scientific editor to ensure proper documentation as well as minimal use of jargon and run on sentences."

[Our response]

Thank you very much for the valuable comments and suggestions. We have
extensively improved the organization and the writing in this revision. In the current version, the following sections have been extensively improved:

(1) The Background section provides the motivation and the contribution of this study. In its new subsection, Literature review and research hypotheses, the research hypotheses have been separated from the literature review. We also have provided a more concise and clearer review of the literature in this subsection. Please refer to the Background section, on pages 3-6 for these improvements.

(2) The Methods section includes the data resources and data collection methods for the secondary data used in this study, along with a brief introduction to the statistical analysis method, structural equation modeling (SEM), and its advantages for our study. In this revision, the used secondary data have been described in more detail. For example, we have provided details regarding service accessibility (please refer to the second paragraph on page 7), and have justified our use of a 40,000 cut-off point for sampling cities/towns (please refer to the last paragraph on page 6).

(3) The Results section summarizes the secondary data (e.g., the mean and standard deviation for each of the measurement variables as shown in the new Table 3) and the hypotheses testing results. Please refer to the Results section on pages 9-10, and the new Table 3.

(4) The Discussion section includes and expands the discussions from the following aspects: (i) the importance of examining the relationships between neighborhood geodemographic profiles of LHINs and healthcare service characteristics, and the pros and cons of using LHINs as neighborhoods; (ii) the discussion on result interpretations and identified LHINs with different geodemographic profiles to address the implications of our findings; (iii) research limitations and future research in terms of neighborhood conceptualization, secondary data for investigation, data collection and statistical methods and research context. In addition, we have got rid of the contents that have not been introduced in the hypotheses and results. For example, we have removed the specified text, e.g., the effect of immigration on healthcare service characteristics. Please refer to the revised Discussion section on pages 10-14.

Furthermore, we have comprehensively improved the writing, and have acquired a professional copyediting service to go through the language and to ensure the quality of the presentation.

Comment 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.”

[Our response]

Thank you for the helpful comment. As we note in our response to Comment 2, this study fills a gap in the literature by (i) exploring the moderating effects of certain geodemographic factors (i.e., educational profile and service accessibility) on the effects of other geodemographic factors (i.e., population size and age profile) and patient arrivals; (ii) providing a comprehensive research model that offers further insight into the relationship between geodemographic factors of interest and healthcare service characteristics, especially wait time. To our best knowledge, this is the first study to examine how certain geodemographics interact to influence healthcare service characteristics in this research context.

In this revision, we have improved the manuscript to show (i) the independent/dependent variables (e.g., population size, age profile, service accessibility, arrival, capacity, and supply) are important factors that may directly/indirectly/moderately affect healthcare service wait time based on the literature review, hence they are necessary to be considered in the research model; and (ii) how and why the variables are conceptualized, e.g., age profile has been conceptualized as the proportion of seniors (i.e., those older than 50) because risk factors for cardiovascular disease are prevalent in the segment of the population aged 50 and above [11][12]. Please refer to the Background section, especially the Literature review and research hypotheses subsection on pages 2-6 for these updates.

Our use of the SEM technique as a data testing method in this study is also a noteworthy contribution. Compared with the traditional statistical tools, such as multivariate regression, logistic regression and ANOVA, the SEM technique is efficient in: (i) modeling latent variables that cannot be directly observed, such as wait time in this study (which is a specific concept and can be measured by the median wait time of urgent/semi-urgent/elective patients, and queue length); and (ii) testing
complex/hierarchical relationships among observed and latent variables. Please refer to the Statistical analysis subsection on page 8 for our detailed explanation.

We have maintained our separate and sequential moderator testing in this revision for the following reasons: (i) testing the effects of each moderator separately allows us to present the moderators’ notable effects on the relationships between independent and dependent variables; (ii) because the moderators (i.e., service accessibility and educational profile) may share an inter-relationship, testing the effects of each moderator separately can ensure that a model does not become too complex to explain; and (iii) the secondary data we use in this study requires that we test the moderating effects together because of the collinearity between the two moderators – a limitation that has been acknowledged in the Limitations and future research section. We believe that it is valuable to propose and test the hypotheses within a single model, and have suggested it as a future research direction (Please refer to the third paragraph of the Limitations and future research section on page 13).

Comment 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.”

[Our response]

We have replaced terminology such as “may be due to” with “related to”, “linked to” or “associated with”, as proposed in Comment 19. Please refer to the Abstract, Results, Discussion, and Conclusions sections for these changes.

Comment 6

“Page 4, last paragraph:

(1) 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’.
(2) As such, I question whether the authors are in fact providing anything new with their hypothesis based on previous Canadian studies.”
Thank you very much for the comments. In the previous version, the last paragraph of page 4 proposed the hypotheses related to the direct effect of educational profile on arrival, and its moderating effect on the relationships between population size-arrival, and age profile-arrival. Our responses to the abovementioned enumerated points are as follows:

1. We agree that it is not appropriate to use evidence from developing countries, as our study is in the context of a developed country (i.e., Canada). Thus, it has been removed. Please refer to the last three paragraphs on page 5 for these revisions.

2. While the sole hypothesis that educational profile has a direct effect on arrival may not be new, according to the literature, we have rationalized the hypotheses that educational profile may have moderating effects on both the population size-arrival relationship, and the age profile-arrival relationship. The potential moderating effects of educational profile on the population size-arrival relationship and the age profile-arrival relationship are new in this research context, to the best of our knowledge.

Moreover, according to Baron and Kenny’s definition of a moderator [13, p.1174] as a “…variable that affects the direction and/or strength of the relation between an independent or predictor variable and a dependent or criterion variable…”, the moderating effect is usually formulated in the following product form [14, p.718]:

$$Y = a + b \cdot X + c \cdot M + d \cdot (X \times M)$$

where X and M are independent variables, “a is the intercept, and b and c are the slopes of X and M, respectively” and “…a so-called interaction term $X \times M$, an additional latent variable in the structural model covering the product of the independent and the moderator variable.” The figure (as shown below) illustrating the moderating effect of $d$ is adopted from [14, p.717].
Therefore, it is necessary to embrace the direct effect of educational profile on arrival when we use the partial least squares (PLS) technique to test the moderating effect of educational profile on the relationships between population size-arrival and age profile-arrival.

Comment 7

“Page 5, paragraph 1:

(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.
(2) 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?
(3) How does this relationship hold together when correlated against income?”

[Our response]

Thank you for this helpful feedback. The first paragraph on page 5 of the previous version relates to the hypotheses that educational profile directly affects arrival and moderates the effects of population size and age profile on arrival. Our responses to the abovementioned enumerated points are as follows:

(1) As age may confound the relationship between education and health outcomes, we have tested the hypothesis that age profile may have a potential moderating effect on the relationship between educational profile and arrival based on our secondary data. The PLS test result is shown below.

Figure 2 PLS test results for age profile moderating the relationship between educational profile and arrival.
As Figure 2 reveals, age profile weakens the effect of educational profile on arrival, suggesting that the more seniors there are in an area, the weaker the negative effect of educational profile on arrival.

(2) In this study, based on the 2006 Canadian Census data (http://www.statcan.gc.ca/start-debut-eng.html), the proportion of the population aged 50 and over with an education level greater than high school is about 35%, and the proportion of the population aged 25 to 49 with an education level greater than high school is about 56%, indicating that the population younger than 50 are better educated than those over 50.

(3) According to our secondary data, the neighborhood age profile (measured by the proportion of population aged 50 years and above) is significantly correlated with income (measured by the proportion of population with $30,000 and above), with a correlation coefficient of $r = -0.168$ ($p<0.05$). A possible explanation is that most seniors are retired and have limited income. This finding is consistent with the work of Luong and Hébert [15], which shows that annual income declines after age 50, based on the labor and income data collected between 1990 and 2004 from Statistics Canada.

Thus, educational profile is positively, but not significantly, correlated with income at a correlation coefficient of $r = 0.092$ ($r>0.05$). Although the correlation is insignificant, the positive relationship still shows that to some extent, the more educated individuals an area has, the higher its income will be.

We have improved the reasoning for proposing hypotheses related to the direct and moderating effects of educational profiles (Please refer to pages 5-6 of the revision for these updates).

Comment 8

“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.”

[Our response]

Thank you very much for this valuable comment. We have removed the hypotheses related to the effects of income profile on arrival for the following reasons.

(1) We agree with the reviewer that we do not have the insurance data to support the claim that higher-income is linked to a higher rate of medical insurance.
(2) The census data do not clearly state how to define “income”, e.g., can a retirement pension be regarded as income? This ambiguity may lead to inferior results. Please refer to the Literature review and research hypotheses section on pages 3-6 for these changes.

Comment 9

“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?”

[Our response]

The work conducted by Seidel et al. [5] (reference 35 in the previous version, and 20 in this revision) notes that patients’ willingness to use healthcare services is negatively associated with the distance from their residence to the destination hospital. Similar conclusions have also been drawn from a survey conducted by the Cardiac Care Network (CCN) of Ontario (see the extension in the last paragraph on page 4 and reference 21 for details).

Based on Seidel et al. [5], we argue that “if there are several accessible hospitals in one area, it is likely that the patient arrivals for any one particular hospital may decrease, as the difference in the time needed for patients to travel to one hospital versus another is negligible”, rationalizing the hypotheses regarding the direct and moderating effects related to service accessibility.

We have clarified our statements and arguments. Please refer to the last paragraph on page 4, and the first two paragraphs on page 5 for the improvements.

Comment 10

“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.”

[Our response]

Thank you for the feedback. We have added general descriptive statics to illustrate how patients residing in one LHIN are dispersed to other LHINs. Please refer to the last paragraph on page 11 for these changes, and for more detailed statistics, please refer to reference 26 of this revision.
Comment 11

"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?"

[Our response]

Thank you very much for the valuable suggestions. This revision uses a 30-minute driving time (based on the work of Bosanac et al. [2] and Chan et al. [3], and a recommendation from the Cardiac Care Network of Ontario [4]) as the threshold to measure geographic accessibility to healthcare services. Please refer to the last paragraph on page 7 for the corresponding definition and justification. In addition, please refer to Table 1 for the differences in service accessibility for each LHIN, and Table 3 for the mean and standard deviation for service accessibility.

Comment 12

"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?"

[Our response]

We agree with the reviewer and have used a consistent population cut-off of 40,000 when calculating driving time for service accessibility approximation, and when sampling cities/towns from which to draw other geodemographic profiles (i.e., population size and age and educational profiles) for LHINs. This threshold was determined such that the cities/towns included in our samples represent approximately 90.72% of Ontario's population (as shown in the new Figure 2).

The driving time is estimated using the "Get directions" function in Google Maps. Please refer to the last paragraph on page 7 for the detailed measurement method.

Comment 13

"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).”

[Our response]

The unit used for the median wait time for urgent/semi-urgent/elective patients is “day”. We have added this information to the text and the associated tables. Please refer to the first paragraph on page 8, Table 2 and the new Table 3 for these revisions.

Comment 14

“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?”

[Our response]

In our study, wait time (as one of the healthcare service characteristics) is a latent variable – a construct that cannot be measured directly but can be assessed by multiple observed variables (that represent different dimensions of the latent variable). It is measured using four observed variables, i.e., the median wait time (in days) of urgent/semi-urgent/elective patients, and the queue length. Each of the four observed variables represents one dimension of wait time.

Please refer to the first paragraph on page 8, and Figure 1 for the statements and illustrations on wait time measurements. In addition, please refer to Tables 2 and 3 for a basic data summary of the four measurement variables.

Comment 15

“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).”

[Our response]

Thank you for the insightful comments. We have improved the discussion section. Some major improvements include:

(1) We have removed the discussion regarding the relationship between immigrant status and patient arrival, as suggested.
(2) We have extended the discussion on result interpretations and identified LHINs with different geodemographic profiles to address the implications of our findings. We have extended the discussion on research limitations to include neighborhood conceptualization, secondary data for investigation, data collection and statistical methods and research context.

Please refer to the Discussion section on pages 10-14 for detailed information.

Comment 16

“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).”

[Our response]

We have removed any inappropriate content that is not mentioned in the Background, Methods or Results sections. Please refer to the Discussion section on pages 11-14 for these updates.

Comment 17

“Abstract, background section: ‘ageing’ is misspelled.”

[Our response]

Thank you for pointing out this mistake. All instances of the misspelling, “ageing”, have been replaced with “aging” in the revision.

Comment 18

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

[Our response]

Thank you very much for this suggestion. The term “integrated” in the Abstract and Methods sections have been replaced by the term “aggregated”.
Comment 19

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

[Our response]

As mentioned in our response to Comment 5, we have replaced any words that may lead readers infer causal relationships between two variables with terminology such as “associated with”, “related to” or “linked to” throughout the manuscript.

Comment 20

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

[Our response]

In the previous version, the term “geosocioeconomic” is used to represent socioeconomic and geographic factors, e.g., service accessibility and educational profile. To clarify the key terms and avoid confusion, we have removed this term. Please see the Abstract, Background and Conclusions sections for these changes.

Comment 21

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

[Our response]

In the previous version, the last sentence in the last paragraph on page 2 is: “Such socioeconomic related geodemographic determinants are strongly associated with the lifestyles and healthcare service utilization behavior [6–8], and hence they may exert impact on healthcare service characteristics, such as demand, operating room capacity, physician supply, and wait time.”

We have added the appropriate reference (reference 13) to the revision. Given that the writing has been substantially improved, please refer to the first paragraph on page 4 of the revision.

Comment 22

“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.”

[Our response]

Thank you for this valuable suggestion. The writing has been significantly improved, so please refer to the Background section on pages 2-3 for the improvements.

**Comment 23**

“Page 3, paragraph 1: define ‘geographic level’.”

[Our response]

In the previous version, “geographic level” was used to differentiate between the literature on uncovering impact factors influencing arrivals of individual people and those of areas. The term “geographic level” has been removed as part of our revision process. Please refer to the Background section to note its absence.

**Comment 24**

“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?”

[Our response]

We have removed the specified content. Please refer to the Background section for the update.

**Comment 25**

“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.”

[Our response]

Thank you for this suggestion. As we note in our response to Comment 24, we have removed the specified content. Please refer to the Background section for the update.
Comment 26

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

[Our response]

Figure 2 in the previous version was generated using Google Maps. However, because we have already stated that our sampled cities/towns are those with populations above 40,000 according to the 2006 Canadian Census data (please refer to the last paragraph on page 6 in the revision), Figure 2 overlapped the text and we have removed it from this revision.
References


- End of responses -