Article details: 2018-0004	
Title	Disparities in health outcomes among seniors without a family physician in the North West Local Health Integration Network: a retrospective cohort study
Authors	Alexandrea Peel BSc MD, Iris Gutmanis PhD, Trevor Bon MD
Reviewer 1	Daniel Kobewka
Institution	Department of Epidemiology and Community Medicine, University of Ottawa, Ottawa, Ont.
General comments (author response in bold)	Very interesting paper that explores the demographic, health utilization and outcomes for people living in a northern LHIN. The lens of family physician (Yes/No) is interesting but any direct claims about causality are over reaching. **We've reworded a number of sentences to make sure that we are not suggesting/inferring causality.** P8L32 should be changed. **This sentence has been removed.** As the limitation section lays out there are unmeasured confounders that likely explain some or all of the associations seen. There are no references to other papers that explore the health outcomes of "orphan patients" or "orphan elders". There must be other data on this. Please related your work to other literature in the field. **We've tried to relate our findings to the scant previous literature examining the impact of having/not having a family physician. While there are a number of papers that have used survey data to examine the characteristics of those who have/lack a family physician, we could not find any that focused on seniors. In addition, in this study we looked at those who were actually rostered with a family physician, not on those who self-reported access to a regular family physician.** **As well, there are a number of papers that have examined the impact of continuity of primary care (impact of information sharing, healthcare provider coordination, and relational continuity (i.e. number of visits with the same healthcare provider) on health, this study did not focus on seeing the same family physician each time, but rather on just being rostered with a family physician.** **As a result, only one paper could be found that looked at seniors who had been admitted to a medical service and the impact of having/lacking a family physician on 3-month readmission.** **As well, we now compare the other study finding, such as the proportion who died during the index admission, to other published results.** The stated primary objective "assess the differential risk of in-hospital mortality and one-year mortality" should be the main focus throughout the paper. The focus seemed to drift from this. **We've tried to make sure that this remains the focus of the paper.** Abstract: You performed regression analysis to risk adjust but the abstract does not mention this. A line in the methods stating that you performed regression analyses would be helpful. **This has been added to the abstract.** P6L25: NNH and Absolute risk difference should not be in this part of the results. These are just crude associations. NNH suggests causation and should only be used when there is a clear exposure or treatment administered. **As no clear treatment was administered and causality cannot be inferred, all references to NNH have been removed from the paper.** P15 Table1: P-values should not be included in table 1. With such a large N and with groups that are systematically different (with and without a family doctor) all covariates will be significantly different from each other. **P-values have been removed from Table 1.** P7L36 - This paragraph would fit better in the discussion section. **This paragraph has been moved to the discussion/interpretation section** Minor edits: P4L38: the word 'one' is referring to a family physician but this is not clear. State 'family physician' **This has been changed.** P6L55: review the grammar of this sentence. The and is not at the right place. **This sentence has been changed.**
Reviewer 2	Patricia Li
Institution	Department of Pediatrics, Montreal Children's Hospital, McGill University Health Centre, Montréal, Que.
General comments (author response in bold)	Overall comments Overall, this study has potential to contribute data on primary care and outcomes for elderly adults in an important and populated rural/remote area of Ontario. However, as written, there are major limitations and missing information to impact on my ability to appropriately evaluate the study and its findings. Major points 1) The methodology is significantly lacking. **Considerable detail has been added to the methodology section. All databases that were used to generate the study data file are now described.** a) It is unclear why this is a secondary data analysis of a cohort – there is no reference for this cohort or description of it. **This is a secondary analysis of data collected by other institutions primarily for administrative purposes. The cohort that was assembled for this study is now described in detail.** b) Along the same comment, the authors do not describe the data sources and linkages: it is stated in the acknowledgments, but not in methods, that ICES data was used – but was that it, or were patient charts also used? If only ICES data, it would be important to describe databases used (i.e. DAD, NACRS, OHIP, etc.) and ability to linkages (i.e. throughout province). **Data bases used for this study are now described. The ICES assigned unique patient identifier was used to link data across databases.** c) The index admission is not well described. Could a patient have more than one index admission (and be counted multiple

times?)? How did the authors ensure that readmissions not counted as index admissions?
The index admission is the first admission during the study period. As ICES assigned numbers are unique, each person could only be in the database once.

d) In terms of the primary exposure, there is no description of how primary care physician status was determined. As well, when were all the variables determined? (at the time of the index admission?)
Details are now provided regarding how primary care physician status was determined. Most study independent variables were determined at the time of the index admission. However, the Charlson Comorbidity Index score was based on information in the Discharge Abstract Database in the two years prior to the index admission date. One-year readmission and one-year mortality were based on the index discharge date.

e) For the analyses: NNH, although mostly a concept used for clinical trials, can be used in observational studies. They should be adjusted and not reported as currently reported in abstract, results, and discussion (Austin PC. J Clin Epidemiol 2010; 63(1):46-55 and 2-6).
As the previous reviewer points out, NNH suggests causation and should only be used when there is a clear exposure or treatment administered. All references to NNH have been removed.

f) Also, why did the authors not explore effect modification? Even with the variable rural yes/no (and without details of remote), it seems that there may be an effect modification by rural status (if I understand correctly, that it takes 3 hours to see primary care service for rural/remote patients)?
The reviewer is correct; effect modification, specifically, statistical interactions between rural/urban status and physician status could be examined. It is suggested that this be examined in future studies.

2) The interpretation would read better if the authors used the format suggested by CMAJ open (Brief summary of the main results of the study (one paragraph); Explanation of the findings; comparison and contrast of findings with other related studies in the literature (one or two paragraphs); Limitations of the study (one paragraph); Conclusion and future directions in the area of the study (one paragraph))
The interpretation section has been changed to this format.

Minor points
Intro:
Line 9: regions should be region's
In an effort to reduce the introduction to two paragraph's, the statistic previously referenced in Line 9 has been removed.

Line 36-37: define Health Care Connect for broad readership
A brief description of Health Care Connect is now provided (an Ontario Ministry of Health and Long-Term Care program designed to help Ontarians without a primary health care provider find one).

Methods
Page 6/25 (labelled page 5):
- Were subjects 65 and over (not just over 65), as per table 1
The study did include people 65 years of age and older. The first sentence in the methods section now states: A retrospective cohort of all adults 65 or more years of age who, at the time of their index admission, were living in a private residence and had been admitted to a medical service at TBRHSC, was created by Data and Analytic Services at the Institute for Clinical Evaluative Sciences (ICES).

- Lines 18-20: I agree that surgical and psychiatric admissions differ from medical ones, but LOS is adjusted for and doesn't seem to be a good reason for excluding. I would assume that reasons for mortality and readmissions differ in surgical/psych admissions compared to medical ones, which is why exclusion makes sense.
Reasons underpinning mortality and readmissions among seniors admitted for medical reasons differ from those among patients admitted for surgical and psychiatric reasons. The text has been changed to reflect this point: Surgical and psychiatric admissions were also excluded as reasons underpinning mortality and readmission differ from those of medical patients.

- The income quintiles are not explained (Q1=low or high?)
Q1 is low income. This has been added to Table 1.

- Line 48-49: missing another bracket after Appendix A
As the wording in this section has changed, there was no longer any need for this suggested change.

- Line 51-52: as written, a bit confusing; which version of the CCI was used? The one by Quan? Sundararajan? ICES? Or are they all the same?
More detail has been added as to how the Charlson Comorbidity Index was calculated. This section now states: The Charlson Comorbidity Index (CCI) score was calculated using the methodology initially described by Deyo et al who used ICD-9 codes from administrative data and subsequently adapted for use with administrative data using ICD-10 codes by Quan et al, Sundararajan et al, and ICES (personal communication). Information on comorbidities was based on information in the DAD in the two years prior to the index admission date and on the adjusted diagnostic groups based on information in the DAD, NACRS, and OHIP databases.

Page 8/25 (labelled as page 7): lines 8-10: "despite the associations of age, sex, income, co-morbidity and one-year mortality" with what? Each other?
We agree that this statement is confusing. As a result, this sentence has been removed.

Results
Table 1: Number (N) for income quintile 1 under orphan elders is probably wrong (doesn't add up to column total of 4899 or to row total of 2987).
This has been corrected.

Table 2: lots of extra statistics at the bottom that I don't think need to be there; the p-values seem redundant; formatting errors

(e.g. Income quintile missing vs. 1; lines are not correct separating rows in table); what is the total N for each column (they are displayed in Table 1 but should be present in Table 2).
Table 1 has been reformatted. As well, all references to p-values have been removed. Instead, as noted above, the following sentence was added to the methods section: Inferential bivariate comparison statistics (e.g., $\chi 2$ test, Student's t-test) were not calculated as the study was population-based.

Table 3: I'm not sure that it's necessary or appropriate to compare the unadjusted to the "full" model, when the authors have chosen to use forward-step wise regression and all covariates were included.
Table 3 has been removed.

I also disagree with the statement that covariates had "little influence" on the OR and HRs; for example, unadjusted HR for one-year mortality was 50% more than adjusted (1.21 vs. 1.14).
We agree and have removed that sentence from the paper.

Interpretation
The authors state there is a discrepancy of the current study findings with reference 4: could there be an explanation why? For example, different methods were used to ascertain primary care provider status?
In an effort to consolidate the interpretation section, the sentence that used reference 4 has been removed. However, it is possible that while overall population figures suggest that in the North West LHIN 20% of people do not have a family physician, that this figure is higher among older people, based on studies that suggest that family physicians are hesitant to take on older adults.

Also, it seems there could be further elaboration of how the current study compares to other studies - are there really no other studies looking at presence/absence of family physician on mortality and readmission? if not, then perhaps further elaboration on the concept of continuity of primary, or what is the mechanism by which primary care is reducing morbidity/mortality in this cohort?
As now noted in the interpretation section, while some studies have examined the characteristics of those who don't have a family physician, often using survey data, and other studies have looked at the impact of continuity of care on health outcomes, only one study could be found that focused on the impact of family physician status on readmission rates among older Canadian adults. Contrary to our study findings, this study found that older adult without a family physician had fewer readmissions in the three months following hospital discharge. However, they acknowledge that they did not include key covariates in their analysis.

Page 9 (10/25):
- line 10-12 needs further elaboration; unclear how "ICU" studies relate to patients transferred to TBRHSC;
As we were very challenged to find papers that discussed readmission of older adults with/without family physicians, this paper seemed to be at least somewhat relevant to the discussion. However, any reference to this paper has been removed as it does not help position this body of work within the appropriate body of knowledge.

- lines 35-36: needs clarification: for those not well-versed in rural vs. remote regions, what is the definition/dichotomy? And the estimated travel time of 3 hours, does that apply to both rural and remote?
All reference to rural vs. remote has been removed to avoid any confusion.

Page 10 (11/25): lines 10-12; I think the outcomes of in-hospital mortality are even more compelling, and so the results may support improving access to primary care physicians for older persons (and not just recently hospitalized ones – i.e. need to get to them before they are hospitalized).
We agree with the reviewer comments. This section and the conclusions are now more focused on the need to improve access to primary care physicians even before hospitalization.

Also, in that sentence, I think the word "legitimize" may be overstated.
This has been removed.

Appendix A:
Need to explain CMA and CA and DA to readership.
Statistics Canada definitions of the terms CMA, CA and DA have been added to Appendix A.

Episode of care: not clear to reader who doesn't know Ontario health admin data; what are the "records" being looked at; CIHI-DAD?
Details regarding how episode of care was determined have been added.

STROBE statement
- Along with the description of data, the authors state that there was no missing data; but there was missing data in the income quintiles;
The reviewer is correct. There was some missing data in the income quintiles. This will be changed.

- Lost to follow-up: it is unclear whether patients were excluded if they did not have OHIP coverage/data for all years of study/follow-up period
All of the patients who were identified as having an index admission had one-year follow-up information. No patients were lost during the year following discharge from the index admission.

Statistician comments:
1. Please include all estimating and testing procedures in the statistics section of the methods.
a. All statistical methods (estimates and tests) associated with the Cox-proportional hazards model and the logistic regression model should be identified in the statistics section. For example, the test for the Proportional Hazards Assumption is only noted in the results; c-statistics and tests in footnotes of tables and figure are not in methods section; indicate the effect estimates (ORs and HRs) computed and 95% confidence intervals considered; was the log-rank test used to compare the two groups.
Considerable detail has been added to the methods section describing which tests were used to evaluate which regression models.

b. Please include proportions, means, standard deviations, ranges (why not interquartile range) among the descriptive statistics

	listed. **Table 1 now includes means, standard deviations, and interquartile ranges.** c. The models should be described as multivariable and not multivariate. **This has been changed throughout the text.** 2. For Table 1 consider using the standardized difference and not p-values in comparing the two groups since the sample size is so large (e.g. age difference between groups is only 0.3 years but is statistically significant because of the large number of patients involved but not clinically important). **We agree that statistically significant differences may not reflect clinically meaningful differences when we have such a large sample size. As a result, no tests of significance are now included in Table 1.** 3. If important unmeasured confounders are missing, these should be identified, and potential direction of bias identified (if possible); and then added as a limitation. **This has been added to the paragraph describing study limitations.** 4. When discussing and interpreting results, take care in properly describing odds ratios (avoid indication of risk and state odds); and avoid indication of 'causality', the best we can have here is 'association'. **We have tried to avoid any indication of causality and have tried to describe only associations that could be examined in future prospective studies.**