ARTICLE DETAILS

TITLE (PROVISIONAL)  Investigating the Effect of Sociodemographic Factors on 30-day Hospital Readmission among Medical Patients in Toronto, Canada: a Prospective Cohort Study

AUTHORS  Smith, Robert; Kuluski, Kerry; Costa, Andrew P; Sinha, Samir; Glazier, Richard; Forster, Alan; Jeffs, Lianne

VERSION 1 – REVIEW

REVIEWER  Alison Mudge
Royal Brisbane and Women's Hospital
Australia

REVIEW RETURNED  22-Jun-2017

GENERAL COMMENTS  Thankyou for the opportunity to review this paper examining prediction of 30 day readmission in a Toronto general medical service. The study utilised a program of prospective patient survey to collect data commonly missing from studies of readmission which often just use routinely collected clinical and demographic data. These included education, literacy, religion, and sexual preferences, as well as disability and self-rated health. They investigated association with 30 day readmissions, and found that only previous hospital utilisation patterns predicted readmission. Although the survey provided a useful opportunity for this study, it does pose methodological problems which significantly weaken the conclusions that can be drawn. It is unclear the number and nature of non-surveyed participants; the authors suggest that “about 75%” of general medicine patients were surveyed but with 5 day per week survey and exclusion of sick patients (I note that less than 20% had any comorbidity, which is highly unusual for a general medicine population)this seems optimistic. This is likely to be a significant source of bias. There is not clear justification for the choice of variables in relation to the outcome e.g.why might religion or sexual preferences influence readmission; and why use three variables to represent literacy? Unfortunately this makes it sound like a fishing exercise with available data rather than a carefully considered testing of hypothesised predictors based on literature or plausibility. There are also some weaknesses in variable definition and measurement; for example the disability construct is very diverse and open to interpretation by surveyor and respondent. I had difficulty understanding the justification for the HARP index as an aggregated tool, unless the intention is to validate the tool (which is not a stated objective); otherwise I think the elements should be considered separately or the variable is being handled very differently in analysis to all the others.
Overall I think a clearer conceptualisation of the constructs being tested and their relationship would be helpful, rather than attributing the negative findings to confounding. As acknowledged, missing data was a major issue for some variables, particularly when it numerically exceeded categories of interest (e.g., race, sexual orientation, income). There is no statement regarding sample size and power; although there are 205 events, the large number of multi-level variables probably mean that the adjusted model is overfitted. I am not familiar with the AIC method so cannot comment on the interpretation, but it seems that neither model explained variance in the outcome usefully. I think the discussion and conclusions speculate beyond the data. Because of the weaknesses limiting conclusions from this study, I suggest that it might better be formatted as a short report.

REVIEWER
Yang Cui
University of Manitoba
Canada
REVIEW RETURNED
28-Jun-2017

GENERAL COMMENTS
This is an interesting topic. The manuscript is easy to read and the analysis is appropriate. I think the manuscript could be improved in a number of ways given below.

INTRODUCTION
• P4 L21, factors such as age and gender are usually captured by health administrative data and other data source. Other factors authors mentioned might have available limitations such as from health administrative data.

METHODS
• In general, statistical textbooks advice against categorization of data. Why was age categorized in this study?
• Why was Charlson used? The original weights are out of date, and Elixhauser may perform better.
• Are Charlson comorbidity index and HARP two correlated variables? Did you check multicollinearity of your model?
• Are there any significant covariate interactions, such as age and gender, etc?
• The Cox proportional hazards assumption was not mentioned in the manuscript and is this assumption satisfied?
• Authors used disability score as an independent variable; please provide details about what kind of disabilities include? Is it a composite score?

RESULTS
• Table 1 should present p for difference between two groups. This will allow us to show the relationship between independent factors and unplanned readmission in a descriptive way.

DISCUSSION
• P8 L6, authors said their findings had several clinical, policy implications, however, many patient-level sociodemographic factors are found not associated with readmission. In addition, as this study was conducted from one hospital in Toronto, it is not sufficient to make the statement.
Reviewer 1: Dr. Alison Mudge

Thank you for the opportunity to review this paper examining prediction of 30 day readmission in a Toronto general medical service. The study utilised a program of prospective patient survey to collect data commonly missing from studies of readmission which often just use routinely collected clinical and demographic data. These included education, literacy, religion, and sexual preferences, as well as disability and self-rated health. They investigated association with 30 day readmissions, and found that only previous hospital utilisation patterns predicted readmission. Although the survey provided a useful opportunity for this study, it does pose methodological problems which significantly weaken the conclusions that can be drawn.

1) It is unclear the number and nature of non-surveyed participants; the authors suggest that “about 75%” of general medicine patients were surveyed but with 5 day per week survey and exclusion of sick patients (I note that less than 20% had any comorbidity, which is highly unusual for a general medicine population) this seems optimistic. This is likely to be a significant source of bias.

Response:
Thank you very much for your time and careful consideration of our manuscript. As mentioned in response to editorial request 2, an exact figure for the number and nature of patients who did not participate in the survey was not attainable due to inconsistencies in non-response recording between July 2013 and July 2014. We acknowledge that not knowing the number and nature of non-surveyed patients introduces uncertainty around the representativeness of our sample. Two additional steps were taken to assess the representativeness of our sample.

First, we conducted post-hoc descriptive analyses of the clinical characteristics of the Mount Sinai Hospital general medical patient population admitted within our observation period and discharged home. There were no substantial differences between our sample and this population in terms of age, gender, primary diagnosis, inpatient length of stay, and general comorbidity level (i.e. not restricted to Charlson comorbidities)(Admissions data acquired from the Mount Sinai Hospital Measurement and Performance Reporting Department). Next we cross-examined the characteristics of our sample with medical patients included in previous hospital readmission studies conducted in Ontario, Canada. Our sample was comparable to a recent population-based sample of medical and surgical patients.(1) Based on these analyses, and the distribution of our independent and dependent variables, we believe that non-response bias likely does not substantially affect the integrity of our findings.

In order to align with previous research, and the Canadian Institute for Health Information’s 30-day medical readmission rate inclusion/exclusion criteria we only included those who were discharged home or home with services. This exclusion criterion likely contributed to the low proportion of patients with Charlson comorbidities represented within our sample. It was however noted that the median number of general comorbidities observed in this sample was two. We believe the alignment of our methodology with that of previous research and performance indicators enhances the utility of our findings for clinicians and policy makers.

Clarification around the response rate, and discussion regarding the representativeness of our sample has been elaborated on in our manuscript on page 5, paragraph 1 (Methods, Study Design and Data Sources) and page 10, paragraph 2 (Discussion).
2) There is not clear justification for the choice of variables in relation to the outcome e.g. why might religion or sexual preferences influence readmission; and why use three variables to represent literacy? Unfortunately this makes it sound like a fishing exercise with available data rather than a carefully considered testing of hypothesised predictors based on literature or plausibility.

Response:
Anderson’s Behavioural Model was the conceptual model guiding our careful selection of variables and subsequent analysis. This has now been clarified on page 5 and 6 (Methods, Sociodemographic Variables and Covariates).

Before analyses were completed, an extensive review of literature was conducted to determine which of the variables included in the Measuring Health Equity survey should be examined in relation to hospital readmission. This review is presented in chapter 2 of Smith.(2) From this review, sexual orientation was the only variable that had not been investigated in relation to hospital readmission among medical patients. We believe that including this variable is justified in light of recent data indicating potential differences in healthcare seeking behaviour and access to regular medical care among those who identify as homosexual and bisexual compared to heterosexual.(3) These points of rationale are now reflected on page 6 (Methods, Sociodemographic Variables and Covariates)

It should be noted that of the three variables related to literacy, only primary spoken language was included in the analyses due to issues of multicollinearity. This information has been moved from the Notes of Table 2 to page 7, paragraph 1 (Methods, Statistical Analysis). The decision to include primary spoken language was supported by previous research.(4) Primary reading language and English proficiency remain in Table 1 for descriptive purposes.

3) There are also some weaknesses in variable definition and measurement; for example the disability construct is very diverse and open to interpretation by surveyor and respondent.

Response:
To maximize the comparability of our results with other studies, the structure of variables studied were informed by previous research.(2) Risk of measurement error particularly for the disability variable was acknowledged and discussed in the Discussion (page 9, paragraph 3). Each variable is defined in Supplementary Materials, Table I.

4) I had difficulty understanding the justification for the HARP index as an aggregated tool, unless the intention is to validate the tool (which is not a stated objective); otherwise I think the elements should be considered separately or the variable is being handled very differently in analysis to all the others.

Response:
The primary purpose for using the HARP score as a composite measure was to achieve parsimony. HARP’s constituent variables were modelled separately in multivariable model 2 to better understand which of these variables were most influential on the incidence of readmission within this sample.

5) Overall I think a clearer conceptualisation of the constructs being tested and their relationship would be helpful, rather than attributing the negative findings to confounding.

Response:
Andersen’s Behavioural Model(5) was the conceptual framework theoretically grounding our analyses and the constructs being examined. This has now been clarified on page 5 and 6 (Methods, Sociodemographic Variables and Covariates).
6) As acknowledged, missing data was a major issue for some variables, particularly when it numerically exceeded categories of interest (eg race, sexual orientation, income).

Response:
We agree, missing data is an important limitation that as discussed merits attention. We have elaborated our original acknowledgement of this limitation on page 10, paragraph 2 (Discussion).

7) There is no statement regarding sample size and power; although there are 205 events, the large number of multi-level variables probably mean that the adjusted model is overfitted.

Response:
Expecting a 14% to 15% event rate (Mount Sinai Hospital's publicly reported 30-day medical readmission rate during observation period), we anticipated having statistical power to examine 19 to 21 variables. We believe that each multivariable regression model was sufficiently powered since less than twenty independent/covariate variables were included in our models (i.e. 10% of the frequency of observed readmission events). This has now been clarified on page 6 in Methods, Statistical Analysis. However, it is worth noting that conservative rules of event-variable guides have been revised, now suggesting that 5 to 9 events per covariate are adequate, and particularly were variables are employed to control for confounding.(6) Also, as discussed on page 6, paragraph 2 (Methods, Sociodemographic Variables and Covariates) to maintain power and reduce the influence of statistical noise, multi-level sociodemographic variables were aggregated to ensure at least 50 participants were observed at each level while still maintaining meaningful categories.

Model overfit is certainly a risk of studying several multi-level categorical variables. It should be noted that, by in large, unadjusted effect estimates among sociodemographic variables were not substantially different from the adjusted effect estimates. The model fit characteristics were also well within acceptable limits.

8) I am not familiar with the AIC method so cannot comment on the interpretation, but it seems that neither model explained variance in the outcome usefully.

Response:
Like the Bayesian information criterion (BIC), the Akaike information criterion (AIC) is a measure of the relative quality of statistical models for a given set of data. Indeed the AIC values indicate that model 2 explained variance to a greater degree than model 1. This is discussed on page 8, paragraph 2. However, AIC values cannot be used to make general inferences regarding the utility of a model.

9) I think the discussion and conclusions speculate beyond the data. Because of the weaknesses limiting conclusions from this study, I suggest that it might better be formatted as a short report.

Response:
Thank you Dr. Mudge for your thoughtful review of our manuscript. In light of our study's limitations, we have prefaced our findings with notes of caution and present important areas for further inquiry. However, we believe our discussion and conclusions remain informative and fair for the following reasons: 1) The methodological approaches we employed closely align with previous research and national measurement standards (e.g. Canadian Institute for Health Information 30-day Medical Readmission Rate); 2) Our findings were largely consistent with relevant studies among medical patients within and beyond Canada, and contrasting findings were thoughtfully discussed; and 3) To the best of our knowledge, this represents the first detailed examination of patient-level sociodemographic determinants of hospital readmission among a Canadian general medical patient population. An expansive body of readmissions literature have examined the clinical, policy, and research dimensions of this adverse health outcome.
In order to enhance the utility of our research for the diverse readership of BMJ Open, we believe it is important to discuss the clinical, policy and research implications of our novel investigation. We therefore believe our study reflects a high level of scholarship which warrants communication as original research article.

**Reviewer 2: Dr. Yang Cui**

This is an interesting topic. The manuscript is easy to read and the analysis is appropriate. I think the manuscript could be improved in a number of ways given below.

**INTRODUCTION**
1) P4 L21, factors such as age and gender are usually captured by health administrative data and other data source. Other factors authors mentioned might have available limitations such as from health administrative data.

**Response:**
Thank you very much for your time and careful consideration of our manuscript. Yes, the limited patient-level sociodemographic data accessible through health administrative databases is an important gap that the Measuring Health Equity project seeks to address. Like administrative data, patient-level sociodemographic data has limitations. These are discussed on page 9, and have been elaborated on page 10.

**METHODS**
2) In general, statistical textbooks advice against categorization of data. Why was age categorized in this study?

**Response:**
Age was categorized in concordance with the structure of the age variable from which HARP index scores are derived.

3) Why was Charlson used? The original weights are out of date, and Elixhauser may perform better.

**Response:**
To enhance the comparability of our study with previous studies, we chose the Charlson index as it was the most commonly used index in the studies we reviewed.(2) Recognizing the original weights were out of date, we used the updated weights from Quan and colleagues.(7) This is mentioned on page 6, paragraph 3 (Methods, Sociodemographic Variables and Covariates).

4) Are Charlson comorbidity index and HARP two correlated variables? Did you check multicollinearity of your model?

**Response:**
There was no substantive evidence of collinearity between Charlson scores and HARP scores. Variance Inflation Factor (VIF) values were used as the primary means of assessing multicollinearity. To calculate VIF values, the SAS procedure “PROC GLMSELECT” with “NOINT” and “SELECTION=NONE” options was used to generate a dataset containing all independent/covariate continuous variables, and dummy variables for each level of categorical independent/covariate variables. Next a linear regression model was constructed to calculate VIF values. The variables included in this model were time-to-censor as the dependent variable, and the independent/covariate variables contained within the restructured dataset. Variables with VIF values in excess of 2.5 were flagged for potential collinearity and considered for removal from the multivariable model. The only variables warranting removal were Primary Reading Language and English Proficiency.
5) Are there any significant covariate interactions, such as age and gender, etc?

Response:
While an interesting avenue for examination, due to an insufficient body of readmissions literature substantiating sociodemographic interaction effects we did not test interaction effects.

6) The Cox proportional hazards assumption was not mentioned in the manuscript and is this assumption satisfied?

Response:
The proportional hazards assumption was assessed and not violated. Time-dependent covariates were not significant when modelled with each independent variable and covariate in separate Cox regression models. Therefore, it was assumed that hazard of readmission was proportional over the 30-day observation window for each independent variable and covariate. We have clarified this on page 7, paragraph 1 (Methods, Statistical Analysis).

7) Authors used disability score as an independent variable; please provide details about what kind of disabilities include? Is it a composite score?

Response:
The disability variable is a composite score which represents the total number of self-reported physical, sensory, learning, and developmental disabilities and/or those related to chronic illness, mental health, or drug or alcohol dependence. This description has been moved from the Notes row of Table 1 to page 6, paragraph 2 (Methods, Sociodemographic Variables and Covariates) Table I of the supplementary materials, presents the disability question as well as response choices.

RESULTS
8) Table 1 should present p for difference between two groups. This will allow us to show the relationship between independent factors and unplanned readmission in a descriptive way.

Response:
We too appreciate the value of tests of difference for assessing the relationship between an independent and dependent variable in a descriptive manner. However, we executed these tests of difference and the results were consistent with those of the unadjusted Cox models. We do not present p values for the tests of difference because we believe the p values generated through univariable Cox regression are a more robust and equally informative means of describing the relationship between each independent variables and readmission.

DISCUSSION
9) P8 L6, authors said their findings had several clinical, policy implications, however, many patient-level sociodemographic factors are found not associated with readmission. In addition, as this study was conducted from one hospital in Toronto, it is not sufficient to make the statement.

Response:
Thank you for your thoughtful review of our manuscript Dr. Cui. The generalizability limitation is discussed on page 10, paragraph 1 (Discussion) and the need for further research to assess the generalizability of our findings has been made more explicit on page 10, paragraph 4 (Discussion). In light of our study’s limitations, we have prefaced our findings with notes of caution and present important areas for further inquiry. However, we believe our discussion and conclusions remain informative and fair for the following reasons: 1) The methodological approaches we employed closely align with previous research and national measurement standards (e.g. Canadian Institute for Health Information 30-day Medical Readmission Rate);
2) Our findings were largely consistent with relevant studies among medical patients within and beyond Canada, and contrasting findings were thoughtfully discussed; and 3) To the best of our knowledge, this represents the first detailed examination of patient-level sociodemographic determinants of hospital readmission among a Canadian general medical patient population. An expansive body of readmissions literature have examined the clinical, policy, and research dimensions of this adverse health outcome. In order to enhance the utility of our research for the diverse readership of BMJ Open, we believe it is important to discuss the clinical, policy and research implications of our novel investigation.

References:


VERSION 2 – REVIEW

REVIEWER
Alison Mudge
Royal Brisbane and Women’s Hospital
Brisbane Australia

REVIEW RETURNED
03-Sep-2017

GENERAL COMMENTS
Thankyou for asking me to re-review “Investigating the Effect of Sociodemographic Factors on 30-day Hospital Readmission among Medical Patients in Toronto, Canada: a Prospective Cohort Study”. The authors have addressed some of my concerns but there are still some important problems with the paper in its current format. It is very helpful to provide the explicit conceptualization of variables using the Andersen model, but the initial definition given at top of page 6 of the third construct, need, is misleading (“the medical care processes and health outcomes experienced upon accessing care”) because in Andersen’s work (and in the current paper) this construct is “perceived and objectively measured need”.

There is the conceptual challenge now of whether previous utilization (hospitalization and ED attendances) truly reflects “need” or reflects expressed behavior, which perhaps could be discussed. That issue aside, the language and concept now needs to be consistent throughout the paper, as the study is not just of “sociodemographic variables” (which aligns with Andersen’s concept of predisposing variables) but also enabling and need variables. I think home ownership (another indicator of wealth) is an enabling variable, and note that some previous authors have also included education language as enabling although this is not consistent. Discharge destination and supports are also enabling. In this context it does not make sense to use the HARP tool, which by this definition is a composite of need and enabling variables. Nor does the testing of which model is a better fit have anything to do with the stated research question. If the authors are in fact wishing to answer the question “do other predisposing, enabling and need variables add to the HARP tool as a predictor of readmission”, then this question needs to be explicitly asked and discussed in the introduction and discussion, model 1 presented and the relative performance (not fit!) of the HARP model with and without the additional variables presented. If on the other hand (and more consistent with the tone of the paper) the authors are asking “what predisposing, enabling and need variables contribute to the risk of readmission” then model 2 should be presented. I suggest this approach is much more useful in an international context where the HARP score is not used. On a more minor note, there is no description or justification of how LOS has been used and the reporting by “median” in the model does not make sense especially in view of the large number of levels of other variables and the literature suggesting a non linear relationship between LOS and readmission. The discussion has not been altered, goes beyond the research question and the data, and needs to be more concise. It needs to be stated in terms of the research question and the underlying theoretical framework. The findings in the models presented clearly show that need variables are the only significant predictors, and this is highly consistent with the literature (despite occasional contradictory findings) and should be stated as such. The implications at least locally are that sophisticated risk stratification based on hard to measure predisposing and enabling variables would seem to be a misuse of resources. It is an overstatement that there are “compelling findings” to be made in comparison with other literature, as the literature is largely highly consistent. Paragraphs 2 and 3 are speculative. Paragraph 5 discussing transition processes is outside the scope of discussion as the modelling did not include these processes as an enabling component and so is also entirely speculative. The strengths and weaknesses are now reasonably described but could be more concise, and the conclusions are at odds with the stated findings which suggest that further investment either in larger studies or in routine collection of these predisposing and enabling characteristics is likely to yield minimal value at least in terms of readmission prediction.
Reviewer: Dr. Alison Mudge

Thank you for asking me to re-review “Investigating the Effect of Sociodemographic Factors on 30-day Hospital Readmission among Medical Patients in Toronto, Canada: a Prospective Cohort Study”.

The authors have addressed some of my concerns but there are still some important problems with the paper in its current format.

Reviewer Comment

It is very helpful to provide the explicit conceptualization of variables using the Andersen model, but the initial definition given at top of page 6 of the third construct, need, is misleading (“the medical care processes and health outcomes experienced upon accessing care”) because in Andersen’s work (and in the current paper) this construct is “perceived and objectively measured need”. There is the conceptual challenge now of whether previous utilization (hospitalization and ED attendances) truly reflects “need” or reflects expressed behavior, which perhaps could be discussed.

Response

Dr. Mudge, thank you for your very thoughtful and constructive feedback. Assuming many readers would not be familiar with Andersen’s latest adaptation of the Behavioural Model,(1) the sentence containing the excerpt you highlight (Pp.5 Ln.57 – Pp.6 Ln.6) was intended to describe the overall structure of the framework (contextual characteristics, individual characteristics, health behaviours, and outcomes). We have attempted to make this clearer (Pp. 6 Para. 2 Sent. 2). The last sentence (Pp.6 Ln.11-14) specifically addresses individual characteristics and defines need factors in a manner that we believe is consistent with the construct and your feedback (“… affecting their perceived and professionally evaluated need for healthcare.(26)”). We believe our conceptualization of previous utilization as a need factor is supported by recent evidence demonstrating that indicators of evaluated and perceived health status are primary drivers of more frequent healthcare utilization in Ontario, Canada.(2)

Reviewer Comment

That issue aside, the language and concept now needs to be consistent throughout the paper, as the study is not just of “sociodemographic variables” (which aligns with Andersen’s concept of predisposing variables) but also enabling and need variables. I think home ownership (another indicator of wealth) is an enabling variable, and note that some previous authors have also included education language as enabling although this is not consistent. Discharge destination and supports are also enabling.

Response

We agree that home ownership can also be conceptualized as an indicator of wealth, and discharge disposition an indicator of care provider availability. For these reasons, we have now included them in our analysis as enabling factors (Pp.6 Para.2 Sent.5). Considering the literature’s inconsistency, and the financial resource and healthcare organization focus of enabling factors in the Behavioural Model, we believe that education and language are appropriately conceptualized as predisposing factors.
Reviewer Comment

In this context it does not make sense to use the HARP tool, which by this definition is a composite of need and enabling variables. Nor does the testing of which model is a better fit have anything to do with the stated research question. If the authors are in fact wishing to answer the question “do other predisposing, enabling and need variables add to the HARP tool as a predictor of readmission”, then this question needs to be explicitly asked and discussed in the introduction and discussion, model 1 presented and the relative performance (not fit!) of the HARP model with and without the additional variables presented. If on the other hand (and more consistent with the tone of the paper) the authors are asking “what predisposing, enabling and need variables contribute to the risk of readmission” then model 2 should be presented. I suggest this approach is much more useful in an international context where the HARP score is not used.

Response

Thank you for these thoughtful comments. We agree that presenting our results in a manner that is consistent with the conceptual framework and as such presenting Model 2 results better aligns with the primary aims of the study. These changes are now reflected in Table 1 (Pp.11), Table 2 (Pp.13), and the results section (Pp.7-8).

Reviewer Comment

On a more minor note, there is no description or justification of how LOS has been used and the reporting by “median” in the model does not make sense especially in view of the large number of levels of other variables and the literature suggesting a non-linear relationship between LOS and readmission.

Response

Many previous studies have included LOS however at the time of our study, consensus in the literature regarding the ideal method for studying LOS in relation to readmission was not clear. Therefore, median LOS was thus chosen for the purposes of maintaining comparability in our results with previous research conducted among medical patients in Ontario, Canada.(2) This rationale is now been reflected in the manuscript (Pp.6 Para. 2 Sent. 10).

Reviewer Comment

The discussion has not been altered, goes beyond the research question and the data, and needs to be more concise. It needs to be stated in terms of the research question and the underlying theoretical framework. The findings in the models presented clearly show that need variables are the only significant predictors, and this is highly consistent with the literature (despite occasional contradictory findings) and should be stated as such. The implications at least locally are that sophisticated risk stratification based on hard to measure predisposing and enabling variables would seem to be a misuse of resources. It is an overstatement that there are “compelling findings” to be made in comparison with other literature, as the literature is largely highly consistent. Paragraphs 2 and 3 are speculative. Paragraph 5 discussing transition processes is outside the scope of discussion as the modelling did not include these processes as an enabling component and so is also entirely speculative.

Response

The findings are now presented and discussed in a manner that is consistent with the Behavioral Model. In paragraph 1 and 3 of the discussion we highlight the consistencies between our findings and previous research. Upon review, we understand how the sentence you elude to could have been interpreted as an overstatement. Briefly, our intended message was that while the literature is largely consistent, we believe insight can still be derived from the studies with contrasting findings and they thus warrant discussion. We have rephrased the sentence (Pp.8 Para.1 Sent. 4).
The discussion is aimed at the following: stimulating discussion and proposing ideas around why the examined sociodemographic factors tend not to be associated with readmission; discussing the implications our and others’ findings have for health policies (e.g. how administrators measure monitor hospital readmission rates for health system performance management), and for clinicians designing interventions to prevent readmissions. Given the multi-faceted nature of readmissions research, its implications, and the novelty of our study within the Canadian context, we believe these are fair topics for evidence-based hypothesis generation and discussion. However, we agree with your assessment of paragraph 5 and have omitted this point.

Reviewer Comment
The strengths and weaknesses are now reasonably described but could be more concise. The conclusions are at odds with the stated findings which suggest that further investment either in larger studies or in routine collection of these predisposing and enabling characteristics is likely to yield minimal value at least in terms of readmission prediction.

Response
The strengths and weaknesses section has been edited for concision and the conclusion has been re-written to align closer to our research question and findings.

References:

VERSION 3 – REVIEW

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| GENERAL COMMENTS | This revision provides a more concise and readable account of this research. |