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Title	Relationship between family physician retention and avoidable hospitalization in the province of Newfoundland and Labrador: a population-based cross-sectional study
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Reviewer 1	Dr. Michael E. Green MD MPH
Institution	Associate Professor Departments of Family Medicine and Public Health Sciences, Queen's University, Kingston, Ont.; Director, Centre for Health Services and Policy Research; Associate Director, Centre for Studies in Primary Care; Adjunct Scientist, Institute for Clinical Evaluative Sciences
General comments (author response in bold)	<p>This study addresses an important and understudied area of primary care policy - the impact of low physician retention on health service utilization. The authors used administrative data in Newfoundland and Labrador to explore the relationship between one possible cause of poor continuity of care - high physician turnover. As noted in their background section there is a well established link between usual provider continuity of care and ACSC hospitalization. They referenced a longitudinal data set as the source of data for this but did not provide a description or summary of how retention was defined. This needs to be added to the paper.</p> <p>In the Methodology section of the original submission we state that "Physician retention was the main predictor of interest and was defined as percentage of physicians practicing in a given EZ at the start of 2005 who were still practicing in the same EZ at the end of 2009".</p> <p>After the retention score was calculated they divided the province into tertiles of high medium and low retention by regional economic zones. Unfortunately, there was no map or description of where these zones are, their relative sizes both geographically or by population etc... and which ones were included in each tertile. This made it difficult to assess the potential for bias related to allocation of these zones into the tertiles and thus the impact of such potential bias on the results. Adding a figure and some information on this would be helpful to the reader and to reviewers.</p> <p>Additional information was added to the Methodology section describing EZs, a link to a map of EZs is cited and EZ populations are provided in an appendix.</p> <p>There are two other issues that I think should be further addressed. One is that as they note there is significant co-linearity between retention and rural/urban status. They do not comment on the relationship between hospitalization rates and rural/urban status, which could be another factor to consider. They did not include their stratified analysis in the paper but might consider doing so and thinking further about the potential impact of this on their results.</p> <p>We actually did comment on the relationship between hospitalization rates and rural/urban status in the Interpretation section of the original submission, which is included in the revised submission. We have also included the results of the regression analysis with urban areas removed (i.e. rural residence only) in a revised version of Table 3.</p> <p>The second factor relates to their hypothesis. Presumably as they have access to population level administrative data they can calculate UPC fairly easily. The study would be strengthened, and their argument more clearly proven if they were able to describe the relationship between retention and UPC and include an adjustment for UPC at then patient level in their model. This may or may not be feasible at this stage but would certainly be an interesting area to explore in the future if it is not.</p> <p>We are unable to calculate indices of continuity of physician care such as UPC at a provincial level because available physician claims data includes only visits to fee-for-service (FFS) physicians. Approximately 35 percent of physicians in the province are non-FFS for whom visit data is not available. These points are mentioned in the Limitations subsection of the Interpretation section.</p>
Reviewer 2	Dr. Wilson Pace MD
Institution	Director, University of Colorado, Family Medicine, Aurora, Colo.
General comments (author response in bold)	<p>This manuscript explores the relationship between family physician retention within a community and hospital admissions related to ambulatory sensitive conditions. The work seeks to add to the considerable evidence related to the impact of continuity of care and similar outcomes. Continuity typically refers to the visit to visit likelihood of seeing the same family physician. While the loss of physician to a panel of patients clearly disrupts the longitudinal nature of the physician patient relationship it does so in an entirely different way than visit to visit variations in clinical providers. The authors also make the case that practice and provincial policies related to changing family physician retention must be approached differently than those that increase visit to visit continuity. The background section, is tight and makes a good case for the research that was conducted.</p> <p>The overall approach of selecting a single province with a high background rate of family physician turnover is reasonable. The province, Newfoundland and Labrador is both fairly small in overall population and relatively rural. The rural nature appears to be highly related to overall low retention rate within selected economic zones and may lower the overall generalizability of the findings. The ability to link claims data to locale and to incorporate clinician movement by year is a strength. The use of economic zones to broaden physician catchment areas appears logical, though drive times anchored to an office location are also available (at least in the US this data is available) and may be an even better approach to defining catchment areas for a group of physicians.</p> <p>We did not have access to data on drive times anchored to an office location.</p> <p>Overall this is a minor concern. The focus on ambulatory sensitive conditions is logical. The presentation of the findings is logical. Secondary analyses that examined various factors related to hospitalization were similar in this population/analysis to other studies lending increased veracity to the results.</p> <p>The loss of 35% of the physician data in rural areas (the primary areas with low retention) is concerning. It is difficult to determine from the manuscript but logically the hospitalization data for the patients utilizing non-fee for service primary care physicians was included but the primary care data was missing. This should be clarified. If this was the case then adjustment of hospitalization rates based on Charlson scores was limited for these patients. The impact on the final results is difficult to discern. If the patients were included in the analysis then some exploration of their impact on the results would be warranted. If the patients were totally excluded then some description of these patients to the rural population in general, even if only to indicate there were no differences in age, gender, raw rate of hospitalizations would be helpful. Also, even though the clinical data from these clinicians was not available their retention rates should be.</p> <p>Just to clarify.... we had retention data (main study predictor variable) on all physicians in the province (including the 35 percent who were non-fee-for-service located mainly in rural areas) as well as health insurance registry data (including economic zone of residence) and hospitalization data on all patients. Thus, our analysis takes into account retention (and hospitalization) in these rural area where retention is the lowest. However, we did not have access to utilization data for the 35 percent of physicians who were non-fee-for-service (mostly in rural areas), and the only study variable affected by this limitation was the CCI Index, which may have underestimated co-morbidity in rural areas.</p>

	<p>Therefore, understanding if these clinicians behaved differently from other rural clinicians would be useful. While these analyses are not likely to fully elucidate the impact, if any, of these patients on the final outcome understanding how patient demographics and raw hospitalization rates and their clinicians' retention rates compare to the rural population in general could help determine if they represent significant outliers. Overall some exploration of the potential impact of these missing data is warranted but overall the current analysis is strong.</p> <p>We have no utilization data on visits to non-fee-for-service physicians and, in the retention data available to us, there was no way to distinguish fee-for-service from non-fee-for-service physicians, thus there is no way to determine differences in behavior of these two types of physicians. Patients of the non-fee-for-service physicians were included in the retention analysis as they were included in the provincial health insurance registry, which allowed us to map them to a particular EZ via a residence identifier. The only effect of not having claims data for these patients was that their co-morbidity scores may have been underestimated.</p>
Reviewer 3	Dr. Bruno D. Riverin PhD
Institution	McGill University, Department of Epidemiology, Biostatistics and Occupational Health, Montréal, Que.
General comments (author response in bold)	<p>General comments: This population-based study examined the association between physician retention/turnover and ACSCs. This question is very relevant to policy makers and clinicians, in Canada and elsewhere; unbiased evidence in support of high physician retention as a strategy to lower costs may prove useful in designing new policies or reforms. Nevertheless, major efforts need to be made to 1) better position results within the scientific literature (e.g., importance of considering physician turnover, how it is linked to continuity of care and evidence, what is the state in Canada and NL, what are the gaps in the literature that this study fill?);</p> <p>In the original submission we indicated in the Introduction section that, "physician turnover, which involves a physician leaving clinical practice in a specific area, may diminish opportunities for establishing trusting physician-patient relationships and reduce quality of communication and information needed for care, which may in-turn lead to poorer health outcomes and greater health care utilization including preventable hospitalization" and point out that we were unable to find studies on effects of physician retention/turn-over on hospitalizations. The latter is the main gap in the literature that we are trying to fill. We also state that the province of NL has a high physician turnover rate. In the Introduction of the new version we better explain the link between retention/turn-over and continuity (turn-over is actually an aspect of continuity of care) and relate the state of physician retention in NL to that of other provinces.</p> <p>2) make objectives and hypothesis clearer and provide rationale (e.g., why subgroup by 65yrs old and over, by controlling for GP capita?);</p> <p>To avoid confusion we removed the regression results for models excluding the 'GPs per capita' variable (and all references to it in the text). We retained the analysis for those age 65+ (including the GP per capita variable) because we expected the relationship between retention and hospitalization to be stronger in seniors than in the all-age analysis. The rates of social complexity and multi-morbidity are higher in this population, and we feel that GPs with improved retention/continuity have a better understanding of these conditions and a better ability to mitigate their effect on hospitalization.</p> <p>3) to align methods with objectives; We are not certain exactly what the reviewer means by this given that there is really only one objective, (i.e. to investigate the association between physician retention and hospitalization for ACSCs).</p> <p>4) present results more clearly in table and say what estimates mean; and Additions were made to tables to more clearly explain results, and meaning of estimates were stated in results.</p> <p>5) to not overstate implications given the limitations. Abstract and Interpretation were revised so as not to overstate the implications. See point #9 for Abstract (on next page).</p> <p>Abstract</p> <p>1. P2L21-22 : Sentence about exclusions may be omitted. Sentence about exclusions omitted.</p> <p>2. P2L26: Define ACSC before using acronym. ACSC defined before using acronym.</p> <p>3. P2L33-37: Remove mention of 'inverse relationship'; rephrase to make clear that the comparison group are EZ zones with highest retention rates, e.g.: Compared to areas with high physician retention rates, those with moderate and low rates had 28% and 35% lower rates of ACSC hospitalizations, respectively. The term 'inverse relationship' was changed to 'negative relationship' and results rephrased to make it clear that the high retention EZs were the comparison (i.e. reference) group.</p> <p>4. P2L40-42: The relevance of sub-group analysis on seniors, and the information about additional adjustment by number of family physicians per capita is unclear; see my comments below. As previously stated, to avoid confusion we removed the regression results conducted without inclusion of the 'GPs per capita' variable (and all references to it in the text). We retained the analysis for those age 65+ (analysis including GPs per capita variable). We point out that we expected the relationship between retention and hospitalization for ACSCs to be stronger in seniors than in the all-age analysis Because the rates of social complexity and multi-morbidity are higher in this population, and we feel that GPs with improved retention/continuity have a better understanding of these conditions and a better ability to mitigate their effect on hospitalization</p> <p>5. P2L47: Replace word 'demonstrate' by 'suggests' Suggested replacement made.</p> <p>6. P2L49: Replace 'reduce' by 'lower rates of' Suggested replacement made.</p>

7. P2L49: 'even after controlling' – controlling for confounding is essential, not a proof that a relationship exists. Consider removing this statement or re-phrasing.

Phrase 'even after controlling' removed as suggested.

8. P2L49-54: 'This is consistent...'; Re-phrase to make clear that this is your hypothesis, to my knowledge and according to your literature review, the link between high physician turnover and poorer continuity of care has not been supported by evidence elsewhere. Also, use more accurate wording, e.g. high physician turnover, poorer continuity of care, etc.

In the Introduction we present available literature/evidence supporting/pointing to the possible link between retention/turn-over and continuity. The wording was re-phrased in a way which we feel makes our hypothesis clear and references were provided with evidence of the association between turnover and continuity of care. Also, more accurate wording was incorporated (e.g. 'high physician turnover'). We also explicitly state our hypothesis in the Conclusion section of the Abstract.

9. P3L3-6: The implications of this research are slightly overstated in light of its limitations.

Agreed. The word 'cost-effective' was removed and the following portion of the sentence was removed: "and suggest that measures to encourage retention of family physicians should be included in these reforms" so as not to overstate implications in of the research in light of its limitations.

Introduction

There appears to be important information missing from the introduction; needs major re-working. For instance, what do the authors mean by 'continuity of care'? It is important to specify so that the reader is clear about what is meant.

The specific type of 'continuity of care' we are referring to (provider or relational continuity) is defined in the Introduction.

The link between physician retention and continuity of care is implied from the first paragraph; I suggest to make this link more explicit. And more importantly, is there evidence to support that there is a link? This evidence is lacking.

In the Introduction we present available literature/evidence supporting/pointing to the possible link between retention/turn-over and continuity and make the link more explicit (i.e. we point out that turn-over is an aspect of continuity).

Also, why are ACSCs a relevant outcome for this study? What are factors normally associated with physician retention? With ACSCs?

ACSC hospitalizations are often used as an indicator of primary care quality because good quality primary care is thought to reduce or prevent hospitalizations for ACSC conditions. As stated in the Introduction of the original submission, there are several studies which have found a relationship between higher continuity of care and hospitalization but no studies have focused on the relationship between physician retention and hospitalization. We felt this was an important research question given the possible detrimental effect that retention/turnover may have on continuity of care.

It was indicated that there are many studies where higher continuity of provider care is associated with reductions in ACSCs hospitalizations. However, because of the manuscript word limit we did not discuss other factors associated with ACSCs here. In the Methods section we point out that included co-variables were previously found to be associated with health care utilization. Also, in the Interpretation section we state that, "...we found hospitalization rates for ACSCs to be higher in rural areas as well as in individuals with higher co-morbidity rates and lower household income, all of which have been reported in the literature."

Do you have numbers about physician turnover in NL? How is it higher than in other provinces...or how does it compare? Physician turnover in NL is compared to that in other provinces and a reference provided in the Introduction section/Reference List.

Also, I recommend that authors use terminology to suggest association throughout, so as not to mislead readers about the causality of the estimates presented later, e.g.: refrain from using 'effects', instead, use association throughout.

As suggested, the term 'associated' is used throughout so as not to mislead the reader about causality of the estimates.

10. P4L17-24: Sentence too long and difficult to follow; break in two.

The sentence was broken into two separate sentences.

11. P4L38-42: 'In addition, measuring...'; this statement is incorrect, please re-phrase to address the following: physician retention may serve as one proxy for continuity of care, but you do not show this in your study, and you do not provide any evidence that there is any link between physician retention and continuity of care.

This statement was revised to indicate that physician retention may serve as one proxy of continuity of care. References were included providing evidence for the link between retention and continuity of care.

12. P4L43: replace 'the effects' by 'the relationship, or association'

Suggested replacement made.

Methods

The methods section needs major re-working. See comments below.

13. I believe CMAJ and CMAJ Open require a figure showing the flow of study inclusions/exclusions; this will help.

A figure illustrating the flow of study exclusions is included in the revised manuscript.

14. Economic zones should be defined in more detail so that readers understand what they are.

Economic zones were defined in more detail in Methods section, a map of economic zones is cited and populations of economic zones are included in an appendix.

15. Authors should give more information about ACSCs.

More information is given on ACSCs. ASCS used and ICD codes used to query them are provided in an appendix.

16. What is the period used for counting rates of ACSCs? 2001-2009? What intervals?

As described in the Methods section, the period used for counting hospitalizations for ACSCs was calendar years

2005-2009 (5-year period). There were no intervals. Rates are presented for the whole 5-year period.

17. P6L48: Describe briefly how income quintiles were assigned; reference is not sufficient for such a high predictor of both 'main predictor' and outcome.

Income quintiles were assigned in the same way as the other DA-level census variables (e.g. percentage completing high school) via postal code of residence.

18. Is assignment of individual covariate values based on 6-digit postal code?

Yes, assignment of covariates was based on 6-digit postal code. This is now clarified in Methods section.

19. Which descriptive statistics? Proportions, rates? I recommend being more specific.

'Descriptive statistics' was changed to 'means and proportions'.

20. The term 'comparison statistics' is too vague; do you mean ratios? In my opinion, a difference is a comparison statistic. I recommend being more specific.

The term 'comparison statistics' was changed to 'inferential bi-variate comparison statistics (e.g. chi-squared or kruskal-wallis test)', in order to be clear which tests were omitted.

You may also simply state that you considered Poisson and NB models, and the choice was based on the chi-square statistic (do you mean the likelihood ratio test?).

We state that the negative binomial model was chosen over Poisson because of better fit based on a likelihood ratio test.

21. I find the term 'log-linear' model here confusing; I suggest replacing by Poisson regression, people will be more familiar with this terminology for modelling rates or count data.

As suggested, we omitted the term 'log-linear' and used the terms 'negative binomial' and 'Poisson' which are more familiar to researchers as models to deal with counts outcome data.

21. I recommend that authors be more specific about the sub-group analysis for those 65 years or over. Why did they do that? I don't suspect that a policy intervention targeting physician retention would want to target particularly those physicians who care for seniors...need to provide a rationale or remove.

We point out that we expected the relationship between retention and hospitalization to be stronger in seniors than in the all-age analysis. The rates of social complexity and multi-morbidity are higher in this population, and we feel that GPs with improved retention/continuity have a better understanding of these conditions and a better ability to mitigate their effect on hospitalization.

22. I am a strong opponent of including/excluding a variable for confounding adjustment based on p-values; there is a vast literature to support this stance. I suspect that your primary interest lies in the association between retention and rates of ACSC, therefore, you I recommend that authors examine whether a covariate is statistically associated with retention and is a potential predictor of ACSCs.

Univariate regressions (i.e. unadjusted analyses) were run for each co-variate using number of ACSC hospitalizations at criterion variables. As indicated in the Analysis subsection of the Methods section, factors were only included in the final model if $p < 0.2$ in the unadjusted analysis (i.e. indicating that the variable is a potential predictor of ACSC hospitalizations. Correlation matrices were also run among all variables with $p < 0.2$ predictors (including retention), and it was determined that retention and rural-urban place of residence were highly co-linear (as explained in the original version of the manuscript). Also, a variable indicating whether or not the physician of the patient was an international medical school graduate was also found to be highly co-linear with retention and rural-urban place of residence and was also removed from the analysis.

23. How were covariates included in the model? For instance, quintiles, or continuous age? Need to say more about model specification.

See #22 immediately above. We also provide more specifics on whether categorical or count/continuous forms or variables included in the final model.

24. What is the rationale for sensitivity analysis excluding urban? Need to specify.

It is now explained in the Methods section (as it is in the Interpretation section) that a sensitivity analysis was conducted excluding urban areas because there was found to be high co-linearity between retention and rural-urban place of residence.

25. Later in the results, authors present estimates obtained from models by whether or not models were adjusted for number of GPs per capita. Why is that? Explain and provide rationale in the methods section.

As previously stated, to avoid confusion we removed the regression results conducted without inclusion of the 'GPs per capita' variable (and all references to it in the text). We retained the analysis for those age 65+. We point out in the Methods section that we expected the relationship between retention and hospitalization to be stronger in seniors than in the all-age analysis. The rates of social complexity and multi-morbidity are higher in this population, and we feel that GPs with improved retention/continuity have a better understanding of these conditions and a better ability to mitigate their effect on hospitalization.

26. Is it possible to get information on the physicians? Age, year of graduation, country of graduation, etc.? I believe those may be important to include; if not consider discussing this in limitations section.

We explored including year of graduation and whether the physician was an international graduate. However the former variable was not found to be a significant predictor of ACSC hospitalization in unadjusted analysis (i.e. $p < 0.2$) and the latter variable was found to be highly co-linear with retention and rural-urban place of residence and, thus, was omitted from the final model.

Results

The results section needs major re-working; in particular, 1) refrain from interpreting results in this section (i.e. dose-response, unless you statistically test for it);

The term 'dose-response' was removed.

2) presenting results by 65yrs or over or controlling (or not) for GP per capita is confusing, and the rationale for this is unclear. As previously stated, to avoid confusion we removed the regression results conducted without inclusion of the 'GPs per capita' variable (and all references to it in the text). We retained the analysis for those age 65+. We point out that we expected the relationship between retention to be stronger in seniors than in the all-age analysis because the rates of social complexity and multi-morbidity are higher in this population, and we feel that GPs with improved retention/continuity have a better understanding of these conditions and a better ability to mitigate their effect on hospitalization.

27. P8L15-20: 'The proportion...'; is this for the full study period? Rate per year? Need to be more specific. This proportion was for the 5-year period from 2005-2009 and this is now specified in Results section.

28. P8L25: What do you mean by 'unequal retention tertile groups?' This term was seen to be confusing and was, therefore, removed.

29. P8L32: Rates are supposed to have a time component attached; number of ACSC per 1000-year? The rates are 5-years rates (data years were 2005-2009). This is clarified in the text and tables.

30. P8L47-50: Dose-response interpretation should not be in the results section; please only present relevant estimates as they are. Also, have you tested for a 'dose-response' i.e., slope different from 0? Was this in your hypothesis? I recommend using more conservative language about the interpretation of the results. Neither dose response nor slope different from 0 were tested for, so the term 'dose-response' was omitted.

31. Table 2: Interval? What does the p-value compare (high vs other? Slope?) 32. Table 3: I suspect that authors present 95% confidence intervals in parentheses; this should be mentioned somewhere. Table 2: A significant p-value indicated a significant group difference in hospitalization rates from a Kruskal-Wallis test. However, we removed the p-value from the table in the final version of the manuscript because we felt we did not need to perform inferential bivariate comparison statistics between groups, given that the study sample was effectively the population being studied, as stated in the Methods section. Table 3: 95% confidence intervals are provided in parentheses. This is now indicated in the table.

Interpretation

Overall good, but needs some re-working to improve clarity, flow, and better highlight the relevance of these results for policy and research given its limitations.

33. P10L14: Consider re-wording, e.g. 'physician retention [may] help reduce rates of hospitalization or other outcomes...' We chose to retain this paragraph but started the paragraph with the phrase above and then continued with, "but the exact mechanism by which retention may exert its effect is not fully understood.....".

34. P10L36: Why did you expect the association to be more pronounced for seniors? This should have been stated earlier; and what is the relevance for policy planning? For clinicians?

We expected the relationship between retention and hospitalization to be stronger in seniors than in the all-age analysis. The rates of social complexity and multi-morbidity are higher in this population, and we feel that GPs with improved retention/continuity have a better understanding of these conditions and a better ability to mitigate their effect on hospitalization.

35. P11L6-20: I don't understand this paragraph. Why begin with a question? Consider re-phrasing to make clearer what you wish to discuss, i.e. study limitations? How does this missing information may have impacted your results? This question at the beginning of the paragraph was changed to a statement, "Other factors may have contributed to the observed association between physician supply and hospitalization." The remainder of the paragraph discusses some of these possible factors, which were not actually measured in this study.

36. P12L4: Non-FFS physicians: does this only affect CCI? Yes, the only variable used in the study affected by lack of visits data for non-fee-for-service physicians was the CCI. Lack of this data also prevented us from being able to calculate continuity of care indices at a provincial level.

37. P12L16-32: I recommend that authors don't overstate the implications of this research given its limitations. Inference about causality, cost-effectiveness, or about link between retention and continuity of care cannot be made from this study. The conclusion is reworded so as not to overstate the implications of the research given its limitations.

Other comments

38. May clustering be an issue? I would like to be convinced of this.

We did not account for clustering in our analysis. If the reviewers feel this is critical we would be prepared to revisit the analysis.