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Article Title: Trends in hospital coding for people experiencing homelessness, 2015-2020, Canada: a descriptive study

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Reviewer 1: Giulio DiDiodato / Royal Victoria Regional Health Centre, Critical Care Medicine

The authors have conducted an important study to raise the awareness of the need to capture homelessness data routinely, both from a mandate perspective but also from a system planning and policy perspective. The report was very well written, and the rationale clearly explained. The methodology was sound, and the results presented in a clear and efficient manner. The conclusions from the data were appropriate. The authors highlighted the very real limitations to the reporting, likely resulting in significant under-reporting of the use of healthcare resources by PEH. I have not identified any issues that warrant revision and would recommend acceptance.

Reviewer 2: Rami Shoucri/ St. Michael's Hospital Department of Family and Community Medicine and St Michael's Academic Family Health Team

I think a little bit more information on how the recording became "mandatory" would be interesting. How was it communicated/implemented? How is this monitored? Any consequences to it not being recorded by hospitals (e.g., to funding)?

The changes were implemented in reference documentation that is routinely used by staff in hospitals who perform the coding process and submit data to CIHI. CIHI supports accurate coding through education and client support. For changes that were effective as of April 2018, CIHI communicated via electronic newsletters and a webinar focused on "What's New" in the coding standards. CIHI does not have information on whether hospitals or regional health authorities are monitoring the consistency of recording Z59.0, or any consequences or not recording it.

Reviewer 3: Amol Verma /St. Michael's Hospital, Li Ka Shing Knowledge Institute

MAJOR COMMENTS

Page 4 Line 56: "Logistic models with different time definitions for the interactions were tried..."

- a) How was the final model selected? Based on model fit? What other models were tried, and why were they discarded? An explanation would be helpful.
- b) Does the inclusion of interaction terms between fiscal quarter and period make the interpretation of the adjusted odds ratios more complex? The authors seem to report 60% increase in odds based on aOR 1.59, but can they explain more whether interactions terms in the model complicates the interpretation?
- c) Given repeated measures across different time periods within the same hospital and/or the

same jurisdiction, would it make sense for this to be a mixed effects model to account for clustering effects? Can the authors report more information about how they specified the model? Did they select only fixed effects adjustment? If so, a justification would be helpful.

d) To report province-specific results, did they fit separate models for each province?

Different interactions with time were used to test potential seasonality/ variations with time that was not explainable with our “period” variable. However, as per the recommendation of the reviewing statistician, seasonality should be tested with a variable (coded Q1 to Q4) being added to the model.

Therefore, our analyses have been updated and this interaction no longer applies.

c) This is an interesting point. I would want to reinforce that the purpose of this modelling approach was simply to describe the difference in the post- mandate period relative to the pre- mandate period. Through external engagements and observations, much of these differences nationally are due to which province/ territory facilities are in given the amount of top-down coding direction that comes from their respective Ministries of Health. We controlled for PT jurisdiction in our models to account for this. However, we acknowledge that there could be differences in the coding practices of different facilities and therefore future studies that consider mixed effects should be explored.

D) Yes. Note this is simply to describe the within-province effects and a model that tested the interaction between PT and period was run and proved to be significant.

The authors report a frequency of PEH of 0.12- 0.41%. They comment in the discussion that this is likely an underestimate, although validation of these ICD-10 codes is outside the scope of this study. Can the authors comment on how this estimate compares to other estimates in the literature, obtained through other methods? Presumably the prevalence of PEH would vary widely across hospitals, jurisdictions, and time, but some sense of the magnitude of difference between this estimate and the existing literature would be helpful.

Most studies, that we are aware of, have been run on data that preceded the 2018 coding mandate, and therefore, would be incomparable.

Furthermore, the next most robust method of identifying people experiencing homelessness in the hospital setting would have come from Richard L, Hwang SW, Forchuk C, et al. 2019 (DOI: 10.1136/ bmjopen-2019-030221) where the authors conducted a study to validate a homelessness ascertainment algorithm using a validated cohort of people experiencing homelessness, and, therefore, was intended to enumerate the entire population rather than the population accessing services.

In short, we do not have a robust and up-to-date method for identifying people experiencing homelessness in health administrative data to compare our results to. We hope that with the publication of this paper, other researchers and jurisdictions will look to investigate the accuracy of this code for their respective populations.

Lucie Richard and colleagues are working on an updated validation study aimed at enumerating the population of people experiencing homelessness.

Can the authors comment more on why they chose logistic regression as the modeling approach? The data in Figure 2 suggest a clear change in prevalence following 2018, but also my crude estimation wonders whether there was an upward trend both before and after 2018. Would time series methods of modelling, specifically assessing for an intervention effect, be a better modeling choice?

You are correct to suggest that ITS would be a more robust approach for assessing the effect of the intervention and was strongly discussed as a study design choice. However, there are a few main reasons why we chose a cross-sectional/ descriptive design:

- **We identified 3 major time periods that could have a substantial impact on people experiencing homelessness being identified in the hospital setting. These 3 being the period before the 2018 mandate, the period after the mandate, and the period during the COVID-19 pandemic. We wanted to capture the probability of Z59.0 coding based on these time periods separately, especially since the impact of volumes for patients experiencing homelessness during the pandemic are unknown at this time.**
- **Building on this point above, we also had the desire to quantify the increase of Z59.0 coding in the post-mandate period in a single value, as it is certainly possible for it to take some time until the new coding standards influence the coding practices of certain facilities.**
- **Finally, we aim to present the results of this work to a wide variety of audiences, including multi-disciplinary researchers, policy makers, and health system planners. Therefore, we wanted results that were easy to interpret.**

Once more years of data are available and more validation studies on the accuracy of the Z59.0 code have been completed, an ITS study will have more value. At this stage, the goal of this work is to raise awareness of an increasing available source of data to identify people experiencing homelessness in health administrative data.

It is surprising that the modeled results for BC suggest a reduction in the incidence of PEH over time, whereas the raw numbers from Table 1 suggest the opposite (an increase from 0.47% to 0.79%). Can the authors comment on why this would be the case?

Note the differences in how time is aggregated. For Table 1, results are shown by fiscal years, but the models had an interaction with fiscal quarter (numerically coded 1 to 24 over the entire observation period). Due to seasonal effects, this observed reduction is likely to do with the time in which we chose to calculate ORs, namely, Q1 of 2018 and Q1 of 2020 (or fiscal quarter 17 and 21, respectively).

However, note that covariate adjustment for seasonality has been changed as per the advice of the reviewing statistician and therefore avoids this confusing result. Overall, there was a small increase for BC in the post-mandate period.

MINOR COMMENTS

What is the definition of “inpatient stay” in HMDB? Presumably this does not include day surgeries. Does it include mental health visits or obstetrical care? A brief definition in the methods would be

helpful (apologies if I missed this).

Note that the unit of analysis for this paper is unique episodes of care, which is often used for CIHI indicators. This method consolidates adjacent acute inpatient hospitalizations and day procedure visits which avoids counting transfers as sperate hospitalizations.

Note some clarifications have been made to avoid confusion. I hope this helps!

Interpretation, page 6, line 28 – the authors report an 84% increase in coding of PEH, but I could not find a similar number in the results, where they report a 60% increase. Also, the abstract reports aOR 1.58 but results report aOR 1.59. Could they align the reporting in the different sections for consistency?

Thank you for this comment. This was an error and should have said 84% in both instances.