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Title	The costs of services for homeless people with mental illness in five Canadian cities: results from a large prospective follow-up study
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Reviewer 1	Dr. Daniel Poremski PhD
Institution	Douglas Mental Health University Institute, Montréal, Que.
General comments (author response in bold)	The proposed manuscript presents costs associated with homelessness in Canada. It is evident that much effort went into obtaining accurate costs for services. The manuscript is well written. The reviewer would suggest that the editor accord slightly more room to allow some of the content presented in the appendix to be inserted into the body of the manuscript. While the current form presents the essentials, interested readers may find the back and forth between the body and appendix fiddly, and distracted readers may gloss over important but esoteric elements presented in the appendix (such as the adjustment for discrepancies in self-report and process data).

The authors may want to elaborate on some points below:
1. In the introduction two estimates of costs are provided, BC and Calgary. The BC data is given more description (sample size and study dates), perhaps it may be helpful to expand on the Calgary estimate by providing sample size to give the reader a better idea of what is meant by "System-level approach" and the merit of the estimate since it is "substantially higher"?
We have provided more detail on the Calgary estimate. The different approach used to obtain that estimate implies that sample size is not relevant. Both estimates are provided to indicate what is currently known; the values turn out to be quite different from each other, implying the need for more thorough investigation. The fact that it is substantially higher does not make it less relevant to report, in our view – on the contrary.

2. Page 8: "For the latter, we combined policy, court appearances and incarcerations" you mean Police?
Indeed. We have now corrected this error.

3. In the results, the authors select certain high costs to report in the text. However what stands out to the reviewer is the higher costs for supported housing in Montreal. Could the authors elaborate on this? and perhaps indicate how many people contributed data to the estimate of supported housing costs? (given that this study uses data from the TAU group of the At Home Chez Soi study). A distracted reader may be tempted to derive the cost of At Home Chez Soi experimental supported housing from the "supported housing" costs presented in table 3. The authors may want to add a footnote to remind the reader that the costs of the supported housing service provided to the experimental group can be found elsewhere.
Following several other authors, we make a distinction between supportive and supported housing. Supportive housing, which is commonly used by TAU participants, normally involves housing with staff on-site staff, whereas supported housing refers to usually ordinary housing without on-site staff, but in which staff travel to the buildings where clients are to provide support. In the At Home/Chez Soi study, all experimental group participants were offered supported housing, whereas few TAU participants had access to it. We have added a footnote to Table 3, clarifying the meaning of supportive housing. We have also clarified that the category in fact also includes subsidized rooming houses without on-site staff, which is more common in Toronto. Finally, we also note this difference in the presentation of the results and again at the beginning of the interpretation, when we summarize the main conclusions.
We note here that 55% of TAU participants used supportive housing at some point over the course of the study. This percentage is second-highest in Montreal (60%), exceeded only by that in Winnipeg (63%). The high cost of supportive housing in Montreal is mainly driven by high use of fairly costly, temporary housing with supports as well to a lesser extent of permanent social housing with onsite supports (ie, supportive housing with no limit to duration of stay). In order not to further lengthen the manuscript, we have not however added these details.

4. In the interpretation the authors present new data related to the cost of medicine from the Montreal site. While the authors do note earlier that data could only be derived from Montreal, they do not provide previous results to support the $3000 estimate. Could the authors indicate where the interested reader may find further information? Has the estimation of Montreal medication costs been published? Or in review?
The estimate was derived directly from data that we obtained for this purpose. We have added one detail to clarify the nature of the estimate: that it is for all types of prescription drugs. To date, these data have been used only for the Masters thesis of one of the lead investigator's students, Angela Ly, the results of which are currently being prepared for publication.

5. Along with the estimate of medication comes the assumption that medication costs would be comparable between cities. Would the authors have a reference to support the assumption? Given that each province negotiates its medication costs individually with the pharmaceutical companies, and may have different incentive programmes for encouraging pharmacists to prescribe generic vs branded medications, it is possible that costs vary as greatly as the cost of inpatient psychiatric care.
We agree with Dr Poremski that costs of medications may vary across sites, both because of the reasons he cites and also because of possible differences in access to prescribers. We had acknowledged the fact that the cost of medications could vary significantly across sites as a limitation. Nonetheless, it seemed to us preferable to provide this for one site, than not to provide it at all: it at least gives an indication of the likely order of magnitude of this cost for other cities.

6. Please apply the same inflation factor to the 2006 BC estimate of homelessness and report it in the interpretation. Would your costs of 46,000 to 60,000 2016 dollars really be equivalent to 55,000 2006 dollars?
This is a good point and we have converted the values of both the BC and Calgary estimates to 2016 dollars. We used Statistics Canada CPI data to this end.

7. The important point about the allocation and distribution of cost/services made on page 11 (starting line 40) is somewhat camouflaged in the bulk of the interpretation. Would the authors consider including it in the conclusion as it does have implications for the way policy should view its allocation of resources?
We thank Dr Poremski for this point and have added to the conclusion accordingly.

8. Please be mindful of neither and nor used independently and double negatives: "Our findings are not inconsistent with that observation"
We have corrected the one instance where neither was used without nor, thank you for noticing that. It seems to

us that the phrase **"Our findings are not inconsistent with that observation"** is clear enough and conveys, as we intend, a weaker meaning than: **"Our findings are consistent with that observation."** The latter would be too strong it seems to us as alcohol use is not associated with higher costs in our data, contrary to what other studies have reported. But the odds ratios, both adjusted and unadjusted, are greater than one, which we think allows us to put things as we do. At the same time we have added a few comments on the surprising fact that alcohol abuse or dependence is associated with an increase in justice-related costs and, surprisingly, a decrease in psychiatric hospitalization costs, with a net effect of only a small non-significant increase.

9. The limitation of self-report is in need of some elaboration given that you note two references for its reliability, but note in the appendix 6 on data analysis that comparing process data to self-report was off by a factor of 0.9 to 3+, and that as a result, adjustments were made. As a side note, please change : "It revealed a strong tendency to under-reporting of visits and of visit duration, with factors ranging from 0.93 to 3.3." to avoid the use of "factor", use instead "multiple." this will help retain the cohesive use of "factor" elsewhere in the manuscript.

We see the relevance of these **points. We do acknowledge in the limitations that "self**-reports may either under- or over-**report service use". We have added a sentence in the limitations section to point out that we have made a** partial adjustment for under- and over-reporting. Secondly, to clarify the distinction between the two meanings of **the word "factor" in the text, we have modified the sentence to read: "were adjusted using the corresponding multiplicative factor". The word "multiple" would normally refer to a multiplicative factor that is an integer, which is not the case here. Note that we have also added some text to this paragraph to increase its clarity.**

Reviewer 2	Dr. Christopher Perlman
Institution	University of Waterloo, Faculty of Applied Health Sciences, School of Public Health and Health Systems, Waterloo, Ont.
General comments (author response in bold)	This well-written manuscript examines an important health and social issue. The findings were quite interesting, particularly related to the factors associated with costs and variations in cost units across jurisdictions. I have a few questions and comments to consider for the manuscript:

1) The introduction seemed somewhat brief, even considering the word limit. For instance, it was noted that the independent variables used in the regression analysis were selected a priori. While these variables make sense in terms of face validity is there some justification that could be provided, perhaps through a brief literature review of cost drivers for homeless populations in the introduction?

This is a very good suggestion. We have identified several recent articles which, while none was able to estimate costs as comprehensively as we were, do provide some indications of predictors of costs among homeless individuals. We did not allude to this literature in the introduction, which the journal prefers to be kept as brief as possible. We have however developed, in the methods section, the justification for the variables we included, citing two articles in support of the variables that we included.

2) What was the rationale for selecting only costs related to the control sample of the At Home/Chez Soi trial? Was it to capture the "status quo" costs associated with homelessness and MI rather than costs effectives/benefit of interventions to address homelessness?

We have addressed this point in our response to the editor.

3) Were the extreme outliers for high costs, e.g., above 150k, evenly distributed across the cities? If not, could these individuals be driving some of the differences in certain costs between the cities, particularly psych hospital costs or police/court costs?

That is a very relevant question. Defining outliers in a standard way as the upper quartile plus one-and-a-half times the interquartile range, we get thresholds ranging from $72,368 in Moncton to $130,841 in Montreal. The percentage of observations above the threshold is somewhat lower in Moncton (3.1%), but fairly similar across the other cities (5.7% in Toronto to 6.6% in Montreal). The sample maximum is $149,929 in Moncton, and for the other cities ranges from $233,698 in Montreal to $341,535 in Vancouver. This suggests to us that our results are not significantly driven by outliers.

4) Among the most interesting findings are the variations in costs per location. In particular, it was interesting to note the differences in costs associated with psychiatric hospitalizations vs. costs for police/court appearances across the large centres. The authors touch upon these findings in the interpretation, suggesting that perhaps more ACT and ICM services may be available in Toronto. Were ACT and ICM included as a cost unit? It seems that "ambulatory visits" are much higher in Toronto (table 2) so this could support such an assertion but it is unclear with the information provided.

To have been included in the trial, people needed to not be followed by an ACT or ICM team at the baseline. However, especially in Toronto where such teams are common, they could have begun to be followed by one during the 2-year follow-up. But our questionnaire did not ask specifically about ACT or ICM teams. Its strategy rather was to ask about visits to and by a large number of types of professionals, as indicated in the table of unit costs now included as an e-appendix.

5) The use of ACT or ICM doesn't explain the differences in costs of police/court appearances. It is difficult to determine if the weighted costs for police/courts (table 3) in Toronto is higher due to a higher cost unit for police compared to other centres (not available in Table 1) or due to increased apprehensions in Toronto.

This in indeed a limitation of reporting costs rather than frequencies. The complete list of unit costs now joined to the manuscript shows that the difference in the unit costs that we used for these services is negligible; the difference must be due to more frequent interactions with the police and the courts.

6) The findings related to functioning are quite interesting. I wonder to what degree social costs differ for individuals with similar functional deficits who are not homeless? Is any information available to provide such a comparison for the interpretation of this study's findings? It is suggested that the MCAS could be used as a needs assessment in service delivery settings. While I agree that needs assessment is essential in any setting, is there a specific justification for recommending the MCAS? Could it be said, simply, that standardized needs assessment should be an important component to service delivery with homeless persons with a MI?

Costs vary greatly across individuals with mental illness in general, and we are not aware of any truly comparable sample, from one of the cities in the study, in which costs would have been assessed similarly, with which we could compare our results. We did not find a significant association between the duration of the longest period homeless, our proxy for chronicity of homelessness, and total costs. However, we did find a significant bivariate association between this variable and total costs. We interpret this in the text as indicating how total costs increase with homelessness chronicity, a process that the data suggest is associated with changes in other variables themselves associated with total costs. (Hence the fact that the coefficient is smaller and non-significant

In the adjusted regression.) In other words, people who have been homeless a long time are different in several respects from people who have only recently become homeless, on average, and these differences together are associated with higher costs. We now estimate the extent of this increase in the text, based on the unadjusted odds ratio of 1.03 and the distribution of duration of longest period homeless in our sample. We return to this point shortly.

As for the justification for recommending the MCAS, it is simply that in this large data set, the MCAS has turned out to be highly predictive of costs. As stated in the Interpretation section, our results indicate that the level of functioning is more strongly associated with costs than alcohol or drug dependence or abuse, or other such factors that we controlled for in the regression.

7) In the interpretation it would be helpful to have some sort of comparison with costs associated with non-homeless persons with a mental illness, if available, for understanding the magnitude of costs for homeless persons with a mental illness.

This relates to the point above. We have added the following paragraph to the interpretation:

While we are not aware of any comparable cost estimates for samples of people with mental illness but who are not homeless, we did find a statistically significant bivariate association between longest single period homeless prior to baseline, and total costs. The odds ratio implies approximately an 8% increase in annual costs of people with homeless histories corresponding to the average in our sample (2.5 years), compared to individuals with the least homelessness in our sample, and a 243% increase for people whose longest single period homeless is 30 years, the maximum value in our sample. This finding suggests that programs designed to prevent people from becoming chronically homeless could be highly cost-effective.

A few editorial notes for consideration:
- Several points related to costs in the first paragraph of the interpretation could go in the results section, e.g., range of costs with and without medications in lines 19-28.

We have followed a fairly standard convention of beginning the interpretation section with a brief summary of the key findings. It seems to us that this enhances readability.

- Including a full list of unit costs would be more informative than a sample list provided in Table 2 or having to contact the authors for a list.

As noted above, we have now provided this for inclusion in an online Appendix.

- Consider incorporating some of the costing information directly into the methods rather than referring the reader to multiple appendices. Some of this information is directly relevant to the interpretation of the findings.

This too has now been done.

- Appendix 6 is somewhat confusing as the first statement refers to "experimental group participants". I thought the study included only control participants.

We acknowledge the difficulty. This study does report only on control group participants. However we felt it necessary to make the adjustments described in what was Appendix 6 to the data for control group participants, based on observations we had made earlier on experimental group participants. To be more specific, in our early analyses of the data, we checked whether we were able to account for what we knew were the costs of the intervention from self-reported data. We found that we could not. Even with carefully calculated unit costs, we were falling substantially short. This then led us to obtain detailed administrative data from the Montreal site and compare them with self-reports, as described in the text. Adjustments were then made to the reported frequencies for these questions, as noted in the text. Also, it seems important that our forthcoming cost-effectiveness analyses include exactly the same data on the TAU group as the present paper.

- The first sentence in Appendix 5 (lines 19-24) is repeated in lines 45-53.

Thank you for pointing this out. We have removed the first instance of this sentence.

Reviewer 3	Dr. Claire de Oliveira MA PhD
Institution	University Health Network, Toronto General Research Institute, Toronto, Ont.
General comments (author response in bold)	The costs of services for homeless people with mental illness in five Canadian cities: Results from a large prospective follow-up study

This study seeks to estimate the economic burden of homeless individuals with mental illness from a societal perspective. In addition, it identified characteristics associated with higher costs. This paper provides an interesting analysis on a topic that is not well studied and therefore represents an important contribution to the field. I have only minor comments to the manuscript; most are related to issues around clarification.

Methods
Measures
1. In the measures section, the authors discuss the various sources from which data were obtained. How was basic demographic information (such as sex and age) obtained?

We have now clarified that these measures, including all other measures except for the Multnomah Community Ability Scale, were obtained from self-reports.

Perspective of economic analysis
2. In the perspective of the economic analysis, the authors state that they employ a modified societal perspective. This is explained in further detail in the appendix; however, it would be nice to have a bit more information in the text. I believe this would help the reader understand this concept a bit better. (I also understand that the word count is a limitation, which is why this is in the appendix.)

As requested, we have moved this information into the main text of the article.

Estimation of unit costs
3. The authors used CIHI data to estimate unit costs for hospitalizations, ER and outpatient visits. What about costs of other health care, such as nursing and long-term care? Also, how did authors obtain data on physician fees? Were these obtained from the fee schedule of each province?

This information is now provided in the Appendix.

Data analysis

4. Regarding the data analysis, the authors used bootstrapping to estimate means and 95% confidence intervals; in particular they used 500 replications. Why not 1,000, for example? My understanding that is that 1,000 should be the minimum.

We had checked on a sample of costs the effects of doing 1000 vs 500 replications, and concluded that it made no material difference. Our understanding is that in such a case, using 500 replicates is sufficient.

5. The authors used GLMs to model costs. I agree with this approach. However, I wonder if they made use of the modified Park test to determine the family distribution and the Pregibon's link test to determine the link function. Nonetheless, the final model was a GLM with a Gamma distribution and a log link. This is generally the case for these data.

We did indeed carry out these tests but had not stated so in the paper. The linktest command in Stata carries out Pregibon's test, after any single-equation estimation command. We had randomly chosen some imputed datasets to do the test and the link tests indicated that the log link worked best.
We had also found a user-written command for the modified Park test. Again, we randomly chose some imputed datasets, and the tests suggested that the Gamma family is a better family distribution for the data.
We have now indicated in the text that we also used these tests, in addition to a comparison of specifications using the AIC criterion.
Were the error terms clustered by city? I think this would make sense given that there are observations from different cities.
We controlled for city in our regressions using dummy variables, which addresses this issue.

6. The authors estimated both unadjusted and adjusted associations. It was not clear what was included in each case.

We have added a footnote to what is now Table 3 to clarify this.

7. Finally, it was not clear how the coefficients in Table 4 were obtained. Perhaps a brief explanation on this would help the reader

The methods section explains that the coefficients come from adjusted and unadjusted GLM regressions with, as noted in the text and discussed above, a Gamma distribution and log link. The new footnote to Table 3 alluded to just above also clarifies the independent variables in the regressions to which each coefficient (odds ratio) belongs. Following a suggestion of the fourth reviewer, Dr Bulloch, we have indicated in what is now Table 3 that the coefficients are odds ratios – they are obtained by exponentiating the raw coefficients from the regressions. We hope this provides sufficient explanation.

Interpretation

8. In the interpretation section, the authors note that spending on psychiatric hospitalizations was much greater in Vancouver and Montreal than in other cities. Do the authors have a sense of why this might be the case? Were the costs of hospitalizations obtained using the cost of a standard hospital stay obtained from CIHI? Could this difference be due to the different provincial values?

As may be seen from the list of unit costs (we used a per diem extracted from CIHI data rather than as such the cost of a standard hospital stay, which does not work well for psychiatric hospitalizations), the difference in costs of psychiatric hospitalizations must be due primarily to differences in days hospitalized. We noted in the Interpretation that this may have been the result of greater access to ACT and ICM teams in Toronto – these consistently reduced hospital days.

9. In addition, the authors find that their estimates were lower than the ones obtained using data from Calgary. Do the authors have a sense of why this is the case?

The Calgary estimates were obtained using a completely different method, namely apportioning overall costs of different kinds of services between homeless vs non-homeless individuals. Many assumptions had to be made to make such an apportionment as organizations did not collect sufficiently detailed data on numbers of homeless individuals who used their services. This is noted in the text.

10. The authors also find that the level of functioning turned out to be more predictive of costs in the regression analysis as opposed to substance abuse, or homelessness history, or specific mental health conditions. Could it be possible that the latter are captured in some way in the former? The level of functioning is likely determined/impacted by these variables.

We agree, and this is what we were trying to say in the interpretation when we suggested that the MCAS appears, from our data, to provide an efficient tool to capture the factors that are most associated with costs.

Reviewer 4	Professor Andrew Bulloch MA PhD
Institution	Mathison Centre for Mental Health Research & Education, University of Calgary, Calgary, Alta.
General comments (author response in bold)	This is a thorough study of the costs of services for homeless people with mental illness in Canada. The paper is elegantly written and a pleasure to read. This kind of analysis is beyond my area of research, but the presentation and thoughtfulness of the study, as well as the reputation of the senior authors, gives me confidence in the results. As far as I can tell the complex costing methodology that involves weighting is appropriate. The authors may wish to pay attention to the following comments:

1. The treatment as usual group is mentioned early on, eg in the abstract. It would provide context if the experimental group could be described early on also.
We have already addressed this point above.

2. I suggest the assessment of mental disorders via the MINI interview is put in the Methods and not just left to the Appendix.
This is now also done.

3. What are "chained equations" (page 6)?
The reference we had included addresses how to combine multiple imputation with bootstrapping, but not, indeed, multiple imputation by chained equations. We have added a second reference that explains what this is.

4. Table 1 is not described in the Results. It would be helpful to point out where there are differences across sites (eg alcohol dependence or abuse).
We have added some text that addresses this.

5. Re point 4, Table 3 is nicely described.

6. I do not understand the point of citing unadjusted point estimates in the first paragraph of page 8.
The adjusted odds ratio for longest single period of homelessness indicates the effect of increasing that value |

holding other covariates fixed. But in fact people who have a long history of homelessness are known to differ from those who do not in several respects, such as poorer health, that are likely to be related to costs.

7. I find the use of ad hoc acronyms such as TAU, ICM and ACT to be confusing (I had to look up the latter 2!)
As noted previously, almost all acronyms have been removed.

8. The estimates in Table 4 are Odds Ratios are they not? If so why not call them by this name?
Thank you for this suggestion. The Table now describes them in that way.