

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Migration to the Downtown Eastside Neighbourhood of Vancouver and Changes in Service Use in a Cohort of Mentally Ill Homeless Adults: A Ten-Year Retrospective Study
AUTHORS	Somers, Julian; Moniruzzaman, Akm; Rezansoff, Stefanie

VERSION 1 - REVIEW

REVIEWER	Daniyal Zuberi University of Toronto, Canada
REVIEW RETURNED	21-Jul-2015

GENERAL COMMENTS	<p>Overall, this article has the potential to make an important contribution if revised to improve the framing of the analysis and implications of findings. It is highly innovative and important that the authors utilize linked longitudinal administrative data. Given the uniqueness and richness of the data, some of the summary statistics alone are very illuminating and clearly important in terms of contributing to debates and for policy recommendations (such as those on p. 9, lines 36-53). The vulnerability and insecurity of the population of study and the longitudinal nature of the administrative data utilized are the paper's strong point, but they also create methodological challenges, requiring well-justified decisions, for example, on how to treat missing or incomplete data.</p> <p>Overall the regression analysis utilized in the article is complex, and the findings less persuasive, and perhaps potentially problematic. I attempt to highlight some potential conceptual and methodological challenges, but feel a reviewer more familiar with the specific GEE method chosen for the analysis would be able to provide a critically important assessment of the authors analysis and conclusions.</p> <p>In terms of framing, this paper is somewhat disconnected from the research literature on the DTES and homelessness in Vancouver. Despite listing many sources related to the issues of homelessness, mental health, etc..., the authors do not explicitly develop their analysis from key findings of the previous research. It would strengthen the paper if more of these key insights or debates were included in the paper itself to set up the analysis. The paper would be stronger if more explicitly framed in light of a debate in the literature about homelessness, mental health and migration, going beyond the current framing which is justified as addressing a somewhat vaguely defined gap in the literature.</p>
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	<p>More specifically, the abstract requires revision. The debate about whether or not services for homeless individuals should be located in neighbourhoods such as the downtown eastside or other “spatially diverse” neighbourhoods should be highlighted at the beginning of the abstract, if this will ultimately be the policy recommendation that the authors stress in their conclusion.</p> <p>In the article, the authors identify some potentially important correlations between migration to the DTES, a spatial concentration of homeless individuals, and hospital admissions, criminal conviction and financial assistance. Yet they go too far in asserting that this is a causal link and these outcomes necessarily reflect the impact of migration to the DTES. In other words, the authors problematically fail to control for some of the unmeasured, but important, differences between the population of homeless individuals studied that end up migrating to the DTES and those that do not. These unmeasured differences could be argued to be causally related to the outcome variables of interest. This results in omitted variable bias, and the inappropriate assigning of causal power to the variable “migration to the DTES” for explaining heightened rates of “hospital admissions, criminal conviction and financial assistance”.</p> <p>I am very sympathetic to the conclusions and the especially the policy recommendations presented in the conclusion, yet feel the evidence presented in this version of the article does not provide more than suggestive evidence to support these claims.</p> <p>I acknowledge this central limitation is listed as a line 16 on p. 3 as part of the strengths and limitations, but it is so fundamental that needs to be presented much earlier in the abstract and the appropriate caveats listed in the conclusion of the abstract.</p> <p>In addition, the relationship of the sample to the broader Housing First research and Vancouver At Home study needs to be included at the beginning of the abstract to further improve the contextualization of the research and sample in the broader field.</p> <p>More specifically, on p. 5, line 7-9, the authors should include a justification theoretically or from the literature as to why the authors hypothesized this correlation.</p> <p>Some of the decisions in the analysis of the data require further explanation and justification. For example on p. 6, line 35, I feel it is somewhat problematic that the authors decided that all respondents with “no known address” were assigned as living in the DTES, given that those with “no known address” are not a random group within the sample, and the importance of migration to the DTES as an independent variable in your analysis. At a minimum, I would like to know what percentage of your sample had no fixed address (later on p. 8, we learn this declines from 20% to 3%), and the robustness of your findings if these cases were removed from the analysis as an alternative approach to adjusting the analysis to account for the missing data. Including a discussion of some other empirical evidence that most of the homeless without fixed addresses are</p>
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	<p>living in the DTES would also support this critically important decision in terms of the analysis of the data.</p> <p>More generally, I worry about missing data and how the treatment of missing data impacts the findings presented in the analysis. For example what happens with the 13% of the sample for which administrative data was not available? Given the population, presumably, some respondents passed away over the study period? How was that dealt with in the analysis?</p> <p>I am also somewhat concerned that females only represent 26% of the sample.</p> <p>One question I would like the authors to address, especially in their conclusion, is the potential counter argument that the more interesting finding based on the analysis is that service utilization by individuals <i>was not</i> statistically significant by spatial location except for in these three domains?</p> <p>p. 10, line 40-51. I am very uncomfortable with the conjectures presented in this section as they are only weakly supported by the data and analysis. Again I am sympathetic to the recommendations, but think the article would be much stronger if it simply highlighted the correlation, and then perhaps suggested some future research to test these ideas as an alternative. The conclusions presented on p. 11, lines 12-21 in the article are much more modest, but more strongly supported.</p> <p>Minor points:</p> <p>I also recommend the following minor revisions to strengthen the article:</p> <p>p. 3 Line 15 Consider shifting the tense: "Hypothesized" should be "hypothesize"</p> <p>p. 3 Line 17 "public service involvement" is awkward, please reframe, perhaps "utilization of public services"</p> <p>p. 3 line 20-21 be clear, recruitment was in the downtown eastside neighbourhood of Vancouver</p> <p>p. 5, line 18-19 The authors need to pick Canadian or American spelling of "neighbourhood" and be consistent throughout the paper.</p> <p>p. 7, line 22 It would be helpful to justify why the authors chose the "number of social assistance payments" as opposed to the total amount of payments as the variable.</p> <p>p. 11 line 24, I am not comforted by the presence of "among those??" in the middle of the sentence.</p> <p>p. 12, line 37, capitalize "J" in Journal</p>
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REVIEWER	Judith Lynam University of British Columbia
REVIEW RETURNED	05-Aug-2015

GENERAL COMMENTS	<p>This is an important study that seeks to make sense of a significant problem – in a community that has a significant concentration of people living in poverty & with mental illnesses – how might we most effectively treat & support people living in poverty facing multiple health challenges).</p> <p>In what follows I share some observations about the structure of the paper & the clarity of the writing style.</p> <p>introduction – implies directionality... homelessness – produces mental illness etc... I would shift the language to more clearly identify that people who are homeless are also more likely to be also living with x, y, z.</p> <p>it would be good (early in the paper) to define how you're using 'migration' & I would suggest that you also identify why migration is an interest, particularly in health research. (e.g. from a service provision perspective – shifts in demand & thus need to put in place resources; , but also because we know that social connectedness, &/or having integrated networks of support can be protective against adversities including poverty, illness etc. Migration typically disrupts such social networks.</p> <p>Similarly, although there is a suggestion that services play a role in 'migration' – it would not be unreasonable to also indicate that healthcare organizations -& communities need to respond to changing demographic profiles. One consequence can be that people elect to migrate to access services.</p> <p>Recruitment Clarify procedures & relationship of your study to the broader study.</p> <p>Recruitment was conducted with collaborators? Do you mean, community partnering organizations assisted you to identify people who met the study criteria to invite them to participate in your study? Or, did these 'collaborators' have a role in identifying people for the home first initiative from which you obtained your sample? Or,...</p> <p>Language when discussing publications that have shown the positive impact, I'd add some words that capture the direction of the impact... like... reductions (or increases) in emergency room visits; reductions in involvement with criminal justice system...</p> <p>Methods</p> <p>The following sentence doesn't make sense...needs edits & should explain what type of data; gathered from whom and most importantly indicate that the analysis aligns with the goals of the original studies...variables are collected.....</p> <p>"study involves the retrospective analysis of data associated with sample recruited for experimental trials (ISRCTN57595077 (Vancouver at Home study: Housing First plus Assertive Community Treatment versus congregate housing plus supports versus treatment as usual) and ISRCTN66721740 (Vancouver At Home study: Housing First plus Intensive Case Management versus treatment as usual)). All variables included were collected pre-randomization</p> <p>The data gathering & analysis plan..would be stronger if you identified the nature of the data (to say it's from questionnaires is fine, but saying what the nature of the concepts etc. that are being examined, similarly, most readers won't necessarily know what you mean by 'administrative data'... so, I would describe this too (is it #</p>
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	<p>& types of clinical visits; etc.?).</p> <p>Similarly, you indicate that you used descriptive statistics, but again it's not clear what you are analyzing & correlating etc. as I read on I see in subsequent paragraphs you identify the dependent & independent variables – which starts to bring the purpose of your present study into focus.</p> <p>The discussion of your analysis is good as it's here that I begin to apprehend what you have actually learned. It's where you begin to 'unpack' why it could be that people like those in your study, despite accessing multiple services, experience declining health & increases in engagement with criminal justice system over time. & You call into question the assumption that more services is better.</p> <p>But, you do not come back to the central concept – migration – it's unclear in the analysis how migration is considered in the pattern of service use etc.</p> <p>Moreover, at this point in the paper It's still unclear the nature of the relationship between your study & the at home study – earlier in the paper you imply that engagement with the at home model improves these peoples' experiences – but your analysis suggests that that isn't the case. Is this because you have included the 'whole' sample rather than focusing on those who were in the intervention?</p> <p>Overall, there needs to be a lot of editing done to more clearly link the reasoning that underpins your study, to clarify the links between your study & the 'parent' study, etc. It's unclear for example, when people provided data (in the questionnaires) and when you were only working with 'Administrative' data. And, whether the patterns you observed were influenced by the study participants' engagement with the 'at home intervention'. Also, a few tables would help to describe the sample, the timeline over which data were gathered & analysed.</p> <p>Also as a note, white, aboriginal & other are not ethnicities...</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Overall, this article has the potential to make an important contribution if revised to improve the framing of the analysis and implications of findings. It is highly innovative and important that the authors utilize linked longitudinal administrative data. Given the uniqueness and richness of the data, some of the summary statistics alone are very illuminating and clearly important in terms of contributing to debates and for policy recommendations (such as those on p. 9, lines 36-53).

We have revised the Introduction to reinforce debate in existing literature regarding the location of services and to clarify the scientific and policy context for the current paper.

The vulnerability and insecurity of the population of study and the longitudinal nature of the administrative data utilized are the paper's strong point, but they also create methodological challenges, requiring well justified decisions, for example, on how to treat missing or incomplete data. Overall the regression analysis utilized in the article is complex, and the findings less persuasive, and perhaps potentially problematic. I attempt to highlight some potential conceptual and methodological

challenges, but feel a reviewer more familiar with the specific GEE method chosen for the analysis would be able to provide A critically important assessment of the authors analysis and conclusions.

GEE has become a standard method for analyzing repeated measures or other correlated response data that could be either continuous or discrete in type. An advantage of GEE is the ability to manage missing or incomplete data. GEE estimates of model parameters are valid if missing data are random. In the present analysis, we have chosen GEE due to the longitudinal and count nature of our outcome variables. We have added the following citations that describe the general approach & assumptions of GEE as well as methods for dealing with missing data.

In terms of framing, this paper is somewhat disconnected from the research literature on the DTES and homelessness in Vancouver. Despite listing many sources related to the issues of homelessness, mental health, etc..., the authors do not explicitly develop their analysis from key findings of the previous research. It would strengthen the paper if more of these key insights or debates were included in the paper itself to set up the analysis. The paper would be stronger if more explicitly framed in light of a debate in the literature about homelessness, mental health and migration, going beyond the current framing which is justified as addressing a somewhat vaguely defined gap in the literature. More specifically, the abstract requires revision. The debate about whether or not services for homeless individuals should be located in neighbourhoods such as the downtown eastside or other "spatially diverse" neighbourhoods should be highlighted at the beginning of the abstract, if this will ultimately be the the policy recommendation that the authors stress in their conclusion.

We have added text discussing the debate regarding the location of services and supports for people who are homeless and mentally ill. We agree that this addition improves the clarity and relevance of our study.

In the article, the authors identify some potentially important correlations between migration to the DTES, a spatial concentration of homeless individuals, and hospital admissions, criminal conviction and financial assistance. Yet they go too far in asserting that this is a causal link and these outcomes necessarily reflect the impact of migration to the DTES. In other words, the authors problematically fail to control for some of the unmeasured, but important, differences between the population of homeless individuals studied that end up migrating to the DTES and those that do not. These unmeasured differences could be argued to be causally related to the outcome variables of interest. This results in omitted variable bias, and the inappropriate assigning of causal power to the variable "migration to the DTES" for explaining heightened rates of "hospital admissions, criminal conviction and financial assistance". I am very sympathetic to the conclusions and the especially the policy recommendations presented in the conclusion, yet feel the evidence presented in this version of the article does not provide more than suggestive evidence to support these claims. I acknowledge this central limitation is listed as a line 16 on p. 3 as part of the strengths and limitations, but it is so fundamental that needs to be presented much earlier in the abstract and the appropriate caveats listed in the conclusion of the abstract.

We fully agree and did not intend to suggest a causal association between variables. The manuscript has been carefully revised to eliminate unintentional inferences regarding causality. A statement has been added to the Abstract calling for further research to investigate the causal relationships between variables that were shown to be correlated in the current paper. A similar statement has been added to Discussion.

In addition, the relationship of the sample to the broader Housing First research and

Vancouver At Home study needs to be included at the beginning of the abstract to further improve the contextualization of the research and sample in the broader field.

More specifically, on p. 5, line 7-9, the authors should include a justification theoretically or from the literature as to why the authors hypothesized this correlation.

We have clarified and expanded on the section describing the relationship between the present study and the Vancouver At Home project. Supporting references have been added regarding the closure and limited capacity of Riverview Hospital during the study period.

Some of the decisions in the analysis of the data require further explanation and justification. For example on p. 6, line 35, I feel it is somewhat problematic that the authors decided that all respondents with “no known address” were assigned as living in the DTES, given that those with “no known address” are not a random group within the sample, and the importance of migration to the DTES as an independent variable in your analysis. At a minimum, I would like to know what percentage of your sample had no fixed address (later on p. 8, we learn this declines from 20% to 3%), and the robustness of your findings if these cases were removed from the analysis as an alternative approach to adjusting the analysis to account for the missing data. Including a discussion of some other empirical evidence that most of the homeless without fixed addresses are living in the DTES would also support this critically important decision in terms of the analysis of the data.

We did not analyze all respondents (n=497). The primary analysis was restricted to participants who provided consent and were linkable to health records (n=433). Among participants with unknown/missing location, we only considered participants who were registered as living within the city of Vancouver with an unspecified location (termed as Vancouver Unknown Place). These individuals were assigned to the DTES because it is the neighbourhood containing the overwhelming majority of homeless people in Vancouver. We clarified this in the text.

Analyses were re-run eliminating participants whose location was “Vancouver Unknown Place” and the findings were consistent with those reported. We have added the findings of this sensitivity analysis as an appendix and report them in the text. Supporting evidence has been cited to substantiate the statement that the majority of Vancouver’s homeless are concentrated in the DTES neighbourhood.

More generally, I worry about missing data and how the treatment of missing data impacts the findings presented in the analysis. For example what happens with the 13% of the sample for which administrative data was not available? Given the population, presumably, some respondents passed away over the study period? How was that dealt with in the analysis?

In the current study, missing data occurred for the following reasons: lack of consent to administrative data (n=60) or unmatchable (n=4) or unknown location (primary independent variable). We did not find any significant differences between participants who provided consent and were linkable and who didn’t (Table 1).

As noted above, we conducted sensitivity analyses among cases with non-missing locations, and confirmed the results reported. As a retrospective study, there was no possible mortality during the study period. We have edited text in Methods to clarify the structure of our research design.

I am also somewhat concerned that females only represent 26% of the sample. One question I would like the authors to address, especially in their conclusion, is the potential counter argument that the more interesting finding based on the analysis is that service

utilization by individuals was not statistically significant by spatial location except for in these three domains?

We have added text stating that the percentage of females in the sample is similar that observed in the Vancouver homeless count. We also added to our discussion of findings to further address the lack of significant differences between participants in different neighbourhoods. We agree that this is an important and interesting result.

p. 10, line 40-51. I am very uncomfortable with the conjectures presented in this section as they are only weakly supported by the data and analysis. Again I am sympathetic to the recommendations, but think the article would be much stronger if it simply highlighted the correlation, and then perhaps suggested some future research to test these ideas as an alternative. The conclusions presented on p. 11, lines 12-21 in the article are much more modest, but more strongly supported.

The discussion on p.10 has been revised with the addition of supporting evidence and the editing of text in order to be consistent with the summary statements presented at the conclusion of the manuscript (p.11).

Minor points:

I also recommend the following minor revisions to strengthen the article:

p. 3 Line 15 Consider shifting the tense: "Hypothesized" should be "hypothesize"

p. 3 Line 17 "public service involvement" is awkward, please reframe, perhaps "utilization of public services"

p. 3 line 20-21 be clear, recruitment was in the downtown eastside neighbourhood of Vancouver

p. 5, line 18-19 The authors need to pick Canadian or American spelling of "neighbourhood" and be consistent throughout the paper.

We appreciate the above suggestions to further strengthen the manuscript. In each case the suggested revision has been made.

p. 7, line 22 It would be helpful to justify why the authors chose the "number of social assistance payments" as opposed to the total amount of payments as the variable.

We chose "number of social assistance payments" as a continuity of social support. However, findings based on the total amount of payments did not differ significantly from the analyses based on "number of social assistance payments". We added this in the text.

p. 11 line 24, I am not comforted by the presence of "among those??" in the middle of the sentence.

p. 12, line 37, capitalize "J" in Journal

We appreciate the above suggestions to further strengthen the manuscript. In each case the suggested revision has been made.

Reviewer: 2

Reviewer Name Judith Lynam

Institution and Country University of British Columbia

Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below

This is an important study that seeks to make sense of a significant problem – in a community that has a significant concentration of people living in poverty & with mental illnesses – how might we most effectively treat & support people living in poverty facing multiple health challenges).

In what follows I share some observations about the structure of the paper & the clarity of the writing style.

introduction – implies directionality... homelessness – produces mental illness etc... I would shift the language to more clearly identify that people who are homeless are also more likely to be also living with x, y, z.

As noted above (in response to a similar comment by the first reviewer) we have altered the text throughout to consistently state the correlational nature of our findings, and the limitation that causal inferences cannot be made from our results.

it would be good (early in the paper) to define how you're using 'migration' & I would suggest that you also identify why migration is an interest, particularly in health research. (e.g. from a service provision perspective – shifts in demand & thus need to put in place resources; , but also because we know that social connectedness, &/or having integrated networks of support can be protective against adversities including poverty, illness etc. Migration typically disrupts such social networks.

Text has been added to address this important point in the Introduction.

Similarly, although there is a suggestion that services play a role in 'migration' – it would not be unreasonable to also indicate that healthcare organizations -& communities need to respond to changing demographic profiles. One consequence can be that people elect to migrate to access services.

We agree and have added to both Introduction and Discussion to further address these dynamics.

Recruitment Clarify procedures & relationship of your study to the broader study.

Recruitment was conducted with collaborators? Do you mean, community partnering organizations assisted you to identify people who met the study criteria to invite them to participate in your study? Or, did these 'collaborators' have a role in identifying people for the home first initiative from which you obtained your sample? Or,...

Recruitment procedures have been edited for clarity.

Language when discussing publications that have shown the positive impact, I'd add some words that capture the direction of the impact... like... reductions (or increases) in emergency room visits; reductions in involvement with criminal justice system...

We have specified the direction of impact in previous studies when this was previously unstated.

Methods

The following sentence doesn't make sense...needs edits & should explain what type of data; gathered from whom and most importantly indicate that the analysis aligns with the goals of the original studies...variables are collected.....

“study involves the retrospective analysis of data associated with sample recruited for experimental trials (ISRCTN57595077 (Vancouver at Home study: Housing First plus Assertive Community Treatment versus congregate housing plus supports versus treatment as usual) and ISRCTN66721740 (Vancouver At Home study: Housing First plus Intensive Case Management versus treatment as usual)). All variables included were collected pre-randomization

We have edited and revised text detailing the relationship between the current sample and the Vancouver At Home project.

The data gathering & analysis plan..would be stronger if you identified the nature of the data (to say it's from questionnaires is fine, but saying what the nature of the concepts etc. that are being examined, similarly, most readers won't necessarily know what you mean by 'administrative data'... so, I would describe this too (is it # & types of clinical visits; etc.?)).

We have clarified that the current study used exclusively administrative data for retrospective analyses, and that questionnaires were used only in selecting the sample. Details have been added concerning the major domains or constructs assessed by recruitment questionnaires.

Similarly, you indicate that you used descriptive statistics, but again it's not clear what you are analyzing & correlating etc. as I read on I see in subsequent paragraphs you identify the dependent & independent variables – which starts to bring the purpose of your present study into focus.

Descriptors are presented in the analytic plan clarifying how specific statistics were applied in our study.

The discussion of your analysis is good as it's here that I begin to apprehend what you have actually learned. It's where you begin to 'unpack' why it could be that people like those in your study, despite accessing multiple services, experience declining health & increases in engagement with criminal justice system over time. & You call into question the assumption that more services is better.

But, you do not come back to the central concept – migration – it's unclear in the analysis how migration is considered in the pattern of service use etc.

Text has been added to clarify the potential significance of the observed association between escalating service use, declining health, and migration in the sample.

Moreover, at this point in the paper It's still unclear the nature of the relationship between your study & the at home study – earlier in the paper you imply that engagement with the at home model improves these peoples' experiences – but your analysis suggests that that isn't the case. Is this because you have included the 'whole' sample rather than focusing on those who were in the intervention?

We have clarified that the analyses presented in this manuscript fall entirely before randomization in the Vancouver At Home study.

Overall, there needs to be a lot of editing done to more clearly link the reasoning that underpins your study, to clarify the links between your study & the 'parent' study, etc. It's unclear for example, when people provided data (in the questionnaires) and when you were only working with 'Administrative' data. And, whether the patterns you observed were influenced by the study participants' engagement with the 'at home intervention'. Also, a few tables would help to describe the sample, the timeline over which data were gathered & analysed.

These important points reiterate comments made above. As stated, we have clarified the relationship between the current study and the randomized trials, emphasizing that the analyses presented here use data from administrative sources and for the period prior to randomization.

Also as a note, white, aboriginal & other are not ethnicities...

We agree that the above descriptors are typically used to describe race. However, our data reflect self-reported descriptions of participants' membership in groups. For example, a Metis person could self-identify as "White" or as "Aboriginal". Due to the self-report nature of this variable we believe that it is more closely related to the construct of ethnicity than to the biologically-based concept of race.

VERSION 2 – REVIEW

REVIEWER	Daniyal Zuberi University of Toronto, Canada
REVIEW RETURNED	02-Oct-2015

GENERAL COMMENTS	The authors have done an excellent job utilizing the feedback from the review to improve the clarity of the contribution, including a much more explicit discussion of limitations. They have addressed the concerns raised, and the revisions have enhanced the clarity of the important contribution made by the paper. I particularly appreciate the completion of additional analyses as part of sensitivity analysis to address concerns related to the treatment of missing data, which further enhanced my confidence in the validity of the findings. Overall, the paper presents important findings that should spark further debate and hopefully research as recommended.
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