Author's response to reviews

Title: Multimorbidity prevalence and patterns across socioeconomic determinants: a cross-sectional survey

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Author's response to reviews: see over
12\textsuperscript{th} December 2011

Dear Editorial board of BMC Public Health,

Thank you for considering our manuscript for publication in your journal.

Please find enclosed the revised manuscript entitled “Multimorbidity prevalence and patterns across socioeconomic determinants: a cross-sectional survey”, by Agborsangaya Calypse B, et al.

We find the reviewers’ comments very insightful and useful. We have also attached a “response to reviewers’ comment”, indicating where changes have been made, with accompanying explanations. We hope to receive a favorable response from you.

Sincerely yours,

Agborsangaya Calypse Bessem, on behalf of all authors.

**Corresponding author**: Jeffrey A. Johnson at the Department of Public Health Sciences, 2-040 Li Ka Shing Center for Health Research and Innovation, University of Alberta, Edmonton, Alberta, Canada, T6G 2E1
Response to reviewers:

Title: Multimorbidity prevalence and patterns across socioeconomic determinants: a cross-sectional survey

1. From Reviewer: Bruce Guthrie
   We sincerely thank the reviewer for the insightful and useful comments.

Comments and responses

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

Comment: I think the paper has to more critically discuss the measure used. There is no gold standard, although the authors should reference the most recent systematic review of 39 measures reported in the literature (Diederichs, C., K. Berger, et al. (2010). "The Measurement of Multiple Chronic Diseases—A Systematic Review on Existing Multimorbidity Indices" J Gerontol A Biol Sci Med Sci First published online November 26, 2010 doi:10.1093/gerona/glq208). Of note is that their measure does include all 11 of the ‘core’ conditions recommended by Diederichs (which is good), but examples of self critique that would be useful include:

Authors’ response:
- Page 14, para 2, lines 6: Thank you for the comment and suggested reference. A strength of our study (the inclusion of all 11 recommended chronic condition) has been noted and the study by Diederichs C., et al (2011) appropriately referenced. In page 14, para 3, we have extensively discussed the limitations of our study, noting that our study still includes only a limited number of chronic conditions.

Comment: a) Overlap between conditions, for example that ‘arthritis’ and ‘chronic pain’ is the commonest morbidity pair. Since the main symptom of arthritis is pain, this is perhaps not surprising. I am not arguing for the authors to change their measure, because the arbitrary nature of disease definitions in practice means that such overlaps will always be common (cardiovascular diseases being an example, where a previously ‘normal’ blood pressure will be transformed into hypertension by a new diagnosis of diabetes immediately making the patient multimorbid), but the discussion would benefit from a more nuanced discussion of measurement.

Authors’ response:
- Page 15, lines 4 -7: Our discussion indicates the potential for over-estimation of the true prevalence of multimorbidity due to redundancy, introduced by overlapping chronic conditions such as arthritis and chronic pain. Because patients reporting chronic pain may experience the condition, independent of arthritis, the extent of over-estimated prevalence cannot be determined.

Comment: b) Similarly, they critique other papers for only including a small number of conditions, but actually their own measure is pretty average in the number of conditions it includes (see Diederichs), and I think it would be helpful for them to specifically acknowledge what they are not measuring (eg other common conditions like alcohol problems, epilepsy, atrial fibrillation, hearing loss, visual loss and so on, many of which are much more common in older people). I don’t think that including more conditions would actually alter their main conclusions (although if nothing else it would increase the
estimated prevalence), but good to discuss at least some of the measurement issues, and the current tone implies that their measure is a superior one (“Moreover, important chronic conditions such as obesity, as well as anxiety and depression were included in this study. We have therefore provided a broader depiction of multimorbidity”). Broader than narrower measures, but narrower than others.

Authors’ response:

- Page 14, para 2, lines 4 -7: The statements have been rephrased to indicate that our study only included a “modest” number of chronic conditions. We have deleted the statement that suggests that our measure of multimorbidity is superior, “We have therefore provided a broader depiction of multimorbidity”. Details of the limitations of this study, including the inclusion of only a limited number of chronic conditions, are well elaborated upon on page 14, para 3.

Comment: c) Linked to this, I don’t know if the authors designed the survey used, or are opportunistic users. If they designed it, then they should explicitly justify the inclusion of the conditions counted. If they are opportunistic users, then they should acknowledge this as part of discussing the potential limitations of their measure. Just to reiterate, I do NOT think that their measure is a bad one, because all such measures are limited since measuring the totality of human disease in large numbers of people is unlikely to be feasible. But I do believe that the authors should be explicit about what is being measured (show us the survey questions) and why, and be self-critical/analytical about how the measurement choices should influence interpretation.

Authors’ response:

- Page 6, line 14: The survey question on chronic conditions from the questionnaire is now stated as is in the manuscript.
- We thank the reviewer for this important comment. We have provided the source for the survey from HQCA and cited this in the manuscript (page 6, para 1, line 2).

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct).

Comment: The authors should add an appendix which shows the survey questions on which the allocation of diseases is based.

Authors’ response:

- Page 6, para 2, line 1: The survey question on chronic conditions from the questionnaire is now stated as used in the survey instrument.

Comment: Figure 1 is pretty redundant. A couple of sentences of text that described Alberta would be more useful for non-Canadians, and in particular the age structure in Alberta. The main point is that the data are standardised to the Albertan population, but my guess would be that they are younger than average for Canada and probably for the rest of the developed world (because of a booming economy and therefore likely in-migration – I have no idea if that is true, but that’s what I want them to tell me). So interpreting the prevalence estimates would be helped by more information about that (ie if true, then most developed countries will have higher rates of MM than found here because they have more older people). I don’t understand the ‘educational level’ variable, and in particular the distinction
between ‘college’ and ‘university’. Since these terms have different meanings in different countries, the methods should spell out what the three categories used mean.

Authors’ response:

- The figure 1 has therefore been dropped.
- Page 6, para 1, Lines 8-10: Statement explaining the use of sampling weights to adjust for under- and over-sampling.
- Page 7, para 2, lines 3-5: A description of the educational categories has been clarified.

Comment: I am sure that the authors have carried out a series of logistic regressions where the outcome is the presence of multimorbidity (binary 0/1). But the table headings then seem very misleading (“Univariate analysis of demographic characteristics associated with respondents with multimorbidity compared to those without multimorbidity by age groups.”). If I am correct, then the OR refer to the odds of having MM compared to the reference group in each category, but that table heading implies something very different. The authors should make them clearer (or explain what model they’ve fitted in the methods if it’s something different).

Authors’ response:

- Tables 3, 4 and 5: The table titles have been rephrased to improve on the clarity.

Comment: Few trivial typos to correct. P7 line 2 “ore”. P7 final para line 3 “statistical significant”. P7 final para line 5 “analysis” should be analyses. Page 12, para 2, line 4 “have another chronic condition than will be in the general population”.

Authors’ response:

- The typos have been corrected within text.

Comment: Discretionary Revisions (which are recommendations for improvement but which he author can choose to ignore) I don’t think table 5 adds anything much to the paper. For one thing, it doesn’t include the youngest age group (possibly because nothing was significant for them, but if that was the case the text or a table footnote should explicitly say so). But otherwise the analysis essentially reaches the same conclusion.

Authors’ response:

- In table 5, we observe that the associations in Table 4 are consistent across different age groups (explained in the discussions). We think that the findings re-iterate the importance of multimorbidity (and its predictors) across all age groups, and not just in the elderly population.
- A footnote has been included in table 5 on reasons for excluding age 18-24
2. **Reviewer:** Helena Britt
   
   We appreciate and thank the reviewer for the insightful and useful comments. The details have helped us improve on the content of the manuscript.

**Comments and responses**

**Major compulsory revisions**

Overall I think the major issues that need to be addressed (detailed below) are:

A. A little more thought in the discussion about the implications for your results. of ‘low recall, knowledge etc of morbidity through patient self report.

**Authors’ response:**
- Page 15, lines 2-5: We have listed some of the major limitations of self-reported morbidity, mentioning also the likelihood of introducing self-declaration bias.

B. Inclusion of some consideration of implications of the ‘missing reported morbidity’ and some indication in the results of the size of this data loss.

**Authors’ response:**
- Please, note that the list of chronic conditions included in this study captures the common chronic conditions. Moreover, the list includes the 11 recommended chronic conditions (Diederichs C., et al (2011)), which makes our findings quite comparable to other studies.
- The chronic conditions listed in the “other diseases” item in the survey are haphazard and capture varied (less frequent) chronic conditions. These may be considered in subsequent analyses.

C. Investigation of the presentation and statements on 65+ Table 5 – are they the wrong way round perhaps… other wise something weird is going on. – sort out

**Authors’ response:**
- Page 11, lines 4 – 6: The statements have been rephrased and clarified.
- A footnote has been included in table 5 to indicate omission of the lowest age group (18 - 24) because of small samples sizes (empty cells within strata).

**Minor essential revisions**

There are many smaller issues addressed below.

1. **Abstract:**
Methods:
1.1 It is important to include self reported” before ‘multimorbidity’ and add ‘by telephone interview’. These are essential parts of the Methods. Correct (‘over 18 years’), to read (‘18 years and over’).

Authors’ response:
- “Self reported” and “by telephone interview” have been added to the methods section of the abstract.
- “over 18 years” has been changed to “18 years and over”

2. Background:
2.1 Para 1: you give the impression that obesity is now included in the definition. This is Starfield’s approach but has not been taken up much elsewhere. Need to make it clear it is Starfield, not all multimorbidity researchers.

Authors’ response:
- We recognize the point that “inclusion of risk factors such as obesity in definition of multimorbidity” is Starfield’s, and has been appropriately cited.

2.2. Page 4: Para 2: First sentence. You fail to report that Fortin’s was in adults, Britt was in the population, and I don’t know off had what ref 11 used. Of course you will get different results with different age groups. Please add a bit more detail.

Authors’ response:
- Page 4, para 2, line 2 – 3: Accompanying the referenced studies, we have indicated the variations in reported prevalence are due to dissimilar study populations or data sources. Differences in the referenced studies are presented in the discussions (page 12, para 2).
- The reference 11 (Taylor et al (2010)) is an Australian population-based study that reported the prevalence of multimorbidity as 17.1%. Again, noting the varied prevalence of multimorbidity reported in different studies.

2.3 Page 5 last para: Line 1: While ref 25 was published in 2010, it was hardly ‘recent,’ as it compared 2005 CCHS data with 2003 Fortin data. Leave out ‘recent’.
Line 3: Are you sure it wasn’t “25 years and over, rather than over 25 years? Check please.

Authors’ response:
- “Recent” has been dropped.
- “Over 25 years” has been changed to “25+ years”.

Methods
3.1: I don't think you need Figure 1. It adds little to the paper.

Authors’ response:
- Figure 1 has been dropped.

3.2. You included an additional 2 diseases. I can understand this due to their prevalence. However, I wonder the exclusion of all other chronic conditions from the data (those extra listed conditions that were not included in the multimorbidity) affected the results. Don’t you wonder? There is no report of
how many chronic conditions were excluded, so it is hard to have any hypothesis as to the effect.

**Authors’ response:**
- A priori, we determined to include the most common chronic conditions, together with the 11 chronic conditions recommended by Diederichs C., et al (2011) (appropriately referenced in text). As observed in another Canadian study (Fortin et al, 2010), we expect that the effect of increasing the number of chronic conditions will, if any, increase the prevalence of multimorbidity. However, as our intent is to provide findings that are comparable to other studies in the general population, with good external validity, we focus on the select list of chronic conditions. In subsequent analysis, consideration will be given to the effect of including the number of chronic conditions.

3.3 Page 7: line 2 – delete the word ‘defined’. They were not ‘defined’; they were included in the study.

**Authors’ response:**
- “Defined” has been dropped.

3.4: Last para: You describe the selection of factors for inclusion in the regression, but never list them as included in it. The results could be cleared if you specified the ones you included at the beginning of the multivariate result.

**Authors’ response:**
- Page 10, para 2, line 2: The variables entered into the multivariate regression model (age, sex, income, education and family structure) have been listed in the results section.
- Because the variables entered into the regression model are based on the univariate analysis, we think it is more appropriate to list them in the results section.

4. Results
4.1 Page 9: Para 1: It is not clear in para 1 that the results you present are age and sex standardised. Please say so.

**Authors’ response:**
- Page 8, lines 2: In the methods section, we state that the results are weighted based on the age-sex population distribution of Alberta. We think that re-stating the point in the results section makes it redundant.

4.2 Last par page 9-on to page 10. ..’70.2% of those with multimorbidity were aged less than 65 years”. There are no data presented by which I could check this statement. (though you reference it to Table 1 in line 2 Page 10.) Do you need to add another column? Or say (results not tabled at the end of that sentence and give the Table 1 ref twice in the par. Further, Table 1 does not include any data for income yet it is presented in Figure 2 and is included in the Multivariate analysis. It needs to be added to Table 1 as well, so we have the descriptive result.

**Authors’ response:**
- The statement “Results not tabled” has been included.
- Data on income have been included in Table 1 (Results were omitted due to a typographic error).

4.4. Correlates:
It seems to me that NOT living with children was the significant predictor – you incorrectly state that it is ‘living with children’. Are you sure the Table has the results the right way round?

Authors’ response:
- Page 10, para 3, line 4: NOT living with children has been corrected.


Authors’ response:
- The sentence has been re-worded to improve on clarity.

4.6 Page 11 para 2, second sentence: The result for 60,000 – 99999 is marginal or not significant (depending on your view of statistics – I would vote for not significant. I would leave that one out and rewrite to the 60,000 cut off.

Authors’ response:
- Page 11, para 1, lines 4 & 5: We think that the observation is important (though statistically marginally significant). Prior to carrying out stratified analysis by age groups in Table 5, we have demonstrated that household income is a strong predictor of multimorbidity. We think that the marginal significance (wider confidence intervals) observed after stratification by age is a consequence of the reduced sample size.

4.7 Page 11 para 2 last sentence. This statement does not agree with the results. 30-59,999 was NOT significant and <30,000 is either marginal or not different (see comment above). Correct this statement.

Authors’ response:
- Page 11, para 1, last line: We have removed the statement indicating an association between household income and multimorbidity. We, however, think that the wide confidence intervals only point to the lack of adequate sample size following stratification (and not the lack of a true association-evident in the ORs).

5. Discussion
5.1 Page 12 para 1: 3rd sentence: these were the most important of those that were measured – many things weren’t, so add a statement to that effect.

Authors’ response:
- Page 12, par 1, line 4: We have changed the statement to “most important MEASURED predictors… ”

5.2 Page 12 para 1: last sentence. This is a presentation of new results. I can’t calculate it from Table 2, and you haven’t reported this in the results section. Either add the results with per cent ages and CIs in the result, or take this out.

Authors’ response:
- The statement has been dropped.

5.3 Page 13 line 2- replace ‘indicating’ with suggesting that people think’ or something like that.
Authors’ response:
• Page 13, para 2 line 2: Changes made as suggested

5.4 Page 13 para 2 third sentence: add, rather than as a disease in its own right.

Authors’ response:
• Page 13, para 2 line 5: Changes made as suggested

5.5 Page 13 last para: Same problem as earlier: this (according tot your data should read NOT living with children

Authors’ response:
• State has been changed to reflect “NOT” living with children.

5.6 Page 14 line 1: you can’t say the idea is ‘novel’ then reference someone else who has done it. Try ‘the influence of family structure has received little attention in the past’ (or something)

Authors’ response:
• Now Page 13, para 3 line 4: Changes made as suggested

5.7 Page 14 para 2: last sentence ‘broader’ than what?

Authors’ response:
• By using “broader”, we mean surveillance and public health interventions which consider more than a single chronic condition, as explained in the first few sentences of the paragraph.

5.8 Page 14 last para: line 1 (presents – change to prevents).

Authors’ response:
• Page 14, para 3, line 1: Change made as suggested

5.9 Page 15: line 3 – again – they were not ‘defined’ – see comment above.

Authors’ response:
• “Defined” has been deleted

5.10 Page 15 last para: this does not scan.

Authors’ response:
• Page 15, last line: Sentence has been rephrased for clarity

5.11 You need to bring into the limitations, the fact that you did not include (how many?) other conditions reported by the patients, and consider the implications of this.

Authors’ response:
• As noted above, we set to include the most common “important” chronic conditions as recommended by Diederichs C., et al (2011) (appropriately referenced in text). As observed in
another Canadian study (Fortin et al, 2010), we expect that the effect of increasing the number of chronic conditions will, if any, increase the prevalence of multimorbidity.

5.12 IN Table 5 Note the very wide CIs on the OR for 65+ not living with children. I would have thought that the proportion of 65+ living with <16 year olds would be small, and that the wide CIs would be on ‘Living with children’. This further suggests to me that the

**Authors’ response:**
- The statement from reviewer is not completed in the reviewer’s comments from the journal. We are not able to determine the reviewer’s concern on this matter.

6. Conclusion: OK
Tables: and graphics
Remove Figure 1 – not needed.

**Authors’ response:**
- Figure 1 has been deleted.

2. Table against Table 5, on 65+ living with children does not add up (see above).

**Authors’ response:**
- Statements have been changed in text to NOT living with children. Again, “see above” section noted by the reviewer is not complete.
3. **Reviewer:** Stewart Mercer

   The authors thank the reviewer for the insightful and useful comments.

**Comments and responses**

In the methods, the authors do not state how they actually did the analysis to identify common pairs, triad, etc of conditions. This information should be added.

**Authors’ response:**

- Page 7, para 2, line 7 – 10: We have detailed the method for assessing the common pairs, triads, quartets and quintets combinations of chronic conditions.

There were 5010 respondents but out of how many calls? Were repeat calls made to people who did not answer, and if so, how many? What times of the day were calls made, and what days of the week?

**Authors’ response:**

- We thank the reviewer for the comment. A reference to the HQCA survey has been cited in the manuscript (Page 6, para 1, line 2). We think that the details of the survey, including the number of calls included, number responding, time of the day, days of the week, and more, are details which are not within the focus of this study on the prevalence of multimorbidity.

Although the results have been weighted to match the province demographics, it is still possible that important groups were under sampled. There should be more explanation of this, and consideration of this in the discussion.

**Authors’ response:**

- Page 15, line 4 - 6; A statement of the included to capture likelihood of under-sampling important groups (e.g. Immigrants) in the survey. However, households were randomly sampled within health regions. We therefore expect that any under- or over-sampling should be random, with no systematic affect on our estimates.

The authors do not state on what basis the 14 conditions were initially chosen. This should be made clear.
**Authors’ response:**
- The 14 chronic conditions constitute a list of common chronic conditions in the general population, together with the 11 core chronic conditions recommended for considerations in studies assessing the prevalence of multimorbidity by Diederichs C., *et al* (2011) cited on Page 14, lines 14-15.

I am concerned about having chronic pain and arthritis as separate diseases. Although not all chronic pain is due to arthritis, most arthritis causes chronic pain. It's no surprise that in the results these come out as the top pair. For me, this is really double-counting the same thing. There either needs to be discussion of this in the discussion, or the data re-analysed counting these as a single condition.

**Authors’ response:**
- Page 15, lines 1 - 4: A statement to indicate the potential for over-estimation of the true prevalence of multimorbidity due to redundancy introduced by overlapping chronic conditions has been stated as a limitation of this study.

The paper by Mercer and Watt 2007 in Annals of Family Medicine should be quoted as it shows the relationship between poverty and multimorbidity, and also the relationship between multimorbidity and mental health in poor versus rich areas.

**Authors’ response:**
- Thank you for this suggestion. In fact, we find the paper interesting and important as a reference in another analysis of this same dataset, which is focused on the association between multimorbidity and socio-economic status.