Reviewer's report

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

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Reviewer: Bruce Guthrie

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Thank you for asking me to review this paper which reports the descriptive epidemiology of multimorbidity in a fairly small but carefully conducted population survey in the Canadian province of Alberta.

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

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:

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.

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.
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.

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

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

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.

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).

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”.

Discretionary Revisions (which are recommendations for improvement but which
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 across all age groups. So I personally would drop it, but up to the authors.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

No to the first 4, but a potential non-financial competing interest: I currently am a co-applicant on a grant developing an intervention to improve quality of life in people with multimorbidity in deprived areas, and as part of this have carried out some descriptive epidemiology of multimorbidity using routine clinical data with a paper in the late stages of preparation.