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Title: Age, multimorbidity, and dementia with health care costs in older Albertan people: a population-based retrospective cohort study
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Reviewer 1: Ms. Anh Pham
Institution: Alberta Faculty of Medicine and Dentistry, University of Alberta, Edmonton, Alta.

General comments (author response in bold)

Thank you for your work. This study about the association between age, dementia diagnosis, other comorbidities and cost is important and has been reported throughly according to STROBE guidelines. I have a few comments to add:

- Line 37 to 48 on page 3 (introduction) should be omitted as the text described sampling methods which has already been stated in methods section.
See our response to the Editor.
Done.

- In methods, (1) though I understand that inform consents from participants are not required in secondary data analysis, a large sample size should not be a reason to qualified for not obtaining consents. The authors might want to check with their data source policies to see how consents were/were not obtained.
See response 9 above under Editor. To be clear, we are using administrative data. The data custodian is our Health Authority.

(2) Line 28, page 4, I have a concern about identifiers, were the authors able to access participants' full address or just their postal code? I guess the latter but if it was the full address provided, further description may be required to ascertain that patients' privacy was protected adequately.
We only had access to postal code which we needed in order to link to rural/urban status and neighbourhood income quintile.

(3) line 29, page 5, since this study also used data back since 1994, age at first diagnosis might be much younger than 65. I wonder if the authors considered length of having a disease as an effect modifier in the association with cost.
We agree with the comment, but have not considered the duration of (diagnosed) disease in our models. Given that the exposures are already quite complex, we will not address this comment unless requested to do so by the Editors.

- In results, the authors mentioned that 3 and 2-way interactions between predictors and outcome were included in the analysis. However, they did not discuss them. I wonder if there is a correlation between dementia and other comorbidities that could affect the results. Also, is LTC associated with more comorbidities? This may also be a potential confounder that needs to be discussed.
See comment 21b above.

- Female is known to be at higher risk of dementia. Sex as a potential confounder should, therefore, be discussed in more detailed.
We did adjust for biological sex in our models, so sex should not have confounded the results as presented.

- My last comment is if there is a rationale for the authors to count the number of comorbidties, instead of using a validated score like the Charlson comorbidity index (https://www.mdcalc.com/charlson-comorbidity-index-cci) which gives different conditions different weight.
Please see our response to the Editors.

Reviewer 2: Dr. Janet MacNeil Vroomen
Institution: AMC Medisch Centrumz, Amsterdam, Netherlands

General comments (author response in bold)

Dear Authors,

While I appreciate the concept of evaluating joint models, the introduction sorely lacks any background on joint models e.g. Rizoploulos is not mentioned or any other major joint model experts are not cited. The clinical literature is also needing to be updated as there are massive gaps missing on population level studies of costs and multimorbid persons with dementia.

{Editorial note: Rizopolous is well-known for developing models that jointly assess time-to-event processes and continuous longitudinal processes (e.g., progression to kidney transplant and eGFR). However, the models do not mention joint effects of age, dementia and comorbid conditions on cost.   We thank the reviewer for this point but we do not feel the authors need to alter the manuscript.}

I would have liked to have understood how you decided on your model distribution (Zero inflated negative binomial). It was just stated and was not even described. Eg. did you look at other models and did you evaluate of for example BIC? That is really the starting point and therefore it is hard for me to further assess the paper as i think the level of detail is just not here.
We have expanded our description of the model (pages 6-7) we selected to work with and how we arrived at that point. See comment 22 above.

In addition in the methods the authors describe "3-way interaction and all three 2-way interactions" these are nearly impossible to assess which to me create a fatal flaw with the paper coupled with the introduction, and the lack of model distribution explanation. For a CMAJ readership I just don't think there is anything clinically interesting and from a methods perspective it just lacks the details required to be convincing. E.g. Dimitris Rizopoulos has done a huge amount of joint modelling work and his work is not even mentioned which to me is a massive gap for the introduction.
We agree that multiple 3-way and 2-way interactions are difficult to assess. Please see comment #21b.