Reviewer's report

Title: Prevalence and Predictors of Depression in a Canadian Palliative Home Care Population: A Cross-Sectional Study

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Reviewer: Laura Goodwin

Reviewer's report:

This is an interesting study, which has recruited a very large sample for a palliative care population, however, there are issues with the methods and particularly the statistical analyses which need attention.

• Major Compulsory Revisions

1. I think that there are issues with the analyses which need addressing. Firstly, I am not sure why spearman’s rank correlations were conducted for categorical variables, rather than polychoric or terachoric correlations. In the same paragraph on the statistical analyses on page 9 it is also not clear how the variables were selected into the multivariable model. It is stated that variables were excluded to avoid issues of multicollinearity but the choice of which variables were actually removed seems quite arbitrary. Paragraph 3 (starting at the bottom of pg. 9) in the statistical analyses is not clear in relation to the discussion of potential mediation, in particular the sentence ‘the palliative care literature suggests the potential for certain variables to mediate the effects of others’. Finally, in the second paragraph of pg. 10 I am unsure why the authors have used ROC curves examined sensitivity and specificity to test the goodness of fit of the model and this needs clarifying. The use of the Hosmer and Lemeshow statistic seems more appropriate.

2. The presentation and interpretation of the results is not always clear. Firstly, over 2000 participants are excluded from the multivariable model because they have some missing data and the differences between those included and excluded should be investigated. On reading the results I was unsure whether the prevalence of depression that is reported is for the full or restricted sample, but from looking at table 1 it appears to be the full sample. The findings of the multivariable model are not explained well and given that the data is cross-sectional many of the statements in the results are likely to be overinterpretations (e.g. in the first paragraph of page 13).

There may also be an error in the results as it is stated in the abstract that gender is associated with depression, but in the results that it is not. In paragraph 2 on page 13, it is not clear why these results in the full sample (excluding prognostic awareness) come after the model in the reduced sample including prognostic awareness which would make more sense. The sentence in paragraph 2 on page 13 that ‘the above regression suggests that this would have little impact on the results’ does not seem particularly scientific. In the same
paragraph it is stated that a cut-off of 2+ is used for the CHESS, but given that 97% of the sample scored above this cut-off a higher cut-off would provide more discriminatory power.

3. In the methods on page 7 it would be helpful if more detail was provided on the DRS given that this is a new scale. It is stated to be a valid and reliable measure, but evidence for this should be included. In relation to the measure of life satisfaction in paragraph 2 on page 8 there is no information on what this measure is or how it has been developed. Additionally, measures of social support do not appear to be referred to in the methods.

- Minor Essential Revisions

4. In the background of the abstract the statement in the aims ‘to determine the prevalence of at risk patients’ should be changed and the aims should only refer to the outcome of depression. In the results section of the abstract the statement ‘life satisfaction may mediate the effect’ is too speculative for the results, either there is evidence for mediation or there is not.

5. Further details on the recruitment of the sample should be provided in the methods as it is not clear how representative the sample is. As mentioned previously, the issue of participants excluded due to missing data should also be addressed in the manuscript.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.