Reviewer’s report

Title: A Comparison of Symptoms in Older Hospitalised Cancer and Non-Cancer Patients in Need of Palliative Care: A Secondary Analysis of Two Cross-sectional Studies

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Reviewer: Saskia Juenger

Reviewer's report:

Overall comment

The manuscript "A Comparison of Symptoms in Older Hospitalised Palliative Cancer and Non-Cancer Patients: A Secondary Analysis of Two Cross-sectional Studies" overall is a well-written paper that addresses a relevant topic in patient care, i.e. the symptom burden of older hospitalised patients in need for palliative care. It is commendable that the authors draw on two different data sets in order to perform a secondary analysis of two multi-centre cross-sectional studies, and that they compare the symptom burden of people with different primary diagnosis to increase scientific evidence on this issue. The statistical analysis overall appears to be sound.

The implications for clinical practice suggested by the authors are a thoughtful discussion on the need to address disease-specific differences in symptom patterns. The section conveys a balanced reflection of the impact of the findings, including attention to potential barriers to the implementation of recommendations into practice.

I am concerned, however, that the rationale for undertaking this research (p. 5, lines 4-7) with a focus on generating evidence on differences in symptom patterns between cancer- and non-cancer patients is not entirely convincing for at least three reasons.

First, I am not a physician, but I wonder whether it would be more useful not to emphasise the dichotomy between cancer- and non-cancer patients but instead to look at risk profiles of different primary diagnoses?

Second, I am not sure about the innovative character of the study. It is true that in the history of palliative care development, there was a focus on cancer patients, while neglecting the need for palliative care in people with other chronic diseases. However, more recently it has been acknowledged - among others, in the two systematic reviews cited by the authors (Solano et al. 2006; Moens et al. 2014) - that palliative care should be available according to need and not to diagnosis. The authors of the cited systematic reviews conclude that palliative care should rather be made available based on the problems and needs patients exhibit, not based on the diagnostic group to which a patient belongs. It is therefore not clear why the authors of this paper emphasise
the dichotomy between cancer- and non-cancer-patients instead of doing justice to the diversity of different diagnostic groups.

Third, there is a disproportion between the rather strong statement of significant differences in symptoms between cancer- and non-cancer patients and the data reported in the manuscript. In my observation, both in the description of the results and in the tables and figures, far more similarities than differences can be observed. The message of the differences between both groups, however, is rather dominant (in the abstract, in the discussion and in the conclusions) which will imply that this is what readers will keep in mind - while the more balance reflection on the need to carefully assess both generic and disease-specific symptoms becomes side-lined.

In summary, my concern is that the message of this paper artificially inflates the scientific evidence about a seeming dichotomy between cancer- and non-cancer patients, suggesting that these are homogeneous but distinguishable entities of patient groups. In terms of the generation of scientific knowledge in palliative care, this conveys a distorted picture. Focusing the message on symptom burden in people with different complex conditions / multi-morbidity in older people in my view would be more meaningful. The message concerning disease-specific differences is much more balanced in the section on the implications for clinical practice. I would therefore recommend revising the research question / rationale for this paper and the results section in order to align its focus with the clearly more balanced message of the section on implications for clinical practice.

Major revisions

1. I recommend that the research question and the results section are adjusted (see overall comments above) and the paper needs respective restructuring. The authors conclude that further research should evaluate the differences in symptoms between different non-cancer patient groups. I wonder why the authors did not do so themselves? If the dataset of the study focusing on non-cancer patients does not allow for this differentiation in data analysis, this should be mentioned explicitly and critically reflected in the limitations section.

2. The reported differences between the two groups are rather small in absolute terms, and the clinical meaningfulness of these differences does not always appear to be reasonable (e.g. p. 9, lines 50 - 55; the differences in symptom frequency and intensity (Table 3), in absolute terms, are often very minor, even if statistically significant). Also the conclusions (p. 13, lines 47 - 53) suggest that the significant differences clearly overrule the similarities, which in my view is not reflected in the data. Again, the very last sentence of the conclusion is much more balanced and valuable for clinical practice.
Minor essential revisions

3. Data analysis: p. 8, lines 23 - 26: I am not sure whether I understand correctly the meaning of this sentence. Do the authors want to say that they calculated mean values and standard deviations for the frequency and intensity of symptoms, although these are ordinal variables?

4. Data analysis, p. 8, lines 40 - 43: The authors state that 100 cancer patients were matched to the 100 non-cancer patients to control for age and geriatric risk profile. On the one hand, this appears to be a sound strategy to make statistics more robust. On the other hand, this might also mean that the selected cancer patients are not representative for the overall population (or the original sample), which further weakens the overall message of the article.

5. Results, p. 10, lines 18 - 21: The rationale for this statistical analysis is not very transparent - why do the authors analyse intensity only for symptoms reported by at least 50% of the respective patient group?

6. Table 1: Even if no statistical differences between groups were observed, I think it is worth a mention that the living status and the living arrangements differ considerably between cancer- and non-cancer patients (more married people living with partner in the cancer group, while more widow/ers and more people living with others in the non-cancer group).

7. Table 3: To me it is not clear whether intensity of symptoms was only registered for those reporting the respective symptoms? In addition, the coding is confusing, since the table reports values starting from 0, while in the methods section (on p. 6, lines 40 - 43) it says that the scales range from 1 - 4 or from 1 - 5, respectively. For better clarity, I suggest reporting scores consistently; if coding was changed for statistical analysis, this should be mentioned.

8. Figure 1: In the pdf version available to me for review, I am afraid that this figure is of very poor graphical quality.
Discretionary revisions

9. I am personally at odds with the labelling as 'palliative patients'. As an alternative, I would suggest saying 'patients in need of palliative care' (e.g. in the title; p. 6, line 9; or in other instances throughout the manuscript).

10. p. 4, lines 36-38: I am not sure whether the wording of this sentence reflects what the authors meant to say. Obviously, in a systematic review, data from different studies are compared while no direct measurements of primary datasets are performed. Here, it sounds a little bit as if the conclusions of this review are based on arbitrary subjective interpretations made by the respective authors (rather than statistical measurements).

11. The potential of researcher bias (since two different researchers collected data from the patients in both studies) is very thoroughly addressed in the methods section on p. 7. However, I suggest including a critical reflection on a potential systematic bias in the assessment of symptoms in the discussion section under 'strengths and limitations'.

12. The writing style in this paper could be a little bit smoother in some instances; some passages read a little repetitious (e.g. p. 9, lines 26 - 31) or redundant (e.g. p. 12, lines 24 - 31).

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Unable to assess

Are the conclusions drawn adequately supported by the data shown?
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No

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