Article details: 2016-0123	
Title	Prevalence of symptoms at the end of life in an acute care hospital
Authors	
Reviewer 1	Dr. Valerie Schulz
Institution	London Health Sciences Centre, Department of Anesthesia & Perioperative Medicine
General comments (author response in bold)	1. I have significant concerns with the database as the it seems only pain was reported as a distressing symptom (5-10) and all other symptoms were either present or absent and therefore cannot be included as distressing. If this is the case, then this is not suitable for publication under this title. In this instance, the authors can complete a secondary analysis on the database collected, perhaps for cancer vs non-cancer symptoms for example. **Thank you for the comment. I agree that we are not able to label any of the symptoms as distressing as this is subjective. I have changed the title and text of the paper to simply refer to 'symptoms' instead of distressing symptoms. Describing symptoms without using the qualifier 'distressing' is more objective and reflects the data we gathered.**
Reviewer 2	Prof. R. Tekanoff
Institution	Urban Care Health Group, London, ON
General comments (author response in bold)	I compliment the effort of the Authors, the paper provided a meaningful insight into encounters of distressing symptoms for patients during end of life care; interest level for me as a reader was high. I feel that addressing this type of issue will be of excellent benefit to clinicians and associated personnel in helping to understand a terminal patient's potential need for adjunct care to manage symptoms common to the dying within a healthcare setting. Overall, the submission meets the majority of recommendations from the STROBE statement, is well written, and confronts an interesting and relevant medical issue. However, I would recommend a thorough Author review and inclusion of additional information which should reinforce reported observations, provide an opportunity for greater level of reader clarity and understanding, and potentially providing a symptom management framework to medical professionals dealing with patients at the end of life stage. Please find attachments of my evaluation of the submission using the STROBE checklist as a quality indicator, in conjunction with the following comments below regarding specificities within the paper. Since observational studies include the limitation that they are open to dispute, and include running the risk of containing confounding biases, it is important to note that inclusion of more detailed information (i.e. patient descriptors and selection, study design and outcomes measures) can mitigate some of these limitations for the reader. Abstract: 1. Please clearly state the "OBJECTIVE' in the Abstract with a title or separation from the Background section. [Ed note: This is not CMAJ Open style] **Thank you for your thorough review and helpful comments. Please see my responses below under each of your comments. I have not changed the abstract to have a separate 'objectives' section because of the editors note that this is not CMAJ Open style. I would be happy to do so if needed.** Study Design and Population 2. P6: 6-16: Expand discussion to include rationale for one geographic, multisite location in opposition to additional sites outside the Ottawa area. (include in limitations section to address potential differences in protocol for end of life management in Ottawa centers versus other Canadian locations- if applicable). Readers in Vancouver may determine that protocols for end of life distressing symptom management differ, thus weakening the observations and arguments made in this submission. **We only included our multi-site hospital because we had access to the data and we knew that documentation standards were the same across all campuses of our hospital. Including other centers could have introduced bias because measurement of co-variates and outcomes would have been different. As you point out being only a single center will limit the generalizability of our results. I have added this point to the limitations section.** Study Variables: 3. P7: 21-29: For Reader interpretation, contextualization and importance to results (in particular for readers unacquainted with this co-morbidity scale). **I have clarified and expanded the description of the Elixhauser comorbidity score.** 4. Recommended expansion of descriptor for Elixhauser Comorbidity Score. Inclusion of such a variable reporting mechanism should be underscored to highlight the eventual significance of associated co-morbidities with symptoms, as described in the results section. **I have expanded the description of the Elixhauser comorbidity score.** 5. Please provide additional information related to co-morbidities in terms of type, severity and

potential association to contributing factors for death itself. (i.e kidney impairment etc.). This will provide a more substantive description, providing readers with a greater understanding of why comorbidities were the greatest contributing factor to distressing symptoms, as discussed in the results area. Addition of a table outlining the number of patients and types of comorbidities would help define the association to prevalence of distressing symptoms.
I have added a table that contains the 10 most common comorbidities in the cohort and the prevalence of each comorbidity.
The other variable that may be helpful in describing the decedent cohort is the 'most responsible diagnosis' (MRDx). The MRDx is defined as the diagnosis that was responsible for the patients' stay in hospital (not necessarily the reason they initially came to hospital or the diagnosis that caused death). I think this will help the reader better understand the cohort and so I have included the top 5 MRDx in text format.

6. Address the rationale for using the Elixhauser Score-Why this was selected over the Charlson Comorbidity Index, for example?
The Elixhauser comorbidity score is similar to the Charleston in that it summarizes patients' comorbidities and is correlated to long term and short term mortality. We used Elixhauser instead of Charleston because it is a better measure of comorbidities that has a closer correlation with mortality, according to a systematic review of comorbidity measures (Med Care. 2012 Dec;50(12):1109-18).

Discussion:
7. I find it curious that the Authors have reported on numbers of females in the study. As a reader, I would find it interesting if there was an expansion on the the discussion of gender association with the results. This is inferred a key variable which should be elaborated upon, as it is included in table 2. There appears to be an important gender relationship to 2 or more distressing symptoms however this is addressed in the table only as the second row of patient demographics (below age). Any further elaboration would be important.
The difference in symptoms by gender was no longer seen in the corrected model. I suspect that it was confounded by comorbidity burden or another variable and therefore I did not discuss this further.

8. P 10: 24-51:For reader clarity: Require postulation on several results: elaborated discussion should be included on i.e. why a significant number of patients had a family physician and in turn, with 2 or more distressing factors, why admission to an oncology service was noted to be a contributor, for example, etc.?
I have modified the discussion to postulate on all significant results from the corrected model.
P 13: 21-39 Strengths and Limitations
Expansion of this section to include additional discussion of questions which may be raised by readers:

9. Why the authors feel the study is generalizable as the study has been conducted in one geographic centre in Canada? Reporting, treatment protocols, physician and nursing assessments can be as diverse one institution in the same health network potentially differing on any one aspect.
Our hospital is the largest single hospital in our country and includes 2 campuses. We think it is unlikely that our findings represent an outlier. Never the less I agree that because it is a single center study generalizability is limited. I have included this in the limitations section.

10. Is there a potential non-standardization of documentation and reporting affecting the outcomes of the study?
Our hospital has a nursing policy stating that patient comfort should be assessed every hour. The policy in standard across all wards and is enforced by nurse managers. Never the less in an observational study differential measurement of the outcome leading to bias is possible. I have addressed this in the limitations section.

11. Is there any potential for under-reporting the distressing symptoms, and why?
Yes. The medical record likely underreports symptoms but would be unlikely to over-report. I have addressed this in the strengths and limitations section.

Tables:
For Reader clarity:
Table 1
12. Please consider modification to this chart to include a "TOTALS" column (adjacent to the TOTAL SYMPTOMS COLUMN- consider renaming this SUBTOTALS) tabulating all TOTALS for 1 distressing symptom (207) and TOTALS for 2 symptoms (140) as referenced in the RESULTS section. I found myself using a calculator to decipher the TOTALS column.
I have changed the name of the Totals column to: Total with any symptom. If I add another column of totals that adds each row then individuals who had one episode of pain and one episode of dyspnea will be counted twice. I think this would be confusing and make the table more difficult to interpret.

	All figures and tables are adequate. References: Well researched and substantial. General 13. Please conduct and thorough review of all spelling and punctuation. **I have reviewed the paper.**