Author's response to reviews

Title: Comparison of different comorbidity measures for use with administrative data in predicting short- and long-term mortality

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Author's response to reviews: see over
February 20, 2010

Melissa Norton, M.D.
Editor-in-Chief, *BMC Health Services*

Dear Dr. Norton,
Thanks for your e-mail of December 31, 2010 regarding our manuscript MS: 6990569582959496. According to the reviewer and editor’s valuable comments, we have responded below to the individual reviewer comments. Please feel free to contact us directly if we can provide any additional information.

Sincerely,

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Note: We have copied the reviewers’ statements verbatim and attached our responses below each point.
Response to Associate Editor's comments

Q. In the abstract, the authors use the word "performance" where they mean "validity".

A. We use the word “performance” when it refers to the comparison between different comorbidity measures/models. We added one reference in the section of introduction and analysis of methods. (Reference 9: Iezzoni LI: Risk adjustment for measuring health care outcomes. 3rd edition. Chicago: Health Administration Press; 2003.) The c-statistic is used to measure the performance of models predicting dichotomous outcomes. By comparing statistical measures, we can find that one method has better performance than the others. However, the predictive validity of a measure can be shown that it is statistically related to the outcome. Thus, it can be used to describe only one method. We emphasize our validation and comparison to other methods of the Elixhauser method in the abstract because it’s so far the first study in the Asian region.

Response to reviewer 1

Reviewer: Ronald Lagoe

Reviewer's report:

Minor Essential Revisions

Q1. One addition to the manuscript which the authors should consider is some additional description of the three models used to evaluate hospital mortality. To those who are less familiar with these three models, some additional explanation would be useful. This material could take the form of a table or graph that compares the components of the models. This presentation could make a visual comparison of components such as comorbidities, demographics, and other factors evaluated. Some additional narrative material in the text could supplement such a table.

A1. Thank you for the suggestions. We added a table in the appendix and additional narrative material to compare the components of the models in the section of analysis (page 9, line 16).

Q2. Another revision to the manuscript could take the form of an expanded section concerning limitations. The current version includes a brief paragraph concerning limitations at the end of the Discussion section. An expanded version might be included in the Methods or Discussion session. This section could comment more
fully on issues such as the use of a single year of data, two diagnoses, and other population issues. It might be suggested that these limitations could be turned into advantages if the design were applied to other populations in Asia. The simplicity of the design could become a strength as well as a limitation.

A2. Thank you for your valuable suggestion. We added one paragraph concerning your suggestions in the section of discussion (page 16-17).

Q3. One additional revision that might be considered would be a paragraph suggesting other opportunities for replication of this research in Asia. The authors may have knowledge of administrative databases in this area that would allow them to suggest a plan for such an initiative.

A3. Thank you for your suggestion. We have put your suggestions in the last paragraph of discussion (page 17).

Response to reviewer 2

Reviewer: Uzor C. Ogbu

Major Compulsory Revisions

Q1. General
The authors build their introduction on a lack of evidence of the validity of the case-mix adjustment measures in the Asian population and go on to state that is it widely used. They then proceed to state a research question aimed identifying the best of the three methods. The build up and question do not seem to match.

A1. Thank you for the valuable suggestion. The Charlson/Deyo method was used most widely in other countries and also in Taiwan. However, there was no study compared the Charlson/Deyo method with the Charlson/Romano or the Elixhauser method in the Asian region. Evidence of comparative performance is limited. We revised the second paragraph of the introduction to make the statements more clear (page 3-4).

Q2. Data Sources
The authors list their data sources but do not provide specific references relating to the validity of the data and its suitability for research.

A2. We added the statements and references of our data sources (page 6).

Q3a. The authors used CCS from AHRQ but do not specifically list the ICD codes they include. In the CCS classification the code for Bronchietasis is included with
COPD. Do the authors also include bronchiectasis in their study population?

**A3a.** We added the ICD-9-CM codes of each disease category in the paragraph of study populations (page 7). We followed the classification of CCS, and the code for Bronchiectasis is included with COPD [21]. The previous study used the same method to identify study populations [7], so that we can compare our results with them. Otherwise, comparative performance can be examined for different methods while using same population [4, 17].

**Q3b.** The authors do not describe the process of excluding patients or make any mention of missing data in the dataset. They also do not describe the range of hospitals included in the study.

**A3b.** We added the process of excluding patients in the section of study populations. We excluded patients younger than 18 years old and those who were not discharged before 31st December 2002 (page 7). All contract providers must regularly submit claim information to get reimbursement (mentioned in the data sources) Thus, all kinds of hospitals were included. Since all data sources were managed by government and we used all information compulsively needed in the dataset, such as demographics, diagnosis etc., we didn’t have any problem of missing data.

**Q3c.** Is there any effect of including patients admitted but not discharged before the end of the study? It would appear their disposition at one year should be available with the death certificates.

**A3c.** If the patient was not discharged before the end of the study, the claim data will continue to the next year. Since we only use the claim data of one year, we didn’t know when the patient was discharged. Therefore, if we include patients admitted but not discharged before the end of the study, we will underestimate in-hospital mortality. Otherwise, comparative performance can be examined for different methods while using same population [4, 17].

**Q3d.** Did the authors examine the cause of death?

**A3d.** We examined all-cause of mortality (added in the outcomes of interest (page 8). The Charlson/Deyo, Charlson/Romano, and Elixhauser all developed their comorbidity measures by predicting all-cause of mortality.

**Q4.** Comorbidity measures

The authors description of the approach they used to overcome the lack of a scoring system with Elixhauser comorbidity measures is not clear as they have written it. The reference does have a clearer explanation. The authors may want to refer to a paper by
van Walraven et al titled "A Modification of the Elixhauser Comorbidity Measures Into a Point System for Hospital Death Using Administrative Data", Medical Care 2009;47:626-633 as an alternative, or addition.

A4. The comorbidity variables were created as individual categories in our analysis. We revised the section of comorbidity measures to make the statements more clear (page 8) and added a table in the appendix to compare the components of the models. This table could make a visual comparison of components such as comorbidities, demographics of these models. We also added the reference suggested above(page 8).

5. Analysis

Q5a. The authors use of the term "data periods" is confusing, making it difficult to understand their description of the three models used. The authors may want to use the more conventional phrase of a lookback period.

A5a. Thank you for your suggestion. We added one section in the methods to illustrate our definition of data periods (page 7-8). Two data periods, the index hospitalization vs. the index and prior 1-year hospitalizations, were used. The index and prior 1-year hospitalizations had 1-year lookback period. The index hospitalization didn’t have lookback period. In addition, we have tried hard to revise all the term “data periods” into “with or without lookback periods” in our manuscript. However, it’s not easy to make our writing clearer. Therefore, we added one section to explain our definition.

Q5b. The authors include ethnicity (aborigine or non-aborigine) but do not define these groups or provide a reference.

A5b. We added the reference of the family registration file which was used to identify whether the patients were aboriginals or not in section of data sources. (page 6)

Q6. Given the cited preference superiority of the Elixhauser adjustments, can the authors theorize as to why the Charlson based methods are preferred by researchers?

A6. In my knowledge, there may be some reasons of the Charlson based methods preferred by researchers.

1. The Charlson based methods developed earlier than the Elixhauser method. It had already been widely used before its adaptation to the administrative data. The relatively less literature showed that the Elixhauser method had better performance than the Charlson/Deyo method in predicting in-hospital mortality. That is one reason for us to have this study.

2. The Elixhauser method uses both ICD-9-CM codes and Diagnosis-Related Groups (DRGs), thus it is more complicated to use.
3. The Elixhauser method has more variables than the Charlson based methods, thus it needs larger sample size. However, the increasingly used administrative databases make sample size larger enough for analysis.

Q7. The authors should narrow the scope of the conclusions as the measures have been shown to perform differently with patient populations. They may consider exploiting their large dataset by extending the study period beyond one year adding a number of other conditions to their study to show its robustness.
A7. We have narrowed the scope of the conclusion. Thank you for your valuable suggestion.

**Minor Essential Revisions**

**Q1. Study population**
The authors should clarify which patient populations they refer to in the last sentence of this section.
A1. We revised the last sentence in the section of study populations (page 7).

**Q2.** In the first paragraph, as currently written it is unclear to which study population the percentages refer to.
A2. We revised the first paragraph in the results (page 11).

**Q3.** The results are a little difficult to follow. The authors may want to restructure this section either along the lines of diagnosis or lookback period used.
A3. Thank you for your suggestion. We have restructured the section of results and added some examples in order to make the results easier to be followed (page 11-13).