Author's response to reviews

Title: A comparison of Chinese and non-Chinese Canadian patients hospitalized with heart failure

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Author's response to reviews: see over
Dear Dr. Noutsias:

Thank you for the invitation to revise our manuscript # MS: 5414588641034783 “A comparison of Chinese and non-Chinese Canadian patients hospitalized with heart failure” for consideration for publication in BMC Cardiovascular Disorders. We have responded to all of the reviewers comments, please see attached, and made changes where appropriate in the main text. These changes should be visible through track changes. We hope that the revised manuscript is acceptable to the editors/reviewers. Thank you for your editorial consideration.

Yours truly,

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Response to Editors and Reviewers’ Comments

Editor’s comments:

1) Competing interests – We have made a statement at the end of the manuscript that none of the authors have financial or non-financial competing interests.

2) Authors contributions - A statement about the authors contributions has also been added to the manuscript.

3) Acknowledgements - There were no other individuals who made contributions to the paper as described.

Reviewer #1-Nadia Khan:

1. …The authors should clarify what is new in this analysis compared to previous work.

We have added a sentence emphasizing that the manuscript goes beyond incidence and mortality outcomes which were the primary emphasis in previous studies.

2. The paper is generally well written but if numbers are provided in tables, they do not need to be repeated in text.

We prefer to repeat the key numbers in the text in order to emphasize key findings.

3. This paper focuses on hospitalized heart failure rather than all heart failure and heart failure readmissions as much of heart failure is diagnosed and managed as an outpatient. The authors should change the title, results, and discussion of heart failure to reflect that it is hospitalized heart failure and therefore, may underestimate the risk of recurrence and this would select for a sicker HF population. This should also be noted as a limitation.

We have revised the title to indicate that the paper is focused on patients hospitalized with heart failure. [Pg 1 Line 1-2]

4. The authors are attributing causes of heart failure based on comorbidity within the past 5 years. Given the inherent limitations in making this assertion, I would recommend that they change to ‘associated’ with HF.
In selected places in the text, we used the word association in describing causes of heart failure [Pg 6 Paragraph 3 Line 9], [Pg 9 Paragraph 3 Line 2], [Pg 12 Paragraph 2 Line 3]

5. The authors should provide justification of using the Charlson-Deyo comorbidity variables to adjust for comorbidity in their risk adjusted models.

We have added a sentence indicating that this is a commonly used comorbidity index [Pg 6 Paragraph 3 Line 3]

6. Coding accuracy for issues like valvular heart disease, atrial fibrillation, hypertension etc. should be provided, as these are important variables in this paper.

A determination of the coding accuracy of these variables is beyond the scope of the present paper.

7. This paper would have been improved if linkage to a cardiac registry were possible to obtain angiogram data, EF and NYHA classification (please include in the limitations).

We have indicated the revised limitations that our paper would be stronger if we had access to data from a HF clinical registry.[Pg 14 Paragraph 3 Line 6]

8. Development of cardiogenic shock should be included if these data are available to try to gauge the severity of heart failure.

Unfortunately, these data are not available.

9. The authors may wish to comment on why the prescribing rates for HF medications appear to be somewhat lower than expected.

We did not have data on what proportion of the patients who had echocardiograms showing diastolic dysfunction, but this would contribute to the lower prescribing patients.

10. The authors note that as one of the main findings, the mortality, were not different comparing Chinese with non-Chinese patients and that this may be due to access. This does not seem to fit with some work suggesting that visible minorities in Canada see family physicians as much as non-visible minority patients.

We agree that this is speculation on our part but we don’t have the data to confirm whether this is the explanation for these findings. We have removed this phrase from the paper.

Reviewer #2- Miyuki Tsuchihashi-Makaya

1. In the description of the statistical analysis presented in the Methods section, the authors stated that the mortality model included 23 candidate predictors, and the readmission model
included 36 candidate predictors, based on previous studies. A detailed description of these candidate predictors should be included in the manuscript or in a table.

We have included the references 12,13 to the original papers describing these models. Because our paper already has 5 tables, we have included the covariates used in the models in a table as an appendix to the article (please see appendix A).

2. Why have adjusted hazard ratios been used to compare the 1-year mortality between Chinese patients and non-Chinese patients?

We did not use adjusted hazard ratios in the analysis.

3. The authors should provide a more thorough discussion of the differences between this study and the study by Kaul, et al. (Heart 2011; 97:1048-53) concerning the impact of ethnicity on mortality.

Unfortunately, we do not have more detailed information on the clinical characteristics or treatment of the CHF patients in the Kaul paper and so cannot more fully explain the differences beyond what is already discussed in the text.