Author's response to reviews

Title: Predicting Costs of Care in Heart Failure Patients

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Author's response to reviews: see over
August 30, 2012

Dear Editor,

On behalf of my co-authors, I am pleased to resubmit the paper “Predicting Costs of Care in Heart Failure Patients” for publication consideration to Biomed Central.

We have responded to each point made by the reviewers, and tracked the changes made in the manuscript.

Sincerely,

David H Smith, PhD
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Reviewer's report:
Minor Essential Revisions

Comment 1:
Discussion: It would be useful if the last paragraph began with ‘In conclusion, previous studies...’ and that it included at the end a short recap of the meaning of the findings for health care planning of heart failure patients and what future research should be performed.

Response: We have modified the paper as suggested.

Major compulsory revision:

Comment 2:
Introduction: The rational for this study can be better explained. Clinical prediction has different rational than cost prediction and doesn’t clarify the rational for this study. The question as to why it is important to predict cost of patients with heart failure has not really been answered. Is it important for future planning of health services for individual patients? Does the knowledge that health care for a particular patient will be cheap/expensive affect the care for that patient? Aside from that, the study is sound and the paper is well written.

Response: We have added statements to better reflect the rationale for the study.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:
I declare that I have no competing interests

REVIEW OF THE MANUSCRIPT TITLED: “PREDICTING COSTS OF CARE IN HEART FAILURE PATIENTS”

Comment 1:
Heart Failure (HF) is well known to be a major public health issue and imposes a major economic burden in developed countries. HF patients generally present with a heavy comorbidity burden. Little information is known about the association of patient characteristics with costs in HF. Hence, the study is pertinent.

1) Is the question posed by the authors well defined?
The authors sought to examine “a comprehensive account of HF’s events and their
cumulative economic burden among a community-based population”, with prediction of overall costs. While economic studies are pertinent and important, the results (based on overall costs) must be interpreted with caution in HF where mortality rates are high.

Comment 2:
2) Are the methods appropriate and well described?
The study has the potential to be rather informative, based on the data sources available, including patient level data on health care expenditure and utilization (including inpatient and outpatient visits, laboratory results and pharmacy utilization). However, the authors had chosen to report on aggregate figures. A report on results, firstly in its natural units and descriptives, eg number of GP visits, inpatient visits, length of stay, outpatient visits, then cost of the resource use that patients had incurred (either to the end of the study period or death), could be more informative. The results have then to be interpreted with caution, given that lower costs are seen in those with higher mortality. These findings ought to be emphasized in the discussion section and conclusion sections.

Response: We agree with the reviewer that there are alternative ways to present the costs of care. Recognizing the modest of our cohort however, we were concerned about the lack of precision of utilization endpoints (e.g. laboratory, GP visits, hospitalization), so we felt it more statistically valid to focus on the primary endpoint, namely total cost. Importantly, our pre-planned primary analysis endpoint was total cost and we felt it important to adhere to our a-priori plan. But future studies could usefully focus on those endpoints.

We noticed that there were counterintuitive findings for some factors, so we explored how those factors might be related to mortality.

Comment 3:
The general methods used for the costing aspect are appropriate, although there are few methodological issues and queries pertaining to other aspects of the study. Costing for multiple comorbidities – HF patients are generally elderly patients, with a heavy comorbidity burden. In patients with multiple comorbidities, how are the costs handled?

Response: We used the same methods for determining costs for all patients, regardless of the number of comorbidities. We relied on regression analysis to parse the contribution of each factor to the overall cost of care.

Comment 4:
Regression methods – The modeling and regression methods need to be clarified. If specific analyses are not undertaken, it should be stated. The authors have not taken into consideration interaction effects at all in the analyses. There are known interaction effects between gender and comorbidities, and with comorbidities like anaemia and chronic kidney disease (CKD), diabetes and CKD, and hence costs. Such has not been reported.
Response: We agree that there are potentially important interaction effects that we have not fully considered due to sample size restrictions. We were also concerned over multiplicity of comparisons. We did not pre-specify those comparisons and were thus reluctant to undertake such post-hoc analyses. As suggested we have noted that limitation in the discussion section.

Comment 5: Use of referent groups - The authors should use the ‘normal’ groups as the referent, to be consistent in the whole paper (see Table 3) For example:
  o Age groups – the youngest agegroup should be used the referent group, not the oldest (>=85 years).
  o Renal function – the group with “renal failure” (GFR <30 ml/min/1.73m2 should not be used as the referent group. Renal function or dialysis is well documented to be highly resource intensive.

Response: We agree that consistency is desirable for reporting within a paper, but are not aware of any convention that disallows the use of arbitrary reference groups – in this case the last listed grouping. We have elected to leave the referent groups as reported. It should be noted that interpretation of the analysis would not change, regardless of the reference group chosen.

Comment 6: Left ventricular ejection fraction (LVEF) – LVEF could be categorized as HF patients with LV dysfunction or HF with preserved ejection fraction (HFPEF).

Response: We agree that this is an alternative way of reporting heart failure, but feel that allowing readers to see the full spectrum of ejection fraction findings is valuable.

Comment 7: Could the authors also clarify if evidence-based therapy for HF had been included in the regression models and also methodology section, since data source included pharmacy utilization.

Response: We did not include treatments of any kind in the analysis because our focus was on the contribution of demographics and clinical findings and not on treatment related factors. Additionally, because treatments are part of the outcome (cost) we felt that their inclusion in the regression as predictors could cause problems of endogeneity.

Comment 8: Identification of HF patients
- The authors should clarify if the data source (the HMO linked electronic database) has been validated for HF. If so, a reference should be indicated on page 5 in the methods section;

Response: We have now added that citation.

Comment 9: Patient selection
  o Are patients age 18 years and over or 20 years and over? See page 5 and page 17, Table 3, the lower limit of youngest age group is 20;
Response: Thank you for catching that inconsistency. We have clarified the age range.

Comment 10: First echocardiogram (ECHO) – The authors have allowed for 30 days POST ECHO, for identification of patients with incident HF. What about ECHO done 30 days before the incident event? How would such patients be considered?

Response: The date of echo + 30 days was the incident date. All patients had to have both an echo and a diagnosis of HF. Patients with an echo preceded by a HF diagnosis were included, but we waited 30 days post-echo to capture incident HF patients. This has now been clarified.

Comment 11: Missing values – How are missing values handled? For example, the use of ECHO and ejection fraction (which is not commonly reported).

Response: We used a complete case analysis and have now reported that in the statistical methods section.

Comment 12: Definition of anemia – The reference for definition of anemia, based on cut-off limit of 11g/dL should be indicated. The WHO criteria use different cut-offs for men and women, in its definition of anemia.

Response: This has now been cited.

Comment 13: Are the data sound? The higher overall costs found in patients with diabetes and incident patients with HF, are consistent with the findings by Shannon Dunlay et al (2011). Prevalent patients (for HF) would no doubt be on evidence-based therapy for HF. Patients with AMI and co-diagnosis of HF are a high risk group. Hence, it should be analysed as a subgroup, if these patients are included.

Response: Thank you for pointing out the reference by Dunlay. We have incorporated this into our discussion. We agree that patients with HF and AMI are a high risk group. These patients are included with coronary artery disease.

Comment 14: Results could potentially differ should different approaches or methods are used and sample size is bigger. A sensitivity analysis should be undertaken.

Response: Thank you for the comment. We have included a discussion of limitations associated with the small sample size.

Comment 15: Are the discussion and conclusions well balanced and adequately supported by the data? The results on costs have to be interpreted with caution, given that HF has a high mortality rate. As the authors have rightly pointed out, “Analysis of costs in a disease like heart failure that has a high death rate underscores the economic methods to consider how mortality should best be considered in costing studies”.
Response: We agree.

Comment 16: The sentence on page 9: “One potential explanation is a survivor effect, in that those patients who survive to older age with HF have a less severe form of disease”, is to be deleted. This is flawed.

Response: We have removed that statement.

Comment 17: The paper by Dunlay et al should be included in the list of references.

Response: We agree and have done so.

Comment 18: Tables should be amended accordingly in the light of the above comments made.

Response: We have elected to make no changes to the tables.

Reference: