Author's response to reviews

Title: Predictors of mortality among elderly dependent home care patients

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Answer to REVIEWER 1: Danilo Fusco

MAJOR COMPULSORY REVISIONS

1) Why the predictive model was built only for hospitalized patients? The comparison between the two predictive models (whole population versus hospitalized subgroup) may be interesting.

The predictive model only worked for the subgroup of patients with previous record of hospitalization.

2) The study design is not sufficiently clear. It would seem that not all patients have had a one-year follow-up after hospital discharge. If my interpretation was correct, why, in the result section, the authors speak of “probability of dying during the next year”? Moreover, in the discussion, reference is made to the “risk of dying during hospitalization”. This creates a lot of confusion.

In fact, the comment by the reviewer comes as a consequence of a poor writing on our side. The subgroup of patients we studied (hospitalised EDPHL patients) refer to those patients that reported hospitalisation the year before they entered the home care program, not those patients that were hospitalised during the follow-up period. We have corrected and improved the text (abstract, introduction, method and discussion) in this respect to avoid confusion.

3) Authors should better explain the different data collection phases of the study.

We believe we have improved the text in this respect.

4) With regard to the statistical analysis, it is not clear what the authors intend for “Fisher test”. Is it the Fisher's exact test?
We have corrected that and added a sentence in the Statistical análisis section as follows “Categorical variables were analysed using the chi-square test and the Fisher’s exact test”.

5) We are in the framework of logistic regression. Therefore, I am not clear why the authors speak of “risks proportionality”. Moreover, the Hosmer-Lemeshow test is a statistical test for goodness of fit. It assesses whether or not the observed event rates match expected event rates in subgroups of the model population. What is the connection with the proportionality?

The reviewer is correct. We have deleted the sentence accordingly and the text now gains in clarity.

6) It is not clear whether the authors have added a “Charlson index by gender” interaction term. If the answer is affirmative, the interaction term should be better described and commented.

We have clarified this in the text by adding the following sentence: “The formula also accounts for the fact that the Charlson index predicts a higher risk of death among women that men”.

DISCRETIONARY REVISIONS

When analyzing mortality among those patients that were hospitalized, authors might use multilevel logistic regression, in order to take into account the degree of resemblance between patients who were treated at the same hospital.

We understand that it would be very interesting to perform a multilevel analysis so as to account for differences among hospitals. Despite, this was not the objetive of our study in the first place we would have it in mind in future work, since in the present study we did not have information on which hospital was used in each case.

MINOR ISSUES NOT FOR PUBLICATION

1) Introduction: the acronym EDPLH is repeated twice. This has now been corrected.

2) Table 5: the table header is not complete. This has now been corrected.

ANSWER to REVIEWER 2: Claudio Bilotta

MAJOR COMPULSORY REVISIONS

1. Authors should perform again multivariate logistic regression analysis taking into account the variables assessed at baseline only (i.e. without hospital admissions in the year of follow-up) and the discussion should be revised accordingly. As a consequence, the second multivariate analysis, performed on the sub-sample of patients admitted to hospital, should be deleted.

First.. In fact, the comment by the reviewer comes as a consequence of a poor writing on our side. The subgroup of patients we studied (hospitalised EDPHL
patients) refer to those patients that reported hospitalisation the year before they actually entered the home care program, not those patients that were hospitalised during the follow-up period. We have corrected and improved the text (abstract, introduction, method and discussion) in this respect.

Second. It would be very interesting to perform a multilevel analysis so as to account for differences among hospitals. Despite, this was not the objective of our study in the first place we would have it in mind in future work, since in the present study we did not have information on which hospital was used in each case.

2. At page 5 Authors stated that ‘certain selected variables, namely informal carer characteristics such as gender, age, and the value of the Zarit test, were excluded from the (multivariate) analysis as they had too many missing values’. However, this is in contrast with data shown in Table 3, in which there are no missing data among the 1,001 participants. Authors should explain such statement at page 5 and then include caregivers’ characteristics in the multivariate analysis.

As shown in Table 3 only 80% and 86% of patients (depending on the groups we refer to) have an informal carer. Therefore, if we had included all patients in the logistic regression we would have excluded from the study 20-14% of patients with no informal carer. We changed the word “missing” to refer to patients with no informal carer.

3. Given the observational nature of the study, in discussing their findings – and in particular in discussing about the clinical relevance of preventive measures against pressure ulcers - Authors should better take into account that pressure ulcers were found to be a predictor of death, not a cause of death. Several potential confounders – i.e. causes of both pressure ulcers and death - were not considered in this study such as malnutrition, frailty syndrome and severity of comorbidity: the Charlson Index, which was used in this study to assess comorbidity, does not consider the severity of diseases, such as severity of dementia, heart failure, chronic pulmonary diseases. Thus severity of comorbidity was underestimated in the study and probably for this reason pressure ulcers were a surrogate for higher physical Vulnerability and predicted death.

We agree with the reviewer comment and we have modified the text accordingly.

MINOR ESSENTIAL REVISION
Percentage of participants who died during follow-up (i.e. 290 out of 1,001) should be provided at page 5, Results.

This has now been included.