Author's response to reviews

Title: Predictors of mortality among elderly dependent home care patients

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Reviewer: Claudio Bilotta

Compulsory revisions:

1) It is still not clear in the text and tables if several variables associated with death at univariate analyses were actually included in the multivariate analyses: cognitive status, health-related quality of life, having a care-giver (data about this variable are available for all 1001 participants), Barthel index, some health services received in the year before basal assessment such as teleassistance, after hours emergency community services, health centre emergency services. These variables should be included in multivariate analyses and their results should be shown in Table 6. Moreover, it might be interesting to perform another multivariate analysis – including the Zarit test among covariates - on the sub-sample of the 821 participants having a care-giver.

As regards the first issue raised by the reviewer (additional variables in Table 6), for simplicity reasons Table 6 only reports on those variables in the model that were found to be statistically significant. The other variables the Reviewer mentions in his review were originally included in the analysis and were found to be not significant and thus not reported.

As regards the second issue raised (a new multivariate analysis with those 821 patients having a care giver), we did perform the suggested analysis and we found that the statistically significant variables were the same in this subgroup as in the entire sample. The Zarit test did not appear as a significant variable either. We thus disregarded reporting those results since they did not add any relevant information and made the reading more complex.

2) Again, Conclusions in the Abstract (‘home care programs for EDPLH patients should aim at preventing pressure ulcers and unnecessary hospital admissions’)
as well as the Discussion (in particular page 8 paragraph starting with ‘In the light of the above findings we would recommend key lines of actions…’) should be completely re-written since are absolutely not supported by the study findings. This study could not assess the effectiveness of any intervention aimed at preventing pressure ulcers, hospitalizations and death of participants. This observational study only identified independent characteristics that predicted the risk of death at a one-year follow-up, without considering some possible confounders such as malnutrition, frailty syndrome, severity of comorbidity as previously explained. This should also be better discussed among the limitations of the study.

We agree that although we have shown that these three factors are predictors of mortality there is a need for future research that proves that the interventions we suggest could improve life expectancy and/or quality of life of these EDPLH patients. This has now been changed in the text.

Further, the limitations of the study accounts now for what the reviewer suggests in his review.

1) As clearly written in the Abstract, even in the text and in Table 4 “Total sample” column it should be clearly mentioned that 226 participants were hospitalised in the year before baseline assessment.

The Abstract already mentions this in the first line (results subsection in the abstract). The text in page 6 and Table 4 has now been changed accordingly.

2) Results, first paragraph. Mean age and prevalence of gender should refer to the 1,001 participants who completed the one-year follow-up.

This has now been clarified in the text.

3) Discussion, page 7. The paragraph starting with ‘Published studies have found that admission to hospital is not always to the benefit of these EDPLH patients because…’ might be deleted since it is not related to this study, which considered previous hospital admissions among covariates and not the risk of hospitalization as dependent outcome.

We believe the paragraph should stand since it reports on previous research on the topic (hospitalisation as risk of mortality).

Comments to the Editor:

The study is based on the whole group of patients and on a subsample of these (those hospitalised the year before). We believe the title reflects the aim and scope of the study and further details on the scope and objective of the paper is given inside the text.

As regards the theoretical framework suggested by the editor we understand that our study makes use all the relevant variables commonly available, and used, by family doctors and nurses in the primary care setting.
The editor’s comment on “frailty and pressure ulcers” has now been considered in the new version of the text and in the abstract.

Following the editor’s suggestion, a section on “sampling” has been included for a better reading.

The first paragraph in the discussion section is a brief summary of the main findings in the study. We believe this allows for a better reading of the rest of the section, including the discussion of such findings and the limitations of the study.