Author's response to reviews

Title: Pharmacotherapy and the risk for community-acquired pneumonia: A case-control study of hospitalized older adults

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Author's response to reviews: see over
May 17, 2010

Melissa Norton, MD.
Editor-in-Chief,
BioMed Central-Geriatrics

Dear Dr. Norton:

On behalf of our study team, I would like to thank you for reviewing our paper entitled “Pharmacotherapy and the risk for community-acquired pneumonia: A case-control study of hospitalized older adults.” We greatly appreciate the valuable comments and suggestions provided by the reviewers and the editor for the first revision of our manuscript.

In this submission of the re-revised manuscript, we have integrated all the comments and suggestions from the reviewers and have made all the changes according to reviewers’ suggestion. Our response to each issue is as follows:

Reviewer 1:

\textbf{Reviewer:} Graziano Onder

1. Authors have removed past medical history of COPD from the logistic regression model because of colinearity with variables for inhaled drugs. However they do not present any data to support this statement. What is the correlation between COPD and use of inhaled drugs?

\textbf{Authors’ reply:} We would like to clarify that we have not mentioned in the first revised manuscript that there is a colinearity with variables for inhaled corticosteroids. The reasons that we have excluded COPD history from the multiple logistic regression model are in two aspects:

1). While we have done multiple logistic regression modeling, we have found that the inclusion of COPD history into the current model (as shown in Table 2) will significantly reduce the Hosmer and Lemeshow goodness-of-fit test result as evidenced by the chi-square 16.4341, df=8, and p= 0.0366. When we excluded the variable of COPD history, the result for the fit test is much improved and suggests a better fit to our data as shown by chi-square: 7.7632; d=8, and P= 0.4569. Therefore, it is evident that the exclusion of COPD is a good choice for our model.

2). Because our study is mainly focused on the pharmacotherapy and the risk for pneumonia, we have included inhaled corticosteroids, β2 agonist and anticholinergic bronchodilators as well as ex-smoking and current smoking histories into the model, but not COPD history. We feel that patients with COPD often use one of the inhalers, and medication use and current smoking or ex-smoking histories are better indicators of COPD than the past medical history of COPD itself. We have added the above statement in the data analysis section (with highlight) in this re-revised manuscript.
As far as the correlation between COPD and use of inhaled drugs, Person’s correlation coefficient between COPD history and the inhaled corticosteroid is 0.2987, which does not reveal a significant co-linearity between these two variables. In our study, COPD history itself is an independent risk factor for community-acquired pneumonia (in addition to inhaled corticosteroids) in our data analysis. The main reason that we excluded COPD history from Table 2 is the reason # 2 as mentioned as above although the reason #1 also suggests that it may be a good thing to do.

2. The lack of data on cognitive function must be listed among the limitations of the study.

Authors’ reply: We have added the statement in the last paragraph of the discussion section.

Reviewer 2:

Reviewer: Claudio Pedone

1) The authors provided a table showing differences between PPI users and non-users. I am still convinced that a table showing the discharge diagnoses of the controls would be helpful to better understand the results.

Authors’ reply: We have added the discharge diagnoses in Table 1 under the controls and the cases columns. We also provided one new sentence (highlighted) in the result section. We authors would like to keep the Appendix Table for a reference.

2) The authors consider albumin levels a protective factor against pneumonia. Strictly speaking, it is not higher albumin that is protective, but lower albumin that is a strong marker of malnutrition and in general of poor health. The variable should be analyzed accordingly, i.e. showing the increase in risk with decreasing albumin, not the other way around.

Authors’ reply: We authors completely agree with the suggestion. We use the normal serum albumin level 4 gm/dL as the cut-off value. Then we use the number of serum albumin levels below 4 gm/dL (i.e., 4 minus patient’s serum albumin level) in the multiple logistic regression model (see the revised Table 2). We have demonstrated that lowering one gm/dL of serum albumin levels independently increases the risk of CAP 1.87-fold (AOR=2.87, 95% CI = 2.00 – 4.12). We have made the changes regarding the lower serum albumin levels and the risk of CAP throughout this re-revised manuscript whenever it is applicable.

We believe that the above changes have strengthened our paper considerably. The authors also attest that the work presented in this revised manuscript has not been published elsewhere nor will we submit manuscripts in the future that duplicate this paper. We sincerely hope that our paper will be accepted for publication in your prominent journal. If you have any questions or need additional information, please contact me at your
convenience.

Respectfully,

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