Reviewer's report

Title: Pre-analytic factors and initial biomarker levels in community-acquired pneumonia patients

Version: 3 Date: 11 June 2014

Reviewer: Arturo Huerta

Reviewer's report:

Major compulsory revisions

In this paper Kutz & cols. Make a characterisation of the inflammatory profile in patients with CAP on the ProHOSP cohort. The principal critic to this paper is that not all the patients where characterized systematically, thus conclusions on the actual value of the pre-analytic factors may be overestimated/underestimated according to the population analysed. There is very little information on the actual characterisation of the comorbidities analyzed (chronic renal failure, chronic liver failure) or its aetiology.

Minor essential revisions

1. Page 4. Lines 8. Substitute is for “are”
2. Beginning of the paragraph should be referenced. Why do the authors do not take into account for the evaluation of other comorbid conditions such as COPD (high leucocytes and high CRP) or cardiovascular (high CRP) diseases that are widely known as inflammatory conditions that might affect your results?
3. Introduction should be structured according to the rules published in the Authors Instructions (http://www.biomedcentral.com/bmcanesthesiol/authors/instructions/researcharticle#formatting-background)
4. Methods: Please specify in this part if any of the patient had during the evaluation a CT scan and/or what was the criteria to take them.
5. If I'm correct, the authors focused on kidney/liver function to help stratify patients and to help in the predictive model of the biomarkers and these patients were dicotomized. However, in table 1, the amount of patients liver insufficiency was very little. Moreover, in patients with other comorbidities (COPD and rheumatological patients for example) its very well known that they have their own raised previous systemic inflammatory profile. How can the authors avoid this bias?
6. Patients who were treated with antibiotic/corticosteroid may diminish the predictive power of your model. May I suggest doing a Propensity Match Score to avoid the bias of the previous treatment?
7. Almost 30% of the population analysed were patients with COPD. I suggest doing an analysis with patients CAP VS. CAP+COPD and running again the model. I think you may find really interesting results (perhaps for another paper).
8. Is mandatory that the list of abbreviations for the manuscript be at the beginning of the manuscript to be accepted in the BMC Anaesthesiology format.

9. In the Results section, the authors mention that three factors were associated with different PCT concentration. If antibiotic pre-treatment is important, that encourages even more to do the Propensity Matching Score.

10. The authors should mention in the methods section, exactly at what time of the evaluation the blood samples were taken (i.e first 24 hrs of admission, first 48 hours, just before treatment)

11. A general review of the English is required before being considered for publication.

12. I’m not sure that the statement of the first paragraph of your discussion reflects the importance of your findings. There seems to be enough evidence on the impact of antibiotic pretreatment, specially in the levels of in the CRP and leucocytes, I would try to rephrase it and reference on these other studies.

13. The discussion should be written in a more scientific way, avoiding colloquial phrases.

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

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 interest