Reviewer's report

Title: Risk stratification by abbMEDS and CURB-65 in relation to treatment and disposition of the septic patient at the emergency department: a cohort study

Version: 3 Date: 19 May 2015

Reviewer: Pietro Caironi

Reviewer's report:

Roest AA and colleagues investigated and compared, in a cohort of patients with sepsis admitted to the Emergency Department of their Institution, the predictive validity and feasibility of two severity scores, i.e., the abbreviated MEDS score, and the CURB-65 score, while assessing also the relationship between the score severity classes and clinical dispositions, such as prescription of antibiotic treatment and admittance.

Although the study deals with an important topic (patient risk stratification in the ED), I have the following major concerns:

1) To me, the rationale, the background and the main hypotheses of the study are weak. Why did the authors chose the CURB-65 score as a reference standard for the comparison with the abbMEDS score? What is the real aim of such investigation and, more importantly, the possible clinical relevance? In general, it seems like the authors have chosen these two scores without a real and strong hypotheses, in relation to patients with sepsis. This statement is even more important when considering that this very same group of investigators has already performed a study comparing several severity scores (Hilderink MJ, et al, Eur J Emerg Med 2014). The authors should clearly address this point.

2) Along the same line of reasoning, I do not understand why the authors have chosen the CURB-65, which was created for patients with CAP (and only afterwards tested in patients with infections), for a study involving patients with sepsis. Secondly, and similarly, it is not clear to me why the authors have decided to test the relationship between the MEDS score with clinical disposition, while this score has not been created for this task (but only for mortality estimates).

3) Did the authors include in their evaluation also patients with CAP? If this is the case, did the calculation of the CURB-65 possibly performed by the attending physician influence clinical disposition? The authors may want to analyze their result also excluding patients with CAP.

4) Are the authors sure that all the data were normally distributed? All the tests applied were parametric…

Minor comments
1) The manuscript is too long, especially in the results and discussion section
2) The English should be revised
3) Abstract, results. The authors may want to report first rate of 28-mortality, just to quantify the extent of their primary endpoint.
4) Introduction. Usually introduction does not need subheadings.
5) Introduction, background, line 4. Notably, this value of mortality rate refers actually to the mortality rates of patients with severe sepsis, and not just with sepsis (as the references quoted pointed out).
6) Did you ask for an informed consent? Has been it withdrawn by the Ethics Committee?
7) The rate of adequate antibiotic therapy applied appears quite low to me, and in comparison with other clinical reports. Any explanation for that? Was the site of the possible infection misdiagnosed very often? Similarly, it is quite strange not to observe any difference in mortality rate between patients treated with adequate vs. inadequate ab therapy…

**Level of interest:** An article of limited interest

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

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests