Reviewer’s report

Title: Clinical and microbiological characteristics and outcomes of community-acquired sepsis among adults: a single center, 1-year retrospective observational cohort study from Hungary

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Reviewer: Stijn Blot

Reviewer's report:

Szabo et al. performed a retrospective cohort study to describe the epidemiology of community-acquired sepsis. Emphasis was on bacterial etiology and outcomes. I have the following suggestions to improve the paper.

1) The definition of community-acquired sepsis needs some fine-tuning. As I read it it seems like this is a set of patients with infectious complications occurring early post-admission, but in patients admitted for another reason. While I guess that this is just a cohort of patients with community-acquired infection/sepsis, including patients admitted for this reason, as well as those in which sepsis occurs early in the course following hospital admission.

2) The definition of appropriate empiric antibiotic therapy does not include a timeframe in which the first dose had to be administered. The relationship between timing of appropriate antibiotic therapy and mortality has been repeatedly stressed (e.g. Kollef M. Drugs 2003). In addition, in severe community-acquired pneumonia processes of care reflecting prompt anticipation on admission of a patient with severe infection and as such preceding antibiotic therapy have been demonstrated to lead to earlier initiation of antibiotic therapy as well as improved survival (Blot S, et al. Crit Care Med 2007). It would strengthen the manuscript if the authors could provide data on timing of antibiotic therapy and/or other processes of care facilitating early antibiotic therapy. If not possible, this must be acknowledged in the limitations part of the Discussion.

3) Statistics: please report continuous variables with median (1th - 3rd quartile) instead of mean and SD. The former just gives more information about the cohort.

4) I need information about the estimated annual incidence. Is this on nation-scale? Can the authors be (reasonably) sure that most of the patients with ID pathology are admitted to this particular hospital? If they cannot give a reliable estimate it is better to avoid this statement.

5) Microbiology: data are reported according to sepsis, severe sepsis or septic shock (albeit that severe sepsis is an old concept following the sepsis-3 definitions). The should think about reporting the data according to infection (without sepsis), sepsis and septic shock. Furthermore, they should consider reporting microbiology according to site of infection, although I do not know whether the cohort is large enough for meaningful conclusions. The idea is that microbiology might differ according to site of infection but not according to severity of disease expression (as reported now).
6) I'm questioning the logistic regression model to assess risk factors for mortality (Table 5). In abstract sense only 3 independent variables can be included as only 30 patients died. I assume this is an investment in over-fitting. Also, how can male gender being an independent risk factor for death in multivariate analysis when it was only associated with mortality at a level of p=0.22 in univariate analysis (according to the Methods only variables with p<0.1 were included in the multivariate model). This needs to be reconsidered.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

No

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If not, please explain in your comments to the authors.

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