Reviewer’s report

Title: The Top Tertile of Hematocrit Change During Hospitalization is Associated with Lower Risk of Mortality in Acute Heart Failure Patients

Version: 0 Date: 23 Jun 2017

Reviewer: Susana Ravassa

Reviewer’s report:

The authors have investigated the prognostic value of the hematocrit (HCT) change during hospitalization in patients with acute heart failure (AHF). The authors have categorized 510 patients hospitalized for AHF by tertiles of the HCT change measuring this parameter at baseline and discharge or close to day 7. The authors conclude that an increase in HCT during hospitalization is associated with lower risk of all-cause mortality.

Major concerns

1. There are several spelling and grammatical errors. Please have the manuscript reviewed by an expert in scientific English.

2. It is already known that hemoconcentration is associated with better prognosis. The authors justify the novelty of the current study on the fact that the analysis of the prognostic potential of the HCT change by tertiles has not been previously approached. I do not believe this is enough justification. I think that the authors should reinforce this particular aspect.

3. It is somehow confusing that the authors provide hazard ratios for the risk of mortality in patients with hemodilution or no change in HCT, considering those with hemoconcentration as the reference group, whereas in the conclusion the authors single out the lower risk of all-cause mortality for hemoconcentration (no specific data on the HR for the risk of mortality for hemoconcentration is provided). Either the authors change the conclusion by focusing the message in those patients with a higher risk of mortality (hemodilution or NC) as compared with hemoconcentration, or repeat the Cox regression analysis providing a HR for patients with hemoconcentration, taking hemodilution (or hemodilution plus NC) as the reference group.

4. The univariate analyses should consider medications at discharge as potential confounding factors to be included in the multivariate model.

5. The analyses proposed by the authors are based on tertiles of the HCT change between two time points: baseline and discharge (or close to day 7). In this regard, a table should be provided including changes in relevant clinical or echocardiographic parameters (e.g. ΔNYHA class, ΔLVEF, ΔNT-proBNP etc.), to obtain more information on the clinical and echocardiographic progression of patients between baseline and discharge by tertiles of the HCT change.
6. In line with the previous comment, it is important to determine whether changes in the clinical and echocardiographic parameters of interest between baseline and discharged predict risk of death and therefore should be considered in the multivariate Cox regression models. For instance, have the patients that exhibit hemoconcentration at discharge experienced a higher improvement in LVEF? Does this factor explain the lower risk of death?

7. In addition, if the HCT is proposed as a biomarker independently associated with mortality, it is necessary to demonstrate that this determination improves the classical predicting factors included in the basal models. For instance, does the change in HCT predict mortality with a higher efficacy that, for instance, changes in the NYHA class or LVEF or eGFR? In fact, these analyses could reinforce the novelty aspects mentioned in comment #2.

Minor comments,

1. It is necessary to state whether the Cox models fulfil the proportional-hazards assumption.

2. The sentence in the Abstract conclusion: "… independent risk factor of survival" has no sense.

3. CRP needs definition at the bottom of Table 1.

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

Yes

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

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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