Reviewer’s report

Title: Tissue oxygen saturation as an early indicator of delayed lactate clearance after cardiac surgery: A prospective observational study

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Reviewer: Jaume Mesquida

Reviewer's report:

I have carefully revised the manuscript by Kopp et al. entitled "Tissue oxygen saturation as an early indicator of delayed lactate clearance after cardiac surgery: A prospective observational study". This is a single-center observational study, including 40 cardiac surgery patients, where the authors explored the association between measurements of StO2 on the thenar eminence and lactate evolution during the first 24 hours after surgery. According to their results, the authors conclude that minimum StO2 values at 1-4 hours after surgery were associated to poor lactate clearance (<10%) at day 1.

Although the study is well designed, and the data are interesting, I might expose several major concerns.

1. - The main objective of the study is to explore the association between early StO2 measurements and lactate evolution. Although the authors do find a positive association between these two variables, I have some concerns when generalizing these results, since according to the results displayed in table 1 and 2, the studied population does not seem a very critical population, and their global lactate values are persistently within a normal range. As the authors expose, both hyperlactatemia (lactate > 3 mmol/L) and lactate clearance (time from maximum lactate to lactate of 1.5 mmol/L) have been associated with poor outcome (Maillet et al. Chest 2003; Lindsay et al. Ann Thorac Surg 2013, respectively). However, the present population seems to have low lactate values, and thus that would be a great limitation when trying to generalize the utility of StO2 as a predictor of lactate disturbances, and being so, as an outcome tool.

Did the authors analyze separately those patients with hyperlactatemia at some point of the study? How many patients showed hyperlactatemia during their evolution?

2. - According to reference #2 of the text, early hyperlactatemia is associated with increased mortality, whereas late hyperlactatemia lacks this association, and seems more related to catecholamine infusion. The authors should enhance the discussion on that matter.

3. - Did those patients with hyperlactatemina receive more interventions than the others? How did that affect to lactate clearance? I wonder that patients with elevated lactate were probably actively resuscitated, and that might have influenced lactate clearance.
Minor comments

1. - Results section, page 6, line 16-18: "...lactate concentration and base deficit were persistently impaired". It does not seem so when looking at table 2.

2. - According to the previous exposed concern, I think it would be very interesting to add the use and rate of catecholamine infusion as covariates in the regression analyses. The authors already used several covariates, which might be of limited value when analyzing such a limited population.

3. - I am not quite sure that the association between StO2 < 75% and increase in CK-MB and troponin are clinically significant. As it happened with lactate values, both, CK-MB and troponin, were pretty low.

4. - Conclusions section. I would eliminate the first sentence: "StO2 demonstrated to be a fast and non-invasive indicator of the tissue oxygenation status". In the present study, StO2 was a mere non-invasive measurement of tissue oxygenation, period.

5. - Conclusions section. "A StO2 below ... 75% ... was an excellent predictor...". I would suggest to eliminate "excellent". The AUC is statistically and clinically relevant, but not excellent.

6. - Table 1 should be improved. It should be clearly displayed Euroscore "value" and "%". The type of operation should be displayed also in %.

7. - Figure 3 should be better explained.

**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?**
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Unable to assess

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