Reviewer's report

Title: Cerebral oxygen saturation during off-pump coronary artery bypass grafting in patients with carotid artery stenosis: a retrospective review

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Reviewer: zaccaria ricci

Reviewer's report:

The authors conducted a study on NIRS monitoring: they evaluated the impact of carotid stenosis on intraoperative rSO2 as reported by cerebral NIRS sensors during off pump CABG.

The study is interesting although heavily biased by several limitations that are not fully acknowledged by the authors.

My major concerns are:

OVERALL: The authors should refocus the main objective of their study or specify some important information in the study introduction: cerebral NIRS simply does not measure FLOW! It measures regional microcirculatory (?) saturation, which might be also affected by flow (other than, as correctly stated, by anesthesia, anemia, surgery, low cardiac output, hypotension, hypocapnia, micro and macro emboli and finally monitor errors). That is why all the paper is a huge speculation of something that could happen at a cerebral level as measured by NIRS monitoring.

In particular the authors might have "validated" their findings by cerebral Doppler (correlation with NIRS) or by clinical outcomes (neurological outcomes): the authors have addressed neither of these aspects.

Apart from clearly stating these biases in the limitation section, the authors should retune their article starting from the title: “Retrospective evaluation of carotid stenosis effect on cerebral oxygen saturation during off-pump coronary artery bypass grafting in adult patients”. The subjects of your study are CABG adult patients and NOT patients with CAS.

-Methodology: it is not clear if the authors addressed the duration of intraoperative cerebral NIRS desaturation and if they compared it in the two groups. This is a MANDATORY ASPECT TO BE observed in such a small cohort.

“The rSO2 values from the right and left frontal lobes were averaged to represent regional cerebral oxygenation”: why? Can the authors provide some of these data separated per each lobe? Was the drift from the two sensors significantly different? Was it different in patients with unilateral CAS?

-I do not seem to have identified in the paper what NIRS device you used: please specify
-Sample size: the authors’ calculation is wrong since they focused the whole study on CAS vs non CAS comparison. As is, it appears as significantly underpowered. However, by following my suggestion of changing the subject, sample size power still might work as you presented

-Results: specify the number of studied patients (beyond referring to flowchart)

Differently from what done by the authors I would have analyzed the cohort as a whole, secondarily providing a quick comparison between CAS and non CAS patients (as theoretically stated in the objectives).

-In this line I would have analyzed intraoperative desaturation in the multivariable regression by putting a dichotomous variable "CAS yes/no" into the model in order to assess its effect. In the same line, in my opinion, comparing the results coming from two populations is barely meaningful

-Also, this might better highlight the effect of MAP/vasoactive drugs/Ci on the overall model

Furthermore, as the authors correctly stated in the introduction, surgery/heart manipulation length is a fundamental aspect that should be included in the multivariable model.

-no data are presented on neurologic outcomes in the text

-It might be rather hard to evaluate neurologic outcomes of patient with stroke/TIA in anamnesis. The study should have had to enroll only patients without neurologic issues. Add this in the long limitations section

-Discussion: please significantly shorten it especially all the part with pathophysiological speculations that cannot simply be confirmed by your data. Also comment on the fact that decreases were not that significant from a clinical standpoint (absolute values never really appeared to decrease below the critical level of 40%).

Furthermore what is totally missing in the discussion is the acknowledgment that, if your results actually showed an insignificant desaturation secondary to CAS, it may simply mean that cerebral bi-hemispheric perfusion by Willis circle was working properly in all patients.

Minor

-Abstract

The authors stated that “In patients with CAS, the OR for a decrease in rSO2 > 10% from preoperative value was 0.711 (95% CI 0.547-0.925; p=0.011) for every 0.1 l/min/m2 increase in cardiac index above 2.0 l/min/m2”: this finding is not reported in the main text of the article. Please revise

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable

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

Declaration of competing interests:
I have no competing interests to declare