

Letter to the Editor

Comments on “Cerebral oxygenation monitoring during resuscitation by emergency medical technicians: a prospective multicenter observational study”

Dear Editor,

We read with interest the recently published article by Hamanaka *et al.*¹ These authors reported that the increase in regional cerebral oxygen saturation (rSO₂) during cardiopulmonary resuscitation (CPR) monitored by emergency medical technicians (EMTs) was higher in patients who survived more than 90 days than those who did not, although the increase was not statistically significant. We would like to discuss three issues from their article regarding their methodology, an illogical leap in the conclusions, and an insufficient literature search.

First, we feel their methodology of rSO₂ measurement might be misleading. According to their table 1 showing the first quartile of peak rSO₂ to be 15%, at least 9 of the 33 enrolled patients had a peak rSO₂ of 15%, that is, their rSO₂ values remained flat as the lower limit of detection of their device was 15%.¹ Moreover, their table 2 indicates that one patient achieved prehospital return of spontaneous circulation with a peak rSO₂ of 15%.¹ We assume that they substituted 15% for the unmeasurable value of rSO₂ as per their previous report.² However, in our prehospital study of patients with out-of-hospital cardiac arrest, we did not observe initial cerebral rSO₂ values of 15% or lower at the beginning of the measurement. This could be because of the small sample size, a technical problem of measurement during resuscitation, or differences in the algorithms of each brand of near-infrared spectrometer used. The readers need to be aware of the methodology they used in the substitution of low rSO₂ values. Further research is needed to evaluate the possible variability of measurements of such low values among the different devices.

Second, we feel that their conclusions are unreasonable. Although we agree with the conclusions written in their abstract, the results do not indicate the benefits of prehospital rSO₂ monitoring for assessing CPR quality and cerebral damage, which these authors stated in their conclusion. This may be an illogical leap because they did not provide convincing arguments regarding CPR quality and failed to assess neurological outcomes and cerebral damage as their primary outcome was survival at 90 days.

Finally, the authors stated that there have been no reports on rSO₂ monitoring during CPR by EMTs, which is an incorrect statement likely based on an insufficient literature

search. Although little is still known about cerebral rSO₂ measurement during CPR in the prehospital setting, we previously reported serial changes in cerebral rSO₂ in seven patients with out-of-hospital cardiac arrest monitored by EMTs (or emergency life-saving technicians).³ There are also other uncited reports on rSO₂ monitoring during CPR by emergency medical services personnel in Germany and Canada.^{4,5}

Our research team has been focusing on trends and serial changes in rSO₂ values during CPR. Future studies are required to clarify the role of rSO₂ measurement during CPR to improve patient outcomes.

DISCLOSURE



Approval of the research protocol: N/A.

Informed consent: N/A.

Registry and the registration no. of the study/trial: N/A.

Animal studies: N/A.

Conflict of interest: None.

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