

Ventilation During Cardiac Arrest and After Return of Spontaneous Circulation: More than a Family Affair . . . the Truth Will Be in the Details

Reply:

We thank Wittig and his colleagues for their response to our review.¹ Indeed, the aim of the work was exactly that: to open up a debate on the topic with the hopeful end result of promoting further scientific inquiry.

At present, evidence regarding the best ventilation modalities and parameters to use for achieving return of spontaneous circulation (ROSC) and adequate neurological performance after ROSC and during cardiopulmonary resuscitation (CPR)—and, even more so, during the use of automatic devices for the delivery of chest compressions—is, to put it mildly, scarce. In our review, we pulled together the little evidence available on this issue, attempting to summarize the state of affairs and, most importantly, place it under the spotlight. Therefore, we agree with Wittig et al that the issues highlighted are still in need of further clarification. We also agree that the issues regarding mechanical ventilation during CPR and after ROSC are different. It was for this reason exactly that we excluded all studies dealing with determining the best ventilatory parameters in subjects who had achieved ROSC but, for whatever reason, still needed to be mechanically ventilated.

Regarding the study by Moskowitz et al,² who tried to establish the relationship

between tidal volume and neurological recovery at 6 h and 48 h after ROSC, we closely analyzed the data presented in their study and considered it carefully. Although we cited that study for the sake of completeness, we did not consider the evidence it reported to be strong enough to draw any tentative conclusions from it. Indeed, we stated that further evidence is needed before any conclusions can be made about the most effective tidal volume during CPR.

Regarding the studies by Ebner et al³ and Oh et al,⁴ we agree that the P_{aO_2} in the blood can increase dramatically within just a few minutes after cardiac arrest. We specifically cited these studies to highlight that such controversies exist in the literature regarding the correct amount of oxygen to administer during CPR. In fact, Patel et al⁵ published a meta-analysis on exactly this topic; although it covered just 2 trials, it provides the strongest evidence to date on this topic. Our statement regarding hyperoxia being deleterious in the period after ROSC, but potentially having a beneficial effect on the achievement of ROSC during CPR, is based on the evidence provided by Patel and colleagues.

Ultimately, although we tried to summarize the scant evidence available to date, we concur with Wittig et al that new studies on this matter are imperative. We are delighted that our review has already set a wheel in motion and provoked some (family) debate within the medical community, and we hope that this will lead to the instigation of new trials.

Daniele Orso

Department of Medicine
University of Udine
Udine, Italy

**Luigi Vetrugno
Tiziana Bove**

Department of Anesthesia and Intensive
Care Clinic
ASUFC University Hospital Santa Maria
della Misericordia
Udine, Italy

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Correspondence: Luigi Vetrugno MD, Department of Medicine, University of Udine, Udine, Italy. E-mail: luigi.vetrugno@asufc.sanita.fvg.it.

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