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Letter to the Editor

Importance of effective ventilation during cardiopulmonary resuscitation on outcomes of out-of-hospital cardiac arrest

To the Editor,

I read with great interest the article by Chang et al. entitled 'Association of ventilation with outcomes from out-of-hospital cardiac arrest'.[1] The results of this study revealed the effectiveness of ventilation during cardiopulmonary resuscitation (CPR) and the reasons for obtaining paradoxical results on the chest compression fraction (CCF) from previous studies.[2–4] I would like to discuss several important points related to the study with the authors and readers of the journal.

In 2009, Christenson et al. reported that increased CCF during CPR was an independent predictor of survival at discharge after an out-of-hospital cardiac arrest (OHCA) with shockable rhythm.[2] Those results appeared to be reasonable because interrupting chest compressions for rescue breathing had already been known to adversely decrease coronary perfusion pressure.[5] However, Beesems et al. reported an opposite result in 2013.[3] They reported that longer ventilation pauses were not associated with poor outcomes and significant trends for increased survival to discharge with increasing ventilation pauses. Some differences in baseline factors, such as the existence of dispatched first responder, initial rhythm, and response time, were noted among ventilation pause durations. After adjusting those baseline factors, the ventilation pause duration was not associated with outcomes.[3] Similar paradoxical results were reported by Cheskes et al. in 2015.[4] They found that the odds ratio for survival at discharge decreased with increasing CCF. The results of these two studies confused us regarding what was an important factor for good outcomes (longer CCF vs. shorter CCF).

The problems in the two previous studies were the lack of data on ventilation quality because the technology used to measure ventilation quality in prehospital setting was not yet available at that time. Therefore, we could not discriminate whether effective ventilation was provided or not during chest compression pauses.

Chang et al. resolved the weakness of previous studies using the bioimpedance technology, which showed that the difference in the number of effective ventilations was significant between the two groups (3 [2, 6] in the group with $<$50% ventilation pauses and 8 [4, 14] in the group with $\geq$50% ventilation pauses; $P < 0.0001$). In contrast, no differences were noted in CCF and ventilation pause duration between the two groups. This result showed that the number of effective ventilations is a key component in identifying OHCA outcomes because $\geq$50% ventilation pauses were associated with improved survival rates. Therefore, I recommend that the author should conduct a subgroup analysis of the two groups according to different CCF. If the number of effective ventilations is a key component for OHCA outcomes, the survival rate might increase with increasing CCF in the group with $\geq$50% ventilation pauses, whereas it might show paradoxical results in the group with $<$50% ventilation pauses.

Conflict of interest

The author has no potential conflicts of interest to disclose.

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Je Hyeok Oh
Department of Emergency Medicine, Chung-Ang University College of Medicine, 84 Heukseok-ro, Dongjak-gu, Seoul 06974, Republic of KoreaDepartment of Emergency Medicine, Chung-Ang University College of Medicine, Seoul, Republic of Korea

E-mail address: jehyeokoh@cau.ac.kr (J. Oh).

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