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Letter to the Editor

Tracheal intubation in patients with cardiac arrest: Should we focus on success rate of intubation rather than the outcome of resuscitation?



To the Editor

We read with great interest the article by Okamoto et al. who compared the tracheal intubation performance between video laryngoscopy (VL) and direct laryngoscopy (DL) in 3360 patients with cardiac arrest in the emergency department (ED).¹ Compared to DL, the authors of that study reported that the use of VL to facilitate tracheal intubation would lead to a higher success rate of first intubation attempt (70% vs. 78%, respectively; $P < 0.001$), better glottic visualisation (adjusted OR 3.84 [95% CI 2.81–5.26] $P < 0.001$) as well as lower rate of oesophageal intubation (adjusted OR 0.45 [95% CI 0.24–0.85] $P = 0.01$). Although the findings in that study may provide significant insights into clinical practice in the ED, some important issues regarding the methodology and outcomes need to be clarified.

First, successful intubation was defined as successful placement of tracheal tube past the vocal cords, with confirmation via quantitative or colorimetric end-tidal carbon dioxide (ETCO₂) monitoring in that study. However, although the detection of ETCO₂ using capnometry or colorimetric devices has been shown to be the most reliable method for confirming correct tracheal intubation,² the efficacy of this method can be hindered in situations in which insufficient carbon dioxide is exhaled because of reduced pulmonary blood flow, such as during cardiac arrest.^{3,4} Accordingly, some authors have suggested the use of other available modalities (e.g., esophageal detector device) in conjunction with proper clinical judgment to verify correct positioning of endotracheal tube in cases of cardiac arrest.⁴ Second, despite the relatively low successful first-attempt intubation rate in both the VL and DL groups (78% and 70%, respectively),¹ the rate of oesophageal intubation was only 2% and 5%, respectively. The finding seems paradoxical after taking into consideration the discrepancy between the rate of first intubation failure (22% and 30%, respectively) and that of oesophageal intubation in VL and DL groups. However, the reasons for failed attempts on first intubation other than that of oesophageal intubation, which would be of clinical interest to the readers, were not documented.

Finally, the authors used the first intubation success rate as the main outcome in accordance with the result of a previous study⁵ that showed a positive association between failed initial intubation

attempts and a decreased likelihood of return of spontaneous circulation (ROSC) among patients with cardiac arrest. On the other hand, the study by Okamoto et al. did not address whether the rate of ROSC in their study¹ was consistent with the finding of that previous study.⁵ Because the clinical significance of successful first intubation is based on the premise that it is associated with an improved rate of ROSC, the significance of the study by Okamoto et al.¹ would be markedly blemished by the lack of this information

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Competing interests

The authors have no potential conflicts of interest to disclose.

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