

used to speculate on the indirect influence of cardiac output on mask ventilation.

We believe that addressing the above issues could further increase the value of this study.

### Competing Interests

The authors declare no competing interests.

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(Accepted for publication July 5, 2017.)

## “Loose Lips Are Actually Good”

To the Editor:

We compliment Sato *et al.*<sup>1</sup> on their recent article concerning mask ventilation during the induction of general anesthesia. A prominent feature of this study is the occurrence of *expiratory* flow limitation during positive pressure ventilation in 29% of subjects. This phenomenon is not widely appreciated as a cause of difficult mask ventilation. Sato *et al.*'s results confirm and extend observations that our group has previously reported.<sup>2</sup> Our audit strongly supports a “floppy” soft palate as the site of expiratory obstruction and demonstrates that opening the mouth during exhalation improves ventilation by allowing gas to exit the lungs.

Sato *et al.*'s article is lacking details of how the anesthesia providers managed mask ventilation other than to indicate that no oral or nasal airway was used. The provider was told to “perform his/her best airway opening technique during the anesthesia induction.” A leak from the mouth during exhalation may have produced the “partial expiratory flow limitation” waveform pattern denoted by the authors as V2

(fig. 2) based on low expiratory flows and a blunted carbon dioxide trace. The study used an anesthesia mask that covered both the nose and mouth, so it is not possible to identify the source of the limited expiratory flow.

This methodologic issue is important because it has the potential to obviate two of the authors' conclusions: first, that partial expiratory obstruction exists, and second, that switching from one- to two-handed mask ventilation is the key move in normalizing ventilation. We did not observe partial expiratory obstruction in our study; it appeared to be an all-or-none phenomenon. In addition, chin lift and head tilt, maneuvers commonly employed in two-hand ventilation, did not relieve the obstruction. What did work was to allow the mouth to open between positive pressure breaths. An oral airway would provide similar benefit. These issues remain open, however, because we used simple observation rather than quantitative methods to determine the presence or absence of obstruction. We join with Sato *et al.* in calling for more detailed studies of the soft palate and surrounding pharynx in patients with expiratory obstruction.

### Competing Interests

The authors declare no competing interests.

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(Accepted for publication July 5, 2017.)

In Reply:

We thank Drs. Zhou and Wang for their thoughtful comments on our study.<sup>1</sup> Certainly, many factors may influence the tidal volume during anesthesia induction. We previously reported that progressive muscle paralysis induced by rocuronium injection did not change the tidal volume during facemask ventilation without airway maneuvers in adult subjects with normal upper airway anatomy.<sup>2</sup> In contrast to Ikeda *et al.*'s study,<sup>2</sup> the tidal volume progressively improved in both non-sleep disordered breathing and sleep disordered breathing groups in Sato *et al.*'s study.<sup>1</sup> We believe there are three major differences between Ikeda *et al.*'s and Sato *et al.*'s study designs: anesthesia depth, initial airway patency, and airway maneuvers by the anesthetists. It is our opinion that anesthesia depth contributes little to time dependence of the tidal volume, given that pharyngeal collapsibility increases only slightly by increasing anesthesia depth with propofol, however, the pharyngeal