

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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## “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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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

collapsibility profoundly increases immediately after loss of consciousness.<sup>3</sup> The pharyngeal airway was initially open in Ikeda *et al.*'s study but was not controlled in Sato *et al.*'s study. We suspect that the pharyngeal airway initially closed, particularly in Sato *et al.*'s patients with sleep disordered breathing; subsequently, surface adhesive forces of the pharyngeal airway mucosa may have played a role in making reopening of the airway difficult.<sup>4</sup> We believe that minor adjustments made for each individual patient throughout the airway maneuver process by the anesthetists, as well as the gradual progression of muscle paralysis and anesthesia depth, will lead to achievement of a more successful and constant airway maneuver. Regrettably, we did not measure the change in rigidity of the neck and mandible, nor the process of proper head extension and mandible advancement during mask ventilation.

In addition to the upper airway mechanisms discussed above, Drs. Zhou and Wang also pointed out the possible contribution of mechanical changes in the lower airways to the results of this study. During progressive increase of muscle paralysis, chest wall compliance increase and laryngeal resistance decrease are expected. Without any doubt, the tidal volume measured in this study is an outcome of interaction between these lower airway and upper airway mechanisms. Drs. Zhou and Wang suggest use of peak inspiratory pressure during volume-controlled ventilation instead of tidal volume during pressure-controlled ventilation. We disagree with their suggestion because higher peak pressure induced during the volume-controlled ventilation causes gastric gas insufflation, particularly in obese subjects, which would consequently increase study risk for the participants.<sup>5</sup> It is our hope that future studies will clarify the mechanisms while controlling the concerns raised by Drs. Zhou and Wang and lead to better techniques of improving the mask ventilation process.

Drs. Wang and Wei raised the possibility of circulatory influence on the onset of muscle paralysis between sleep disordered breathing and non-sleep disordered breathing groups. The techniques used in rocuronium injection ideally should be identical between the groups; however, to our regret, perfect control was not obtained, while rocuronium was rapidly injected through a forearm intravenous line in all participants. Delayed onset time may have influenced the tidal volume due to difficulty in performing airway maneuvers under less paralyzed condition. It should be noted, however, that the train-of-four ratio did not differ between the subgroups in which significant tidal volume difference was confirmed. The idea of reduced cardiac output in patients with sleep disordered breathing as an explanation for the results of this study is interesting, but would not explain the reason for a smaller tidal volume in sleep disordered breathing patients even with two-hand mask ventilation under complete paralysis. The techniques of shortening rocuronium onset time may be worth using in clinical practice, however, for maximizing the positive impact of muscle relaxant on mask ventilation during anesthesia induction, particularly in patients with sleep disordered breathing, as recent airway guidelines strongly recommend.<sup>6,7</sup>

As commented by Drs. Buffington, Wells, and Soose, the results of this study strongly support expiratory flow limitation as an important mechanism for difficult or impossible mask ventilation during anesthesia induction. Although we agree with the possibility of "valve-like behavior of the soft palate" as a cause of expiratory flow limitation, we did not directly confirm it in this study. We have certainly experienced that triple airway maneuvers, including mouth opening with two hands and/or use of an oral airway, eliminate the expiratory flow limitation on the airflow tracing and improve mask ventilation. Furthermore, as they pointed out, the valve-like behavior of the soft palate may or may not be all-or-none and the "partial expiratory flow limitation" waveform pattern in figure 2 could be caused by expiratory airflow through the mouth. It is of paramount importance to explore the mechanisms of expiratory flow limitation. We completed a clinical study to address these issues and are preparing for publication (University Hospital Medical Information Network; UMIN000011821: Dynamic behavior of the soft palate during mask ventilation).

### Competing Interests

The authors declare no competing interests.

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