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# Dexmedetomidine Sedation and Airway Collapsibility: Comment

## To the Editor:

We have just read the article "Upper Airway Collapsibility during Dexmedetomidine and Propofol Sedation in Healthy Volunteers: A Nonblinded Randomized Crossover Study,"[1] and we really appreciate this nice work demonstrating the equal possibility of dexmedetomidine and propofol leading to upper airway obstruction or ventilatory depression. However, we wonder whether the patients in either group were in similar stable condition by the end of first airway assessments, as a run-in period or washout period was not mentioned in this crossover study. Liu *et al.* found that terminal half-life of dexmedetomidine could be as long as 4.4 h,[2] so during the infusion of two sedatives by Lodenius *et al.*, is it possible that the residual sedative effect of dexmedetomidine overlaps with sedation of propofol after overcross, or *vice versa*? As the total time lasted only 101 min, it would be very useful if the authors could supplement details of crossover for this study.

### Competing Interests

The authors declare no competing interests.

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# Dexmedetomidine Sedation and Airway Collapsibility: Reply

## In Reply:

We thank Drs. Zhu and Zhang[1] for their interest in our study comparing the effect of sedation with dexmedetomidine and propofol on upper airway collapsibility.[2] We are also thankful for the opportunity to clarify facts regarding washout time between airway assessments during sedation with the two drugs.

It is true that without an adequate washout period, a sedative effect of the first drug could affect the result when evaluating the second drug in a crossover study. However, in our study, the time between testing airway collapsibility with the two drugs was 7 days or more. A residual effect of the first drug at the second airway assessment therefore seems very unlikely.

### Competing Interests

The authors declare no competing interests.

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# Lung-protective Ventilation in Cardiac Surgery: Comment

## To the Editor:

We read with great interest the article "Intraoperative Mechanical Ventilation and Postoperative Pulmonary Complications after Cardiac Surgery" by Mathis *et al.*[1] We appreciate the authors' great work. The lung-protection ventilation bundle and its component of driving pressure have a strong correlation with the decrease of postoperative pulmonary complications, but several concerns remain.

First, the definition of postoperative pulmonary complications does reduce the comparability between studies. A recent consensus, cited also by this article, points out that considering the common pathologic pathway, perioperative pulmonary complications should include atelectasis, pneumonia, acute respiratory distress syndrome, and aspiration pneumonia,[2] these indicators are easy to achieve in clinical practice, especially in cardiac surgery with a higher monitoring level. Some of the indicators selected by the authors, including reintubation and prolonged initial postoperative ventilator duration longer than 24 h, might be partially attributed to the patient's circular instability and consciousness disorder, not just the pulmonary complications themselves. Moreover, these endpoints are somewhat like the consensus definition of respiratory failure under mechanical ventilation, a more serious condition requiring respiratory support[2]; it is conceivable that the actual incidence of postoperative pulmonary complications may be underestimated. Different definitions may lead to different results, the inconsistency of endpoint criteria might be solved by further sensitivity analysis.

Second, the cut-off point selection of the lung-protective ventilation bundle and its components is empirical and selective in this article, this may lead to a nonoptimal clinical choice. Moreover, nonsignificant statistical relationship of tidal volume less than 8 ml/kg (according to predicted body weight) and positive end-expiratory pressure (PEEP) greater than or equal to 5 cm $H_2O$ with occurrence of postoperative pulmonary complications may also be attributed to the hasty choice. It might be more appropriate to conduct an exploratory study to analyze the lung-protective ventilation components and the optimal combination in the first step; a previous study showing a PEEP of 5 cm $H_2O$ and median plateau pressure of 16 cm $H_2O$ or less was associated with the lowest risk of postoperative respiratory complications.[3]

Third, according to this article, the probability of postoperative pulmonary complications is higher at both poles of body mass index (BMI) classes (underweight and high-class obesity), and the distribution of pulmonary complications with BMI was unlikely to be linear, but rather binomial, distribution. This may be explained by the accompaniment of malnutrition with being underweight and with severe obesity being prone to atelectasis—both classes are associated with increasing postoperative pulmonary complications.[4,5] Additionally, BMI is associated with increasing intraabdominal pressure and decreasing pulmonary compliance.[5] For example, driving pressure is more difficult to maintain at 16 cm $H_2O$ in severe obesity compared to a normal BMI with the same tidal volume and PEEP. This may lead to a bias in the distribution of protective ventilation across different BMI ranges. Eventually, the interpretation of regression results might be affected by the aforementioned factors. Moreover, in a recent study, airway closure happens with an impressive incidence in patients with obesity, lead to an overestimation of driving transpulmonary pressure.[6] This complicates the interpretation of the findings in patients with obesity. However, in the subgroup analysis, the lung-protective ventilation bundle showed the same protective effect at all BMI levels, alleviating the aforementioned considerations to some extent.

## Competing Interests

The authors declare no competing interests.

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