In Reply:

I appreciate the constructive comments of Nguyen *et al.* with regard to the recent trial published by Ramgolam *et al.*[1] and the accompanying editorial in Anesthesiology.[2] I certainly agree with the observation that the work station used may have an impact on how long an inhalational induction would take, and this may have an impact on the likelihood of complications during induction; however, in this case it transpires that the researchers did not use a Draeger Primus (Draeger, Germany) for induction but, as is common in Australia, used a separate anesthesia system with a back bar that connected to a T-piece where wash-in times were minimal. This indeed should have been clarified in the paper. Apart from the work station, several other aspects of an inhalational induction may vary between practitioners, such as use of nitrous oxide, fresh gas flow, choice of circuit, and the degree of overpressure used. It is certainly plausible, but not definite, if or how these variations may have an impact on the risk of complications. Nevertheless, variations in practice that could plausibly impact research findings should always be considered when translating trial findings to practice.

Competing Interests

The author declares no competing interests.

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References

1. Ramgolam A, Hall GL, Zhang G, Hegarty M, von Ungern-Sternberg BS: Inhalational *versus* intravenous induction of anesthesia in children with a high risk of perioperative respiratory adverse events: A randomized controlled trial. Anesthesiology 2018; 128:1065–74
2. Davidson AJ: Induction of anesthesia for children: Should we recommend the needle or the mask? Anesthesiology 2018; 128:1051–2

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In Reply:

We thank our international colleagues for their interest in our study, "Inhalational *versus* Intravenous Induction of Anesthesia in Children with a High Risk of Perioperative Respiratory Adverse Events: A Randomized Controlled Trial,"[1] and have summarized our responses as follows.

Nguyen *et al.* raised the question of complications in the postinduction period and the effect of the route of the induction on complications. It is important to note that our study was neither designed nor powered to address respiratory adverse events within individual anesthesia phases, and data should be interpreted with caution. As demonstrated in table 1, the inhalational group did have the majority of respiratory adverse events during the induction of anesthesia, highlighted by Nguyen *et al.* However, there were no significant differences in the incidence of respiratory adverse events during the other phases of anesthesia. Complications increased across the whole perioperative period (primary outcome measure), as reported in our study.[1] While we cannot specifically comment on the impact on the method of anesthesia induction on complications within each anesthetic phase, we believe the significant reduction of complications within the induction phase and across the perioperative period warrants individual practitioners to give consideration to their clinical practice.

We do agree with Davidson[2] and Nguyen *et al.* that the IV inductions tend to be much faster as compared to inhalational inductions, and that the duration of the induction phase may result in higher complications during that phase and therefore be a mediating factor. In this study, the induction of anesthesia was not performed with the Draeger Primus workstation, which was used throughout the surgery. In our institution, the induction of anesthesia is performed in a separate anesthesia bay using a back bar, which, at the time of the study, had ULCO Engineering - AC30 Systems (ULCO Medical, Australia) connected to a T-piece. While we have not specifically recorded inhalational wash-in times, we would observe that these are minimal using a T-piece and do not feel that potential differences in induction times between IV or inhalation induction will have significantly influenced the incidence of respiratory adverse events in our study.

Daoud raised the issue of the tension between increasing external application of the outcomes and the internal validity of the study. We note that this study was designed in a pragmatic way to improve external validity, and therefore some specific aspects of anesthetic care were not rigidly controlled. One potential impact on complications noted by Daoud was the application of nitrous oxide in the inhalational group and subsequent decreases in oxygenation to less than 95%, which was one of the perioperative complications recorded in this study.[1] In this study, patients undergoing inhalational induction of anesthesia received nitrous oxide at a median ratio (range) of 0.5 (0.5 to 0.66), which is not a large range, particularly given the fact that mask seal is not always perfect during induction in young children. We agree that this means that the inhalational group received a higher F_{IO_2} compared with the IV group, which could possibly have led to an underestimation of the difference between the two techniques, given that preoxygenation was not routine in the IV group, in line with routine practice in many institutions. Our study was not designed nor powered to detect significant differences in individual complications. However, we would note that desaturation was not statistically