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In Reply:—We thank Dr. Eikermann *et al.* for their interest in our article[1] and their suggestions for conducting randomized controlled studies to determine the optimal time after an upper respiratory infection (URI) for providing anesthesia and to characterize the optimal technique for airway management.

First, we would like to emphasize that the work that claims the use of a laryngeal mask airway (LMA) as an alternative to tracheal intubation in children with recent URI was completed in a randomized controlled study that included 41 children in each group.[2] Although the incidence of laryngospasm was 10% with the LMA (twice higher than with an endotracheal tube), this did not reach statistical significance because of the low number of children involved. To our knowledge, our study is the first to report a large number of children with a recent URI and to provide some evidence that despite the use of an LMA, the incidence of respiratory complications remains high. However, we agree with the authors that multiple attempts to insert the LMA are often associated in clinical practice with difficult anatomical conditions or light or inadequate anesthesia and are independent of the presence of an URI. However, if laryngospasm were an epiphenomenon, one would observe the higher incidence of complications at insertion, which was not the case in our study because the higher odds ratio for laryngospasm was observed intraoperatively and even in the postoperative care unit. Furthermore, multiple attempts to insert the LMA were only found in the univariate analysis for all respiratory complications, whereas URI was almost the only factor that remained in the multivariate analysis, which further confirms that a recent URI is a risk factor for the occurrence of perioperative respiratory complications with the use of an LMA.[1]

The second point raised by the authors on the time delay after a recent URI before proceeding with anesthesia is of interest. Although we agree that we cannot provide strong evidence that anesthesiologists should consider at least a 2-week interval, we still believe that the absence of evidence does not translate into the evidence of absence. Please note that the underlying pathophysiologic mechanisms involved in the occurrence of respiratory complications after insertion of an LMA are completely different than those observed in the case of an endotracheal tube. The nonadrenergic, noncholinergic autonomic nervous system is primarily involved with the LMA stimulating the sensory nerves (C fibers), whereas the cholinergic system is the main pathway that is activated by the insertion of an endotracheal tube. This explains in part why the incidence of bronchospasm is negligible in the presence of an LMA. Furthermore, we disagree with the authors that there was no "control group" *per se* because fever can be present independently of a URI and cough is the second most common symptom in childhood (10–20% of preschool children), not necessarily associated with a recent URI.[3] We based our definition of URI on the parents' statements because parental confirmation of the presence of a cold has already been identified as a predicting factor for the occurrence of adverse events during anesthesia.[4] Therefore, we believe that our categorization into two groups precludes the possibility of mixing URIs of different severities.

We finally agree with the authors that URI dilemma remains an issue. Please note that there is no evidence in the literature that waiting several weeks after an URI will decrease the incidence of respiratory complications. Although we agree that a randomized controlled study may definitely add some evidence to our statement, we would like to point out that prospective observational studies produce invaluable information about perioperative morbidity in pediatric anesthesia. We are aware that such studies include many confounding factors that were, however, closely examined in our study by integrating different models in the multivariate analysis. Therefore, we believe that our results are of great importance in designing for the future randomized control studies to confirm our findings without integrating these confounding factors.

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Ultrasound-guided Catheterization of the Internal Jugular Vein

To the Editor:—We read the article by Hosokawa *et al.*[1] with interest. The authors must be commended on doing this study in small children with a group of resident/fellow trainees. However, we have a few concerns.

Most people would agree with us that a randomized study associated with risks will automatically require informed consent. Although the authors argue that informed consent was not required because of the wide use of ultrasound in their practice, it is likely that if the trainee was unsuccessful after a few needle passes with the skin-marking technique (in real practice outside of the study), they would probably have resorted to using real-time guidance. Also, we are intrigued to note that when one trainee is unsuccessful after three attempts (we assume that an attempt is a single needle pass; this needs to be defined in the Materials and Methods section), he or she is replaced by another trainee (is the other trainee going to follow the same puncture marks on the line or going to pick another mark, and if so, who guides them?). The authors do not report the range (or the mean) of the number of attempts with both techniques, and we would like to know where they produced the trainees from (especially in case of multiple attempts). We also question whether the practice of replacing one trainee with another was their standard of care or whether it was only for this study. Trainee failure and dismissal is followed by the attending in all training programs with which we are familiar. We believe that with their study design, informed consent is mandatory.

The authors bring the "old dog and new trick" concept. By their own admission, the ultrasound manipulations were done by the two experienced attending physicians and not by the trainees. How does this concept work in their case? Besides, the authors need to state clearly

(in their Discussion) that the real-time ultrasound-guided technique (in their study) was a two-person approach. The study by Grebenik *et al.*[2] involved just one person. This is an important observation because it is easier to control needle manipulations with both hands without having to manipulate the ultrasound. We would also like to know in what way the attending anesthesiologist intervened if the trainee was advancing the needle toward the internal carotid artery (or in any other wrong direction) with the real-time ultrasound-guidance technique. Were they also giving directions to the trainee?

The fundamental principle of visualizing the needle passing through tissue and penetrating the vessel being better than surrogate, blind landmarks (skin markings, muscles, or pulsations) is undeniable. Making the technology–operator interface work satisfactorily is what is needed.

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In Reply:—We owe Drs. Ganesh and Jobes a great debt of gratitude regarding our study.[1] Their comments surely reinforced our point of view about the usefulness of real-time ultrasound-guided central venous catheterization. First, we agree with their suggestion about the need for informed consent. Although our institutional ethics committee did not request that we obtain formal written consent, we explained the details of our study to the parents or guardians of each patient design and obtained verbal informed consent. Because the application of the ultrasound technique *per se* had already become our standard measure for central venous catheterization in pediatric cardiac surgery, the informed consent was only focused on the possible application of the real-time technique. All procedures were directed and supervised by the two experienced attending anesthesiologists (N.S. or Y.K.) who also manipulated the ultrasonography probe during the real-time procedure. An attempt was defined as a single needle pass, and if one trainee failed more than three attempts (which did not happen in the real-time group), the supervising attending decided the next procedure. In our study, 3 of 27 cases in the skin-marking group were regarded as unsuccessful (2 complications and 1 with more than 20 min). These cases were replaced by real-time guidance that was performed successfully from another puncture point by another fellow (appointed by the supervising attending who also handled the probe) who is more familiar with the procedure. Four other cases in the skin-marking group were performed with a total of four to six attempts. In these cases, all replaced second fellows completed the procedure within 20 min; therefore, these were counted as successful. Another 20 cases in the skin-marking group and all cases in the real-time group were successfully punctured and cannulated with no more than three attempts by the first trainee. During the procedure, the two experienced attending physicians were ready anytime to take over a role if the second trainee was unsuccessful, but there was no chance for them to show off. All selected 10 trainees who conducted this study were more or less accustomed to the use of ultrasonography and well experienced with the real-time procedure with adult cardiac patients (after at least 6 months training) and were closely guided by two attending physicians during the study. As suggested, it is true that trainee failure and dismissal is usually followed by the attending physicians in the training program, even at our institute, but if the attending notes that it is relatively safe, the second trainee with more skill could be appointed with care. Also, we now believe that the success of this procedure is largely dependent on who manipulated the ultrasonography probe. Because we dealt with the small children, whose veins were small and collapsible, meticulous handling of the probe was the key to successful puncture. Therefore, if the inexperienced trainee handled the probe, our success rate might not have achieved 100%. We are sorry that we forgot to mention that the device we applied in this study was not so cumbersome compared with the one that was used in the study of Grebenik *et al.*[2] and that we did not use a needle guide or needle guide bracket.

We also agree with the authors' suggestion that the two-person approach enabled the trainee to use both of his or her hands and concentrate on the puncture site and the aspiration of blood. Therefore, our two-person approach with trainee (as operator) and attending (as supervisor and manipulator of the probe) might be the key for the success in this study. And this should be one of the best examples for hands-on training.

Because Grebenik *et al.*[2] previously demonstrated poor outcomes associated with the real-time method compared with the anatomical landmark method, we thought to conduct this trial comparing the two ultrasound-guided techniques. In this sense, as suggested, the reference of "old dog and new trick" might have been inappropriate because the ultrasound was manipulated by the well-trained "old dog."

We do believe that ultrasound-guided central venous puncture should be an absolute prerequisite for every anesthesiologist. And we would like to stress that seeking appropriate methodology with new technology that especially focuses on operator friendliness and patient safety is crucial in critical care settings.

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