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*In Reply:*

We thank Adachi *et al.* for their interest in our article[1] showing that a rapid infusion of hydroxyethyl starch (HES) but not acetate Ringer's solution decreased plasma propofol concentration during target-controlled infusion. They have focused on the influence of rapid fluid infusion on propofol pharmacodynamics. As it is unlikely that a rapid fluid infusion of HES itself changes the anesthetic potency of propofol in the brain, we discuss the influence of HES on the pharmacokinetics of propofol.

There is a good study by Takizawa *et al.*[2] for this issue. Briefly, this study clarified that a 30 ml/kg but not a 10 ml/kg isovolemic hemorrhage followed by crystalloid resuscitation significantly increased the unbound fraction of propofol in blood and also showed that a 10 ml/kg isovolemic hemorrhage did not decrease the bispectral index (BIS) value. Their results suggest that an 8 ml/kg rapid fluid administration without hemorrhage in our study[1] is unlikely to have increased the ratio of unbound propofol in blood. As a rapid infusion of 8 ml/kg HES decreased total plasma concentration of propofol in our study,[1] there is a possibility of anesthetic awareness under propofol anesthesia during a rapid infusion of HES.

A previous study suggested that a chemical interaction between propofol and HES might influence the pharmacokinetic behavior of propofol *in vivo* although this interaction was confirmed *in vitro*.[3] This interaction might decrease unbound propofol in blood, which might increase anesthetic awareness during rapid infusion of HES under propofol anesthesia.

A single bolus of rocuronium without an additional dose was administered to all patients so that the levels of neuromuscular block were different among the patients. As neuromuscular block can influence the BIS value,[4] we did not evaluate the BIS values in our study although we monitored BIS values to avoid anesthetic awareness. Therefore, we would like to note that we recommend to consider increasing the targeted concentration of propofol during a rapid infusion of HES and that the influence of a rapid HES infusion should be examined in a further study as described in the conclusion of our article.[1]

## Competing Interests

The authors declare no competing interests.

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# Front of Neck Airway: The Importance of the Correct (Obese) Models and (Trained) Participants in Study Design

*To the Editor:*

We read with great interest Professor Asai's editorial on the management of "can't intubate, can't oxygenate" situations[1] and Heymans *et al.*'s[2] study of cricothyrotomy in cadavers. We agree with Professor Asai that this topic is extremely difficult to study and that it is difficult to recommend a definitive best technique for front of neck airway. The importance of the topic is exemplified by a recent statement and editorial by several anesthetic and surgical bodies in the United Kingdom supporting cricothyrotomy (not tracheostomy) as the first option in "can't intubate, can't oxygenate"[3] and a response to this by the Australian and New Zealand College of Anaesthetists (Melbourne, Australia).[4]

Our first concern is that Professor Asai places weight on the finding of Heymans *et al.*'s[2] study in which medical students performed cricothyrotomy in cadavers. Without disrespecting medical students, they are not equipped with the knowledge, attitudes, or skills of those likely to be performing front of neck airway. As such it is not possible to disentangle whether the study tells us most about the model, the techniques, or the operators. Such studies are most valuable when performed on appropriate models by clinicians likely to be involved in such emergencies.

Our second concern is that Professor Asai emphasizes that cannula cricothyroidotomy was less likely to be successful than a surgical approach in the United Kingdom's Fourth National Audit Project.[5] This study was a joint project organized by the Royal College of Anaesthetists (London, United Kingdom) and the Difficult Airway Society (London, United Kingdom) and it looked at all complications of airway management in the United Kingdom in a 1-yr period. The reasons for this were multifactorial. Importantly, needle-based approaches were mostly performed by anesthetists in "end-of-algorithm" situations in which they had to abandon the upper airway and attempt the procedure in a peri-arrest situation—a situation familiar to the American literature

too.[6] Conversely, surgical approaches generally involved trained otolaryngologic surgeons performing their procedure while the anesthetist continued attempts to maintain oxygenation from above. The fact that many of these cases took up to 30 min, and in some more than 60 min, illustrates how different they were from those managed with a cannula. Importantly, the Fourth National Audit Project tells us little about how anesthetists manage performing surgical or scalpel front of neck airway techniques.

Finally, we wholeheartedly agree with Professor Asai's statement that front of neck airway is technically more difficult in obese patients, where "the tissues overlying the larynx are thick and it is difficult to locate the cricothyroid ligament."[1,7] We note that the very patients who tend to need an emergency front of neck airway are often patients with a high body mass index[5] and are concerned that this is often forgotten in manikin and model design. We believe that more emphasis should be placed on investigating which techniques work best in obese patients. In a study we performed evaluating an "obese neck manikin," all trained anesthetists reported increased difficulty than with a standard neck manikin and 40% required a change from their planned technique to successfully establish a front of neck airway.[8] Hybrid techniques such as that proposed by Heard *et al.*[9] or described in recent U.K. guidelines[10] are likely to be more appropriate. Training is usually carried out using manikins that mimic a patient with a slim neck: we advocate training for both the slim (*e.g.*, cachectic, postradiotherapy) neck and for necks with several centimeters of subcutaneous fat. Liaison with industry is needed to improve realistic manikins to assist with the latter.

## Competing Interests

The authors declare no competing interests.

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*In Reply:*

I thank Dr. Kelly and Professor Cook for their comments to my editorial[1] on emergency cricothyrotomy. I stated in my editorial[1] that "because it is difficult to carry out randomized controlled studies in patients, we still do not know which method is the most reliable." In the absence of randomized controlled studies, we need to decide the most effective method of emergency cricothyrotomy, based on nonrandomized clinical studies or simulation studies. As Dr. Kelly and Professor Cook correctly point out, nonrandomized clinical studies or simulation studies have limitations, and thus we should carefully assess the evidence level of each study.

In my editorial,[1] I described that "there is growing evidence that percutaneous cricothyrotomy using a narrow-bore cannula—once advocated for use for its simplicity—may frequently be ineffective." This statement was drawn not only from the study by Heymans *et al.*[2] but also from several other studies. For example, a recent systematic review indicated that transtracheal jet ventilation *via* a narrow-bore cannula may frequently fail and may be associated with life-threatening complications.[3] I referred to the report of the Fourth National Audit Project[4]—although this is not a randomized study and thus the reasons for lower success rates of cannula cricothyrotomy may be multifactorial—I believe that the report provides a high evidence level. In fact, Professor Cook himself states in his previous article that "the Fourth National Audit Project … concluded that needle or cannula cricothyroidotomy performed by anaesthetists had a particularly low success rate."[5]

I described in my editorial[1] that "the problem that we are facing now is that we do not know which model is effective for simulation training for emergency cricothyrotomy." Nevertheless, a cadaver (in particular, a cadaver with lifelike conditions [Thiel embalming technique])[6] and a manikin or animal model of an obese neck or neck with burn[7] is likely to be a more suitable than a conventional manikin model, and thus studies using these models would be regarded as providing higher evidence. In the study by Heymans *et al.*,[2] participants were students, who may not be good representatives of experienced clinicians, but the simulation model (cadavers with Thiel embalming technique) can be regarded as the best