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(Accepted for publication March 7, 2019.)

Intubation in Operating Room *versus* Intensive Care: Reply

In Reply:

We thank Dr. Wang, Dr. Xu, and Dr. Deng for their useful comments about our article entitled “Comparison of Tracheal Intubation Conditions in Operating Room and Intensive Care Unit: A Prospective, Observational Study.”¹ It is true: we had a mistake in table 2. The complication of hypoxia less than 80% was 29 patients (14%) in the intensive care unit (ICU), but table 2 mistakenly shows 19 patients (14%).

In our study, nearly 90% of patients in the ICU were intubated at first attempt, and the neuromuscular blocking drug used was succinylcholine. We did not record the duration of the apnea, but it probably was less than 1 min in patients intubated at first attempt. We agree with Dr. Wang *et al.* that it is important to minimize this apnea time during intubation for critically ill patients. In the “Guidelines for the management of tracheal intubation in critically ill adults,” published

in November 2017, Higgs *et al.*² recommend nasal oxygen at 15 l/min or high-flow nasal oxygenation during intubation attempts. In October 2017, Oliveira *et al.*³ published an interesting review and meta-analysis to evaluate the effectiveness of apneic oxygenation during emergency intubation. They observed that apneic oxygenation was associated with increased periintubation oxygen saturation and decreased rates of hypoxemia. We finished our study in November 2017 when the guidelines for the management of tracheal intubation in critically adults were published.² From that moment on, we used apneic oxygenation to try to achieve decreased rates of hypoxemia. As of now, in our intensive care unit we are routinely using apneic oxygenation during tracheal intubation.

Wang *et al.* propose an interesting method to decrease rates of periintubation hypoxemia. The method consists of an anesthesia mask that can ventilate during tracheal intubation with a fiberoptic bronchoscope.⁴ They described this technique in their letter to the editor; however, the authors did not report whether this technique was used in surgery anesthetized patients, hypoxic patients, critically ill patients, obesity patients, or awake patients. Similar endoscopic masks like the Patil mask have been described in the literature to overcome the problem of ventilation during fiberoptic intubation. For example, Aoyama *et al.*⁵ compared the efficacy of delivery of mechanical ventilation through different airway devices during fiberoptic intubation. One of these devices was an endoscopy mask like the one presented by Wang *et al.* However, Aoyama *et al.*, unlike Wang *et al.*, described equipment and preparation, anesthetic management, placement of the airway devices, and fiberoptic intubation procedure. One important limitation of their study is that patients with a difficult airway, lung disease, or morbid obesity were not included. Thus, their results may not apply to those groups of patients or to critically ill patients.

We think that the use of a fiberoptic intubation with a mask ventilation for critically ill patients has several limitations. The first limitation is that this mask is useful only when a patient can be easily ventilated. The second limitation observed with these types of masks is gastric insufflation,^{5–7} especially when a long time is required for intubation. Many ICU patients have a full stomach with a risk of aspiration, and gastric insufflation may be dangerous. The third limitation is that a fiberoptic intubation can be difficult in a lot of ICU patients because of the presence of hemorrhage, laryngeal edema, and increased secretion in the airway.

In conclusion, we agree with Wang *et al.* that hypoxia is an important problem during intubation in the ICU. Probably the use of mask ventilation during fiberoptic intubation with their limitations may be useful in selected cases in the ICU, but we do not recommend routine use. We think this method should be further evaluated in the ICU setting and in patients with a difficult airway, lung disease, or morbid obesity.

Competing Interests

The authors declare no competing interests.

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(Accepted for publication March 7, 2019.)

Maintenance of Certification: Comment

To the Editor:

We read with great interest the editorial “Maintenance of Certification: Has MOC Gone Amok?” by Nelson

and Butterworth, which was published in the October 2018 issue of *ANESTHESIOLOGY*, commenting on our recent publication examining the relationship between participation and performance in the Maintenance of Certification in Anesthesiology program and medical license actions against anesthesiologists.¹ As evidenced by this and several other publications,^{2–6} the American Board of Anesthesiology (Raleigh, North Carolina) is committed to a rigorous and transparent evaluation of Maintenance of Certification in Anesthesiology.

The carefully crafted title asks a rhetorical question using the hyperbolic word “amok,” effectively calling into question the purpose and vision of Maintenance of Certification in Anesthesiology. We take seriously and welcome this conversation within the anesthesiology community. Although a full discussion on the purpose and vision of Maintenance of Certification in Anesthesiology is beyond the scope of a letter to the editor, we will address this issue succinctly at the conclusion of this letter.

We first comment on two general areas addressed by the editorial. The authors view the publication as “research to determine whether Maintenance of Certification in Anesthesiology 2.0 is fulfilling its promise.” However, our research included only diplomates certified between 1994 and 2005 and followed them through their first 10 yr of certification, which was between 2004 and 2015, depending on the year in which the diplomates were initially certified. Maintenance of Certification in Anesthesiology 2.0 did not launch until 2016, so the results and conclusions of our study reflect diplomates’ participation (or nonparticipation) in the original Maintenance of Certification in Anesthesiology program not the redesigned program (Maintenance of Certification in Anesthesiology 2.0). Thus, the premise of the editorial is not valid; the publication does not at all evaluate Maintenance of Certification in Anesthesiology 2.0, but rather the original Maintenance of Certification in Anesthesiology program (“Maintenance of Certification in Anesthesiology 1.0”).

The authors also argue that mandating any type of learning experience violates tenets of adult learning theory, and that certifying boards should consider reverting to a recertification approach in which only a periodic high-stakes examination is administered. Their argument implies that Maintenance of Certification in Anesthesiology or its equivalent should serve solely as a credential, rather than a learning opportunity. Our findings provide support for the utility of the original Maintenance of Certification in Anesthesiology program (“Maintenance of Certification in Anesthesiology 1.0”) as a credential, as those who completed their requirements in a timely manner were at lower risk for license actions.² However, adult learning theory supports the concept that, if properly designed, Maintenance of Certification in Anesthesiology can also serve as a learning experience. As noted by the authors, adult learners want choices and “possess a deep need to be treated as capable of self-direction.” This concept underlies the requirement by state medical boards (and Maintenance of Certification in Anesthesiology) for continuing medical