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Delayed Sequence Intubation: Danger in Delaying Definitive Airway?

To the Editor:

Weingart et al[1] have coined the new term "delayed sequence intubation" in accordance with their study of 62 patients treated with ketamine and either noninvasive positive-pressure ventilation or nonrebreather preoxygenation before delayed intubation. They note that delayed sequence intubation is essentially procedural sedation for airway intubation and that delayed sequence intubation seems safe and effective.

We thank the authors for describing their delayed sequence intubation technique for preventing hypoxic episodes in the airway management of critically ill patients. Nevertheless, we caution the reader who considers adoption of this practice that they treat as "usual care." As a convenience sample with no comparators, the study sample is subject to significant selection bias and remarks on safety and efficacy are precarious. Although the authors describe good results in this cohort, there is no evidence that these patients would not have done equally well with routine rapid sequence intubation. More than 60% of the patients who received delayed sequence intubation received noninvasive positive-pressure ventilation beforehand and then became intolerant, begging the question of whether initial rapid sequence intubation would have been most appropriate.

The danger of a lead *Annals* article published by a well-known educator is that "delayed sequence intubation" could be adopted in difficult situations by less capable hands. As such, this article deflects attention from the primary purpose of rapid sequence intubation, which is to decisively control the airway with well-defined pharmacologic (including ketamine) and bag-valve-mask ventilation techniques followed by decisive intubation.[2] Preoxygenation with a tight-fitting mask, such as found with a bag-valve-mask apparatus or an anesthesia circuit, is reliable.[3,4] The implementation of delayed sequence intubation adds the associated risk of delays in definitive intubation compared with using a simple bag-valve-mask setup.

We acknowledge that a true prospective trial of procedural sedation for optimizing intubation would be difficult. In lieu of such a trial, we welcome greater input from the emergency medicine community on whether there is a perceived need for delayed sequence intubation and whether this technique has been safe in other settings.

We also note a discrepancy concerning the total number of intubated patients in the study by Weingart et al,[1] which is 60 in Figure 2 but 62 in the text. Specifically, Figure 2 depicts 64 patients intubated, with 2 exclusions, for a total of 62 study patients, of whom 60 were intubated. The accompanying text on page 352, however, reads "[F]ifty-five patients were intubated in an ED setting; 7, in an ICU." This equals 62 intubated patients.

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1. Weingart S, Trueger S, Wong N, et al. Delayed sequencing intubation: a prospective observational study. *Ann Emerg Med*. 2015;65:349-355.
2. Godwin S, Burton J, Gerardo C, et al. Clinical policy: procedural sedation and analgesia in the emergency department. *Ann Emerg Med*. 2014;63:247-258.
3. Benumof J. Best method for both efficacy and efficiency? *Anesthesiology*. 1999;91:603-605.
4. Baraka A, Taha S, Aouad M, et al. Comparison of maximal breathing and tidal volume breathing techniques. *Anesthesiology*. 1999;91:612-616.

In reply:

We thank Skupski et al for their comments and for the opportunity to emphasize some of the aspects of our article on delayed sequence intubation.[1]

The letter's authors state that there is no evidence that these patients would not have done equally well if instead managed by *routine* rapid sequence intubation. But of course these patients would not have been eligible for routine rapid sequence intubation. The core of such intubation is the provision of preoxygenation and denitrogenation to allow a safe apneic period. The patients' delirium prevented the provision of these 2 actions. Therefore, if rapid sequence intubation had been undertaken, the patients would have lacked a buffer of oxygen-filled alveoli, which would have led to more rapid desaturation. Many of the patients were already hypoxemic. Proceeding with standard rapid sequence intubation in this latter group has been associated with critical desaturation and severe adverse events.[2] Although not level 1 evidence, the demonstrable allowance of denitrogenation and the increase in oxygen saturation in the patients with delayed sequence intubation is interpreted as beneficial to any clinician skilled at emergency airway provision.

Reading further in the letter by Skupski et al leads to the interpretation that they are not putting forth routine rapid sequence intubation as the alternative to delayed sequence intubation, but instead are proposing modified rapid sequence intubation. In this technique, the patient actually receives ventilation during the apneic period.[3] Unfortunately, we believe they are misunderstanding the tenets of this technique.

Providing positive-pressure breaths to an apneic, nonfasted patient is a double-edged sword. Although small breaths may maintain the functional residual capacity and augment safe apnea, even perfect breaths may cause gastric insufflation, and imperfect breaths guarantee it.[4] However, as the articles by Benumof[5] and Baraka et al[6]—as well as many others—demonstrate, obtaining full denitrogenation requires at least 8 vital capacity breaths. Providing a nonfasted, critically ill patient with multiple vital capacity breaths (rather than the modified rapid sequence intubation's described small tidal-volume breaths) would be considered by any airway expert to be dangerous. In fact, the avoidance of having to provide these breaths is the reason we formulated delayed sequence intubation. Furthermore, intensivists understand that vital capacity breaths provided by positive pressure are not analogous to those generated by a spontaneously breathing patient.

The letter authors state that delayed sequence intubation adds delays to definitive intubation, but of course the only delay is the denitrogenation time itself, which is an essential part of any rapid sequence intubation and which would be avoided only at the patient's peril.

Skupski et al wrote that 60% of the patients received noninvasive positive-pressure ventilation beforehand and then became intolerant. A careful reading would reveal that this was not the case—these patients' entry into the study was because they would not tolerate any provision of noninvasive positive-pressure ventilation.

Furthermore, they state that a true prospective trial of the technique would be difficult. Of course, our study was a true prospective trial. In fact, each patient in this prospective trial acted as his or her own control. We think that Skupski et al may have meant a randomized, prospective trial would be difficult; on this point we agree.

As for the discrepancy in the wording raised by the letter authors, we appreciate their alert. The word *intubated* should have read *managed*.