may not be as problem-free as described by the authors.

e) The use of bag-valve-mask techniques to provide continuous positive airway pressure/spontaneous positive end expiratory pressure is widely practiced by anesthetists in the United Kingdom.

Further to these concerns, we do not entirely understand the statement regarding minute ventilation in the reoxygenation section. The authors state that to provide high levels of oxygenation, all that is required at an FiO_2 of 0.5 is a minute volume of 500 mL/min. This would provide the minimum quoted oxygen requirement for basal metabolic function at rest, but will not provide "high levels of oxygenation," and oxygen requirement in a critically ill patient is likely to exceed this figure.

The article suggests that using nasopharyngeal airways or oropharyngeal airways (NPAs/OPAs) are advanced techniques best performed by experts and may be dangerous in the hands of novices. In our opinion, the performance of a rapid sequence induction and intubation, especially in a critically unwell patient, is an advanced technique and should be performed only by experienced personnel. The recent National Audit Project 4 performed by the Royal College of Anesthetists highlighted how dangerous intubation in the Emergency Department can be (2). NPAs/OPAs are simple airway adjuncts that should be familiar to most health care providers working in the acute setting.

We do, however agree entirely with the comments made regarding the importance of maintenance of a patent airway post induction to allow for apneic oxygenation. The importance of reoxygenation in the face of dropping saturations, rather than persisting with attempts at intubation, also cannot be overstated.

In summary, in rapid sequence induction, the time from induction to establishing a definitive airway should be as short as possible due to a potential risk to the airway or likely precipitous fall in oxygenation. Preoxygenation is an integral part of this by providing a supply of oxygen that will maintain oxygenation during apnea. To delay the definitive treatment of patients in respiratory distress does not strike us as best practice.

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REFERENCES

1. Weingart SD. Preoxygenation, reoxygenation, and delayed sequence intubation in the Emergency Department. J Emerg Med 2011;40: 661–7.
2. Cook TM, Woodall N, Harper J, Benger J, Fourth National Audit Project. Major complications of airway management in the UK: results of the Fourth National Audit Project of the Royal College of Anaesthetists and the Difficult Airway Society. Part 2: intensive care and emergency departments. Br J Anaesth 2011;106:632–42.

☐ RE: PREOXYGENATION, REOXYGENATION, AND DELAYED SEQUENCE INTUBATION IN THE EMERGENCY DEPARTMENT

☐ To the Editor:

I thank Drs. Gill and Edmondson for an opportunity to expand on some of the points in my manuscript (1). That previous sentence was the first in the response I began to draft when I received the letter to the editor printed above. I then realized that this form of communication was outdated; I would write a response, perhaps addressing the questions raised or perhaps missing their intent entirely. No actual communication or fostering of more refined ideas would be achieved. Instead, I searched for contact information for Dr. Gill and e-mailed him to ask if he'd like to have an actual conversation about the things he had written. Further, I got his permission to record the conversation and make it freely available. So although I will respond briefly to each of the points of the original letter below, I believe more understanding may be gained by listening to the podcast of the conversation available here: [http://emcrit.org/preox-response/].

The first point we discussed was the pulse oximetry reading as a measure of preoxygenation. Although maximal denitrogenation is indeed a goal during preoxygenation, in a critically ill patient it is necessary, but insufficient. As mentioned in the original paper, denitrogenation can be achieved with eight vital capacity breaths or 3 min of tidal volume breathing while on a high FiO_2 source. But if the patient's oxygen saturation is still low after being placed on high FiO_2, the patient is exhibiting physiological shunt. In fact, the work by Davis et al. demonstrated that the pre-rapid sequence intubation (RSI) oxygen saturation is the best predictor of the rapidity of decline during the intubation with a curve that nearly mirrors the oxyhemoglobin dissociation curve (2). This is physiologically intuitive, because as the arterial saturation diminishes, progressively lower-saturated venous blood will be sent through the shunt areas of the lung. The larger the shunt fraction, the worse will be this rapid cyclical decline of arterial oxygen saturation.

The solution to this situation is to minimize the shunt fraction. Clinically, this is indicated by achieving an oxygen saturation as close to 100% as possible before proceeding to intubation. Proof of this physiologic concept is demonstrated by seven studies comparing maximal attempts at standard preoxygenation with preoxygenation utilizing non-invasive positive pressure ventilation or positive end expiratory pressure (PEEP); these studies are reviewed in a separate manuscript (3). In each of these studies, the strategy of shunt elimination resulted in a prolongation of the time until significant desaturation during the apneic period.

We then went on to discuss the situation of a patient who was resistant to this application of PEEP, due to delirium from hypoxemia or hypercapnea. This patient population was the entire focus of the delayed sequence intubation (DSI) portion of the manuscript. Dr. Gill raised concern that the dose of ketamine (1–1.5 mg/kg) may push the patient from a state of disassociation to a plane of general anesthesia. I believe this may be a misunderstanding of the pharmacologic effects of ketamine. Doses far in excess of the one recommended in the article will still not place a patient in the apneic plane of general anesthesia; even in overdose, adult patients will retain respiratory drive and airway reflexes (4). One key point is that when performing DSI, the practitioner should not walk away to see other patients while the patient is receiving the ketamine-facilitated preoxygenation. Instead, all medications, equipment, and staff for RSI should be prepped at the bedside before performing DSI. If a complication such as prolonged apnea did occur (which would be case reportable), the team should proceed to RSI and the situation would be the same as if RSI was performed as the initial strategy.

Dr. Gill raises the question of whether the minimum minute volume ventilation of 500 mL/min at 0.5 FiO_2 stated in the manuscript is relevant to a critically ill patient. In the circumstance we are describing, the critically ill patient would be paralyzed and sedated in the course of the RSI procedure, thereby temporarily ameliorating the upregulated oxygen utilization of critical illness. Further, whereas the above parameters were stated as the minimum necessary, the manuscript recommended giving 10 breaths at close to 1.0 FiO_2, to achieve a greater than 10-fold margin of safety.

Before speaking with Dr. Gill, I did not understand the portion of the letter relating to the use of nasopharyngeal or oropharyngeal airways being best performed by experts. My intent in the manuscript was to state that bag-valve-mask ventilation is best performed by experts. Even in the hands of trained personnel, when in the midst of the management of a sick patient, respiratory rate and speed of ventilation can dramatically exceed desired levels (5,6). The manuscript suggested that a better way may be to outsource this work to a machine capable of consistent respiratory parameters regardless of the stress of the situation, such as a ventilator or hand-held automatic respirator.

After the conversation with Dr. Gill, I believe we came to agreement on most of the points discussed. I can say, definitively, that it was more productive than my simply responding to their letter in this forum. I encourage journal editors to forward contact information when sending these letters to manuscript authors for response and, further, to encourage interaction before the responses are written. Fostering this communication may bring us into a new generation of interactivity beyond the pages of bound journals.

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REFERENCES

1. Weingart SD. Preoxygenation, reoxygenation, and delayed sequence intubation in the emergency department. J Emerg Med 2011;40:661–7.
2. Davis DP, Hwang JQ, Dunford JV. Rate of decline in oxygen saturation at various pulse oximetry values with prehospital rapid sequence intubation. Prehosp Emerg Care 2008;12:46–51.
3. Weingart SD, Levitan RM. Preoxygenation and prevention of desaturation during emergency airway management. Ann Emerg Med 2012;59:165–1751.
4. Strayer RJ, Nelson LS. Adverse events associated with ketamine for procedural sedation in adults. Am J Emerg Med 2008;26:985–1028.
5. Aufderheide TP, Sigurdsson G, Pirrallo RG, et al. Hyperventilation-induced hypotension during cardiopulmonary resuscitation. Circulation 2004;109:1960–5.
6. O'Neill JF, Deakin CD. Do we hyperventilate cardiac arrest patients? Resuscitation 2007;73:82–5.