with extreme caution to patients who may be at risk for development of prolonged QT syndrome, are more than 65 yr old, abuse alcohol, or when used concomitantly with benzodiazepines, volatile anesthetics, and IV opiates. Electrocardiogram monitoring should be performed before treatment and continued for 2–3 h after completing treatment to monitor for arrhythmias.3 Being on palliative care and having a history of VT are not on the black box. You are correct that a total of 523 patients had a documented QTc more than 440 ms in the medical record within 1 yr before receiving droperidol. Use of this drug in these patients is not consistent with the black box warning, but none of the patients had polymorphic VT.

I certainly respect your views and I respect the desires of the Food and Drug Administration efforts to protect patients, but now we have two large safety studies that indicate the black box warning possibly should not apply to low-dose droperidol administration. Also, a recent quantitative system review of 25 trials of low-dose droperidol in 2,957 patients by Schaub et al.4 found that prophylactic doses of droperidol less than 1 mg were antiemetic and there were no reports of QT prolongation or cardiac arrhythmia. Droperidol is not a perfect drug. I tend to use it only when it is strongly indicated, because its administration can be associated with dysphoric mentation. As you may know, the Food and Drug Administration also placed a Drug Safety Communication on ondansetron in 2011.* There are a multitude of commonly used drugs, which prolong the rate-corrected QT electrocardiographic interval, including volatile anesthetics, muscle relaxants, other antiepileptics, and some opioids. Should all these agents have black box warnings?

I certainly agree that caution should always be used, but the current black box warnings are based mostly on case reports. Would it not be better to base these warnings on science? I realize that retrospective studies are not robust science. I realize that retrospective studies are not robust science, but they are better than no science.

Gregory A. Nuttall, M.D., Mayo Clinic, Rochester, Minnesota. gnuttall@mayo.edu

References


(accepted for publication May 22, 2013)


To the Editor:

We read with great anticipation and interest the latest “Practice Guidelines for Management of the Difficult Airway: An Updated Report by the American Society of Anesthesiologists Task Force on Management of the Difficult Airway” by Apfelbaum et al.1 Although we would like to congratulate them on a comprehensive review and distillation of evidence and opinion, in general, we must admit that we were incredibly disappointed that determining a patient’s risk of aspiration, which is a critical and essential assessment when considering whether to perform airway management before or after the induction of anesthesia, failed to be considered and added to the new guidelines.

This oversight represents a significant missed opportunity to further enhance the airway management recommendations from our specialty and could possibly compromise the safety of patients given the magnitude of this omission. Certainly, one could argue that aspiration risk assessment is so basic as to mitigate its need to be introduced into the current guidelines. However, the task force’s recommendation that everyone should have a preoperative airway evaluation and that everyone should receive preoxygenation seems as obvious and important so as to not be considered self-evident.

As a simple example, take the patient with advanced scleroderma. In these patients, fascial calcinosis alone or with concurrent temporomandibular joint disease is common and can severely limit mouth opening, necessitating fiberoptic bronchoscope-assisted intubation, generally through the nares. Although a patient with limited mouth opening can often be easily intubated with a fiberoptic after induction, esophageal involvement makes aspiration a real risk in these patients (their incidence of gastroesophageal reflux disease is 90%),2 and an awake intubation would be the only prudent means of securing the airway. And although this is but one example, the reader can imagine several others where nonreassuring airway findings and an aspiration risk mean that performance of a rapid sequence induction and intubation
Updated Difficult Airway Algorithm: Confusing and Contradictory

To the Editor:
We read with great interest and some concern the revised Practice Guidelines for Management of the Difficult Airway. The Difficult Airway Algorithm, presented in figure 1, has some confusing recommendations.

Before entering the flow chart in figure 1, the reader is asked to assess the difficulty of supraglottic airway (SGA) placement. Multiple factors are known to increase the probability of difficult mask ventilation and difficult laryngoscopy and/or intubation. However, similar evidence is absent for the assessment of difficult SGA placement. Excluding an inadequate mouth opening for insertion, difficulty with an SGA is usually unanticipated.

Next, in the “AWAKE INTUBATION” pathway, if awake intubation fails, the authors suggest inducing general anesthesia assuming that “mask ventilation will not be problematic.” It seems that if an awake intubation was attempted, difficult mask ventilation and/or laryngoscopy is anticipated, and induction of general anesthesia is not in the best interest of patient safety. This is supported by the 2005 closed claims analysis that found that two thirds of difficult airway claims were associated with an induction of anesthesia. Given recent discussion suggesting the decreased skill of providers with bronchoscopy in the presence of newer airway equipment (i.e., video laryngoscopes), perhaps part of this pathway should include consulting a colleague or considering an alternative awake airway approach including, as mentioned by the authors, invasive airway access.

Finally, if the “NONEMERGENCY PATHWAY” (“FACE MASK VENTILATION ADEQUATE”) shifts to the “EMERGENCY PATHWAY” (“BOTH FACE MASK AND SGA VENTILATION BECOME INADEQUATE”), the algorithm details to use “Emergency noninvasive airway ventilation.” Similarly, the use of “Emergency noninvasive airway ventilation” is also recommended when “SGA NOT ADEQUATE OR NOT FEASIBLE.” However, as defined in figure 1E, “Emergency noninvasive airway ventilation consists of an SGA”. If SGA ventilation was already proven to be inadequate, how does this recommendation improve the situation?

We recognize and appreciate the authors’ efforts to provide a clear algorithm for this complicated topic. However, we feel that the above issues may create confusion.

Sylvia H. Wilson, M.D.,* Latha Hebar, M.D., F.R.C.A.
*Medical University of South Carolina, Charleston, South Carolina. wilsoh@musc.edu

References


(Accepted for publication May 24, 2013.)