Anesthesia delivery in the United States has evolved significantly over the past 30 years with the introduction and routine use of pulse oximetry, capnography, modern drugs, and newer airway management devices, and there is general consensus that the anesthetic experience in 2011 is far safer than the anesthetic experience of 1980. However, those of us delivering anesthesia care during much of this period of rapid innovation and safety improvements have seen little in the way of change in “bread and butter” practice in the past 15 years, and our new students and residents are taught to perform a “standard” general anesthetic with endotracheal intubation pretty much as we “seasoned practitioners” did in the year 1995. We are not talking about cardiac anesthesia, with the emergence of transesophageal echocardiography, or regional anesthesia, with the commonplace use of ultrasound techniques, but rather colon resections, cholecystectomies, urological procedures, gynecological procedures, and so on, where special monitoring or imaging is not used routinely.

There are reasons for this, of course, the most important being that our technique for a standard general anesthetic with endotracheal intubation works and works well, and we provide our surgeons with a stable patient who wakes up quickly in the recovery room with a minimum of pain and nausea. So why revisit this topic, or in the words of one of my old Tennessee colleagues, “why fix what ain’t broke?” Herein lies the heart of the issue: Are there advantages in tweaking a well-oiled machine, advantages significant enough to motivate experienced anesthesia practitioners to modify what they teach new trainees? Will the Anesthesia Quality Institute (AQI), which is certain to address the complications resulting from the much bigger iceberg, and to remain relevant anesthesia providers had to address the complications resulting from the much bigger problem of medical, surgical and anesthetic interactions. Early work by researchers such as Yaeger et al. and Lieberman et al. had already provided important examples of the potential influence of anesthesia technique on what traditionally had been considered anesthesia complications represented only the tip of the iceberg, and to remain relevant anesthesia providers had to question some of our tried and true traditions for the induction, maintenance, and emergence of a standard general anesthetic with endotracheal intubation, including the optimal range of end-tidal CO2 intraoperatively.

I arbitrarily define the start of the era of modern anesthesia as coinciding with the publication of the Institute of Medicine’s “To Err is Human: Building a Safer Health System.” With the Institute’s acknowledgment of the success of our efforts to reduce traditional anesthesia complications, we became confident enough to join broader efforts to reduce overall perioperative complications, such as the Surgical Care Improvement Project. Numerous important works by Sessler et al., Shakhar and Ben-Eliyahu, and many others began to appear on the long-term consequences of anesthesia care. Roy and Calicott emphasized that the purview of what had formally been considered anesthesia complications represented only the tip of the iceberg, and to remain relevant anesthesia providers had to address the complications resulting from the much bigger problem of medical, surgical and anesthetic interactions.

Early work by researchers such as Yaeger et al. and Lieberman et al. had already provided important examples of the potential influence of anesthesia technique on what traditionally had been considered surgical or medical morbidity. Simultaneously, prominent figures such as Joshi, Akca, Lindahl and Mure, and Lumb began to question some of our tried and true traditions for the induction, maintenance, and emergence of a standard general anesthetic with endotracheal intubation, including the optimal range of end-tidal CO2 intraoperatively.

Horlocker and Brown have emphasized that in anesthesia it is difficult to obtain level A Class 1 evidenced-based medicine studies that demonstrate the advantages of one technique over another. They noted that “observational data or even pathophysiological reasoning may be the best evidence available to support a clinical decision. ” I implore the reader to keep this key concept in mind as we discuss some of the topics in this article. Much of what we do in current anesthesia practice is based on tradition and consensus of “experts.” Suggestions that we change anything in our routine for a standard general anesthetic with endotracheal intubation are controversial, and many excellent anesthesia providers may take offense or disagree with the proposals I put forth below. But in the spirit of “The Open Mind” series, and in light of the previous remarks of Horlocker and Brown, I believe these proposals will stimulate discussion.
dogma to verify our ability to mask ventilate before administering a muscle relaxant, and the practice of using 100% oxygen for induction and emergence from anesthesia. Other anesthesia dogmas and shibboleths (traditions characteristic of a group, often considered outdated) were also being challenged or discarded. In what follows, I have selected a few of these exciting trends for discussion.

To immediately hone in on the points I will be making, I have listed six current dogmas and shibboleths, along with a modified version of each, which I believe better reflects current literature and (in the words of Horlocker and Brown) “pathophysiological reasoning.” This in turn is followed by a discussion of each item and the reasons I believe these modest changes in our method of practice are worthy of consideration.

1. Shibboleth: Venous thromboembolic prevention is strictly a surgical issue.
   Modified version: Venous thromboembolic prevention is a shared responsibility of the surgeon and anesthesiologist.

2. Dogma: Administration of a muscle relaxant should only occur after ability to mask ventilate is verified.
   Modified version: Administration of a muscle relaxant should occur without verifying ability to mask ventilate.

3. Shibboleth: Anesthesia providers should use fentanyl to blunt the sympathetic response to tracheal intubation.
   Modified version: Anesthesia providers should use substitutes for fentanyl such as alfentanil and remifentanil to blunt the sympathetic response to tracheal intubation.

4. Shibboleth: Anesthesia providers should maintain the end-tidal CO$_2$ level during surgery between 32 to 37 mm Hg.
   Modified version: Anesthesia providers should maintain the end-tidal CO$_2$ level during surgery between 40 to 45 mm Hg.

5. Dogma: All general anesthetics that result in a stable, comfortable patient in the recovery room are equivalent with respect to long-term outcome.
   Modified version: General anesthetics that result in a stable, comfortable patient in the recovery room vary greatly in their effects on long-term outcome.

6. Dogma: All patients should be awakened from anesthesia while receiving 100% oxygen.
   Modified version: Emergence from anesthesia in most patients should involve administration of 80% oxygen.

**DISCUSSION**

1. Time out and venous thromboembolism (VTE): Most anesthesia providers consider that prevention of VTE is strictly within the domain of surgical responsibility. Aggressive initiatives by the Surgical Care Improvement Project, the Institute for Healthcare Improvement, and various other organizations have made thromboprophylaxis with perioperative low molecular weight heparin/unfractionated heparin and/or leg sequential compression devices (SCDs) a standard of care for major surgical procedures. Our understanding of the pathophysiology of venous thrombi indicates that thrombi begin to form at induction of anesthesia, secondary to a marked reduction in venous blood flow in the lower extremities. This reduction in flow is further aggravated by initiation of positive pressure ventilation. The classic 1970 monograph by Hume et al. as well as Martino et al.’s work have both noted that half of the venous clots associated with surgery are formed during the intraoperative period. In a randomized trial by Delis et al., epidural anesthesia was shown to increase venous blood flow during anesthesia induction. This may indeed explain the well-known decrease in thromboembolic risk with epidural use. In addition to venous stasis, surgery activates the clotting system. In fact, the “sludging of blood” during periods of stress and illness was described 50 years ago by Melvin Knisely et al. Further complicating this picture of clot formation, studies on the rheological properties of blood suggest the existence of a minimum shearing force (“yield stress”) required to restart blood movement once bloodflow ceases. In rheological parlance, blood is known as a “Bingham fluid.” This is a viscous fluid that acts as a solid at low shear stresses, but that flows like a (non-Newtonian) fluid once the minimum shearing force is exceeded. As a result of this yield stress, venous stasis begets more venous stasis. Because venous stasis after anesthetic induction and initiation of positive pressure ventilation likely begins in the deep calf veins, there are obvious advantages in providing pulsatile movement to the lower extremities before anesthetic induction. It is therefore surprising that verification with the circulating nurse of the application and activation of SCDs before induction is not taught routinely to new trainees. With anticoagulant thromboprophylaxis, there is also evidence that early or preoperative initiation of low molecular weight heparin or low-dose heparin reduces the risk of thromboembolic complications after surgery, although many surgeons in the United States prefer to initiate these pharmacological measures postoperatively. Therefore, the simple step of verifying that the nursing staff has initiated mechanical thromboprophylaxis (i.e., the SCDs are on and “squeezing”) before anesthetic induction assumes even greater significance, with potential major benefits for our patients. Even the simplest of chemical engineering models of the nidus of clot formation in the venous sinuses, the continuous stirred tank reactor, would suggest that interruption of bloodflow by delayed application of SCDs may be quite harmful. Adding such a check to the preinduction portion of the checklist suggested by Haynes et al. and Weiser et al. seems a reasonable and prudent step in improving anesthesia safety with

---

respect to both short- and long-term outcome measures. I believe that such a checklist will ultimately become an AQI performance measure. In a study by Einstein et al.\textsuperscript{24} of a patient group with a very high risk of VTE, those with gynecological malignancies, adding pharmacological prophylaxis to patients whose SCDs were started before anesthesia induction resulted in a 60% additional reduction in the incidence of VTE, such that the overall incidence was about 2%, compared to roughly 20%–40% in the untreated patient!

2. Administration of a muscle relaxant should be delayed until adequacy of mask ventilation is established: The dogma to “check for ability to ventilate before administering a muscle relaxant” has existed for as long as I can remember in anesthesia training. In 2009, Pennant and Joshi in their paper, “Traditions, Dogmas and Myths in Anesthesia Practice,” noted that, “the more logical and safer approach in all patients would be to administer a muscle relaxant at the earliest opportunity without having to demonstrate facemask ventilation beforehand.”\textsuperscript{25} Salem and Ovassapian\textsuperscript{26} emphasized the same point, noting this particular dogma is not supported by available clinical evidence. The study of Warters et al. reported an identical conclusion, demonstrating a favorable effect of neuromuscular blockade on the ease of difficult mask ventilation.\textsuperscript{27} Also, Calder et al., in a provocative editorial, “Could Safe Practice Be Jeopardizing Safe Practice?”\textsuperscript{28} discussed the potential disadvantages resulting from a ventilation check before administration of a muscle relaxant. Pennant and Joshi’s admonition, as well as the main inference of these other publications, is that administration of a muscle relaxant at the time of induction, immediately after the induction drug, is in most cases a safer and better practice than first checking for ability to mask ventilate. In real world practice, inability to ventilate after administration of an induction drug is usually followed by injection of a muscle relaxant. The modern anesthesia provider has many tools at his or her disposal that were not available a few decades ago, including pulse oximetry, a difficult airway algorithm, experience with awake and asleep fiberoptic intubations, and numerous difficult airway rescue devices. Clinically, the idea of letting the induction drug wear off and allowing the patient to wake up is often difficult and impractical, leading to struggles with mask ventilation, laryngospasm, and pulmonary aspiration. Perhaps it is time to reevaluate this deeply embedded dogma. As always, in cases where difficulty in ventilation or intubation is anticipated an awake (or asleep) fiberoptic intubation is still often the method of choice.

3. Administration of fentanyl, 50 to 150 mcg before intubation: It is customary for many anesthesia providers to administer 50 to 150 mcg of fentanyl to “reduce the sympathetic response to intubation.” However, as Shafer and Varvel noted 20 years ago in an elegant pharmacological study comparing alfentanil, fentanyl, and sufentanil, “if only a single peak effect is desired, such as might be needed to blunt the response to intubation during a very brief procedure, then alfentanil is the drug of choice.”\textsuperscript{29} Remifentanil was not available at the time of Shafer and Varvel’s study. Alfentanil has a much more rapid onset and offset than fentanyl, which would seem to make it preferable over fentanyl for intubation (as they note). If one follows the recommendation to administer the muscle relaxant immediately after the induction agent, then alfentanil should be administered concurrently, instead of 3 to 5 minutes earlier, as fentanyl usually is. At least one older anecdotal report suggests alfentanil provides better intubating conditions than fentanyl.\textsuperscript{30} Very conveniently, on a volume basis, the dosing of alfentanil is identical to that of fentanyl. If analgesia is required before surgical incision, a longer acting opioid can then be given. However, alfentanil’s short duration of action will reduce the need for vasopressors compared to fentanyl before commencement of the operation. If there is a 15- to 30-minute delay between anesthesia induction and surgical incision (which is quite common in an academic institution), then this small change from fentanyl to alfentanil or another very short-acting narcotic such as remifentanil for intubation would seem worthwhile. Available literature actually suggests remifentanil may be preferable to alfentanil in this regard,\textsuperscript{31} but remifentanil requires dilution whereas alfentanil does not. Certainly, if remifentanil is going to be used intraoperatively and an infusion has already been prepared, then using it before tracheal intubation makes good sense. In the case of cancer surgery, there are additional advantages in minimizing narcotic use and duration of narcotic action.\textsuperscript{32} If other non-narcotic analgesics are used to provide analgesia intraoperatively, the sooner the narcotic effects after intubation are gone, the better. We suspect, in the vein of Horlocker and Brown’s work,\textsuperscript{33} that switching from fentanyl to alfentanil would likely never have the support of evidence-based medicine behind it, because the clinical effects are likely rather small; however, it does make good sense from a “pathophysiological standpoint.”

4. Intraoperative end-tidal CO\textsubscript{2}: When asked about the relationship between end-tidal CO\textsubscript{2} and arterial CO\textsubscript{2}, most anesthesia providers would reply that arterial values are approximately 3 to 5 mm Hg higher than end-tidal values. This relationship between end-tidal and arterial carbon dioxide, although highly variable and dependent on multiple physiologic factors, is the basis for the common tradition of keeping the end-tidal CO\textsubscript{2} in the range of 32 to 37 mm Hg intraoperatively, corresponding to an approximate arterial level of 37 to 42 mm Hg. What is not realized, and what Hill and Joshi,\textsuperscript{34} among others, have emphasized, is that when the level of arterial CO\textsubscript{2} is within the control of the anesthesia provider, there are often significant advantages to maintaining this level above the usual physiologic limits. Yet, many clinicians do not avail themselves of these numerous advantages. Although far from an exhaustive list, elevated arterial
CO₂ levels improve tissue oxygenation, increase cerebral, coronary and tissue bloodflow, and help to maintain arterial blood pressure by both sympathetic stimulation and less reduction in venous return." The work of Akca et al. summarizes many of these benefits⁹,³³ and Hill and Joshi suggest an end-tidal CO₂ of 40 to 45 or higher should come to be the standard of care intraoperatively.¹³ Thus, the best practice of anesthesia in 2011 might very well include maintaining higher end-tidal CO₂ levels. The resulting modest level of respiratory acidosis shifts the oxygen hemoglobin dissociation curve to the right, thus unloading more oxygen to tissues and increasing the effectiveness of oxidative killing by neutrophils; acidosis improves hemodynamics as a result of the mild sympathetic stimulation and reduced interference with venous return; and improves arteriolar bloodflow. The detrimental effects of mild hypercarbia are significantly outweighed by the numerous positive benefits. Distressing clinical reports of cerebral ischemia during surgery in the sitting position, due to hypotension and a reduced effective cerebral perfusion pressure below that measured by a traditional arm blood pressure cuff, are often accompanied by end-tidal CO₂ values in the low 30s.³⁴ In Pohl and Cullen’s report, the end-tidal CO₂ varied from the high 20s to the low 30s mm Hg. New Advanced Cardiac Life Support guidelines also stress the detrimental influence of hypocarbia on global cerebral perfusion.⁹ In conclusion, from a pathophysiological standpoint, it would appear prudent when possible to maintain a mildly elevated end-tidal CO₂ in any case involving controlled hypotension.⁹ Caveat: There are clearly acute and chronic medical conditions where elevated end-tidal CO₂ values are inappropriate, such as elevated intracerebral pressure, hyperkalemia, and cor pulmonale. We recommend higher CO₂ levels in routine clinical practice.

5. Anesthesia and cancer: In the gripping and insightful new book on the history of cancer, "The Emperor of All Maladies," Mukherjee noted that as early as 100 BC, it was well known that surgical removal of a cancerous tumor often hastened the cancer’s spread. Mukherjee’s insider’s view is unique and brings into crystal clear focus the immense challenges this powerful and pervasive illness presents. For colon cancer surgery, as well as the majority of surgeries involving primary resection of a solid tumor, seeding or microscopic spread occurs during tumor removal.⁴⁻⁶ The likelihood that these micrometastases will either grow or die depends in large part on the body’s natural killer cells. Both seeding and the importance of natural killer cells are accepted physiologic facts. A large body of experimental studies suggests that natural killer cell function is suppressed by inhalation anesthetics and narcotics.⁴⁻⁶,³¹,³²,³⁶⁻³⁹ That anesthesia during cancer surgery may possibly be able to reduce the spread of micrometastases is of enormous clinical consequence. Therefore, techniques that minimize narcotic and inhaled anesthetic use would appear to offer benefit to the cancer surgery patient. Clearly, drugs such as nonsteroidal anti-inflammatory drugs, clonidine, dexmedetomidine, and particularly propofol, allow minimization or elimination of inhaled anesthetics and narcotics, and until proven otherwise, would appear to be useful adjuvants for cancer surgery.³²,³⁶⁻³⁹ Although clinical trials on a vast scale are not currently available, it would appear prudent to consider the plentiful laboratory results and the handful of clinical studies. Thus, for primary resection of a cancerous tumor, the best practice of anesthesia might well be a technique that uses regional blocks or drugs such as dexmedetomidine, nonsteroidal anti-inflammatory drugs, and a total IV anesthesia technique with propofol, minimizing or eliminating narcotic, and inhaled anesthetic use.³⁷ For breast cancer, the most common cancer in women, with almost 1 million new cases a year expected in the United States within a few years, some highly regarded centers have already adopted this practice, but this is the exception rather than the rule. We propose that barring some contraindication, anesthesia techniques shown in the laboratory to be effective in reducing cancer spread after primary tumor resection be used routinely. As in the case of pulse oximetry, strong laboratory evidence coupled with common sense make a strong case for this change in practice, despite the lack of incontrovertible evidenced-based human data.

We do need to close by noting that not all studies show a beneficial effect of regional blockade. For example, Myles et al. noted no benefit from epidural analgesia for major abdominal cancer surgery.⁴¹ However, Tsui and Green pointed out that Myles et al. still used inhalation anesthetics and narcotics intraoperatively, and those cancer patients also received intraoperative blood transfusions.³² This author believes in the adage “in for a penny, in for a pound” with cancer surgery and prefers a strict total IV anesthetic technique with propofol, minimal narcotics, and regional analgesia when possible.

6. Wakeup on 100% oxygen: As in the year 1995, for the typical anesthetic emergence in 2011, the trainee is instructed to awaken their patients on 100% oxygen. This provides an extra measure of security in the event of laryngospasm, airway obstruction, or inadequate gas exchange after tracheal extubation. Recently, this practice has been questioned by a number of leading authorities.¹⁰,¹¹,⁴³ Both Lumb and Lindahl and Mure noted that brief periods of 100% oxygen lead to significant atelectasis, which may persist up to 4 days after surgery, and increases the likelihood of postoperative pulmonary complications.³⁴ In a recent study, Lumb et al. also demonstrated that this atelectasis cannot be reversed effectively by standard recruitment maneuvers.⁴⁵ Benoit et al., using computed tomography scans to measure the extent of atelectasis, noted a significant, 6.8% degree of atelectasis in
those patients awakened on 100% oxygen versus 2.6% in patients awakened on 40% oxygen, with both groups of patients having performed vital capacity maneuvers before tracheal extubation.34 Loeckinger, using a porcine model, measured postoperative gas exchange 30 minutes after tracheal extubation by a multiple inert gas elimination technique.36 One hundred percent oxygen before extubation compared to 30% oxygen before extubation more than doubled the bloodflow to poorly ventilated units.46 Lumb et al.45 and Hedenstierna and Edmark43 emphasized that a number of mechanisms, in addition to 100% oxygen, may be involved in postoperative atelectasis, but it is clear that a decrease in inspired oxygen at anesthesia emergence does ameliorate the severity of atelectasis.43 Although an oxygen saturation of 96% is considered acceptable in most institutions for postanesthesia care unit discharge, this corresponds to an oxygen partial pressure of perhaps 70 to 75, consistent with 5%–10% atelectasis in a healthy individual. Both Lumb and Lindahl and Mure suggested that awakening patients in most instances on 80% oxygen by combining oxygen and air is far more logical in helping eliminate this troublesome issue.10,17 Hedenstierna and Edmark noted that “Recruitment at the end of the anesthesia followed by ventilation with 100% oxygen causes new atelectasis before anesthesia is terminated but not with ventilation with lower fraction of inspired oxygen (FiO2).”44 Akca et al. showed more than a decade ago, using computed tomography scanning, that awakening patients on 80% oxygen caused only slightly more atelectasis than 30% oxygen.47 Thus, in general, the best practice for tracheal intubation all fall into this category. Perhaps my main take-away lesson is that not all anesthetics that result in a stable patient intraoperatively who wakes up quickly in the recovery room with a minimum of pain and nausea are equal, and many of the multiple things we do or do not do during the time the patient is under our care have implications that last far beyond the postanesthesia care unit period.

DISCLOSURES

Name: Ronald J. Gordon, MD, PhD.
Contribution: This author designed the study, conducted the study, analyzed the data, and wrote the manuscript.
Attestation: Ronald J. Gordon approved the final manuscript.

This manuscript was handled by: Sorin J. Brull, MD, FARCISI (Hon).

REFERENCES

4. Exadaktylos AK, Buggy DJ, Moriarty DC, Maeda, E, Sessler DI. Can anesthetic technique for primary breast cancer surgery affect recurrence or metastasis? Anesthesiology 2006;105:660–4
5. Sessler DI. Long-term consequences of anesthetic management. Anesthesiology 2009;111:1–4
11. Lumb AB. Just a little oxygen to breathe as you go off to sleep: is it always a good idea? Br J Anaesth 2007;99:769–71


