it safe! But perhaps we should fault an anesthesiologist who unnecessarily canceled elective procedures because he or she was uncomfortable anesthetizing the patients before their MH status had been established by biopsy. The debate about the usefulness of the muscle contracture test has had a long history.[4] In our era of evidence-based medicine and cost-effective analyses, should we not also reevaluate muscle biopsy testing for MH?

**Igor Kwetny, M.D., F.R.C.P.C., D.E.A.A.,** Red Deer Regional Medical Center, Red Deer, Alberta, Canada. ikwetny@hotmail.com

## References

1. Tautz TJ, Urwyler A, Antognini JF, Riou B: Case scenario: Increased end-tidal carbon dioxide. A diagnostic dilemma. ANESTHESIOLOGY 2010; 112:440–6
2. Kwetny IM, Finucane BT: Negative arterial to end-tidal carbon dioxide gradient: An additional sign of malignant hyperthermia during desflurane anesthesia. Anest Analg 2006; 102:815–7
3. Larach MG, Localio AR, Allen GC, Denborough MA, Ellis FR, Gronert GA, Kaplan RF, Muldoon SM, Nelson TE, Ording H: A clinical grading scale to predict malignant hyperthermia susceptibility. ANESTHESIOLOGY 1994; 80:771–9
4. Larach MG: Should we use muscle biopsy to diagnose malignant hyperthermia susceptibility? ANESTHESIOLOGY 1993; 79:1–4

*(Accepted for publication May 26, 2010.)*

# All Valve Malfunctions Are Not the Same

*To the Editor:*

We congratulate Tautz and colleagues[1] on an insightful case presentation of malignant hyperthermia and systematic analysis of increased end-tidal carbon dioxide. We write to clarify a detail in their analysis that may be misunderstood.

The capnograph/meter is an essential tool for deciphering the etiology of increased carbon dioxide during anesthesia. As a point of clarification, inspiratory and expiratory valve malfunctions in anesthesia breathing circuits do not result in identical capnograms, as shown in figure 1 of the article. The capnogram in the upper left panel of this figure shows increased carbon dioxide with increased inspiratory baseline. Although this is accurate for a stuck expiratory valve, the capnogram of a stuck inspiratory valve is actually quite different, because there is a dampening of the inspiratory downstroke on the capnogram, which does in fact get to zero.[2]

Consider a circuit with the inspiratory valve removed. In this scenario, the exhaled breath with carbon dioxide-rich gas is exhaled about equally into both limbs of the breathing circuit; therefore, about half of the exhaled tidal volume partially fills the inspiratory limb. With the next breath, the carbon dioxide–rich gas from the inspiratory limb is reinspired first, followed by fresh gas without carbon dioxide. The capnometer thus displays a sluggish inspiratory downstroke (or a $\beta$ angle greater than 90°).[2] The inspiratory baseline will therefore return to zero during the second half of inspiration. These capnogram differences may seem subtle but can be critical in the identification of machine fault etiologies.

**Chris Giordiano, M.D., Nikolaus Gravenstein, M.D., Mark J. Rice, M.D.*** *University of Florida College of Medicine, Gainesville, Florida. mrice@anest.ufl.edu

## References

1. Tautz TJ, Urwyler A, Antognini JF, Riou B: Case scenario: Increased end-tidal carbon dioxide: A diagnostic dilemma. ANESTHESIOLOGY 2010; 112:440–6
2. van Genderingen HR, Gravenstein N, van der Aa JJ, Gravenstein JS: Computer-assisted capnogram analysis. J Clin Monit 1987; 3:194–200

*(Accepted for publication May 26, 2010.)*

*In Reply:*

Dr. Kwetny argues that contracture testing has limited usefulness in the management of patients who might be susceptible to malignant hyperthermia (MH). As a biologic test, 98% sensitivity is commendable. Very few commonly used diagnostic screening tests approach that level of accuracy. We formulate anesthesia plans on a daily basis using tests with much poorer positive predictive value (*e.g.*, electrocardiogram, echocardiogram, creatinine, hematocrit).

Furthermore, contracture testing has been a useful tool to identify genetic mutations in 60–80% of MH families. Because the number of identified causative mutations in MH families has increased over the past decade we now can offer noninvasive and less expensive genetic testing to many MH families.

In addition, we disagree that a nontriggering anesthetic is 100% safe (*e.g.*, propofol infusion syndrome, awareness). Volatile anesthetics have real and unique benefits. We wonder whether, because of his belief that a nontriggering anesthetic is 100% safe, Dr. Kwetny provides nontriggering anesthetics to all of his patients, regardless of MH status.

What is most disturbing is the reticence not to consider the test at all and label a patient MH-susceptible based solely on clinical criteria, especially when those criteria are minimal. Permitting patients to be labeled MH-susceptible by individual clinicians who might not have the requisite expertise can subject that patient and his or her family to the hardship of finding clinicians who will care for them.

We counsel numerous patients referred for potential testing with vague personal or family history of potential MH. These are patients who have tried to obtain anesthetic care in the community and have been told that they cannot be anesthetized until they have been tested for MH. We are at a loss to explain why so many anesthesiologists are reluctant to provide nontriggering anesthetics before a biopsy procedure, especially if that is exactly what they will provide after the biopsy.

Finally, various factors influence a patient's decision to undergo contracture testing, including: size of family and pedigree, profession, fear, budget, insurance, and location of the closest laboratory. Anesthesiologists who exclusively favor or disfavor contracture testing have a quite simple paternalistic view, which may be appropriate in some locales but not others.

We echo Dr. Kwetny's call for evidence-based data on the usefulness of contracture testing—but that goal will only be accomplished when we have accumulated enough contracture testing data, which in turn requires muscle biopsy and the contracture test, the very test Dr. Kwetny so fervently wants to ignore and discard.

Dr. Giordiano and colleagues rightly point out a limitation in our presentation of causes of increased end-tidal carbon dioxide. Under normal circumstances, a faulty inspiratory valve will produce a capnogram that differs from the stylized version shown in our figure—a version that is more typically seen with malfunction of the expiratory valve.

As we mentioned in the text, and is pointed out by Giordiano *et al.*, a faulty inspiratory valve would lead to an increased amount of carbon dioxide in the inspired gas, although the nadir of the inspired carbon dioxide would approach or equal 0. We hasten to add, however, that under certain conditions (*e.g.*, low fresh gas flow and low tidal volumes), the inspired carbon dioxide might not reach 0 when an inspiratory valve malfunctions.

**Joseph F. Antognini, M.D.,* Timothy Tautz, M.D., Albert Urwyler, M.D.** *University of California, Davis, Davis, California. jfantognini@ucdavis.edu

*(Accepted for publication May 26, 2010.)*

## Safe Epidural Catheter Removal in the Patient Receiving Warfarin: Does Anybody Really Know What (Prothrombin) Time It Is?

*To the Editor:*

We read with interest the study by Benzon *et al.*[1] regarding international normalized ratio (INR) levels, epidural catheter removal, and guidelines developed by the American Society of Regional Anesthesia and Pain Medicine (ASRA). In summary, the authors evaluated the factor VII activities and INR in 121 patients during the initiation of warfarin therapy. Warfarin therapy was started the night of surgery; no additional antiplatelet or anticoagulants, including low-molecular-weight heparin, were administered. The authors reported that on postoperative day (POD) 1, 11 patients had prothrombin times greater than the 1.4 level recommended by ASRA for removal of an epidural catheter.[2–4] In 8 of these 11 patients, despite an increased INR, the factor VII activity levels were within the normal range. In the remaining 3 patients, the factor activities were 45%, 24%, and 22%, corresponding to INRs of 1.5, 1.5, and 1.8. Based on these results, Benzon and coworkers concluded that, for patients receiving epidural analgesia and warfarin for deep vein thrombosis prophylaxis, there is "no evidence that epidural catheters should not be removed even with INRs up to 1.9."

Unfortunately, such a conclusion cannot be supported by their data, because their study did not directly test this hypothesis. Specifically, none of the 121 patients included in their retrospective study had an epidural catheter removed with a concurrent INR of 1.5–1.9. Consequently, the rate of epidural hematoma after epidural catheter removal at this intensity of anticoagulation cannot be estimated from this study, and it is impossible to conclude that such a practice is safe. Moreover, it is unclear from the data presented whether any of these patients even had an epidural catheter in place. *If* epidural catheters were indwelling, catheter management, including the duration of epidural catheterization, factor VII activity, and INR at the time of catheter removal are critical to interpretation of the results.

The ASRA recommendation that epidural catheters be removed with the INR $\leq$ 1.4 was derived from studies correlating normal or near normal hemostasis with clotting factor activities greater than 40%. Benzon *et al.* draw attention to the eight of eleven patients on POD 1 with INRs more than 1.4 and normal factor VII activity levels, citing the potential for unnecessary epidural catheter retention and discontinuation of warfarin therapy (until the INR $\leq$ 1.4) as their practice has established to be consonant with the ASRA guidelines. However, they do not address the issue of potential spinal hematoma in the two patients with an INR more than 1.4 on postoperative day 1 who had factor VII activities that were reduced sufficiently (24% and 22%, respectively) to increase the risk of bleeding with an invasive procedure. We question whether anesthesiologists would feel comfortable in removing an epidural catheter with an INR between 1.5–1.9 when almost 20% of such patients are potentially at an increased risk of bleeding.

Conversely, of the 110 patients with INRs $\leq$ 1.4 on POD 1, *none* of the factor VII levels were below 40%, supporting the ASRA guideline of 1.4 as an appropriate cut-off value.

Importantly, Benzon *et al.*[1] (and the "What This Article Tells Us That Is New" journal highlight) do not emphasize that their recommendation to remove an epidural catheter with an INR greater than 1.4 (and as high as 1.9) pertains solely to POD 1, when the INR reflects primarily a reduction of factor VII. With additional doses and time, an INR greater than 1.4 is typically associated with factor VII activity less than 40% (and the potential for inadequate clotting).[5] As Benzon *et al.* noted, "… The INR represents the activity of several coagulation factors during the onset and the steady state of warfarin therapy." Thus, it is imperative that not only the INR but also the duration of warfarin therapy be considered. Notably, many patients receive a *preoperative* dose; their INR on POD 1 would represent 48 h of warfarin therapy.[6] Spinal hematomas have been