To the Editor—I have concern about the article entitled “Performance Characteristics of Five New Anesthesia Ventilators and Four Intensive Care Ventilators in Pressure-support Mode: A Comparative Bench Study,” which appeared in the November 2006 issue of Anesthesiology.1 Several statements made in the article are incorrect, and we appreciate an opportunity to set the record straight. In table 1 of the article, the Avance (GE-Datex-Ohmeda, Munchen, Germany—which would also apply to our Aestiva, Aisys, and Aespire 7900) is listed correctly as a flow-triggered system, but the specifications for flow triggering are incorrect. Flow triggering on the above GE systems is selectable from 0.2 to 10 l/min, to better address the needs of a pediatric population. Also, the inspiratory-to-expiratory cycle is listed as fixed, 25% of peak flow. Instead, pressure-support ventilation on all of the above systems is adjustable, from 5% to 50% of the peak inspiratory flow, again, to better meet the needs of a pediatric population.

A more serious error, however, is the following statement: ‘The best characteristics of the pressurization phase for the anesthesia ventilators were obtained with the Fabius, Primus, and Avance under all tested conditions and were comparable with those obtained with the ICU [intensive care unit] ventilators. The Fabius, Primus, and Avance are piston ventilators, which use an electric motor to compress gas in the breathing circuit, creating the driving force for mechanical insufflation to proceed. Therefore, they use no driving gas and may be used without depleting the oxygen cylinder in case of oxygen pipeline failure.’ These features may explain in part that these more recent anesthesia ventilators have comparable performance to modern ICU ventilators.

The GE Healthcare Avance Anesthesia Carestation does not use a piston, nor do any of the other anesthesia systems from GE Healthcare. We use a microprocessor-driven flow control valve system like all major intensive care unit ventilators (including our own Engstrom critical care ventilator) marketed in the United States. The excellent performance of the Avance in this study is due to the rapid and frequent sensing (0.25 ms) of pressure in the patient’s lungs via the flow sensors, and the rapid response of the flow valves in the ventilator.

The SmartVent system uses a variable orifice flow sensor on both the inspiratory and the expiratory side of the breathing system. These flow sensors incorporate pressure sensors on either side of a bidirectional Mylar flap. As gas flows through the sensors and encounters the flap, a pressure difference is created between the two sides of the flap. If there is a lot of gas flowing, the pressure difference between the two sides of the flap is more pronounced. If the gas flow is less, the pressure differential is less pronounced. The Mylar flap flexes more or less (hence the variable orifice attribute) depending on the flow, which makes the sensor accurate across the complete flow range. This also allows the SmartVent to cover the complete patient range from tiny neonates to obese adults. The SmartVent uses these pressure differential measurements on the inspiratory side to determine the total flow rate (fresh gas and flow from the bellows). This allows for tidal volume compensation, so that the correct tidal volume is delivered, regardless of fresh gas flow, oxygen flush, or compliance losses in the breathing system.

Because the flap is bidirectional, the SmartVent notices which side of the flap has a greater pressure and so can determine flow direction, allowing for notification of the clinician if a reverse flow condition occurs in the circle system. The inspiratory flow sensor also communicates pressure changes in the patient’s lungs to correctly deliver pressure-control ventilation, or to limit the airway pressure in volume-control ventilation. The inspiratory flow sensor also responds to negative flow during spontaneous modes of ventilation, such as pressure-support and synchronized mandatory ventilation. The inspiratory flow sensor, microprocessor, and flow valves give perioperative patients the advantages of critical care ventilation. The expiratory flow sensor is an independent monitor that reports the patient’s exhaled tidal and minute volumes, and is not involved in volume compensation, ventilation calculations, or responses.

GE Healthcare Anesthesia Systems also have a multiple-breath, standing bellows, unlike the other anesthesia units cited, which gives visible confirmation of the integrity of the breathing system and the adequacy of fresh gas flow. Fresh gas is delivered directly to the inspiratory limb, not through the breathing bag, as is the design with a decoupled piston ventilator. Therefore, changes made by the user to fresh gas and anesthetic agent settings take effect at the patient much more quickly in the Avance Carestation than in fresh gas decoupled systems that use the breathing bag as a reservoir. The volume of the breathing circuit is only 2.7 l, so the Avance has less than half the volume (and time constant) of fresh gas decoupling systems, especially when those decoupled systems use a 3-l rebreathing bag.

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Reference


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trol valve system like all major intensive care unit ventilators combined with a dynamic regulation of the internal compliance of the ventilator. These features may explain in part why these more recent anesthesia ventilators have comparable performance to modern intensive care unit ventilators.

As stated in the editorial by Hossam Tantawy and Jan Ehrenwerth, this study permitted an important addition to the literature because it shows that technology improvements in traditional intensive care unit ventilators can be transferred to the operating room and the anesthesiologist must understand the pressure-support ventilation indications and limitations, know how to implement it properly, and determine its cost-effectiveness.

Anesthesiology 2007; 107:1030

Can 1% Sevoflurane with 5 ng/ml Remifentanil Prevent Implicit Memory?

To the Editor.—We read with much interest the recent report “When Is a Bispectral Index of 60 Too Low?” by Manyam et al. They wrote that 1% sevoflurane is usually sufficient to produce clinically adequate anesthesia with an effect site concentration (Ce) of 5 ng/ml remifentanil. They used the Observer’s Assessment of Alertness/Sedation scale to assess the sedation level simultaneously measuring the Bispectral Index (BIS) and auditory evoked potential index (AAI), and they concluded that targeting a BIS less than 60 or an AAI less than 30 may result in an excessively deep anesthetic state during sevoflurane–remifentanil anesthesia.

Previous reports have shown that opioids reduce the Ce of anesthetics at loss of consciousness. We think this phenomenon does not always indicate that opioids decrease the adequate Ce of anesthetics for maintenance of anesthesia. We cannot assess the level of hypnosis at the surgical level of anesthesia with the Observer’s Assessment of Alertness/Sedation scale, because the scale is always 0 when BIS decreases below 50, 40, or less. Currently, there is no direct evidence whether opioids can reduce the Ce of anesthetics to maintain an adequate level of hypnosis for surgery when a medium to high dose of opioids is used. At the surgical level, we cannot tell whether electroencephalogram- or evoked potential-derived parameters, such as the BIS, are adequate to indicate the level of hypnosis, because we cannot assess the adequacy of level of hypnosis other than by using the monitors based on electroencephalographic or evoked potentials.

The major goal of general anesthesia is to maintain unconsciousness and amnesia during surgery. And we should pay much attention to memory, especially “implicit memory.” In most cases, the concentration of anesthetic that prevents explicit memory is lower than the concentration that prevents implicit memory; our view is that we should prevent it better.

Can 1% Sevoflurane with 5 ng/ml Remifentanil Prevent Implicit Memory?

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5. Smith C, McEwan AI, Jhaveri R, M MW, Goodman D, Smith R, Canada AT, Takahiko Mori, M.D., Takashi Mashimo, M.D., Ph.D. *Osaka University Graduate School of Medicine, Osaka, Japan. hagihira@anes.med.osaka-u.ac.jp

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In Reply.—We appreciate the interest of Dr. Hagihira et al. regarding our recent article.1 We would like to address their concerns regarding our methodology and recommendations.

In our volunteer experiments, we were careful to allow adequate time for equilibration after each step change in sevoflurane to achieve a pseudo-steady state. At each time point, we confirmed a stationary anesthetic level by directly measuring the end-tidal concentration of sevoflurane using a calibrated agent analyzer. An additional indirect confirmation of equilibration was the steady Bispectral Index® (BIS®)2,3 and Auditory Evoked Potential ARX Index (AAI; Alaris Medical Systems, Norwood, MA) measurements in unstimulated volunteers. This is similar to the methodology used by Katoh et al.4 Although the “average” BIS values of 1.0% and 1.5% sevoflurane reported by Katoh et al.4 are different than those that we reported, we believe that both studies had achieved adequate equilibration because the observed and average values of the BIS at each of the modified Observer’s Assessment of Alertness/Sedation scores were similar. In addition, the range of BIS values observed by Katoh et al.4 when the end-tidal sevoflurane was 1.0% (50–75) and 1.5% (30–55) are similar to what we observed.

The discrepancies between the predictions made by Katoh et al.4 and those made by our model are most likely due to the differences in the raw observed data range and the mathematical models used to fit the raw data. We believe that fitting a third-order polynomial to a smaller range of measured concentrations (0–2.5%) compared with generating a response surface for a wider range of measured sevoflurane concentrations (0–6%) resulted in the different predictions of the average BIS values at the various sevoflurane concentrations.

PREVENTING explicit recall is a vital goal of general anesthesia. Multiple investigators have demonstrated that opioids consistently reduce both the minimum alveolar concentration (MAC)5,6 and the MAC required to follow commands (MAC, awake)7 in addition to decreasing the effect site concentration of anesthetics required to produce loss of consciousness. There should be no reason why the observed pharmacodynamic interactions we observed in our human volunteer laboratory should not be evident during maintenance of anesthesia. Therefore, moderate to high doses of opioid should allow a persistent decrease in the anesthetic required to prevent explicit recall throughout the perioperative period.

We also agree with Dr. Hagihira et al. that the importance of implicit recall of intraoperative events is less clear. Unfortunately, implicit recall is much more difficult to evaluate, and therefore, the literature is full of conflicting data on the anesthetic requirements necessary to abolish implicit memory. The data from Iselin-Chaves et al.4 are thought provoking, but the large variety of anesthetic techniques used does not allow conclusions to be made on the efficacy of moderate to high opioid concentrations to block implicit memory. However, if implicit recall can be prevented by diminishing noxious stimulation-induced activation of the auditory centers8 or the arousal centers,9 then moderate- or high-opioid anesthetic techniques may be useful alternatives to hypnotic-based anesthetics. In addition, if “too deep” anesthesia is indeed associated with a worsening in 1-yr survival,10 the use of low or moderate doses of hypnotic with a balance of analgesia (via regional anesthesia, systemic opioids, or a combination) may be desirable.

It is important to reemphasize that our data were derived from volunteers where the electroencephalogram-based assessments of depth of anesthesia were performed independent of response to painful stimuli. This is in contrast to what typically occurs in the operating room, where clinicians make electroencephalogram-based and subjective assessments of sedation in the presence of continuous surgical stimuli. It is conceivable that patients may require more sevoflurane and remifentanil than volunteers to achieve the same degree of electroencephalogram-based sedation. However, based on the current data and the available literature, our recommended anesthetic regimen should prevent explicit recall and may inhibit the formation of implicit memories.

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(Accepted for publication July 13, 2007.)

Does Combined Epidural–General Anesthesia Increase Risk over Either Technique Used Alone?

To the Editor.—Having read the editorial views expressed by Heitmiller, Martinez, and Pronovost,1 I commend the goal of implementing procedures to improve patient safety in the practice of anesthesia. Certainly, changes in teaching practices and techniques that allow safer placement and management of central venous access catheters offer significant opportunities to have a profound impact on serious morbidity and mortality. Changes to connection ports for epidural and intravenous lines to make infusion of unintended substances more difficult also has some potential to decrease risk, although likely on a much smaller and less cost-effective scale when the relatively low incidence of misconnects is taken into account. Clear and better labeling of epidural catheters may provide nearly equal risk reduction at a lower cost. Further study may help to elucidate the ideal solution.

I do have concern, however, about the authors’ remarks in relation to three deaths in patients receiving combined general–epidural analgesia, specifically regarding the authors’ comment, “This is concerning, although perhaps not surprising,” going on to imply that mortality among other complications may be additive by combining the total risk of the two techniques performed independently. Although it may be true that neurologic injury, failure to intubate, or infusion of an incorrect substance

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To the Editor:—I read with great interest the excellent article by Dr. Hove et al. 1,2 detailing their closed claims review of deaths related to anesthesia in Denmark (1996–2004). The accompanying editorial highlights the preventable deaths related to the conduct of central venous cannulation (CVC).1,2

There has been a significant lag in quality improvement for CVC since its role in patient mortality and morbidity was highlighted in 1999 by the Institute of Medicine Report To Err Is Human. 3 It is time for the worldwide anesthesia community to develop and implement evidence-based guidelines, teaching, and standardization for the practice of CVC.

To my knowledge, there is no published practice guideline for perioperative CVC, despite the fact that it is a common peripera-
tive procedure associated with significant medical error and consequent serious patient risk. The literature review that would be part of this guideline’s development would quickly prove the pivotal role of ultrasound guidance. The discussion points at this juncture are not about whether ultrasound guidance is required, but rather about the aspects of its application, such as the superiority of real-time imaging, the role of a needle guide, Doppler imaging, and/or two-dimensional imaging.1,2

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Perioperative Central Venous Cannulation: It Is Time for Action
To the Editor.—We read with great interest the analysis of anesthesia-related deaths registered by the Danish Patient Insurance Association.¹ Hove et al.¹ are to be congratulated for reporting these important results. The authors categorized 24 fatal cases by their underlying causes: airway management, ventilation management, placement of a central venous catheter, medication errors, transfusion error, infusion pump problems, and regional blockade. We noted that 8 of the 24 anesthesia-related deaths described were most likely attributable to a drug error: 4 overdoses (benzodiazepines, methohexital, thiopental, nitroglycerine), 3 infusion pump errors, and 1 patient likely received a large intrathecal dose of mepivacaine. Therefore, the frequency of medication-related incidents exceeded the 4 deaths that resulted from loss of the airway and 4 from complications related to central venous line insertion. It seems that the single most common cause of anesthetic-related death was a drug error.

These findings are consistent with an analysis from the Canadian Medical Protective Association of closed medicolegal claims against anesthesiologists.² The Canadian Medical Protective Association provides malpractice insurance for most physicians in Canada. From 1998 to 2002, there were 232 closed legal actions against anesthesiologists. Medical error was the most common cause involving 52% of the claims. It is noteworthy that the American Society of Anesthesiologists Closed Claims Project reports the proportion of drug errors as 4%.² This number has been consistent throughout the 1980s and 1990s. Reasons for the discrepancy in the relative frequency of medication errors reported in the American Society of Anesthesiologists Closed Claims Database from those in Denmark and Canada requires further exploration but may be attributed, in part, to differences in the categorization of root causes.

The impact of drug error in anesthetic practice is not new and will not surprise experienced anesthesiologists. A survey by the Canadian Anesthesiologists’ Society found that 85% of participants had experienced at least one drug error or “near miss.”³ Most of these errors were of minor consequence; however, 1.8% resulted in major morbidity (cardiac arrest, stroke, permanent injury) or death. The misidentification of a syringe was the most common cause. In 1984, Cooper et al.⁴ published a classic analysis of critical incidents in anesthesia management. Breathing circuit disconnect was the most common identified factor; however, reanalyzing their data set indicates that medication-related events far exceeded airway and ventilation problems. Of a total of 507 incidents, 169 were attributed to errors or problems in drug administration. Equally important, when incidents with “substantive negative outcomes” were further analyzed (defined as mortality, cardiac arrest, cancelled operative procedure, or extended recovery room, intensive care unit, or hospital stay) approximately 25% of them resulted from a drug error.

Together, these studies suggest that the impact of medication error has been underestimated by the lack of a common taxonomy for anesthesia-related adverse events. More importantly, the data beseech us to acknowledge the problem and develop innovative strategies to reduce the likelihood of a drug error in anesthetic practice.

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We never said, or intended to imply, that mortality per se is increased with a combined general and regional anesthetic technique compared with a single technique. Rather, we said that basic probability tells us that the risk of complications will be greater when two techniques are combined compared with each technique alone. We agree that combined techniques may result in benefits, and those benefits may offset the risk.

When deciding whether a patient should have a combined anesthetic technique versus a single technique, the patient and anesthesiologist must decide whether the benefit outweighs the risk. The patient should have the opportunity to make an informed choice. As an example, for a patient who requires general anesthesia for a procedure and is considering the addition of a regional technique, is the benefit of 2 days of modestly improved pain control with a combined anesthetic technique (visual analog scale pain score of 3 with parenteral opioids vs. 2 with epidural analgesia) worth the risk of nerve injury? Although our current knowledge of true estimates of risk is imprecise, we do know that some risks of regional anesthesia are very rare, such as epidural hematoma, but others, such as nerve injury, are more common. Because long-term complications are generally identified by retrospective surveys of clinicians, claims, or chart reviews rather than prospective clinical follow-up of patients, the true risk of injury may be higher than that published for any given anesthetic technique. Are patients informed that they may be trading a potential long-term complication for a short-term gain?

We hope research continues to provide us with more precise estimates of risks and benefits. In the meantime, to the extent possible, patients should be apprised of these risks so they can make an informed choice.

We agree with Dr. Augoustides that evidence-based guidelines are imperative for achieving safe practices for central venous catheter insertions, and we applaud efforts to standardize training in and placement of central venous catheters. There is one published guideline on the use of ultrasound guidance for central venous catheter insertion. The Agency for Healthcare Research and Quality published an evidence report in 2001 that listed the use of real-time ultrasound guidance during central venous catheter insertion to prevent complications as being a highly rated safe practice. The strength of the evidence was considered powerful enough to support widespread implementation.

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return to the hospital until paraplegic. We now provide written advice to patients receiving epidural analgesia, detailing specific signs and symptoms that require medical investigation should they occur after discharge.

Chlorhexidine in alcohol has been our standard solution for skin preparation for many years. We note that in 2004, Cameron et al. reverted from chlorhexidine back to an iodine solution for skin preparation. Since then, their incidence of insertion site infections seems to have increased. We wonder whether they think this increase is significant enough to justify returning to an alcoholic chlorhexidine solution.

Common with most of the published data on epidural infections, the implicated organism in their patients was *Staphylococcus aureus*, which was cultured from five of the nine patients with infective complications. Unfortunately, routine methicillin-resistant *S. aureus* screening was not performed during our survey. We were thus unable to investigate whether methicillin-resistant *S. aureus* colonization of the patient, the staff, or the ward predisposed to these complications. We would now, however, be reluctant to insert an epidural catheter in any patient colonized with methicillin-resistant *S. aureus*.

Cameron et al. discovered that epidural abscesses were associated with epidural site infection that in turn was related to the duration of epidural catheterization and abdominal or thoracic surgery (odds ratio, 3.3). Interestingly, in our series, insertion of an epidural at a thoracic level did not increase the risk of developing either an epidural abscess or meningitis (odds ratio, 1.18; 95% confidence interval, 0.3–4.7).

Perhaps, as they suggest, it is the duration of the epidural infusion that is the more critical factor. We recommend that epidurals are removed within 3 days of insertion.

Although Cameron et al. identified two spinal hematomas, only one was epidural. We would be interested to know where the other hematoma was located in relation to the dura and how it presented (i.e., whether it affected patient 1 or patient 3).

When we presented the results of our survey, both locally and nationally, the frequency with which these complications occur came as a surprise to many. It is reassuring that on the other side of the globe, the incidence seems to be similar. Only through continued audit of our practice can we determine the true risk of the interventions we undertake to benefit rather than harm our patients. The Royal College of Anaesthetists has recently started a national audit of these complications in the United Kingdom, and we would commend to others to consider a similar register.

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In Reply.—We thank Drs. McCabe and Christie for their letter, which describes adverse neuraxial outcomes from their own clinical practice.† published in an article almost simultaneously with our own report.‡ It is indeed reassuring to find that most of their conclusions are consistent with our experience, and overall we hope this will heighten awareness of the insidious problem of epidural space infections and help to avoid long-term adverse outcomes.

In their article, they describe three cases of meningitis as separate adverse outcomes after analgesic epidural infusions, all being diagnosed within 5 days of catheter removal. We did not report any discrete episodes of meningitis, and on searching our database specifically for the term, we have not discovered any such events. It would seem that meningitis *per se* after epidural analgesia is an uncommon event.

The reason for our change to alcoholic Betadine solution for skin preparation was because we found it difficult to see the light pink alcoholic chlorhexidine on the skin, to ensure full coverage of the area. The apparent increase in incidence of site infections subsequent to this is being monitored and will be reviewed at the end of this year. Clinical recommendations tend to favor alcoholic chlorhexidine skin preparation,§ and if our trend is not simply an aberration, we will adopt a return to alcoholic chlorhexidine.

The second spinal hematoma was an intrathecal bleed in patient 3. An L2–L3 epidural was placed for analgesia after removal of a femoral nail and insertion of a hip screw. A “bloody tap” on the first pass complicated insertion of the epidural, which was restated and managed effectively for 2 days. The catheter was dislodged and subsequently removed after 1 h after 5,000 U dalteparin, and the patient subsequently developed neck stiffness and nausea. Magnetic resonance imaging of the spine revealed intrathecal blood from L1 to L3. The patient’s symptoms resolved rapidly with no specific management.

We support the recommendations of Drs. McCabe and Christie for auditing these complications. As part of a statewide government initiative commissioned by the Victorian Quality Council, Department of Human Services (Melbourne, Victoria, Australia), we have developed a toolkit for monitoring and reporting adverse neuraxial outcomes after epidural analgesia as well as other adverse events such as respiratory depression necessitating naloxone treatment associated with systemic opioids. This is being implemented throughout the state of Victoria and is hoped to lead to a more accurate and balanced understanding of the incidence of all serious adverse outcomes relating to acute pain management.

Finally, the provision by Drs. McCabe and Christie of written advice at discharge to patients who have had an epidural is an initiative to be commended.


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(Received for publication August 20, 2007.)
To the Editor—Although I agree that the case “Etomidate-induced Pacemaker-mediated Ventricular Tachycardia” could represent an important contribution to our literature, I am concerned that the data presented in this article cannot justify the conclusion that myoclonus from the etomidate was actually responsible for this pacemaker-driven tachycardia (this is not actually an arrhythmia). Therefore, this article might be cited by future practitioners when, in fact, etomidate likely made little or no contribution to the ventricular pacing shown at 140 beats/min.

The rate-responsive mechanism in this Medtronic Prodigy pacemaker (St. Paul, MN) is a piezo crystal attached to the case of the pacemaker. It is activated by case deformation (pressing on the case). The rate-responsive algorithm has a number of programmable settings that determine how the pacemaker will change the pacing rate (called activity-indicated rate) due to stimulation of this crystal. These settings include the following (not specified in the article): lower pacing rate, upper activity rate, activity threshold, activity rate response, acceleration time, and deceleration time. In the default setting, the acceleration time is 0.5 min, so in 30 s, the pacemaker will achieve 90% of the difference between the current paced rate and the higher (calculated) activity-indicated rate consistent with the activity level.

The default deceleration time is 5 min and is of interest here. Upon abrupt cessation of activity, the pacing rate begins decreasing almost immediately, and it will decrease over 5 min by 90% from the current (high) paced rate to the new, lower activity-induced rate. In this case, assuming that the lower pacing and upper activity rates had been programmed to 70 and 140 beats/min, respectively, the actual pacing rate would have been approximately 122 beats/min at 30 s, 109 beats/min at 1 min, and 92 beats/min at 2 min (fig. 1). No time intervals were reported in the article, but 30 or more seconds likely elapsed from the last patient movement to the reprogramming of the pacemaker.

Therefore, I believe that the continued pacing at 140 beats/min suggests that something else was affecting the pacemaker. It might have been the weight of the programming head. In fact, the Prodigy manual warns: “Clinical studies of activity rate responsive pacemakers have reported instances of ... an increase in the pacing rate up to the programmed Upper Activity Rate ... by pressing the programmer head over the pacemaker.”

The stability of the heart rate (determined by the intervals between the QRS complexes) in the author’s published figure 2 suggests that the pacemaker was sensing continuous mechanical activity. If the mechanical activity had ceased, the pacing rate would have been slowing, which would result in the successive lengthening of the intervals between the QRS complexes.

This article is an important reminder that pacemakers (and implanted cardioverter-defibrillators) are mere computers that respond to electrical and environmental signals in a predictable fashion. The authors should have emphasized that these devices can produce unusual pacing behavior that might be misinterpreted by clinicians (likely true in this case) and, therefore, induce inappropriate treatment, which did not happen here. For example, pressure on the chest over a pacemaker or defibrillator due to instruments or surgical personnel, as well as mechanical activity over a pacemaker or defibrillator from surgical site preparation, can lead to a paced tachycardia approaching the upper activity rate. In fact, misinterpretation of pacemaker (pseudofunction) behavior is common, and sometimes it leads to disastrous results with patient injury.

Finally, the comment in this article that “It is essential that the pacemaker should be programmed to an asynchronous mode” is not justified by any part of the case presentation, the discussion, any literature, or any manufacturer recommendation. Pacemaker and implantable cardioverter-defibrillator manufacturers do recommend a comprehensive interrogation of any device that has “seen” an external cardioversion or defibrillation.

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In Reply:—We thank Dr. Rozner for his interest in our case report, “Etomidate-induced Pacemaker-mediated Ventricular Tachycardia,” and we appreciate the opportunity to address some of the points he raises in his letter.

He first questions our use of the term pacemaker-mediated ventricular tachycardia, asserting that the rhythm we reported is not actually an arrhythmia. We agree that the rhythm we observed was indeed not a clinical arrhythmia. Perhaps a more accurate description might be his term, abnormal pacemaker-driven tachycardia.

Dr. Rozner then refers to the programmable settings that affect the rate-responsive function. In our patient, the lower pacing rate and upper activity rates were 70 and 160 beats/min, activity threshold was medium, acceleration time was 0.5 min, and deceleration time was 5 min. Dr. Rozner’s discussion of the rate-responsive mechanism demonstrates his well-known expertise in the area of implantable cardiac devices, but his calculation using the deceleration time is not relevant to our case. His letter examines the situation where the stimulus activating the rate-responsive function terminates, and the pacing rate thus begins to decrease gradually as he describes. However, we have stated that the rapid rhythm (shown in fig. 2 of our case report) ceased immediately after the rate-responsive function was disabled. That is, the stimulus (myoclonus) did not stop, but instead the pacemaker was reprogrammed to stop responding to the stimulus. Once the rate-response function is disabled, a pacemaker will immediately revert to its baseline settings (VVI at the lower pacing rate of 70 beats/min, in our case) as shown in figure 2.

Therefore, we agree with Dr. Rozner’s statement that “the pacemaker was still sensing activity,” but we maintain that etomidate-induced myoclonus is a much more plausible explanation than the weight of the programming head without any additional pressure being applied. If the programming head were solely responsible for the rapid ventricular pacing (as Dr. Rozner suggests), we would expect to have gotten the same result each time the rate-responsive function was restored, not just the first time.

We have considered other possible causes for tachycardia in this setting, such as erroneous programming of the pacemaker (e.g., if the operator accidentally turned off the mode switch in a patient with atrial fibrillation, the device might pace at the upper activity rate) or inappropriate atrial sensing (e.g., 1:2 sensing of an atrial rate of 280 beats/min). However, we believe that the observed myoclonic activity led to this case of pacemaker-driven tachycardia. This abnormal rate-response is clearly corroborated by the data shown in figure 2. Our hypothesis is also supported by the fact that there were no further episodes of rapid ventricular pacing with reactivation of the rate-responsive function, after waiting to ensure complete cessation of the myoclonus. Persistent myoclonic activity for this amount of time is reasonable; etomidate-induced myoclonus has been reported to last up to 8 min.

Finally, Dr. Rozner takes exception to our recommendation that the pacemaker be set to an asynchronous mode. Although we agree that this statement is not valid for a cardioversion, it remains sound advice for anesthesiologists who take patients to the operating room, where unipolar electrocautery is routinely used, and we apologize for any confusion this generalized statement may have caused.

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The authors thank Robert N. Goldstein, M.D. (Assistant Professor of Medicine, Director, Cardiac Device Clinic, Division of Cardiology/Electrophysiology, University Hospitals Case Medical Center, Cleveland, Ohio), for his assistance in the preparation of the manuscript.

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Potential Hazard Associated with a Laryngoscope Blade

To the Editor—A problem was recently encountered with one of our laryngoscope blades (Classic Mac 3 FO; Heine Optotechnik, Herrsching, Germany) that could have resulted in significant consequences. During cleaning and high-level sterilization of the blade, it was noted that the distal, rounded tip of the blade had become loose to the point where it could easily be detached from the body of the blade (fig. 1). Most anesthesiologists are under the impression that the metal blade is made of one single piece, but in fact the rounded tip is welded onto the blade.

This defective blade was produced in February 2003 and has been used at our hospital on a regular basis since. However, it is not known how many times this particular blade has been used. It is difficult to determine whether the defect resulted from a manufacturing problem or from rough handling (e.g., dropping of the blade to the floor). The finding was reported to Heine Canada, Health Canada, and the Federal Drug Administration, and none of them is aware of a similar case.

Most anesthesiologists are under the impression that the metal blade tip are remarkably sharp and rugged. In the case where the tip would still be attached to the blade upon insertion of the laryngoscope blade into the hypopharynx, the small metal tip could be dislodged during surgical manipulation of the larynx.

Fig. 1. The loose tip of the laryngoscope blade (partially reattached for illustration purposes) as noticed after cleaning and sterilization.

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To the Editor:—We have lead to severe mucosal trauma because the edges of the unprotected blade tip are remarkably sharp and rugged. In the case where the tip would still be attached to the blade upon insertion of the laryngoscope blade into the hypopharynx, the small metal tip could be dislodged during surgical manipulation of the larynx.

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intubation and projected into the tracheobronchial tree, requiring rigid bronchoscopy for removal (if detected immediately) or leading to a significant range of complications, such as segmental atelectasis, obstructive emphysema, pneumonia, or even perforation.

After the detection of the problem, all blades at our institution were inspected, but none was found to have the same defect. Although this seems to be a rare finding, it is recommended to check the integrity of the laryngoscope blade on a regular basis. A quick inspection with manual pulling on the tip may help to prevent serious complications.

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CORRESPONDENCE

In Reply.—We were most concerned to learn of the issue with the Fiber Optic Laryngoscope blade at the Hospital for Sick Children in Toronto. Both Mr. Matthews and Dr. Luginbuehl were very helpful in bringing the information to our attention and coordinating matters to ensure proper findings.

It is important to note that the blade was sent to us for further examination. We tested the blade completely and examined it completely through a high-power microscope. We were not able to determine exactly why the blade tip became separated; however, we believe that there may not have been sufficient soldering to hold the tip in place if the blade were to receive a shock of some kind. We can only speculate due to the fact that soldering matter could have fallen out after the tip separated.

It is important to note that Heine Optotechnik (Herrsching, Germany) has been manufacturing this product since 1983. Since that time, we have manufactured several million units. This is the first and only reported incident worldwide. We have investigated whether any
In Reply.—As described in the letter, after initial communication from Massachusetts General Hospital (MGH) on this topic, we provided MGH with a software update that allows the user to select either SmarTone (Masimo Corporation, Irvine, CA), which uses Masimo SET’s sophisticated signal processing to enable saturation tones even during low signal-to-noise conditions, or a more traditional pulse tone algorithm, which uses signal morphology only, causing breaks in the tone in the presence of low perfusion or signal interference. We have been offering SmarTone as a user-selectable feature on our monitors for some time now. However, we believe it is important that users understand the value of SmarTone so that they can decide which pulse tone to choose for their particular needs.

The American Society of Anesthesiologists standard that Mr. Forde et al. at MGH cite as being diminished by the Masimo SmarTone algorithm is exactly the standard that many other clinicians believe we have enhanced. This American Society of Anesthesiologists standard was established in the context of oxygenation, which implies that tracking the pitch of the tone and thereby indicating changing saturation values was the true intent of the standard. With this understanding and the knowledge that interruptions in the pulse tone caused by low perfusion, motion, and electrical or other interference are common, causing frequent “false alarms” and decreasing the amount of time that the tone was available, we concluded that it would be of clinical value to decrease these interruptions in the pulse tone.

The pulse tone feature on many pulse oximeters is related to the morphology of the plethysmographic waveform, emitting a tone only when a clean pulse signal is recognized. In the presence of low signal-to-noise conditions, the arterial pulse waveform can be virtually impossible to distinguish when looking at the raw plethysmograph. In these instances, most other pulse oximeters either discontinue the tone until a valid pulse signal is recognized or sound a tone based on the noise frequency, which is not indicative of the patient’s pulse. This leads to frequent, long periods without audible information on oxygenation status or false low-saturation indications.

Masimo SET’s unique signal processing algorithms, which include five distinct signal processing engines working in parallel, enable identification of the arterial pulse wave under far more of these difficult clinical conditions. This allows Masimo to provide a variable pitch saturation tone during periods of signal disturbance that would cause loss of signal or false saturation tones in conventional pulse oximeters. The SmarTone feature uses real-time signal morphology, similar to conventional pulse oximetry, to create the tone during periods in which the pulse can be clearly recognized. During periods wherein the pulse signal becomes obscured, Masimo uses its advanced signal processing algorithms to identify the pulse and provides a tone indicative of true oxygen saturation. Feedback from current and prospective customers has been overwhelmingly positive toward this feature.

The case reported in the MGH letter focuses narrowly on one aspect of the SmarTone feature. Because of the sophisticated signal processing involved, during periods of signal disturbance or low perfusion there can be up to an 8-s delay before the cessation of the pulse tone. This short delay is well within internationally recognized performance requirements for heart rate meter response to asystolic events.*

The letter makes reference to a bradycardic period preceding the asystole, during which the pulse tone continued. It is not clear that the surgical team was able to observe whether the pace of the tone was consistent with the rate of the bradycardia. It is likely that the Masimo device tracked the bradycardia, as evidenced by several independent and objective studies that have demonstrated the superior ability of Masimo SET to track sudden changes in heart rate.†‡§

The letter also suggests that a simulation using a blood pressure cuff reproduces this clinical scenario. Although these two scenarios may seem similar, they are actually very different because in one case the heart has stopped, whereas in the other a pressure cuff is occluding the flow from a beating heart. Our own pressure cuff testing, which is supported by the literature, shows that it is very difficult to occlude 100% of the blood pulsations on certain patients. Because Masimo SET has been shown to have superior low perfusion performance, it may be the only pulse oximeter to continue to read during cuff inflation when weak arterial pulsations are present. We recently performed more than 100 cuff inflations on various subjects, using a dual bladder tourniquet inflated to 250 mmHg with a Masimo Radical pulse oximeter and a Nellcor N-600 (Nellcor-Covidien, Mansfield, MA) attached to the test arm. In every case, the Radical would display a reading during cuff inflation only if there was a visible arterial pulse wave in the plethysmograph, and it would zero out after the plethysmograph became flat. Overall, the results were inconsistent, with no clear pattern of either monitor (Nellcor or Masimo) zeroing out before the other, and neither monitor consistently zeroed out within 8 s of cuff inflation. We have captured a number of these cases on video and are pleased to share these results upon request.

As discussed above, after initial communication from MGH on this topic, we provided MGH with a software update that allows the user to select either SmarTone or a more traditional pulse tone algorithm. Initial feedback regarding this new software has been positive. Since early this year, SmarTone has been offered as a user-selectable feature on all of our products, allowing clinicians to decide for themselves which pulse tone to use, based on their clinical scenario.

We value the feedback and suggestions of our customers in helping us make our products the safest and best pulse oximeters in the world, and we appreciate the useful comments of the MGH team. We trust that giving clinicians the option of choosing whether to activate the SmarTone feature will provide them greater flexibility in making informed decisions regarding the clinical management of their patients.

§ Correspondence and comments can be sent to Ryan E. R. Forde, B.S., Frederic M. DeBros, M.D., Emily L. Guimarães, M.D., Warren S. Sandberg, M.D., Ph.D., Massachusetts General Hospital, Boston, Massachusetts. rforde1@partners.org

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