

Letters to the Editor

Evaluating a Patient's Airway

To the Editor:

The article by Khan et al. (1) attempts to give us a new way of evaluating a patient's airway that is both clinically useful to the practitioner as well as descriptively accurate in communicating the most about the airway to others with the least explanation, a great help to both clinician and researcher. A quick bite of the upper lip certainly is easy to perform in the cooperative patient and the results readily communicated. In trying their method, I quickly found a potential problem not addressed in their paper, the edentulous patient and one with oversized caps. I think it would be better to view this bedside test as an adjunct to the clinical examination of the entire airway. I do not believe it can replace the Mallampati Classification, as there are sufficient numbers of patients that would be disqualified for evaluation based on their lack of dentition.

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Reference

1. Khan ZH, Kashfi A, Ebrahimkhani B. A comparison of the Upper Lip Bite Test (a simple new technique) with the Mallampati Classification in predicting difficulty in endotracheal intubation: a prospective blinded study. *Anesth Analg* 2003;96:595-9.

DOI: 10.1213/01.ANE.0000074657.05733.30

In Response:

While the Mallampati Classification (1) which has prevailed as the only single and reliable test in predicting difficult airway for the past two decades is important, we believe that the Upper Lip Bite Test (ULBT) (2) is undoubtedly a distinct improvement.

Dr. Herschman's question of edentulous patients whom our new technique—the ULBT—did not encompass is correct. We excluded the edentulous patients from our study, as appears in the "Methods" section. We may add that the test was not designed to incorporate those devoid of dentition because of the inherent difficulty of this class of population to take a bite—which of course is the cardinal and standing feature of the ULBT.

In our approach to the edentulous patients who form a significant proportion of our patients in today's statistics, the principal author and innovator (Z.H.K.) of ULBT has further introduced his next new technique, the Upper Lip Catch Test (ULCT) in predicting difficulty in endotracheal intubation in edentulous patients to accommodate the aforementioned class for airway assessment. In conducting the ULCT in edentulous patients, we propose the following criteria for classification:

Class Zero (0): the lower lip gliding or rolling over the upper lip reaching as high as the rhinion or else positioning itself at any point above midway between the vermilion line and the rhinion (Figs. 1A and 2A).

Class I: the lower lip catching the upper lip, between a point 2 mm above the vermilion line and the point midway between the vermilion line and the rhinion (Figs. 1B and 2B).

Class II: the lower lip catches the upper lip at the level of the vermilion line or positioning itself just above it (2 mm) (Figs. 1C and 2C).

Class III: the lower lip just caresses the upper lip but falls short of obliterating the vermilion line (Figs. 1D and 2D).

Like our earlier test, the ULBT, the ULCT is easy to demonstrate and easy to follow. We have conducted a pilot study on a limited number of patients and intend to continue it on a large sample of cases. We hopefully and emphatically claim that the ULCT for all its

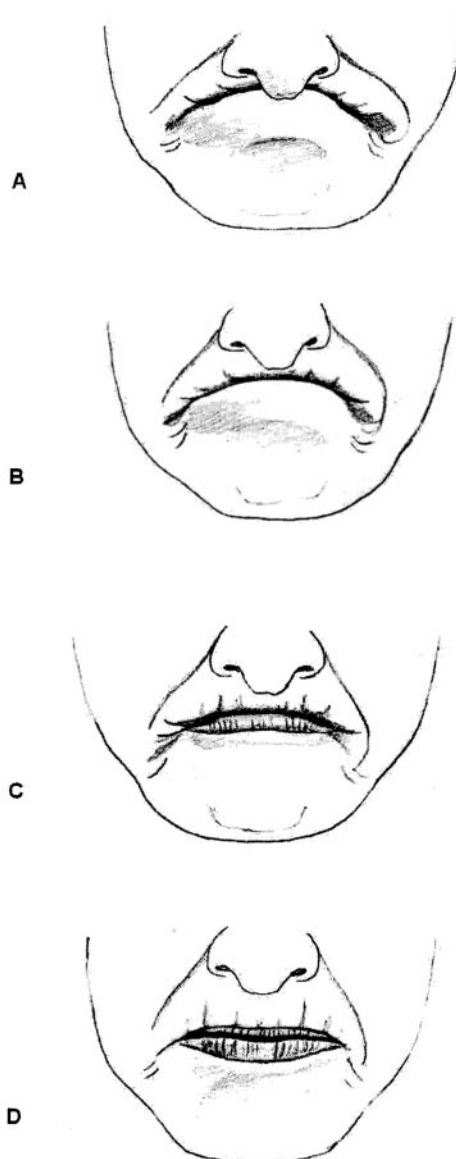


Figure 1. Schematic frontal view of ULCT. A, Class 0; the lower lip rolling over the upper lip advancing as high as the rhinion or else positioning itself at any point above midway between the vermilion line and the rhinion. Note that the lower lip rolling over the upper lip exhibiting wrinkles and folds of senility, and the area above the chin contracted giving a goose pimple appearance. B, Class I; the lower lip catching the upper lip, between a point 2 mm above the vermilion line and the point midway between the vermilion line and the rhinion. C, Class II; the lower lip catching the upper lip at the level of the vermilion line or positioning itself just above it (2 mm). D, Class III; the lower lip just caresses the upper lip unable to make further advancement and falls short of vanishing the vermilion line.

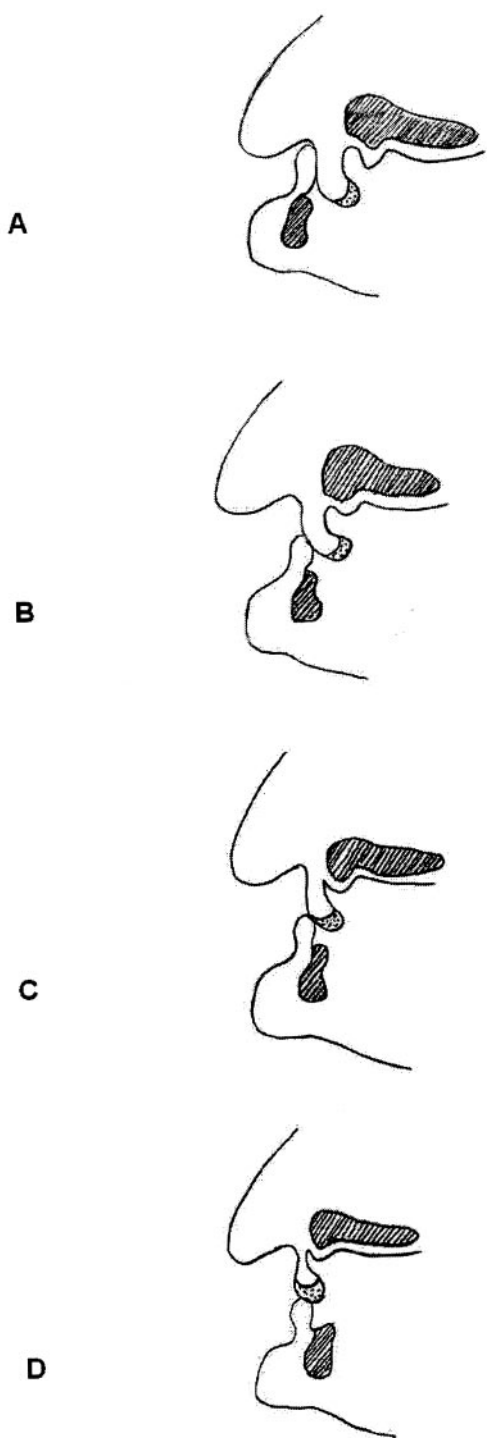


Figure 2. Schematic lateral view of ULCT. Dotted area depicts the mucosa of the upper lip. A, Class 0; the lower lip rolling over the upper lip advancing as high as the rhinion or else stationing itself at any point above midway between the vermilion line and the rhinion. B, Class I; the lower lip catching the upper lip, between a point 2 mm above the vermilion line and the point midway between the vermilion line and the rhinion. C, Class II; the lower lip catching the upper lip at the level of the vermilion line or positioning itself just above it (2 mm). D, Class III; the lower lip just caressing the upper lip but fails to obliterate the vermilion line.

subtlety would attest to the validity of our earlier perceptions and help in providing an umbrella for the edentulous patients.

It might be argued that this technique might impart stress on this senescent population, but we are of the opinion and the conviction that its inherent and potential benefits are beyond proportions, making its use not only prudent, but also lifesaving for those unidentified cases of difficult intubation who lack dentition.

Patients with oversized caps on the other hand can be tested and assessed with our ULBT, and we feel that the ULBT has the inherent potential to address this issue.

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Use of Term "Patient-Controlled" May Be Confusing in Study of Elastomeric Pump

To the Editor:

We read with interest the recent technical communication by Capdevila et al. titled, "Patient-Controlled Perineural Analgesia After Ambulatory Orthopedic Surgery: A Comparison of Electronic Versus Elastomeric Pumps" (1). The phrase "patient-controlled" has historically been applied to medication delivery modes in which a patient may give a bolus of medication *without* the intervention of a health care provider. For example, a nurse drawing up additional local anesthetic in a syringe and injecting a labor epidural for breakthrough pain has not been described as "patient-controlled," as the patient is dependent on the provider. The same is true for intravenous opioid patient-controlled analgesia (PCA): a patient having to request a nurse to draw-up and then administer the medication has not been described as utilizing a PCA, or else there would be no difference between standard administration and "patient-controlled."

With this in mind, we are confused by the description of the technique used in this study for the elastomeric pump as being patient-controlled. The methods section explains, "In Group 1 [using an elastomeric pump], the patient or the nurse injected the bolus by means of a syringe connected to the catheter by a three-way tap." However, the discussion section concludes, "Our results show that continuous infusion of ropivacaine by means of LV5[®] disposable elastomeric pumps associated with bolus injections of the same drug *by a nurse* is a simple technique for patients in their home [emphasis added]." Since the LV5[®] has a flow-rate restrictor of 5 mL/h which cannot be bypassed, to give a local anesthetic bolus requires drawing up medication and injecting it *via* a syringe and stopcock. Our questions to the authors are: 1) Did the authors send patients home with syringes, needles, and ropivacaine vials, and ask them to draw up the medication and inject it themselves in a sterile fashion as suggested in their methods section? 2) If so, do they suggest this should become a standard practice? And 3) if not, do the authors believe the term "patient-controlled" should be expanded to include a visiting nurse administering a patient-requested catheter bolus to an ambulatory patient?

We do not believe that is a trivial matter of semantics. We have studied in the laboratory (2,3) and clinically used various elastomeric (4) and electronic (5,6) infusion pumps. We would agree that elastomeric pumps are often easier to use and less intimidating to patients than electronic pumps. However, to our knowledge, only one nonelectronic pump is capable of true patient-controlled boluses (4), and then only with a maximum bolus dose of 2 mL every 15 min. There is evidence that patient-controlled local anesthetic infusion is superior to the continuous infusions which nearly all elastomeric pumps provide

(7,8), and this is the main reason we continue to utilize electronic pumps for ambulatory perineural infusions in our clinical practice. At least in the health care environment of the United States at this time, it is simply not feasible to have a visiting health care nurse visit patients at their homes when a bolus injection is required, and therefore electronic pumps are often preferable to their less complex counterparts. It is for these reasons that we believe the recent title, “Patient-Controlled Perineural Analgesia After Ambulatory. . . . [emphasis added]” may be confusing and is important to clarify to readers.

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DOI: 10.1213/01.ANE.0000074521.79078.25

In Response:

We would like to thank Ilfeld et al. for their interest in our work (1) and for the pertinent and important comments in their letter to the editor.

We are entirely in agreement with Ilfeld et al. when they state that, strictly speaking, the technique used by the patients of the

“disposable pump” group cannot be considered patient-controlled administration. To uphold the established protocol, we discharged patients with the possibility of performing self injections of 0.2% ropivacaine after training in our unit, or with the help of the home care nurses who saw the patients three times daily.

Obviously, this is not standard practice and this amount of individual care cannot generally be provided for all ambulatory patients. Our study was intended to evaluate home use of elastomeric pumps with possibility of self-administration, and to compare them with electronic infusion pumps.

Ilfeld et al. found that “a patient-controlled local anesthetic infusion is superior to the continuous infusion that nearly all elastomeric pumps provide (2,3).”

This conclusion should, however, be tempered by the fact that, in the studies cited, only continuous infusion with concomitant PCA capacity was superior to continuous infusion. Furthermore, in the studies reported by Ilfeld et al. (4,5) only 2 or 3 patients out of 30 actually used the PCA function of their electronic infusion pump, with no evidence that the postoperative analgesia of these 2 or 3 patients was superior to that of the patients who did not use the PCA function.

We are currently using multiple rate disposable pumps with a bypass PCA chamber of 5 mL (Fig. 1) for our patients after ambulatory orthopedic surgery. This new protocol corresponds to a veritable “patient-controlled” regional analgesia.

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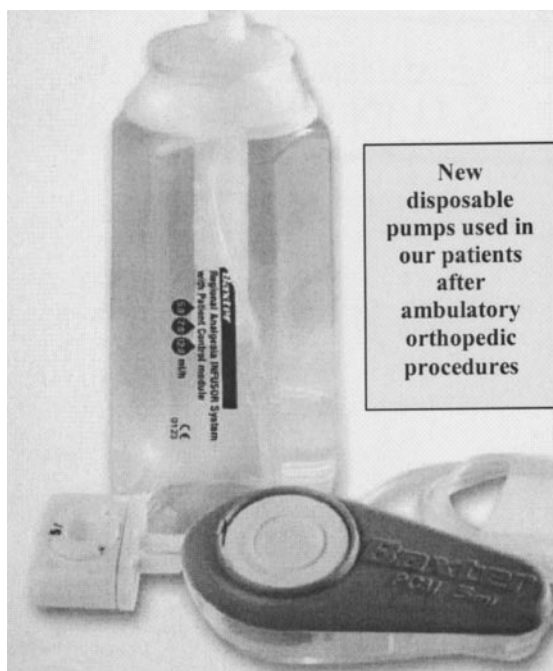


Figure 1. Multiple rate disposable pump with a bypass PCA chamber of 5 mL.

Curved-End Guidewire for Central Venous Cannulation in Neonate

To the Editor:

Central venous cannulation is routinely performed in pediatric cardiovascular anesthesia. The internal jugular vein (IJV) is the preferred access site for this procedure and has a reasonably infrequent incidence of complications. The majority of the catheters are placed via the Seldinger technique that includes a guidewire with a J-shaped tip.

Because of a smaller target vein diameter and the variable position of the vein to the artery, catheterization of infants and children is more technically challenging than when performed in adults (1). Several studies have demonstrated improved rate of successful cannulation using an ultrasound-guided technique (2–4). While the ultrasound has great advantage in identification of the local anatomy, our problems with cannulation of small target veins in neonates and infants have persisted despite the use of the ultrasound

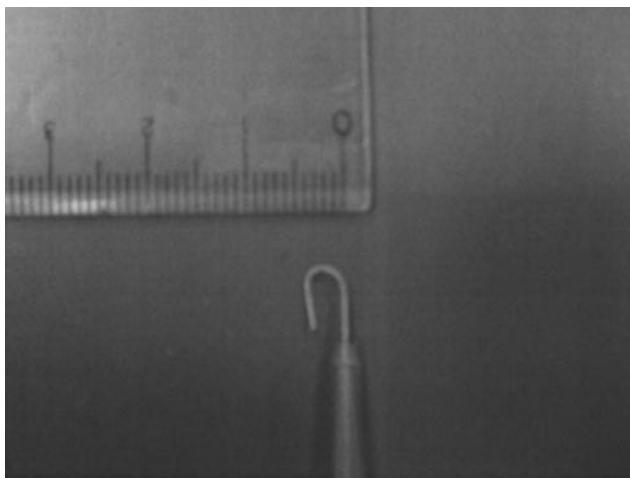


Figure 1. Curved end of the guidewire provided in the kits of a 4F, 8-cm double-lumen catheter (Arrow International, Reading, PA).

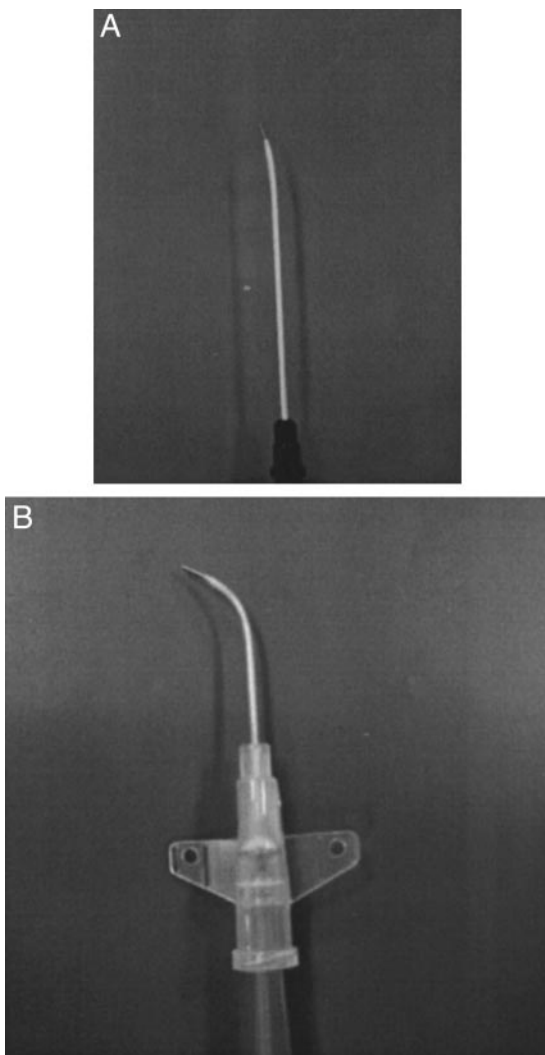


Figure 2. Distal end of the catheter in which the guidewire is inserted. (A) 22-gauge, 44.5-mm introducer catheter (Arrow International). (B) 24-gauge, 19-mm needle-catheter assembly (Insyte-W, Becton Dickinson, Sandy, UT).

device. Specifically, we encountered difficulties in advancing the J-shaped tip guidewire through the needle into the vein.

The radius of the curvature of the wire's J-shaped tip is often identical to or larger than the diameter of the neonate vessel. For example, the guidewire provided in the kits of a 4F, 8-cm double-lumen catheter (Arrow International, Reading, PA), has a curved-end radius approximately 5 mm (Fig. 1). In our own study, small diameters of the IJV (5 mm) were mainly seen in infants whose body weight was <5 kg (5). Therefore, the guidewire architecture is not well suited to neonate vein size.

The properties of the needle also affect the success rate of the procedure. A sharp, short and small-gauged needle has advantage of easy handling and lower incidence of hematoma. The IJV can usually be accessed within a depth of 20 mm from skin in most of the infants (5-7). Thus, we used a 24-gauge, 19-mm needle-catheter assembly (Insyte-W, Becton Dickinson, Sandy, UT) for central venous cannulation rather than the introducer catheter provided in the kit. However, this smaller-gauged introducer catheter is more compliant and bends excessively with J-shaped tip guidewire insertion (Fig. 2). This distortion of the introducer catheter may lead to misplacement of the wire into the extravascular space. Advancement of the introducer catheter completely into the vein or the use of a flexible straight end wire would prevent this problem.

In conclusion, small-diameter introducer catheter distortion by curved-end guidewire is a significant problem that affects the success of central venous cannulation in infants and neonates, so clinicians should pay particular attention to this phenomenon.

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DOI: 10.1213/01.ANE.0000074652.28044.EF

Postoperative Epidural Analgesia: How About Quality Assessment?

To the Editor:

We read with interest the article by Kampe et al. (1) on the current practice of postoperative epidural analgesia in Germany. As this survey was conducted about 1 year after another German survey that included 446 responding hospitals, a comparison of the data might be of interest (2,3).

The availability of a 24-h acute pain services (APS) in German hospitals was 36% in the 1999 investigation and 41% in the 2000 survey. Kampe et al. emphasized that there is no generally accepted definition of an APS, and this is in accordance with previous investigations (4,5). However, several national guidelines for postoperative pain management provide recommendations and indicate the main components of an APS. The 1999 survey revealed that only half of the German APS fulfilled five basic quality criteria, such as designated personnel for pain treatment, organization of patients' care at nights and weekends, written protocols for pain management, and regular

assessment and documentation of pain scores. These data question the quality of pain services in general, not only in Germany, but also in other countries, since former surveys revealed that quality criteria have not been emphasized up to now. For example, the statement that epidural analgesia (EA) is performed for postoperative pain management does not assume that the quality of care is high. Similarly, continuous epidural infusion or availability of PCEA does not provide any information about quality, e.g., pain scores, patients' satisfaction, nurses' view, and the attitude of the surgeons. PCEA was available in about 20% of the departments in 1999 and 2000. However, we should question this impressive percentage in light of the fact that the majority of the departments do not comply with some basic quality criteria of pain management such as regular evaluation of pain scores and designated personnel for pain management (2,3). Is EA also performed on general wards or restricted to intensive or intermediate care units? What about the frequency of adverse effects and severe complications, outcome data, and cost-effectiveness? The information that EA is used in 40.9% of the patients scheduled for cardiothoracic surgery is amazing and unexpected. However, is patients' care improved by these frequent rates of postoperative EA? Furthermore, peripheral regional techniques (e.g., sciatic, psoas compartment block) increase in popularity, especially in trauma and orthopedic patients. Those techniques are less invasive and do not bear the potentially severe complications as neuroaxial blocks do. So, we have to ask whether a figure of 17.2% of the orthopedic patients receiving EA as reported by Kampe et al. is good or bad news?

The implementation of standards of postoperative pain management, organizational structures (e.g., APS in larger institutions) and quality assurance measures have to be emphasized. Rawal (6) characterized the actual situation of APS in terse words: "good from far, far from good."

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Dr. Kampe does not wish to respond.

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DOI: 10.1213/01.ANE.0000074661.05733.59

Meta-Analysis Demonstrates Statistically Significant Reduction in Postoperative Myocardial Infarction with the Use of Thoracic Epidural Analgesia

To the Editor:

We read with interest the secondary analysis of the MASTER trial in which the authors found no significant improvement in major morbidity or mortality after major abdominal surgery from perioperative epidural analgesia (1). The authors comment that their results are at odds with the results of previous meta-analyses (2-4).

We wish to clarify some questions raised by Peyton et al. regarding our meta-analysis (2). All patients, including those who died in the first 24 h after surgery, were included in our analysis. Funnel plotting suggested an absence of publication bias. Furthermore, we examined the effect of known comorbidities on the outcome. Thoracic epidural analgesia reduced postoperative myocardial infarction.

Comparison: 01 Postoperative Epidural Analgesia Versus Postoperative Systemic Opioid Analgesia

Outcome: 01 Postoperative Myocardial Infarction

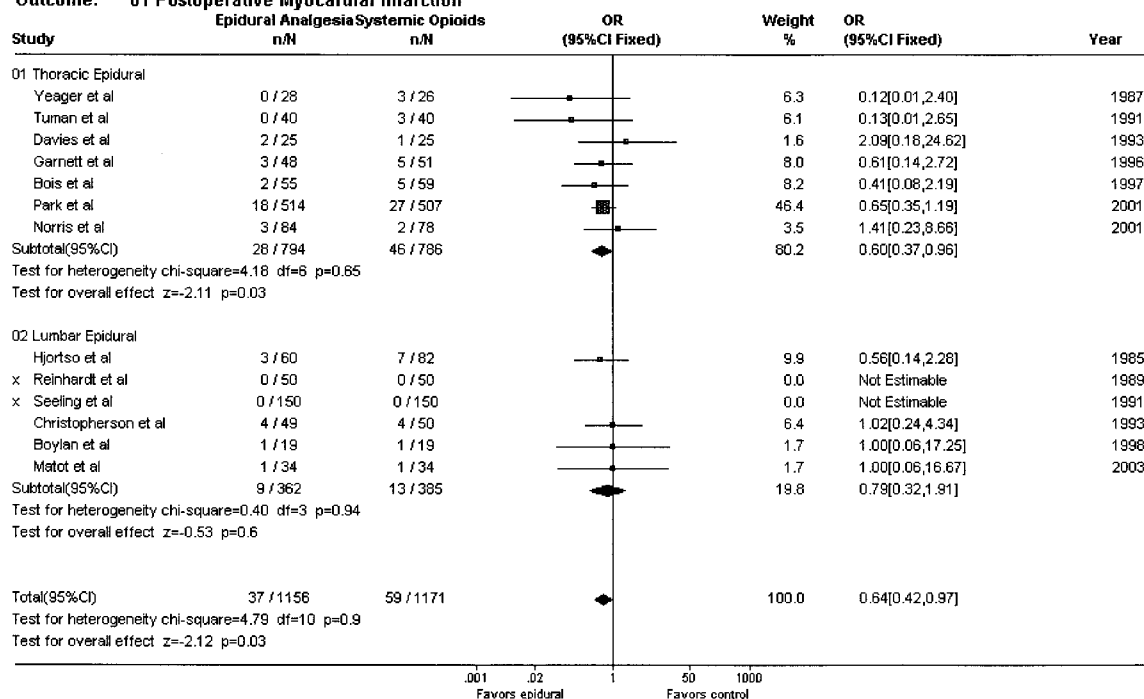


Figure 1. The effect of postoperative epidural analgesia on postoperative myocardial infarction. CI = confidence interval, OR = odds ratio.

tion (MI) despite a higher proportion of patients with angina, previous MI, use of beta-blockers, or chronic obstructive pulmonary disease in the group receiving epidural analgesia.

Four randomized controlled trials (RCTs) (5-8), which compared postoperative epidural analgesia with systemic opioid analgesia, were published after our meta-analysis. We have updated our meta-analysis on postoperative MI with the results from three of the RCTs (5-7) in which data were available (Fig. 1). Our meta-analysis continues to demonstrate statistically significant reduction in postoperative MI with the use of thoracic epidural analgesia (odds ratio 0.60; 95% confidence interval 0.37, 0.96; $P = 0.03$).

The MASTER Trial was powered to detect an absolute risk reduction of 10% (or a relative risk reduction of 20%) with a control event rate of 50%, a type I error rate of 5%, and a power of only 80% for the combined outcome of mortality and major postoperative morbidity (8). Unfortunately, with the exception of the outcome "at least one morbid end point," the rates of all postoperative complications were less than 50% in the control group (1). The MASTER Trial appears underpowered to detect clinically significant differences in important postoperative outcomes. For example, to detect a 20% relative risk reduction (or increase) in cardiovascular events with 5% type I error rate, 80% power, and the control event rate seen in the MASTER Trial (24%), a sample size of 2,388 subjects is needed.

Peyton et al. have pointed out that large RCTs may reach different conclusions from meta-analyses of small RCTs addressing the same clinical question. In such instances, the estimate of treatment effect from the large RCT would be more accurate than the estimate from the meta-analysis. We agree with the authors' concerns with the limitations of meta-analysis. However, with regards to specific postoperative complications such as pneumonia or MI, the definitive answer to the question of benefit (or harm) of perioperative epidural analgesia remains to be found. A much larger RCT is needed.

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DOI: 10.1213/01.ANE.0000074660.05733.10

Bronchospasm Due to Malpositioned Esophageal Temperature Probe

To the Editor:

We present an interesting complication of bronchospasm caused by a malpositioned esophageal temperature probe. The patient is a 42-yr-old, 87-kg man who presented for a single-level lumbar microdiscectomy. After atraumatic intubation with an 8.0-mm endotracheal tube, a 12-gauge esophageal temperature probe was inserted easily and without resistance. After placing the patient in the knee-chest prone position, breath sounds were bilateral and equal, without any leak. Surgery proceeded uneventfully for 1 h, with the patient being ventilated with a tidal volume of 900 cm³ and peak inspiratory pressures of 20-21 cm H₂O. Temperature ranged from 36.0-36.2°C.

At the conclusion of surgery, the patient was returned to the supine position, at which time we unsuccessfully attempted to remove the temperature probe. This precipitated severe bronchospasm, which was treated with albuterol, intravenous lidocaine, and deepening of the anesthetic with sevoflurane. The second emergence was without bronchospasm; however, it still was not possible to remove the temperature probe. The patient was extubated removing the endotracheal tube and the esophageal temperature probe as one unit; however, some force was required to remove the tube. Examination of the endotracheal tube revealed that the esophageal temperature probe was in a complete knot superior to the cuff.

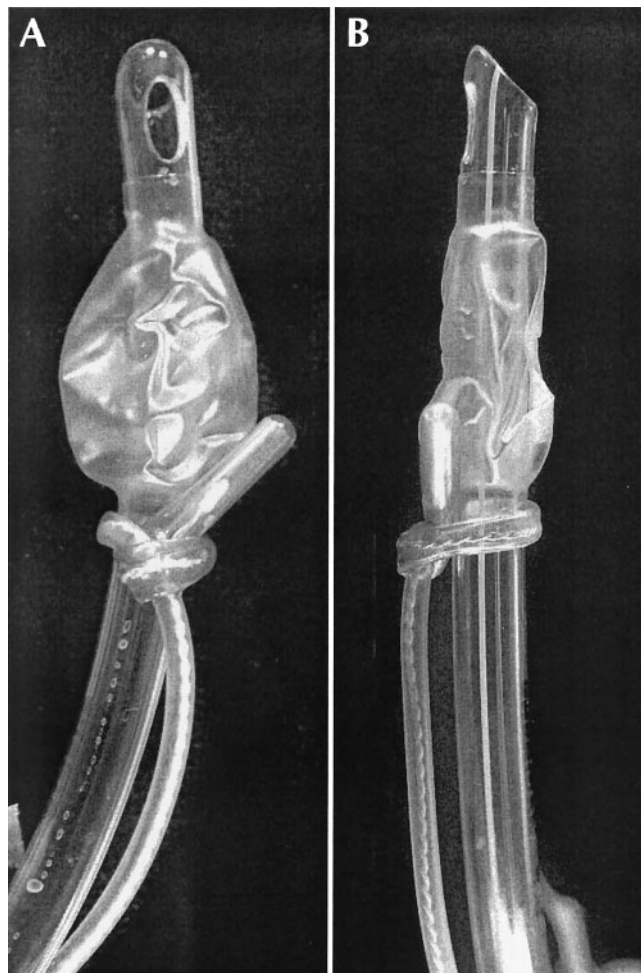


Figure 1. Endotracheal tube showing esophageal temperature probe in a complete knot superior to the cuff.

(Fig. 1). After extubation, the patient was quite hoarse. He underwent fiberoptic laryngoscopy by an otorhinolaryngologist and was found to have no damage to the vocal cord structures and normal vocal cord motion.

There was no evidence of airway leak or unexpectedly low temperature reading to indicate that the temperature probe had been placed in the trachea. We believe that our attempt to remove the temperature probe had caused the knot to tighten, irritating the trachea and precipitating bronchospasm.

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DOI: 10.1213.01.ane.0000090333.93847.b3

A New Connector Assembly with the Potential to Make Endotracheal Intubation Using the Intubating Laryngeal Mask Airway Faster and Safer

To the Editor:

We have designed a new endotracheal tube connector assembly for the wire-reinforced endotracheal tube that is provided for use with the intubating laryngeal mask airway (ILMA-ETT). On preliminary evaluation under simulated conditions, this connector assembly (provisionally named MAMC-connector-assembly or MCA after the name of our institute) reduced the time required to intubate through an intubating laryngeal mask airway (ILMA), and decreased potentially deleterious events.

The ILMA has been used to manage expected and unexpected difficult airways (1–5) since its first clinical use to “facilitate the passage of endotracheal tube (ETT) while maintaining ventilation during and between intubation attempts” (6). However, a number of potentially harmful events, such as alarming ETT movement and periods of no oxygenation and ventilation, can occur during intubation. These are mostly due to the fact that the ETT connector provided with the ILMA-ETT fits so snugly with it that both detachment of a well-fitted connector and its proper reconnection are clumsy. If fitted gently to start, the connector leaves a nonarmored area that can kink because of the weight of the attached circuit. Also, one or two assistants are required if the ventilation is to continue during most of the intubation process. The MCA circumvents these problems.

The MCA (Fig. 1A) has two portions: the MALE portion is an approximately 3-cm long connector made by slicing off both the ends of a 2-mL syringe barrel after its plunger has been removed. The FEMALE portion is a size-10 straight ETT connector (Portex™).

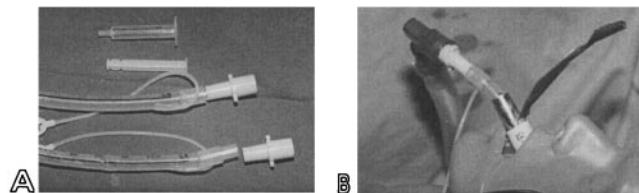


Figure 1. (A) The MALE portion of the MAMC connector assembly (MCA), made out of a 2-mL syringe (top) fits snugly into the intubating laryngeal mask airway-endotracheal tube (bottom); and allows the FEMALE portion of the connector assembly (size-10 endotracheal tube connector) to mount quickly and firmly over it (middle). (B) The MALE portion mounted beforehand on the intubating laryngeal mask airway-endotracheal tube does not leave any nonarmored area where the tube can kink due to the weight of the anesthesia circuit attached to it through the FEMALE portion, even when not supported by an assistant.

Half of the length of the MALE portion is kept affixed into the nonarmored portion of any of the three sizes of the ILMA-ETTs. This does not compromise the ILMA-ETT lumen or its movement through the ILMA. The other half of the MALE portion easily, yet snugly, fits into the tapered patient end of the FEMALE portion, which then connects with the anesthesia circuit (Fig. 1B).

We compared the MCA with the usual connector during intubation through a size-4 ILMA inserted into an intubation trainer (AMBU™). Forty intubations (20 each with ILMA-ETT having either MCA or the usual connector) were carried out in 10 sessions of 2 insertions each by two experienced operators on different days. The time taken for the intubation procedure was significantly less with the MCA than with the standard connector (mean $\pm$ SD, 28 ± 4 s vs 51 ± 9 s, $P < 0.001$; paired t-test), (Mean difference [95% confidence interval]; 23 s [19.6–26.4 s]). ETT kinking, ETT movement, need to disconnect the anesthesia circuit from the connector during its reattachment to the ETT, and need for dry gauze pieces during connector reattachment occurred significantly more often during intubations using the usual connector ($P < 0.001$ for all; Fisher's exact test). The change in depth of ETT during circuit reconnection after the removal of ILMA was significantly more with the usual connector ($P = 0.0018$; paired t-test) (Mean difference [95% confidence interval]; 1 cm [0.46–1.54 cm]). In clinical terms, these differences mean a shorter period without oxygenation and ventilation, reduced airway stimulation, less chance of endobronchial intubation and accidental extubation, and reduced need for assistant(s) with MCA. The MCA may thus facilitate faster and safer intubations through the ILMA.

We thank C. K. Dua, MD and Sonia Sood, MD for their help in the projects involving the use of the ILMA, Shashi Sharma, MA (Stats) for statistical guidance, and Raminder Sehgal, MD.

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DOI: 10.1213/01.ANE.0000077161.24188.73

Midazolam, Spinal Anesthesia, and Myoclonic Jerks

To the Editor:

We report the case of a 78-yr-old man who presented for elective femorotibial bypass grafting surgery. The patient's medical history included chronic obstructive pulmonary disease, coronary artery bypass grafting, and chronic renal failure requiring peritoneal dialysis. Routine blood tests were normal except for Na^+ (131 mmol/L), K^+ (3.4 mmol/L), urea (27.4 mmol/L), and creatinine (924 $\mu\text{mol/L}$). Midazolam (1 mg) was administered intravenously and lumbar spinal anesthesia performed using isobaric bupivacaine (17.5 mg),

epinephrine (100 μ g), and morphine (150 μ g). Immediately thereafter, during the insertion of a catheter into the radial artery, the patient became agitated with involuntary, spasmodic movements of both arms (myoclonic jerks). While unresponsive to verbal commands, he was hemodynamically stable and breathing spontaneously (SaO₂ 96% with O₂ delivered at 3 L/min through a face mask). Interpreted as a paradoxical response to midazolam, intravenous boluses of propofol were given resulting in upper airway obstruction necessitating endotracheal intubation (thiopental 200 mg, rocuronium 40 mg, fentanyl 100 μ g). Anesthesia was maintained with desflurane and nitrous oxide. The surgery was uneventful. On emergence from anesthesia, the myoclonic jerks were observed again and the trachea remained intubated. In the recovery room, further examination of previous blood tests showed a low magnesium level of 0.52 mmol/L prompting the administration of 2 g magnesium over 15 min. The myoclonic jerks resolved and the trachea was extubated. The neurological examination on the next day was normal. We propose that the unusual occurrence of myoclonic jerks in this patient was a manifestation of metabolic encephalopathy (hypomagnesemia and chronic renal failure) combined with central sensory deafferentation from neuraxial blockade and midazolam.

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DOI: 10.1213/01.ANE.0000074666.12395.11

Another Way to Reduce Operating Room Gas Contamination

To the Editor:

The study and correspondences by Dunn (1) and Corn and Panni (2,3) suggest that there continues to be an interest in finding ways to reduce anesthetic gas contamination of the operating room. The following is another method that can be used in many situations.

During anesthetic induction, volatile anesthetics are frequently administered via mask ventilation prior to endotracheal intubation or laryngeal mask insertion. Often in an effort to decrease operating room anesthetic gas contamination, the vaporizer is turned off while the fresh gas flow (FGF) is left on at a high flow rate. This probably has the opposite effect of what is desired. With this practice, the FGF carries volatile anesthetic into the operating room while diluting the agent concentration in the circle system. Thus if a patient has a hyperdynamic response to intubation, it will take longer to suppress that response.

Alternatively, instead of shutting off the vaporizer, one can turn off the FGF before removing the mask. (Most machines will have a minimum FGF of approximately 300 mL oxygen/min.) Thus there is essentially minimal or no FGF driving anesthetic out of the circle system into the room. It is likely that the sampling line of a capnograph will aspirate most if not all of the circle system gases that would tend to exit at the Y-connection.

I have used this method for several years and have not recollection of ever smelling potent anesthetic. The resistance to change often comes from the concern that one may forget to turn the oxygen back on. Even if that happens, with all the alarms that are used in contemporary practice, one would quickly be reminded well before any harm is done to the patient. Either the ventilator bellows not rising or the reservoir bag not filling would be the first abnormality that would be noticeable.

From my experience, few practitioners seem to practice or even be aware of this technique. Since this technique may provide benefit for only a small amount of time and not the entire case, I would expect there to be differences of opinion as to how valuable this technique is.

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DOI: 10.1213/01.ANE.0000077160.24188.3A

The MASTER Trial Has Successfully Addressed Requirements of Protocols for Large Trials

To the Editor:

In his editorial (1), Dr. de Leon-Casasola raised a number of questions regarding the conduct and findings of the MASTER Trial to which we would like to respond. As stated in our accompanying publication (2), the protocol demanded catheter insertion at a level, so as to achieve an epidural block of the proposed surgical wound and an additional two segments either side, using intraoperative local anesthetic with or without epidural opioid (fentanyl or meperidine) followed by an epidural infusion of local anesthetic and opioid postoperatively. Preoperative catheter placement and establishment of epidural block were fundamental to the trial.

The postoperative pain management protocol aimed for adequate analgesia in both arms of the trial. As is the case in routine clinical practice, this commonly required use of ancillary analgesic drugs such as nonsteroidal antiinflammatory drugs and/or paracetamol, as well as the use of parenteral opioids, in patients allocated to epidural analgesia if pain relief remained inadequate despite maneuvers to optimize epidural block. If epidural block was thought to have failed, then the catheter could be replaced, as is the case in routine practice. Postoperative pain review was conducted according to the practice protocol of the acute pain service of each participating hospital. The data we provided in our earlier publication (3) showed that adequate analgesia was indeed achieved and also demonstrated clearly the superior pain relief provided by epidural analgesia, especially in the first 24 h postoperatively.

Ethical considerations prevented us from placing a ceiling on the inspired volatile anesthetic concentration due to a need to reduce the risk of awareness. This is particularly important in combined regional and general anesthesia techniques where sympathetic blockade can mask signs of inadequate hypnosis. A robust protocol for anesthesia for major abdominal surgery requires general anesthesia, either alone or as a combined technique with regional anesthesia.

Our study protocol, and publication (2), included tracheal extubation criteria. However, due to the complexities of postoperative management, particularly in the presence of the extensive comorbidities seen in our high-risk study population, timing of discharge from intensive care and hospital were left to the discretion of the treating physicians.

We agree with Dr. de Leon-Casasola that the effects of epidural blockade on coronary perfusion reported in a few small series are interesting. However, given his own assertion that this possible cardioprotective effect demands catheter placement at a high thoracic level (T1-4), it has little relevance to epidural block for abdominal surgery.

There were several criticisms raised regarding the study design and statistical analyses. We had also raised some of these issues in our

publication. We agree that the overlap between the entry criteria relating to arterial blood gas results and one of our endpoint definitions raises the possibility of a spurious association. This arose from the inclusion of commonly used standard criteria for respiratory failure based on oxygen and carbon dioxide partial pressures, which are less useful in the postoperative setting where opioid-induced respiratory depression is common. Few patients (control group $n=6$, epidural group $n=8$) were recruited to the trial on this basis, so it had no effect on the results. Estimates of mean and standard deviation require assumptions of normality that are not entirely accommodated with log-transformation. We therefore reported both estimates for each group in order to satisfy the reader that the length of stay and intubation times were equivalent. We are well aware that multiple comparisons can lead to spurious findings, and in the footnote to our main results table declared that our P values were taken from unadjusted chi-square tests. Yet, despite doing multiple exploratory testing, we were still unable to demonstrate any important differences between groups.

In questioning our findings, Dr. de Leon-Casasola quotes a number of previously published papers that our results contradict. These are not large randomized controlled trials, but are either reviews, smaller studies, or meta-analyses of these. These smaller studies have looked at a range of physiological variables, such as markers of respiratory function. There has been widespread expectation that improvement in these “surrogate endpoints” would translate into real improvements in postsurgical outcome. Large randomized trials are required to confirm this, since they are the gold standard for assessing whether a particular therapy is beneficial. While following stringent guidelines regarding randomization, reporting and blinding (where possible), as laid down by the CONSORT group (4), the protocols for large trials should represent typical clinical practice to provide useful answers to practicing anesthesiologists. These guidelines are key determinants of quality of evidence and the strength of inference that clinicians can draw from such research (5). We believe the MASTER Trial has successfully addressed these requirements, increasing the Level 2 evidence that no significant improvement in outcome is associated with epidural analgesia in major abdominal surgery. Together with many other clinicians who practice combined regional and general anesthesia for major surgery, we remain open to the possibility that epidural block may benefit our patients, for example in aortic surgery (6). Those who remain convinced that epidurals are more widely beneficial must now undertake large studies to prove that this is so.

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Dr. de Leon-Casasola does not wish to respond.

Table 1. Update of Major Outcomes

Study	Patients randomized		Total mortality		Myocardial infarction		Pneumonia		Renal failure	
	GA	RA	GA	RA	GA	RA	GA	RA	GA	RA
Master (1, 2)	441	447	19	23	n/a	n/a	46	37	36	33
Vacs (3)	507	514	17	20	27	18	40	28	7	9
Cortra (5)	4,688	4,871	144	103	59	45	238	149	32	18
Total	5,636	5,832	180	146	86	63	324	214	75	60
Odds ratio			0.78		0.71		0.62		0.78	
95% CI			(0.63–0.97)		(0.51–0.98)		(0.52–0.75)		(0.55–1.09)	
P-value			0.03		0.04		<0.0001		0.15	

GA = general anaesthesia; RA = regional anaesthesia; n/a = Not available; CI = confidence interval.

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DOI: 10.1213/01.ANE.0000074665.12395.CA

Need for an Updated Overview to Assess the Benefits of Epidurals

To the Editor:

The recent MASTER trial (1,2) concluded that “most adverse morbid outcomes in high-risk patients undergoing major abdominal surgery are not reduced by the use of combined epidural and general anesthesia and postoperative epidural analgesia.” The authors reported that their findings were similar to the equivocal results from a recent randomized trial that also included about 1,000 participants (3) However, the VACS trial did not report any confidence intervals for their main outcomes, making it difficult to assess the range of plausible effects or their consistency with past evidence (4) With only about 40 deaths and up to a few dozen major endpoints in both the MASTER and VACS trials, the confidence intervals are wide and compatible with benefits seen in a previous overview (5). Rigg et al. (1) note that “there is more agreement . . . than may seem apparent.” Lack of evidence of effect should not be interpreted as evidence of lack of effect. This is particularly so when trials involve only a few dozen events. In this setting, the most reliable assessments are obtained from trial overviews. A basic update of some major outcomes in our previous overview (see Table 1) still indicates that regional anesthesia provides important improvements in major postoperative complications.

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DOI: 10.1213/01.ANE.0000074662.12395.04

In Response:

We read with interest the comments of Rodgers et al. and comparison of our results with their meta-analysis. We agree entirely that the confidence limits in the MASTER Trial (1,2) for the incidence of major morbidities are sufficiently wide that the question of the influence of epidural analgesia on outcome in major abdominal surgery remains open.

We are keen to see our data incorporated into further meta-analysis. However we assume from the numbers involved that their Table contains data from all four surgical subgroups studied in the original CORTA analysis (3), which found significant benefit in orthopedic surgery only.

As practicing anesthesiologists we tailor our anesthetic management to anticipate the specific problems for each patient and their surgery. Perioperative management and risks of major abdominal surgery present different challenges to those of lower limb or peripheral surgery, both surgically and in terms of anesthesia and postoperative analgesia. Generalized conclusions regarding anesthetic management based on data from disparate surgical groups may be misleading.

We believe the clinical question should now be specifically addressed to major abdominal surgery. Data from the MASTER and VACS (4) trials will greatly enhance the power and validity of this targeted meta-analysis. Some of us have been collaborating with the CORTA group to update the evidence base of this group of patients (by meta-analysis of all relevant trials) and we eagerly await these results.

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Pharmacokinetic Drug Interaction Between Propofol and Remifentanyl?

To the Editor:

We read with interest the paper by Koitabashi et al. (1) on the addition of remifentanyl to propofol infusions and the resultant decrease in Bispectral Index (BIS). The authors concluded that remifentanyl has sedative/hypnotic properties, or that it potentiates the hypnotic effect of propofol. Puri has commented that other

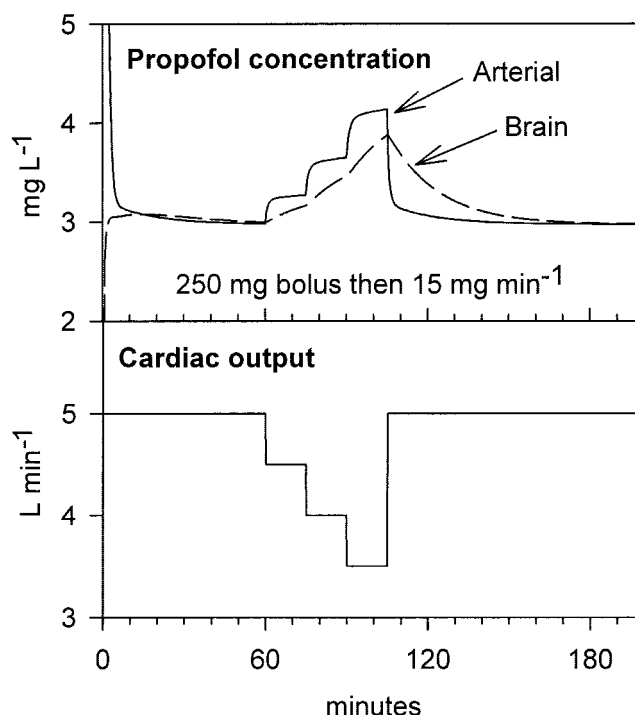


Figure 1. Simulation of physiological pharmacokinetic model.

stimuli can add to the effect of remifentanyl on BIS (2). Both explanations have focused on pharmacodynamic mechanisms—we would like to suggest an alternate pharmacokinetic hypothesis.

The pharmacokinetics of propofol is dependent on cardiac output after both bolus administration (3) and during constant rate infusions (4). The mechanisms of this effect are a combination of recirculatory concepts and blood flow limited clearance (5).

In the study of Koitabashi et al., remifentanyl administration, especially in conjunction with a vasopressor, was likely to have produced a dose-dependent decrease in cardiac output. This would have resulted in an increase in arterial and brain propofol concentrations, and therefore anesthetic effect. This has been simulated in Figure 1, using a published physiological pharmacokinetic model (6).

We also suggest that circulation-based pharmacokinetic drug interactions may potentially provide an explanation for other studies in which vasoactive drugs, such as clonidine and epinephrine (7), have been reported to affect BIS.

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Dr. Johansen did not respond.

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DOI: 10.1213/01.ANE.0000077162.24188.A8

Latex-Free Pulmonary Artery Catheters

To the Editor:

Latex allergy is now recognized as a serious cause of intraoperative anaphylaxis, and latex is second to muscle relaxants as a common cause of intraoperative anaphylaxis. The balloon of the standard pulmonary artery catheter (PAC) is made out of latex, and there is a report of a severe anaphylactic reaction after the balloon of a PAC was inflated (1). Until recently, there were some manufacturers of PAC with a latex-free balloon, such as American Biomed in Texas and World Med in Florida. We recently wrote a review article on the topic of latex allergy (2) and indicated that American Biomed's CLS thermodilution catheter (7F, 110 cm) was available. I recently received an inquiry from a cardiac anesthesiologist in St. Louis who had difficulty obtaining a latex-free pulmonary artery catheter and tried to contact this company. He was unable to find any information on this company, and contacted me to ask for details. I did an extensive search and found American Biomed had been dissolved. In addition, I found that World Med, the only other company that was producing latex-free PACs, also went bankrupt a few years ago. I then contacted Edwards Lifesciences (www.edwards.com), a large distributor of PACs, and found that there is no currently available latex-free PAC on the market here or abroad. Edwards informed me that they currently have a custom-made PAC without a balloon. In addition, they carry a PreSep central venous catheter that measures continuous oxygenation (SCVO₂) and is latex-free. Finally, they have FDA approval for a latex-free PAC with a balloon that is due to be released soon.

While it may be unusual for a review article to have some material out of date immediately after its publication, the publication process takes close to a year, and some of the small biotechnology companies may go out of business in this time span. In addition, there was a disclaimer in Table 1 of our article (2), stating that latex products and safe alternatives available vary between companies, product series, and with time. The purpose of this table was to offer a general guideline of latex products and available alternatives and was not meant to be a comprehensive review of all available latex-free products. It is always imperative to check for latex content in medical materials if they are being used in a latex allergic individual. Fortunately, as of September 30, 1998, all products that contain latex must be labeled as such.

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DOI: 10.1213/01.ANE.0000077163.24188.E1

Tender Active Acupoint Is Not an Ideal Control for Acupressure Study

To the Editor:

Fassoulaki et al (1) selected a control point located 2 cm lateral to the left eyebrow. This location corresponds to the extra meridian point Taiyang (2) and for that reason alone is not an ideal control point. Furthermore, the volunteers experienced an unpleasant feeling during acupressure on the control point, whereas acupressure on the Yintang point (extra 1) was not associated with

this feeling. The level of discomfort was so great that volunteers were not able to tolerate pressure on the control point long enough to match the duration of treatment on the extra 1 point. This probably increased the anxiety and stress and created uneven treatment conditions for the study days. It is not quite clear, then, why the authors used this point as their control.

Acupressure on either point could have produced sedation and reduced anxiety owing to "treatment expectation" placebo effect associated with endogenous opioid release (3,4). However, painful stimulation of the control point might have aroused (5) the subject as stress-mediated catecholamines release (6) can override the relaxing effect.

In my opinion, the results of this study would be more convincing if the authors had chosen another intact non-tender point on the forehead as the control point.

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DOI: 10.1213/01.ANE.0000077164.24188.2F

In Response:

As we describe in the Methods section of our article (1), the control point was located 2 cm from the lateral of the eyebrow, and this location is above the Taiyang acupoint. Underneath the control point, the bone protrudes rather, and this may be a reason for the unpleasant feeling. According to figures and descriptions, the Taiyang point lies between the lateral end of the eyebrow and the outer canthus (2) (Figure 1 of the article) or at the level of the outer canthus (3) (Figure 2 of the article). Besides, as most of the acupoint points, it lies in a depression not on a protruding bony area. So, we did not apply pressure on another acupoint. It is quite clear why we used this control point. As described in the Methods section, the location of the control point was selected to be close to the BIS sensor, so that possible interferences of pressure application with the BIS sensor and signal would be observed in both the treatment and the control group.

The assumption by Dr. Chernyak, that painful stimulation of the control point might have aroused the subject as stress-mediated catecholamines release can override the relaxing effect, is not accurate. Volunteers described an unpleasant feeling, not pain, and catecholamine levels were not measured. Painful stimulation and unpleasant feeling are not identical concepts. We limited the pressure duration to 5 min for two reasons: a) these volunteers were kindly offered without any reward, and b) a longer lasting unpleasant feeling might have an impact on recruiting more volunteers.

Uneven treatment conditions apply to every study using a placebo treatment versus an effective treatment. The acupressure application on the extra 1 point was assessed also by BIS, which is an objective measurement versus the feeling of reduced stress and tension, which is subjective.

About treatment expectation and placebo effect, despite the unpleasant feeling due to pressure, the level of anxiety was also reduced in the control group by 14% (Figure 4 of the article), but this reduction was significantly less when compared with the 50% reduction in the treatment group. Finally, we wish to emphasize that due to BIS sensor, which occupies a large area on the forehead, and

due to the bone underneath the forehead, which is sensitive to pressure application, an ideal control point is not feasible.

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"Slow Puncture" of a Pulmonary Artery Catheter Balloon

To the Editor:

Pulmonary artery false aneurysm formation, induced by a Swan-Ganz catheter is a well-known complication that may be caused by repeated manipulations and a too far advanced tip of the catheter. It is estimated to occur in 0.06 to 0.2% of cases with pulmonary artery catheter and is associated with a mortality rate of 45 to 65% (1-3).

Prior to cardiac transplantation surgery, we recently introduced a pulmonary artery catheter (Swan-Ganz CCOmbo V CCO/SvO₂/CEDV REF 744 HF 75, Edwards Lifesciences, Germany) through an 8.5F introducer sheath (Percutaneous Sheath Introducer Set REF Product No SI-09875-E, Arrow, Germany). After *in vitro* calibration, the catheter was removed from its package. While routinely flushing the catheter ports, wetting the balloon and catheter, and testing of the balloon by inflating it and then releasing the inflation syringe before insertion into the patient, no problems were obvious. Then the catheter was inserted cautiously into a contamination shield with the proximal and distal hubs in the widest open position.

The left internal jugular vein was cannulated with the introducer sheath. After cautious insertion of the catheter through the introducer sheath, the balloon was inflated by filling it slowly with 1.5 mL of air. Then the catheter was advanced under pressure-wave guidance, showing normal wave patterns. The catheter was advanced incrementally until 65 cm. Only pulmonary artery but no wedge tracing could be obtained. The inflation syringe was released, and only 0.5 mL of air released spontaneously back into the syringe. Under the perception of a damaged balloon, it was decided to replace the catheter. When the catheter was removed, the balloon seemed still intact. It expanded symmetrically when the air was inflated rather quickly. The inflated 1.5 mL of air released back into the syringe. Therefore, the same catheter was reinserted, but again no wedge tracing could be detected, and the catheter was removed again. Another Swan-Ganz catheter from the same manufacturer, but with a different production date, was placed. This time wedge tracing was seen at 62 cm. Later on the testing of the first catheter revealed that slow inflation of the balloon allowed the escape of air through a tiny hole. The balloon remained flat. The damaged catheter was sent to the manufacturer for investigation. To date, we have not received any report on their results.

This is our own explanation for the observed phenomenon: The puncture in the balloon needs to be positioned in the fold, meaning the junction between the catheter and the balloon. In the case of rapid inflation, more air enters the balloon than can escape through the puncture, and once the balloon is fully inflated, the puncture is sealed by being within the fold. Slow inflation means that air can escape through the puncture at a rate more equal to that of inflow, therefore the balloon cannot expand and the puncture will not be covered.

We would like to bring to the attention of the reader that, despite obeying all rules and precautions, mechanical damage of a balloon may not be recognized.

The catheter was removed from an undamaged package without problem; it seems unlikely that it was damaged at that time. Also protective sheaths are known sources for damage to the balloon. Again all precautions to prevent damage have been fulfilled: wetting of catheter and balloon, widest opening of the hubs, and cautious insertion into the introducer sheath (4).

After repeated pre-and reinsertion testing, the damage of the balloon could not be anticipated. But a high index of suspicion prevented the operator from further advancement of the catheter that could have caused pulmonary artery rupture. The exchange of the catheter proved to be the right decision.

The respective catheter was sent for further investigation to the manufacturer. The patient did not experience any adverse outcome from placing the PA-catheter.

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DOI: 10.1213/01.ANE.0000074659.05733.9D

Does Intrathecal Mepivacaine Cause Post Lumbar Puncture Headache?

To the Editor:

In the article by Meiningner, et al. (1), the authors report an incidence of post lumbar puncture headache of nearly 10% (9/100) using a 25-gauge pencil-point needle. This is a more frequent incidence than is usually found with this type of needle and is of the same order of magnitude as used to occur using 25-gauge Quincke needles. The authors do not comment on this nor do they mention how many patients required epidural blood patch for treatment.

I wonder if mepivacaine could be the reason for this phenomenon.

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Reference

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DOI: 10.1213/01.ANE.0000074663.12395.4D

In Response:

We thank Dr. Lee for his interest in our article. We agree with Dr. Lee that an incidence of postdural puncture headache (PDPH) of 9% using a 25-gauge pencil-point needle has been higher than the incidence reported by Vallejo et al. using this type of needle (1). In our study, all parturients were seen postoperatively every 24 h for the first 72 h after spinal anesthesia by a resident or staff physician. Parturients were asked for the presence or absence of headache and any accompanying symptoms. PDPH as well as any kind of postpartal headache were recorded and all patients

have been treated like patients with PDPH leading to an overall incidence of headache of 9%. Severe postdural puncture headache requiring an epidural blood patch has not been recorded in our study group, headache has always been described as mild and resolved spontaneously (2).

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Evaluation of Blood Ordering Algorithms

To the Editor:

We read with great interest the recent article of Palmer et al. (1) and we appreciate their attempt to reduce the consumption of the logistic resources of blood banks for the provision of blood products. However, we doubt whether their patient-specific blood ordering system (PSBOS) is really superior to the maximum blood ordering system (MSBOS) in achieving this aim. This is because this study seems to suffer from a major methodological flaw. In their study design, Palmer et al. only include patients where the MSBOS predicts transfusion. The exclusion of patients who had only a type-and-screen performed, i.e., the patients in whom the MSBOS would predict no transfusion, primarily has two major unfavorable effects on this study: first, it is expected to increase the a priori probability of transfusion in the group of patients having the PSBOS performed, thereby leading to bias. Second, sensitivity and specificity cannot be sensibly determined, and the negative predictive value (NPV) cannot be determined at all for the MSBOS in this setting (2). However, the negative predictive value of the two algorithms was of major interest, since the authors pointed out that they were most interested in those patients who were very unlikely to require a blood transfusion during surgery.

One can easily imagine the following scenario based on the data reported by Palmer et al.: assume that 200 eligible patients during the study period had only a type-and-screen performed, and assume the data presented in Table 1 for the MSBOS and the PSBOS had occurred

This scenario provides a completely different result. It cannot be ruled out that this outcome might have occurred if Palmer et al. had conducted their study appropriately. These data show no clinically relevant difference between the two algorithms any more. Moreover, the MSBOS might even have been better than the PSBOS in predicting patients unlikely to receive a transfusion.

Table 1. Comparison of 2×2 Tables for the MSBOS and the PSBOS (see text for details)

MSBOS (n = 377)	Patient transfused?		
	Yes	No	
Prediction of transfusion	Yes	28	149
	No	4	196
PPV 16% NPV 98% Sensitivity 88%, Specificity 57%			
PSBOS (n = 377)	Patient transfused?		
	Yes	No	
Prediction of transfusion	Yes	11	10+20
	No	17+4	139+176
PPV 27% NPV 94% Sensitivity 34%, Specificity 91%			

The data of Palmer et al. also suggest that the PSBOS may be more susceptible to short-term changes in the (planned) surgical procedure that might further reduce its applicability. This could also not be addressed with the chosen study design.

In conclusion, in order to compare the two algorithms, one has to use them independently of each other and has to include the patients who had only a type-and-screen performed into the analysis. What the authors can state is that, after the MSBOS had been considered, the PSBOS might be an additional tool in predicting patients who are at risk of receiving blood transfusions during surgery.

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DOI: 10.1213/01.ANE.0000074656.05733.79

In Response:

We appreciate the opportunity to respond to Professors Karger and Kretschmer, and appreciate their interest in our paper. Despite multiple revisions of our manuscript, we still did not make clear one salient assumption in our design, the point being that we were entirely focused on those patients for whom the maximum blood ordering system (MSBOS) recommends blood be crossmatched prior to surgery. We hoped to provide a means to enhance the effectiveness of the MSBOS system, not to replace it entirely. As Professors Karger and Kretschmer point out, MSBOS is very good at identifying operations in which crossmatching is not needed in any case. Our patient specific blood ordering system (PSBOS) would be of little use applied to cases where we already know there will be no need for transfusion. The entire focus of our study and paper was to reduce unnecessary crossmatching—by definition, if the MSBOS recommends only a type and screen, there will be no crossmatch performed. Our only attempt was and is to reduce the crossmatches advised by the MSBOS, but where there will be great variability in transfusion due to patient and surgeon variables. There was no methodological flaw in our design, but we did not clearly communicate to our readers this key distinction.

Professors Karger and Kretschmer are correct that our title and concluding statement are misleading as written, implying that we believe that our algorithm should entirely supplant the MSBOS system. In truth, we believe that the MSBOS is a very useful tool that is quite accurate in predicting which operations are essentially free of blood loss, and thus are unlikely to ever require a transfusion despite great variability in the patients who undergo these operations. However, for those cases where MSBOS recommends blood be crossmatched, our data demonstrate that the vast majority of patients do not receive blood, and that many of these crossmatches are unnecessary. Our intent was to study those cases in which MSBOS recommends blood be crossmatched, and apply patient specific factors in those cases. Our data demonstrate that, in those patients for whom MSBOS recommends blood be crossmatched, application of an algorithm that incorporates patient-specific factors can identify which patients are unlikely to receive blood, thus reducing “unnecessary crossmatching”.

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The Analgesic Effect of Interscalene Block Using Clonidine as an Analgesic for Shoulder Arthroscopy: Where Is the Catheter?

To the Editor:

We congratulate Iskandar et al. for performing this study with the interesting topic of the analgesic effect of clonidine applied in the interscalene space (1). The results of this study are important, since they completely disagree with those found by Sia et al. (2). With respect to known literature (2,3) and the methods described by the authors, the presented results may raise concern. The scientific description of the chosen approach to the interscalene space and a detailed explanation of the catheter insertion procedure have not been described. It would be of great interest to have more data about the nerve stimulator settings (milliseconds, hertz), other than just that the output had to be lower than 0.7 mA. Very important information is also lacking about how far the catheter was threaded past the tip of the needle. All these points lead directly to our main concern: there is in this study no validation of the correct placement of the catheter before commencement of surgery. This would have been easily performed with a short acting local anesthetic bolus. Without this, we can only speculate whether the catheter was placed correctly or not, and therefore, we do not really know whether the drug given through the catheter may have any effect at all on the nerves, making the validity of the results doubtful. Another point to mention is the imprecise description of the authors' management of remifentanyl at the end of surgery. The timing and mode in which remifentanyl is stopped on completion of surgery is very important for the postoperative pain setting, since its rapid discontinuation produces a hyperalgesic state (4). Concerning the statistics, post hoc tests for correction of repeated measurements are lacking, as well as a power analysis.

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DOI: 10.1213/01.ANE.0000074658.05733.D4

In Response:

We thank Blumenthal et al. for their attention to our study examining the analgesic effect of interscalene block using clonidine as an analgesic for shoulder arthroscopy. We would like to respond to several points raised by Dr. Blumenthal.

First, the interscalene catheter was placed by an experienced anesthesiologist, with nerve stimulator help (Stimuplex Braun). The stimulation frequency was set as 2 Hz, while the duration of stimulation was set as 0.2 ms. The intensity of stimulating current, initially set to deliver 2 mA, was gradually decreased to ≤ 0.7 mA after the appropriate muscular response was observed. Then the catheter was advanced and secured with a transparent occlusive dressing. Although there is no validation in this study of the correct placement of the catheter before commencement of surgery, all catheters were effective for postoperative analgesia, indicating their correct placements.

Second, the continuous administration of remifentanyl during general anesthesia was stopped at the end of surgery in all patients (before arrival to the PACU), thus this drug does not influence postoperative analgesia.

We believe that clonidine applied alone to the interscalene plexus enhanced postoperative analgesia after shoulder arthroscopy.

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