

Good Ideas Deserve Repeating

To the Editor:—In a recent letter, Erickson and Lanier¹ reported on a useful method to preoxygenate claustrophobic patients who will not tolerate a facemask, by having the patients hold the L-connector of the anesthesia circuit in their mouth. They may be interested to know that an identical method, complete with an almost identical photograph, was published as a correspondence by Keifer and Stirt² of this department in ANESTHESIOLOGY a decade ago. I and others have used this technique on numerous occasions and agree with Drs. Erickson and Lanier that is extraordinarily easy and successful.

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In Reply:—We thank Dr. Baum for his letter, bringing the 1995 publication of Keifer and Stirt¹ to our attention. This experience lends a new interpretation to the comment of Ambrose Bierce, the 19th-century American writer, that “there is nothing new under the sun, but there are lots of old things we don’t know.”* Neither of us was aware of a previous description of the technique we labeled “mouth-to-circuit,”² and of the dozens of people with whom we have shared it, all have received our description as new and refreshing. Hence, we assume that none of them knew of the earlier report¹ either.

Perhaps this oversight resulted from our approach to the problem. The senior of us (W.L.L.) has long been interested in introducing readily incorporated solutions to perplexing problems of airway management and oxygenation. Examples include reports on improving mask fit in edentulous patients³ and a technique for providing prolonged oxygen administration in aircraft.⁴ Our report in the February issue of ANESTHESIOLOGY² was directed at improving oxygen delivery and a sense of psychological well-being in claustrophobic patients. Our screening of the literature, using PubMed, followed this view. Searches combining the term *claustrophobia* with *preoxygenation*, *airway*, *oxygen*, *anesthesia*, *anesthesiology*, and *mask* all failed to identify the 124-word Keifer and Stirt publication. This is likely because the Keifer and Stirt report refers to patients experiencing “fear” or a “sense of ‘smothering’” but never mentions the term *claustrophobia*.

Our report offers some features not provided in the earlier Kiefer and Stirt report, including (1) a quantitative expression of our considerable experience and success with the technique, (2) the physiologic and psychological factors contributing to the success of the method, (3) alternative techniques that can be considered, (4) the role of nasal occlusion, and (5) circuit gas analysis when using the mouth-to-circuit technique *versus* preoxygenation using a conventional mask ap-

* http://classclit.about.com/cs/profileswriters/p/aa_abierce.htm. Accessed May 10, 2006.

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proach. We acknowledge, however, that the Kiefer and Stirt report effectively and concisely covered all of the fundamentals of the technique, and our additions are merely gilding to the core story. Had we known of their publication early on, we would not have considered submitting our report for publication or performing the institutional review board—approved research included in our report (*i.e.*, the portion of the report identified by the Editor-in-Chief of ANESTHESIOLOGY as critical for approval). Had we discovered the Kiefer and Stirt report later, we certainly would have given them their due credit for describing this useful technique.

If there is a lesson to be learned from this experience, it is that—in the current era of rapidly growing medical literature and dependence on computer-facilitated methods to archive information—literature searches are dependent on identifier- and metadata-rich titles and (when appropriate) abstracts to facilitate retrieval.⁵ Had Kiefer and Stirt been sufficiently prescient in 1995 to foresee this revolution in information management, they probably would have loaded the title of their report with more information-rich phraseology than the as-published single word, “Preoxygenation.”¹

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Thoracic Epidural Anesthesia/Analgesia for Cardiac Surgery: Don't Put Down Your Tuohy Needles Yet

To the Editor:—We read the recent article in your journal by Hansdotir *et al.*¹ In it, they described a small (113 patients), prospective, randomized controlled trial on the use of patient-controlled thoracic epidural anesthesia/analgesia (TEA) for several types of cardiac surgery that did not show any difference on postoperative outcome comparing this technique with patient-controlled analgesia morphine.

In their article, the authors quote the article from our department in 2001 that showed significant improvements in outcome but not hospital discharge using TEA for coronary artery bypass surgery.² In this article, we took statistical advice from the outset to achieve a 90% power at a 1% level of significance for a reduction in the incidence of cardiac arrhythmias from 25% (considered to be the normal incidence in non-TEA patients) to 10%. We were advised that for one operation, we would need to investigate 420 patients, which we did in an open prospective manner. In doing so, we also found significant differences in several other outcome measures, including chest infection, acute renal failure, and postoperative confusion. At the time, our statistician calculated that we would need almost twice as many patients to determine true differences in the incidences of myocardial ischemia and infarction, given the much lower incidence for these after coronary artery bypass surgery. In the subsequent meta-analysis, Liu *et al.*³ calculated that 6,000 patients are needed in a randomized controlled trial to make definitive statements about the effects of different therapies on myocardial ischemia.

In simple terms, the study by Hansdotir *et al.* is grossly underpowered to confirm or refute the findings of any previous study on postoperative outcome had they investigated only *one* procedure. For the authors to include five different operations in their analysis of only 113 patients and for none of these subsets to have similar numbers of patient-controlled analgesia and TEA patients suggests that the study has no statistical merit in its design. Finally, we are not told how many valve procedures are aortic or mitral, or whether they were repairs or replacements.

Of greater concern is the conduct of the epidural regimen. In our opinion, there are two main reasons for using TEA, more so than analgesia: First is sympathetic blockade, with its consequent beneficial effects on postoperative organ dysfunction—this we were able to confirm in our study—and second, by avoiding moderate to severe pain, is the ability to avoid the use of systemic opioids in patients receiving TEA. This is a major target for modern acute pain services. It is also the reason that in our hospital we also use clonidine, rather than an opioid, in our epidural infusion because it too has both analgesic and sympatholytic properties.

In a clinical arena of such a highly controversial nature, to insert an epidural catheter and not establish that it is truly working when the perceived risk of complications is so high is surely inappropriate. Therefore, establishment of the block before both surgery and anesthesia is a fundamental requirement of the technique both from a philosophical and practical viewpoint.

The fact that the two groups are so similar in virtually all of the parameters measured by the 40-item quality of recovery questionnaire is strongly suggestive that Hansdotir *et al.* were comparing like with like and that patients in the TEA group did not have an effective block. This seems to be borne out by closer inspection of their methodology. The epidural was sited the day before surgery between T2 and T5, and 4 ml lidocaine, 1%, was given. This would merely confirm that the

catheter was not intrathecal and is too little local anesthetic to confirm an effective epidural block. There is also no science to a loading dose or infusion rate based on ml/kg or ml · kg⁻¹ · h⁻¹, and neither guarantees an effective block. At the end of surgery, a bolus of 0.1 ml/kg was apparently given to all patients in the epidural group. Why was this necessary if the epidural was effective? Therefore, no data are presented to confirm adequate placement of the catheter, spread of the local anesthetic, and effectiveness of the block.

For these reasons, we cannot agree that the failure rate was only 5.2% as the authors claim, and given the amounts of local anesthetic used intraoperatively and the need for an immediate top-up postoperatively, it is more likely to be closer to 100%. Moreover, to rely on a patient self-administering morphine as a means of determining a successful block is not appropriate because many patients will have good analgesia without an epidural catheter or, in patients who do have an epidural, without a demonstrable block.

The results of this study are similar to those of Fillinger *et al.*,⁴ who demonstrably failed to provide an adequate and effective block, thus ensuring no differences in outcome between groups. That study, too, allowed surgeons unblinded to the techniques to control the primary endpoint of their study, namely time to hospital discharge.

The literature on this topic does not need any more studies of this kind, which are too small to detect significant differences. Ideally, a large multicenter study should now be approved, but for a variety of reasons, this is unlikely to happen. In its stead, the only reasonable alternative is a thorough audit by centers that are experienced in the use of the technique. To that effect, we can confirm that since our study was completed, we have converted to the routine use of TEA for all coronary artery bypass surgery patients. In total, we have performed 2,700 TEAs for coronary artery bypass surgery, with a 28-day mortality of 0.9% and no incident of epidural hematoma. We are aware of similar data from other European centers with a total of around 15,000 patients, confirming that this technique is not dangerous, and we have now started to use it for valve patients as well, with additional precautions with regard to the use of warfarin.

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Thoracic Epidural Analgesia after Cardiac Surgery

To the Editor:—We read with interest the prospective evaluation by Hansdottir *et al.*¹ of patient-controlled thoracic epidural analgesia *versus* intravenous patient-controlled analgesia. Given the important implications of their data showing no analgesic or other benefit and questioning the risk-benefit of patient-controlled thoracic epidural analgesia, we have several comments about the study.

Figure 1 indicates that 7 and 6 patients, respectively, were excluded from each group for analysis (leaving 48 and 49), although tables 1 and 2 list 55 patients in each group, and other figures and tables do not indicate “n.” Of the 55 patients in the patient-controlled thoracic epidural analgesia group, 1 died intraoperatively and 1 had surgery postponed, making intraoperative data for 55 patients unlikely.

Although 7 patients with malfunctioning catheters were analyzed as intention to treat, 3 had catheters replaced postoperatively (with inherent absence of epidural activity for a definitive period), and 4 did not have the catheter replaced. Therefore, given the study protocol for extended postoperative infusion, the absence of epidural effect for this 7-12% of the group would likely have prejudiced results of the patient-controlled thoracic epidural analgesia group.

Similarly, the 2 and 4 patients in each group who experienced confusion or stroke are included in the tabulated complications, but

the authors do not discuss their ability or inability to meaningfully complete quality of recovery or analgesia scores.

Given the potentially significant findings of this study, it is important to clarify the conduct of the study for practicing anesthesiologists. Unfortunately, the reality of elective cardiac surgical practice in the United States for this relatively healthy cohort will preclude either epidural placement 1 day before surgery or a postoperative stay of almost 10 days (which would alter the power of the study as originally applied to this Swedish environment).

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In Reply:—We thank Dr. Scott *et al.* for their interest in our article.¹ In their eagerness to defend their use of thoracic epidural anesthesia/analgesia (TEA) in all cardiac surgery patients, Scott *et al.* attempt to invalidate the results of our study. They claim that our study “is grossly underpowered to confirm or refute the findings of any previous study on postoperative outcome” and that our study has “no statistical merit in its design.” We strongly disagree. For the purpose of our study, our population size is adequate. We had no intention to evaluate the effect of patient-controlled thoracic epidural analgesia (PCTEA) on cardiac, renal, pulmonary, or neurologic complications as *primary* postoperative outcome variables. Our study was designed to evaluate the potential effects of PCTEA *versus* patient-controlled intravenous analgesia (PCA) on length of hospital stay and patient-perceived quality of recovery, as also stated in the title of our article. We performed a power analysis (clearly described in our article) and found that an estimated 40 patients in each group were needed at a statistical power of 80% and a significance level of 5% to detect a clinically meaningful, 2.5-day reduction in length of hospital stay. Allowing for failures to complete the study, more than 100 patients were recruited.

Scott *et al.* raise concerns on “the conduct of the epidural regimen” in our study. They suggest that “patients in the TEA group did not have an effective block.” They even give the statement that the epidural failure rate of our study “is more likely to be closer to 100%.” Furthermore, they imply that we did “not establish” that our epidurals were “truly working.” Our group has a documented 20 yr of experience in providing high TEA in patients with severe coronary artery disease and unstable angina pectoris.²⁻⁷ We have repeatedly shown that 4 ml bupivacaine, 0.5%, induces a blockade corresponding to Th1-Th7, *i.e.*, a complete cardiac sympathetic blockade.²⁻⁷ We carefully tested that our epidurals were working, *i.e.*, caused a cardiac sympathetic blockade, on the day of insertion. Perioperatively, the patients received 8 ml bupivacaine, 0.5%, as a bolus and 8 ml/h as epidural infusion, which is an even higher dose than that administered by Scott *et al.*,⁸ who reported that they induced a neuraxial block from Th1 to Th10 in their

study. More importantly, the catheter function was also tested after surgery in all patients before discharge from the intensive care unit. Therefore, 93% of patients allocated to PCTEA treatment did receive effective pain treatment with PCTEA during the time period when major endpoint variables were collected. Other evidence that our epidurals were “truly working” were that the patients were extubated earlier, they received 20-30% less propofol and remifentanyl during surgery compared with controls, and the mean visual analog scale score was less than 1 on the 3 postoperative days of the study, as seen in table 3 of our study.

Scott *et al.* propose that the fact that the PCTEA and the PCA groups “are so similar in virtually all of the parameters measured . . . is strongly suggestive that [we] were comparing like with like and that patients in the TEA group did not have an effective block.” Surprisingly, it seems that the opinion of Scott *et al.* on the usefulness of TEA in cardiac surgery is so biased that it never occurred to them that our findings that PCTEA offers no major advantage with respect to hospital length of stay (LOS) and quality of recovery could be explained by the possibility that PCA might be a pain treatment that is as effective as PCTEA. Instead, they insinuate that the lack of effects of TEA on primary endpoints of our study¹ and in the study by Fillinger *et al.*⁹ is caused by malfunction of the epidural catheters.

Regarding the statement from Scott *et al.* that “this technique is not dangerous,” we would like to remind them that there are at least two published case reports of epidural hematomas related to TEA in cardiac surgery patients.^{10,11}

We also thank Drs. Pivalizza and Warters for their constructive comments on our article. They raise questions mainly regarding the presentation of our data. Fifty-five patients in the PCTEA group received a successfully placed epidural catheter, and 55 patients were allocated to PCA treatment (fig. 1). We decided to present the preoperative data of all these patients in table 1, irrespective of whether they were finally analyzed. They correctly point out an error in table 2. In one of the patients in the PCTEA group, surgery was postponed, and

the patient was transported (with the epidural catheter) to another hospital for surgery. Obviously, intraoperative data from this patient were not obtained. However, intraoperative data from the patient who died in the operating room after end of surgery were obtained. Thus, intraoperative data were obtained from 54 patients in the PCTEA group and not 55 patients as stated in table 2. In tables 3 and 4, data on postoperative visual analog scale and mobilization scores, as well as lung function, were obtained only from patients who were finally analyzed according to figure 1.

Yes, 7 patients had malfunctioning epidural catheters when assessed before intensive care unit discharge. Three of these patients had functioning epidural catheters replaced before discharge, whereas the remaining 4 received PCA. This is the clinical reality. Perioperative treatment with TEA will never be 100% successful. There will always be patients with suboptimal function of the epidural catheter. That is why we used an intention-to-treat analysis; it gives a more reliable estimate of true effect because it replicates what we actually do in routine practice. In our study, 51 of 55 patients (93%) allocated to treatment with PCTEA did receive PCTEA during the time period when the primary and secondary endpoint variables were collected.

The six patients who had postoperative stroke or confusion were not able complete quality of recovery, visual analog scale, or mobilization scores or lung function tests and were not finally analyzed according to figure 1.

It is difficult to compare various institutions with respect to hospital LOS after cardiac surgery. This variable is dependent on many factors not related to, for example, pain treatment itself. To circumvent this problem, the time to fulfillment of prospectively defined criteria for hospital discharge was assessed for each patient by observers blinded to treatment. Furthermore, one must define how LOS is assessed. In our calculation of actual hospital LOS, we included the day of admittance, *i.e.*, the day before surgery and the day of discharge. Actual mean hospital LOS in our study on a mixed population of cardiac surgery patients, including patients undergoing combined procedures, were 7.5 ± 3.3 and 7.9 ± 2.8 days for the PCTEA and PCA groups, respectively. In recent studies on patients undergoing low-risk coronary artery bypass surgery, hospital LOS varies between 5 and 7 days.

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Improving the Success of Retrograde Tracheal Intubation

To the Editor:—Lenfant *et al.*¹ are to be congratulated on their study comparing two techniques for retrograde orotracheal intubation using the cricothyroid approach in cadavers. Their modified technique is similar to that reported by Tobias,² although he used a fiberscope as a catheter through the endotracheal tube into the trachea before removing the guide wire.

The authors report 22 failures with the classic technique *versus* 8 with the modified technique. It is not clear whether esophageal intubation occurred in all of these cases or whether there was supraglottic placement in some. Also, there is no mention of the size or type of endotracheal tube used in the study.

It is crucial that the tip of the endotracheal tube is positioned beneath the vocal cords for subsequent successful passing of the catheter into the trachea. Although passing a thin catheter over the guide wire is easier, resistance may be encountered while advancing the endotracheal tube over the catheter, especially at the level of the glottis. Similar difficulty with the advancement of the endotracheal tube over a fiberscope³ or a bougie⁴ is well documented and is related to the size and type of endotracheal tube and the position of the bevel. It would be interesting to know the incidence of such "hanging-up" phenomenon and the requirement for any maneuver to overcome the

same in the current study. It is important to understand the dynamics of failed retrograde intubations to improve the success rate.

The authors comment that the subcricoid approach may be more dangerous than the cricothyroid approach. Literature on the use of the subcricoid approach for retrograde intubation is sparse. Subcutaneous emphysema, minor skin bleeding, and incorrect positioning are the reported complications (10%) with percutaneous minitracheostomy using the subcricoid approach in 50 patients.⁵ On the contrary, it is shown that retrograde intubation using the cricothyroid approach has more potential to cause vocal cord trauma than the subcricoid approach.⁶ Various complications, including pneumomediastinum, have been reported after the cricothyroid approach.⁷ The area from the cricoid cartilage to the lower border of the first ring of the trachea is devoid of major blood vessels or nerves,⁸ whereas the cricothyroid membrane is crossed by the cricothyroid artery superiorly.⁹ The subcricoid approach of retrograde intubation using the cricotracheal membrane seems relatively safe, with the advantage of improved success rate.^{6,8}

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In Reply:—We read with great interest the comments Dr. Nadarajan made regarding our study comparing two techniques for retrograde orotracheal intubation,¹ and we thank him for the consideration he gave to our work.¹

Regarding the causes of the failure, we agree that, if the endotracheal tube has not been positioned beneath the vocal cords, the catheter may be difficult to insert or placed in a wrong position, and tracheal intubation will fail. In our study, most of the failures were due to a supraglottic placement of the endotracheal tube as a consequence of a wrong position of the catheter. The size of endotracheal tube was adapted to the patient's morphology to allow its easy insertion into the trachea, as mentioned in the guidelines related to the airway management edited by the French Society of Anesthesia and Intensive Care.² The rotation of the endotracheal tube was also used to help the right positioning of the bevel and facilitate its passage between the vocal cords. Also, as discussed in the article,¹ we believe that, in alive patients, the analysis of the expired gas at the extremity of the catheter may be of great importance to confirm the correct position of the catheter before removing the guide wire.

The literature regarding the retrograde tracheal intubation is sparse, and to our knowledge, there is no study comparing cricoid and subcricoid approaches in terms of complications, morbidity, and mortality. The subcricoid approach, enhancing the distance between the vocal cords and the site of the puncture, has been proposed to decrease the incidence of accidental extubation during retrograde tracheal intubation.³ If the incidence of failure is decreased, the safety of this approach remains questionable. Regarding the cricoid approach, some of the complications have been reported during minitracheostomy,^{4,5} and one can suppose that an incision may be more deleterious than a puncture with a needle and the insertion of a guide

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wire. It is difficult to conclude that the subcricoid approach should be preferred to the cricoid approach for retrograde tracheal intubation, and there are some good reasons to recommend the cricoid approach.⁶ Because of its superficial localization, the cricothyroid membrane is easy to localize, the risk of accidental puncture of the thyroid gland is very low, and finally, the cricoid approach is easy to learn because the puncture of the cricothyroid membrane is used for other purposes, such as cricoid local anesthesia.

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In Line with the Ultrasound Beam, the Third Dimension to Ultrasound-guided Nerve Blocks

To the Editor:—Dr. Gray, and before him Dr. Marhofer *et al.*, described essentially two spatial relations of the nerve stimulator needle to the ultrasound beam for ultrasound-guided nerve blocks^{1,2}:

1. Perpendicular to the ultrasound plane (SAX OOP and LAX OOP)
2. Parallel to the ultrasound plane (SAX IP and LAX IP)

Although both approaches keep the needle orthogonal to the ultrasound beam, we and others use a third orientation, especially for ultrasound-guided supraclavicular blocks:

3. In line with the ultrasound beam (SAX UB and LAX UB)

We advance the needle perpendicular to the skin in the direction of and in line with the ultrasound beam, *i.e.*, at right angle to both above approaches. In real time, we can see the needle penetrate the various tissue planes like a drill. We accept the critique that we may not continuously visualize the needle tip, but from the markings on the needle, we know its depth at all times. Because the depth of the target structure is also exactly shown on ultrasound, we can be sure not to reach structures beyond it. When it comes to supraclavicular

nerve blocks, for example, we are therefore sure not to puncture the pleura, because the nerve bundle of the brachial plexus runs lateral and superficial to artery and pleura, both being visualized.

In particular, because we are going to use three-dimensional ultrasound for needle guidance in the future also in regional anesthesia, we should discuss and use the third dimension.

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In Reply:—I thank Dr. Andreae for his thoughtful comments on the recent Clinical Concepts and Commentary article.¹ He raises two critical issues regarding ultrasound-guided regional block: the method of approach and needle tip visibility. With the in-plane approach, the needle tip and shaft are contained within the plane of imaging. Positioning of the needle parallel to the skin surface (perpendicular to the sound beam) will yield strong specular reflections. Although parallel needle positioning is ideal from a needle visibility standpoint, it is not essential to the in-plane approach because backscatter echoes are still received by the transducer (nonparallel approach is illustrated in fig. 1B of the article).

With the out-of-plane approach, the needle tip crosses the plane of imaging. Skilled operators can often maintain the needle tip within the plane of imaging by sliding and tilting the transducer so as to follow the needle tip as it is advanced. In the nomenclature put forth in the article, the approach described by Dr. Andreae would be considered an extreme example of the out-of-plane approach. One disadvantage of needle entry close to the transducer is that skin displacement by the needle can disrupt the acoustic coupling between skin and transducer (contact artifact). Another problem is the discrimination of tip and shaft echoes.

We have made controlled measurements of needle tip visibility for regional block needles in phantoms that mimic tissue.² Over a range of angles from 0 to 65°, needle tip visibility is reduced at steep angles away from the surface. As Dr. Andreae suggests, the out-of-plane approach is less susceptible to this effect than the in-plane approach. The use of compound imaging technology (steering of the beam to different angles to produce a composite image) will reduce (but not eliminate) the influence of needle angle.³

The dawn of three-dimensional imaging of nerves is now upon us.⁴

David C. Wartier, M.D., Ph.D., served as Handling Editor for this exchange.

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Although it has been used to guide other interventions and can improve needle pass efficiency,⁵ it may not ultimately be embraced by our specialty. The principal limitations include the time for acquisition and rendering of reconstructed images, as well as the interpretation of the display in a setting where the acoustic interfaces do not have marked contrast. This could detract from the dynamic nature of ultrasound guidance for regional block that so many anesthesiologists have found appealing.

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Ultrasound Is Not the Only Technique to Visualize Third Occipital Nerve Blockade

To the Editor:—It is with great interest that we read the article from Eichenberger *et al.*¹ published in the February issue of ANESTHESIOLOGY.

We congratulate the authors for providing another example of the clinical application of high-resolution sonography. However, the authors suggest that this is the only available technique for visualizing this nerve. This is not correct.

Magnetic resonance imaging is also capable of visualizing the third occipital nerve (fig. 1), and magnetic resonance imaging-guided blocks have been used to localize the source of headache pain.^{2,3}

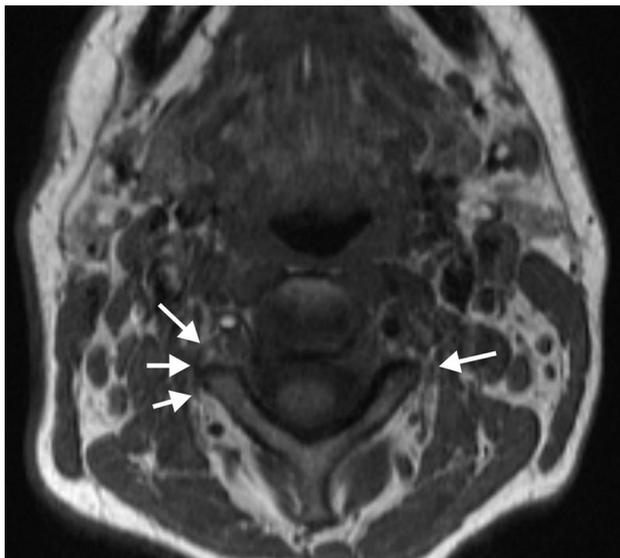


Fig. 1. Transversal T1-weighted three-dimensional fast spin echo sequence with a repetition time of 500 ms, an echo time of 12 ms, an iPAT factor of 2, and a pixel size of $0.6 \times 0.6 \times 1.0 \text{ mm}^3$. The three-dimensional slab contains 40 slices. Arrows show the third occipital nerves on both sides.

In Reply:—We appreciate the interest in our article¹ and the comments provided by Dr. Galiano *et al.* We agree that it may be possible to visualize the third occipital nerve by magnetic resonance imaging too and that ultrasound is therefore not necessarily the only available technique to visualize the third occipital nerve. However, we do not believe that the sources cited by Dr. Galiano *et al.* support the routine use of magnetic resonance imaging. Ultrasound is a better option for pain physicians because magnetic resonance imaging is much more expensive and still not accessible to most practitioners. The findings of our study are not the result of a subjective interpretation of ultrasound guidance. Because fluoroscopy is the current standard to perform medial branch blocks,² we compared our new method with this technique and injected the same amount of local anesthetic, *i.e.*, 0.9 ml to block the third occipital nerve as we do during blocks performed under fluoroscopic guidance.³ Compared with fluoroscopy, our needle tip was found 82% of the time to be in the predefined target zone, and the findings corresponded with the clinical results of the block. As we stated in the article, our method is an encouraging first step: The new technique should be subject of future studies.

Whether simple cadaver or magnetic resonance imaging studies would provide stronger scientific evidence to support the use of a new

Our experience^{4–8} with high-resolution sonography actually makes us believe in the claims by Eichenberger *et al.*¹ But other methods may be equally useful, and additional studies are needed to validate this sonographic approach.

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method, compared with the combined radiologic and clinical control performed in our study, remains questionable.

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Key Components of Risk Associated with Ophthalmic Anesthesia

To the Editor:—I read with interest the closed claims analysis “Injury and Liability Associated with Monitored Anesthesia Care” by Bhananker *et al.*¹ and the accompanying editorial opinion by Hug.² The study indicated that more than one in five monitored anesthesia care claims in the database occurred with patients undergoing elective eye surgery. It also reiterated that the most common causes of patient eye injury and anesthesiologist liability linked to ophthalmic anesthesia consisted of complications related to the eye block and perioperative patient movement. More than four fifths (83%) of ophthalmic anesthesia monitored anesthesia care cases associated with inadequate anesthesia and/or patient movement, either during the block or intraoperatively, resulted in ocular injury and, presumably, poor visual outcome. A previous American Society of Anesthesiologists Closed Claims Project, “Eye Injuries Associated with Anesthesia” by Gild *et al.*³ published in the Journal identified 21 cases of blindness allegedly the result of intraoperative movement during ophthalmic surgery. Movement was the foremost mechanism of injury cited. Five of those claims occurred during regional anesthesia and were attributed to “restlessness” or coughing during the procedure.

Regional anesthesia is a vital part of the scope of anesthesia practice. Because of its safety and efficacy, it is a preferred option for many ophthalmic surgical procedures.⁴ Aside from intraoperative analgesia and akinesia, advantages of conduction anesthesia for ophthalmic surgery patients include suppression of the oculocardiac reflex and provision of postoperative pain relief. In those eye cases where general anesthesia has been the traditional modality of choice, such as open-globe injuries, regional anesthesia may be a fitting alternative when general anesthesia confers an unacceptable level of systemic or ophthalmic risk.^{5,6}

Globe puncture is a dreaded complication of needle-based ophthalmic regional anesthesia. Its incidence varies inversely with education and experience. This is confirmed by a number of previous reports of adverse sequelae by inadequately trained/educated anesthesia personnel.⁷⁻⁹ As noted in a previous letter to the Journal, no formal training or education in ophthalmic regional anesthesia is provided to anesthesia residents in the majority of programs.^{10,11} Anesthesiologists can acquire these skills *via* university programs, Refresher Courses, and workshops at the annual American Society of Anesthesiologists meeting or through an organization such as the Ophthalmic Anesthesia Society. In addition, newer ophthalmic anesthesia techniques may minimize the risk of iatrogenic globe puncture. Ultrasound guidance allows for direct visualization of the needle, whereas sub-Tenon regional anesthesia replaces needles altogether with blunt cannulas.^{12,13}

Topical anesthesia has gained acceptance for surgical procedures of the anterior segment of the eye. Its use, particularly for cataract operations, has surged in recent years.¹⁴ Topical anesthesia does not render the eye akinetic, and requires the patient to focus on the microscope light. Because oversedation may precipitate patient movement and depth of analgesia may be less than with traditional regional anesthesia techniques, the term “vocal local” has been used to describe the occasional reality of ophthalmic anesthesia *via* topical anesthesia and minimal sedation.¹⁵

Regional and topical anesthesia for ophthalmic surgery are certainly not without inherent risks. Unlike general anesthesia, these techniques mandate patient cooperation. Because the majority of ophthalmic surgical cases are elective, the article by Bhananker *et al.*, as well as others, attests to the wisdom of postponing surgery until such time that the patient is in optimal condition to remain still if an increased risk of perioperative movement is noted during the anesthesiologist's preoperative assessment.^{13,16}

Patient movement during block or intraoperatively due to cough, fluctuating levels of consciousness, rebreathing of carbon dioxide under occluded drapes, or restlessness with prolonged duration of surgery can induce dire visual consequences. Deliberate patient selection and judicious choice of suitable anesthesia technique is requisite to determine the optimal anesthesia care prescription.

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The above letter was sent to the authors of the referenced articles. The authors did not feel that a response was required.—Michael M. Todd, M.D., Editor-in-Chief

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Light Wand: Showing the Way

To the Editor:—A 21-yr-old woman with stricture esophagus after ingestion of a corrosive 3 yr previously was scheduled to undergo esophagocoloplasty. Anesthesia was induced in a standard manner, and the trachea was intubated with a 7.5-mm-ID cuffed polyvinyl chloride tube. After mobilizing the colon in the abdomen, an incision was made on the left side of the neck to open the esophagus for anastomosis. However, the surgeon had difficulty in identifying the esophagus. Insertion of a nasogastric tube was not successful, possibly because of extensive scarring in that region. At that time, a light wand (Surch-lite, Orotracheal Lighted Intubation Stylet; Aron Medical, St. Petersburg, FL) was introduced into the oral cavity under direct vision using a laryngoscope and was negotiated behind the laryngeal opening. It was gently pushed in the region of the upper end of esophagus, which appeared deformed because of extensive scarring. A glow of light was seen through the cervical incision. When the light wand could not be advanced further, a nick was made by the surgeon at the

center of the glow to open up the esophagus. Thereafter, anastomosis was established between the colon and the upper end of the esophagus uneventfully.

The light wand is a malleable bougie-like device with a light source at the distal end that is operated through a switch located at the back of the handle. It is used for tracheal intubation without performing direct laryngoscopy.

We wish to highlight that the light wand can be helpful in identifying the esophagus in the operative area in the neck. We have used it in five patients who have undergone esophagocoloplasty and pharyngocoloplasty. This device is simple to use, is safe, and is commonly available in most operating rooms. We recommend its use in patients where identification of the esophagus is difficult.

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Pseud thrombus in the Aorta

To The Editor:—Intraoperative transesophageal echocardiography is a helpful monitoring device during cardiac surgery, but possible artifacts sometimes lead to critical misinterpretation. We experienced a rare artifact in a patient undergoing emergent mitral valve replacement.

A 61-yr-old man (height, 165 cm; weight, 63 kg) with no previous medical history came to our hospital reporting dyspnea. Transthoracic echocardiography demonstrated papillary muscle rupture of the mitral valve and severe mitral valve regurgitation but otherwise no remarkable findings. Intraaortic balloon counterpulsation and percutaneous cardiopulmonary support had been established before the patient entered the operating room because blood pressure could not be maintained with continuous infusion of catecholamine. The flow of percutaneous cardiopulmonary support was approximately 3.8 l/min, and augmented systolic blood pressure was approximately 120 mmHg. Figures 1 and 2 demonstrate transesophageal echocardiographic views of the aorta soon after induction of anesthesia. A high-echoic mass almost occupying the sinus of Valsalva was recognized. There was narrow retrograde blood flow through the center of the mass. (Additional information regarding this is available on the ANESTHESIOLOGY Web site at <http://www.>



Fig. 1. Longitudinal transesophageal echocardiographic view of the aorta. A high-echoic mass-like object almost occupies the sinus of Valsalva. Arrow = ruptured papillary muscle. Ao = ascending aorta.

anesthesiology.org.) At that time, no pathologic electrocardiographic change was observed. We considered it to be a large thrombus due to insufficient anticoagulation, so the aorta was incised immediately after conventional extracorporeal circulation was established. The surgeon inspected above and beneath the aortic valve; however, no thrombus could be found. The rest of the surgical procedure was uneventful, and the patient was discharged from the hospital without neurologic complication.

In retrospect, it is speculated that the “mass” was displayed by the extremely stagnant blood flow in the sinus of Valsalva. No one present could insist confidently in the imminent situation that it was an artifact. We regret that we did not follow the image continuously until aorta incision.

Additional material related to this article can be found on the ANESTHESIOLOGY Web site. Go to <http://www.anesthesiology.org>, click on Enhancements Index, and then scroll down to find the appropriate article and link. Supplementary material can also be accessed on the Web by clicking on the “ArticlePlus” link either in the Table of Contents or at the top of the HTML version of the article.

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Fig. 2. Cross-sectional transesophageal echocardiographic view at the level of the aortic root. LA = left atrium.

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An Alternative Approach to Ganglion Impar Neurolysis under Computed Tomography Guidance for Recurrent Vulva Cancer

To the Editor:—Cancer pain arising from the pelvic viscera can be a devastating manifestation of advanced malignancy. When conservative therapies are inadequate or associated with intolerable side effects, interventional neurolytic therapy should be considered. We describe an alternative technique to blockade of the ganglion impar using computed tomography (CT) guidance through a lateral approach.

A 76-yr-old woman with recurrent squamous cell carcinoma of the vulva was referred to the pain management service. Diagnosis was first made in 2004, and she had undergone radiation therapy and chemotherapy in the same year. This was followed by a modified left hemivulvectomy and a modified radical vulvectomy in July and December 2005, respectively, for recurrent disease. She presented 4 months later with worsening rectal and vaginal pain. Perineal pain was aggravated by defecation and micturition. Physical examination revealed a fungating lesion involving the perineum and biopsy was consistent with recurrent cancer. Pain was not adequately controlled with high doses of opioid medication. CT pelvis demonstrated soft tissue thickening and irregularity extending from the vulva to the anus. The patient consented to a neurolytic block of the ganglion impar under CT guidance.

With the patient positioned prone on the CT scanner, preliminary axial scout images were obtained to identify the sacrococcygeal junction. Two entry points approximately 10.5 cm lateral to the midline on the left and right were marked on the skin. After preparing a sterile field, local infiltration with 1% lidocaine was given. Under intermittent CT fluoroscopic guidance, a 22-gauge 5-inch spinal needle was introduced *via* a lateral approach such that the tip of the needle approached the region of the ganglion impar. A second 22-gauge 5-inch spinal needle was then introduced from the contralateral side to bring the needle tip near the location of the ganglion impar. Iodinated contrast, 0.2 ml, was injected, and CT images were obtained to confirm correct needle placement (fig. 1). A prognostic block using 2 ml bupivacaine, 0.5%, was injected through each needle. Ten minutes later, the patient reported a

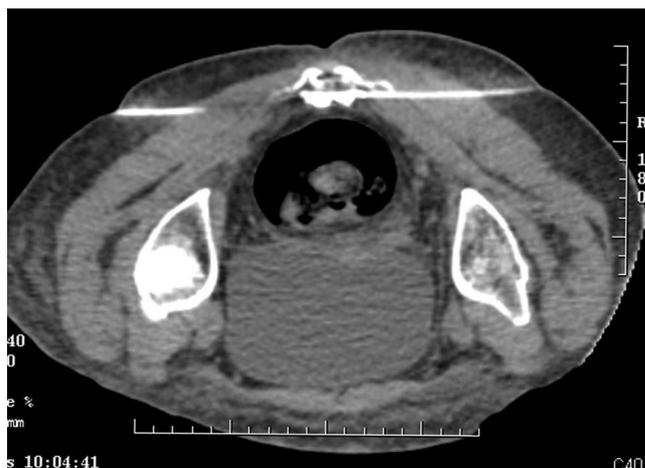


Fig. 1. Injection of contrast dye to confirm correct placement of the needle tip anterior to the sacrococcygeal junction.

significant reduction in pain intensity from 8 on the numeric rating scale (0 being “no pain” and 10 being the “worst imaginable pain”) to 2. Gentle palpation over the sacral region did not elicit any pain. A 4-ml mixture containing 1 ml bupivacaine, 0.5%, and 3 ml alcohol, 100%, was then injected through each needle. Both needles were cleared with 1 ml lidocaine, 1%. The patient tolerated the procedure well, with no complications. After the procedure, she continued to have good pain relief and also reported a decrease in pain on defecation and micturition.

The ganglion impar is the terminal portion of the sympathetic chain located anterior to the sacrococcygeal junction. It is a solitary ganglion that is formed by merging of the left and right sympathetic chains inferiorly. Ganglion impar blocks are effective for treating visceral pain originating from the perineum.¹ Different techniques to block the ganglion impar have been reported. Plancarte *et al.*² first described the anococcygeal approach to the ganglion impar using a bent needle. This was later modified to a curved needle

Support was provided solely from institutional and/or departmental sources.

technique by Nebab *et al.*³ The ganglion impar can also be reached by passing a needle through the sacrococcygeal junction.^{4,5}

Computed tomography-guided neurolysis of the ganglion impar is an alternative to performing the procedure under fluoroscopy. Using a bilateral approach, two needles were advanced from the lateral side of the sacrum horizontally until the needle tips were positioned anterior to the sacrococcygeal junction. This avoided the risk of rectal perforation in the anococcygeal approach and the risks of infection, bleeding, and needle breakage in the sacrococcygeal approach. The needles pass through skin, subcutaneous tissues, and muscles only, and it can be almost pain free with adequate local anesthesia. This technique is relatively easy and safe to perform.

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Neuromuscular Blockade Monitoring Complicated by the Unknown Preoperative Cosmetic Use of Botulinum Toxin

To the Editor:—The ever-increasing popularity of facial botulinum treatment underscores the importance of obtaining a complete and accurate history before administration of paralytic agents. We report a case of a man undergoing laparoscopy during general anesthesia with an unknown history of facial botulinum injections, and the inability to accurately assess level of paralysis using facial nerve stimulation.

A 72-yr-old, 60-kg man with a preoperative diagnosis of small bowel obstruction was scheduled to undergo an exploratory laparoscopy. The patient's medical history was significant for diverticulitis, which was treated with colon resection; hypertension; and coronary artery disease, which was treated with angioplasty 15 previously. There was no other systemic disease present. On physical examination, there were no abnormalities. After rapid sequence induction with 150 mg propofol and 100 mg succinylcholine, anesthesia was maintained using isoflurane and then desflurane. The patient received a total of 8 mg vecuronium during the remainder of the case for muscle relaxation. The surgeons performed lysis of adhesions, and the operation lasted approximately 2 h. Upon closing the abdominal fascia, the surgeon stated that the patient's muscles were not relaxed, making closure difficult. Sixty minutes had elapsed since the last dose of vecuronium (2 mg) was administered. Using a peripheral nerve stimulator, train-of-four (TOF) was assessed at the orbicularis oculi muscles bilaterally, and no twitches were noted. The nerve stimulator leads were then placed over the ulnar nerve to assess recovery of the adductor pollicis muscle, and there was indeed recovery of TOF, with a ratio greater than 0.7. The patient was given a small dose of vecuronium, and only one twitch was subsequently observed. After the surgery was completed, there were three TOF visible twitches from the adductor pollicis muscle, and the paralysis was reversed with 5 mg neostigmine and 1 mg glycopyrrolate. The patient emerged from anesthesia smoothly and was extubated in the operating room. It was realized that the patient had appeared younger than his stated age, and it was then considered that the patient may have had cosmetic treatment, which would have

affected the musculature of his face and possibly reduced the response to TOF stimulation. On postoperative questioning, the patient indeed confirmed a history of botulinum toxin injections to the upper facial muscles, 4 weeks before this surgery.

In our case, the depth and recovery of neuromuscular blockade was assessed using TOF stimulation with a peripheral nerve stimulator. The patient's use of botulinum toxin around the orbicularis oculi muscles interfered with accurate assessment of muscle paralysis. Normally, the orbicularis oculi muscle has a shorter latency and faster recovery to TOF ratio of 0.80, compared with the adductor pollicis muscle.¹ However, botulinum toxin had denervated the patient's facial muscle fibers, and no twitches could be elicited from stimulation of the orbicularis oculi muscles. This was demonstrated by the simultaneous presence of four visible twitches at the adductor pollicis site after TOF stimulation. This case demonstrates that botulinum toxin injections may interfere with the monitoring of neuromuscular blockade. It may indicate a higher degree of neuromuscular block than is actually present. Botulinum toxin may lead to a significant flaccid paralysis for months after injection. The exponential growth in the use of botulinum toxin for cosmesis should prompt the anesthesiologist to inquire about the use of botulinum toxin in patients who appear significantly younger than their actual age. Possible courses of action would be to obtain a baseline TOF before administration of neuromuscular blocking drugs, and to check an alternate site if a deeper-than-expected block is observed.

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