To the Editor:—Firth and Head\(^1\)\(^2\) are to be congratulated for providing a comprehensive review of the pathophysiology of sickle cell disease and erudite discussion of its implications for anesthesiologists. We also appreciate their attempts to apply evidence-based knowledge to our understanding of the perioperative care of these patients. Although this will undoubtedly become a valuable resource for anesthesiologists, we are compelled to provide some corrections in the text and tables as well as provide alternative interpretations of some of the evidence.

The authors refer to a randomized no-transfusion group in their discussion of Vichinsky et al. (reference 98, page 776).\(^2\) In fact, there was not a randomized no-transfusion group. The authors seemed to have missed the fact that this report and several more by the same authors were reports of subpopulations of the work by this group. In this and subsequent articles from the Preoperative Transfusion in Sickle Cell Disease Study Group as initially reported by Vichinsky et al.,\(^3\) the groups were the same (described below). Furthermore, the second to last sentence in the third paragraph on page 776 should read, ‘This [acute chest syndrome] occurred in 21% of cases in both the aggressive transfusion and the nonrandomized nontransfusion group, 8% in the conservative transfusion group, and 3% in the nonrandomized transfusion group,’ as described in table 4.

In table 4 on page 775, regarding Haberkern et al., \(^1995,\)\(^4\) the numbers and percents for ‘cholecystectomies, complications’ for the four groups are in fact the numbers of patients in the groups and the percents of sickle cell events, not the numbers and percents of complications. (This study was actually published in 1997.) The percent-ages of total complications and acute chest syndrome in the four groups (as listed) are in fact as follows: group 1, randomized aggressive transfusion: 36%/9%; group 2, randomized simple transfusion: 39%/11%; group 3, nonrandomized nontransfusion: 43%/19%; and group 4, nonrandomized transfusion: 41%/7%. These groups are the same in all of the studies reported by the Preoperative Transfusion in Sickle Cell Disease Study Group. These corrected data underscore concerns regarding the risk of perioperative complications in the nontransfusion group.

In the discussion of Griffin and Buchanan,\(^5\) the authors concluded that ‘any potential benefit from transfusion would therefore be low and risks of transfusion were not justified for minor procedures.’ However, the actual conclusion from this report stated that “operative transfusions might be avoided in children with sickle cell disease who undergo most minor surgical procedures.” The overall complication rate was 26%, thoracotomy/laparotomy 50%, tonsillectomy and adenoidectomy 56%, others 5%. This report neither provided evidence to withhold transfusion in any group nor lobbied against transfusions.

In their table 5 on page 777 (Guidelines for the use of Perioperative Prophylactic Erythrocyte Transfusion), the foundation for this table is not clear and certainly not evidence based. It suggests guidelines for perioperative transfusion that are misleading given the absence of prospective, randomized data to support a nontransfusion approach.

We acknowledge the lack of a proven causal relation between hypoxia, dehydration, and hypothermia and sickling events in the perioperative clinical setting (page 782). However, in the context of sickle cell disease, we are compelled to prove that no such relation exists before abandoning practices that have been associated with decreased perioperative morbidity and mortality in these patients. A conservative approach to children with sickle cell disease in the perioperative period has been and continues to be adequate hydration and correction of anemia. To propose a therapeutic nihilistic approach to the treatment of these patients in the absence of substantive evidence is dangerous. Despite the best care today, the perioperative mortality rate in patients with sickle cell disease of 1 in 100 is severalfold greater than that in nonsickle adults, approximately 1 in 300,000, and in nonsickle children, 1 in 50,000–80,000. Before our current practice patterns for these children are changed, prospective randomized studies that examine anesthetic practices in this and other diseases should be conducted.

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problems.

With respect to the work of Griffin and Buchanan, the correspondents seem to have formed their opinions from the abstract summary. We refer the correspondents to the tables, text, and prominent citations discussed by Griffin and Buchanan. The complications comprising the rates cited by the authors were largely atelectasis and transient, uncomplicated postoperative pyrexia, which the investigators emphasized were not specific to sickle cell disease. Again, it is misleading to cite the incidence of a heterogeneous group of complications in a heterogeneous population as motivation for an intervention against the lower occurrence of a specific subset of complications in a specific subpopulation. The incidence of complications specific to sickle cell disease in the relevant minor procedure subpopulation was, as reported, 2% (n = 1 in 46). The sole patient with the sickle complication of acute chest syndrome had significant preexisting cardiopulmonary dysfunction. The researchers cited these facts in their extensive discussion of the limits of any potential role for transfusion. They concluded, "our data support the concept that preoperative blood transfusions may be unnecessary for children with sickle cell disease... undergoing most minor operations (who) might therefore be spared the cost, inconvenience, and risks of infection, alloimmunization and transfusion reactions inherent in RBC transfusions" (page 685).

In response to the suggestion that table 5, Guidelines for the Use of Perioperative Prophylactic Erythrocyte Transfusion, from the original article is not evidence based, we refer the correspondents to the text of the review and relevant selected references. Some of the rationale and evidence that simple transfusion be avoided in low-risk situations has been reemphasized above. As the correspondents' call for prospective randomized trials acknowledges, the evidence for the efficacy of simple transfusion in intermediate-risk situations is incomplete, and we cannot therefore say conclusively that transfusion is or is not indicated. Our guidelines for high-risk cases were based on a systematic review of the primary neuroanesthetic and cardiothoracic literature of the preceding four decades, although we limited our citations of this fragmentary evidence to a short selection at the request of the Anesthesiology reviewers. We concede that our guidelines for transfusion in uncomplicated pain crises are not evidence based—simply because we are unaware of conclusive evidence to support or refute the practice of transfusion. We cannot advocate an intervention in the absence of supportive evidence and concur with authoritative peer opinion that transfusion is not indicated. A detailed critique of practice based on the largely uncontrolled data on transfusion for acute chest syndrome was beyond the scope of an already lengthy review. The guidelines on acute chest syndrome are consequently limited to the well-documented evidence that transfusion can improve arterial hemoglobin oxygenation, a predictable and possibly nonspecific physiologic consequence of increasing mixed venous saturation and pulmonary capillary transit time by correction of anemia in the face of pulmonary shunting and impaired gas exchange. The guidelines are therefore based on what data are available. Because this evidence is incomplete, we simply provided guidelines, rather than making more prescriptive recommendations.

The correspondents state that we are compelled to prove that no causal relation exists between hypoxia, dehydration, and hypothermia and acute perioperative complications before abandoning practices associated with decreased perioperative morbidity and mortality. The only definitive way to do this, subjecting patients to these injuries in a well-constructed study, is practically difficult and ethically impossible. The studies cited, including exposure to inhalational and hypobaric hypoxia, the use of occlusive arterial tourniquets, and the coexistence of sickle cell disease and cyanotic heart disease, strongly suggest that hypoxia-induced sickling (and by extension acute cellular dehydration) are not triggers of acute complications. The relevant practices that are associated with an apparent decrease in morbidity and mortality are probably the overall general improvements in basic anesthetic and perioperative care, not specific unproven deviations from standard practice. We do not advocate therapeutic nihilism, as the correspondents aver, but rather, we encourage adherence to these basic standards of anesthetic care.
them to long-established alternative approaches of avoiding perioperative transfusion. For evidence of harm, consider a recent study of 150 multiply transfused American patients that found an incidence of hepatitis C infection of 35.3%, a sobering demonstration of iatrogenic injury. By contrast, in Jamaica, where transfusion practices are far more conservative, a study of 250 patients documented an infection rate of 2%. Rather than preventing acute sickle problems, transfusion can actually precipitate acute pain and pulmonary complications. We do not suggest that our review is the last word on management or even that the model we outline explains all aspects of sickle cell disease. We therefore strongly support the call for prospective randomized studies of anesthetic practice for sickle cell disease. Given the proven potential for iatrogenic injury, we urge the correspondents to consider the cautious methods of others and, in the absence of conclusive evidence, not to abandon a well-established practice: First do no harm.

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References


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Circadian Influences, Low-dose Isoflurane, and the Ventilatory Response to Hypoxia

To the Editor.—Pandit et al.1 are to be congratulated on their study on the effects pain and audiovisual stimulation on depression of the acute hypoxic ventilatory response by low-dose halothane. Their results are in good correspondence with some of the key studies in this complex field of research. As stated by the authors, there is now ample evidence for the existence of quantitative differences in the ability of low-dose inhalational anesthetics to depress the ventilatory response to acute hypoxia in humans. For example, 0.1% end-tidal halothane depresses the response by 50–60%, whereas the same concentration of isoflurane has much less of an effect (reduction 30–40%). The authors discuss several explanations for the observed differences between halothane and isoflurane, such as differences in pharmacokinetics, differences in the production of reactive oxygen species, and differences in their interaction with sites in the central nervous system involved in behavioral control of breathing. Evenly important are issues related to methodology. Often, very small differences in protocols may cause large differences in study outcomes. I would like to give an example of the latter. In three subjects, the ventilatory responses to hypoxia at three time points on one single day were measured: 8:00 AM, noon, and 4:00 PM. At 9:00 AM, one additional response during inhalation of 0.2% end-tidal isoflurane was obtained. The ventilation (Vi) response to five end-tidal PO2 levels was analyzed using the following equation: Vi = G exp(D PTO2) + y0 (G is the hypoxic sensitivity, D is a shape parameter, and y0 is ventilation at hyperoxia). Control and recovery responses varied considerably by 20–30% for parameter G and 30–40% for parameter y0 (fig. 1). Consequently, the depression of the isoflurane response relative to the control and recovery responses was evenly variable and varied from 50 to 70%. The picture that emerges is that the circadian rhythm has important influences on the ventilatory response to hypoxia and consequently on the influences that low-dose anesthetics have on the response. How the behavioral control system interacts with circadian influences remains unknown. Although I realize that the study I present here is of small sample size, it points toward (1) an important and complex role for circadian

Fig. 1. Ventilatory response to hypoxia obtained at five oxygen levels in a 25-yr-old woman. Nondrug studies were performed at 8:00 AM, noon, and 4:00 PM. At 9:00 AM, the effect of 0.2% end-tidal isoflurane was measured. The lines through the data are curve fits to the data using the following equation: Vi = G exp(D PTO2) + y0. The variability among control and recovery responses is apparent. PO2 = partial pressure of oxygen.

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In Reply—I thank Dr. Dahan for his interest in my article. He presents data from three subjects (he acknowledges that this is a very small sample) that show some “within-day variation” in the acute hypoxic ventilatory response (AHVR). The figure he presents for one subject indicates that minute ventilation in euoxia is lowest in the morning, highest at midday, and in-between in the afternoon. I do not know whether this same pattern is the same in all subjects. The minute ventilation in euoxia was as high as approximately 20 l/min, and this implies either that the end-tidal partial pressure of carbon dioxide (Pco2) of Dahan’s subject varied widely during the day or that, if end-tidal Pco2 was constant, the metabolic CO2 output of the lung must have varied widely during the day. The AHVR variation must therefore have correlated with end-tidal Pco2 and/or CO2 output in Dahan’s small series. However, neither Sahn et al.2 nor Zhang and Robbins3 were able to find such correlations.

Nonetheless, the within-day variation reported by Dahan may indeed be a “circadian” influence, and I agree that the observation needs further study. Variation in the AHVR within individuals on repeat testing is well established,4 but it seems that between-day variation is greater than within-day (circadian) variation.5 The study of Zhang and Robbins5 sheds important light on the issue of variation. They found that the method of inducing hypoxia (they studied square wave hypoxic input, incremental hypoxic steps, and simulated rebreathing) did not influence the AHVR measured. So it seems that the methodological influences to which Dahan refers may not be as influential as we all (intuitively) might think them to be. This is perhaps further supported by my analysis that there is no actual difference in the result of studies using rebreathing as compared with those using step hypoxia in assessing the reduction in AHVR by low-dose anesthetic.5

Zhang and Robbins5 also found that between-day variation was greater than within-day (circadian) variation, so although the within-day variation found by Dahan is of interest, it is possible that between-day variation is more important.

There are two practical questions: (1) Does this variation in AHVR impact upon the results of the relevant studies, and (2) how can we account for it or control for it experimentally?

With regard to the first question, if the variation in AHVR influenced the outcome of studies, it might prove difficult (because of the large confidence intervals) to show, for example, that anesthetics blunt AHVR. However, this is not the case, and many different studies consistently find that halothane, enflurane, and sevoflurane reduce AHVR. The only agent for which there is less consistency in different studies’ results is isoflurane. Although this may be a result of inherent variation in AHVR, it is also possible (and perhaps more likely) to be due to some property of isoflurane that gives it a more variable effect than other agents.

With regard to the second question, there seem to be two general ways to control for variation. One is to conduct each experimental period in a study at precisely the same time of day. However, this does not control between-day variation (which seems more important). The second way is to conduct experimental periods at random times of day in a suitable number of different subjects, ideally using repeated experimental periods in the same subject, and then average the results to reduce any systematic variation. One problem is that repeated exposure is often (ethically) undesirable in anesthetic studies, but where possible, I prefer this second approach.

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References


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the SCV approach. However, we would like to make a few comments regarding the article.

First, the longest overlap length between the lower border of the clavicle and the SCV in the inner third of the clavicle was one of the main reasons for quoting the lowered shoulder position. However, a needle path aiming at the suprasternal notch may cross the lower border of the clavicle somewhere between the midclavicular line and the inner third of the clavicle. In addition, the shaded overlap area in figure 1 of the article extends out of the SCV, even to the innominate vein.

Second, one of the most important factors for successful catheterization is probability of contact between the puncture needle and the SCV. Because the cross-sectional area rather than the diameter on a plane is more important for the probability of contact, the cross-sectional area of the SCV beneath the inner third of the clavicle should be regarded as a major factor. If the cross-sectional area is not available, there may be another way of analyzing the probability of contact. Overlap length of the SCV on an imaginary line drawn between the needle entry point and the midportion of the clavicular head seems to be more relevant than that on the lower border of the clavicle. In figure 1 of the article, we can realize that the neutral and lowered positions are comparable, but the elevated position has the shortest overlap length on the imaginary line.

Third, with regard to the small clinical trial, why were the neutral and lowered positions not compared? Where was the catheter tip located? Were there any catheters directed to the internal jugular vein (IJV)? As explained in figure 4 of the article, the angle formed between the SCV and the innominate vein becomes narrower with the shoulder lowered. Considering the fact that, in children, a right SCV catheter is frequently positioned in the IJV because the angle formed between the SCV and the innominate vein is more acute, there seems to be a risk of directing the SCV catheter into the IJV when the shoulder is lowered.

Last, changing the shoulder position after successful venipuncture, as suggested in the article, carries the risk of withdrawing the puncture needle out of the vein. Even changing the shoulder position after advancing the guide wire may not be helpful, because a guide wire may already be directed to the IJV because of the short distance between the SCV puncture point and the confluence of the IJV and the SCV. If SCV catheterization is attempted with the shoulder lowered, tilting the head toward the catheterization side without movement of the shoulder may be more helpful in reducing the incidence of catheter malposition into the IJV.

All the above-mentioned concerns should be clarified before deciding whether we should adopt the lowered shoulder position during SCV catheterization.

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References

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In Reply: We thank Drs. Bahk and Ryu for their interest in our article and valuable comments. We would like to take the opportunity to address the issues raised by their insights, point by point.

First, the longest overlap length between the lower border of the clavicle and the subclavian vein (SCV) in the inner third of the clavicle was cited as one of the principal reasons for recommending a lowered shoulder position. Drs. Bahk and Ryu discuss concern about cases in which a needle may cross the lower border between the midclavicular line and inner third of clavicle. However, a lowered shoulder seems to offer a more appropriate position than other shoulder positions, because overlap increases with extension to the lateral side from the inner third of the clavicle, as described in our article. The lowered shoulder also increases the proximity of the SCV to the undersurface of the clavicle. This allows reliable SCV puncture and reduces the risk of complications such as pneumothorax during the use of basic SCV puncture technique (needle advancement contacting the undersurface of the clavicle), because the needle is not advanced beyond the necessary depth. The shaded area in figure 2 of our article extends from the SCV to the innominate vein because we were using the definition of overlap described by Land as the area of clavicle overlapping with the SCV, including the innominate vein.

Second, although we assessed probability of contact between the needle and the SCV using the longest and shortest diameters of the vein, assessment using SCV cross-sectional area may be more suitable, as indicated by Drs. Bahk and Ryu. We therefore reevaluated contact probabilities using the products of both halves of the longest and shortest diameters and the circular constant, expressing the area of an ellipsoid, and then compared values among the three shoulder positions, because the cross-sectional area of the SCV can be regarded as an ellipsoid, as indicated in figure 2 of our article. The result was $A_{\text{neutral}} = A_{\text{up}}$, being consistent with the results as assessed using SCV diameters, resulting in being substantially comparable in the contact probability among the three shoulder positions. Although we assessed overlap using the same methods described by Land and Tan et al. as mentioned before, we agree with the proposal by Drs. Bahk and Ryu that overlap length of the SCV on an imaginary line drawn between the needle entry point and the midportion of the clavicular head is more relevant than using a point on the lower border of the clavicle. We are grateful to both doctors for making this recommendation.

Regarding the third and fourth comments made by Drs. Bahk and Ryu, the small number of patients participating in this clinical trial limited comparisons of SCV cannulation success rates between elevated and lowered positions only, where comparisons were expected to identify the most marked differences. From our experience, we believe that a clinical trial with a sufficient sample population will confirm the superiority of a lowered shoulder position over a neutral position in terms of success rates for SCV puncture. However, this issue must be clarified in a randomized clinical trial in the future. All catheterizations performed in the present trial were inserted into the right SCV, and no catheters were directed into the internal jugular vein. With our procedure inserting a catheter or guide wire into the SCV, advancement after a change in shoulder position from lowered to neutral may contribute to leading the catheter or guide wire toward the innominate vein. In addition, although Drs. Bahk and Ryu express concern regarding the risk of withdrawing the puncture needle from the SCV during the change in shoulder position after successful venipuncture, we believe that the risks associated with the procedure in actual practice are not as large as they suggest. Even if shoulder position changes from a lowered to a neutral or even a slightly elevated position, we have experienced minimal movement of the puncture needle. This is due to movement of the needle and syringe in an integrated manner along with the clavicle and surrounding tissue and assessed using SCV diameters, resulting in being substantially comparable in the contact probability among the three shoulder positions. Although we assessed overlap using the same methods described by Land and Tan et al. as mentioned before, we agree with the proposal by Drs. Bahk and Ryu that overlap length of the SCV on an imaginary line drawn between the needle entry point and the midportion of the clavicular head is more relevant than using a point on the lower border of the clavicle. We are grateful to both doctors for making this recommendation.

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In conclusion, we believe that the information presented in our article is sufficient for those using small-caliber subclavian access needles for central venous access in children.
CORRESPONDENCE

Anesthesiology 2005; 103:210–1

Myocardial Performance Index and Tissue Doppler Systolic Wave Velocity Are Preload Dependent

To the Editor:—We read with great interest the article about myocardial performance index and tissue Doppler imaging in patients with single ventricles, comparing two anesthetic regimens, sevoflurane and fentanyl–midazolam.

The authors found no change in myocardial performance index, which is a global index of both systolic and diastolic function. However, they describe a significant decrease of both myocardial Doppler imaging systolic (Sm) and early diastolic (Em) wave velocities from baseline to dose 1 and dose 2 with fentanyl–midazolam (table 3 in their article). Nevertheless, both (neo)atrial flow and time–velocity integral decreased significantly.

An important limitation of this study, the preload dependency of both parameters used, was not fully discussed. It has to be speculated that anesthetics, in a setting as used in this study, although not shown, induce major changes in loading conditions. Assessment of ventricular function implies that load-independent methods should be used. It has been indirectly suggested in the literature that myocardial performance is preload dependent: Broberg et al. described a close relation between this index and dp/dmax, the latter being strongly preload dependent. Recently, this preload dependency was also suggested by our group when we described a close relation between myocardial performance index and preload adjusted maximal power.

In addition, the same problem arises with the systolic flow wave velocity of the tissue Doppler imaging. Our group recently showed clearly that this flow wave velocity is load dependent. Therefore, a decrease in the length of the myocardial fibers due to a decrease in EDV will lead to decreases in stroke volume, the velocity of shortening, and systolic tissue velocity obtained with tissue Doppler imaging.

Echocardiographic parameters as myocardial performance indexes and systolic Doppler myocardial velocity must be used cautiously in view of various physiologic and pathophysiologic interfering factors, in particular when one is assessing hemodynamics in conjunction with various dose regimens of anesthetics.

References


(Accepted for publication March 28, 2005.)

In Reply:—The load dependence of the myocardial performance index in congenital heart disease has been evaluated. No significant change was found before or after surgical correction of atrial septal defect (right ventricular preload), pulmonic stenosis (right ventricular afterload), or congenitally corrected transposition of the great vessels (right ventricular preload and afterload). Acute changes in myocardial performance index with manipulation of preload/afterload have been demonstrated in adult volunteers with healthy hearts—to Valsalva maneuver, leg lifting, and nitroglycerine administration. However, the patients in this study with previous myocardial infarction and abnormal ventricular function did not exhibit any changes in myocardial performance index.

Doppler tissue imaging has load dependence but has been demonstrated to be more independent of preload than conventional Doppler measures of mitral inflow. In addition, in patients with ventricular dysfunction, changes in preload affect Doppler tissue imaging velocities much less and correlate well with invasive measures of left ventricular diastolic pressure. Significant increases in afterload seen in patients with aortic stenosis do decrease Doppler tissue imaging velocity. However, no significant impact on Doppler tissue imaging
To the Editor:

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Transsacroccocygeal Ganglion Impar Block for Postherpetic Neuralgia

To the Editor—I read with interest the case report about sacral postherpetic neuralgia and the excellent results in its treatment using the paramedial approach to the ganglion impar.1 The authors included in their discussion the different approaches that have been undertaken in the past to block the ganglion impar. It was also impressive to see the double-bent needle used to achieve this block. Many practitioners have elected not to perform this block because of their impression of its complexity and technical difficulty. Unfortunately, this has led to a decrease in popularity of this effective block in pain medicine.

In the past, I described a case in which coccydynia was controlled with the blockage of the ganglion impar through the sacrococcygeal junction.2 The authors briefly mentioned this technique, but I am afraid their description could be interpreted to mean that this approach is of minimal value. They state, “This approach can be useful in patients with normal anatomy but may prove challenging in patients with arthritic changes in the bones and calcification of the ligaments of the sacrum and coccyx.” I have to disagree with the authors and would like to clarify that this approach is quite simple, straightforward, and equally effective. Minimal arthritic changes occur in this area, and, if any, the bone changes seldom are major challenges with this approach. I believe that the message we should be sending to practitioners is that ganglion impar block could be performed through an easily achieved technique under fluoroscopy and that it is effective in the treatment of coccydynia.


4. de Leon-Casasola OS: Transsacroccocygeal ganglion block for analgesia after coccygectomy. Anesthesiology 1998; 89:1157-60


management of sacral pathologies, including postherpetic neuralgia. It is discouraging to see a good intervention fade into disfavor because of the complexity of the technical aspect of its performance when a technically simple alternative is present and equally effective.

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References


Ganglion Impar Block in Noncancer Perineal Pain: What Drugs, What Strategy?

To the Editor—We read2 with great interest the recently published case report by McAllister et al.1 describing the successful management of postherpetic neuralgia in the perineal area using repeated ganglion impar blockade by a local anesthetic solution. This is one of the first case reports describing the efficacy of blockade of the terminal part of the sympathetic chain in non–cancer-related pain. We congratulate the authors on their therapeutic success, but several questions remain to be answered.

Ganglion impar blockade is not a routinely used analgesic procedure. Initial articles have reported chemical neurolysis of the ganglion impar as adjuvant therapy of cancer-related pain in the perineal region. In their overview, De Medicis and de Leon-Casasola provide a summary of studies evaluating the efficacy and complication rate of ganglion impar blockade.2 They conclude there have been only two studies reporting good efficacy of neurolytic blockade using 6% phenol for visceral perineal pain of cancer origin in a total of 36 patients. There is only one study addressing the efficacy of ganglion impar inhibition in noncancer pain. We congratulate the authors on their therapeutic success, but several questions remain to be answered.

In our experience with the management of noncancer perineal pain in 26 patients with chronic pain after perineal surgery, vulvodynia, and vulvar pruritus to date, we are able to make a preliminary outcome determination using the ganglion impar procedure: A testing blockade with a local anesthetic has an analgesic efficacy of 2–5 days. Multiple blockade was accomplished in these patients using a mixture of a local anesthetic with clonidine (10 ml bupivacaine, 0.375%, plus 75 μg clonidine). In this pilot study, clonidine extended the analgesic effect to as long as 14 days. Clonidine administered to the sympathetic nervous system presumably prolongs the duration of blockade, as demonstrated by Kimura et al.5 Still, to achieve long-term analgesic effect in this cohort, most patients required chemical ganglion impar neurolysis or radiofrequency thermoablation.

Further larger and randomized clinical studies are needed to confirm the acceptability of ganglion impar blockade and destruction in non–cancer perineal pain.

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References


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References


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In Reply—I would like to thank Dr. Kabbara for his further information and clarification of the case of the transsacrococcygeal approach to the ganglion impar. It was not my intent to impugn the utility of this approach, but to highlight the case of the paramedial approach to the ganglion impar as a useful alternative. It is my hope that this potentially useful sympathetic block will find favor with clinicians, by either approach, and that many patients will benefit from it.

The interest and comments of Michalek et al. are appreciated. They note that the duration of efficacy of sympathetic block of the ganglion impar with local anesthetics and steroids in my case report does not

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match the duration of relief that they have seen in treating patients with perineal pain of noncancer etiology (chronic postoperative perineal pain, vulvodynia, and vulvar pruritus). However, the mechanism of pain in postherpetic neuralgic (PHN), although not completely understood, seems to be quite different from that in the patient population treated by Michalek et al.

Treatments of PHN with sympathetic blocks, with or without steroids, is controversial but has been described in several studies with favorable results far outlasting the normal duration of action of the local anesthetic or steroid. Forrest injected 1–2 ml bupivacaine, 0.5%, with 60–120 mg methylprednisolone once a week for 5 weeks. At 1 month, 57% of the patients were pain free, and 6 months later, 86% of these patients continued to be pain free. Forrest also reported on 37 patients with longstanding PHN who were treated with three epidural steroid injections given at 1-week intervals. Significant reductions in visual analog scale ratings were noted at 1 month, and 89% of the patients were pain free at 1 yr. Milligan and Nash also reported favorable long-lasting relief of PHN after stellate ganglion blocks. The mechanism of the prolonged effect is unclear. Hetherington advocated sympathetic blocks as a major adjunctive therapy for all PHN patients, although it is recognized that there are no guidelines as to how many to perform or how often to perform them.

Therefore, the experience of Michalek et al. with duration of efficacy may not necessarily apply to PHN due to complex and differing mechanisms of pain. I agree that further investigation is warranted. However, because PHN in the sacral dermatomes is uncommon, it will be difficult to conduct a well-controlled study to find more definitive answers.

References

To the Editor:—In the current days of high-tech equipment and well-defined safety regulations, technical failures are less likely to occur, and so are we to consider them when clinical complications happen. Endotracheal tube defects are probably seen by today’s anesthesiologists as a thing of the past. A review of the literature showed that all of the cases reported in relation to defective tubes occurred when tubes were submitted to repeated sterilizations.

A 39-yr-old woman, with insignificant past medical history, was scheduled to undergo lumbar discectomy. General anesthesia was induced with thiopental (450 mg), fentanyl (150 μg), and vecuronium (8 mg). Tracheal intubation was performed with a 7-mm reinforced endotracheal tube (Safety Flex; Mallinckrodt Medical, Athlone, Ireland; expiration date 2006-09). The patient was positioned in prone position. Normal breath sounds were heard equally in both lungs. Anesthesia was maintained with sevoflurane (1.0–1.5%) and nitrous oxide (67%) in oxygen, and muscle relaxation was achieved with vecuronium bolus. Approximately 2 h after induction, we started to experience difficulties in ventilating the patient’s lungs. Peak inspiratory pressure started to increase while compliance and tidal volume decreased. Pulmonary auscultation remained normal. The capnograph showed a positive deflection on the inspiratory phase; it was assumed that relaxation was insufficient, and a 4-mg bolus of vecuronium was administered. A suction catheter was used to remove possible airway secretions. It was noted that the catheter did not pass through the tube. Tube obstruction was then suspected. Ventilation became very difficult, even using bag ventilation. The patient was quickly returned to the supine position. The face, neck, and upper trunk showed congested veins. Bradycardia with bigeminism occurred and ventilation became impossible, but oxygen saturation did not decrease below 98%. The endotracheal tube was replaced with a new one. All abnormal variables returned to baseline. The patient was repositioned for surgery, and the procedure was completed without further complications. Recovery and the postoperative period were uneventful.

Later inspection of the tube showed a transparent halo on the outside surface. The inner view showed an intramural bubble that completely occluded the lumen. The 3-cm-long bubble was located 11.5 cm from the distal end. A fiberscope was used to obtain a photo of the bubble from the inside of the tube.

Ventilatory distress in the prone position may be a serious complication. The differential diagnosis included consideration of pneumothorax and bronchospasm, but auscultation of both lungs remained possible and was normal. In the case reported, ventilatory distress was due to a bubble protruding into the lumen of the tube. This bubble most probably was not due to any sort of extrinsic damage to the tube during its use. The inclusion of tiny air bubbles in the wall of a reinforced tube can be a result of the production procedure of this kind of tube.1 Most certainly, nitrous oxide exposure and diffusion was due to a bubble protruding into the lumen of the tube. This bubble may have been a result of some valve mechanism due to the presence of the bubble prevented the contraction of the neck veins. It is possible that manual ventilation forced gases into the lungs but that some valve mechanism due to the presence of the bubble prevented expiration. This, combined with the 100% oxygen used, would also explain why the oxygen saturation did not decrease. It is interesting to note that oxygen saturation may not be an indicator of severe airway problems. To our knowledge, this is the first report of this kind of endotracheal tube obstruction with a disposable armored tube. It shows that even in modern days of increased attention to quality control, simple technical defects may occur. The manufacturer was notified about the incident.
during manufacture to ensure that all quality criteria associated with high-pressure gas cycle, we managed to create occlusions and other ever, we were able to create an occlusion in a Safety Flex product by our DHCA protocol that has been previously described.1–6

patient was taken emergently to the operating room for repair using aortic aneurysm involving the aortic arch (Crawford type I). The circulatory arrest (DHCA).

nal aneurysm using cardiopulmonary bypass and deep hypothermic aprotinin exposure in an adult undergoing repair of a thoracoabdomi-

et al.

potential root cause of this problem.

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Mallinckrodt manufactures Safety Flex tracheal tubes with defined validated processes, and the product is checked at several key intervals during manufacture to ensure that all quality criteria associated with this product range are met before release of the product from the plant.

In Reply— Tyco Mallinckrodt (Athlone, Ireland) would like to point out that unfortunately, in this instance, the Safety Flex product in- volved in the complaint was not returned to the manufacturer. There- fore, a comprehensive evaluation could not be conducted relating to potential root cause of this problem.

In the absence of the sample in question, we have attempted to simulate the problem described in the letter by Santos et al. We have been unable to do so under normal simulated use conditions. How- ever, we were able to create an occlusion in a Safety Flex product by subjecting it to sterilization cycles that are different than those used at our manufacturing site. Using a moist heat sterilization cycle or a high-pressure gas cycle, we managed to create occlusions and other disfigurations in the product.

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• 2 million kallikrein inhibitory units intravenously as a load,

as follows: 2 million kallikrein inhibitory units intravenously as a load, followed by an infusion of 0.5 million kallikrein inhibitory units per hour. The cardiopulmonary bypass crystalloid prime was also loaded with aprotinin (2 million kallikrein inhibitory units).

The patient was positioned in the right lateral decubitus position.

Surgical incision, dissection, and initiation of cardiopulmonary bypass were uneventful. Heparinization for cardiopulmonary bypass was ti- trated to maintain the activated coagulation time (kaolin activator) of greater than 600 s. The surgical repair was technically uncomplicated; the aorta was replaced with a Hemashield vascular graft (Boston Sci- entific, Natick, MA). The cardiopulmonary bypass time was 212 min, with a deep hypothermic circulatory arrest time of 38 min.

After successful separation from cardiopulmonary bypass, protamine was administered in a titrated fashion without adverse reaction. De- spite aprotinin and adequate protamine (calculated to neutralize the full dose of heparin), hemostasis in the surgical field was not achieved. There was significant microvascular bleeding. No overt vascular bleed- ing was detected despite careful surgical inspection. Platelet infusion was begun. Ten minutes after initiation of the platelet infusion, the patient experienced cardiac arrest that was refractory to pharmacologic resuscitation. Transesophageal echocardiography revealed dif- fuse intraaortic thrombosis (figs. 1 and 2). The patient did not respond to further resuscitative efforts. The family declined a postmortem examination.

Catastrophic thrombosis with aprotinin in DHCA was first noted in the mid-1990s.7 The most likely explanation for these observations was inadequate heparinization because it was only appreciated in the mid-1990s that aprotinin exposure prolongs the activated clotting time.8 At our institution, we perform 70–80 thoracic aortic procedures with DHCA per year. Our DHCA protocol includes routine application of an antifibrinolytic, either aminocaproic acid or aprotinin. Aprotinin is reserved for the high-risk DHCA population.1–4

Inadequate heparinization as the cause of the prothrombotic state in

References


(Footnotes begin page 216.)
this case is extremely unlikely, given that our standard DHCA protocol was followed and given the DHCA experience at our institution. It is more likely that the prothrombotic state was multifactorial in etiology: disseminated intravascular coagulation triggered by hypothermic cardiopulmonary bypass, the antifibrinolytic action of aprotinin, neutralization of heparin, and the presence of transfused functional platelets. The presence of hemostatic aortic graft material in this case was Hemashield (Boston Scientific), which, although hemostatic, has not been associated with acute intravascular thrombosis.8,9 The mechanism of this fatal thoracic aortic thrombosis is unclear.

Fatal thrombosis has been reported in adult DHCA; the antifibrinolytic, however, was not aprotinin but aminocaproic acid.11 This syndrome has also been reported in pediatric cardiopulmonary bypass; the antifibrinolytic was aprotinin.12

Therefore, our case is the first reported case of fatal thrombosis in an adult undergoing DHCA in the presence of aprotinin despite adequate heparinization. Despite adequate heparinization, this syndrome is still possible in cardiovascular surgery necessitating cardiopulmonary bypass, regardless of antifibrinolytic or patient age. Further hypothesis-driven perioperative research is required to understand and prevent this uncommon but important complication associated with antifibrinolytic therapy.

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Fig. 1. Short axis view of descending aorta with transesophageal echocardiography, showing aortic graft lumen filled with thrombus.

Fig. 2. Long axis view of descending aorta with transesophageal echocardiography, showing aortic graft lumen filled with thrombus.