Sickle Cell and Anesthesia: Do Not Abandon Well-established Practices Without Evidence

To the Editor.—Firth and Head1,2 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,’ 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 percents 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 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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References


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In Reply.—We wish to thank the correspondents for their interest in our review1 and for correcting the typographical errors. Although a small typographical error in the sickle genetic code has extensive consequences, the equivalent inaccuracies in the review fortunately do not have similarly far-reaching effects on our conclusions. Regarding the work by Vichinsky et al.,2,3 the fact that this was analysis of...
problems. These general outcomes are not the appropriate endpoints by which to assess the need for or effect of transfusion.

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 complications against the lower occurrence of a specific subset of complications misleading to cite the incidence of a heterogeneous group of complications.

The correspondents seem to have formed their opinions from the abstract summary. We concede that our guidelines for transfusion in uncomplicated situations are consequently limited to the well-documented evidence that transfusion can improve arterial hemoglobin concentration and 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 concentration and can improve arterial oxygen saturations. 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 that transfusion 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 hypertonic 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.

We are surprised by the correspondents’ continued enthusiasm for transfusion, given their familiarity with the relevant literature. The previously widespread adoption of a practice, exchange transfusion, in the absence of controlled studies, was no guarantee of efficacy or freedom from injury. For considerations of effect, we direct

Table 1. Outcomes from Recent Studies of Perioperative Blood Transfusion*

<table>
<thead>
<tr>
<th>Reference</th>
<th>Study Details</th>
<th>Categories</th>
<th>R-XTF</th>
<th>R-TF</th>
<th>NR-TF</th>
<th>NR-NTF</th>
</tr>
</thead>
<tbody>
<tr>
<td>Griffin and Buchanan (1993)</td>
<td>Retrospective nonrandomized; 66 children</td>
<td>Minor procedures</td>
<td>14%</td>
<td>7%</td>
<td>2%</td>
<td>46</td>
</tr>
<tr>
<td>Koshy et al. (1995)</td>
<td>Retrospective nonrandomized; 1,079 adults, children; low- and moderate-risk surgical procedures</td>
<td>Hb SS—low risk</td>
<td>248</td>
<td>15%</td>
<td>4,8%</td>
<td>12,9%</td>
</tr>
<tr>
<td>Vichinsky et al. (1995)</td>
<td>Prospective randomized; 604 assorted procedures</td>
<td>Procedures</td>
<td>303</td>
<td>15%</td>
<td>15%</td>
<td>301</td>
</tr>
<tr>
<td>Haberkern et al. (1997)</td>
<td>Prospective randomized, nonrandomized; 364 patients</td>
<td>Cholecystectomies</td>
<td>110</td>
<td>12%</td>
<td>19%</td>
<td>97</td>
</tr>
<tr>
<td>Vichinsky et al. (1999)</td>
<td>Prospective randomized, nonrandomized groups; 138 procedures</td>
<td>Cholecystectomies</td>
<td>120</td>
<td>12%</td>
<td>19%</td>
<td>97</td>
</tr>
</tbody>
</table>

* This table is an amended version of table 4 from the original article.

Complications refers to sickle cell disease–specific complications, such as pain crises or acute chest syndrome. ACS = acute chest syndrome; Hb SS = patients homologous for hemoglobin S; NR-NTF = nonrandomized nontransfusion; NR-TF = nonrandomized conservative or aggressive transfusion; R-TF = randomized conservative transfusion; R-XTF = randomized aggressive transfusion.
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


(Corrected for publication March 3, 2005.)

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

To the Editor.—Pandit et al. 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 Peto2) + 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 Peto2) + 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.1 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.”5,6 The study of Zhang and Robbins7 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 thought for it or control for it experimentally?

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 variability (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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Position of the Shoulder for Subclavian Approach

To the Editor—We read with interest the article by Kitagawa et al.1 regarding the proper shoulder position for subclavian vein (SCV) puncture. They are to be congratulated for performing an important study about the anatomical backgrounds of SCV catheterization. The major finding of their article was that, when compared with neutral and elevated positions, the lowered shoulder position is the best for...
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 midpoint 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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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.1

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)aortic 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.2 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.3 described a close relation between this index and dp/dtmax, the latter being strongly preload dependent.4 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.5

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.6 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.

References

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).1 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 nitroglycerin administration. However, the patients in this study with previous myocardial infarction and abnormal ventricular function did not exhibit any changes in myocardial performance index.2

Doppler tissue imaging has load dependence3,4 but has been demonstrated to be more independent of preload than conventional Doppler measures of mitral inflow.3,5,6 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.6 Significant increases in afterload seen in patients with aortic stenosis do decrease Doppler tissue imaging velocity.7 However, no significant impact on Doppler tissue imaging...
results presented as mean ± SD; combined systemic and pulmonary vascular resistance index units are dyn s cm⁻⁵ m⁻².

* P < 0.05, different from baseline by one-way repeated-measures analysis of variance.

velocities of increased left ventricular preload with ventricular septal defects was noted.

With regard to the loading conditions for the patients in our current study,₉ preload was not specifically assessed, largely because of the inability to use conventional methods to compute left ventricular end-diastolic volume, because of the irregular geometry of the functional single ventricle. Central venous pressure, a rough estimate of preload, was not changed with either of the two anesthetic levels with the two regimens we studied. In a previous study of two-ventricle patients with congenital heart disease, we demonstrated that neither fentanyl–midazolam nor sevoflurane, in doses similar to those used in the current study, had any effect on left ventricular end-diastolic volume measured by the biplane method of Simpson.⁹ In the same previous study, neither fentanyl–midazolam nor sevoflurane changed systemic vascular resistance index, calculated echocardiographically. In the current study, we did not specifically calculate systemic vascular resistance index because the patients had a functional single ventricle, and systemic and pulmonary blood flow both occurred in the aorta of most patients, so the conventional concept of systemic vascular resistance did not apply. However, if one was to calculate a combined systemic and pulmonary vascular resistance index in the patients of our current study, by dividing the difference between mean arterial pressure and central venous pressure (assuming no change in central venous pressure from baseline) by the aortic or neoauric outflow, there is no change in systemic and pulmonary vascular resistance index with fentanyl–midazolam but a significant decrease with sevoflurane at both anesthetic levels (table 1).

Therefore, the changes in loading conditions induced by the anesthetic regimens for congenital heart disease can be estimated to be no change with fentanyl–midazolam and a 17% decrease in afterload with sevoflurane.

Although we agree with the caution of Drs. Poelaert and Amà to consider changes in loading conditions and other pathophysiologic factors when performing and interpreting echocardiographic studies assessing response to anesthetics, we believe our conclusions are valid for single-ventricle infants. Myocardial performance index in particular represents an appropriate method to assess myocardial function in patients with abnormal ventricular geometry, such as those with a functional single ventricle.

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(Accepted for publication March 28, 2005.)

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.

de Leon-Casasola³ described the transsacroccygeal approach as the easiest way of performing the ganglion impar block. Over the years, many practitioners have tried to simplify the approach, and many reports have been published advocating the transsacroccygeal route for its simplicity and effectiveness.¹,⁵

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

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Transsacroccygeal 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.

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

To the Editor—We read with great interest the recently published case report by McAllister et al. 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. 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 patients. The procedure was found to be ineffective in 20 patients with coccygodynia. In addition to this study, there have been only a couple of case reports on the successful management of perineal pain of noncancer etiology using this technique of blockade.

In their paper with postherpetic neuralgia, McAllister et al. used a corticosteroid as an additive to a local anesthetic, administering it to the presacral area. The analgesic efficacy of corticosteroids to sacral sympathetic structures has not been demonstrated to date, and the mechanism of its analgesic activity remains unclear.

Another thing we found surprising was the long-term analgesic effect of local anesthetic solution (3–5 months). The common duration of the analgesic effect of local anesthetics administered to that area in diagnostic/prognostic blockade is 2–7 days.

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. 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 noncancer perineal pain.

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References


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match the duration of relief that they have seen in treating patients with perineal pain of noncancer etiology (chronic postperineal 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.

Treatment 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 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.² Milligan and Nash³ also reported favorable long-lasting relief of PHN after stellate ganglion blocks. The mechanism of the prolonged effect is unclear. Hetherington⁴ advocated consideration of 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.

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Valve Leaks with New Disposable Extraglottic Airway Devices

To the Editor:—We would like to report six cases of spontaneous cuff deflation that occurred during a clinical trial of a new disposable extraglottic airway device, the CobraPLA® (Engineered Medical Systems, Inc., Indianapolis, IN).³ The cuff pressure decreased to almost zero during maintenance of anesthesia. These events were detected by continuous cuff pressure monitoring using a transducer attached to a monitor (S/5 AM; Datex-Ohmeda, Helsinki, Finland) during the maintenance phase of anesthesia and did not influence the clinical performance of the device, primarily because the patients were breathing spontaneously. The patients were not moved during surgery, with the transducer positioned on the patient’s shoulder throughout the duration of the procedure. The devices’ cuffs were checked for leaks before use as recommended by the manufacturers. When checked after removal of the device, the cuff itself did not show any defect, but submersion in water revealed tiny bubbles of air escaping from the pilot balloon valve, showing a continuous leak at the cuff deflator valves with their surroundings (fig. 1).

A similar problem has been reported with another new extraglottic airway mask, the Marshall Laryngeal Airway Device (Marshall Products Ltd., Bath, United Kingdom). In the United Kingdom, the Medical Devices Regulatory Agency issued a Medical Device Alert in 2003 regarding cuff failure due to a small number of pilot balloon valves leaking. This led to the manufacturer’s recall of the product.

Cuff deflation is undesirable because it can lead to a loss of seal with the respiratory/gastrointestinal tracts and put the patient at risk of ventilatory failure, aspiration, and gastric insufflation.²

We urge the manufacturers of new extraglottic devices to ensure better quality control of their products, including the cuff deflator valves. However, because one cannot guarantee 100% quality control at all times, we also urge clinicians to incorporate cuff pressure monitoring into their routine practice.


References


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In Reply:—We fully agree with Dr. van Zundert et al. that manufacturers of extraglottic devices (and all other medical devices) should assure quality control of their products.

Since the CobraPLA® (Engineered Medical Systems, Inc., Indianapolis, IN) was introduced into clinical practice in 2003, Engineered Medical Systems has ensured the quality of the cuff deflator valve with a comprehensive validation and production procedure of every product made, which includes a pressure test of the deflator valve and a full 16-h inflation test of the deflator valve/cuff assembly, with no leakage allowed.

For a manufacturer to fully assess and address a valve issue such as Dr. van Zundert et al. describe, it is imperative that the products in question be promptly returned to the producer (in its original condition at the time of use) for evaluation. Unfortunately, Engineered Medical Systems never received the six devices in question back at our facility for a proper inspection to be conducted. When we visited Dr. van Zundert in the Netherlands after receiving a copy of his correspondence to Anesthesiology, we were presented with a bundle of more than 50 CobraPLA®s, which had been used and then cleaned. There was no documentation as to which ones had been used in his report. As a result, we cannot be certain as to the cause of the failures.

Considering the fact that Engineered Medical Systems has not received a single other report of a cuff deflator valve failure from all the units distributed worldwide, we might assume that a very limited number of devices (the ones used by Dr. van Zundert et al.) had a minimal leak that could not be identified at preoperative check. Alternatively, we have been notified by another investigator that in some units, during continuous cuff pressure monitoring, a small leak can occur at the monitoring connection site and not from the device itself; that clinician did not think that the valve was malfunctioning. Finally, damage to the devices in question could have also occurred during forceful cleaning before the postoperative submersion test. It is reassuring to Engineered Medical Systems that the deflated cuff did not influence the clinical performance of the device or the end-tidal carbon dioxide measurement during the study of Dr. van Zundert et al., which speaks highly to the performance of the CobraPLA®.

Although we cannot state with certainty that the experience of Dr. van Zundert et al. was unique, it seems that it was isolated and that valve failure has not posed a significant clinical problem for clinicians. We are confident that the CobraPLA® meets the rigorous quality standards clinicians and patients deserve in their medical products.

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during manufacture to ensure that all quality criteria associated with 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 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. However, 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.

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 validated processes are met before release of the product from the plant. Feedback is very important because we strive to maintain a high-quality product and a high level of customer communication and satisfaction.

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Fatal Thrombosis in an Adult after Thoracoabdominal Aneurysm Repair with Aprotinin and Deep Hypothermic Circulatory Arrest

To the Editor—We report a case of fatal aortic thrombosis after aprotinin exposure in an adult undergoing repair of a thoracoabdominal aortic aneurysm using cardiopulmonary bypass and deep hypothermic circulatory arrest (DHCA).

A 78-yr-old woman presented with a ruptured thoracoabdominal aortic aneurysm involving the aortic arch (Crawford type I). The patient was taken emergently to the operating room for repair using our DHCA protocol that has been previously described.1–6 Anesthetic induction was with titrated fentanyl and midazolam. Neuromuscular blockade was achieved and maintained with titrated pancuronium. The trachea was intubated with a left-sided double-lumen tube (Tyco Healthcare/Mallinckrodt, St. Louis, MO). Correct endotracheal position was confirmed by serial fiberoptic bronchoscopy. Intraoperative monitoring included American Society of Anesthesiologists routine monitors as well as indwelling radial arterial and pulmonary arterial catheters. Because of the emergent nature of the procedure, a lumbar cerebrospinal fluid drain was not placed. Anesthesia was maintained with isoflurane in oxygen, as well as titrated fentanyl and midazolam.

The patient was given aprotinin (Bayer Corporation, Pittsburgh, PA) 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 titrated 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 Scientific, 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. Despite 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 bleeding 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 diffuse 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 early 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...
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.9,10 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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