

Sniffing Position: An Easy Way to Carry Out, Not a Glottic Exposure Guarantee

To the Editor:—I read with interest the article “Randomized Study Comparing the ‘Sniffing Position’ with Simple Head Extension for Laryngoscopic View in Elective Surgery Patients” by Adnet *et al.*¹ The authors challenged the sniffing position as the standard head position for laryngoscopic intubation, and they concluded “routine use of the sniffing position appears to provide no significant advantage over simple head extension for tracheal intubation.” I read through the article carefully and found some questions about their study.

First, the degree of glottic exposure depends on the effort during the laryngoscopy. One proposed advantage of the sniffing position is that it reduces the effort required to expose the glottis.² However, the authors simply compared the glottic exposure between the sniffing position and the head extension position, without noting the effort during the laryngoscopy.

Second, the total number of patients with each IDS score in figure 2 is not equal to the number the authors reported. The sum of the patients in the sniffing position shown on each bar is 228 ($128 + 36 + 34 + 10 + 14 + 1 + 3 + 2 = 228$), and the sum of the patients in the head extension group is also 228 ($111 + 62 + 30 + 13 + 4 + 2 + 2 + 1 + 1 + 1 + 1 = 228$). However, the authors claimed that 225 patients were included in the sniffing position group and 231 in the simple head extension group. This mistake should be clarified. Nevertheless,

the other conclusion they achieved is thought-provoking: “The sniffing position appears to be advantageous for obese and head extension-limited patients.” Is more laryngoscopy effort required to expose the glottis in these two kinds of patients? The advantage of the sniffing position might lie beneath that question.

Based on these considerations, I am not convinced that the findings of Dr. Adnet *et al.* could challenge the clinical usefulness of the sniffing position. I believe that the sniffing position is still the optimal position in reducing the effort to expose the glottis during laryngoscopy.

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Comparison of Intubating Positions: The End Point for Position Should Be Measured

To the Editor:—Adnet *et al.*¹ are to be congratulated for performing an important study comparing the efficacy of the sniffing *versus* simple head extension positions for tracheal intubation. However, since the end points for the sniffing position were not measured, I have three concerns as to whether a true sniffing position was achieved by the study protocol. First, the sniffing position consists, in part, of approximately 35° of flexion of the lower cervical spine on the chest. The 35° of neck flexion on the chest can be achieved by elevating the occiput by 7 cm. In the study protocol, the neck flexion on the chest was achieved by placing the patient’s head on a 7-cm-high “cushion.” However, the compressibility of the “cushion” was unspecified. I placed the heads of normal-sized patients on 7-cm-high uncompressed pillows and foam supports that are used in my operating rooms for head support and found that the height of both the pillow and the foam supports decreased by 50%. Thus, compression of the “cushion” by the head significantly decreases the height of the “cushion” (and the flexion of the lower cervical spine on the chest) and results in only a partial sniffing position.

Second, simple elevation of the occiput by 7 cm will not result in the sniffing position in many obese patients because one must also reposition the abnormally contoured scapula, shoulders, and nape of the neck (*i.e.*, recontour the upper body and head and neck with a blanket and towel ramp) in order to achieve not only 35° flexion of the neck on the chest but also 85–90° extension of the head on the neck at the atlantooccipital joint. Thus, it is unknown exactly what position produced the significant improvement in the laryngoscopic view, compared to simple head extension, in the obese patients.

Third, for the laryngoscopies performed in the sniffing position, the authors did not affirmatively or positively state that the head was actually extended on the neck at the atlantooccipital joint at the time of laryngoscopy. Failure to do so would constitute failure to achieve the sniffing position.

Thus, for these three reasons, and given the fact that head extension from the neutral position causes flexion of the cervical spine on the chest to some degree, exactly what was studied and what was the difference between the two positions are unclear. The proper scientific end point for the two intubating positions would have been to externally measure the planes and axes of the chest, neck, and head. Because the end points for the two positions were not measured, I do not believe the authors are entitled to draw conclusions regarding the comparative efficacy of the “sniffing” and simple head extension position for laryngoscopy and tracheal intubation.

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There's More to Intubation Than Laryngoscopy

To the Editor:—In the article by Adnet *et al.*,¹ "Randomized Studies Comparing the 'Sniffing Position' with Simple Head Extension for Laryngoscopic View in Elective Surgery Patients," the authors graded two views of each patient's larynx and the degree of difficulty with subsequent intubation. Their results showed no difference in laryngeal exposure between the extended head and sniffing position either at initial laryngoscopy or following repositioning of the head, except in obese patients and those with limited neck mobility. Visualization in these subgroups was best with the head in the sniffing position, but whether this observation was relevant to ease of intubation was not established. Approximately 11% of laryngoscopies were judged to be difficult, while fewer tracheal intubations proved problematic.

One can reasonably reinterpret the authors' findings and formulate several generalizations that are applicable to all routine intubations. First, resting the head comfortably on a small pillow with the neck flexed and head extended is an acceptable position for laryngoscopy. Moving the head when a difficult laryngoscopy is encountered may sometimes improve visualization but is not likely to enhance the intubating process. Second, a standard technique of intubation, as described in the article by Adnet *et al.*, is suitable and successful with easy, as well as most, but not all, difficult laryngoscopies. Adnet's experience with intubations also defines the problem for which a solution must be found: a small number of patients required a complex approach to intubation involving participation of an assistant, multiple attempts at passing the endotracheal tube, and possible changes to the method of intubation. Although these patients were exposed to prolonged manipulation, none was harmed under conditions of a controlled trial for elective surgery. However, similar attempts could prove inadequate in other clinical settings, *e.g.*, emergency surgery for trauma, bowel obstruction, burns, ICU care, and obstetrics where successful first try intubation could be critical to patient outcome. Third, in practice, when a difficult laryngoscopy is encountered, the first view obtained by an experienced operator is probably the best. If the operator proceeds using a standard method of intubation, attempts at introducing the endotracheal tube will be as difficult as the ongoing laryngoscopy. Under this circumstance, the difficult laryngoscopy and difficult intubation appear inseparable, while in actuality, laryngoscopy and tracheal intubation are distinct processes and independent of each other. It is possible to experience a difficult laryngoscopy and yet easily intubate the trachea if the technique of intubation is correct. If an advance is to be made in improving intubation of patients with difficult laryngoscopies, it will not be found in attempts at bettering laryngeal view since near-maximal exposure has been attained with presently

used equipment and techniques of laryngoscopy. The answer is to routinely use a technique of intubation that works equally well for both easy and difficult airways.

During difficult intubation, any technique must overcome physical obstacles within the upper airway that hinder movement of the endotracheal tube toward the glottis. Several considerations include:

1. Current techniques of laryngoscopy create a passage within the upper airway of unique size and shape for a given individual, which is not easily altered by manipulation carried out during laryngoscopy. The anesthesiologist is forced to use the passage through the upper airway as it exists at the time of initial laryngoscopy.
2. To reach the vocal cords, the endotracheal tube must travel through the laryngoscopic channel created by a curved laryngoscope blade without touching any part of the airway; otherwise, it will be deflected away from the glottic opening.
3. The endotracheal tube must have a correct shape and must be delivered in a proper direction towards the glottis in order to move freely through the laryngoscopic channel. Shape and direction are important in controlling the endotracheal tip and for successful intubation.
4. A practical means of providing and maintaining the endotracheal tube with a prescribed shape is to routinely employ a stylet within the endotracheal tube lumen. When properly combined and conformed, the endotracheal tube and stylet form a working unit—the oral tracheal stylet unit to be used for every routine intubation.²

Does this approach to intubation, used safely and successfully over many years, expand the nature of investigations needed to develop the editorial suggestion "Common Practice and Concepts in Anesthesia: Time for Reassessment"³ and help highlight the essential relationships between the airway, laryngoscopy, and tracheal intubation?

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The Sniffing Position

To the Editor:—We commend Adnet *et al.*¹ for daring to question a traditional, though not scientifically validated, aspect of anesthesia practice—the sniffing position for intubation. However, several aspects of their study warrant comment.

Although the authors acknowledge that the unblinded nature of the study was a limitation, this deserves more emphasis. Bias may have been quite easily introduced during assessment of the view at laryngoscopy (the main outcome variable) and/or during the force exerted

on the laryngoscope blade at laryngoscopy. The potential for bias in this particular study is amplified by the fact that four of the investigators were coauthors of two previous articles refuting the ability of the sniffing position to produce alignment of the three axes considered important for optimal laryngoscopic view.^{2,3}

Another problem with this study is the lack of use of neuromuscular blocking agents. As the majority of adult intubations during surgery in this country are facilitated by neuromuscular blocking agents, their elimination creates a situation which may be difficult to extrapolate to usual clinical practice. Neuromuscular blocking agents may both improve and obscure the view at laryngoscopy. Eliminating patient

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movement and facilitating mouth opening contribute to the former, while a decrease in muscle tone may precipitate collapse of the pharyngeal walls, thereby hindering visualization. The net effects of these opposing forces is unpredictable and may vary from individual to individual and even time to time.

We very much agree with the authors' attempts to standardize the type of laryngoscope blade used in their study. Nonetheless, not all MacIntosh No. 3 blades are created equal. For example, the eMAC Welch Allen® and the Propper® blades have much less curvature than many others. It would be important to know exactly which type of MacIntosh blade was used in this study, and to account for the differences during data analysis if more than one type was utilized.

Finally, the conclusion that the sniffing position does not aid laryngoscopy except during two specific circumstances, obesity and decreased neck movement, is misleading. The authors present four other potential predictors of difficult intubation which were not associated with an improvement of laryngoscopic view with the sniffing position. However, for three of these (limited mouth opening, short thyromental distance, and "other anatomic factors"), the number of patients with each predictive factor was between zero and four—numbers much too small for statistical analysis. Even for the remaining factor, Mallampati classes III and IV, the trend was toward improvement in laryngoscopic grade with the sniffing position, but the sample size was too small to

achieve statistical significance. Thus, the authors have merely demonstrated that the sniffing position was useful for the two most frequent predictors for difficult intubation studied. It is possible that the sniffing position is beneficial for all potentially difficult intubations but unnecessary for easier intubations. This is consistent with our clinical impression that when an airway is "easy," it does not really matter what one does, but when an airway is more difficult, one must resort to a variety of intubation "tricks," such as the sniffing position.

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To "Sniff" or Not to "Sniff": That Is the Question

To the Editor:—We read with interest the recent article by Adnet *et al.*¹ in which the authors compared the sniffing position with simple head extension for glottic visualization in adults who were anesthetized but not paralyzed. We find the authors' conclusions not only lacking in statistical power, but also in rationale.

The authors showed the advantage of the sniffing position for visualizing the glottis during laryngoscopy in patients with a body mass index (BMI) greater than 30 as well as for those with head-extension limitation. They demonstrated the *superiority* of the sniffing position *versus* simple head extension in these two predictably difficult intubation scenarios. Furthermore, since the authors did not show any *inferiority* of the sniffing position (compared to simple head extension) in regard to glottic visualization,² we disagree with their conclusion that "systematic application of the sniffing position offered no appreciable advantage over simple head extension for improvement of glottic visualization with use of direct laryngoscopy and a Macintosh blade." In addition, since most patients are more comfortable with several centimeters of occipital support, which coincidentally approximates the sniffing position, and since it is certainly easier to remove a pillow placed *a priori* under the patient's head than to attempt to place one under the head should the indication arise, then it is logical to commence laryngoscopy with a pillow placed under the patient's occiput.

We also disagree with the method the authors chose for their sample size calculation. They calculated the appropriate sample size based upon the *assumption* that the sniffing position might reduce the incidence of difficult laryngoscopy to approximately the same degree as laryngeal manipulation.^{1,3,4} However, the authors failed to provide any rationale for such an assumption. Such an arbitrary assumption may have resulted in the calculation of too small a sample size and hence a type II error with resultant insufficient statistical power to discriminate between the experimental groups. For a given sample size, the probability of a type II error is inversely proportional to the degree of difference between the two experimental conditions. Since it was not possible to alter the degree of difference between the two experimental conditions to minimize the likelihood of a type II error, the authors should have (1) made the sample size larger or (2) reduced

the population variability. Consequently, once the authors decreased the variability in their sample population by analyzing only the subset that was obese using a multivariate analysis, they demonstrated the distinct advantage of the sniffing position over simple head extension. This advantage was not apparent when they analyzed their entire sample population due to their small sample size, which led them to unjustifiably conclude that "systematic application of the sniffing position offered no appreciable advantage. . . ."

The authors' statistical analysis may have been further limited by the phenomenon known as *convenience sampling*. Convenience sampling refers to the phenomenon whereby researchers are limited to using those patients who happen to show up at their hospitals. It also refers to the nuances of the surgical schedule, the good will of the referring physicians and attending surgeons, and the willingness of patients to cooperate. At best, convenience sampling is representative of the patient population at any given institution, with absolutely no assurance that those patients are similar to patients elsewhere. Only 17 to 18% of the patients studied by Adnet *et al.* had a BMI greater than 30, whereas according to The National Institutes of Health, in the United States, 22.3% of adults are obese.⁵ Consequently, we believe convenience sampling may apply to the authors' sample population. The small percentage of patients with a BMI greater than 30 in the study of Adnet *et al.* may have significantly influenced their results and, hence, their statistical analysis, resulting in their conclusion that "systematic application of the sniffing position offered no appreciable advantage over simple head extension for improvement of glottic visualization." One would expect to demonstrate a clear superiority of the sniffing position as compared to simple head extension even with systematic application if a similar population size as Adnet's contained a greater percentage of patients with a BMI greater than 30, such as is common in the United States. Likewise, one would not be at all surprised to find no advantage of the sniffing position compared to simple head extension in a sample size population similar to that of Adnet *et al.* but having a smaller percentage of patients with a BMI greater than 30 even with multivariate analysis. Subsequently, when determining if a procedure should be "systematically applied," the study sample population should resemble the patient population to which it is to be applied.

We also encountered a possible calculation error on the part of the authors. They calculated a sample size of 222 patients in each group based upon the assumption that the sniffing position might reduce the incidence of difficult laryngoscopy to the same degree as would laryngeal manipulation. However, in table 2 of the study of Adnet *et al.*, group A included 225 patients with a male/female ratio of 114/98 (or, 212 patients). Not included were 13 patients; however, there was no explanation for their omission from the data. Similarly, in group B, which consisted of 231 patients with a male/female ratio of 130/86 (or, 216 patients), again, 15 patients were omitted, also apparently without explanation. We wonder if this represents a simple typographical error on the part of the authors, or if this was a deliberate exclusion of certain subjects by them. Since these 28 "excluded/typographical error" patients comprise 6% of their total study patient population, their exclusion might represent a serious alteration in the results attained and, hence, their statistical analysis of the data, as well as their power of analysis since the total number for each group is less than the 222 patients per group calculated by the authors.

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A Reconsideration of Three Axes Alignment Theory and Sniffing Position

To the Editor:—It is interesting to read the recent study by Adnet *et al.*,¹ its accompanied editorial, and other related reader comments on the "sniffing position." We applaud the efforts of Adnet *et al.* in challenging the classic three axes alignment theory² and conducting subsequent studies^{1,3} to prove their conviction. From our experience as clinicians, we believe the sniffing position is by no means a "gold standard" for laryngoscopy. It is simply one alternative among several techniques, such as laryngeal lift⁴ or the BURP⁵ maneuver, to facilitate laryngeal visualization in some clinical situations, such as in patients with limited head extension or obesity as found in the study of Adnet *et al.*¹

Adnet *et al.*² showed that a successful direct laryngoscopy does not require alignment of the three (oral, pharyngeal, and laryngeal) anatomic axes. Adnet *et al.*³ further demonstrated that anatomic alignment of the three axes is impossible to achieve in neutral head position, simple head extension, or sniffing position. Adnet *et al.*¹ concluded that the sniffing position is not any better than simple head extension for facilitating direct laryngoscopy. Thus, we are convinced that the three axes alignment theory is invalid and standard practice doctrine must be rewritten. As clinicians, we must ask, what is the mechanism of direct laryngoscopy? How can we explain the benefit of the sniffing position? To this end, we have proposed a "two axes, tongue, mobility, and space" approach,⁶ as an outgrowth of the simple original teachings in Gillespie's classic textbook⁷:

... In the normal position of the structures the line from the upper incisor teeth through the pharynx to the glottis is almost a right angle. This must be converted into a straight line by the laryngoscope to bring the glottis into the line of vision. To achieve this straight line the base of the tongue and the epiglottis must be lifted anteriorly. ... In this (sniffing) position there is no tension on the muscles of the neck, and the distance from the teeth to the glottis is shortened. ...

We note that there is no hint of a laryngeal axis in Gillespie's teaching of direct laryngoscopy and explanation of sniffing position. In fact, the benefit of the sniffing position may also be illustrated by a

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simple geometric principle utilizing only the oral and pharyngeal axes.⁶

Lastly, from experience, we find that in patients with a short mandibular ramus,⁸ the sniffing position may worsen glottic exposure. However, if less head extension and more cervical flexion than usual are exerted, glottic exposure may be improved. We suggest that in patients with a short mandibular ramus, in whom the floor of oral cavity is already high (more toward the skull),⁸ maximal head extension not only increases neck muscle tension but also raises the oral cavity floor even higher; thus, the glottis is further away from the teeth. We do not know if the 11% of negative effect of the sniffing position in the study of Adnet *et al.*¹ represents a similar patient group.

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In Reply:—We appreciate the interest shown in our article, as evidenced by the letters from Drs. Daley and Norman, Benumof, Chen, Khorasani *et al.*, Chou and Wu, and Stasiuk regarding our randomized study comparing the sniffing position *versus* simple head extension.¹

We agree with Drs. Daley and Norman that the fact that four of the investigators were coauthors of the previous experimental study regarding the sniffing position may be interpreted as a potential bias. Since it is difficult (or impossible) to perform a double-blind study of laryngoscopy, and since we wished to determine whether our previous experimental work might have clinical relevance, we performed this study. Of course, we would welcome the efforts of any “independent” researchers to perform a complementary study. The fact that NMBA influences glottic exposure is obvious. We did not use NMBA in this study because a portion of patients scheduled for elective surgery did not require neuromuscular blockade for surgery. Standard care for such patients in our service is to perform intubation using topical anesthesia without paralysis. The advantage of this procedure is the performance of two laryngoscopies; thus, each patient serves as his/her own control for glottic evaluation. Nevertheless, we cannot speculate on the possible influence of NMBAs. The power of our study was calculated to test the hypothesis of the superiority of the sniffing position. Of course, analysis of a subgroup is always limited by a smaller subsample size. We would strongly agree with the desirability of another study designed to examine the sniffing position in patients with predicted difficult intubations.

We agree with Dr. Benumof that the compressibility of the cushion changes critical angles involved in laryngoscopy. Once again, we only evaluated the potential benefit of the sniffing position as the position was used *in our routine practice*, rather than performing a rigorous experimental evaluation of a more “extreme” sniffing position (*i.e.*, greater elevation of the occiput). It is possible that a more exaggerated sniffing position might have some utility, but that is entirely speculative; our study clearly demonstrated that the systematic use of up to 7 cm of occipital elevation (and this might clearly vary with the weight of the patients head) had no influence. We do agree with Dr. Benumof that the most critical point is that simple head extension on a flat surface leads to neck flexion on the chest and facilitates direct laryngoscopy. We did not modify our protocol for obese patients.

It is well established that the intensity of lifting force during laryngoscopy, as advanced by Dr. Chen, modifies the glottic visualization. We did not quantitate lifting forces. However, the Intubation Difficulty Scale (IDS) indirectly takes this parameter into account, and we found no significant difference between the two positions in the distribution of IDS scores.² Rather than focusing the study on one parameter (lifting force) that can interfere with the quality of intubation, we chose to measure the global complexity of intubation, characterized by the IDS score. We thank Dr. Chen for correcting a typographical error concerning the number of patients in figure 2. For IDS = 0, the numbers of patients in the SNIFF and EXT groups were 131 and 108, respectively.

We thank Dr. Khorasani *et al.* for their interesting statistical comments. The our study was not powered to demonstrate the superiority of the

sniffing position in patients with a body mass index greater than 30 and/or those with head extension limitations. The sample size (and power) was calculated to test our hypothesis in a general surgical population at our institution. To specifically examine the effects of position on laryngoscopy in such selected groups of patients would require a very different series of sample size/power calculations—which we never attempted to perform. We chose our sample size based on previous studies demonstrating superiority of a simple physical maneuver to improve glottic visualization under direct laryngoscopy. As we have already noted, there are no comparative studies evaluating the sniffing position, and hence, we were forced to base our estimate on the reported effects of a different but reasonably similar intervention. Evaluation of laryngeal manipulation on glottic exposure has been widely studied, and we hypothesized that the sniffing position would have the same rate of improvement as laryngeal manipulation. This hypothesis may be discussed and criticized. The analysis of subgroups of patients with predictive factors of intubation difficulty was not the end point of our study. We have observed—*not demonstrated*—that obese patients and those with head limitation might benefit from the sniffing position. Another randomized study, including only patients with predictive factors of intubation difficulty, is warranted for demonstrating this observation. On the other hand, it is well established that the incidence of obesity in European countries is smaller than observed in the US. Current prevalence data from individual national studies suggests that the range of obesity prevalence in European countries is from 10 to 20% for men and 10 to 25% for women (France: average of 12%).^{*} The calculation errors noted were due to an error in transcribing data from our statistical program to the manuscript. The sex ratios (M:F) in groups A and B were 126:99 and 141:90, respectively.

We agree with Drs. Chou and Wu that the mechanism of direct laryngoscopy warrants further experimental studies in order to find the best “patient/laryngoscope adjustment.” Dr. Stasiuk reinterprets our findings, and actually, if the systematic use of a cushion improves the comfort of the patient, there is really no reason to remove it in terms of optimization of the laryngoscopic procedure.

In conclusion, our study should be considered as a preliminary study of the place of the sniffing position in intubation among scheduled surgical patients. Additional larger studies, perhaps evaluating selected patient subgroups, might determine whether the sniffing position may be considered as contributory in the management of difficult intubations or whether it should be used as a standard head position before general anesthesia and intubation attempts.

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^{*}Source International Obesity Task Force. Available at: <http://www.ietf.org/home.shtml>

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Quality Improvement in Anesthesia for Volunteer Medical Services Abroad

To the Editor:—We read with interest the article by Fisher *et al.*¹ concerning quality improvement in anesthesia for the Operation Smile organization. The authors are to be congratulated for attempting to improve the overall quality of anesthesia care in the third world countries that Operation Smile helps by improving the lives of children with cleft lips and palates and orthopedic problems.

Having been part of the teams for the organization, we personally know some of the problems faced by the teams as pointed out in the article. If pulse oximetry is now a standard for intraoperative and postoperative units, this is a significant step forward for the anesthesia team (as having an extra anesthesia team member available to help with inductions and breaks). Another improvement would be to have a double-boarded pediatric anesthesiologist as a team member who is involved with the preoperative assessment, the postanesthesia care unit, and the emergencies that may occur both on the postoperative ward and in the operating room.

However, not mentioned is the realization that not all members of the anesthesia team are fully trained pediatric anesthesiologists. We make these suggestions only as a secondary point to the effort the authors have put forth to help improve the care of these children.

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Good Outcome and Volunteer Medical Services in Developing Countries Are Compatible

To the Editor:—We read with interest both the article by Fisher *et al.*¹ on assessing pediatric anesthesia practices for volunteer medical services in underdeveloped countries and the accompanying editorial by Warner *et al.*² The authors have detailed suggestions for improving outcome. Ours is a small, specialized group: Heart Care International, headquartered in Greenwich, Connecticut. We published our initial report³ on a single trip for a 9-day period to Guatemala City, Guatemala, in 1995.

We have a different approach, and our outcome results support our methodology as detailed below. We visit the same location consecutively over a period of several years for purposes of training the local physicians and to provide follow-up care. We confine ourselves to pediatric cardiac surgery, which for all practical purposes was nonexistent in Guatemala at that time. For each operating room, there was a pediatric cardiac surgeon, a surgical assistant, and a pediatric cardiac anesthesiologist. Another anesthesiologist circulated between two rooms and was present at critical periods, such as induction of anesthesia, separation from bypass, and the end of surgery. An additional anesthesiologist circulated between the operating rooms and the intensive care unit (ICU). One anesthesia technician was also available. There was a scrub nurse and a circulating nurse for each operating room. The ICU was staffed by a nurse for every two patients, a cardiologist, a pediatric intensivist, and a respiratory therapist; 24-h ICU coverage was provided. We had nighttime operating room coverage at the same staffing level for any possible emergencies. Each specialty team of doctors, nurses, and ancillary staff had a leader who helped to coordinate all the activities. A screening team arrived several weeks before the main group to examine the surgical candidates who were presented to us by the local cardiologist and, if necessary, performed cardiac catheterization to arrive at the appropriate diagnosis.

Like others, we were also faced with obsolete anesthesia machines and ventilators. Our group included a biomedical engineer, who made

sure that however primitive the equipment, it was functional. We utilized portable uninterruptible power sources for each anesthesia location and the ICU as there are frequent power outages. We stringently followed American Society of Anesthesiologists guidelines for the standard of care. Every anesthetic location, along with the standard electrocardiogram and blood pressure monitors, had a monitor for inspired oxygen concentration, a pulse oximeter, an expired carbon dioxide analyzer, and an anesthetic gas analyzer. All patients, except those undergoing relatively minor procedures, such as correction of patent ductus, had arterial and central venous pressure monitors. All patients were transported with monitors from the operating room to the ICU. The ICU team stayed on for a week after the end of surgery to safely discharge all the patients.

We attribute our success to the following reasons. The group was initially assembled at Columbia Presbyterian Medical Center in New York City. We all knew each other and had no difficulty functioning as a group. Since the early beginning, members have migrated to other institutions, but we still function as a group and understand each other's strengths and weaknesses. The group is dedicated to this endeavor, and our turnover rate is very low. All patients were carefully examined prior to surgery and by the anesthesiologist on the day of surgery. We took absolutely no shortcuts in patient monitoring. Our group was liberally staffed; an understaffed group is false economy. Though we had no formal didactic program, we encouraged local physicians and nurses to participate in all aspects of patient care. We limit ourselves to one trip a year, and always to the same location. Indeed, we feel it is of paramount importance to be invited by the medical practitioners of the host country so that we may learn what their needs are and may effectively transfer our expertise to the local medical establishment. It is important that we are not perceived as surgical raiders who are on a mission to do as many cases as possible in order to gain experience doing a particular procedure that will benefit us upon our return to the United States.

We are pleased to say that Guatemala has now become self-sufficient, and we have moved to a new location, Santo Domingo, in the Dominican Republic. We now staff three operating rooms, and surgery

The Volunteer missions were funded by Heart Care International, Greenwich, Connecticut.

is limited to maximum of 2 weeks. We also fully staff a one-room interventional cardiac catheterization laboratory requiring anesthesia care, once again, with two anesthesiologists. The facility is in the same hospital complex, but a little away from the main operating rooms. We have now treated over 300 children surgically, performed over 100 diagnostic cardiac catheterization procedures, performed over 20 interventional cardiac catheterization procedures, and provided guidance for medical therapy for over 1,000 children in Guatemala and the Dominican Republic. A preliminary report was presented at the American Society of Anesthesiologists annual meeting,⁴ and our results compare very favorably to the best in our country.

Thus, an experienced, cohesive group, careful planning, adequate staffing, proper patient screening, patient monitoring second to none, and a biomedical engineer who visited the site prior to the group's arrival and made sure that all equipment was functional and remained so throughout the visit reflected the outcome of our patients. We carried all our noncontrolled drugs, supplies, and equipment with us. We take pride in that a team of cardiologists visits the location approximately every 6 months to follow up on all the children we cared for to evaluate their health status and quality of life. We have actually put in practice all along most of the suggestions made by Warner *et al.*²

Quality-of-care measurements are moving toward the use of standardized quality of life indicators and away from morbidity and mortality figures as these are becoming less frequent. It is in the follow-up

of patients and their health status at a time distant from the surgical intervention that the appropriateness of their management is now being assessed. We strongly believe that good patient outcome is possible during volunteer medical services throughout the world if one follows a stringent set of rules similar to what we have outlined.

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In Reply:—The comments of Drs. Mayhew and Burrows are cogent and relevant. We believe that anesthesiologists practicing abroad for elective surgery should strive to apply the standards of care they apply in their home practices as closely as possible. There should be a clear understanding of what are appropriate perioperative risks for elective surgery in the selection of patients, the techniques used, the monitoring applied, and the skill mix of personnel involved. Credentialing of volunteers is an especially complex issue. We agree that it would be advantageous to have fully trained pediatric anesthesiologists available for all missions. This is not always practical. Although agencies may not want to limit their programs because of a lack of pediatric anesthesia expertise, programs treating pediatric patients should require their volunteers to have extensive experience dealing with children and with perioperative issues specific to the planned surgical procedures (e.g., bleeding and edema related to palate repair). The most important but also most elusive credential is the ability to understand one's limitations in the austere setting, and to exercise what we all understand (but cannot precisely describe) as "good clinical judgment."

The ultimate goal of many of the short-term voluntary programs is to transfer the ability to provide service to entirely local providers through modeling, training, and perhaps provision of supplies. Dr. Khambatta *et al.* describe an excellent and intensive approach to pediatric cardiac anesthesia in a developing country. They are to be congratulated for their devotion to this cause. Many of the concerns of plastic surgical groups who undertake to train interested general surgeons, dentists, or otolaryngologists are quite different than the tertiary level of care of Heart Care International. However, the important themes they reiterate—meticulous planning, team cohesion and continuity, good patient selection, and follow-up assessment—are the foundations for success in all of these endeavors.

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The BIS Inverse Problem and Pharmacodynamics

To the Editor:—In their recent article, Kuizenga *et al.*¹ studied pharmacodynamics of propofol using a processed electroencephalogram. In the results, they state, "... we observed an unexplained increase in the BIS value during increasing blood concentrations." The question remains: unexplained by whom?

The Bispectral Index (BIS) is good in assessing depth of hypnosis. It is, however, unrealistic to expect BIS to yield a meaningful value when the electroencephalogram is pathologic or changes rapidly. It suffers also from the "inverse problem" of all single electroencephalogram descriptors: a certain BIS value may be produced by many different electroencephalogram samples with different patterns of different physiologic meaning. Samples with propofol could, for instance, have spindles or short suppressions,² or epileptic activity with sevoflurane mask induc-

tion,^{3,4} and yield the same BIS values as electroencephalogram samples without these patterns. All of these are readily seen and counted visually but not with analyses like BIS or aperiodic analysis. Epileptiform patterns in sevoflurane anesthesia, for instance, result in unpredictable BIS values (V. Jääntti, Tampere, 2001, unpublished observation).

In this kind of study, both electroencephalogram patterns and artifacts should first be analyzed by an experienced electroencephalogram reader. Then, methods which can track fast changes like filter banks, including wavelets, should be applied, and only after that should aperiodic analysis and BIS be used. Only after that we could call fluctuation of the BIS value "unexplained."

Apart from signal analysis, the physiology of brain is also different during rapidly increasing concentrations of anesthetic than during

steady state: consider, again, the epileptogenic effect of sevoflurane during rapid mask induction.^{3,4}

In summary, in this kind of study, careful inspection and analysis of electroencephalographic patterns from raw and filtered time domain electroencephalogram might explain the "unexplained." What did the raw electroencephalogram look like during those intervals of "unexplained" increase?

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In Reply:—We thank Drs. Jäntti and Alahuhta for reading carefully our manuscript and appreciate their comments. Drs. Jäntti and Alahuhta focus on the phrase "we observed an unexplained increase in the BIS value during increasing blood concentrations" in the Results section of our manuscript.¹ We did not intend to explain how either electroencephalographic amplitude or Bispectral Index (BIS) are derived from the analog electroencephalogram, but to determine the reproducibility of the pharmacodynamic relationship between blood concentrations of propofol and electroencephalographic amplitude and BIS during sequential propofol infusions. The propriety BIS algorithm was developed to show a unidirectional decrease of its value upon decreasing levels of consciousness.² We did not observe burst suppression, spindles, spike activity, or electromyographic or other artifacts in the analog electroencephalogram during this episode in these two patients. Other derived parameters, like spectral edge 95% and median frequency, did not show an increase. Therefore, we could not explain the increase of the BIS value during the increasing concentration of propofol. More recent software versions of the BIS algorithm might not have shown such an increase.

Jäntti and Alahuhta also state that the physiology of the brain during rapidly increasing concentrations is different from that during steady

state situations. This statement is concordant with our explanation for the poor prediction of electroencephalographic effect during the sequential infusions, that as a result of the complexity of the brain the electroencephalographic signal is not necessarily a simple function of the blood concentration, especially during changing blood concentrations.

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A New Cuff Design Prevents N₂O Diffusion

To the Editor:—It was interesting to read the article by Combes *et al.*¹ We fully agree with the authors that, during N₂O anaesthesia, the main cause for the increase in intracuff pressure, when filled with air, is the diffusion of N₂O into the cuff. Other factors, such as the diffusion of O₂ into the cuff and the warming of gases inside the cuff, play a small role in the increase in the intracuff pressure. Various factors affect the rate of diffusion of N₂O, including the difference in partial pressure of N₂O inside and outside the cuff, the area available for diffusion, and the cuff material. We have evaluated pressure changes in a new design endotracheal tube cuff, the Portex Soft Seal (Portex Ltd., Hythe, United Kingdom) during N₂O anaesthesia.² The new design prevented increases in the intracuff pressure and remained stable throughout the procedure with pressure changes similar to those in the saline group observed by Combes *et al.*¹ In the Portex Soft Seal tube cuff, the plasticizer added to soften the polyvinyl chloride makes the cuff much less permeable to N₂O despite having a thickness of 0.06 mm, which is very similar to the Mallinckrodt Lo-Contour (Athlone, Ireland) used by Combes *et al.*¹ We agree with the authors that filling the cuff with

saline is not recommended in routine clinical practice as the cuff is not designed to be filled with saline. We believe that using the Portex Soft Seal cuff prevents tracheal injury caused by the increased intracuff pressure due to N₂O diffusion and makes filling the cuff with saline and monitoring the cuff pressure throughout the anesthetic course unnecessary.

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In Reply:—I read with interest the letter of Dr. Al-Shaikh. As we have emphasized it in our article, filling the cuff with saline was done for the purpose of the study and cannot be recommended in routine clinical practice.¹ It is clear that a tube cuff design that prevents increase in the intracuff pressure during N₂O anesthesia is of great interest for daily clinical practice. The new design endotracheal tube cuff evaluated by Dr. Al-Shaikh *et al.*,² because of a very low permeability to N₂O, allowed a very good intracuff pressure stability during N₂O anesthesia and seems particularly promising. Nevertheless, we believe that intracuff pressure measurement after cuff inflation is necessary. Indeed, the pressure/volume curve of endotracheal tube cuff has an asymptotic shape and reaches rapidly (for low volume of the cuff) a relative plateau around which very small variations of volume are associated with large increase in cuff pressure. Then, even without N₂O diffusion, the cuff pressure can be high because of an inappropriate initial cuff

inflation. We recommend, whatever the endotracheal tube device used, at least one cuff pressure control after initial cuff inflation to prevent excessive intracuff pressure and occurrence of tracheal injury.

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Kaolin-activated Thromboelastography

To the Editor:—We read with interest the elegant study by Avidan *et al.*¹ investigating the effect of different activators on the thromboelastogram in the absence and presence of aprotinin. Although they suggest that kaolin use with the thromboelastogram has not been reported, we recently presented data in abstract form from an ongoing *in vivo* study.²

In 12 patients undergoing cardiopulmonary bypass and receiving low-dose aprotinin (1 million-KIU loading dose), we found no differences between the celite- or kaolin-activated R time, K time, angle, or maximum amplitude either prior to or after the aprotinin loading dose (table 1). Our preliminary results do not support the authors' findings of a shorter R time and greater angle and maximum amplitude in kaolin-activated parameters in the *absence* of aprotinin.

In the *presence* of aprotinin, there was a tendency toward similar prolongation of the R and K times (40%) and decreases in the angle

(10%) in both celite and kaolin samples compared to baseline (table 1), although this interim analysis was not adequately powered to confirm statistical significance. Until completion of our study in patients receiving both high- and low-dose aprotinin, we echo the caution of Avidan *et al.* that changes in the celite- or kaolin-activated thromboelastogram R time may occur in the presence of aprotinin.

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Table 1. Celite- and Kaolin-activated Thromboelastogram Parameters before and after Aprotinin Loading Dose

	Celite (before LD)	Kaolin (before LD)	Celite (after LD)	Kaolin (after LD)	P Value (ANOVA)
R time (mm)	10.4 ± 2.9	9.9 ± 1.5	14.0 ± 1.0	15.4 ± 3.2	0.1
K time (mm)	2.7 ± 0.9	2.3 ± 0.7	3.8 ± 2.1	3.7 ± 2.2	0.3
Angle (°)	75.1 ± 5.2	73.3 ± 5.3	66.6 ± 10.7	66.6 ± 10.2	0.2
MA (mm)	71.7 ± 6.1	68.5 ± 8.2	66.7 ± 8.9	68.0 ± 8.9	0.7

LD = loading dose; ANOVA = analysis of variance; MA = maximum amplitude.

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In Reply:—I am thankful for the opportunity to comment on the interesting letter from Dr. Pivalizza and Dr. Warters. They, too, have demonstrated that aprotinin affects the thromboelastograph trace even when kaolin is used as the activator. We did not mention their work in our article as our submission to ANESTHESIOLOGY preceded their abstract. Unlike our results, they have not noted a difference in the R times and maximum amplitudes with celite and kaolin activation. This may relate to the concentrations of these activators. As the kaolin and celite concentrations are increased, there is a decrease in the R times and a slight increase in the maximum amplitudes of the thromboelastograph

traces. We used 37 μ l of 1% kaolin and 1% celite solutions per milliliter of blood. Perhaps the kaolin or celite concentration used by Dr. Pivalizza and Dr. Waters was different. This illustrates one of the potential problems with near patient tests, such as thromboelastography. It is difficult to assure quality control and to standardize results among centers.

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Causes of Elevated Intraocular Pressure during Prone Spine Surgery

To the Editor:—In the article by Cheng *et al.*¹ on the effect of prone positioning on intraocular pressure (IOP) during spinal operations, they reported an increase in IOP from 19 ± 1 mmHg supine baseline to 40 ± 2 mmHg at the end of the procedure in the prone position. The range of IOPs at the end of the case in the prone position was 25 to 54 mmHg. Despite an approximate 47% reduction in ocular perfusion pressure (MAP – IOP) from baseline to the end of the case, and an average duration of 320 ± 107 min, no visual deficits were reported. The very high IOP in this study is difficult to accept at face value.

As the authors pointed out, Lam and Douthwaite² reported that IOP increased in awake volunteers from 13.5 ± 2.01 mmHg baseline supine to 20.0 ± 3.27 mmHg in the prone position. Why was the initial prone value of IOP (27 mmHg) so much higher in the study of Cheng *et al.* with anesthetized patients compared to the study of Lam and Douthwaite with awake volunteers? Our studies have also shown lower initial prone values of 18.1 ± 0.8 mmHg, with peak prone IOP values of 24.6 ± 1.1 mmHg.³ It seems that that technical error might be responsible for this difference. One of the most important causes of spuriously high recordings of IOP is inadvertent pressure on the globe while retracting the eyelids. This problem can be difficult to avoid when there is significant periorbital/conjunctival swelling, particularly in the prone position. In addition, contact of the tonometer with the globe must be made at a 90° angle. Failure to perform IOP measurements with these guidelines will result in erroneous values.

Although the authors discuss the possibility that an increased arterial carbon dioxide tension (Paco₂) may increase the IOPs, it is unlikely that this would result in IOPs up to 54 mmHg. We have found that the measurement of IOPs during emergence (unpublished data) results in greatly elevated IOPs, similar to normal awakening.^{4,5} In the study of Cheng *et al.*, although reportedly not statistically significant, there is a trend toward increasing mean arterial pressure (MAP) at the end of the case ("prone 2" MAP, 84 ± 11 mmHg, and "supine 2" MAP, 91 ± 11 mmHg) compared to the initial supine and prone MAP measurements ("supine 1" and "prone 1" MAPs of 72 ± 7 and 75 ± 9 mmHg, respectively). This is consistent with "lightening" of anesthesia and perhaps early emergence from anesthesia. It is plausible that partial emergence from anesthesia, in conjunction with the technical challenge of retracting edematous eyelids, contributed to the extremely high IOPs ob-

served in this study at the end of the procedure in the prone position.

Another explanation for increased IOP is the effect of fluid administration. With increasing duration of surgery, one would expect greater fluid requirements. The significantly elevated IOP (31 mmHg, "supine 2") even after return to supine at the conclusion of surgery, suggests this mechanism may be operative. Our studies also demonstrated a statistically significant increase in IOPs at the end of the case (average duration, 450 min) in the supine position (21 ± 1.1 mmHg, SEM), compared to baseline values (12 ± 0.7 mmHg), with very large average estimated blood loss and intravenous fluid administration.³ Unfortunately, this study lacks a control supine group to evaluate this possibility in isolation. Elective supine cases should be matched for duration, estimated blood loss, and quantity of intravenous fluid administration. Given that those cases are difficult to find, it may be acceptable to study IOP in either supine operations of long duration or with comparable estimated blood loss and large intravenous fluid administration. A control group would allow changes in IOP caused solely by position to be evaluated independently of the other factors. The study results, while interesting and relevant, must be interpreted in the absence of these necessary controls.

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In Reply:—We read with attention the letter by Lee *et al.* regarding our study, “The Effect of Prone Positioning on Intraocular Pressure in Anesthetized Patients.” There are several points in their letter that we would like to address. First, we would like to point out that we titled our study “The Effect . . .,” rather than “The Causes of Elevated IOP. . .” since the sole purpose of our study was to observe the effects of prone positioning on intraocular pressure (IOP) in anesthetized patients.

Second, while we agree that our IOPs in the prone position might be considered strikingly high by some, we found that the fact that the results were reproducible in 20 patients seems to indicate that they may indeed be real. For the authors of the letter to suggest that the results are too high because none of our patients had visual deficits despite a lowered perfusion pressure is ludicrous, as it is plainly known that perioperative visual loss is extremely rare and of as yet unknown cause(s).

Third, the authors of the letter also attempt to justify their claim that our results are flawed by comparing the data from a previous study of *awake* volunteers¹ with those we obtained in *anesthetized* patients. Extrapolating data from awake volunteers for comparison to anesthetized patients is denying any effects of anesthetics on physiology.

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Fourth, the authors question our methods in terms of how the IOP was measured. Using a pinned head holder prevents all of the obstacles they delineate in their letter (*i.e.*, eyelid retraction, and approach to the eye with tonometer).

Fifth, it is difficult for us to take at “face value” the authors’ reference to their unpublished data.

We appreciate the commentaries and criticisms of our study. This study is just the a piece of the puzzle that is perioperative visual loss. We look forward to seeing further data in this area of research.

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Thrombosis after Deep Hypothermic Circulatory Arrest with Antifibrinolytic Therapy: Is Factor V Leiden the Smoking Gun?

To the Editor:—I read with interest the report by Fanashawe *et al.*¹ describing two cases in which deep hypothermic circulatory arrest (DHCA) with aminocaproic acid was complicated by massive thrombosis once heparin was reversed by protamine. Postmortem studies were conducted to search for occult hypercoagulable states, and one patient turned out to carry factor V Leiden (FVL). The authors assert that FVL was “almost certainly contributory” to the event and suggest consideration of preoperative testing for FVL to guide decision-making in cases in which DHCA is planned.¹

While I find these cases very interesting, there is no evidence in the present literature to support that FVL “almost certainly” contributed to the thrombotic risk in one of these patients. Although this conclusion is physiologically plausible, FVL does not always carry risk in specific clinical settings. One example is the lack of association of FVL with venous thrombosis after orthopedic surgery.^{2,3} Also, most population studies have failed to show an association of FVL with coronary thrombosis and stroke,^{4–7} although FVL may increase arterial thrombotic risk in specific patient subgroups.^{8,9} A few studies have suggested (but not proven) that FVL may carry clinical significance in cardiac surgery,^{10,11} and it is possible that larger studies may characterize this significance in greater detail. However, it is currently pure speculation to suggest that risk of DHCA with aminocaproic acid is increased by the presence of FVL.

It is equally misguided to suggest preoperative screening for FVL and to base patient care decisions on anecdotal reports. No prospective, controlled studies exist to validate the use of preoperative testing for FVL, so it is very unclear how this information should be used. If my next patient is known to carry FVL, what do I do next? Is antifibrinolytic therapy unnecessary? Is it dangerous? Is DHCA safe with aprotinin? Do we need to cross-match fewer units of erythrocytes? Since these genetic studies are becoming widely available, it is concerning

that a possible abuse of this test may follow the authors’ suggestions. If the authors themselves had tested their second patient for FVL preoperatively, they may have felt better justified in using aminocaproic acid, only to be dreadfully surprised when they observed a similar thrombotic event.

The risk of FVL in cardiac surgery is presently unclear. The risk of FVL in the presence of cardiac surgery, DHCA, and antifibrinolytic therapy is even less well understood. Fanashawe *et al.* perform an important service by reporting a tragic complication of antifibrinolytic therapy and therefore highlight the need to carefully research the possible contributing factors. However, we must be exceedingly cautious not to draw conclusions about genetic risk factors or issue recommendations for patient care based on anecdotal reports. What is needed is a large-scale association study, examining gene-environment interactions, to characterize the role of FVL in cardiac surgery. Only then will we be justified in concluding that FVL is “contributory” toward a particular outcome or in proposing preoperative screening in an effort to reduce risk.

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In Reply:—We appreciate the comments presented by Dr. Donahue regarding our report of two cases of fatal aortic thrombosis in association with synthetic antifibrinolytic therapy and deep hypothermic circulatory arrest. We agree that prospective large-scale association studies are necessary in order to make recommendations regarding genetic risk factors and the expenditure of healthcare dollars. The purpose of our report was not to definitively identify any causative agents or conditions in these thrombotic events, but merely to highlight their occurrence in a population of patients that is potentially at higher risk. Certainly, it would not be feasible to recommend that all asymptomatic patients scheduled to undergo cardiac surgery undergo genetic testing for a mutation in the expression of factor V. However, it behooves us as practitioners to be vigilant in our preoperative assessment of patients who may have a personal or a family history of hypercoagulability. This is true because even asymptomatic carriers of the factor V Leiden mutation have an appreciable incidence of thromboembolism that increases when risk factors for thrombosis are present.¹

The use of antifibrinolytic therapy and the consumptive coagulopathy seen in cardiac surgical patients make this population quite distinct from orthopedic surgical patients or cardiovascular medicine patients in whom factor V Leiden has not been associated with increased thrombosis risk. Prophylactic antifibrinolytic therapy is given with impunity to hundreds of thousands of patients per year who undergo cardiac surgery.² The use of these drugs has been of apparent benefit to blood conservation practices, but a careful risk-benefit analysis should be performed in light of the question of a small but worrisome possibility that fatal hypercoagulability may be associated with antifibrinolytic therapy.

Since the publication of our case report, we have cared for a patient who experienced fatal intraoperative venous thrombosis in association with aprotinin therapy and deep hypothermia during valve replacement surgery. While the analysis of this case is ongoing, the initial investigation has shown that the patient's son carries the factor V Leiden mutation.

Though causality should not be inferred from our small anecdotal experience, these types of associations are important in forming the basis for future population-based studies. Finally, the prudent clinician should carefully consider the relative risks and benefits of antifibrinolytic therapy in cardiac surgical patients with known hypercoagulable conditions, such as the factor V Leiden mutation.

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Hemodynamic Stability after Pediatric Epidurals

To the Editor:—In the December 2001 issue of *ANESTHESIOLOGY*, Dr. Sciard *et al.*¹ describe continuous lumbar plexus blocks performed in two pediatric patients, aged 4 yr and 16 months. They state, "The absence of epidural spread was assessed by hemodynamic stability after the performance of the blocks." Hemodynamic stability should not be used to exclude an epidural block since infants and children up to approximately 5 yr of age show little or no change in blood pressure following central neuraxial blockade, even dense spinal blockade to high thoracic levels.^{2,3}

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In Reply:—We would like to thank Dr. Rebecca Lowery for her interest in our case report. The quote to which Dr. Lowery referred was only half of our original sentence, which read “The absence of epidural spread was assessed by hemodynamic stability after the performance of the blocks *and* by the presence of an adequate reaction to pin prick of the opposite leg after emergence.” Although we also recognize that the hemodynamic changes associated with neuroaxial blockade may be absent or less pronounced in infants and young children, it is critical to acknowledge also the following. (1) Dohi *et al.*¹ and Oberlander *et al.*² reported on a very limited number of infants and young children. Although Dohi *et al.*¹ reported that significant hypotension might be observed in young children around 5 yr of age and older, extrapolating from these articles, the concept that 5 yr represents the absolute lower limit for children to be exposed to such risk following a neuroaxial block may be excessive. (2) More importantly, in infants and young children, any diagnosis is often the result of the convergence of

symptoms, and therefore, even if the risk is minimum, it should not be ignored. (3) We would like to believe that Dr. Lowery is not recommending not monitoring blood pressure following the performance of lumbar plexus block in infants and young children. In conclusion, we are happy to have provided an opportunity for Dr. Lowery to remind us about the expected hemodynamic changes associated with neuroaxial blocks in infants and young children. However, we would like to maintain that monitoring blood pressure is essential when performing a lumbar plexus block, even in this patient population.

Didier Sciard, M.D., Maria Matuszczak, M.D., Ralf Gebhard, M.D., Jennifer Greger, M.D., Tameen Al-Samsam, M.D., Jacques E. Chelly, M.D., Ph.D., M.B.A.* *The University of Texas Health Science Center at Houston, Houston, Texas. Jacques.Chelly@uth.tmc.edu

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Anesthesia Respiratory Circuit Failure

To the Editor:—Safe delivery of a general anesthetic requires an intact anesthesia respiratory circuit. An anesthesia circuit leak test is recommended as part of the routine preoperative anesthesia machine check.* However, a negative leak test preoperatively does not guarantee that a significant respiratory circuit leak will not develop during the case.

We report here a series of 13 cases of anesthesia circuit failure resulting from circuit leaks found in the tubing of a (click-expandable) poppel 96-in adult anesthesia circuit (catalog No. K150E6000; Mallinckrodt Inc., St. Louis, MO). These cases occurred over a 1-month period at the Johns Hopkins Hospital (Baltimore, Maryland). It is of interest that the circuit design had recently been modified without notification from Mallinckrodt. These newer circuits are of wider diameter and narrower wall thickness.

In all 13 instances of circuit failure, a leak check was performed by the anesthesiologist prior to the case. In three cases, the leak test results were positive, with the magnitude of the anesthesia circuit leak greater than 10–15 l/min fresh gas flow. These circuits were replaced prior to the start of the cases. In 10 cases, although the preoperative leak test was negative, a leak and anesthesia circuit failure developed after the induction of general anesthesia. Two of these circuit failures occurred well into the operative cases, at approximately 45 min and

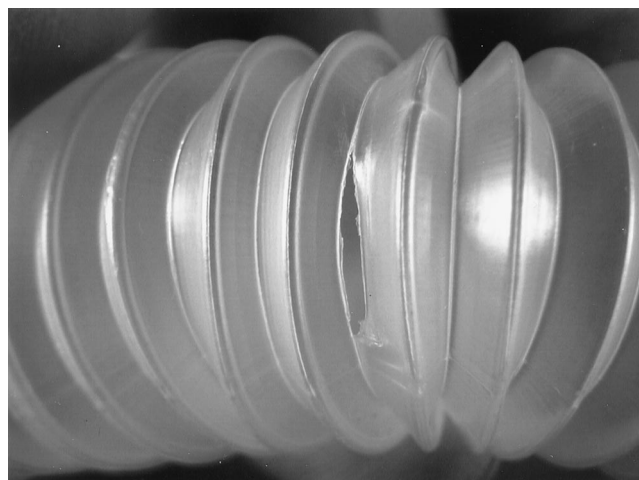


Fig. 1. Defective anesthesia circuit.

2 h, respectively. All 10 postinduction circuit failures were associated with an inability to ventilate the patients. Nine cases of circuit leak occurred either during or sometime after placement of the circuit tubing into the tube tree holder. In four cases, circuit failure occurred after the patient's head was repositioned. Each patient was rescued through emergent replacement of the respiratory circuit.

Upon inspection of the 13 failed circuits, 11 had a crack or tear in an inner fold of expandable circuit tubing (fig. 1). The other two circuit leaks were manifestations of longitudinal tears in the circuit tubing at the joint between the tubing and the end connector. On two occa-

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*Anesthesia Apparatus Checkout Recommendations, 1993. Available at: <http://www.fda.gov/cdrh/humfac/anesckot.html>

sions, the respiratory circuits severed into two separate pieces of hosing at an inner fold of circuit tubing. These were respiratory circuits that had passed preoperative leak tests but subsequently developed large and unacceptable leaks during routine manipulations and use.

A self-inflating bag and an additional anesthesia circuit were immediately available in all situations. No patient experienced significant

oxygen desaturation or injury. In spite of this, anesthesia respiratory circuit failure did occur at critical times and added risk.

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Echo Bib

To the Editor:—When intraoperative transesophageal echocardiography is planned, baseline images are often desirable before surgical incision, especially if electronic suppression of electrocautery interference is not available. However, out of fear of droplet contamination of the operative field, we had been reluctant to manipulate the echo probe before completion of surgical prepping and draping. Now we secure an additional interference-free imaging opportunity by hanging a barrier drape across the chin of the patient before the surgical skin preparation is started (fig. 1). Commercially available transparent drapes bearing an adhesive strip are convenient for this purpose. Sternal wound infections are rare, and intraoperative echocardiography is not a quantitatively obvious risk factor. However, any plausible measure should be taken against so dreaded a potential complication.¹

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*Massachusetts General Hospital, Harvard Medical School, Boston, Massachusetts. talston@partners.org

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Fig. 1. An echo bib in place. Rotated 180°, the drape can also serve as a lobster bib.

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ECMO Resuscitation after Massive Pulmonary Embolism during Liver Transplantation

To the Editor:—Pulmonary embolism¹ during liver transplantation has been the subject of many case reports and is often fatal.^{1,2} We report successful resuscitation of a liver transplant patient using ECMO after massive pulmonary embolism, as a reminder that ECMO should be considered in this setting.

The patient was a 54-yr-old woman with primary biliary cirrhosis complicated by ascites and pruritus. She had no history of thromboses. Her prothrombin time, fibrinogen concentrations, and platelet count were within normal limits. The patient was hemodynamically stable during induction of anesthesia and throughout hepatectomy. One gram ε-aminocaproic acid was given 3 h after incision, and total venovenous bypass was started at a flow of 3 l/min. At the end of the anhepatic phase, pulmonary artery pressures rose suddenly, and the patient became hypotensive. Simultaneously, venovenous bypass flow was noted to drop to 1 l/min. She rapidly developed electromechanical dissociation, and cardiac compressions were performed. A transesophageal probe was placed, and echocardiography revealed a massive clot

in the right atrium, right ventricle, and on the mitral valve. During resuscitation, the patient received 100 mEq sodium bicarbonate, 0.4 mg scopolamine, and infusions of norepinephrine and calcium chloride but remained hypotensive. Heparin, 8,000 U, was given, and within 45 min of diagnosis, the patient was placed on venoarterial extracorporeal membrane oxygenation (ECMO) using the existing left femoral venous line and cut-down on the right femoral artery. Hemodynamics improved immediately. Vascular anastomoses were completed 3.5 h after vena cava cross-clamping.

The patient regained full consciousness in the intensive care unit on postoperative day 1, and ECMO was discontinued in the operating room after completion of the biliary anastomoses. The patient's trachea was extubated on day 4, and she was discharged home on day 12.

A week later, the patient was readmitted, complaining of weakness, and was found to have a large inferior vena cava thrombus extending from the renal vein to the right atrium with 90% caval occlusion. She developed multiorgan failure and died 9 weeks after transplantation. The clinical course of this patient suggests an underlying prothrombotic diathesis of unknown etiology (factor V Leiden mutation was not found in this patient).

Supported by the Department of Anesthesiology, University of Michigan, Ann Arbor, Michigan.

Several important points are illustrated by this report. First, earlier use of transesophageal echocardiography may have allowed earlier diagnosis of the developing thrombus. Second, it was clear that rapid institution of ECMO was life-saving during the transplant, for which thrombectomy is considered especially risky.¹ Others have reported using venovenous oxygenation for a similar patient,⁵ and this technique could have been instituted here while awaiting the ECMO team.

In summary, routine use of transesophageal echocardiography during liver transplant may aid in preventing catastrophic pulmonary embolism. In centers where ECMO is available, it is a valuable adjunct to the treatment of massive, acute intraoperative pulmonary embolism during transplantation.

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T-wave Abnormalities on Preoperative Electrocardiogram Caused by "Cardiac Memory" Related to Intermittent Preexcitation

To the Editor:—T-wave inversions on the electrocardiogram attributed to abnormal ventricular repolarization may reflect the presence of ischemic heart disease. However, alteration in the ventricular depolarization pattern seen in intermittent left bundle branch block, ventricular pacing, or preexcitation can also cause abnormal ventricular repolarization (T-wave abnormalities on the electrocardiogram) persisting even after normalization of ventricular depolarization is restored.¹⁻⁴ This curious phenomenon is termed "cardiac memory." We present a patient with intermittent preexcitation in whom T-wave inversions on a preoperative electrocardiogram seen during the absence of δ waves could be explained by cardiac memory but not by myocardial infarction/ischemia.

A healthy 38-yr-old man (weight, 70 kg; height, 176 cm) was scheduled for left knee arthroscopy under spinal anesthesia. He had no remarkable past medical history. He did not smoke, and he was not taking any medications or excessive alcohol. A standard 12-lead electrocardiogram on admission showed T-wave inversions in the limb

leads of II, III, and aVF, which could not exclude inferior myocardial infarction/ischemia (fig. 1A). However, he was completely asymptomatic, and he had never experienced chest pain. Laboratory data, including cardiac enzymes, was normal, and the chest radiograph was unremarkable. Transthoracic echocardiography was normal, without any regional wall motion abnormalities. An exercise stress test was not performed because of left knee pain. At the preoperative anesthetic interview, the patient reported that he had experienced a few episodes of palpitation that spontaneously dissipated within a few minutes. Therefore, we performed electrocardiographic monitoring at his bedside, which demonstrated intermittent preexcitation with obvious δ waves. We thought the T-wave inversions on the admission electrocardiogram might likely be caused by cardiac memory related to intermittent preexcitation. The next day, spinal anesthesia was administered with 0.5% hyperbaric bupivacaine (Marcain® spinal 0.5% hyperbaric; AstraZeneca, Osaka, Japan), 2.0 ml, at the L3-L4 interspace. The anesthetic course was uneventful. There were no episodes of parox-

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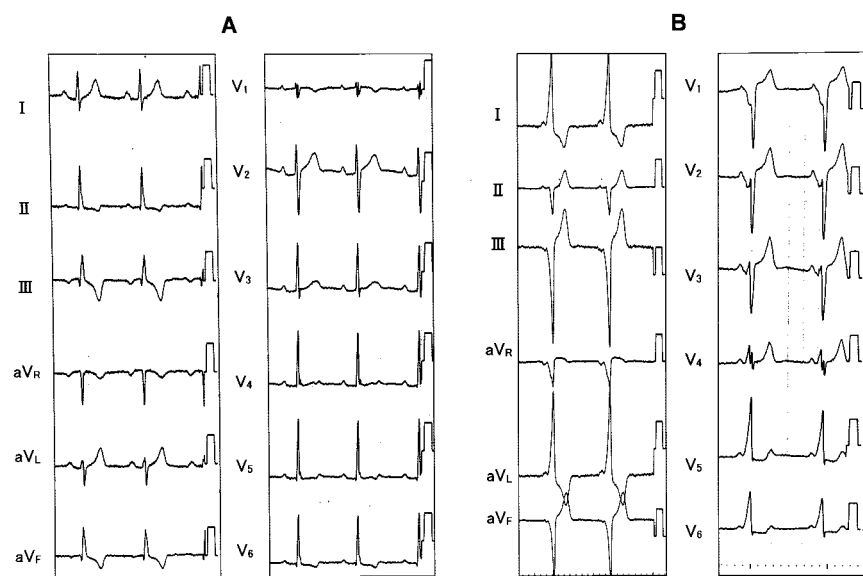


Fig. 1. Standard 12-lead electrocardiogram obtained from a patient with intermittent preexcitation. (A) The electrocardiogram on admission presented the absence of δ waves. Marked T-wave inversions were observed in leads II, III, and aVF. (B) The postoperative electrocardiogram demonstrated ventricular preexcitation. Note negative δ waves in leads II, III, and aVF, where T-wave inversions were marked during the disappearance of δ waves.

ysmal supraventricular tachycardia, although δ waves appeared intermittently throughout the surgery.

A postoperative 12-lead electrocardiogram showed ventricular pre-excitation with negative δ waves in leads II, III, and aVF, the leads where T-wave inversions had been observed most prominently (fig. 1B). These findings further support that the T-wave inversions on the admission electrocardiogram were caused by cardiac memory rather than by myocardial ischemia. No further cardiac investigations have been performed because of a lack of symptoms.

Normally, ventricular repolarization proceeds in a direction opposite to that of depolarization, resulting in the same polarity for the T wave as for the QRS complex.³ When cardiac memory is noted, the T-wave direction is typically similar to the direction of the QRS complex during the period of altered ventricular depolarization (e.g., left bundle branch block pattern) but not that of the QRS complex after normalization of ventricular depolarization, hence, the term "memory."¹ In cardiac memory phenomena associated with preexcitation, the vector of T-wave changes occurring after loss of preexcitation is in the same direction as the previous δ -wave vector, and thus, T-wave inversions are most commonly observed in leads with negative δ waves.³ In this case, the preexcited electrocardiogram showed negative δ waves in leads II, III, aVF, and V1, which may indicate a right posteroseptal accessory pathway. The T-wave inversions were marked in leads II, III, and aVF.

We cannot be absolutely certain that the T-wave inversions were caused by cardiac memory related to intermittent preexcitation because no further investigations, including exercise stress test and/or

coronary angiography, were performed to rule out the possibility of myocardial ischemia. However, in a healthy young patient with no cardiac risk factors or symptoms and a normal heart on echocardiography, as in our case, it is unlikely that myocardial ischemia-induced T-wave inversions occur.

In conclusion, we present a case suggesting cardiac memory, a curious electrophysiologic phenomenon that is unfamiliar to anesthesiologists. This phenomenon should also be considered if T-wave inversions are observed on the electrocardiogram before anesthesia.

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Avoid Luer Connectors on Blood Pressure Cuffs

To the Editor:—As new products are introduced, novel potential problems may be noted.¹ My hospital system recently changed intravenous tubing systems to a set with valve ports (Smart Site; Alaris Medical Systems, San Diego, CA). These sets have female luer lock valve ports and comply with needle-free protocols. I find that the intravenous valve ports (especially when filled with propofol) are similar in appearance to the female luer lock connector and tubing of our blood pressure cuffs (Classic Cuf 2103; Critikon, Tampa, FL; fig. 1). Indeed, the noninvasive blood pressure inflation hose can be connected di-

rectly into the intravenous port (fig. 2). Activating our noninvasive blood pressure monitor (Spacelabs Medical 90426, Redmond, WA) in this case would deliver approximately 900 ml of air over 30 s intravenously, potentially causing serious injury or death.

Luer connectors, originally designed for attaching a syringe to a needle, have become a standard for parenteral systems. Because of the simple, effective design and added locking feature, it has been adopted for use in enteral, respiratory, epidural, intrathecal, and other miscellaneous systems as well. When syringes or tubings from different systems are misconnected, patient injury may result. A Spacelabs Medical safety alert² warned that:

... [A] hazardous situation results from connecting the air tubing from a Spacelabs Medical NIBP monitor to a vascular access, such

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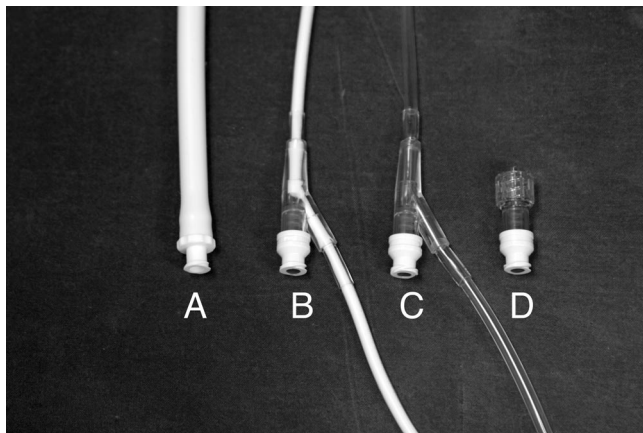


Fig. 1. (A) Critikon Classic Cuf 90426 connector. (B) Alaris extension set 30263E filled with propofol. (C) Alaris extension set 30263E. (D) Alaris "heparin lock" 2000E.



Fig. 2. Spacelabs Medical blood pressure inflation hose connected to an Alaris "heparin lock" 2000E instead of the Critikon Classic cuff 90426 connector.

as an in-place patient IV. If air pressure is delivered to the patient during such a connection, it can cause serious injury or death. Spacelabs has received four reports of such incidents, one of which resulted in a patient's death. . . . We recommend that any blood pressure cuff using a luer connector fitting be placed on a different limb than the IV needle. . . . Warning labels will be sent for you to place on your NIBP monitors and hoses. . . . Use extreme caution when connecting the monitoring hoses to a blood pressure cuff that has a luer fitting.

There are Food and Drug Administration Manufacturer and User Facility Device Experience reports that mention misconnection of Spacelabs Medical noninvasive blood pressure inflation hose to intravenous connectors.³

Other misconnection injuries have been reported. Recent deaths have been caused by epidural local anesthetics given intravenously,⁴ intravenous chemotherapeutics given intrathecally,⁵ and enteral feedings given intravenously.⁶ A European Committee for Standardization Health Care Forum Focus Task Group⁷ recommended that luer connectors should be used only with syringes and devices connected to the vascular system, and that two new incompatible connectors be designed for the enteral and respiratory routes. Cyna *et al.*⁸ proposed an alternative incompatible connector specifically designed for epidural and intrathecal use. It may take time before changes like these are adopted and a new standard established.

I would recommend that one avoid luer connectors on blood pres-

sure cuffs. A well-designed system should not allow a single error to result in such a catastrophe. There are a multitude of alternate connectors and adaptors available for blood pressure cuffs.

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