

Methodological Remarks on Transcranial Doppler Ultrasonography for PFO Detection

To the Editor:—With great interest and appreciation, we read the article of Stendel *et al.*¹ about contrast-enhanced transcranial Doppler ultrasonography (c-TCD) for detection of a patent foramen ovale (PFO) before surgery in the sitting position. Although we agree that this is a significant alternative to the gold standard set by contrast-enhanced transesophageal echocardiography, we would like to make some methodologic remarks.

Contrast-enhanced transcranial Doppler ultrasonography is an indirect approach to detect right-to-left shunting and does not allow for an exact anatomic localization of shunts. Precisely, in the mentioned diagnostic setting, a PFO is likely to cause high-intensity transient signals in c-TCD, but one should be aware that a PFO is not proven by this method. To ensure the diagnosis, contrast medium has to cross the atrial septum following the pressure gradient produced by a Valsalva maneuver during an interval of time that excludes pulmonary passage. Therefore, this interval should start with presence of contrast agent in the right atrium and should not exceed 10 cardiac cycles. Stendel *et al.*¹ allow for 3–15 heart cycles after 5 s injection and 5 s Valsalva maneuver, following the protocol of Schwarze *et al.*,² but pulmonary arteriovenous fistulas can be a reason for the detection of high-intensity transient signals during the mentioned interval.

In c-TCD, we frequently observe high-intensity transient signals that meet with the criteria to diagnose a PFO, whereas in contrast-enhanced transesophageal echocardiography, there is no evidence for it. These findings can be explained either by leakage of the capillary lung filter for microbubbles in some individuals or by pulmonary passage of

contrast medium parts below standard size, despite correct preparation and handling of the D-Galactose contrast medium.

When c-TCD is positive with Valsalva maneuver, we recommend an additional contrast injection without Valsalva maneuver to exclude pulmonary passage. In the normal heart cycle, an intermittent right-to-left pressure gradient occurs in the early systole because the tricuspid valve physiologically closes a little earlier than does the mitral valve. Therefore, with c-TCD, even at rest an atrial septal defect or a permanent PFO allowing for spontaneous right-to-left shunting can be detected. Spontaneous right-to-left shunting indicates high hemodynamic relevance of the shunt and may be a valuable finding to select surgical and anesthesiologic procedures to avoid intraoperative paradoxical air embolism.

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In Reply:—Kampen *et al.* address the problem of differentiating cardiac and pulmonary right-to-left shunts by means of contrast-enhanced transcranial Doppler ultrasonography (c-TCD). It is important to distinguish transpulmonary and intrapulmonary passage of air. The microbubbles generated by means of the echo-contrast medium used are small enough to pass the capillaries, but they are also short-lived and thus dissolve before they can pass through the lungs.¹ Bedell *et al.*² present a case of paradoxical air embolism (PAE) developing after venous air embolism as a result of transpulmonary passage of air. The authors conclude that for transpulmonary air passage to occur, the amount of air and overload of the pulmonary filter mechanism are crucial. Such an overload is not to be expected from the small amount of air generated by injection of the contrast medium mentioned.³

Some authors assume that late high-intensity transient signals due to contrast medium are indicative of pulmonary shunting.⁴ In contrast, Horner *et al.*⁵ report that the transit times in the presence of pulmonary shunts are comparable to those of cardiac shunts, concluding that TCD cannot distinguish between these two types of shunts. The time limit for the occurrence of microbubbles after the Valsalva maneuver is still a matter of debate: values range from 5 s (Job *et al.*⁶) to 25 s (Jauss *et al.*⁴). In determining the time limit, it is necessary to compromise between the sensitivity and specificity of c-TCD. The time interval of 15 cardiac cycles chosen in the study by Stendel *et al.*⁷ is within the range proposed in the recent literature⁸ and was associated with a specificity of c-TCD of 1 and a sensitivity of 0.92. This interval is also confirmed by the study of Karnik *et al.*⁹ This group investigated 36 patients by c-TCD, contrast-enhanced transesophageal echocardiography, and contrast-enhanced transthoracic echocardiography. In two

patients with an echocardiographically proven patent foramen ovale (PFO), microbubbles in c-TCD occurred only after 10–20 cardiac cycles. The specificity of TCD was 1.

Another important aspect to consider is that the interval before microbubbles occur is also dependent on shunt size. Therefore, a larger shunt volume may be assumed not only when a large number of high-intensity transient signals due to contrast medium are observed, but also when they occur after a short interval.⁸

False-positive results of c-TCD as reported in the letter by Kampen *et al.* were not observed with the protocol used in our study.⁷ Isolated late microbubbles seen in c-TCD may be due to particles initially adhering to the injection needle or the cardiac valve and entering circulation at a later time, as suggested by Droste *et al.*¹⁰

The Valsalva maneuver increases the sensitivity of c-TCD¹¹ and allows for differentiating pressure PFOs from permanent PFOs.⁷ However, a reliable differentiation between a PFO and a pulmonary shunt is not possible because high-intensity transient signals due to contrast medium have also been shown by c-TCD in patients with pulmonary right-to-left shunting with and without Valsalva maneuver,¹² and the transit times associated with pulmonary shunts are comparable to those of cardiac shunts.⁵

It is conceivable but remains to be shown that patients with a pressure PFO have a lower risk of PAE after venous air embolism than do patients with a permanent PFO. Therefore, preoperative risk estimation must identify these two patient groups. This can only be done by performing a Valsalva maneuver.

Assuming that a permanent PFO and a high shunt volume are associated with a higher risk for PAE, the authors recommend the

following broad risk classification based on the results of c-TCD with and without Valsalva maneuver: low risk for the occurrence of PAE; pressure PFO, few microbubbles, longer latency; high risk for the occurrence of PAE: permanent PFO, many microbubbles, short latency. However, a quantitative estimate of the risk profile regarding the development of PAE cannot be made on the basis of the current data.

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Is Chloride or Dilution of Bicarbonate the Cause of Metabolic Acidosis from Fluid Administration?

To the Editor:—I read with great interest the editorial of Dr. Prough¹ about articles by Liskaser *et al.*,² Rehm *et al.*,³ and Waters and Bernstein.⁴ What I found most interesting was that these different authors did not agree on the etiology of fluid-induced metabolic acidosis. I agree with Dr. Prough's summary that dilution of bicarbonate from extracellular volume expansion has at least a partial role in fluid-administered metabolic acidosis.

The study of Waters and Bernstein⁴ seems to refute the editorial of Dr. Prough¹ by concluding that chloride and not volume expansion causes metabolic acidosis. They based their conclusion on their findings that hetastarch (containing 154 mEq/l chloride) caused metabolic acidosis and that equivalent amounts of albumin (containing 93 mEq/l) did not cause metabolic acidosis. However, albumin contains pH buffers, including sodium bicarbonate, which increase bicarbonate concentrations and thus may limit metabolic acidosis from volume expansion.

The study of Liskaser *et al.*² about cardiopulmonary pump prime found that plasmalyte (98 mEq/l chloride) and polygeline-Ringer's solution (151 mEq/l chloride) both caused the equivalent extent of metabolic acidosis when measured immediately after the start of cardiopulmonary bypass.² The most likely reason for the plasmalyte prime's causing a metabolic acidosis is simply a dilution of bicarbonate from a sudden extracellular volume expansion combined with the acetate and gluconate in plasmalyte not having time to be metabolized by the liver to form bicarbonate. The study of Liskaser *et al.*² shows how such solutions as lactated Ringer's or plasmalyte limit metabolic acidosis primarily from lactate, gluconate, and acetate contained in these solutions being metabolized to form additional bicarbonate and not from these solutions containing lower chloride concentrations.

This controversy about the etiology of metabolic acidosis from fluid administration also occurred approximately 3 yr ago in *ANESTHESIOLOGY* with four letters to the editor based on a case report we wrote describing "dilutional acidosis."⁵ In our response to these letters, we

mentioned a study performed over 30 yr ago, which was not mentioned in any of these articles.^{6,7} Asano *et al.*⁷ found that marked metabolic acidosis occurred when 5% dextrose and water (D5W) were administered to dogs. No cations or anions were administered, but metabolic acidosis occurred. Furthermore, dogs to which equivalent amounts of normal saline were administered had the same extent of metabolic acidosis as those dogs that received D5W. From that study, we concluded in our reply letter that simple volume expansion with dilution of bicarbonate was the most likely cause of metabolic acidosis.

I would appreciate any of the authors' comments about the study of Asano *et al.*⁷ and about whether Stewart's analysis of strong ions or actual simple dilution of bicarbonate explains the findings of this study.

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What Is the Clinical Relevance of Dilutional Acidosis?

To the Editor:—Three recent articles and an editorial explain how an infusion of normal saline can cause a “dilutional acidosis.”^{1–4} However, these studies do not address the difference between acidosis caused by iatrogenic infusion of normal saline and those caused by pathologic intracellular processes. These differences may have clinical relevance.

In caring for patients, we are often interested in gaining information about a patient’s metabolic status. Because most of the body’s metabolism is performed intracellularly and there is no practical method to obtain intracellular samples, we obtain a blood sample and presume it reflects what is occurring intracellularly. Typically, a metabolic acidosis reflects an intracellular metabolic derangement, which is eventually reflected in the acidification of the blood. The dilutional acidosis resulting from normal saline infusion does not reflect an intracellular metabolic derangement. Therefore, one would surmise the problems associated with dilutional acidosis are related only to the problems caused by the acidification of the blood *per se*, and perhaps intracellular acidification if there are pH shifts across cell membranes. Therefore, for the same degree of acidemia, a dilutional acidosis might be expected to be less worrisome than a metabolic acidosis caused by an intracellular metabolic derangement for at least two reasons. First, there is no other major primary metabolic derangement with a dilutional acidosis. Second, for the same degree of acidemia, one might expect a lesser intracellular pH decrease in the dilutional acidosis patient compared with a greater intracellular pH decrease in the metabolically deranged patient. In the dilutional acidosis patient, the initial pH change occurs extracellularly, and a lesser or delayed pH change would occur intracellularly. Conversely, in the metabolically deranged patient, the initial pH change occurs intracellularly, and a lesser or delayed change would occur extracellularly.

Situations can occur in which the metabolic acidosis can be caused both by an internal metabolic derangement and by fluid resuscitation with normal saline. Two examples are lactic acidosis from hypoperfusion and diabetic ketoacidosis; fluid resuscitation is required in both situations. If one is using the degree of acidemia to gauge the severity of illness, it may be difficult to determine how much of the acidosis is

due to the probably more benign dilutional acidosis. A given degree of acidosis may imply a variable severity of illness, depending on how much of the acidosis is due to an internal metabolic derangement and how much is due to dilutional acidosis. Clinical criteria that consider blood pH may need to be reevaluated if normal saline infusion occurred.

Like most good studies, more questions are generated than answered. Although the determination of clinical relevance was not the intent of these studies, one of the articles commented, “The acidosis in both groups seems to be without major clinical relevance.”² From the perspective of a clinician, it would be helpful to have the following questions answered:

1. What degree of dilutional acidosis, if any, is harmful?
2. Is there a maximum degree of dilutional acidosis that can result just from normal saline administration?
3. How long should it take for a dilutional acidosis to correct after normal saline infusion is discontinued?
4. When should dilutional acidosis be treated?
5. If treatment is indicated, would sodium bicarbonate, THAM, or both be appropriate for treating dilutional acidosis?

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In Reply:—Dr. Mathes’ letter raises the ultimate question regarding the pathogenesis of fluid administration–associated metabolic acidosis. Unfortunately, this fascinating question cannot be easily resolved with current research technology. The reason is that fluids either come with an abnormally low strong ion difference (SID) in comparison to plasma (plasma SID = 42, saline SID = 0, D5W SID = 0), which inevitably increases the chloride concentration relative to sodium, or they come with so-called buffers, which allow a low chloride fluid to be given, thus leaving the SID essentially unchanged and avoiding acidosis. It is impossible to know whether the acidosis induced by saline or D5W is due to dilution of bicarbonate or a decrease in SID because both happen simultaneously. It is also impossible to know whether the lack of acidosis seen with the administration of fluids, such as plasmalyte or lactated solutions, is due to bicarbonate production or the near normal or normal SID of the solution. It is impossible to prove that the independent variable is either chloride or bicarbonate. We remain uncomfortable with the term “dilutional” acidosis because it implies that what is merely a scientific hypothesis has been proven right. It has not. If anyone can devise the experiment that will convincingly prove which theory is right, we think they should receive the Nobel prize in medicine and chemistry simultaneously!

Dr. Roth raises important clinical issues in relation to fluid administration–associated acidosis. We do not know whether such acidosis is

harmful or beneficial, whether we should treat it, and, if we should, how. In fact, we do not know whether we should ever treat any acidosis.¹ The degree of acidosis that can be achieved with saline administration alone in human beings is unknown because massive saline administration typically occurs in critically ill patients with other disorders of acid–base balance. Animal models suggest that it can lower the pH to 7.15–7.2.² Recovery from such acidosis in humans is variable depending on metabolic state, liver function, and renal function. Data from our study³ show that in cardiac surgery patients, even after 3–4 h, this acidosis has not yet fully resolved. In our opinion, the most important aspect of studies like ours is to highlight that fluid therapy can induce a metabolic acidosis and that it must therefore be considered in its differential diagnosis. We are particularly concerned about the possibility of trauma patients who receive large amounts of saline, are then found to have a worsening metabolic acidosis, and are taken for an exploratory laparotomy to exclude intestinal infarction, when the acidosis is mostly iatrogenic.

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In Reply:—We are grateful to have the opportunity to respond to the thoughtful comments by Drs. Mathes and Roth. Dr. Mathes asks for an interpretation of the results of the study of Asano *et al.*¹ His main question is whether Stewart's analysis² of strong ions or simply the dilution of bicarbonate according to the classical approach by Sigaard-Andersen³ explains the findings of this study. Asano *et al.*¹ infused very large amounts ($3.5 \text{ ml} \cdot \text{kg}^{-1} \cdot \text{min}^{-1}$ for 25 min) of 5% glucose or mannitol in dogs and observed an almost identical decrease in pH by infusing the same amount of 0.9% saline. The question whether volume changes of the extracellular space (and the dilution with bicarbonate-free solutions) or changes in the ionic composition of the extracellular space cause metabolic acid-base disturbances is interesting, but possibly more a philosophical than a physiologic one. Diluting the extracellular space (with probably any solution) should result in the combination of a change in the respective volume *and* a change in the respective ionic composition. Recently, we demonstrated that during transurethral resection of the prostate and absorption of approximately 500 ml irrigation fluid, a moderate metabolic acidosis occurred.⁴ In this case, ion and bicarbonate-free irrigation fluid (consisting of 2% ethanol, 0.54% mannitol, and 2.7% sorbitol in water) was administered. In the study group (the group with a marked irrigant absorption), we observed not only volume changes (decrease in the hemoglobin concentration as an indication of an increase in the extracellular volume), but also changes in the strong ion difference (-3.9 mm) and in the amount of weak plasma acid (-1.5 mm). These changes sufficiently explained the observed acid-base changes by means of Stewart's equations. However, to answer Dr. Mathes' question, it would be necessary to investigate isolated changes in the strong ion difference, the amount of weak plasma acid, or both without changes in the volume of the extracellular space and to investigate changes in the volume without changes in the ionic composition of the extracellular space. However, we believe that such experiments are not possible in any clinical setting. In the study by Asano *et al.*,¹ large volume changes must have occurred (the volume infused in dogs by Asano *et al.*¹ corresponds to approximately 6 l within 25 min in a 70-kg human), and we think that these were accompanied by major changes in the strong ion difference and the amount of weak plasma acid, too. Unfortunately, the question whether these changes could completely explain the observed metabolic acidosis according to the Stewart model cannot be answered because no concentrations of electrolytes or plasma protein were presented.

Regarding Dr. Roth's questions, we do not have a final answer for any of them.

1. We believe that also in dilutional acidosis, a decrease in pH below 7.2 may be harmful because cardiac contractility might be compro-

mised.⁵⁻⁷ This generally implies a therapy of dilutional acidosis when base excess decreases below -10 mm .

2. The maximum degree of dilutional acidosis is not known to us. Obviously, it is not recommended to generate pH values below 7.2, and our experience shows that the pH value can decrease below 7.2 by infusing large amounts of saline. In the investigation by Scheingraber *et al.*,⁸ there was a clearly dose-dependent relation between the volume of saline administered and respective changes in pH.
3. We have not yet investigated systematically the time span in which dilutional acidosis usually will be corrected after discontinuation of saline infusion. Probably, it is a matter of several hours, and possibly, this could take longer in patients with renal dysfunctions.
4. Especially in the postoperative period, we frequently see a mixed acidosis of a metabolic acidosis caused by infusion and of a respiratory acidosis due to alveolar hypoventilation caused by anesthetic agents. Therefore, an intraoperatively generated moderate dilutional metabolic acidosis with a base excess below -7 mm , for example, may quickly result in postoperative pH values below 7.2 when arterial carbon dioxide tension exceeds 50 mmHg. As a consequence, we usually perform intraoperatively an alkali therapy at base excess values below -5 mm .
5. Whether in this case sodium bicarbonate or THAM is preferable is currently being investigated in our laboratory.

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In Reply:—I would like to thank Drs. Roth and Mathes for their interest in my recent article.¹ First, I would like to address the comments of Dr. Roth. He correctly points out the importance of differentiating between intracellular and extracellular mechanisms for the creation of an acidosis, concluding that an extracellular mechanism probably has minimal impact on outcome. Like Dr. Roth, I believe that it is critical to make this distinction. The importance of this differentiation is best shown by the many publications that have evaluated the intracellular-to-extracellular potassium flux after an acid-base change. These articles describe a wide spectrum of mechanisms for creating an acid-base change. As a result, each article reaches a different conclusion regarding the effect of acid-base change on intracellular or extracellular potassium movement.²

I agree with Dr. Roth in his speculation that the impact on outcome of a dilutional acidosis created through the administration of 0.9% saline solution is probably limited; however, there is some evidence that 0.9% saline solution may have some effect on perioperative blood loss. In a study of patients undergoing major blood loss surgery, Martin *et al.*³ found greater blood loss in patients to whom hetastarch in a normal saline solution was administered when compared with patients to whom hetastarch in a buffered electrolyte solution was administered. They suggested that the hyperchloremic acidosis after the normal saline-containing solution was responsible. Gan *et al.*⁴ reported a difference in blood loss between two similar study groups. Like Dr. Roth, I believe that there are more questions to be answered about the effect of 0.9% saline solution administration.

The question raised by Dr. Mathes relating to a study by Asano *et al.*⁵ is more difficult to address. In this study, a metabolic acidosis was found in dogs to which 5% dextrose in water was administered. An explanation for this acidosis using Stewart's analysis requires some understanding of Stewart's acid-base theory. According to Stewart,^{6,7} the acid-base status is determined by the strong ion difference (SID), the albumin concentration, and the partial pressure of carbon dioxide (P_{CO}₂). Respiratory acid-base change results from changes in P_{CO}₂, whereas metabolic problems relate to changes in albumin concentration and the SID. The weak electrolyte, albumin, needs to be changed significantly before it has significant impact on acid-base status. For this reason, in the operating room, most metabolic derangement can be explained by changes in the SID. The SID is the difference in concentrations of Na⁺, K⁺, Cl⁻, and lactic acid. The difference between the cation and anion concentrations can be affected by the amount of free water in which the electrolytes are dissolved. This

relation between the SID and water is the explanation for the findings of Asano *et al.*⁵

Understanding of this concept is best aided by an example. If one were to take a liter of water containing only sodium and chloride, the law of electroneutrality (Σ cations = Σ anions) must be maintained by dissociation of water. For example, if a liter of water contains 140 mEq/l sodium and 110 mEq/l chloride, the SID of that solution is 30 mEq. This positive charge difference would need to be balanced by water dissociation. If we were to add another liter of water without adding any more electrolytes, the solution would contain 70 mEq/l sodium and 55 mEq/l chloride, and the SID would be 15 mEq. Because we have decreased the positive charge contribution of the SID from 30 mEq to 15 mEq, a decrease in [OH⁻] would occur with an increase in [H⁺], and a true dilutional acidosis would be seen. This phenomena is of importance during transurethral resection of the prostate where large volumes of free water can be absorbed.⁸ This is how I would explain the findings of Asano *et al.*⁵ using Stewart's analysis.

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Rhinorrhea by Nasal Fentanyl

To the Editor:—Galinkin *et al.*¹ found that intranasal fentanyl during halothane or sevoflurane for minor ear surgery causes decreased postoperative agitation without an increase in vomiting, hypoxia, or discharge time. The satisfactory score of the patients' parents at home was also excellent.

I read the article with much interest because intranasal fentanyl caused severe nasal discharge in my adult patients several hours after anesthesia. I wonder whether the children in the study of Galinkin *et al.*¹ showed such signs.

In 1988, when nasal sufentanil was reported to be effective in children, I applied the nasal fentanyl technique for adult patients undergoing minor otolaryngologic surgery during halothane anesthesia. The technique was tested in these patients for its efficacy in calming the violent agitating reactions during the inhalation induction period.

Nasal fentanyl, 0.5 ml (0.025 mg), was administered to the patients in each nostril. This quickly showed excellent sedative effect for induction of anesthesia, with much less respiratory depression than

the intravenous dose caused. The emergence from anesthesia also was favorable.

However, we soon realized that the method has an unfavorable side effect. Severe nasal discharge developed in almost half of the patients several hours after the end of the surgery. In those who underwent surgery in the afternoon, rhinorrhea occurred early in the evening and lasted long enough to disturb the patients' sleep during the first postoperative night. For this reason, we discontinued the technique.

Fentanyl does not directly activate mast cells to induce histamine release.² Reduction in the central sympathetic discharge or local catecholamine concentrations instead enhances the effect of fentanyl on narcotic receptors.³ I believe this may have been a cause of the time interval between the application of fentanyl and the development of rhinorrhea, possibly caused by mast cell activation in my patients.

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In Reply:—We thank Dr. Ueda for his interest in our study of nasal fentanyl in children undergoing myringotomy with placement of ventilating tubes.¹ We chose the nasal route of administration because it avoids the need for vascular access in an operation that takes less than 5 min to perform, and this route has been used for administering drugs in children, including opioids and midazolam.²⁻⁴ We chose fentanyl over other opioids, such as sufentanil, because of its lower cost. Dr. Ueda has raised concerns about increased rhinorrhea when this technique of nasal fentanyl was used in adults. There are data to indicate midazolam is more irritating to nasal mucosa of children than sufentanil is.^{2,4} Other investigators have not reported increased nasal secretions after using sufentanil by the nasal transmucosal route in children, but it is unclear whether these investigations were specifically designed to examine this question.

The parents of all 265 patients enrolled in our study received phone calls from experienced nurse practitioners on the day after the procedure, and no parent reported an increase in rhinorrhea during the follow-up period. However, children undergoing bilateral myringotomy and pressure equalization tube placement commonly have nasal congestion from associated allergic rhinitis or during recovery from frequent viral upper respiratory infections. This may skew the parents' perspective, and their concerns about nasal congestion may not reach the threshold for reporting it during the follow-up phone call.

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Costs and Recovery Profiles of Caudal Anesthesia for Anorectal Surgery in Adults

To the Editor:—We read with much interest the article of Li *et al.*¹ These authors designed a study to test the hypothesis that the use of local anesthesia combined with propofol sedation for ambulatory anorectal surgery was superior to both general and spinal anesthesia with respect to recovery times, postoperative side effects, patient satisfaction, and total costs to the healthcare institution. Local anesthesia was performed according to the technique of Nivatvongs^{2,3} and associated with propofol infusion titrated to maintain a stable level of grade 3 sedation in the Observer's Assessment of Alertness-Sedation score. They found that the use of local anesthesia with sedation is the most cost-effective technique for anorectal surgery in the ambulatory setting.

The authors did not include caudal anesthesia in their comparative study. Caudal anesthesia can also be used for sacroperineal surgery in

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In conclusion, we have not noted findings similar to those reported by Dr. Ueda, but we agree that additional follow-up should focus on determining whether fentanyl increases nasal secretions to the point to which it interferes with the child's sleep on the first night after surgery.

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adults. In a recent study,⁴ we found that caudal anesthesia is a reliable and safe anesthetic technique for hemorrhoidectomy in adults, despite the 10% rate of technical failure because of an absent hiatus due to wide anatomic variation in this region.⁵ When low volume (14 ml) of a mixture containing 2% lidocaine with 0.5% bupivacaine and 5 µg/ml epinephrine was used, adequate surgical anesthesia was obtained. All patients were satisfied with the anesthetic care. Time to oral intake was immediate. When the mixture of lidocaine with bupivacaine and epinephrine was used alone, no postoperative side effects occurred. First analgesic requirement after surgery, time to spontaneous standing, and first spontaneous voiding were 276 ± 131, 141 ± 26, and 406 ± 36 min, respectively. Using the same critique as Li *et al.*,¹ we estimated that the marginal costs of drugs and resources in our study was similar. Because propofol was not used, the cost may be lower.

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Opening Anesthesiologist-proof Vials

To the Editor:—Leighton and Mitchell¹ describe methods for opening non-flip-top vials using sharp objects, such as a screwdriver, a ball pen, or a hemostat. We suggest an even better way for opening such vials by pushing hard on the cap center with a small, nonsharp object, such as the tip of a syringe, a hemostat, or even a shelf's sharp edge (fig. 1). This prevents the need for the exact insertion of the sharp object into the gap between the cap and its center.

Although we were unable to obtain dantrolene bottles, we tested this method on 25 outdated non-flip-top vials. All were opened easily within less than 2 s. Use of this method will allow faster opening of vials and will prevent injury in an emergency.

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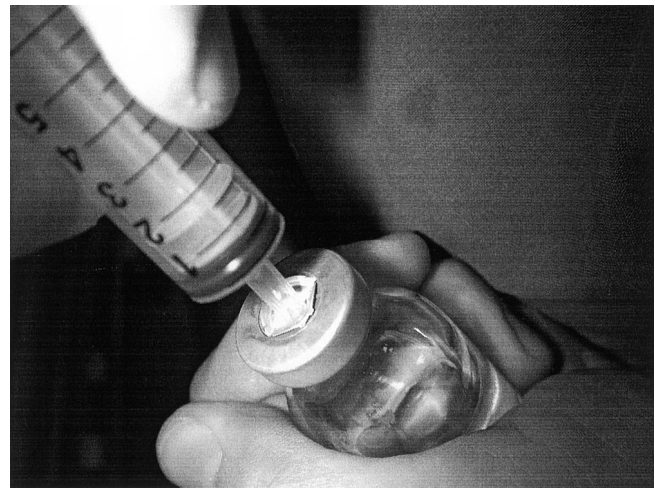


Fig. 1. Opening of a non-flip-top vial by pushing hard on the center of the vial's cap with a syringe.

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Using a Spinal Needle As an Introducer for a Spinal Needle

To the Editor:—An introducer needle is often used as a means of stabilizing subsequent placement of a small-gauge needle during spinal anesthesia. Commercially available kits that include 25-gauge needles (or smaller) usually include an 18- or 20-gauge short (1-1.5 in) needle that is placed into the interspinous space before placement of the spinal needle through it in an attempt to guide the smaller-gauge needle, which is likely to bend during insertion through soft tissue.

Obese patients can have a large amount of soft tissue overlying their interspinous ligaments, and the short introducer needle does not provide the desired level of stability and guidance for the subsequent placement of the very long (5.5-6.0 in) spinal needles that are often required to reach the dural sac.

We have found that in these cases, a standard 3.5-in, 20-gauge spinal needle (Quincke tip) can be used as an introducer for subsequent placement of longer 25- and 26-gauge needles, in effect steering the smaller gauge spinal needles through the supraspinous and interspinous ligaments.

Because risk of post-dural puncture headache increases with increased needle bore, care should be taken to avoid dural puncture with the 3.5-in, 20-gauge spinal needle. We use this technique when we are unable to identify the space with the shorter introducer and spinal needle because of insufficient needle length; therefore, the dural sac should be well past the tip of the 20-gauge needle.

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A Leak in a Capnography Sampling Line Induced a Difference between Arterial and End-tidal CO₂

To the Editor:—Capnography is widely used to monitor respiration during anesthesia. Herein, we report how a pinhole in a sampling line caused a problem with capnography during anesthesia.

A 59-yr-old man was scheduled for sinus surgery. Anesthesia was induced with propofol (50 mg) and vecuronium bromide (6 mg) intravenously, and the trachea was intubated with an endotracheal tube uneventfully. Immediately after induction, the capnography monitor (BP508, sidestream type; Nippon Colin Co., Komaki, Japan) showed an end-tidal carbon dioxide (ETCO₂) value of 35–37 mmHg. After disinfecting around his nose, the patient's face was covered with sterile drapes. ETCO₂ decreased suddenly to approximately 20 mmHg, but the patient's vital signs did not change. We checked the connection of the endotracheal tube, checked for leaks in the endotracheal tube cuff, and checked the respirator circuit and auscultation of the chest but did not find any abnormality. Next, we checked the blood gases, and the arterial carbon dioxide tension (PaCO₂) was 50 mmHg. We reconnected the capnography plug and recalibrated the capnograph, but ETCO₂ was unchanged. Next, we replaced the capnograph with another capnograph without exchanging the sampling line, but ETCO₂ remained low. Then, we exchanged the capnograph including the sampling line. ETCO₂ was then 48 mmHg.

After the operation, we examined the original sampling line. Although it seemed normal, when it was submerged in water, we found a pinhole in the sampling line near the endotracheal tube connector.

In a later experiment, we attempted to reproduce this observation. For this test, we used a test lung (Lung simulator SMS; Datex-Ohmeda, Helsinki, Finland) supplied with carbon dioxide and ventilated with an oxygen-air mixture. A capnograph similar to that used in the operating

room was tested. Respiration was controlled with a respirator at a rate of 6 breaths/min, a tidal volume of 500 ml, and a fraction of inspired oxygen (FIO₂) of 0.4. We made a pinhole in the sampling line with a 23-gauge needle, and ETCO₂ immediately decreased from 35 mmHg to 24 mmHg (fig. 1). We then enlarged the pinhole with an 18-gauge needle, and ETCO₂ decreased from 24 mmHg to 14 mmHg (fig. 1).

Factors that increase the difference between arterial and end-tidal carbon dioxide (P_{(a-ET)CO₂}) include increased pulmonary ventilation/perfusion mismatch and gas sampling problems.¹ The situation described is an example of a gas sampling problem caused by enhancement of room air *via* the unrecognized pinhole.

We did not detect the pinhole before anesthesia, and no pinhole was found when we tested for a leak after the operation, which suggests that a pinhole is difficult to find. Therefore, we recommend changing the sampling line of the capnograph regularly and considering a small leak in the sampling line when seeking the cause of an increase in P_{(a-ET)CO₂}.

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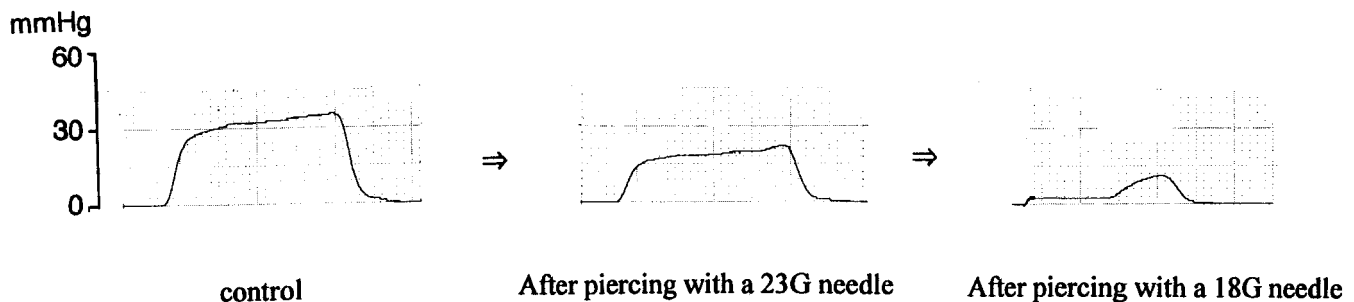


Fig. 1. Typical changes in the end-tidal carbon dioxide (ETCO₂) waveform resulting from a pinhole in the sampling line, made with a 23-gauge needle. A capnography monitor (BP508, sidestream type; Nippon Colin Co., Komaki, Japan) was used, and gases were sampled by the capnometer at a rate of 200 ml/min. A pinhole made with a 23-gauge needle decreased ETCO₂ from 35 mmHg to 24 mmHg; when we enlarged the pinhole with an 18-gauge needle, ETCO₂ decreased from 24 mmHg to 14 mmHg.

The Release of Dissolved Gases from Solution during Decompression after Hyperbaric Treatment: Effervescent Tablets and Henry's Law

To the Editor:—The indications for, contraindications to, and adverse effects of hyperbaric oxygen have been established by the Committee on Hyperbaric Oxygenation of the Undersea and Hyperbaric Medical Society (Kensington, MD) and are revised every 3 yr, with the last edition published in 1999.¹ All hyperbaric chambers and related support systems must be designed, fabricated, installed, and operated in accordance with the requirements of the American National Standards Institute and American Society of Mechanical Engineers Pressure Vessels for Human Occupancy 1. For fire safety reasons, certain electrical devices without formal Food and Drug Administration approval for use in hyperbaric conditions are prohibited in hyperbaric oxygenation chambers.² This includes some patient-controlled analgesia devices. For that reason, patients receiving continuous, intravenous analgesic treatment may need to receive alternative medication during long hyperbaric oxygenation sessions. Oral analgesic compounds, such as acetaminophen, are often sufficient. However, use of analgesics prepared as effervescent tablets is potentially dangerous if administered during the period of compression. The problem relates to the release of dissolved carbon dioxide during decompression. After a tablet is dissolved in water, the liquid is saturated with carbon dioxide; Henry's law indicates that as ambient pressure is reduced, this dissolved gas leaves the solution. We assessed gas production over a 60-min period

beginning at the moment of dissolution of a single 500-mg effervescent analgesic tablet in 200 ml water (Dafalgan[®]; UPSAMEDICA, Brussels, Belgium).

In the 1 atmosphere absolute (ATA) group (outside the chamber), 75 ml gas was released during the initial "bubbling phase" (lasting approximately 2 min), and an extra 15 ml was released over the remainder of the hour. When identical tablets were dissolved at 2.5 ATA, only 15 ml gas was released during the initial bubbling phase. Then, the chamber was decompressed, and an additional 75 ml gas left the solution. It is not difficult to imagine the possible consequences of similar gas release occurring during decompression in patients to whom dissolved effervescent tablets are administered during hyperbaric therapy, particularly if two or more tablets are used.

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