To the Editor:—We read with interest the investigation by Dr. Wan et al. of postoperative impairment of cognitive function in rats. We would like to ask whether the investigators believe there was an adequate control group with which to compare the results of their surgical intervention. In addition to the neuroleptic analgesia group alone, it may have been advantageous to include a sham surgical group that received a similar skin incision to the splenectomized group of rats with postoperative treatment of bupivacaine infiltration. This may have permitted an assessment of the contribution of a surgical incision alone with wound infiltration of 0.25% bupivacaine, because, contrary to the suggestion by the authors, the two groups of rats may not have received identical anesthetic regimens, i.e., there was no assessment of the contribution of the local anesthetic infiltration. Bupivacaine has been shown on its own to suppress systemic cytokine activation when given locally or systemically.  

The investigators suggest the neuroinflammatory changes are related solely to the splenectomy surgical injury and not to other factors, such as the development of postoperative infection, which may or may not have been recognized in these rats. Splenectomized animals would more likely develop postoperative infections, and that by itself may alter neurocytokine levels. In addition, surgical healing and its potential impairment of full ambulation in the rats after splenectomy may have also contributed to their difficulty in completing the learning maze, which may or may not have been related to neurocognitive impairment. A few animals with postoperative infection or impairment in their ambulation may have skewed the results on days 1 and 3 after surgery and may explain the large range of learning abilities seen in the Y-maze testing on those days. It would be interesting to note whether there were individual animal correlates of increased neuroinflammation with increased learning times in the maze.

We appreciate the work presented by Dr. Wan et al. because they raise important issues with regard to postoperative cognitive impairment and the role of inflammatory changes in the central nervous system.

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In Reply:—We thank the Editor for giving us the opportunity to respond to the questions about our work raised by Drs. Joana and Moeen Panni. Their suggestion that we should have included an additional group receiving a surgical incision plus bupivacaine infiltration is of interest. Less extensive surgery (i.e., without incising the peritoneal cavity or dissecting around the splenic pedicle) could have resulted in less neuroinflammatory response and hence a shorter (or no) period of postoperative cognitive dysfunction; this contention is supported by clinical data showing that the incidence of postoperative cognitive dysfunction is lower after minor than after major surgical procedures.

The possibility that removal of the spleen predisposes to infection is an important consideration because this organ can modulate the organism's immune function, albeit much less in adults; interestingly, removal of an injured spleen does not enhance infectious complications in multiorgan-injury patients. Rats, in common with other rodents, are quite resistant to infective processes even after splenectomy complicated by fecal contamination. During our study, we also did not observe clinical evidence of postoperative infection. Last, we do not believe that surgery-induced immobility could have contributed to the splenectomized rats' inability to learn because it was their choice of arm entry and not the speed or distance traveled that was assessed.

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(Accepted for publication July 2, 2007.)
To the Editor.—We were interested in the recent review from Minski et al. on diagnosis and treatment of vascular air embolism. These authors should be commended for a nice review of the existing literature on this specific problem. However, we would like to suggest some minor corrections.

First, regarding the incidence of venous air embolism (VAE) during craniosynostosis repair reported by Faberowski et al. and Tobias et al. in this journal, the two citations have been reversed. Faberowski using precordial Doppler detection found a high (82.6%) incidence of VAE, contrasting with an incidence of only 8% for Tobias et al. This difference has been, in part, explained by the fact that a less invasive surgical method was used in the report of Tobias et al., but the sensitivity of the detection method used in the former study could also be questioned.

Second, and more concerning for us, our method for preventing VAE in sitting neurosurgical children seems to be incompletely reported in the review by Minski et al. In our original article published in the British Journal of Anaesthesia some years ago, we described the combined use of Military Anti-Shock Trousers (MAST) suit and positive end-expiratory pressure (PEEP) with a 0% incidence of VAE compared with a 30% incidence in the control group. We found that this method could be reliable and effective to prevent clinically significant VAE without deleterious side effects. Since that time, we have routinely used this method of prevention in 30–40 procedures performed in sitting children each year.

In our experience, MAST suit inflation and PEEP induce a reliable and sustained increase in right atrial pressure, sufficient to increase jugular bulb venous pressure above atmospheric level in children, and to prevent clinically significant VAE. We do agree with Minski et al. that PEEP alone cannot be recommended as a routine prevention method, because hemodynamic disturbances related to its use balance negatively its potentially beneficial effect on intrathoracic and right atrial pressures, especially in seated anesthetized patients. Preventive low levels of PEEP are so only used to amplify the increase in right atrial pressure, and to restore adequate ventilation in lower lung compartments that could be compressed with MAST suit inflation.

Considering the potential hazards of this method, with ‘venous’ pressure inflation, namely 40 mmHg in the abdominal compartment and 30 mmHg in the lower limbs compartment, we never observed the described potential risks of hyperperfusion to intraabdominal organs, and compartment syndromes. In our experience, urine output, which could be very sensitive to hyperperfusion of the kidneys during abdominal compression, never decrease under 1 ml · kg⁻¹ · h⁻¹. Moreover, we demonstrated that plasma creatine phosphate kinase level, which could reliably reflect muscular hyperperfusion and ischemia during MAST inflation, was not significantly increased in children with prolonged MAST inflation.

When comparing the incidence of VAE occurrence during procedures performed in the prone or sitting position with PEEP and MAST suit inflation in children, we did not find a significant difference, with only one episode of VAE related to a major surgical vascular effraction during dural closure occurring in a sitting patient. This could be an additional argument to maintain the use of the sitting position in selected patients, provided that detection and prevention of VAE could be as efficiently secured in both situations. We therefore recommend consideration of the use of a MAST suit and moderate PEEP levels in children when surgical conditions require positioning of the patient in the sitting position.


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(Accepted for publication July 3, 2007.)

Gas Embolisms Revisited

To the Editor.—We read with great interest the comprehensive work on vascular embolism by Dr. Mirski et al. but would like to elaborate on some clinically important points. In some situations, this must include an active therapeutic role for the physician.

The ability of a patient to tolerate gas embolism is critically dependent not only on the volume and rate of accumulation, but also on the type of gas injected. Carbon dioxide, for example, is 25 times more soluble in blood than nitrogen, the primary constituent of air. Carbon dioxide can be carried in the blood not just in the dissolved form, but also takes advantage of bicarbonate buffering and combination with hemoglobin and plasma proteins. Increased solubility and rapid elimination explain why the lethal dose of carbon dioxide is approximately 5 times greater than that of air. For these reasons, carbon dioxide is often chosen over air for laparoscopy, and because of carbon dioxide’s relative safety, it is used as an intravascular contrast agent for angio-
EXTERNAL PRESSURIZING SALVAGED BLOOD REINFUSION BAGS: PREDICTABLE, PREVENTABLE CAUSE OF FATAL AIR EMBOLISM

To the Editor—Eight years ago I wrote a letter to the editor\(^1\) in response to a Medical Intelligence Article published in Anesthesiology on air embolism (AE).\(^2\) My letter said, “one totally preventable and recurring cause of potentially fatal venous air embolism that was not mentioned in the article . . . is externally pressurizing a reinfusion blood bag that has been filled with blood from a surgical field scavenging-blood processing system.” I was dismayed and astounded that the recent review article published in Anesthesiology on AE\(^3\) also did not mention the predictable, preventable, often fatal AE that occurs from externally pressurizing reinfusion bags filled with surgical field-scavenged/processed/salvaged blood. In the majority of multiple models made by multiple manufacturers (e.g., Cobe Cardiovascular Inc. [Arvada, CO] and Hemonetics [Braintree, MA]), the processing unit (centrifuge bowl wherein the scavenged cells are washed) must send a 70- to 80-ml column of air into the reinfusion bag before any blood from the processing unit can enter the reinfusion bag. In addition, there are a number of processing blood circumstances that will cause the reinfusion bag to contain multiples of 70–80 ml of air. If the dead space volume of the infusion line to the patient is less than 70–80 ml, venous AE due to external pressurization of a scavenged/processed/salvaged blood reinfusion bag is a 100% physical certainty. Because the AE bolus is introduced into a sizable vein over 1–2 s, there is a significant probability that significant compromise of the circulation will ensue.

Indeed, Linden reported five cases of fatal AE due to administration of recovered blood under pressure in New York State alone from January 1990 to June 1995,\(^4\) and the American Society of Anesthesiologists Closed Claims database contains “several” fatalities “from air in the blood from the cell saver.” In my own medical-legal experience, I have reviewed two fatalities in the past 3 yr and six fatalities in the past 10–12 yr due to pressurizing recovered blood. All of the cases I reviewed had the same horrific story: Multiple bowls of salvaged blood were processed without purging the air from the reinfusion bag, the reinfusion bag was externally pressurized, there was a sudden cardiac arrest within 1 min of the reinfusion bag being emptied of blood, there was a failed resuscitation (including aspiration of air from central veins), and then the autopsy showed a heart filled with air. The obvious, but lifesaving, take-home message of these considerations is that reinfusion blood bags that have been filled with blood that has been scavenged and processed by an autotransfusion system should never be externally pressurized.

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Copyright © 2007, the American Society of Anesthesiologists, Inc. Lippincott Williams & Wilkins, Inc.
To the Editor.—The continuing medical education-accredited article “Diagnosis and Treatment of Vascular Air Embolism” by Dr. Mirski et al.1 in the January 2007 issue of this journal merits some comments. The term vascular air embolism was new to me, and the PubMed search with the search term vascular air embolism yielded 7 results. The search term venous air embolism, however, with which I am more familiar, yielded 432 hits. In Dr. Mirski’s article, there are 150 references, but in only one of the cited articles, which is a radiologic article, is the term vascular air embolism used.2 The Merriam-Webster Online Dictionary defines embolism as “sudden obstruction of a blood vessel.”3 Therefore, by definition, there is no embolism other than vascular. The abbreviation VAE that Dr. Mirski uses for vascular air embolism in the literature is generally attributed to venous air embolism.

This is no semantic hair splitting. It has rather a fundamental clinical impact as a consequence of various pathophysiologic mechanisms and distinct clinical sequelae. The term venous air embolism (VAE) describes the entrainment of air into the venous blood system leading to right ventricular constraint, elevation of pulmonary pressure and an increase in pulmonary vascular resistance, and it eventually ends in right heart failure and death. The sudden increase of right ventricular pressure might cause a reopening of the foramen ovale with consecutive transition of air bubbles from the right to left ventricle, leading to an obstruction of cerebral vessels, coronary arteries, the ophthalmic artery, or other branches of the arterial system, resulting in cerebral infarction, myocardial ischemia, blindness, or other symptoms of hypoperfusion or nonperfusion. In contrast to VAE, the entrainment of air into the arterial system, either directly or secondary through a persistent or a reopened foramen ovale, is termed arterial or paradoxical air embolism (PAE).

These considerations and the fact that VAE occurs in up to 100% all of operations in the sitting position4,5 lead to question which preoperative tests are necessary to identify patients with an increased risk to experience VAE and PAE intraoperatively. The persistent permanent foramen ovale, which is found in an unselected population with a prevalence of approximately 25%,6 is such as a major risk factor for PAE. Transesophageal echocardiography (TEE) and transcranial Doppler ultrasonography are equally effective in the detection of a permanent foramen ovale before surgery in the sitting position.6 There is broad consensus that patients with a permanent foramen ovale should be excluded from surgery in the sitting position; at least the advantages of the sitting position should be carefully weighed against the risk of the potentially hazardous consequences of massive VAE and PAE in the sitting position.

With respect to intraoperative monitoring, TEE is considered to be the accepted standard in detecting VAE, with an indisputable high sensitivity and specificity. Moreover, it is the only monitor to intraoperatively detect PAE, persistent foramen ovale, and pulmonary passage of air bubbles during massive VAE.7 There is no intraoperative monitoring device other than TEE to reliably and quickly clarify the cause of sudden hypotension and acute hemodynamic instability, which can be due to VAE, hypovolemia, and left or right ventricular failure of any other cause.

Transesophageal echocardiography requires some expertise and, for various reasons, it is not readily available everywhere. Nobody, however, would seriously have doubted the usefulness of intraoperative pulse oximetry, even in the early years after its introduction in clinical practice when it was not on hand in every operating room on the planet. Of course, anesthesiologists strongly advocate the use of pulse oximetry because it is a highly useful and an even life-saving monitoring device even if the incidence of intraoperative hypoxia is definitely not as high as VAE during operations in the sitting position. At least for procedures where one has to face potential life-threatening complications such as VAE and PAE with an incidence of up to 100%, the use of a monitor, such as TEE, with a nearly 100% specificity, sensitivity, and accuracy, has to be strongly recommended.

Recently, a task force of expert neuroanesthesiologists has been entrusted by the Scientific Neuroanesthesia Subcommittee of the German Society of Anesthesiology and Intensive Care Medicine to update the recommendations regarding neurosurgical operations in the sitting and semisitting positions. Preoperative assessment and intraoperative monitoring in patients scheduled to undergo operations in the sitting or semisitting position is of paramount importance. For the task force, the usefulness of TEE in this setting is beyond any doubt, even if there are no prospective, controlled studies to answer the questions whether and how far the use of TEE has a positive impact on clinical outcome parameters such as reduction in complications and mortality.

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References


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Vascular Air Embolism: Lung Injury and Its Pathophysiology Also Need to Be Considered

To the Editor.—In their review of vascular air embolism, Mirski et al. define the condition as the entrainment of gas into the venous or arterial vasculature, but virtually the entire review is about venous air embolism (VAE). And arterial air embolism is described solely as secondary to VAE. Although blast injury and thoracentry are included in a comprehensive list consisting mainly of VAE causes, no explanation of etiology is given for these two conditions, and readers may be led to believe that their pathophysiology is the same as that of VAE. Two case reports that actually describe arterial air embolism are also cited among the many VAE causes, and the explanation is simply excessive ventilatory pressure.

Arterial air embolism is a far more dangerous condition, requiring little air to inflict serious damage, even fatal injury, and deserves a better explanation. One of the most important causes of arterial air embolism is lung trauma (occurring in 4% of major thoracic trauma), as the following case illustrates. A 20-year-old jockey fell off a horse in the middle of a race. The jockey was trampled by several horses and sustained severe injuries, including those involving the chest. By the time the patient arrived at the hospital, there were few signs of life. The trachea had been intubated, and assisted ventilation had already begun. A radial artery line was started during cardiopulmonary resuscitation. Copious amount of gas bubbles were aspirated from the arterial line. The jockey died from the injuries.

If the pulmonary parenchyma has been damaged, direct communication between the pulmonary vasculature and bronchial tree becomes possible. Whereas this injury may manifest in a spontaneously breathing patient as hemoptysis (pulmonary vascular pressures > airway pressure), arterial air embolism may occur when positive-pressure ventilation reverses this pressure gradient and facilitates air entry into the pulmonary veins, from whence air bubbles travel in particular to the brain and the coronary arteries, with devastating results. Through this mechanism, arterial air embolism has been reported following external chest compression, lung biopsy, rapid decompression or rapid ascent from deep sea diving with a closed glottis, and rupture of an intrapulmonary bronchogenic cyst. Single contralateral lung ventilation and immediate thoracotomy for hilar clamping (if the injury is unilateral), avoidance of high airway pressure, and maintenance of adequate pulmonary venous pressure, where appropriate, are remedies that have been suggested. Without understanding the underlying pathophysiology, however, one might wrongly assume that venous air has traveled to the arterial side via a cardiac defect or an overwhelmed lung vasculature, and the above measures would make no sense (and hence would not be considered).

Some important causes of VAE are also missing from the review. In coronary artery bypass grafting, the saphenous vein grafts are sometimes harvested endoscopically using carbon dioxide insufflation. The purported advantages include reduced hematoma, pain, wound complications, hospital stay, and vein trauma. Using transesophageal echocardiography, carbon dioxide VAE may be detected in more than 17% of patients, some of them severe and necessitating emergency cardiopulmonary bypass. Cardiovascular collapse has also been described. Decreasing the carbon dioxide pressure reduces the problem, and continuous transesophageal echocardiography monitoring of the inferior vena cava has been recommended.

Venous air embolism has also been reported during hepatic resection using the Cavitron Ultrasonic Surgical Aspirator. Minimization of the duration of using the Cavitron Ultrasonic Surgical Aspirator device (Valleylab Inc., Boulder, CO) has been suggested. Even in a comprehensive review, lists often cannot be totally exhaustive. Nevertheless, we thought the above causes have sufficient relevance that they should have been included.


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(Accepted for publication July 3, 2007.)

In Reply.—We very much appreciate the comments from our esteemed anesthesia colleagues. We must immediately apologize to Drs. Faberowski and Tobias in our error in referencing their published data in table 1 of our review. Indeed, as Drs. Meyer et al. point out, the percent of vascular air embolism (VAE) detected in their studies was reversed; an 82.6% incidence of VAE was recorded in the study by Faberowski et al., whereas an incidence of 8% (4 of 50 patients) was

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

Correspondence: Anesthesiology, V 107, No 5, Nov 2007

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To the Editor:—In a recently published article, Jullia et al.1 describe risk factors for the occurrence of electroencephalographic abnormalities with sevoflurane anesthesia. Although it is appropriate to state that “induction with high sevoflurane concentrations may trigger epileptiform electroencephalographic activity” and “only electroencephalographic monitoring allowed the diagnosis,” the final sentence of the article seems to extrapolate electroencephalographic findings to clinical safety. Did the patients with epileptiform activity demonstrate a postictal state? Was recovery prolonged? The decision to perform an inhalation induction with sevoflurane, as with all clinical decisions, should indeed undergo “careful consideration,” but the risk–benefit analysis should be based on evidence-based trials of clinical outcomes.

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No studies to date have demonstrated that intraoperative epileptiform activity during induction of anesthesia translates into adverse clinical outcomes, postoperative seizure activity, or other perioperative morbidity, and findings of this well-done study should not be extrapolated to inappropriate clinical conclusions.

**Roy G. Soto, M.D., Stony Brook University, Stony Brook, New York. roysoto@yahoo.com or roy.soto@stonybrook.edu**

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**Reference**


(Accepted for publication July 11, 2007.)

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**In Reply.—** We thank Dr. Soto for raising this important point and appreciate the opportunity to reply.

Sevoflurane can be proconvulsive at high alveolar concentrations.1,2 Our study demonstrated that rapid induction may result in the occurrence of epileptiform discharge. The important question is whether these epileptiform discharges result in deleterious brain effects.

The postoperative clinical outcome of the patients included in our study was outside the scope of our article. However, we did not observe any especially prolonged recovery or postoperative clinical seizure, although we did not do any systematic postoperative assessment. The question of the deleterious clinical effects of nonconvulsive seizure remains a matter of debate.3 For this reason, although we cannot demonstrate that nonconvulsive epileptiform discharges are damaging for the brain, we cannot demonstrate the opposite, either.

We agree that it would be interesting to study perioperatively morbidity associated with sevoflurane-related epileptiform action. However, for evident ethical reasons, we cannot schedule a new study where we purposefully use epileptogenic sevoflurane concentrations to answer these questions.

However, sevoflurane remains a safe anesthetic agent because of its pharmacologic and clinical properties. Our purpose is not to contraindicate inhalation induction with sevoflurane, especially when the risk–benefit ratio is in favor of this technique. To decrease the risk of the occurrence of epileptiform discharges, incremental induction might be used.4

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**To the Editor.—** We were interested in the recent study by Watt et al.1 to reduce epistaxis during nasotracheal intubation in children by telescoping the tip of the endotracheal tube into the funnel end of the red rubber urethral catheter. However, we have a few points to make.

The results of using a prewarmed tube for nasotracheal intubation are variable.1-3 Although studies in adults suggest reduced bleeding, its efficacy in pediatric patients seems to be poor.4-6 In the control group in the study by Watt et al.,1 where tracheal intubation was achieved by using a tube at room temperature, the incidence of bleeding was 56%, significantly higher than in the control group of Elwood et al.,4 wherein a thermosoftened tube was used and the incidence of bleeding was only 29%. Further, although Watt et al.1 report that a 39% incidence of epistaxis in patients in whom the nasotracheal tube was carried with a thermosoftened uncuffed tube is consistent with that reported by Elwood et al.4 in their thermosoftening group (29%), it is still higher in the study of Watt et al. Considering these facts, in the study by Watt et al., the higher incidence of bleeding in patients in whom a prewarmed tube was used can be attributed to the use of a larger tube (selected by Cole’s formula, i.e., ID (mm) = (age/4) + 3.5) rather than lack of efficacy of thermosoftening and topical vasoconstriction, especially when no direct comparison of epistaxis, with and without a nasal topical vasoconstrictor, was made by the authors. For nasotracheal intubation, it is strongly recommended to use an endotracheal tube with an OD 0.5-1.0 mm less than that used for an oral tube, to allow for smooth andatraumatic passage of the nasal tube.5

This is evident in various adult and pediatric studies of nasotracheal intubation. Elwood et al. selected the uncuffed tube for nasotracheal intubation by the formula of Motoyama6 (i.e., ID (mm) = (age/4) + 3.5) and, as aforementioned, had better results in the thermosoftening group than achieved by Watt et al., who used endotracheal tubes at room temperature and after thermosoftening.4,7,8 Although we wholeheartedly agree with Watt et al.’s report of better results than Elwood et al. in regard to clinically relevant bleeding (5% vs. 9.4%) in patients in whom a red rubber catheter was used, we believe that selection of the tube by the formula of Motoyama or Khine (i.e., ID (mm) = (age/4) + 3.0) by Watt et al. could have further reduced the incidence of clinically significant nasal bleeding in all three groups.5,6

Regarding the intubation attempts, Watt et al. did not mention the number of times each naris was entered or the navigability (smooth or impinged) of the endotracheal tube. No data were provided regarding postoperative nasal complications such as nasal pain, persistent discharge or bleeding, difficult breathing, or crusting, which are important secondary outcomes in patients undergoing nasotracheal intubation and are likely to be significantly affected by the size of tube used, thermosoftening, and whether topical nasal vasoconstrictor drops were used.9-11 Including these sequelae in their trial could have better delineated the role of the vasoconstrictor in delayed outcomes of pediatric nasotracheal intubation.

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**Nasotracheal Intubation in Children**

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More about Telescoping for Nasotracheal Intubation in Children

To the Editor—I read with interest the study by Watt et al.1 of telescoping tracheal tubes into catheters to minimize epistaxis during nasotracheal intubation in children. The authors selected uncuffed tracheal tubes (nasal RAE; Mallinckrodt, St. Louis, MO) for their 2- to 10-year-old study patients using the formula (age of the patient in years divided by 4) + 4. Khine et al.2 demonstrated that 25% of their patients, full term to 8 yr old, needed reintubation to obtain an appropriate fit using the same modified Cole formula for uncuffed Mallinckrodt tubes. I have had air leaks often with the uncuffed Mallinckrodt tracheal tubes selected by the same formula; sometimes the proper size tube was two sizes larger. The authors chose not to investigate this relevant sequel. As a result, readers are left pondering how the authors managed the rest of the anesthetics. In a previous study about telescoping by Elwood et al.,3 the nasal RAE uncuffed tubes, selected using the formula (age/4) + 3.5, were even smaller. In 1 of 103 study patients, the leak around the tube was large, necessitating a change of tube. Could there be a regional difference of larynx size in the United States?

The OD of the funnel end of a 10- or a 12-French Davol® urethral catheter (Davol Inc., Cranston, RI) is approximately 9–10 mm. The end is made to accept a connector from a urine-collecting bag. When an ID 3.0 cuffed endotracheal tube is telescoped into the funnel end, the combination is bulky. It is difficult to visualize how this combination, without any modification, can pass the naris or choana easily.

In Reply—We thank the authors of both letters for their thought-provoking comments. In the first letter, Dr. Mahajan et al. refer to the incidence of nasopharyngeal bleeding after native tubes in our study1 (56%) and the incidence after warmed tubes and a vasoconstrictor in the study by Elwood et al.2 (29%). Although these were both “control” groups, the two treatments have no common basis for comparison. Considering the incidence of bleeding associated with warmed tubes in the two studies,1,2 Dr. Mahajan et al. posit that the 0.5-mm-larger tube diameter in our study contributed to the greater incidence of bleeding. This is a possibility that we did not test. Whether the larger tube diameters in our study contributed to the incidence of bleeding with native and warm tubes is a moot point because the incidence of bleeding with a larger diameter tube telescoped into a soft rubber catheter seems to be trivial (5%).1 Further direct comparisons between the results of the two studies should be limited because we did not test the effect of oxymetazoline in our study. Although the larger-diameter uncuffed tubes may have contributed to the incidence of bleeding in our study, this was a compromise rooted in our other concern to minimize the magnitude of the endotracheal tube leak. The authors correctly point out that we did not report complications from nasotracheal intubation that occurred in the postanesthesia care unit and thereafter. We can state categorically, though, that whereas surrogate markers for complications may have occurred, neither ear, nose, and throat consultation nor admission to or delayed discharge from the hospital occurred as a result of nasopharyngeal bleeding.

Dr. Wu also questions the sizes of the tubes that we used and the magnitude of the endotracheal tube leak. We did not change any of the tubes because the tracheal leak was too large. This is not to suggest that such a situation cannot occur. However, during dental surgery, we minimize the clinical impact of a small endotracheal tube leak by maintaining spontaneous ventilation and by packing the throat with gauze. There is no evidence that the size of the larynx in children in the United States varies geographically. Dr. Wu describes a modification to the proximal end of a cuffed straight nasal tube to facilitate its use in dental surgery, which is analogous to using the old-fashioned metal...
Anesthesiology 2007; 107:857

Electroconvulsive Therapy: Compassion in Care and Risk–Benefits of Discomfort

To the Editor—The recent case report by Litt and Li raises several unmentioned issues. The anesthetic for electroconvulsive therapy (ECT) typically involves the administration of the lowest effective dose of induction agent (affording amnesia and minimizing drug-induced seizure inhibition) and liberal administration of muscle relaxant to prevent injury, especially in patients with severe osteoporosis (demographically, elderly women predominate). It is not uncommon for the relaxant to outlast the anesthetic agent (in which case, liberal use of postseizure amnesic agents is recommended to prevent awake paralysis). ECT is frankly a situation inviting awareness and requiring close clinical observation and communication (via isolated limb responses to ensure amnesia in patients able to communicate [many cannot because of psychiatric illness]). Repeated ECT raises seizure thresholds, reducing duration and effectiveness, often requiring changes from methohexital to induction agents less inhibitory, including etomidate to ketamine. Meticulous vigilance to ensure effective amnesia is important, because anesthetic dosage varies widely and changes in injection technique and cardiac output can influence induction characteristics of minimally effective, empiric doses.

Awareness is to be regarded as a frequent risk of ECT, mitigated only by careful, continual observation. Specifically regarding the case report, I found it unusual that these authors chose to allow the patient to remain conscious after determining awake paralysis and before ECT was delivered, because the judicious injection of additional induction agent rapidly terminates this unpleasant situation. They instead allowed conscious paralysis to persist for several minutes until neuromuscular recovery was complete, determined then that the patient ‘described being awake and paralyzed and not liking it,’ only to start again shortly thereafter. I would suggest, contrary to the title provided, that this indicates recall was present, but retrograde amnesia occurred with the successful subsequent ECT treatment, something quite unpredictable.

In Reply—We thank Dr. Kempen for his letter to the editor, which again draws attention to the importance of careful planning when providing anesthesia care for electroconvulsive therapy (ECT). We agree that our case report raised several unmentioned issues. The first key point in Dr. Kempen’s letter is that as soon as we appreciated that the patient was awake and aware, an additional dose of methohexital could have been given to restore unconsciousness. In general, additional doses are appropriate for restoring unconsciousness and are well within standards of care. For example, a typical clinical recommendation for adult methohexital administration is 1–1.5 mg/kg intravenous for induction, and 20–40 mg intravenous every 4–7 min for maintenance. However, in our case, the patient was completely awake, responsive to complex yes/no questions with toe response, and therefore not treatable by a small additional methohexital dose. A concern there was that additional methohexital doses might lead to a reduction in seizure duration, possibly to a point where the duration was inadequate.

The relation between anesthetic agent and effect on seizure duration is an important issue. In our case, the patient, who may have had a high anesthetic threshold, would have required a second induction dose, not a small additional maintenance dose. Etomidate is a commonly used anesthetic for ECT and has been shown to provide longer seizure durations. The same study also found a dose-dependent reduction of seizure duration by methohexital in the dose range 0.75–1.50 mg/kg. Indeed, a 2003 randomized, double-blind, crossover study of middle-aged patients found that using remifentanil with a reduced methohexital dose of 0.625 mg/kg markedly improved the marginally acceptable seizure duration that results from a methohexital dose of 1.25 mg/kg.

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To the Editor:—Pain is one of the major stressors experienced by the patients hospitalized in an intensive care unit (ICU). The DOLOREA study has shown that 55% of the ventilated patients experienced pain at rest and 56% experienced pain during a procedure. Recently, we have reported that a systematic evaluation of pain and agitation at rest in ventilated and nonventilated ICU patients was associated with a better outcome. Although literature evaluating procedural pain is consistent, surprisingly, one cannot depend on retrograde amnesia being covered in the informed consent discussion with anesthesiologists. Perhaps it would be a good thing to add to the ECT consent, although it certainly is covered in the informed consent discussion with anesthesiologists. Dr. Kempen’s letter highlights several important aspects of providing ECT anesthesia care, and we are deeply grateful for his observations and comments.

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The back and limbs are the main areas of pain in medical patients. This could be explained in part by the fact that medical patients had a greater rate of sepsis upon admission than the surgical patients. Myalgia and arthralgia are common clinical features associated with fever, determined in part by inflammation and the muscle hypercatabolism induced by the thermogenesis. \(^{10}\) Inflammatory cytokines and sympathetic amines have been implicated in the hypernociceptive state associated with inflammation. \(^{11}\) Back and limb pain may be also related to the obligatory immobilization of patients in the ICU bed, often requiring the use of sedatives or physical restraint. \(^{12}\) Considering that turning of the patient is the most painful procedure in the ICU \(^{12,4}\) and that the pain before the procedure is associated with pain during the procedure, \(^{5,13}\) efforts to better manage pain at rest should be encouraged. Moreover, decreasing pain at rest and its stress response may be associated with a better outcome in critically ill patients. \(^{5}\) If so, pain at rest should be considered as a major clinical diagnostic symptom.

In conclusion, the incidence and intensity of pain in ICU medical patients are not lower than in surgical-trauma patients in this cohort of ventilated and nonventilated patients.

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Quantitative data are expressed in median [25th–75th percentiles]. BPS = behavioral pain scale (0–12 points); ICU = intensive care unit; NRS = numerical rating scale (0–10 points).
To the Editor—This letter is in response to the “wake-up call” issued by Schwinn and Balser1 and the 2006 Ravenstine lecture by Reves2 regarding research training in academic anesthesiology. Our goal with this letter is to report on research staff hours per month before and after the implementation of the Health Insurance Portability and Accountability Act (HIPAA), which was enacted in April 2003. Training future clinical researchers, we believe, will have significant budgetary implications in the settings of (1) HIPAA and (2) institutional review board interpretations of HIPAA and other escalating regulatory burdens in clinical research contexts.

In our 12-week prospective study3 from May 2001 to October 2004, patients were followed up daily for 4 days and then at 1, 3, 7, and 12 weeks after surgery. Our study recruitment target was 8 participants per month, to meet sample size requirements (n = 270) in 36 months. We queried our data to determine (1) months in which our recruiting target was met and (2) months with evidence of hard-copy communication log updates (or “work logs,” which are a conventional method to track tasks in clinical research). Both (1) and (2) represent important indicators of research team function. We then determined research staff hours (individually and in aggregate), as well as months in which staff overtime was incurred. Finally, we described each study month with the following dichotomous designations: “recruitment target met,” “work log(s) updated,” and “HIPAA implemented.” These three independent, dichotomous variables were inserted into a linear regression equation to determine the continuous dependent variable “number of hours per month of research staff time.”

Our research staff consisted of a full-time monitor–coordinator (June 2001 to November 2003), a part-time recruiter–coordinator at the surgeons’ clinic (author K.A.F., June 2001 to February 2003), and a full-time recruiter–coordinator (author M.T.B., December 2002 to October 2004). There was significant task sharing among research team members.

Nineteen of 40 months met the study recruitment target (table 1). Enrollment continued for 4 months beyond the originally planned finish date of June 2004. Four of the 6 “below-target” months after HIPAA was implemented were May through August 2003, which we attributed to seasonality (as opposed to identifying which elements of HIPAA may not be uniformly applicable to other clinical research settings. In other words, recruiting months were associated with the continuous dependent variable of monthly staffing hours. The first was evidence of updates of work logs, and second was whether HIPAA had been implemented. In the final linear regression model, the “total hours constant” for general recruitment, follow-up, and case form reporting was 106 h per month (95% confidence interval, 162–228) per month (n = 41 months), averaging 146 h per month per full-time equivalent staff member. Four epochs are described in table 1, in which each epoch indicated an appreciable change in the composition of the research team. Over the four epochs, staff hours per full-time equivalent-month increased. There was no relation between meeting the monthly recruiting target and either the full-time monitor–coordinator or the full-time recruiter–coordinator surpassing the “overtime threshold” of hours worked (data not shown).

Of the three independent, dichotomous variables analyzed in the multivariable linear regression equation (recruitment target met, work log(s) updated, and HIPAA implemented), only two of these factors were associated with the continuous dependent variable of monthly staffing hours. The first was evidence of updates of work logs, and second was whether HIPAA had been implemented. In the final linear regression model, the “total hours constant” for general recruitment, follow-up, and case form reporting was 106 h per month (95% confidence interval, 42–169), with work log updates associated with 75 additional hours per month (95% confidence interval, 16–134), and HIPAA implementation associated with 77 additional hours per month (95% confidence interval, 32–121).

Work log updates and HIPAA implementation were each associated with a 70% increase beyond monthly base work hours (of recruiting and follow-up). Meeting recruiting targets (as a dichotomous variable) did not seem to influence work hours per month, nor did numbers of newly recruited study participants. Junior clinical investigators in anesthesiology and their research mentors and department chairs may find these calculations useful for forecasting study budgets and hiring the correct mix of research staff to specifically address patient recruitment and follow-up, work log documentation, and HIPAA and associated regulatory compliance.

This analysis was limited in at least two respects. First, these results may not be uniformly applicable to other clinical research settings. In addition, we did not specifically investigate in detail what aspect of HIPAA actually increased staff hours, primarily because research staff members were simply asked to comply as best as possible with our institution’s specific interpretation of HIPAA and mandates generated therefrom (as opposed to identifying which elements of HIPAA may...
have increased workload). As a retrospective review, our regression equation estimates are likely biased downward.

Whether HIPAA is a help or a hindrance to clinical research does not warrant specific discussion here, but there have been reports describing the real costs of HIPAA to clinical research enterprises, both at the level of the university (faculty and staff training requirements, paperwork and administrative burden, and so forth) and at the level of researchers choosing or not choosing to submit new or revised protocols.

In our experience, we had staff budgeting challenges after HIPAA was implemented, in addition to existing complexities of clinical research in anesthesiology related to initial patient encounters for research purposes occurring on the day of surgery. In our study, we did not believe that one person was sufficient to accomplish preparative recruiting tasks (on-site in several surgeons’ outpatient clinics, to satisfy our institution’s response to HIPAA), plus day-of-surgery study coordination, plus work log maintenance (such as the manual of operating procedures, communications with the institutional review board, funding sources, and vendors). When we were able to accomplish all of these tasks, one of our coordinators primarily managed surgical office patient recruitment and day-of-surgery study coordination proceedings; while the other coordinator managed postoperative patient follow-up and most work log updates. When we were unable to replace one of the coordinators because of the budgetary drain associated with HIPAA implementation, the work logs were no longer reliably updated. In either case, we consider ourselves fortunate to have been able to complete the described study, given that no additional resources were allocated to compensate for the anticipated workload increases associated with HIPAA implementation.

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