To the Editor:—An article published by Holdcroft et al.¹ in the May 2006 issue of ANESTHESIOLOGY reported the analgesic and adverse effects of an oral cannabis extract for postoperative pain management. To date, only three other manuscripts investigating the role of cannabinoids in postoperative pain have been published.²⁻⁴ The conclusions from these studies are that cannabinoids are not ideally suited to manage postoperative pain, being either moderately effective,¹,² not different from placebo,³ or even antianalgesic at high doses.⁴ However, a definitive conclusion of the role of cannabinoids in the postoperative setting cannot yet be made because only 202 patients were studied using different drugs, dosages, routes of administration, and protocols.

In their study, Holdcroft et al.¹ used an escalating-dose technique, which leads to two main problems: the lack of blinding and the absence of a placebo group. Furthermore, Holdcroft et al. stated, “The study recruited all types of surgical patients” and “Apart from the different distribution of surgical types, the three dose groups were similar at baseline.” This obviously introduces a major problem in the interpretation of their results.⁴ Another potential problem with the study by Holdcroft et al. is that the 65 patients enrolled in their study were recruited from eight different centers, which does not help to obtain consistent data.

The actual design of the study could also be criticized because patients were only studied for a 6-h period (periods longer than 6 h are advocated)³ and, more importantly, because the study drug was administered only when clinical evidence showed that patient-controlled analgesia morphine was not necessary anymore. Therefore, the first hours (or days?) immediately following the operation were not studied. The authors do not report the time when patients were in fact recruited and when they were given the cannabis extracts. This information is crucial to understanding when the study took place. Furthermore, in real life, using the so-called multimodal analgesia approach, patients should receive adjuvant analgesics (acetaminophen, nonsteroidal antiinflammatory drugs) at the beginning of the postoperative period and not after morphine administration has been stopped. Finally, pain on movement was measured, but no details were given on how these assessments were made considering the many types of surgery performed.

A last comment is on the choice of Cannador (IKF, Berlin, Germany) as the cannabinoid of choice for this study. Although it contains tetrahydrocannabinol, its association with cannabidiol and other cannabis extracts (which ones and in what proportions?) is certainly another variable that potentially complicates the interpretation of the results.

Despite these limitations, the authors must be congratulated because this research area is not easy: there are difficulties in funding such trials, an unfavorable political climate, and societal and institutional concerns related to the use of cannabinoids. It is reasonable to question why there has been so little research conducted in this area, and it is possible that obstacles to the conduct of such research continue to exist.

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References


(Accepted for publication October 1, 2006.)

In Reply:—We thank Pierre Beaulieu for his interest in our study of cannabinoids for postoperative pain. However, we cannot concur with his view that cannabinoids are only moderately effective analgesics. We found that, across different surgeries and institutions, pain relief equivalent to the best available postoperative analgesia was achieved. We agree that results from one type of cannabinoid may not be comparable to those from another type and that larger studies are needed. However, we found that our stringent entry criteria, intended to exclude patients with serious medical conditions, made it essential to recruit from a large number of centers.

Our study was designed to measure the effectiveness of cannabinoids alone without the advantage of other analgesic combinations, as occurs in clinical practice.¹ Hence, the time to escape analgesia was an important outcome. In comparison with the study of Beaulieu,² in which the outcome measure was morphine consumption, we can be sure that the effects we measured were from cannabinoids, not the result of synergistic effects from an opioid-cannabinoid combination, as has been described in preclinical studies.³ Our aim was to investigate whether cannabinoids were analgesic when administered as the sole agent and to find their most effective dose; our results provide confirmatory evidence for analgesia at the higher doses we used.

Interestingly, the duration of our study was determined in pretrial workshops by the same authors that Beaulieu cites as advocating durations longer than 6 h. The rationale was that this was the first time that cannabinoids had been used postoperatively, in fasting patients, and a single-dose study was safest; furthermore, a 24-h study would have required waking the patient at night to obtain regular pain scores, which might have made recruitment even less attractive. In retrospect, the times to rescue analgesia also support this view. Because total pain relief and pain intensity differences summary measures carry forward the final value before rescue analgesia, sensitivity would be lost if assessments continued until all patients requested escape analgesia. In addition, the use of an oral preparation precluded early administration after major surgery, and the single dose was delivered to most patients within 24 h of surgery. This regimen was similar to that used clinically in the study centers for orally administered analgesics.

Beaulieu asks how pain on movement was measured. The protocol for the study standardized these movements for each type of surgery to establish conformity across sites; our method was comparable to his. He also requests justification for and content of Cannador (IKF, Berlin, Germany). Most of these details are in the article. In addition, the study was conducted in parallel with the CAMS study⁴ because postoperative pain and multiple sclerosis were both considered to be important areas to investigate the medicinal uses of cannabis.
Paravertebral Blocks in Thoracoscopy: Single No, Continuous Yes

To the Editor:—We read with great interest the article by Hill et al.1 regarding the analgesic efficacy of single-dose, multilevel paravertebral nerve blockade (PVB) for thoracoscopic surgery. Given our own experience with PVBs (160-200 patients per month using both single and continuous, unilateral and bilateral PVBs for a wide variety of cases) in thoracoscopic surgery, we find their results most believable. Single-shot PVB analgesia is not long-lasting, and pain after thoracoscopic surgery is actually quite significant in the first 24 h and even beyond (especially with the continued presence of a chest tube). Our quarrel with these authors is not with their methods or their findings, but with their conclusions. It is akin to concluding that, because single-dose nerve blocks do not provide prolonged analgesia after total knee replacement, peripheral nerve blocks are of no use for this surgery. Clearly that would be a perverse extrapolation, and most would recognize that such findings indicate the need for continuous blockade.

Our approach to postoperative pain management after thoracoscopic surgery includes routine preoperative placement of a single paravertebral catheter at a level of T5 or T6. This is much more time efficient and comfortable for the patient than placing multiple blocks. We have found no loss of analgesic efficacy by eliminating the single-shot blocks at multiple levels and have observed both clinically and with contrast dye injection that the sole catheter does indeed provide for multiple levels of paravertebral blockade. Besides simplicity and a minimum of side effects, the advantages of a single continuous paravertebral catheter are its effectiveness, its flexibility, and its adaptability. A PVB catheter allows for titration of the local anesthetic and extension of nerve blockade as needed. With further bolus dosing and adjustment of infusion rates, PVB analgesia is individualized before patient discharge from the postanesthesia care unit. In fact, our post-anesthesia care unit nurses work closely with our acute interventional postoperative pain service and are most comfortable working with peripheral nerve block infusions. By having a catheter in place, we can also continue the nerve blockade until removal of the chest tube (typically the determining factor in timing of hospital discharge after thoracoscopic surgery), thus minimizing pain and opiate consumption for the duration of this period. Moreover, in the event that the thoracoscopic procedure turns into an open thoracotomy, the PVB catheter is already in position to readily provide postoperative analgesia and adjust it to the patient’s needs.

It has been suggested that thoracic PVB may replace the thoracic epidural technique as the gold standard for providing analgesia for patients undergoing thoracotomy.2 In our institution, this has been the case for some time, and it has had a profound and positive impact. We urge our colleagues to move forward in learning and applying continuous PVB in their practices.

Bruce Ben-David, M.D.,* Rita Merman, M.D., Jacques E. Chelly, M.D., Ph.D., M.B.A. *University of Pittsburgh Medical Centers Presbyterian-Shadyside Hospital, Pittsburgh, Pennsylvania.

References


In Reply.—The search for the optimal pain management technique after thoracoscopic surgery remains a clinically important undertaking. Although I support the enthusiasm of Drs. Ben-David, Merman, and Chelly for the development of safe and effective postoperative analgesia, I disagree with their assessment of our clinical trial. Because the mode for hospital length of stay in my institution after thoracoscopic surgery is 1 day, we evaluated single-dose, multilevel paravertebral nerve blockade in a randomized, double-blind, placebo-controlled clinical trial using postoperative patient-controlled morphine usage as our primary endpoint. We found a significant reduction in narcotic requirement for the first 6 h after block placement, but the benefit did not persist. Therefore, we concluded, “Single-dose paravertebral nerve blockade with bupivacaine is effective in reducing pain following thoracoscopic surgery,” but only during the first 6 h after nerve blockade. Due to the limited duration of effect with currently available local anesthetic agents, our data suggest that, at present, this technique is not indicated in the setting of thoracoscopic surgery.3-4 We did not study continuous paravertebral local anesthetic infusion, nor did we conclude that this technique would be ineffective.

Whereas I support continued study of paravertebral catheter placement with continuous local anesthetic infusion, I caution against advocacy for a therapeutic intervention with associated risk and expense based on clinical experience in a series of patients without adequate study in a randomized, double-blind, placebo-controlled clinical trial. Before our trial, I was enthusiastic about the technique and was of the opinion that the results of the study would be unequivocally positive. I had observed that patients seemed to wake up with less pain and required less narcotic administration in the recovery area. Although these observations proved to be correct, patients were less comfortable after leaving the recovery room. From 6 to 12 h after nerve blockade, the treatment group used more patient-controlled morphine than the control group, making the cumulative morphine dose indis-
tunguishable between groups by the 12th h. Although morphine usage
may be an imperfect endpoint, we were unable to explain the in-
creased narcotic requirement in any way other than loss of paraverte-
bral block efficacy. Although adverse events were mild and similar
between groups, increased narcotic usage by patients undergoing
pulmonary resection after leaving the closely monitored recovery area
could potentially lead to respiratory complications. Our results do
suggest that continuous paravertebral local anesthetic infusion could
be a superior technique for this patient population. However, investi-
gator bias can only be eliminated by well-designed clinical trials.
Therefore, I suggest further randomized, double-blind, placebo-con-
trolled studies of continuous thoracic paravertebral nerve blockade
before promoting this technique as the analgesic gold standard in
thoracoscopic surgery.

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Reference

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Newman MF. Efficacy of single-dose, multilevel paravertebral nerve blockade for

(Accepted for publication October 2, 2006.)

To the Editor—The excellent article by Jonsson
et al. provides bio-
physical insight into the mechanism of action of succinylcholine on the
muscle-type acetylcholine receptors. They conclude that succinylcho-
line activates these receptors followed by desensitization.
The initial phase of activation results in an endplate potential that
opens the adjacent voltage-gated sodium channel, resulting in re-
petitive waves of action potentials that manifest as initial muscle
fasciculations. Because succinylcholine is not metabolized by the
specific cholinesterase at the endplate, the succinylcholine-induced
depolarization is maintained, and the outer voltage-gated sodium
channel remains open. However, the inner time-dependent sodium
gate will close, resulting in an endplate-muscular block. Because the
depolarizing block is beyond the endplate, it is not characterized by
tetanic fade or posttetanic facilitation and is potentiated by neostig-
mime (fig. 1A).

Prolonged exposure of the endplate to succinylcholine will result in
progressive desensitization to the depolarizing action of succinylcho-
line, as well as to the chemical transmitter acetylcholine; hence, the
block will gradually change from a depolarizing endplate-muscular
block (Phase I) into a desensitizing Phase II neuromuscular block,
which is characterized by progressive tetanic fade and posttetanic
facilitation. The neuromuscular block may be antagonized by neostig-
mime. The degree of reversal by neostigmine is proportional to the
extent of fade and posttetanic facilitation (fig. 1, B and C).2

Fig. 1. Tracings of the twitch response to
ulnar nerve stimulation in three patients
with homozygote atypical plasma cho-
linesterase. (A) Administration of succi-
nylcholine 0.1 mg/kg resulted in a depo-
larizing block characterized by minimal
tetanic fade (T) and no posttetanic facili-
tation (PTF). Neostigmine 0.05/mg poten-
tiated block. (B) Twitch response in a sec-
ond patient with atypical esterase
showing recovery of the twitch response
after succinylcholine 0.1 mg/kg, associ-
ated with moderate tetanic fade and post-
tetanic facilitation. Neostigmine 2.5 mg
accelerated recovery. (C) The twitch re-
sponse in a third patient with atypical
esterase. Injection of succinylcholine 1
mg/kg resulted in a very prolonged neu-
romuscular block. After 90 min, recovery
started and was associated with marked
tetanic fade and posttetanic facilitation.
Administration of neostigmine 0.05
mg/kg could completely reverse the
block. Modified from Baraka.2
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In Reply.—We thank Dr. Baraka for his insightful comments on our article.3 As Dr. Baraka pointed out, one of our findings is that the muscle-type nicotinic acetylcholine receptor, when expressed in Xenopus oocytes, is desensitized by succinylcholine after an initial activation. Because we have not studied the neuromuscular junction with all its components, we cannot from this type of study fully investigate the mechanism of action by succinylcholine-induced neuromuscular block.

To the best of our knowledge, succinylcholine seems to cause neuromuscular blockade due to a prolonged depolarization of the endplates,7 which might include both desensitization of the muscle nicotinic acetylcholine receptors and inactivation of voltage-gated sodium channels. In addition, we suggest that the low affinity of succinylcholine for the presynaptic α3β2 nicotinic acetylcholine receptor subtype explains the lack of tetanic and train-of-four fade during succinylcholine-induced neuromuscular block.1

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References


(Accepted for publication October 2, 2006.)

To the Editor.—Investigations dealing with blood transfusions and their components have recently been more and more focused on the risk-to-benefit issue. This has led to the investigation of specific organ functions under isovolemic anemia to define rational transfusion triggers. From these results, investigators have tried to define physiology-based parameters for transfusion triggers.1–7

In their recent article, Weiskopf et al.8 showed that erythrocytes stored for 3 weeks are almost as efficacious as are fresh erythrocytes for reversing the cognitive functions. Based of these results, the authors reject the current opinion that stored erythrocytes off-load oxygen less than fresh erythrocytes do.

As Spahn and Madjdpour argued in the corresponding editorial,9 the authors did not prove a direct correlation between better neurologic adaptation10 and endurance capacity11 in young iron-depleted but nonanemic women. Knee extensor exercise in nonanemic but iron-depleted women is also improved after iron supplementation.12 In the context of Weiskopf et al.’s investigation, it is of great interest to note that iron supplementation has been shown to improve some cognitive functions in nonanemic iron-depleted adolescent girls.13

Withdrawing one blood unit (450 ml), as described by the authors, corresponds to 250 mg of iron, which is almost 10% of the volunteer’s (mean weight 64 kg) functional iron. One unit has been withdrawn 4–5 weeks before the first experimental day (to gain stored blood for the experiments). Further blood units (iron included) were withdrawn to induce isovolemic anemia without (mentioned) iron substitution. According to previous articles,1–3 hemoglobin levels were roughly halved by withdrawing further blood in 450-ml steps for each experiment. Halving the hemoglobin concentration goes hand in hand with remarkable additional iron depletion. Unfortunately, no data concerning the volunteers’ iron status have been given.

Based on this view and supported by the literature,10–15 one could argue that, with the authors’ protocol, iron was withdrawn and retransfused in comparable amounts in both groups (“fresh” and “stored”). Iron could have been one of the “important” factors explaining the observed results, which are comparable to the cognitive test results reported by Bruner et al.15 Thus, one may wonder whether the results observed in Weiskopf et al.’s investigation could be partly explained by transient iron deficits.

For the future, it will be of great interest to investigate parameters other than hemoglobin levels and oxygen release by 2,3-diphosphoglycerate when dealing with the question of whether to transfuse blood or not.

Symptoms like fatigue or dizziness, which are frequently observed in postoperative orthopedic surgery, could be greatly reduced by improving iron status—preoperative and postoperative prescription—instead of postoperative blood transfusions.

Hans Jutzi, M.D., Markus Risch, M.D., Stephan Blumenthal, M.D., Alain Borgcat, M.D.*, ‘Balgrist University Hospital, Zurich, Switzerland. alain.borgcat@balgrist.ch

References


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Anemia-induced Neurocognitive Dysfunction: Is Oxygen the Only Player?

To the Editor,—Investigations dealing with blood transfusions and their components have recently been more and more focused on the risk-to-benefit issue. This has led to the investigation of specific organ functions under isovolemic anemia to define rational transfusion triggers. From these results, investigators have tried to define physiology-based parameters for transfusion triggers.1–7

In their recent article, Weiskopf et al.8 showed that erythrocytes stored for 3 weeks are almost as efficacious as are fresh erythrocytes for reversing the cognitive functions. Based of these results, the authors reject the current opinion that stored erythrocytes off-load oxygen less than fresh erythrocytes do.

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Symptoms like fatigue or dizziness, which are frequently observed in postoperative orthopedic surgery, could be greatly reduced by improving iron status—preoperative and postoperative prescription—instead of postoperative blood transfusions.

Hans Jutzi, M.D., Markus Risch, M.D., Stephan Blumenthal, M.D., Alain Borgcat, M.D.*, ‘Balgrist University Hospital, Zurich, Switzerland. alain.borgcat@balgrist.ch

References

In Reply.—We thank Drs. Jutzi, Risch, Blumenthal, and Borgeat for their interest in the report of our finding that erythrocytes stored for 5 weeks are as efficacious as fresh erythrocytes (3.5 h storage) in reversing anemia-induced cognitive function deficits in healthy humans.1 The measured hemoglobin \( P_0 \) at the time of cognitive testing, a few minutes after transfusion, support the lack of a physiologically significant increase in the stored erythrocytes' \( P_0 \) from the measured low value of 15 mmHg, as is to be expected from the measured in vivo rate of regeneration of 2,3-diphosphoglycerate in erythrocytes stored in citrate-phosphate-dextrose-adrenaline.2 This is not in agreement with the conjecture3 based on 2,3-diphosphoglycerate data from erythrocytes stored in acid-citrate-dextrose and transfused more slowly.4 Jutzi et al. suggest that the neurocognitive deficit created by isovolemic anemia was secondary to iron deficiency and that the reversal of the cognitive deficit for erythrocytes of both storage durations was produced by transfusion of iron, rather than an increase of oxygen delivery by transfused hemoglobin. Although our results do not seem to be consistent with an inability of 2,3-diphosphoglycerate–depleted erythrocytes to release oxygen from hemoglobin, we do not believe that there is evidence to support the suggestion of Jutzi et al.

Beutler has concluded that it is unclear whether "iron deficiency without anemia" can cause symptoms. As pointed out by Beutler, the lack of clarity is, at least in part, owing to the difficulty in separating these experimentally.5 When iron is administered to patients with a "normal" hemoglobin concentration (i.e., at the lower end of the normal range), the latter, nevertheless, can increase.6 Although Jutzi et al. cite work showing that iron therapy improves exercise capacity and endurance, fatigue, and cognition in weeks to months, other studies have failed to find such improvement.7 Most importantly, the improvement found in those studies was among patients with chronic, not acute, iron deficiency, and the reported improvements occurred after weeks to months of therapy. We are unaware of reports of such improvement in the few-minute time frame of our study, or even within a few days. A systematic review found "no convincing evidence"8 of an effect of iron therapy on improvement of psychomotor development and cognitive function 5-11 days from the commencement of therapy.9 The sole study cited by Jutzi et al. as having demonstrated improved cognitive effects reported a very small effect in only one of four subanalyses (by multiple linear regression but not analysis of variance), but not the overall evaluation, after 8 weeks of iron therapy, with a concomitant increase in hemoglobin concentration that resulted in a greater hemoglobin concentration than in the control group.8

Decreased blood oxygen content and delivery caused by hypoxia decreases maximal oxygen consumption (exercise capacity) immediately,10 and acute isovolemic anemia to hemoglobin concentration of 5 g/dl alters central processing10 and cognitive function11 and increases fatigue,12 whereas increasing oxygen content at this critical level reverses these effects. Chronic iron-deficiency anemia alters central processing as determined by P300 latency,13 and 90 days of iron therapy reverses the iron deficiency and improves the anemia but does not improve the prolonged P300 latency.14 Increasing hemoglobin concentration immediately normalizes acute anemia-induced prolongation of P300 latency.10 Breathing oxygen for 5 min, without alteration of iron stores, completely reverses anemia-induced cognitive deficits.11 These results indicate that oxygen, not iron, is responsible for the improved cognitive function. Acute anemia to the identical degree does not alter peripheral or central nerve conduction,14,15 which also argues against an immediate direct neural effect of an acute decrease in blood iron content.

As we stated in our article, we do not have data to support or refute the several possible explanations we discussed.1 However, we do not feel compelled to add iron deficiency and replacement as a possible cause of our findings.

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References


To the Editor—I read with interest the report from Nyktari et al. on the interaction between the physical properties of halogenated vapors and pulmonary resistance.1 Their observations have important implications on the choice of anesthetic agents in selected patient populations.

These results are consistent with modeling their experimental apparatus as a simple orifice. Flow through an orifice is directly proportional to the square root of the density of the gas and inversely proportional to the square root of the density of the gas. This is shown in the following equation

\[ Q \propto \sqrt{\Delta P/\rho}, \]

where \( Q \) is the volumetric flow, \( \Delta P \) is the pressure gradient, and \( \rho \) is the density. As the authors calculated resistance from the pressure gradient needed to deliver a constant flow, equation 1 can be modified by squaring both sides and rearranging terms into a form analogous to Ohm’s law (\( V = IR \)):

\[ \Delta P \propto Q \times (Q_0). \]

In equation 2, the term \( Q_0 \) corresponds to the resistance; when the flow is held constant, as in the described experiment, the resistance will increase linearly with the density of the gas. Plotting the density (calculated as the weighted sum of the molecular weights divided by the molar volume at standard temperature and pressure) of a mixture of various minimal alveolar concentration (MAC)–multies of desflurane, sevoflurane, and isoflurane at 25% oxygen and 75% nitrogen produces a graph that is strikingly similar to the authors’ figure 4 (see fig. 1). Using only the mean values in the authors’ figure 4, there is a strong linear relationship between MAC–multiple and resistance. The linear relationship between MAC–multiple and resistance is shown in the following equation

\[ Q \propto \sqrt{\Delta P/\rho}, \]

These physical principles are the justification for the use of helium, a low-density gas, in patients with stenotic airways. As such, desflurane should probably be used with caution in patients with airway obstruction, even ignoring its propensity to aggravate airway reflexes. Despite the authors’ note that desflurane has been successfully used in spontaneously breathing patients, it should be recognized that desflurane will, through its effects on gas density, increase the work of breathing in patients with a native glottis (facemask or laryngeal mask airway), in which the vocal cords act as an orifice. The final decision as to whether these considerations are of clinical significance awaits further study, but the authors should be congratulated for bringing this issue to our attention.

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Reference


(accepted for publication October 23, 2006.)
Anesthesiology 2007; 106-403

Use of Macintosh Laryngoscope No. 2 for Adult Patients with a Short Thyromental Distance

To the Editor—We read with great interest the article by Tripathi and Pandey1 reporting that the use of the Macintosh laryngoscope No. 3 (Mac #3) in patients with a short thyromental distance was associated with great difficulty in laryngoscopy and intubation compared with the Mac #2. We previously assessed the laryngeal aperture fiberoptically during direct laryngoscopy with the Mac #3 in 17 patients whose glottis was invisible under direct vision (difficult laryngoscopy).2 In one fourth of these patients, the laryngoscope could not provide an adequate fiberoptic view of the laryngeal aperture because of an inability to lift the collapsed laryngeal tissues caused by general anesthesia and the muscle relaxant.3 That is, in these patients, it is difficult to place the blade tip of the Mac #3 in the position necessary to lift the epiglottis and the laryngeal soft tissues. The authors clarified this problem by measuring the intubation distance and overcame it by using the Mac #2 with its thinner flange and greater curvature of the spatula. We respect their ideas. However, adult patients whose airway is predicted as difficult by a short thyromental distance have a small mandible, but the size of their maxilla is usually normal (defined as micrognathia), which is different from pediatric patients. Moreover, they often have protruding upper incisors. Thus, we are concerned that when the Mac #2, which is 1.5–2 cm shorter than the Mac #3, is used with these patients, the whole blade gets into the oral cavity, and a good laryngoscopic view is not obtained even if the blade tip reaches the optimal position required to lift the laryngeal soft tissues.

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References


Fig. 1. Comparison of the effect of different volatile anesthetics at equivalent concentrations on total pulmonary resistance. At 1 minimal alveolar concentration (MAC), only desflurane significantly increased pulmonary resistance compared with isoflurane and sevoflurane (P < 0.001 for both comparisons). The difference between isoflurane and sevoflurane was not statistically significant (P = 0.15). At 1.5 MAC, sevoflurane significantly increased total pulmonary resistance compared with isoflurane (P = 0.015), whereas desflurane caused a more pronounced increase compared with isoflurane and sevoflurane (P < 0.001 for all comparisons). The same findings apply at 2 MAC concentrations. Sevoflurane significantly increased pulmonary resistance compared with isoflurane (P < 0.001), and desflurane caused a significant increase compared with the other two volatile agents (P < 0.001 for both comparisons).

The lung model used for the experiment simulates the central part of the respiratory system, mainly the trachea and the main bronchi, and our results are not explained by modeling the laboratory lung as a simple orifice. Nevertheless, our findings suggest that desflurane can increase the work of breathing in patients with upper airway obstruction. The respiratory system is much more complicated than our laboratory model, and many factors affect the overall pulmonary resistance. Because desflurane may possess a degree of bronchodilatory properties, we should await further studies in humans to clarify the clinical relevance of our observation.

Vasilia G. Nyktari, M.D., Alexandra A. Papaioannou, M.D., Ph.D., D.E.A.A.,* George Prinianakis, M.D., Ph.D., Dimitris Georgopoulos, M.D., Ph.D., Helen Askitopoulou, M.D., Ph.D. *University Hospital of Heraklion, Crete, Greece. fraidak@med.uoc.gr

Reference


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to remove the EZchange assembly and mount the canister directly to the machine. When this problem occurred, warning messages (e.g., ‘flow sensor error’ or ‘system leak’) appeared, and the machine might be difficult, but we have not encountered this problem. The problem seemed to be intermittent and, in some instances, was not detected or appreciated during automated checkout. The exact cause of failure of this connection is still unclear and, according to a company representative, is under investigation by the manufacturer. A solution has been developed and is expected to be available soon.

To the Editor:—The recent letter by Fetterman et al. on esophageal misplacement of a size 14-French Cook airway exchange catheter (Cook Critical Care, Bloomington, IN) highlights potential complications of this equipment. We believe that the length of these catheters may be excessive. Cook airway exchange catheter sizes 11-, 14-, and 19-French have a length of approximately 83 cm; however, because they are intended for use with single-lumen endotracheal tubes, we believe that a length of 56 cm is all that is really needed. Many of the complications associated with airway exchange catheter use result from overly deep placement of these catheters; a length of 83 cm far exceeds the length necessary for safe endotracheal tube exchange and may lead to overly deep airway exchange catheter placement by inexperienced users who inadvertently ignore the guide marks. Whereas we realize that a length of 56 cm can still allow for complications, we believe that reducing the length of these catheters may help to reduce the incidence of such complications. Finally, we would like to echo the authors’ recommendations on capnographic confirmation of tracheal airway exchange catheter placement.

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Malfunction of the New Aisys® Anesthesia Machine

To the Editor:—We recently replaced most of our anesthesia machine fleet with the newly available Aisys® Carestation (GE Healthcare, Waukesha, WI). In the course of using these second-generation electronic machines, we have encountered two clinically significant problems that have each occurred on more than one unit.

The first problem relates to the EZchange absorber bracket. This component should create a gas-tight seal between the anesthesia machine and absorbent canister. It is also designed to automatically seal the circuit when the absorbent canister is removed, as during absorbent canister changes. We found instances in which a significant leak existed both with and without the canister in place (with one case even requiring patient ventilation with a manual resuscitator to achieve adequate tidal volumes.) The problem was localized to the interconnect valve between the canister and EZchange (fig. 1). The problem seemed to be intermittent and, in some instances, was not detected or appreciated during automated checkout. The exact cause of failure of this connection is still unclear and, according to a company representative, is under investigation by the manufacturer. A solution has been to remove the EZchange assembly and mount the canister directly to the machine.

The second problem involves the built-in spirometry sensors. When this problem occurred, warning messages (e.g., tidal volume not achieved, check flow sensors, system leak) appeared, and the calculated expired tidal volumes were well below those actually delivered. Further analysis revealed that excess moisture in the

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In Reply—GE Healthcare would like to thank Anesthesiology for the opportunity to respond to the Letter to the Editor by Drs. Wax and Neustein.

In their letter, the authors discuss two issues pertaining to the Aisys® machine. The first concerns the function of the EZchange absorber manifold, the second concerns the effect of water on the inspiratory and expiratory flow sensors.

We have been able to reproduce the leak described by the authors. The breathing circuit leaks experienced at Mt. Sinai (New York, NY) were the result of an inadequate seal between the manifold and the absorber canister; small frays on the drain port of the disposable absorber canister produced the inadequate seal. This issue may be identified during the Aisys® automatic system checkout procedure; we have verified this with affected canisters. Of course, if the canister is changed in the middle of a case, there would be no additional system checkout and a leak may result.

We have not been able to reproduce a system leak when the EZchange manifold is in place without a disposable canister because the leak exists where the manifold and the disposable absorber canister connect, as suggested by the authors.

We have taken a two-pronged approach to resolving this issue. First, we are addressing the root cause, the disposable absorber canister, by working with the third-party supplier to remedy the issue with the drain port flashing. Second, we are currently revising the drain seal on the EZchange module so this flashing, even if unchanged, will not affect the seal between the canister and the manifold.

With respect to the moisture and flow sensor issue, the root cause was most likely the impact that moisture or water may have on the function of the flow sensor. Our flow sensors are a vital component of the Aisys® ventilator. Like anesthesia practice, these sensors have undergone extensive evolution since they were first introduced. As clinical anesthesia has moved toward lower and lower fresh gas flows, the impact of the increased humidity has necessitated a redesign of the basic flow sensor. At the time of the events, Mt. Sinai was using an earlier version of the flow sensor. The current flow sensor incorporates an offset in the area of the flow sensor flap that helps to overcome the issue associated with high moisture or water. This version can be readily recognized by the presence of grooves on the bezel of the flow sensor closest to the patient.

The final issue the authors describe is the use of the D-Lite sensor to obtain spirometry instead of using the flow sensors. The D-Lite sensor and the direct patient monitoring of respiratory mechanics the use of the D-Lite may provide are a design feature of the Aisys®. Using the D-Lite will not resolve alarm issues produced by the inspiratory and expiratory flow sensor. The authors’ description of the ventilator alarms suggests that the old-style flow sensors they were using at the time may have either had some moisture on the inspiratory sensor or may not have been calibrated.

The flow sensors should be calibrated daily simply by removing the flow sensor module, waiting for the “no insp flow sensor” and “no expiratory flow sensor” messages to appear, and then reattaching the flow sensor module. The alarm messages reported by Mt. Sinai would not have occurred with calibrated offset flow sensors.

GE Healthcare commends the authors for their insightful and accurate Letter to the Editor.

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Decreases Carboxyhemoglobin Level in Inpatients Undergoing Surgery

To the Editor:—Over the past two decades, many hospitals in advanced countries have declared a ‘smoke-free hospital,’ and adoption of such a policy has recently, albeit very belatedly, begun in Japan. Because cigarette smoking is a risk factor for mortality and morbidity, this implementation may be expected to produce measurable benefits for the majority of patients, provided it effectively reduces smoking in the hospital environment.1 In cigarette smokers, as well as in passive smokers, blood carboxyhemoglobin concentration (COHb) is known to be elevated.2 Herein we report a significant reduction in COHb values in surgical inpatients following the implementation of such a policy.

We compared COHb values before and after the implementation of a smoke-free hospital (on April 1, 2003) after more than a year’s preparation. We collected all arterial blood oxymetry data (measured using an ABL700, Radiometer, Copenhagen, Denmark) obtained from those inpatients undergoing surgery who had an arterial puncture or an arterial line in place for collection of arterial blood samples in the operating room. Arterial blood samples taken just before or after the induction of anesthesia were immediately subjected to the measurement of arterial blood gas tensions and COHb. The implementation of a smoke-free university campus was begun on April 1, 2005. Differences in COHb were examined via a one-way analysis of variance with an unpaired t test (with a Bonferroni correction) being used for post hoc comparisons.

As shown in figure 1, the mean values COHb were 1.65 ± 0.87% (n = 656, mean ± SD) over the 3 months before the implementation of a smoke-free hospital and 1.15 ± 0.50% (n = 614) just after the implementation, and this decreased COHb level remained stable. After the implementation of a smoke-free university campus (April 2005), it showed a slight decrease to 0.98 ± 0.40% (n = 713) over the next 3 months. There was no difference in age distribution, hemoglobin concentration, or arterial oxygen tension (PaO2) before and after the implementation. Whereas in 2002 the percentage of surgical patients who were smokers was 26.9% and the average hospital stay before surgery was approximately 6.7 days, in 2005, these were smaller (22.5% and 5.4 days, respectively). Among outpatients, the mean COHb values were 1.74 ± 0.94% (n = 1,069) for 12 months before the implementation of the smoke-free hospital and 1.64 ± 0.72% (n = 1,475) after the implementation of a smoke-free university campus.

These data document that the implementation of a smoke-free hospital caused a dramatic decrease in COHb values among surgical inpatients. It is unlikely that upon implementing a smoke-free hospital, all patients who smoked had stopped before admission. However, such patients were no longer able to smoke in the hospital buildings and had an approximately 6-day period of forced abstinence or reduction of smoking before surgery after the policy implementation. These data could also preclude the possibility that seasonal variations in COHb among the population admitted to our hospital might have affected the results, as COHb did not significantly change during the observation period, except the timing of the implementation of the smoke-free hospital.

Fig. 1. Changes in the blood levels of blood carboxyhemoglobin concentration (COHb; %) in inpatients undergoing surgery before and after the implementation of a smoke-free hospital on April 1, 2003 (A). Sp = spring (April to June), Sm = summer (July to September), Au = autumn (September to December), Wn = winter (January to March). No data are available for spring 2004 because of moving to the new hospital. The implementation of a smoke-free university campus was begun on April 1, 2005 (B). a = P < 0.01 compared with autumn 2002; b = P < 0.01 compared with winter 2003; c = P < 0.01 compared with outpatients (OP) 2005; N.S. = not significant.

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Implementation of Smoke-free Policy in University Hospital

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