Letters to the Editor

The Use of Pulse Oximeter Functional Testers in Evaluating $\text{SpO}_2$ Accuracy

To the Editor:

We read with interest the study of pulse oximeter probes (1), but must take exception to the study’s methodology and conclusions. The study was intended to evaluate and compare the accuracy of prototype nonproprietary probes (OSS/Dolphin Medical) to their counterpart proprietary sensors from the original equipment manufacturers (Nellcor, DATEX-Ohmeda, and Criticare). The protocol used a bench-top patient simulator (BioTek Index) to compare the various sensors’ pulse oximeter $\text{SpO}_2$ readings with the simulator set points. The study’s conclusions suggest equivalence or superiority of performance in nonproprietary probes.

Unfortunately, the authors have grossly misinterpreted the ability of this simulator to evaluate the probe’s contribution to $\text{SpO}_2$ reading accuracy. While the Index and several other commercially available patient simulators can test sensor and pulse oximeter functionality, they are incapable of providing the required data needed to properly evaluate the accuracy of $\text{SpO}_2$ readings (according to a letter to Nellcor Puritan Bennett from Peter M. Weith, BioTek Instruments, October 1995). With respect to pulse oximeter probes, the patient simulator used in the study is capable only of “testing for shorts, continuity, opens and LED functionality” (2). Evaluating $\text{SpO}_2$ reading accuracy would require, at a minimum, accommodating the wavelength characteristics of the sensor and reproducing the complex optical interaction of the sensor and the patient’s tissue.

There are no bench-top simulators available today that can provide the data the authors would need to support their conclusion. The ASTM and ISO Committees for developing pulse oximetry standards have recognized the issue of bench-top accuracy testing (3), and an effort supported by the European Union to create such a device is currently underway (4,5). We are aware of only one validated method to assess $\text{SpO}_2$ reading accuracy—direct comparison to sampled arterial blood $\text{SaO}_2$ measured using a laboratory co-oximeter.

We therefore disagree with the authors’ two conclusions; their study demonstrates neither equivalency of the nonproprietary probes, nor that a simulator can be used to evaluate pulse oximetry probe accuracy.

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References

In Response:

We thank the authors for their interest in our paper and their comments. However, we believe that for the most part they do not address our study specifically, but rather present editorial comments unrelated to the purpose and conclusions of our study. We would, however, like to clarify the one issue regarding the design of our study that they appear to have focused on. It was designed to compare sets of commercially available probes from three manufacturers with prototype probes from OSS. We incorrectly used the word accuracy once in the context of our study, and then only in the abstract, but believe it is clearly stated in the methodology, results, and discussion that we tested variability among specific probes, not absolute accuracy.

We used a “black box” method, looking at inputs and outputs. Inputs are simulated signals applied to the probes, and outputs are the SpO2 values calculated by the monitors. The Index SpO2 tester (Bio-Tek) is designed for this purpose and the 510(k) submission, which we obtained and reviewed in detail before submitting the manuscript, states “The testers can also be used as a transfer standard for pulse oximeters. That is, a pulse oximeter’s performance may be compared to another of the same type or of a different type” (1). The testing includes the probe and the pulse oximeter unit, because with the Index, they are tested together.

We demonstrated by experimentation that we did not add any variability with our experimental design, and thus any variability we measured is caused by differences in probes (most likely due to variability in the LEDs). It is this variability that we have noted in our paper.

We are fully aware of the limitations of the Index tester and clearly stated them in the manuscript. Contrary to the comments of the authors, the tester is not limited to testing for shorts, continuity, opens, and LED functionality. While this is true for the E model of the tester (it does not contain the artificial finger and is limited to testing the electrical signals only), this is not true for the models containing the artificial finger, which is what we used and clearly stated.

We stand by our conclusions that the prototype OSS (Dolphin Medical) probes are equivalent to other manufacturers’ probes and that a simulator can be used to provide input signals for the evaluation of pulse oximeter probes. We agree that these methods are not sufficient to determine absolute accuracy as compared to a “gold standard” such as blood gas analysis.

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Reference
1. Bio-Tek 510(k) Index 2, E, F, EF & CardioSat 100 E, F, EF. FDA approval K971273, obtained under Freedom of Information request.
Bleeding Diathesis Due to Failed Antagonism of Heparin: Successful Treatment with Recombinant Factor VIIa

To the Editor:

We here report on a patient whose protamine allergy was unknown to us, and who was admitted to the intensive care unit (ICU) after open heart surgery with a bleeding diathesis due to failed heparin antagonism. Despite transfusing red blood cells, fresh frozen plasma, and platelets, bleeding went on undiminished. Since treatment with recombinant factor VIIa (rFVIIa) has been described in intractable bleeding after redo coronary artery bypass graft surgery (1), we decided to treat this patient with rFVIIa. We infused 90 µg/kg rFVIIa. The following 30 min blood loss was about 100 mL, and cessation of bleeding could be observed during the next hour. Considering the mechanism of action of rFVIIa, which is described elsewhere (2), cessation of bleeding shortly after the application of rFVIIa suggests, however, a true procoagulant effect rather than a mere epiphenomenon. The patient recovered and no morbidity (i.e., myocardial infarction due to bypass-thrombosis) was observed. Long-term follow-up (3 mo) revealed complete recovery. With this case, we assume that application of rFVIIa is a therapeutic option when, due to protamine-allergy, antagonizing heparin fails. To our knowledge, this is the first case that reports on treatment with rFVIIa in this clinical scenario.

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References

Can Organophosphates Facilitate Acute Tongue Necrosis?

To the Editor:

A 76-year-old woman was admitted to the emergency department with respiratory insufficiency, vomiting, and diarrhea 4 h after she had eaten some cabbage contaminated with organophosphates (OP). On physical examination, she was comatose, her pupils were 2 mm in diameter, her temperature was 35°C, heart rate was 100 bpm, arterial blood pressure was 70/50 mm Hg, and arterial blood gas analysis revealed severe hypoxia and acidosis (pH: 6.99, PaO₂: 55 mm Hg, PaCO₂: 55 mm Hg, HCO₃⁻: 15 mmol/L, BE: −15, SO₂: 80%). The trachea was intubated and the lungs ventilated with 100% O₂. KCl replacement, insulin, and dopamine were administered via IV route in ICU. Approximately 10 h after her arrival, anuria and acute renal failure occurred. Subsequently, peritoneal dialysis was established, and intense medical treatment was continued. Eight days after exposure of OP, necrosis in 2/3 anterior of the tongue was realized (Fig. 1). The plastic surgeon recommended mouth care and elective serial debridement of necrotic tissues. Unfortunately, the patient died 24 days after OP exposure because of sepsis and OP poisoning complications before elective surgical treatment of tongue defect.

Circulatory disturbances and even necrosis of the tongue are extremely rare because of its rich blood supply (2). Ischemic lingual necrosis is most often due to temporal arteritis and systemic lupus in elderly women (3). In this case, local necrosis of the tongue did not occur as a result of only pressure from the endotracheal tube or airway. To our knowledge, none of the patients in our ICU have ever developed tongue necrosis. Therefore, we thought that this rare complication (tongue muscle necrosis) may be related to ingestion of OP via cabbage. OP may cause muscle fiber degeneration and necrosis in muscles such as diaphragm and myocardium (4,5). Also, pancreatic necrosis and parotitis may develop in OP poisoning. In sheep exposed to OP, histopathological changes have been found in tissue samples of tongue, myocardium, lungs, jejunum, liver, kidneys, etc. (6). To our knowledge, tongue necrosis due to OP poisoning has not been reported in human patients in the published literature.

Although tongue necrosis is extremely rare, OP poisoning may be often observed in developing countries. In our case, both OP and mechanical ventilation due to compression of plastic instrumenta-

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3. Korn S, Huppert A, Spitzer S, DelHoratius RJ. Systemic lupus erythematosus present-

Figure I. Appearance of tongue necrosis.
Difficult Airway in Obstetric Using Ilma-Fastrach®

To the Editor:
Failed intubation in obstetrics is a rare but life-threatening event. We report a case in which a pregnant patient with an unidentified difficult airway required emergent cesarean delivery for “fetal distress,” necessitating rapid sequence induction of general anesthesia. After preoxygenation and induction of anesthesia, several intubation attempts of the trachea with a Macintosh 4 blade failed. An Ilma-fastrach® was then inserted and intubation was easily performed through the device. Neither the parturient nor the neonate suffered any morbidity or mortality. On the basis of this case, we believe that ILMA-fastrach® may be a useful alternative to the methods which have been previously proposed for management of the failed intubation in obstetrics. Its ease of use does not require new training for anesthesiologists who are experienced with use of the classic LMA, and it is associated with a high success rate of blind intubation while allowing for continuous ventilation between attempts. While our experience suggests that the ILMA may have a place in the initial management of the difficult obstetric airway, further study is necessary to evaluate the potential advantages and disadvantages of this method.

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Hypertensive Encephalopathy Mimicking Postdural Puncture Headache in a Parturient Beyond the Edge of Reproductive Age

To the Editor:
Caring for the parturient beyond the edge of reproductive age is a new (and challenging) aspect of practice of many obstetricians and obstetric anesthesiologists. Although some complications (e.g., pregnancy-induced hypertension [PIH]) may occur more frequently in older mothers (1), there is no universal consensus that older age per se complicates either gestation and/or parturition (2,3).

Hypertensive encephalopathy is a syndrome consisting of headache, visual changes, seizures, and other neurological disturbances in patients with elevated systemic blood pressure. Diagnosis based on clinical and radiological findings, which are not specific, may be difficult to establish (4). Jurcic et al. (5) reported that hypertensive encephalopathy might develop gradually even when blood pressure is lower than that of malignant hypertension.

Indeed, we recently encountered a 52-year-old otherwise healthy parturient with twin gestation (assisted reproduction after oocyte donation) who required Cesarean delivery (conducted under uneventful epidural anesthesia) at 33 weeks gestation for worsening PIH. Postoperatively, the patient developed headache initially thought to resemble postdural puncture headache, however, neurological consultation and diagnostic radiological studies established the diagnosis of hypertensive encephalopathy. The most likely etiology was PIH. To our knowledge,
this is the first reported case of a hypertensive encephalopathy-related headache in the parturient.

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The Change of Difficult Intubation with Growth in a Patient with Treacher Collins Syndrome

To the Editor:

Treacher Collins syndrome (TCS) is characterized by maxillary, zygomatic, and mandible hypoplasia and known to be associated with difficult intubations (1,2). There has been no report about changes of the degree in difficulty in endotracheal intubation among growing children with TCS.

We reviewed the anesthesia records of a child with TCS who received seven consecutive operations during 15 years. The degree of difficult intubations was assessed with the following score: Grade 1, intubated within two attempts with conventional laryngoscopy; Grade 2, intubated more than two attempts with conventional laryngoscope; and Grade 3, specific equipment and/or techniques (fiber-guided intubation, special designed laryngoscope, laryngeal mask airway, etc.) required. Grade 2 and 3 were regarded as difficult intubation. Intubations were performed by experienced pediatric anesthesiologists.

Results are summarized in Table 1. The operations he performed were as follows: canthoplasty at 2 years old, exodontias at 3 years old, otoplasty at the age of 7, 8, 9, and 10 years old, and osteotomy of mandibula at the age of 17. Grade 3 difficult intubation was not noted, however, the degree of difficulty in endotracheal intubation became severe with increasing age.

Craniofacial abnormalities often affect airway management. In pediatric patients, another complicating factor is growth, that is, how abnormalities change with growth. Airway abnormalities may change or remain the same as craniofacial structures mature (3). This communication showed that intubation for a patient with TCS became difficult with growth. The TCS is frequently associated with considerable difficulty in endotracheal intubation. On the other hand, it is not true that difficult intubation is encountered in every patient with TCS. These conflicting data regarding difficult airway in TCS may be partly caused by the differences in the stage of development.

While our case report may not apply to all patients with TCS, it is very available in planning airway management, patients with TCS must require reevaluation of the airway each time.

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The SuprACLavicular Block with a Nerve Stimulator: Where Is the Needle Tip, That Is the Question

To the Editor:

We congratulate Franco et al. for performing this interesting study (1), comparing the quality of suprACLavicular block with different current outputs of the nerve stimulator, before the drug was injected. The results of their study are intriguing, since they disagree with those found by other investigators (2,3). However, the important question raised by this study cannot be adequately answered by the results of this investigation. The results of this trial are blunted, since it is impossible to know (or to guess) what would have been the minimum current still able to elicit a motor response, which would have indirectly demonstrated how close to the cord the tip of the needle was. As experienced performers of the suprACLavicular block, Franco et al. initially might have placed the stimulating needle very close to one cord, which is underlined by their statement that “it is possible that some or all of the twitches elicited at 0.9 mA could have still been present at 0.5 mA.” The authors cannot rule out that the tip of the needle in the 0.9 mA group would have been closer to the cord than in the 0.5 mA group. Without comparing the parallel between the reduction of the current and the motor response until the minimum current was found, no valid conclusion can be drawn. The conclusion of their study can lead less experienced colleagues to inject the local anesthetic too far from the nerves with consequently a high block failure rate. When dealing with output currents of nerve stimulator and needle tip-nerve distance, it is mandatory to mention the stimulus duration (2,3). Describing the relationship between current intensity needed to produce a motor twitch and the needle tip-nerve distance, the authors mentioned an “inverse proportion,” which probably is a spelling error, since it is a proportional relation.

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The Degree of Difficulty in Intubation According to Age

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<th>Age</th>
<th>Grade 1</th>
<th>Grade 2</th>
<th>Grade 3</th>
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Lipid, Not Propofol, Treats Bupivacaine Overdose

To the Editor:

Mayr et al. (1) recently reported the comparative efficacies of epinephrine, vasopressin, and a combination of the two drugs in a porcine model of bupivacaine overdose. They used a single 5 mg/kg IV bolus of bupivacaine, applied advanced cardiac life support 1 min after asystole, and administered drugs 2 min later and at 5-min intervals thereafter. Monophasic countershocks were applied as dictated by rhythm disturbance. Rates of survival were 5/7 for vasopressin, 4/7 for epinephrine, 7/7 in the combined treatment group, and 0/7 in controls.

By comparison, we reported that injecting a 20% lipid emulsion in combination with cardiac massage leads to successful return of normal hemodynamics in 9/9 dogs after a bolus injection of 10 mg/kg bupivacaine (2). Lipid infusion in 6 of these dogs was delayed for 10 min to approximate a clinical scenario. A normal rhythm was established in all 9 dogs within 5 min; no electrical counter shock was required. No control animal demonstrated return of BP or HR.

Dogs and pigs may differ in terms of susceptibility to bupivacaine cardiac toxicity; the porcine and canine models may not be completely comparable for this and other reasons. However, we and others (3) believe the rapid return of normal rhythm and hemodynamics in both dogs and rats following massive bupivacaine overdose (twice the dose used in Mayr’s study), indicates superior efficacy of lipid rescue for bupivacaine toxicity to drugs, such as epinephrine and vasopressin that are components of the generic ACLS protocol for cardiopulmonary arrest (4). Perhaps Dr. Mayr will consider comparing combined epinephrine/vasopressin with lipid rescue in the porcine model of bupivacaine cardiac toxicity.

Mayr et al. (1) also incorrectly cite us as indicating that “...a lipid infusion such as propofol increases the dose of bupivacaine required to induce cardiac arrest, and, therefore, this strategy has been suggested as a potential means to improve outcomes from such toxicity.” We have never recommended use of propofol for treating bupivacaine overdose, and strongly suspect that its use in cardiac arrest will impede resuscitation.

We have recommended treating bupivacaine-associated cardiac arrest by injecting a 1 mL/kg bolus of 20% lipid emulsion (such as Intralipid) and starting an infusion of 0.25 mL/kg/min for 10 min, while continuing basic life support (5). The bolus could be repeated every 5 min, two or three times if needed. The upper dose limit of 20% lipid emulsion is not known, but a total of more than 8 mL/kg is not likely to be needed, nor successful if lower doses are not. Note that this protocol will deliver a significant volume load (several hundred mL in an adult). The standard formulation of propofol is 10% lipid and 1% propofol. Therefore, gram quantities of propofol would accompany our recommended regimen and only half the dose of lipid, the necessary ingredient, would be delivered.

Propofol is not an acceptable treatment for bupivacaine overdose.

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References


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In Response:

We would like to thank Weinberg et al. for their interest in our work, as well as for their constructive comments. First, we sincerely apologize for having incorrectly cited Weinberg et al. by confounding propofol and intralipid; we completely agree with their statement that propofol administration cannot be recommended for managing a bupivacaine overdose. When indicating in the Discussion section that “...a lipid infusion such as propofol increases the dose of bupivacaine required to induce cardiac arrest, and therefore, this strategy has been suggested as a potential means to improve outcomes from such toxicity,” we did not suggest to use propofol for treating bupivacaine toxicity, nor that Dr. Weinberg et al. used propofol for treating bupivacaine toxicity. We share the same opinion that usage of propofol in cardiac arrest may impede resuscitation. With our statement about a “lipid infusion such as propofol...”, we only wanted to state the reason why we did not use propofol but isoflurane and nitrous oxide to maintain anesthesia in our experiment. Instead of saying “...a lipid infusion such as propofol...”, it would have been better to state “...as propofol is a lipid infusion which may increase the dose of bupivacaine required to induce cardiac arrest...” Second, beneficial lipid effects during massive bupivacaine overdose as described by Weinberg et al. resulted in impressive outcome data. However, their conclusion drawn in the letter that these results indicate the superiority of this treatment regime in comparison to advanced cardiac life support including epinephrine and vasopressin has not been proven. The comparative investigation of the epinephrine/vasopressin combination and the lipid rescue protocol in the same animal model of bupivacaine cardiac toxicity can only provide reliable information in this respect.

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In Response:

Few data are available regarding postdischarge symptoms in patients recovering from general anesthesia. Recovery from general anesthesia can resemble narcolepsy with respect to tiredness, sleepiness, and the general feeling of “being down.” As stated in the article, this was a proof of a concept study with its own limitations. Furthermore, we believe in the face validity of subjective symptoms reported by the patients. Prior to performing our study, we evaluated the face validity of the items in the questionnaire by asking a group of five patients to read each question and judge whether they had any problem understanding the meaning of the questions. While fatigue, worn-out, and exhaustion are similar, they are qualitatively different. A patient may feel fatigue and worn-out without being exhausted.

Frazer et al. question the validity of the last two sentences of our study. They question why we stated in our article “patients recovering from general anesthesia can significantly benefit from modafinil.” This sentence was supported by the previous sentences stating, “Modafinil significantly reduces the degree of fatigue, incidence of moderate to severe fatigue, and postoperative distress.” We believe that a significant reduction in the degree of fatigue, exhaustion, or feeling worn out signifies significant improvement and benefit. In addition, as has been stated in the article, content analysis of the patient’s description of recovery from general anesthesia demonstrates improved feelings of alertness and energy in those receiving modafinil. We do understand that there is much to learn about recovery from general anesthesia. We also believe that the effect of modafinil on recovery from anesthesia is measurable and that its effect on drug-induced central nervous system (CNS) depression warrants further evaluation.

The Methods section of the study states that both parametric and nonparametric statistics were used to analyze data. In addition, our recalibration shows that 11.3 (9.1) is significantly different than 21.0 (13.8); the difference between sample means = 9.7, standard error of the difference = 4.01, df = 32, t = 2.04, 95% confidence interval for the difference between means is 1.53 to 17.9.)

We have seen many patients with a history of exaggerated postoperative CNS depression; many of them require a few days to recover to their preoperative cognitive states. These patients may be the group that would benefit the most from modafinil. We understand that much more work needs to be done if modafinil is to be...
recommended for this new indication. We also understand that thorough dose-response evaluation, including different doses or multiple dosing, will be needed before such recommendations are made. We stand behind our findings, statements, and interpretations of our results.

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In Response:

Dimitriou et al. are correct in stating that we provided no information about the frequency of esophageal intubation; however, the authors incorrectly state that we provided no information about the frequency of adjusting maneuvers and esophageal intubation. We found that the ILMA was successfully inserted in all patients at the first attempt and flexible lightwand-guided intubation was successful in all patients, with an average of 1.09 attempts and 0.58 adjusting maneuvers. The frequency of esophageal intubation was 6%. There were no differences in the time taken, the number of insertion attempts, the number of adjusting maneuvers, or the number of accidental esophageal intubations between the right and left lateral and supine positions. These findings support those of Komatsu et al. (1).

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References

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Omentum Through the Vulva as First Sign of a Uterine Rupture

To the Editor:

We report a case of rare uterine rupture. Our 38-year-old patient had suffered three curettings in the past. Spontaneous vaginal delivery took place under epidural anesthesia, and after placental outcome, the omentum prolapsed through the vulva. The patient became nauseated with a heart rate of 110 bpm but was not hypotensive. Blood specimen for type screen was obtained, and a laparotomy was immediately performed after a rapid sequence induction of general anesthesia. The uterine rupture was repaired and blood transfusion was unnecessary. The tray was extubated after surgery, and the patient was transferred to the PACU and discharged from hospital 4 days later.

Uterine rupture is uncommon (1,2). Marsden (3) described a patient whose omentum prolapsed after a vaginal delivery. A risk factor could be a previous curettage. The anesthetic choice depends on maternal hemodynamics, risk of hysterectomy, and expected or real blood loss (1,4). We preferred general anesthesia, because we expected a massive blood loss. Classical signs of a uterine rupture were not observed (5). Other signs observed were vernixuria (6) and an extrusion of fetuses into the urinary bladder (7). With an in-house staff and utilizing close maternal monitoring, uterine rupture does not result in higher mortality (8).

This situation may be seen more often in future, because of the increasing number of cesarean sections.

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References

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Hyperbaric Oxygen Therapy and Pain Management in a Child with Continuous Infraclavicular Brachial Plexus Block

To the Editor:

The advantages of infraclavicular brachial plexus block (ICB) were first described by Tissot et al. (1) in children, and since then several descriptions have been published (2,3). Dadure et al. (4) first reported the feasibility and efficacy of a continuous ICB in postoperative pain in children. We have been confronted with the case of an 11-year-old traumatized patient who was jeopardized in a traffic accident in which his hand was injured. He was subjected to several orthopedic surgeries, iterative care, and hyperbaric oxygen therapy for the anaerobic bacterial infection. A few days after his arrival, he became agitated and aggressive. With the advice of a pedo-psychiatrist, clonazepam was administered as a way to calm the child, but it was not totally effective. Thus, we decided to perform continuous ICB (Fig. 1) to allow more comfort to the child and to avoid iterative general anaesthesia. Continuous ICB broke the vicious circle consisting of pain, agitation, anxiety, aggressiveness, and fear, without any adverse effects or complications, and permitted the discontinuation of all sedatives or analgesics, avoiding side effects. Thus we confirm the use of continuous ICB for pain management of the upper limb in children.

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Postoperative Analgesia and Recovery After Open and Laparoscopic Prostatectomy

To the Editor:

We compared the postoperative recovery and requirement of analgesia between open radical prostatectomy (ORP), which is the surgical technique of reference for treatment of early prostatic carcinoma, and laparoscopic radical prostatectomy (LRP), which has been recently introduced (Table 1).

A total of 174 patients had undergone radical prostatectomy over a period of 3 1⁄2 years in two hospitals. Postoperative analgesia was regularly maintained with 8 g of propacetamol daily, completed as required by patient with morphine either using patient-controlled analgesia device or by subcutaneous injections.

LRP needed a significant longer operative time than ORP but allowed a more rapid patient recovery and earlier hospital discharge. Postoperative pain, as assessed by adjuvant morphine consumption was less in LRP group, but surprisingly, this difference was not significant. Other studies had previously found a significant lower need of analgesia with LRP (1). This difference could be explained by the small transverse suprapubic incision (Pfannenstiel's incision) regularly used for ORP by our surgeons, and which had been reported to produce much less tension on the abdominal wall than vertical incisions usually used for this surgery (2).

Table 1.

<table>
<thead>
<tr>
<th></th>
<th>ORP (n = 115)</th>
<th>LRP (n = 59)</th>
<th>P*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age (yrs)</td>
<td>63.3 ± 5.7</td>
<td>64.4 ± 5.9</td>
<td>n.s.</td>
</tr>
<tr>
<td>Operative time (mn)</td>
<td>161.5 ± 37.5</td>
<td>201.4 ± 46.2</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Hospitalization stay (days)</td>
<td>10.8 ± 2.9</td>
<td>7.9 ± 3.1</td>
<td>&lt;0.001</td>
</tr>
<tr>
<td>Morphine (mg)</td>
<td>48.6 ± 54.2</td>
<td>37.7 ± 24.5</td>
<td>n.s.</td>
</tr>
<tr>
<td>Duration of morphine need (h)</td>
<td>34.4 ± 13.9</td>
<td>36.4 ± 18.2</td>
<td>n.s.</td>
</tr>
</tbody>
</table>

n.s. = not significant.
* Student’s t test.
We conclude that LRP is a comparable technique to ORP as regards postoperative need of analgesia; however, it is associated with a shorter postoperative recovery and hospital stay.

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References

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MRI of the Upper Airway and McCoy-Balloon Laryngoscopy with Left Molar Approach in a Patient with Arthrogryposis Multiplex Congenita and Previous Unsuccessful Endotracheal Intubation

To the Editor:
A patient with amyoplasia congenita (1) and previous unsuccessful endotracheal intubation with standard/McCoy laryngoscopy, intubating laryngeal mask, and fiberoptic bronchoscopy, was scheduled for scoliosis correction. Simplified airway risk index score (2) amounted to 10 (Table 1). Preoperative magnetic resonance imaging (MRI) revealed long, counterclockwise-rotated, and left-shifted epiglottis, clockwise-rotated hyoid, and short epiglottis tip-to-retrohyaryngeal wall distance (Fig. 1); anatomic abnormalities were probably explanatory of preceding intubation failure (Fig. 1, see legend). Following anesthesia-induction, a number 4 McCoy blade carrying a 7F Fogarty catheter (Fig. 2A) (3) was introduced through left mouth corner and above the left molars (4). Blade-tip was directed posteromedially (4), epiglottis was visualized, and blade-tip was advanced deep into vallecula. The handle was rotated clockwise so that its convex surface was parallel to line AT of Figure 1B. Fogarty catheter balloon inflation (Fig. 2B), forceful laryngoscope elevation, and external thyroid pressure resulted in exposure of posterior glottis commissure. A gum elastic bougie was introduced into the larynx, and a 6.5-mm-internal diameter endotracheal tube passed over it into the trachea. Conclusively, combined sagittal and transverse upper airway MRI sections may aid in difficult airway management planning. Anatomic abnormality-associated Table 1. Preoperative Airway Evaluation

<table>
<thead>
<tr>
<th>Determined variable</th>
<th>Result</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mallampati airway class</td>
<td>III</td>
</tr>
<tr>
<td>Interincisor gap (cm)</td>
<td>3.2</td>
</tr>
<tr>
<td>Thyromental distance (cm)</td>
<td>4.3</td>
</tr>
<tr>
<td>Range of neck motion (degrees)</td>
<td>&lt;80</td>
</tr>
<tr>
<td>Ability to prognath (Yes/No)</td>
<td>No</td>
</tr>
<tr>
<td>History of difficult tracheal intubation (Yes/No)</td>
<td>Yes</td>
</tr>
<tr>
<td>Simplified airway risk index score*</td>
<td>10</td>
</tr>
</tbody>
</table>

*In addition to the presented airway risk factors, the patient exhibited prominent upper incisors; * determined as in El-Ganzouri et al. (2).

Figure 1. Preoperative magnetic resonance imaging sections of the upper airway. A, sagittal section 1 cm left to midline during “quiet” inspiration. The black dot represents the root of the epiglottis and the white dot represents its tip; epiglottis length amounts to 3.23 cm. The distance between epiglottis tip and retrohyaryngeal wall (R) is approximately 3 mm. H = hyoid bone; V = vallecula epiglottica; G = glottis. B, transverse section at the level of epiglottis tip (T). The epiglottis is shifted to the left and rotated counterclockwise; the angle between line AB and upper airway transverse axis (CD) is 25.1 degrees; line AB is tangential to the edges of the epiglottis laryngeal surface. The hyoid bone is rotated clockwise. C1 = right greater cornu of hyoid; C2 = left greater cornu of the hyoid. During the preceding, failed intubation attempts (see also main text), the abnormal epiglottis-hyoid anatomic relationship was the probable cause of ineffective epiglottis lifting with standard/McCoy laryngoscopy, whereas laryngeal mask insertion could have resulted in downward-folding of the elongated epiglottis; the elongated/ left-shifted epiglottis probably resulted in inability of the endoscopist to advance a fiberscope around the epiglottis side and through the vocal cords. During the laryngoscopy reported herein, the McCoy-balloon blade was introduced into the vallecula epiglottica and then rotated clockwise so that its convex surface became approximately parallel to line AT; this maneuver along with the balloon-enhanced capability of upward hyoid/epiglottis lifting (see also Fig. 2B) should have been the major factors of the successful endotracheal intubation.
Prick Testing for Neuromuscular Blocking Drugs

To the Editor:

I was interested to read the study of Dhonneur et al. (1) describing a randomized controlled prick testing study in healthy volunteers. Their study showed a frequent incidence of positive prick tests to undiluted rocuronium and vecuronium.

In 1997, our group published a prospective comparison of prick and intradermal testing in 212 consecutive patients referred to an anesthetic allergy clinic (2). Prick testing was performed with undiluted drugs with the exception of morphine, and a positive prick test was recorded if a wheal of greater than 0.4 cm occurred.

The results have been revisited. A total of 157 patients were tested by prick and intradermal testing with 1:1000 dilutions of vecuronium 4mg/mL and undiluted vecuronium 4mg/mL, respectively (Table 1 on page 1881).

There were nine positive prick tests to vecuronium. All were associated with positive intradermal tests. The patients who showed the positive tests all had anaphylactic reactions: three to vecuronium, two to pancuronium, two to alcuronium, one to succinylcholine, and one to atracurium. In seven of these reactions the culprit drug was confirmed by radioimmunoassay for muscle relaxant-specific IgE. Cross-sensitivity between neuromuscular blocking drugs is well documented (3–5).

There were no false positive prick tests to undiluted vecuronium in contrast to the French study (1). These results are surprising and could be related to operator or patient factors. We have no data for rocuronium, and unfortunately the study was performed prior to the availability of mast cell tryptase assays, which improve the ability to detect anaphylaxis as a mechanism (6). We note that a Scandinavian study found no positive prick tests to undiluted vecuronium in 30 volunteers in 1980 (7).

Dhonneur et al. (1) suggest their findings call into doubt whether neuromuscular blocking drugs are the principal cause of anaphylaxis during anesthesia. I believe this statement is not supported by the evidence from large studies that include radioimmunoassay, intradermal testing and mast cell tryptase results as part of the diagnosis (8,9). I note that the Danish study cited (10) uses undiluted and 1:10 dilutions of vecuronium and rocuronium in its exemplary testing protocol; if these concentrations were inappropriate, the investigators should also have found a high incidence of reactions to these drugs. That they do not is because their population is different.

The major weakness of skin testing after anesthetic reactions is that it only explores one mechanism. To validate the results properly would require provocation or challenge, which is difficult to justify when the results of subsequent anesthesia based on available tests show infrequent incidence of second reactions (11).

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References

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In Response:
It is a great honor being criticized by such an expert in the area of allergy in anesthesia. I understand the reactions of major Australian and French specialists in this area to our results published recently in Anesthesia & Analgesia (1). Indeed, our results demonstrate that with our present level of understanding, the current practice in skin
testing is invalid, and we cannot formally rely on prick tests to confirm allergy to neuromuscular blocking agents (NMBAs).

I am not sure that the results of our French study are conflicting but rather complete the interesting study performed by Levy et al. (2), which has clearly demonstrated that intradermal 10^{-10} \text{M} of most NMBAs promoted degranulation-free skin reactions, suggesting that “intradermal nonreactive” NMBA concentration is equal or smaller than 10^{-6} \text{M}. We have shown that a 100-fold increase of this “intradermal nonreactive concentration” corresponded to prick nonreactive concentration. Our observation is coherent with the hypothesis that skin response to NMBAs is mainly the result of a direct effect upon cutaneous vasculature.

I agree that the results of our French study contrast with those obtained by Professor Fischer. However, both studies are not comparable in either their design or the studied population. We have performed a randomized controlled prick testing study in young healthy anesthesia-naive adult volunteers comparing bioequivalence of rocuronium and vecuronium in terms of skin sensitivity. The Australian study compared prick and intradermal testing in patients suspected to be allergic to vecuronium after an anesthetic reaction (3). Although not comparable in terms of methodology and populations, I believe that operator factors cannot explain the frequent incidence of positive prick tests to undiluted rocuronium and vecuronium we demonstrated. In order to limit this risk factor, our study was performed in the dermatology department of the most important French CRO, a single specialized physician administered all 300 prick tests, wheal and flare measurements were performed by an independent technician, and source data have been revisited by two investigators without any major discordance. We have built and performed a methodologically strong study, and our results question the reliability of prick testing with undiluted solution of steroid-derived relaxants for the diagnosis of allergy.

I am not an expert in epidemiology or evidence-based medicine, but I believe that any diagnostic test, like skin testing, should be evaluated in term of specificity and sensitivity before being exported to clinical practice. In other words, a large cohort of healthy volunteers from several countries and of different skin colors should be tested in order to determine skin sensitivity to NMBAs. Unfortunately, in the absence of major studies for better applications of skin testing and validation of other diagnostic approaches of allergy to anesthetic agents, it is not clear that we are making the correct diagnosis of allergy to rocuronium and vecuronium using prick responses to undiluted stock solutions. I believe that a rate of 40–50% false positive is unacceptable for a diagnostic test, but a 10% incidence of false positive would have been also probably not acceptable. Under these circumstances, unexplained reactions during anesthesia question whether these steroid-derived relaxants are the principal cause of anaphylaxis during anesthesia.

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References

Table 1. Positive Intradermal and Prick Tests to Vecuronium in 157 Patients Referred to an Australian Anaesthetic Allergy Clinic

<table>
<thead>
<tr>
<th>Cause of reaction</th>
<th>Number</th>
<th>Vecuronium intradermal positive</th>
<th>Vecuronium prick positive</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alcuronium</td>
<td>17</td>
<td>2</td>
<td>2</td>
</tr>
<tr>
<td>Alcuronium/succinylcholine</td>
<td>1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Ampicillin</td>
<td>1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Atracurium</td>
<td>19</td>
<td>1</td>
<td>1</td>
</tr>
<tr>
<td>Atropine</td>
<td>1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Cephalothin</td>
<td>1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Cetrimide</td>
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<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Dextran</td>
<td>1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>d-Tubocurarine</td>
<td>3</td>
<td>0</td>
<td>0</td>
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<tr>
<td>Flucloxacillin</td>
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</tr>
<tr>
<td>Gallamine</td>
<td>1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Methohexitone</td>
<td>1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Morphine</td>
<td>1</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Pan</td>
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<td>2</td>
<td>2</td>
</tr>
<tr>
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<tr>
<td>Platelets</td>
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<td>Pro</td>
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<td>Thiopentone</td>
<td>13</td>
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<td>Thiopentone/alcuronium</td>
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<td>Thiopentone succinylcholine</td>
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<tr>
<td>Trimethoprim/sulfamethoxazole</td>
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<tr>
<td>Vecuronium</td>
<td>3</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Not anaphylaxis/preoperative</td>
<td>42</td>
<td>1</td>
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</tr>
<tr>
<td>Cause not determined</td>
<td>6</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>157</strong></td>
<td><strong>11</strong></td>
<td><strong>9</strong></td>
</tr>
</tbody>
</table>
Should CO₂ Laser Jet Ventilation Be Abandoned?

To the Editor:

I read with interest the recent case report by Leemann et al. on the use of a Teflon catheter for high-frequency jet ventilation during carbon dioxide (CO₂) laser laryngeal surgery employing a jetting pressure of 2.3 atm (1). The authors noted the occurrence of subcutaneous emphysema to the neck and thorax, bilateral pneumothoraces, and a mediastinal shift to the left. The case was terminated before the tumor was completely removed, but the patient underwent a second procedure using a jetting pressure of 0.5 atm for removal of the residual tumor on the next day. However, the subcutaneous emphysema recurred after only a few seconds. Surgery was then successfully continued employing a Mallinckrodt Laser-Flex™ endotracheal tube. It was later determined that the Teflon catheter had been damaged by the laser.

Several points are in order in a discussion of this case:

1. The original pressure employed for the jet ventilation was 2.3 atm. This pressure is equivalent to 2,367 cm of water and can easily cause the kinds of problems encountered in the case (2).
2. Teflon was already known to be vulnerable to the CO₂. For example, the Teflon wrapping on the Xomed Laser Shield II endotracheal tube was shown to be rapidly vaporized by this laser (3).
3. The occurrence of the kinds of barotrauma noted after the first operation indicates the occurrence of a mucosal rent or passageway for the jetting pressure. Thus, once these complications have occurred, the technique should not be used again on the next day.
4. Cases of this type can be handled successfully employing laser-resistant endotracheal tubes, thus avoiding the severe risks of high-frequency jet ventilation.

Mitchel B. Sosis, MD, PhD

References

In Response:

We would like to thank Dr. Sosis for his interest in our case report. The operating pressure for high-frequency jet ventilation through a transtracheal catheter is between 2.8 to 4.0 atm (2800 – 4000 cm H₂O, respectively) in order to overcome the high resistance of the 13-gauge catheter. To prevent barotraumas under these conditions the pressure in the trachea is automatically controlled and our ventilator stops, if pressure is higher than 50 cm H₂O. Even though we have been using laser resistant tubes over more than 10 years (1), our surgeons prefer percutaneous transtracheal catheters because they give perfect surgical conditions (2,3). Retrospectively, it can surely be discussed whether the primary use of a laser resistant tube for the second operation would have been the better choice.

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References