Which Troponin I Peak Concentration?

To the Editor.—We read with interest the article of Conzen et al., demonstrating a reduction of cardiac troponin I release after coronary artery bypass grafting on the beating heart when sevoflurane was compared to propofol. Most previous studies identified peak postoperative troponin I as the major endpoint. It is not clear from the article of Conzen et al. which was the peak postoperative troponin I value in the two groups and whether this value was significantly different.

Rajesh Mahajan, M.D.,* Rahul Gupta, M.B.B.S. * Acharya Shri Chander College of Medical Sciences, Jammu, India.
drmahajan@yahoo.com, drmahajan@rediffmail.com

References


Cerebrospinal Fluid Physiology and Cerebrospinal Fluid Drainage

To the Editor.—It was interesting to read the case report by Fleck et al. suggesting an appropriate duration of cerebrospinal fluid drainage after thoracoabdominal aneurysm repair. However, toward the end of their discussion, the authors mention that draining 1,700 ml cerebrospinal fluid over 24 h is seven times greater than the normal 250 ml cerebrospinal fluid that is produced in the same period. Although it may be a minor point, cerebrospinal fluid is actually formed at the rate of 500–550 ml/24 h and not 250 ml, as mentioned by the authors.2–5

Rajesh Mahajan, M.D.,* Rahul Gupta, M.B.B.S. * Acharya Shri Chander College of Medical Sciences, Jammu, India.
drmahajan@yahoo.com, drmahajan@rediffmail.com

References


To the Editor.—We appreciate the interest of Drs. Zangrillo, Landoni, and Putzu in our article. The authors state that we did no statistical analysis on the peak troponin I values found in the patients of our study, whereas other authors based their conclusions on such data.

The main reason for us to refrain from comparing “peak” troponin I was because of the discontinuous character of this parameter, hence the time intervals that elapsed until the next blood sample was collected. All parameters that cannot be determined continuously have an immanent risk that the true peak may be missed. In the early postoperative period of our study, blood samples were obtained every 3 h. Thereafter, even longer intervals were allowed. Considering these intervals, we cannot exclude the possibility that the true troponin I peak value in our patients was missed. This is supported by the finding that the individual maxima were obtained at different measuring points, and that would render a direct comparison problematic. We therefore compared concentrations found at the specific measuring point as well as the changes over time. This was accomplished by two-way analysis of variance.

Peter F. Conzen, M.D., * Susanne Fischer, M.D. * Ludwig-Maximilians-University, Munich, Germany. peter.conzen@med.uni-muenchen.de

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(Accepted for publication February 6, 2004.)
In Reply—We thank Drs. Mahajan and Gupta for their comment. Our data were based on a neuroanatomy text written by the head of the anatomy department of the Medical University of Vienna.1 However, we wish to stress that the main purpose of our case report is to discuss the clinical value of a longer period of cerebrospinal fluid drainage. The details of normal cerebrospinal fluid physiology are beyond the scope of our article.

Tatiana Fleck, M.D.,* Martin Grabenwoger, M.D. * Medical University of Vienna, Vienna, Austria. t9204604@hotmail.com

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Anesthesiology 2004; 100:1621

Using the Visual Analog Scale

To the Editor—I read with interest the report of Woehlck et al.1 on the use of acetazolamide in the treatment of referred pain after laparoscopic surgery. To assess postoperative pain, the authors report using a visual analog scale but state that some patients were unable to obtain such a verbal response at the necessary time, which was often immediately after anesthesia and surgery. The potential difficulty in accurately pointing to or physically marking the intended number when postsurgical pain and residual anesthetic effects were present may have made this maneuver impractical.

The rationale for accepting a positive response when a patient could not respond with a number, either verbally or by pointing, is logical because some patients were in too much pain to care about the study while just emerging from anesthesia or were still too groggy to give anything except a positive response regarding the presence of pain. For this reason, we performed a sensitivity analysis substituting midrange (5), high (7), or low (3) values for positive responses, and recalculated the statistical analysis. For each of these values, the acetazolamide group had lower initial referred pain scores than the placebo group, demonstrating that the actual value of the positive responses was not important.

The question of how to statistically handle the VAS in a general sense, i.e., whether to treat it as an ordinal value or a continuous variable, is important. Fortunately, this question of whether VAS scores can be evaluated using parametric statistical analyses, such as a t test or analysis of variance test, or should only be subjected to nonparametric analyses, such as the Wilcoxon rank sum test (nonparametric two sample t test) or Kruskal-Wallis test (nonparametric analysis of variance), has already been studied. Parametric tests compare means of data, and these data should ideally be normally distributed. The methods of analysis have been controversial, and early manuscripts from the 1970s and 1980s emphasized using nonparametric tests to avoid erroneous claims of difference when none existed.2 However, more recently, Dexter and Chestnut3 determined that the t or analysis of variance tests had slightly greater power than nonparametric tests to detect differences between groups and are good choices to compare VAS scores. Using actual data and computer simulations, Dexter and Chestnut further demonstrated that parametric analysis of as few as five categories of VAS data had power similar to that of a continuous VAS, although the use of fewer than five categories had reduced power.

**Table 1. Initial Referred Pain Sensitivity Analysis and Statistical Significance with Inclusion of Nonparametric Analyses**

<table>
<thead>
<tr>
<th>Initial Referred Pain Sensitivity Analysis</th>
<th>Acetazolamide Group</th>
<th>Placebo Group</th>
<th>P Value (t Test)</th>
<th>P Value (Nonparametric)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Replaced by 3</td>
<td>0.78 ± 1.55</td>
<td>3.10 ± 3.42</td>
<td>0.012</td>
<td>0.021</td>
</tr>
<tr>
<td>Replaced by 5</td>
<td>1.00 ± 1.94</td>
<td>3.40 ± 3.48</td>
<td>0.014</td>
<td>0.020</td>
</tr>
<tr>
<td>Replaced by 7</td>
<td>1.22 ± 2.52</td>
<td>3.70 ± 3.70</td>
<td>0.022</td>
<td>0.020</td>
</tr>
</tbody>
</table>

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Anesthesiology, V 100, No 6, Jun 2004

References

statistical power to identify a real difference. However, if large numbers of patients (> 16%) cluster at the extremes of the data range, nonparametric analyses may be less prone to error. 3

Applying this general question to our study, 4 45% (placebo group) and 78% (acetazolamide group) of patients had no referred pain at the initial postoperative measurement, and thus had a VAS of 0, representing no pain and hence a true, absolute zero. The VAS extended to 10, producing 11 categories, which exceeds the minimum of five found to be necessary by Dexter and Chestnut. 2 In our acetazolamide study, the data clustered around the lower extreme but not both extremes, and the SDs were small. In addition to the t tests, we simultaneously performed a nonparametric analysis, and both methodologies had similar P values. Space limitations encouraged us to neither publish nor describe the details of the nonparametric analysis because the results were redundant. Table 1 in this letter is a revision of table 2 from the original study. It includes the P values derived nonparametrically for comparison with the parametric results originally published. 4

Harvey J. Woehlick, M.D.,* Mary Otterson, M.D., Hyun Yun, Ph.D., Lois A. Connolly, M.D., Daniel Eastwood, M.S. *Medical College of Wisconsin, Milwaukee, Wisconsin. hwoehlick@mciw.edu

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(Accepted for publication February 9, 2004.)

Increased Margin of Safety of Morphone-6-glucuronide Relative to Morphine

To the Editor:—With interest we read the Editorial View by Dr. Jeffrey Gross in the October 2003 issue of ANESTHESIOLOGY 1 commenting on our article in that same issue. 2 In this article, we estimated an effect site (i.e., brain) concentration of morphine-6-glucuronide (M6G) to cause 25% reduction in breathing during hypercapnic and hypoxic stimulation (C25) of 530 and 870 nM, respectively. Dr. Gross compares these values to plasma concentrations of 400 nM from a study by Lötsch et al. 3 at which no analgesia was perceived. We believe that these numbers are incomparable. Our C25 values are the estimated brain concentrations, whereas the value of Lötsch et al. is a plasma concentration. Taking into account a blood effect site equilibration half-life of approximately 6 h for M6G, 4, 5 we calculated that the brain M6G concentration in the study of Lötsch et al. was maximally 144 nM, which is a factor of 4–6 less than our values. For morphine, the corresponding C25 values estimated by us were 30 and 20 nM for hypercapnic and hypoxic breathing, respectively. 3

In fact, in a subsequent study in which we tested the analgesic properties of M6G, we observed that a steady state or brain M6G concentration (C25) of 275 nM is needed for a 25% increase in electrical current (pain tolerance). 5 The corresponding C25 value of morphine is 30–30 nM. 6 This indicates that much greater M6G concentrations are needed to suppress respiration relative to the values needed to induce analgesia greater than placebo. 7 In sharp contrast, for morphine, these values are of the same order of magnitude. Evidently, how our C25 and C25 values derived from an electrical pain model reflect concentrations needed for postoperative pain relief necessitates further study.

Finally, we were surprised to find the most important statement in the Editorial View in a footnote. In that footnote, Dr. Gross indicates the need for a respiratory model that adequately describes apnea at relatively high opioid concentrations. In contrast to the other model described in the review, the model used by us (the power or Leiden model) is able to predict apnea at realistic opioid concentrations. For example, the Leiden model indicates that hypercapnic breathing is abolished at a morphine brain concentration of approximately 100 nM in a volunteer without pain or stress. This is a very realistic value.

Raymonda Romberg, M.D., Erik Olofsen, M.Sc., Elise Sarton, M.D., Ph.D., Luc Teppema, Ph.D., Albert Dahan, M.D., Ph.D. * Leiden University Medical Center, Leiden, The Netherlands. adahan@anesthesia.nl

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(Accepted for publication February 10, 2004.)
Interdental Distance and Direct Laryngoscopy

To the Editor:—I read with interest the article by Calder et al.1 showing that limitation of craniocervical extension to less than 36° limits interdental distance. They propose this as another possible mechanism for difficult airway management. However, the relation between interdental distance and difficult direct laryngoscopy is uncertain. Charters2 has shown in a mathematical model that extreme mouth opening actually inhibits direct laryngoscopy. Clinical experience suggests that extreme mouth opening is not practiced as part of laryngoscopy technique. Previous studies showing a correlation between interdental distance and difficult laryngoscopy have included edentulous patients in the measurements,3,4 or they have not specified whether edentulous patients were included.5–7 Not having maxillary teeth (which undoubtedly improves the line of vision to the glottis) is an entity different from interdental distance per se.

Leonard Lee, M.B.B.S.
Royal Melbourne Hospital, Melbourne, Australia. leonard.lee@mh.org.au

References

Mouth Opening, Craniocervical Extension, and Laryngoscopic Positioning

To the Editor:—I read with great interest the study of mouth opening and craniocervical extension by Calder et al.1 Although the authors were circumspect in their interpretation, the relation between extension and mouth opening may provide an important basis for laryngoscopic positioning. Adnet et al.5–6 demonstrated that for routine intubation, the ‘sniffing position’ does not confer any advantage over simple head extension. Put another way (again, for routine intubation), the essential advantage of the sniffing position over the neutral position results from head extension. Other studies of Adnet et al.5–6 have shown that neither the sniffing position nor simple extension result in the alignment of the oral, laryngeal, and pharyngeal axes. Collectively, these data resulted in a fundamental question: If extension is the essential advantage of the sniffing position over neutrality and extension does not align the oral, laryngeal, and pharyngeal axes, wherein lies its value? The study of Calder et al. makes an important step toward an answer: The value of extension may be that it facilitates mouth opening rather than axis alignment.

It has clearly been shown that limitations of mouth opening are associated with difficult intubation.5–6 The study by Karkouti et al.6 demonstrates that poor mouth opening and craniocervical extension are predictive of difficult intubation. These two “independent” variables have now been shown to be linked, suggesting that limitation of interdental distance is the ultimate disadvantage imposed by impaired extension. It is of interest to consider whether other predictors of difficult intubation, such as micrognathia or decreased thyromental distance, are also related to mouth opening or impaired extension. It is of further interest to consider the Mallampati classification as another measure of mouth opening. Mouth opening is expressed as interdental distance in the study of Calder et al., whereas with the Mallampati classification, it can be expressed as palatoglossal distance. Stated...
another way, interdental distance reflects true mouth opening, while palatoglossal distance reflects effective mouth opening, i.e., the actual volume of free space achievable in the oral cavity independent of the interdental distance. When either interdental or palatoglossal distance are limited by conditions such a decreased cervical mobility, micrognathia, or hyperglossia, intubation is rendered more difficult.

The study of Calder et al., in conjunction with the studies cited above, suggests that cranio cervic oreal extension is a crucial aspect of laryngoscopic positioning and that its value lies in optimizing the degree of interdental distance. As the authors suggest, the next step is to investigate this in the setting of true laryngoscopic positioning, evaluating the maximal gape-facilitating angle of extension in anesthetized patients whose oral airways are actively being manipulated. I would further suggest that mouth opening—delineated as true (interdental distance) and effective (palatoglossal distance)—may emerge as the most important measure of difficult intubation and that its maximization may form the basis of rational laryngoscopic positioning.

George A. Mashour, M.D., Ph.D.
Harvard Medical School, Massachusetts General Hospital, Boston, Massachusetts.
gmashour@partners.org

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In Reply.—We thank Drs. Lee and Mashour for their interest in our article. We established that there is a link between mouth opening and cranio cervic oreal position in conscious volunteers. When the subjects were prevented from extending from the neutral position, we found that the upper 95% confidence limit of interdental distance was 37 mm, which is the same as the lower limit of normal interdental distance in young people.1

The motions of the temporomandibular joint include depression, elevation, protrusion, retraction, and side-to-side grinding movements.2 From the airway management point of view, although almost infinite protrusion should be advantageous, we agree with Dr. Lee that excessive depression can be inimical to our interests.

Dr. Mashour mentions the Mallampati examination. It was the unexpected success of the Mallampati examination in patients with cervical spine disease7 that led us to perform our investigation. As far as we know, the positive predictive value of the Mallampati examination has only exceeded 50% (the same as tossing a coin) in our study3 and Dr. Mallampati’s original series.1 Unfortunately, the Mallampati examination has not proved to be a useful predictor of difficult direct laryngoscopy in any other patient population.5

Our findings may not be reproduced in anesthetized patients. Nevertheless, we suspect that one of the reasons that the sniffing position is popular for direct laryngoscopy5 is that it produces cranio cervic oreal extension and facilitates mouth opening.

Mouth opening is a complex phenomenon, and we have complemented its analysis because the cranio cervic oreal junction must now be included in the list of factors involved. We hope that some of those involved in airway management or oropharyngeal surgery will find it useful to know that active mouth opening can be facilitated by cranio cervic oreal extension and impeded by flexion.


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Patient Positioning and Ultrasound Guidance Are Important in Bilateral Cannulation of Internal Jugular Veins

To the Editor.—We congratulate Stocchetti et al.1 on their interesting case report and the favorable outcome of the patient. We agree with the authors that bilateral cannulation of internal jugular veins may worsen intracranial hypertension. Nevertheless, we wish to make some comments:

For internal jugular vein cannulation, their patient’s head and torso were placed in a flat position, and the head was even slightly rotated. All these procedures are known to increase intracranial pressure in patients with reduced intracranial compliance.2 In the context of increased intracranial pressure, we wonder why the internal jugular vein route was preferred for the insertion of the 8-French introducer and Swan-Ganz catheter to the subclavian or external jugular vein route.

In our opinion, Doppler or B-mode ultrasound guidance should always be used in these patients. Recently, we were able to demonstrate the safe cannulation even of the internal jugular veins in patients...

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with reduced intracranial compliance under Doppler guidance without the need for change in body position.³

Following the arguments of Stocchetti et al. that the introducer itself was responsible for the obstructed cerebral venous drainage, a B-mode ultrasound image of the left internal jugular vein should have shown a small-sized vessel. With regard to the high thrombogenicity of large-bore catheters in small-sized veins, another access route would have been preferred.³ In this context, we refer to a fatal incident of intracranial hypertension we encountered after tracheostomy.⁵ During neck extension, the patient experienced brain herniation due to obstruction of the accessory brain drainage pathway (vertebral venous plexus) in the context of unrecognized bilateral internal jugular vein obstruction (left: thrombosis after central venous cannulation; right: hypoplastic vein).

Blood drains from the brain by two major routes: the internal jugular veins and the vertebral venous plexus.⁶,⁷ Valdueza et al.⁵ have shown that predominance of the jugular veins in cerebrovenous drainage is limited to the supine position. When outflow through the internal jugular veins is compromised, the vertebral system becomes the major channel for blood leaving the cranium.⁶

Various clinical implications, such as bilateral neck dissection, or metastatic spread of tumors, including the controversies about head positioning in increased intracranial pressure, underline the importance of a postural influence on cerebrovenous drainage.⁸

In patients at risk, such as patients with reduced intracranial compliance, central venous access procedures should be performed under ultrasound guidance.³

Wolfram Schumunner, M.D.,* Claudia Schumunner, M.D.* Friedrich-Schiller University of Jena, Jena, Germany. cws.m.schumanner@gmx.de

References


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To the Editor.—We read with interest the case report by Stocchetti et al.¹ entitled “Bilateral Cannulation of Internal Jugular Veins May Worsen Intracranial Hypertension.” We agree with the authors’ hypothesis that bilateral cannulation of the internal jugular veins with large-bore catheters in patients with increased intracranial pressure (ICP) and limited buffering capacity of the intracranial system may cause an additional increase in the intracranial volume and hence ICP. We disagree, however, that their case report supports this hypothesis.

The authors describe a head-injured trauma patient with increased ICP to 30 mmHg. A 16-gauge catheter had been placed retrograde in the bulb of the right internal jugular vein. After placement of an 8-French introducer into the left internal jugular vein, a pulmonary artery (PA) catheter of unspecified size was inserted. With the patient supine, the ICP increased to more than 50 mmHg. After removal of the introducer, leaving the PA catheter in place, the ICP suddenly decreased to previous levels.

Although an increase in resistance of the jugular vein may theoretically result in an increase in ICP, we disagree with the authors that their case report illustrates this concept. To unequivocally prove this, the authors should have measured jugular venous pressure proximal to the insertion site both before and after the insertion of the catheters and shown a significant increase in jugular venous pressure after their insertion. Only then could they relate the increase in ICP to the obstruction of blood flow secondary to an increase in jugular venous resistance due to the presence of the catheter.

The authors believe the cause of the increase in ICP after insertion of the 8-French introducer (and a PA catheter of unspecified size) and the subsequent reduction of ICP after removal of the introducer (leaving the PA catheter in place) was solely related to the large size of the introducer. An increase in resistance in a blood vessel after the insertion of a catheter is directly related to the degree of reduction of the axial cross-sectional area of the blood vessel (due to the presence of the catheter) and the length of catheter in the blood vessel. Seven French is the smallest size of PA catheter available in adults, which is not significantly smaller than an 8-French introducer. The axial cross-sectional area of an 8-French introducer is approximately 5.09 mm² versus 3.90 mm² for a 7-French PA catheter, a difference of 1.19 mm². Furthermore, knowing that increasing the length of a catheter in a blood vessel results in increasing resistance to blood flow and considering the greater length of the PA catheter inside the vessel as compared with the introducer, the difference on the effect of increased resistance between the introducer and PA catheter becomes even more insignificant. Therefore, the authors should not have observed such a significant reduction in ICP after removal of the introducer while leaving the PA catheter in place.

The authors observed an increase in ICP to greater than 50 mmHg, which is approximately twice the value noted before the insertion of the introducer (fig. 1 in their published article).

Because cerebral blood flow remained constant according to the authors, the only remaining variable that could possibly explain the doubling of the ICP is a 50% reduction in the cross-sectional area of the internal jugular vein due to insertion of an 8-French introducer. Comparing the axial cross-sectional area of the left internal jugular vein (114.93 mm²)² and that of the introducer (5.09 mm²), one may expect an approximate 4.42% increase in resistance, and hence ICP, compared with baseline values when the introducer is placed into the vein. Considering bilateral drainage of cerebral blood, one would expect an even smaller increase in resistance, approximately half or 2.21%. The authors state, "Leaving the Swan-Ganz catheter in place, we withdrew the introducer from the vein: the ICP suddenly decreased to previous values (fig. 1)." However, their figure 1 actually illustrates a reduction in the ICP after removal of the introducer to levels even lower than the baseline values of 20–30 mmHg into the range of less than 10 mmHg. There has been an inexplicable “overshooting” of the...
decrease in ICP greater than that which should have occurred by merely removing the introducer. Therefore, removal of the introducer cannot be the explanation for their observations. We believe their explanation is erroneous in that it falls short of providing adequate justification for the observed phenomena.

Leila Khorasani, M.D.,* Kenneth D. Candido, M.D., Ahmed H. Ghaleb, M.D. * University of Washington, Seattle, Washington. Lkhorasai@u.washington.edu

In Reply:—We are grateful to both correspondents for their stimulating comments. The experience of Drs. Schummer with ultrasound is highly informative and potentially useful for preventing both short-term and long-term complications of jugular cannulation. We agree with them that a previous estimation of the left internal jugular vein (IJV) size in our case could have been extremely useful and that alternative venous accesses should have been used.

The calculations given by Khorasani et al. are detailed and intriguing but based on questionable assumptions and partially incorrect. Briefly, they assumed a caliber of the left IJV, calculated a reduction in its caliber, extrapolated a small reduction in flow, and concluded that this reduction could not explain the observed massive intracranial pressure (ICP) increase.1 The caliber of the left IJV in our patients is unknown. It has been reported that the caliber of the IJV is highly variable and that there is asymmetry between the two sides. In fact, the IJV diameter may range from 2.5 to 12 mm.2 3 The assumed left IJV cross-sectional area is based on an article4 in which the authors acknowledged as a limitation of the study the inaccuracy of the method used to estimate the jugular vein diameter. We would be reluctant to apply data from that article to this specific patient. Khorasani et al. calculated the reduction of the left IJV caliber due to cannulation as the difference between cross-sectional areas, which is appropriate for perfectly coaxial tubes, without considering that the catheter enters into the vessel with a slope. The vein wall is thin and extremely deformable; therefore, the geometry at the level of cannulation could have been profoundly distorted.

However, even accepting these assumptions, Khorasani et al. did not consider that the resistance to flow is inversely proportional to the forth power of the radius, if laminar flow is likely to occur. Therefore, in their simplified model, the reduction of the left IJV radius caused by the introducer would cause an 11% resistance increase, rather than a 4.4% increase.

Finally, Khorasani et al. state that a 50% reduction in the cross-sectional area is required for almost doubling ICP. This assumption is definitely wrong. Intracranial pressure is not linearly related to cerebral blood flow because it depends on intracranial volumes, including cerebral blood volume, and intracranial elastance. When intracranial elastance is critically reduced, an increase in cerebral blood volume as small as 2.8 ml may increase ICP from 30 to 50 mmHg (assuming a pressure volume index of 13 ml, which is the critical value reported by Marmarou et al.). Therefore, a small change in venous outflow can cause a sharp ICP change if cerebral blood volume changes even slightly in the context of exhausted intracranial compensatory mechanisms.

The response of the intracranial vasculature to an abrupt ICP increase is complex and usually implies a marked vasodilatation to keep cerebral blood flow constant despite the reduced cerebral perfusion pressure. The opposite happens when the venous outflow improves, because an increase in cerebral perfusion pressure could induce vasoconstriction, which may explain the “overshooting” of ICP after removing the introducer.5 We cannot prove this assumption in our clinical case because we did not measure blood flow and intracranial elastance; therefore, we propose our interpretation of the facts as probable rather than proven, as we specified in our case report.

We believe that increased resistance to blood flow in the IJVs due to vascular communications remains a potential cause of sudden and deleterious ICP increase.

Nino Stocchetti, M.D.,* Luca Longhi, M.D., Valerio Valeriani, M.D. * University of Milano, Ospedale Maggiore Policlinico IRCCS, Milano, Italy. stocchettipoliclinico.mi.it

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(Accepted for publication February 10, 2004.)

Do We Need a Low Dose of Succinylcholine to Facilitate Intubation in Teaching Hospitals?

To the Editor:—We read with interest the article by Naguib et al.1 in which the authors recommended using 0.5–0.6 mg/kg succinylcholine to facilitate tracheal intubation in a rapid-sequence induction. In their study, all of patients were intubated successfully; however, the authors did not provide data regarding whether all patients were successfully intubated on the first try or some patients required two or more tries.

We work in a teaching hospital, and half of the employees in our department are residents and trainees. Routinely, these young anesthesiologists have at most two tries; if they fail, the attending anesthesiologists perform the intubation, when at least 2–3 min has passed since succinylcholine was injected. Although recovery of adductor pollicis from a dose of 0.5–0.6 mg/kg succinylcholine (T1 to 10%) occurs at

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Correspondence

Should the Intubating Dose of Succinylcholine Be Revisited?

To the Editor—We read with interest the article of Naguib et al.1 and the Editorial View of Donati2 regarding optimal succinylcholine dose used for intubation. In their article, Naguib et al. conclude that the recommended dose of 1 mg/kg succinylcholine for intubation at 60 s may be excessive and that reducing the dose to 0.56 mg/kg would provide some intubating conditions in 95% of patients with normal airway anatomy and allow a more rapid return of spontaneous respiration.

In our opinion, patients with normal airway anatomy, as those included in the study of Naguib et al., are not the challenging cases to anesthesiologists. They can easily be intubated with a reduced dose of succinylcholine, and it is indifferent whether return of spontaneous respiration in these patients is rapid or slow. However, patients who are muscular, are obese, or have abnormal airway anatomy and parturients undergoing cesarean delivery present a challenge to anesthesiologists during intubation. Profound and intense muscular relaxation is necessary to obtain an optimal/best attempt at intubation in these patients, and we question whether this can be achieved using a reduced dose of 0.56 mg/kg succinylcholine. Finally, if a difficult intubation is anticipated before initiation of general anesthesia, we would be prone to follow the American Society of Anesthesiologists algorithm for difficult intubation rather than using a reduced dose of succinylcholine with a presumption that it would allow a more rapid return of spontaneous respiration in case of failure to intubate.

Zafer Salim Tabboush, M.D. Kingdom Hospital, Riyadh, Saudi Arabia. zafer@cyberia.net.lb

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(accepted for publication February 20, 2004)

What Is the Correct Dose of Succinylcholine?

To the Editor—Although we find the two studies by Naguib et al.1 and Kopman et al.2 to be theoretically interesting, we believe that it is dangerous to link them with our everyday anesthetic practice.

The data sheet for Scoline (Evans Medical, Surrey, United Kingdom),3 which used to be one of our available preparations of succinylcholine in the United Kingdom, states in its precautions that “Scoline must never be given when there is doubt about the ability to ventilate the lungs.” In keeping with this, it has never been our practice to rely on the short duration of action of succinylcholine as a “get-out clause” in the event of a failed rapid sequence intubation.

The wide variability in a patient’s breakdown of succinylcholine is well known, and surely both of these studies show that using a lower dose of succinylcholine makes an intubation attempt more likely to fail. There may then still be the problem of prolonged paralysis.

Because a successful intubation is of paramount importance in this group of patients, we want to have the best intubation conditions. Therefore, on the basis of these studies, we believe it would be more clinically relevant to have studied the effects on the intubating conditions of using a higher dose of succinylcholine than the standard dose of 1.0 mg/kg rather than studying lower doses.

Mark Messent, M.D., F.R.C.A.,* Michael S. Lim, F.R.C.A. * The London Chest Hospital, London, United Kingdom. mark.messent@bartsandthelondon.nhs.uk

References


(accepted for publication February 20, 2004)
Optimal Intubating Dose of Succinylcholine

To the Editor—Regarding the optimal intubating dose of succinylcholine, 1,2 I vividly recall the late Francis Foldes, M.D., then Chairman of the Department of Anesthesiology at Montefiore Medical Center, Bronx, New York, exhorting his residents to not dare use any more than 0.6 mg/kg for tracheal intubation. Furthermore, he taught us to administer the drug over 30–60 s, thus virtually eliminating fasciculations and myalgia, a benefit that neither Naguib et al. nor Kopman et al. mentioned.

In their seminal reports 50 yr ago, Foldes3 and Hampton4 point out that the main advantage of succinylcholine is that it provides brief intense relaxation for intubation thus permitting the anesthesiologist to use smaller amounts of nondepolarizing muscle relaxants as dictated by the nature of the case. In the past decade, with the proliferation of newer short-acting nondepolarizing muscle relaxants and inexpensive train-of-four stimulators, this advantage seems to have been deemed insignificant or entirely forgotten. For example, to avoid large doses of nondepolarizing muscle relaxants, Caldwell5 suggested intubating with high doses of propofol or opioids and never mentioned succinylcholine even to condemn its use.

It is nice to see low-dose succinylcholine revisited.

Steven S. Kron, M.D. Avon, Connecticut. sskme2@comcast.net

In Reply—In their letter, Drs. Lu and Yu posed two questions. The first one concerns the number of attempts at tracheal intubation in our patients.1 The second question relates to the need for high doses of succinylcholine when teaching residents. All patients in our study 1 were successfully intubated on the first attempt. This study demonstrated that, for routine tracheal intubation, there is no need to administer more than 0.6 mg/kg succinylcholine. I believe that, in certain circumstances, as addressed in our article, there might be a need to administer greater than 0.6 mg/kg succinylcholine. Provided that the patient’s airway anatomy is normal and it is possible to ventilate the lungs easily (and to maintain anesthesia), there should be no problem with higher doses of succinylcholine in the scenario described by Drs. Lu and Yu. However, when unanticipated difficult tracheal intubation is encountered and accompanied with an inability to ventilate the patient’s lungs, the duration of apnea associated with high doses of succinylcholine could be disastrous. Inadequate ventilation and unanticipated difficult tracheal intubation represented, respectively, 38% and 17% of the 1,541 claims in the American Society of Anesthesiologists Closed Claims database.2

I disagree with Dr. Tabboush’s contention that patients with abnormal airway anatomy should be given higher doses of succinylcholine. Similarly, morbid obesity is frequently associated with factors that could impose difficulties for mask ventilation, rigid laryngoscopy, and/or intubation (for review, see Adams and Murphy5). It was never suggested or implied in our article that succinylcholine (even in small doses) should be used in patients with anticipated difficult airways. If a difficult airway is anticipated, I believe that the most appropriate and safe course of action is to proceed with an awake fiberoptic intubation. Therefore, it seems that Drs. Messent and Lim must have also misinterpreted our study. I also cannot agree with their comments about the effect of decreasing doses on recovery time. A NESTHESIOLOGY 2003; 99:1045–9

Mohamed Naguib, M.B., B.Ch., M.Sc., F.F.A.R.C.S.I., M.D. University of Iowa, Roy J. and Lucille A. Carver College of Medicine, Iowa City, Iowa. mohamed-naguib@uiowa.edu

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In Reply—We certainly have no argument with the position of Drs. Messent and Lim that when serious doubt exists about one’s ability to secure a patent airway, the administration of neuromuscular blocking agents is contraindicated. As our data1 make clear, even after doses of succinylcholine as low as 0.40 mg/kg, significant recovery (at the adductor pollicis) may on occasion not begin until 6–7 min have elapsed. Nonetheless, we feel compelled to point out that even experienced anesthesiologists may misjudge the ease of tracheal intubation. In these circumstances, the shorter the period of “cannot intubate, cannot ventilate” is, the better.

Forty years ago, when the senior author was a resident, a common intubation sequence consisted of little more than 4 mg/kg thiopental and 0.60 mg/kg succinylcholine. In that era, this protocol produced some fairly “ugly” intubations. Four decades later, after an induction consisting of propofol and a short-acting opioid, the same dose of succinylcholine produces highly satisfactory conditions for tracheal intubation in the majority of patients. For routine nonemergent intubations, we rarely exceed this dose. Nevertheless, as we pointed out in our conclusion, when complete neuromuscular block is critical, doses as high as 1.0–1.5 mg/kg may be still be appropriate.

We would also like to thank Dr. Kron for his kind remarks. However, we think that the efficacy of the slow administration of succinylcholine as a means of reducing the magnitude of fasciculations and the incidence of postoperative myalgia is controversial. Certainly, this strategy has been offered by several authors.2,3 On the other hand, there is equally convincing data to suggest that this technique is not efficacious.4,5 We would suggest that the hypothesis that small initial doses of succinylcholine produce a “self-taming” effect remains unproven.


akopman@rcn.com

References

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Outbreak of Severe Acute Respiratory Syndrome in Singapore and Modifications in the Anesthesia Service

To the Editor—The World Health Organization issued a global alert on atypical pneumonia and severe acute respiratory syndrome (SARS) and the risk to healthcare workers on the March 12, 2003, after reports from Hong Kong, China, and Vietnam.* This letter reports the changes in anesthetic practice in our hospital during the 2-month outbreak in Singapore.

All patients who had fever and dyspnea were treated as suspect SARS patients, even if they had no SARS contact history. All elective surgery was postponed during the outbreak; only emergency and cancer operations were performed. The operating complex reception staff completed a checklist (table 1) and checked the temperature of all patients. Patients with unexplained temperatures 37.5 °C or higher had surgery delayed until a cause was established. Patients were categorized as low-, medium-, and high-risk patients (table 2).

All staff were provided with thermometers to check and report their temperature thrice daily. Anyone with a temperature 37.5 °C or higher had to stop work and have a checkup at the staff clinic. All staff kept a log of all patients they had contact with, to facilitate contact tracing, and the information was collated daily centrally. The anesthesiology and operating room (OR) staff were grouped into two teams working alternate stretches, so that if one team became infected, the other team could take over and maintain the service. Contact between the two teams was strongly discouraged, and all department meetings were postponed.

All staff had to pass an assessment on the correct use, removal, and disposal of personal protection equipment. At mask-fitting and leak-testing exercises, different makes of masks were tried, and those who were unable to achieve a good mask fit were posted out of the OR to areas with low risk of SARS exposure. For low-risk patients undergoing surgery in the general ORs, all OR staff donned N95 masks, goggles, splash-proof gowns, and gloves. The anesthesiologists practiced double gloving and removed the outer gloves immediately after any invasive or airway procedure before handling clean equipment. Eight medium- and high-risk patients were considered SARS exposed or suspect SARS patients, and they underwent surgery in the isolation OR. These patients were transferred into the operating complex wearing an N95 mask if they had no dyspnea, and the anesthesiologists used additional protection of powered air-purifying respirator systems (3M Jupiter Air filter unit and HT-101 hood; 3M UK PLC, Bracknell, United Kingdom; or T4 Personal Protection System; Stryker Instruments, Kalamazoo, MI) and surgical gowns. Probable SARS patients were not operated on in this hospital but were transferred to a designated SARS hospital.

The OR at the end of the last bank of ORs, closest to an entry point into the operating complex, was designated the isolation OR. Access to this OR was reduced to two points with double doors, and the other doors were sealed. All equipment not used during a particular opera-

Table 1. Checklist for Suspected SARS before Admission of Patient into the OR

<table>
<thead>
<tr>
<th>Checklist Item</th>
<th>Description</th>
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<tbody>
<tr>
<td>Cough</td>
<td>Shortness of breath, breathing difficulty, and body aches accompanied by increase of body temperature 37.5 °C or higher</td>
</tr>
<tr>
<td>Close contact</td>
<td>Direct contact with a person who has been diagnosed with SARS or any SARS hospital staff/patient/visitor in the past 3 weeks (close contact means having cared for, having lived with, or having had direct contact with)</td>
</tr>
<tr>
<td>Having been discharged from, worked in, or visited the SARS hospital in the past 3 weeks</td>
<td></td>
</tr>
</tbody>
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History of travel to areas reporting cases of SARS by having been issued with a quarantine order

OR = operating room; SARS = severe acute respiratory syndrome.
modified personal protective suits during tracheal intubation procedures in 40 patients who contracted SARS.

Since April 2003, there were 70 probable and suspected SARS patients in our medical center, which is a 2,400-bed tertiary hospital. The SARS patients were kept in negatively pressurized isolation units for intensive care. Among them, 14 patients eventually died because of intractable respiratory failure. All criteria for the diagnosis of probable and suspect SARS cases were based on the guidelines made by the World Health Organization. During the outbreak, 40 probable SARS patients were intubated by 8 anesthesiologists and 23 nurse anesthetists. Our practice guideline for tracheal intubation in SARS patients was similar to that mentioned by others.4 Because the personal protective equipment recommended by the Centers for Disease Control and Prevention, such as Stryker Ti (Stryker Instruments, Kalama-zoo, MI) and powered air-purifying respirators (e.g., 3M AirMate; 3M, St. Paul, MN) were not available during the outbreak, we had to modify our own protective suits and equipment against SARS. The safety and efficiency of our modified suits, however, had not yet been proven. Figure 1 displays mode 1 (an N-95 mask [3M], goggles), protective suits [Tyvek; DuPont, Wilmington, DE], and gloves), mode 2 (a half face piece with P-100 filter; 3M), and mode 3 (a half face piece with P3-level filter; SR-100, Sundstrom; The SEA Group, Warriewood, NSW, Australia). On top of one of three modes, a protective hood (SR-64; The SEA Group) and a full face shield, which was made of a piece of plastic cloth and a transparency film, were used to cover the head, neck, and front chest (fig. 2, left and middle panels). We also adapted mode 3 by connecting a nonbreathing oxygen mask with a reservoir bag to the P3 filter (fig. 2, right panel). With such a device, it provided pure oxygen from a portable oxygen cylinder with a flow rate greater than 10 l/min. When air might be drawn from outside the mask when minute ventilation exceeded airflow rate, the virus load of inhaled air should be diluted. With the modified protective suits, 8 anesthesiologists and 23 nurse anesthetists participated in intubating 40 patients during the SARS outbreak. None of our anesthesia staff members contracted SARS.

Case reports of SARS to the World Health Organization were updated on July 2, 2003, including 8,442 cumulative cases and 812 deaths. Surveillance indicates 76% of infections were acquired in a healthcare facility. In 10–20% of cases, the respiratory illness was severe enough to require intubation and mechanical ventilation. The fatality rate among persons with illness meeting the current World Health Organization case definition for probable and suspected cases of SARS was around 3.4%. In Taiwan, there were 674 probable cases and 84 deaths (including 2 internists and 5 nurses) up until July 2. In our medical center, 14 of 70 SARS patients died (including 1 internist). Sixteen healthcare workers were infected, and 13 of them had partic-
ipated in tracheal intubation procedures on three SARS patients. None of them had worn any protective suits during the intubation procedure and caused a high infectivity rate of SARS (100%, 13 of 13). Although standard precaution against droplets and contact has been shown effective for routine procedure,4–5 a higher level of personal protective equipment is recommended by the Centers for Disease Control and Prevention for high-risk procedures such as tracheal intubation. Because of the limited medical resources during the outbreak, powered air-purifying respirators and Stryker T4 protection systems were beyond our expectation. Using the modified protective suits during tracheal intubation procedures in 40 SARS patients (figs. 1 and 2), none of our anesthesia staff members contracted SARS (0%, 0 of 31). It should be noted, however, that not all of the commercially available protective equipment have been proven to be totally effective against coronavirus infection. Therefore, our limited experience on this suit modification should not be regarded as an accepted standard but only an alternative option when powered air-purifying respirators and Stryker systems are not available. Based on the Centers for Disease Control and Prevention recommendation and a recent editorial viewpoint,7 powered air-purifying respirators or Stryker T4 systems should be the minimum requirement when intubating SARS patients cannot be avoided.

Kow-Aung Chang, M.D., Hsiang-Ning Luk, M.D., M.S., Ph.D.,* Bruno Jawan, M.D., Hsiao-Feng Lu, M.D., Hung-Shu Chen, M.D., Chao-Long Chen, M.D. * Chang-Gung Memorial Hospital, Kaohsiung Medical Center, Kaohsiunghsien, Taiwan. luk1015@adm.cgmh.org.tw

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To the Editor:—Airway management is a major responsibility for the anesthesia personnel. Patients with obvious signs suggestive of difficult direct laryngoscopy and intubation (i.e., orofacial abnormalities, short thick neck, protruding teeth, high arches palate, and others) are usually treated with extreme caution. However, some patients who appear normal on conventional examination may still present an unanticipated difficult airway. It is all the more alarming in morbidly obese patients who have a higher-than-usual incidence of difficult mask ventilation and precipitous oxygen desaturation.1 Here I present a case of unanticipated difficult intubation in a morbidly obese patient in whom the difficulty in performing direct laryngoscopy and intubation was a direct result of altered neck extension and mouth opening resulting from a change in the position of the patient from the sitting to the supine position.

A 35-yr-old African-American man (weight, 190 kg; height, 178 cm) presented for gastric bypass surgery under general anesthesia. He had no significant medical history. His preoperative airway examination revealed relatively large face, a thick neck, adequate mouth opening (> 4 cm), and intact upper and lower dentition with no evidence of overbite. He was assigned an airway Mallampati score of II. He had a full range of active neck flexion and extension. All examination was performed while the patient was in the sitting position. A doughnut-shaped pillow (10 cm thick) was placed under his head. Anesthesia was induced with thiopental sodium, and muscle relaxation for intubation was achieved with succinylcholine. Soon after fasciculation had passed, we faced significant difficulty extending the neck and opening his mouth. A slight extension at the neck allowed us to open the mouth enough to introduce the Macintosh No. 4 blade. Further attempts at neck extension were unsuccessful. The headboard of the operating table was lowered, with no improvement in neck extension. A roll of towels under the neck seemed to further limit the extension at the neck whenever the patient was placed in the supine position, which may be one of the causes of unanticipated difficulty in intubation. Because during passive jaw opening the angle of the mandible moves backward, the ease of doing that depends in part on the softness of the tissues that it displaces. In my personal experience, I have encountered this problem most often in patients with thick muscular necks with prominent sternocleidomastoids.

The difficult tracheal intubation is more common among obese than nonobese patients. Unfortunately, among the classic risk factors for difficult intubation, only a Mallampati score of III or IV has been identified as a risk factor in obese patients. It includes assessment of range of extension of the cervical spine, mobility at the temporomandibular joint, maximum mouth opening, and tongue size. Here I have identified two such factors, which may contribute to the increased incident of difficult laryngoscopy and intubation in a subset of obese patients. Consequently, I recommend that all obese patients and patients with thick, short necks be reevaluated while in the supine position for adequacy of range of motion and mouth opening to avoid unanticipated difficult airway on induction.

Govind R. Rajan, M.D. Veterans Affairs Medical Center, St. Louis, Missouri. govind_r@hotmail.com

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Unanticipated Difficult Intubation due to Significant Change in Mobility at the Neck and Jaw on Assuming Supine Position

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CORRESPONDENCE

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