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LARYNGOSCOPY AND TRACHEAL INTUBATION IN THE HEAD-ELEVATED POSITION IN OBESE PATIENTS: A RANDOMIZED, CONTROLLED, EQUIVALENCE TRIAL

Anesth Analg 2008;107:1912-1918

Rao SL, Kunselman AR, Greg Schuler H, DesHarnais S

Abstract

Purpose The purpose of this study was to determine if the time required for tracheal intubation with the table-ramp method was equivalent to the blanket ramp method in obese patients.

Background Proper positioning of the patient in the “sniffing” position is critical to ensuring optimal intubating conditions, especially in obese patients. Studies suggest intubation is more difficult in obese patients compared to non-obese patients. Multiple folded blankets are typically used to achieve an optimal sniffing position in obese patients. Unfortunately, using multiple folded blankets to achieve a proper sniffing position can be tedious and time consuming. An alternative maneuver is to configure the operating room table like a reclining chair with the back or trunk portion of the table raised up to achieve an optimal sniffing position.

Methodology This study was a prospective, randomized, controlled, equivalence trial comparing the blanket ramp method with the table ramp method in 85 obese (BMI >30 kg/m²) ASA I or II patients age 21-65 years of age undergoing general anesthesia. In the blanket group, patients were ramped by layering multiple folded blankets on a flat OR table so that the patient’s head was above the shoulders and the external auditory meatus and sternal notch were in the same horizontal plane. In the table-ramp group a pillow was placed under the patient’s occiput, and then the OR table positioned like a reclining chair. The table controls were used to flex the table at the trunk-thigh hinge and raise the back of the table at a height necessary to align the external auditory meatus to the sternal notch.

All patients were positioned by the same anesthesiologist. Laryngoscopy was performed by anesthesia residents, CRNAs, and attending anesthesiologists. A standard intravenous induction was used and tracheal intubation performed with a Macintosh size 3 blade, with second or third attempts with a Macintosh 4 or Miller 2 blade. The primary outcome variable was equivalence in the time to tracheal intubation between the two methods (pilot study mean estimate was 160 ± 81 seconds; maximum allowable limit of clinical significance, or “bounds of equivalence,” was -55 to +55 seconds). Time to tracheal intubation was defined as from the time interval between loss of consciousness to confirmation of successful intubation with presence of ETCO₂. Secondary outcome variables were number of airway obstructions, requirements for two-hand mask ventilation, SPO₂ ≤ 95% before tracheal intubation, best tidal volume, peak airway pressures, number of intubation attempts, and grade view.

Result A total of 85 patients (15 men and 70 women) completed the study. Of the 85 patients, 64 (31 blanket group, 33 table-ramp group) had bariatric surgery, and the remaining 21 (12 blanket group, 9 table-ramp group) had orthopedic, urologic, or gynecologic surgery. No differences in demographic or treatment variables (i.e., drug dosages, primary laryngoscopist) were noted between the two groups. The exception was neck circumference. The blanket group had significantly larger necks (44.2 cm vs. 42.2 cm, P = 0.03).

Secondary outcome variables were similar between the groups. Time to intubation was not correlated with BMI. The mean
Intubation time was 175 (±66) seconds in the blanket group and 163 (±71) seconds in the table-ramp group. Ninety-three percent (93%) and 92.8% of patients in the blanket group and table-ramp group were intubated on the first attempt. The 95% confidence interval for the difference in time to intubation between the two groups was -36 to +13 seconds, which was within the bounds of equivalence (-55 to +55 seconds). Because the 95% confidence interval was within the maximum allowable limit of no clinical significance, the two methods were equivalent.

**Conclusion**

The results of this study suggested that the blanket ramp and table-ramp methods were equivalent with regard to time to tracheal intubation and quality of laryngeal exposure. It was recommended that anesthesia providers consider including the table-ramp method as an additional strategy to optimize positioning of obese patients for intubation.

**Comment**

I thought this study was important because it provided some evidence supporting an alternative method to “ramping” the patient with multiple blankets. During my training I was taught to always ramp obese and pregnant patients to achieve a good sniffing position. The problem is that this is time consuming, and in some cases expensive. Not to mention that it is difficult to remove the sheets once the patient is under anesthesia. I routinely will position obese patients in a slight reverse trendelenburg position to facilitate mask ventilation, but never thought that I could intubate the patients in a 25 degree head-up position. One problem I see with this technique is that some anesthesia providers may need to use a step-up to get in a good position to intubate.

I thought this study was interesting because the investigators used a statistical analysis method not many of us are familiar with. Typical statistical tests are done to look for differences between groups. However, sometimes we want to know if two sets of data are equivalent. In this study the investigators wanted to demonstrate that the table-ramp method was similar in terms of time to intubation when compared to the blanket ramp method. In other words, they wanted to answer the question, are the blanket ramp method and table ramp method close enough to be clinically indistinguishable. If so, then the table ramp method could be used as an alternative to the blanket ramp method. Pilot study results indicated that the mean time to intubation was 160 ± 81 seconds in a sample of patients undergoing bariatric surgery and positioned with blankets. To be clinically indistinguishable, the researchers felt the variability in time to intubation should be < 60 seconds for both methods. So the investigators hypothesis was that the mean difference in time to intubation between the two groups would be no less or no more than the maximum limit of no clinical significance, or ±55 seconds. The 95% confidence interval for time to intubation was -36 to +13 seconds, which fell within the bounds of equivalence. Thus, with 95% confidence the researchers concluded the two methods were equivalent.

One issue I had with the study was that the first laryngoscopy attempt was with a Macintosh 3, and subsequent attempts with a Macintosh 4 or Miller 2. A Macintosh 3 blade was probably the right blade choice for most of the patients, given the majority of them were female. Looking at the data, it appears that in the blanket group there were a few more males and the neck circumference was significantly larger (44.2 (±3.9) vs. 42.4 (±3.3) cm, P = 0.03). I wonder how many of the males, especially those in the blanket group, required a second laryngoscopy attempt with a Macintosh 4? From a methodological standpoint I agree with trying to control for blade type by limiting the initial attempt to a Macintosh 3, however from a clinical standpoint I would probably not limit providers to this. One of the points I try to stress with my students is to choose the appropriate blade size for the patient, and in my experience sometimes a Macintosh 3 blade will be too small for a large male. Fortunately, in this study there were no adverse events and the investigators clearly described the plan for management of a difficult intubation.

Nonetheless, I think this study is important and the next time I have an obese patient I may try this technique. Maybe it will save my back and save the hospital some money.
Dennis Spence, PhD, CRNA


PERIOPERATIVE ACUTE ISCHEMIC STROKE IN NONCARDIAC AND NONVASCULAR SURGERY

Anesthesiology 2009;110:231-8

Bateman B, Schumacher C, Wang S, Shaefi S, Berman M

Abstract

Purpose This study evaluated the risks, risk factors, and mortality of perioperative stroke in non-cardiovascular surgery.

Background There have been very few studies to evaluate the risks of acute ischemic stroke (AIS) in noncardiac and nonvascular surgery. Although there are obvious stroke risks in cardiac and vascular surgery, risks are not readily identifiable in other types of procedures. A nationwide database is available that identifies certain types of complications associated with surgery type. This study attempted to analyze the database in order to identify the risk of stroke in three common surgical procedures.

Methodology The Nationwide Inpatient Sample (NIS) is the largest public use database of hospital information providing encoded data from about 20% of the non-federal acute care hospitals in the United States. The NIS was used to evaluate the acute ischemic stroke (AIS) risk for three surgical procedures. Those procedures were hemicolectomy, total hip replacement, and segmental lung resection completed between the years 2000 and 2004. Patients included in the database were age 18 years and older. Demographic information was collected, as well as comorbidities including diabetes, atrial fibrillation, congestive heart failure, COPD, bronchitis, history of stroke, renal disease, cardiovascular disease, ischemic heart disease, and peripheral vascular disease. Multivariate analysis was performed using generalized estimating equations to determine the incidence of perioperative stroke in the selected cases, as well as its effect on patients discharge status and mortality.

Result This retrospective, database study included 371,641 patients who met study criteria. AIS was identified in 0.7% of those patients undergoing hemicolectomy, 0.2% in total hip replacement, and 0.6% in segmental lung resection cases. When only cases involving patients greater than 65 years old were included, the stroke complication rate rose to 1.0%, 0.3%, and 0.8% respectively. The independent stroke risk factors identified in the study included advanced age, female gender, diabetes, atrial fibrillation, congestive heart failure, and cardiac valvular disease. It was also found that stroke risk was less in teaching hospitals as compared with non-teaching facilities. When those patients who developed perioperative strokes were evaluated for pre-discharge mortality, it was found that 6.0% of the hemicolecotmy patients died, as did 21.4% of the total hip patients, and 13.3% of the segmental lung resection patients. When compared to similar numbers of patients who underwent coronary artery bypass grafts in the same time period, the analysis found that 1.9% of those patients had a perioperative stroke and of those stroke patients 16.1% died before hospital discharge.

Conclusion This study established that the risk of perioperative stroke in certain nonvascular surgical cases was significant and lead to a mortality rate comparable to cardiac surgery. Certain risk factors were also identified, some of which can be managed more effectively to perhaps reduce the risk of perioperative stroke and/or subsequent mortality. In particular, the study indicates that patients with atrial fibrillation should be aggressively treated. Perhaps the anticoagulation therapy recommended for cardiac surgery patients with atrial fibrillation should be considered for noncardiac surgery patients as well.
Comment

In recent years, the Nationwide Inpatient Sample (NIS) has become a valuable research tool. This study demonstrated the importance of the NIS tool. When previous studies of small samples demonstrated the very broad range of stroke risk for general surgery to be 0.08-0.7%, the NIS tool has allowed evaluation of extremely large samples and demonstrated a more specific risk with specific procedures. Of course the data is only as good as the accuracy of the information provided, but the larger the database, the more likely the information will provide valid results. What I like about this particular study is that it uncovers risk factors and concerns that have not been well studied in the past. It helps us realize that the risk of stroke and subsequent problems related to stroke are very real even for fairly routine procedures like hemicolectomy, lung resection, and hip replacement. One thing that this study cannot provide is some insight into what role anesthesia providers play in the risk of perioperative stroke. I suspect that careful management of blood pressure, proper selection and management of anesthetic technique, as well as management of anticoagulation therapy are a few things that we can pay particular attention to.

Steven Wooden, MS CRNA
Does Epidural Versus Combined Spinal-Epidural Analgesia Prolong Labor and Increase the Risk of Instrumental and Cesarean Delivery in Nulliparous Women?


Abstract

Purpose The purpose of this study was to compare the duration of labor and manner of delivery in nulliparous women who received epidural vs. combined spinal-epidural analgesia during labor.

Background Epidural analgesia is commonly used for pain relief during labor and delivery. An alternative labor analgesia technique is combined spinal-epidural analgesia. With the combined spinal-epidural, a subarachnoid dose of local anesthetic and/or opioid produces faster onset of analgesia than an epidural alone, while the epidural catheter remains available to prolong the analgesia throughout labor and delivery. Combined spinal-epidural labor analgesia has been associated with a faster rate of cervical dilation and fewer assisted deliveries in some studies. Other studies have shown no difference between techniques.

Methodology This retrospective, non-randomized, observational study included term, nulliparous women between 3 and 5 cm cervical dilation when labor analgesia was requested. Some received epidural analgesia and others combined spinal-epidural analgesia according to the decision of the anesthesiologist. The manner of delivery was determined by the attending obstetrician.

In the epidural group a Tuohy needle was placed by loss of resistance to air at the L3-4 or L4-5 interspace. A 20 g catheter was then advanced into the epidural space. After an epidural test dose, 8 mL of 0.125% levobupivacaine with 40µg fentanyl was used as the initial dose. In the combined spinal-epidural group a 27 g Whitacre spinal needle was used to puncture the dura before the epidural catheter was placed. Through it, 2 mg levobupivacaine and 15 µg fentanyl was injected into the subarachnoid space. Both groups were then connected to a Patient Controlled Epidural Analgesia (PCEA) pump running a baseline infusion of 8 mL/h 0.1% levobupivacaine with 2 µg/mL fentanyl. The patient controlled bolus was set to 3 mL of this same solution with a 30 minute lockout. Patient pain perception was measured with a verbal rating scale of 0 to 10. The first assessment of pain recorded for the study was 20 minutes following placement of the epidural catheter.

Result The records of 788 laboring women were reviewed for this study; 322 (41%) in the epidural group and 466 (59%) in the combined spinal-epidural group. Demographic characteristics and cervical dilation were similar between groups.

Thirty-nine women in the epidural group (12%) and 46 women in the combined spinal-epidural group (10%) underwent cesarean section (P=ns). There was no difference in the incidence of assisted delivery, time from analgesia to delivery, oxytocin use, total dose of local anesthetic administered, or maternal satisfaction between groups.
Conclusion  There was no difference in the duration of labor, total dose of local anesthetic, or manner of delivery when comparing epidural and combined spinal-epidural labor analgesia in nilliparous women.

Comment

It has been some time since it was demonstrated that properly conducted labor epidural analgesia did not increase the risk of assisted or operative delivery, whether instituted early or late in labor.1 Some (very few) previous studies have shown combined spinal-epidural analgesia to be associated with faster cervical dilation and a lower assisted delivery rate than traditional epidural analgesia. (I’m not convinced yet.) The limitations inherent in this retrospective study without randomization are partially offset by the unusually good statistical analysis. Still, about all one can say for the results of this study is that it doesn’t give us any reason to keep asking whether spinal-epidural analgesia allows faster or more successful labor than an epidural alone. Both techniques have advantages and disadvantages. I see no evidence that the decision between techniques should be made based upon concerns over how one or the other will affect the duration of labor or the incidence of assisted or operative deliveries.

Michael Fiedler, PhD, CRNA

ACCIDENTAL DURAL PUNCTURE AND POST DURAL PUNCTURE HEADACHE IN OBSTETRIC ANAESTHESIA: PRESENTATION AND MANAGEMENT: A 23-YEAR SURVEY IN A DISTRICT GENERAL HOSPITAL

Anaesthesia 2008;63:36-43

Sprigge, JS, Harper, SJ

Abstract

Purpose The purpose of this study was to describe the incidence and management of accidental dural puncture (ADP) and post dural puncture headache (PDPH) after spinal and epidural analgesia on a single obstetrical unit over 23 years in the United Kingdom.

Background Epidural labor analgesia is a safe and reliable method of pain relief that has been used for 50 years. However, the incidence of ADP during labor epidural placement ranges from 0.5% to 2.5%, with up to 26% being unrecognized. Between 80-86% of parturients will develop a PDPH following ADP. PDPH may also occur after spinal anesthesia for cesarean section, especially when larger cutting needles (i.e., 20-22 g Quinke) are used. Patients with a PDPH typically present within 3 days of delivery complaining of a severe frontal or occipital postural headache which is relieved with supine position, and occasionally neck ache, photophobia, nausea, vomiting, and tinnitus. The PDPH may also hinder the mothers’ ability to care for her baby. Approximately 85% of PDPH resolve within 6 weeks, but in rare cases the headache may last months to years. Bed rest, caffeine and sumatriptin provide some relief, though the most reliable method is epidural blood patch (60-65% success on first treatment).

Methodology This was a retrospective observational survey of 23,538 epidurals and spinals administered over a 23 year period on a single obstetrical unit at a large district teaching hospital in the UK. Patients with ADP or PDPH were followed on a daily basis and details about signs, symptoms, and treatment were recorded on a standard form. Treatment was based on regularly updated guidelines, with conservative management for about three days. If the PDPH persisted, then an epidural blood patch was offered. During the epidural blood patch, sterile technique (surgical scrub, masks, caps, gowns and gloves) was used and blood cultures were sent on all patients.

Result The overall incidence of ADP after epidural was 0.9% (167/18,337 patients) with 88% (147/167) developing a PDPH. In most cases, there was no obvious reason recorded (i.e., anatomical difficulty, obesity, or poor patient cooperation) for the ADP. After spinal anesthesia 1.04% developed PDPH (52/5021). The rate of PDPH was higher with 27-30 g Quinke needles (3.5%) compared to 24-26 g pencil point needles (0.8%). Dural punctures presented as: CSF in the Touhy needle (42%); a high block with test dose (6%); high sensory level with top-up (3%); or no known ADP (36%). Three patients required intubation and mechanical ventilation for 3-4 hours; 2 of the 3 occurred after receiving unfractionated 8 mL top-up doses of bupivicaine (30-40 mg). In the 3rd patient, intubation was required after a 3 mL bolus of 0.5% bupivicaine after a recognized ADP. Symptoms of PDPH included a postural headache (88%), neck ache (49%), and photophobia (19%).

Conservative treatment (bed rest and mild analgesics) was only effective in 14% of patients. The first epidural blood patch relieved PDPH in 65-72% (61/85 after epidural; 13/20 after spinal) of patients, and in 50% of the remaining patients with a second epidural blood patch. Only 3 of 6 patients received relief when the epidural blood patch was administered > 4 weeks after the conduction block. Of those not treated with epidural blood patch (60 patients) PDPH lasted on average 5.3 days in 54 patients, and >1 month in 6 patients. One patient developed meningitis after spinal anesthesia (anesthetist not wearing mask), and one patient developed
permanent nerve injury (S1-2 motor weakness) after spinal anesthesia (severe pain reported with needle insertion). Eleven patients made formal complaints about their care, with 7 related to deviations from protocol, and 5 related to the anesthetists’ demeanor (“brusque manner of their anesthetist”).

Conclusion In this survey the incidence of ADP and PDPH was 1% after spinal and epidural anesthesia, with 88% of patients experiencing a severe PDPH after ADP with epidural anesthesia. Epidural blood patches provided good relief of PDPH in 70% of patients on the first attempt, and 38% (5/13 patients) on second blood patch. Patients should be informed of the approximate 1% risk of PDPH after spinal and epidural anesthesia. Meticulous and compassionate care is essential if an ADP or PDPH occurs.

Comment

This study is important to clinical practice because it highlights several important points to consider on the management and treatment of ADP and PDPH. First, it demonstrates the importance of having a quality assurance (QA) process in place for tracking ADP and PDPH. I applaud the investigators for establishing guidelines on the management of ADP and PDPH and their attention to detail in tracking the outcomes on over 23,000 neuraxial procedures in a busy teaching hospital. I have worked at many hospitals, both large and small, and never have I seen an obstetrical anesthesia QA system this thorough or extensive. Having a QA system in place is very important for obstetrical anesthesia practice because it is a high visibility area that requires thorough and sympathetic care, especially when complications such as PDPH occur. In my experience severe untreated PDPH really limits the mothers’ ability to care for her baby or other children, and is a significant source of maternal dissatisfaction.

Another important finding of the study was that it confirms the rate of ADP and high incidence of PDPH after an ADP with epidural placement (approximately 88%). This data should be used by anesthesia providers in discussing the risks of labor epidural anesthesia. I also noticed that in a majority of the ADP during epidural placement there was no obvious reason for the ADP. I speculate that many of the epidurals were placed by residents and that their incidence of ADP waxes and wanes like many teaching hospitals as new residents and SRNAs arrive. I also think it is important to point out that in 9% of the cases ADP was only recognized after the test dose or top-up dosing. The two cases of high levels necessitating intubation highlight the importance of using small fractionated doses of local anesthetic, with the appropriate time in between boluses. This is a point that cannot be stressed enough, especially with new residents or SRNAs.

In terms of the treatment, I think many of us offer conservative therapy initially for PDPH, however I believe that in the majority of cases an early blood patch is needed. The results presented here I think provide some support for this argument, especially when you look at the lower success rates with late blood patches (70% with 1st blood patch vs. 50% with 2nd blood patch), and the duration of PDPH in patients who did not receive a blood patch (average 5.3 days, and >1 month in six patients). I am a strong advocate of blood patches, but I also believe it is important to discuss all the risks, benefits and alternatives to therapy, because the procedure is not without risk.

Finally, I agree with the authors that meticulous and compassionate care is needed when complications such as PDPH occur. One only has to read their discussion on formal complaints to see how important the demeanor of the anesthesia provider is in keeping litigation low. As one of my former faculty told me in training, the most important way to keep from getting sued is to develop a rapport with the patient.

Dennis Spence, PhD, CRNA
Abstract

Purpose       The primary purpose of this study was to determine what the effective dose of dexamethasone was for post operative nausea and vomiting (PONV). The secondary purpose was to determine what analgesic effect dexamethasone had on post operative tonsillectomy patients.

Background    It is suggested that dexamethasone is effective for both PONV and control of post operative pain in tonsillectomy. The effective dose and risk profile has yet to be determined for dexamethasone in these cases.

Methodology  The study included children ages 2 years to 17 years old who underwent elective tonsillectomy. Children with mental retardation, a recent vaccination, a varicella infection steroids allergy, and children recently treated with steroids or antiemetics were excluded.

Children were randomly assigned post induction doses of dexamethasone at 0.00, 0.05, 0.15, or 0.5 mg/kg. All patients were given oral midazolam preoperatively followed by an induction and intubation with either sevoflurane or propofol and a non-depolarizing blocking agent. Intraoperative maintenance was accomplished with sevoflurane and alfentanil. The patients were also given a rectal dose of acetaminophen 40 mg/kg and the tonsil beds were infiltrated with bupivacaine and epinephrine. Three different surgical techniques were used by multiple surgeons including cold steel dissection, electrical cautery, or a combination of both. Postoperative pain was treated with morphine, acetaminophen with codeine, or ibuprofen as needed.

Result        The study was terminated early because of a surprisingly high number of postoperative bleeding episodes. The 200 recorded patients prior to study termination were evaluated and the results showed dexamethasone to have a dose dependent antiemetic effect. The placebo dose (0.00 mg/kg) had a 44% rate of PONV, 0.05 mg/kg was 38%, 0.15 mg/kg was 24%, and 0.5 mg/kg was 12%. In relationship to pain relief, any dose of dexamethasone resulted in less ibuprofen being used, but the use of morphine did not decrease between the placebo group and any of the dexamethasone groups.

Ultimately the focus of the study became the unexpected postoperative bleeding episodes. In the first 10 days postoperatively, 10.6% of the children had at least one episode of bleeding. The placebo group had 2 bleeding patients, 0.05 mg/kg group had 6, 0.15 mg/kg had 2, and 0.5 mg/kg had 12 (P = 0.003 compared with all dexamethasone groups). Eight children needed emergency reoperation for a bleeding episode. Analysis of the variables did not find any evidence that bleeding was associated with a postoperative medication or a specific surgeon.

Conclusion    The Paediatric Anaesthetists of Great Britain and Ireland recommended the use of dexamethasone 0.15 mg/kg. Experts throughout the world recommend the routine use of dexamethasone in surgical patients to prevent PONV. This study indicated that dexamethasone creates a significant dose dependent reduction in PONV, a reduction in the use of postoperative
ibuprofen, but created a significant risk of postoperative bleeding in pediatric tonsillectomy patients. The authors did not expect the bleeding problem and felt that chance, surgeon, or selection biases were unlikely causes. However, residual confounding of variables or unrecognized coagulopathy were possible causes. The most convincing biological cause might be delayed wound healing caused by glucocorticosteroids. Dexamethasone cannot be excluded as the cause of postoperative bleeding in this study, but further investigation is needed.

**Comment**

I first saw an abstract of this study in the AANA bulletin warning of the postoperative bleeding potential caused by dexamethasone. My first impression was skepticism, but I felt compelled to take a look at the full study and evaluate the results myself. On the surface the study appears to support the possibility that dexamethasone can cause postoperative tonsillectomy bleeding. However, I think the study creates more questions than it provides answers. Because the study did not originally intend to evaluate postoperative bleeding, its structure did not try to control variables that would affect bleeding outcomes. I personally have used dexamethasone to prevent nausea in ENT cases for many years and have not seen a postoperative bleeding problem. I asked several ENT surgeons what their impression of this study outcome was, and all of them were also skeptical that dexamethasone caused the bleeding. When I compare the overwhelming clinical experience of several surgeons and anesthesia providers, it is not consistent with the 200 patient case study provided here. I am not saying that the study should be ignored, but a better and more focused study needs to be conducted before I will be convinced that the risks of dexamethasone given for PONV outweigh the benefits.

Steven Wooden, MS CRNA
Abstract

Purpose The purpose of this study was to determine whether or not 2% lidocaine solution instilled into the pharynx during induction of general anesthesia would attenuate the hemodynamic responses to intubation.

Background Laryngoscopy and endotracheal intubation can cause undesirable increases in heart rate (HR) and blood pressure (BP). Spraying the hypopharynx and trachea with lidocaine requires laryngoscopy for visualization which may, in itself, cause the undesired increases in HR and BP. Lidocaine provides effective topical anesthesia. When instilled into the pharynx of a supine individual it pools in the pharynx and flows into the trachea.

Methodology This prospective, double-blind, randomized study included healthy patients aged 20 to 60 years scheduled for elective surgery with general anesthesia. Those with obesity, an anticipated difficult airway, or current medications affecting the cardiovascular system were excluded from participation.

No patient received any premedication. All patients had baseline vital signs recorded in the OR prior to induction of anesthesia and at one minute intervals until data collection was complete three minutes after intubation. Anesthesia was induced with 2 µg/kg fentanyl, 2 mg/kg propofol, and 0.6 mg/kg rocuronium. Forty-five seconds after induction, patients randomly received either 5 mL of 2% lidocaine or saline placebo instilled into the back of their throat from a syringe without a needle attached. The tongue was not pulled forward during lidocaine instillation. Patients were ventilated with 100% oxygen for three minutes before laryngoscopy and intubation with a 7 mm (women) or 7.5 mm (men) endotracheal tube. All laryngoscopies were performed by the same individual with a Macintosh blade. If laryngoscopy required more than 15 seconds, the cords were not visualized, or more than one laryngoscopy attempt was needed the patient was not included in the analysis. After intubation, anesthesia was maintained with 1% isoflurane and oxygen for the remaining three minutes of the study period.

Result Fifty-six patients were included in the study, 28 in each group. Demographic characteristics and baseline vital signs were similar between groups.

At 1, 2, and 3 minutes after intubation, average systolic BP (approximately 17 torr), mean BP (approximately 18 torr), diastolic BP (approximately 14 torr), and HR (approximately 10 bpm) were significantly lower in the lidocaine than the control group (P<0.01). In fact, BP and HR in the lidocaine group were not significantly different than baseline values one minute after intubation. More individual patients in the control group (50%) than the lidocaine group (7%) became hypertensive post-intubation.

Conclusion Orotracheal instillation of 5 mL of 2% lidocaine three minutes before intubation significantly attenuated the hemodynamic response to intubation in healthy patients.
Comment

This is a simple, clinically helpful study with generally good methodology and appropriate statistical analysis. I love the idea and I’m anxious to see how well it works in my hands. We know that lidocaine sprayed into the upper trachea and hypopharynx from an LTA kit can attenuate the sympathetic response to intubation if it is given time to work. And we know that lidocaine is safe to instill into the trachea so there is no concern about aspirating lidocaine. Simply pouring 2% lidocaine into the back of the throat during induction not only eliminates the stimulation of a laryngoscopy to apply the lidocaine, it is also faster than an extra laryngoscopy. While waiting three minutes for the lidocaine to work will be longer than we want to delay intubation at times (not to mention longer than the surgeon wants to wait), I suspect slightly less time will be sufficient. As always, there is a trade off; better hemodynamic control takes a little longer.

The investigators did miss the boat in one regard, though. They did not control for cigarette smoking. Even modest daily cigarette smoking has been shown to markedly affect the hemodynamic responses to intubation. If, in a worst case scenario, the control group was full of smokers and the lidocaine group wasn’t, the results could easily have been produced by the difference in smoking status rather than the lidocaine. This worst case scenario is unlikely, but one is left wondering how large an effect smoking status may have had on the results of this study.

Michael Fiedler, PhD, CRNA

ONSET AND EFFECTIVENESS OF ROCURONIUM FOR RAPID ONSET OF PARALYSIS IN PATIENTS WITH MAJOR BURNS: PRIMING OR LARGE BOLUS

Br J Anaesth 2009;102:55-60

Han TH, Martyn JAJ

Abstract

Purpose The purpose of this study was to compare the speed of onset of rocuronium administered as a single bolus or a priming and bolus dose in severely burned patients to a single bolus dose in non-burned control patients.

Background Resistance to the neuromuscular blocking effects of nondepolarizers begins several days after burns greater than 20% of body surface area. The diminished effect persists for months. The onset of paralysis is slower and may result in reduced levels of paralysis and/or reduced duration of paralysis. The cause is thought to be an increase in the number of acetylcholine receptors on skeletal muscle fibers. An alternate or additional cause may be the increased volume of distribution that accompanies fluid resuscitation, third spacing, and edema formation in burn patients. Whether or not a large, single bolus of rocuronium or preceding an intubating dose of rocuronium with a smaller priming dose will speed the onset of rocuronium paralysis in severely burned patients is unknown.

An effective priming dose of nondepolarizing muscle relaxants is usually 10% to 20% of the drug’s ED95. Priming doses generally shorten the time to paralysis by 10% to 40%.

Methodology This prospective study included patients with > 20% body surface area burns aged 18 years to 59 years old undergoing burn related procedures. Patients were between 6 and 92 days post burn. Exclusion criteria included anticipated difficult intubation, difficult mask ventilation, greater than 30% variance from ideal body weight, myasthenia gravis, and drugs or organ system disease that might alter the dynamics of rocuronium. Neuromuscular block was monitored with an acceleromyograph via the ulnar nerve and hand motion.

Patients were assigned to one of five study groups, three experimental and two control. All groups received rocuronium for muscle relaxation at induction. The Prime group received a priming dose of 0.06 mg/kg rocuronium followed by a bolus of 0.94 mg/kg (the balance of a total 1 mg/kg rocuronium dose) (n=29). The Bolus 1 group received only a 1 mg/kg bolus (n=29). The Bolus 1.5 group received only a 1.5 mg/kg bolus (n=35). (This group was added after the study was begun when 1 mg/kg failed to provide adequate relaxation in some burn patients.) Rocuronium was also administered to two groups of non-burned patients who served as controls. They received a total of 1 mg/kg rocuronium for intubation. In all groups that did not receive a priming dose of rocuronium a sham prime of normal saline was used as a placebo to mimic the appearance of the priming dose. Priming and sham priming doses were administered three minutes before the intubating dose of rocuronium.

All patients receive midazolam in holding. Induction of general anesthesia included propofol 2 – 2.5 mg/kg, fentanyl 1-2 µg/kg, and rocuronium. The airway was managed with mask ventilation during the onset of rocuronium.

Result Ninety-three patients were enrolled in the study. Three patients were not included in the analysis because their burn was less than 20% of total body surface area. Burn surface area was comparable among the three burn groups (Prime, Bolus 1, and
In the control groups, there was no difference in the initial onset of paralysis, time to 90% paralysis, or time to complete paralysis between groups (primed vs. non-primed). Burned patients had times to initial onset, 90%, and complete paralysis that were significantly longer than the control groups (P<0.05). In burned patients a priming dose of rocuronium resulted in a faster onset of block than a 1 mg/kg bolus but onset was no different than a 1.5 mg/kg bolus.

In the Bolus 1 group endotracheal intubation was graded as “difficult” in one patient and was unsuccessful in another due to inadequate muscle relaxation. This was despite the complete absence of any twitch at the adductor pollicis. Intubating conditions were “good” or “excellent” in all other burned and non-burned patients. Intubating conditions were graded as “excellent” in 71% of burned Prime patients and 56% of burned Bolus 1 patients. Diaphragmatic movement, cough, or movement of upper extremities occurred in 50% of burned Prime patients, 63% of burned Bolus 1 patients, but only 14% of burned Bolus 1.5 patients. In non-burned control patients intubating conditions were no different between Primed and Bolus 1 control groups and no patient movement occurred.

Approximately 10% each of both burned and non-burned patients receiving a priming dose of rocuronium reported problems breathing after the priming dose was administered. Specifically, they complained of chest tightness, choking, or dyspnea. Their Train-Of-Four ratio at the time of complaint was 87%, 100%, and 98% in the burned patients and 71%, 77%, and 100% in the non-burned control group.

**Conclusion** Priming accelerated the onset of rocuronium paralysis in severely burned patients at total doses less than 1.5 mg/kg. Rocuronium 1.5 mg/kg also improved intubating conditions in burn patients when compared to a 1 mg/kg dose. In burned patients, neither priming nor a 1.5 mg/kg bolus dose achieved the speed of onset seen when non-burned control patients were administered 1 mg/kg. Priming caused respiratory complaints in about 10% of all study patients.

**Comment**

We have long known about the risk of hyperkalemia following succinylcholine in patient with widespread soft tissue injury. And we have long known that patients with significant burns were resistant to nondepolarizing muscle relaxants. This study looked at whether or not priming would speed the onset of the already fast acting rocuronium in major burn patients. While the results were probably not what the investigators hoped for, this fact was not due to any fault in their study design. The only significant methodologic question I have is whether or not patients were randomized to the different study groups; three groups for burned patients and two groups for non-burned patients. This was not specifically stated. While not ideal, I understand why the investigators added the 1.5 mg/kg group after the fact and these patients were likely not randomized as this group was added after it became clear that 1 mg/kg was not going to adequately paralyze burn patients. But if none of the patients were randomized, some of the results may have been due to unknown factors that clustered in one group or another. This uncertainty weakens the impact of an otherwise well conceived study.

It is pretty clear that the speed of onset and quality of paralysis was not as good in burn patients. Inadequate relaxation and patient movement contributed to sometimes poor intubating conditions. Clinically, though, we would do well to remember that adequate anesthesia plays an important role in intubating conditions. None of these patients received any inhalation agent during mask ventilation and the dose of fentanyl they received averaged less than 2 µg/kg and even that was not given sufficient time to work before intubation. Sometimes, due to the risk of aspiration or an emergency situation we need raw speed. Most other times, however, speed is a convenience, not a necessity. In these situations 80% or 90% paralysis and adequate anesthesia makes for great intubating conditions and there are lots of ways to achieve adequate anesthesia.
We know that the priming technique speeds the onset of nondepolarizers under many circumstances. But like other anesthesia techniques, it has risks and benefits. This study shows that priming had little or no benefit in most burn patients. When an intubating dose of 1 mg/kg or less is used, priming resulted in 90% twitch depression in 1 minute compared to 2 minutes with the bolus dose. In some circumstances this difference could be very important clinically. The price for using priming is the increased likelihood that patients will become weak before induction of anesthesia.

While only 10% of primed patients complained of respiratory problems, I expect the problem is larger clinically. This study used a very specific protocol to determine the priming dose and the time between priming and induction. Clinically, when doses are close estimations and the priming interval is less likely to be “by the clock” I think more patients would become weak. Furthermore, some patients may not complain of weakness, yet still experience an increase in airway risk factors due to partial neuromuscular block. If, as I am, one is a believer in cricoid pressure, I think it should be applied immediately following a priming dose in patients at risk for aspiration rather than waiting until induction. The downside is that properly performed cricoid pressure is uncomfortable.

Lastly, I understand the investigators were looking at the onset of paralysis and not the duration but I sure wish they would have expended just a little more effort and reported the time until it was possible to reverse patients. Clearly, at least 1.5 mg/kg of rocuronium produces the best overall intubating results in major burn patients. If I knew how long it was going to last I could more easily decide what burn patients I was willing to use that dose in.

Michael Fiedler, PhD, CRNA

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A COMPARISON OF EPINEPHRINE CONCENTRATIONS IN LOCAL ANESTHETIC SOLUTIONS USING A “WASH” VERSUS MEASURED TECHNIQUE


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Abstract

Purpose
The purpose of this study was to compare the concentration of epinephrine in local anesthetic solutions when epinephrine was added to the local anesthetic by the “wash” technique versus measuring the amount of epinephrine added.

Background
When adding epinephrine to plain local anesthetic solutions, some anesthesia providers use a “wash” technique, rather than measuring the desired amount of epinephrine. The “wash” technique involves drawing up some 1 mg/mL epinephrine and squirting it out then drawing up the local anesthetic. The actual mg dose of epinephrine thus added to the local anesthetic depends upon the dead space in the syringe and adherence of epinephrine to the inside of the syringe.

Methodology
Five experienced anesthesiologists prepared syringes of local anesthetic with epinephrine. Each anesthesiologist prepared a large volume local anesthetic syringe for epidural use and a small volume syringe for spinal use. Each syringe was prepared twice, once measuring the dose of epinephrine added and once by the “wash” method. Each of these four exercises was repeated by each anesthesiologist five times.

Epinephrine was measured using a 0.5 mL insulin syringe with attached needle. Measured epinephrine was then squirited into the local anesthetic syringe. For the “wash” technique, 1 mg/mL epinephrine was drawn up into the local anesthetic syringe while it was still empty through either a filter straw or needle. Next the epinephrine was squirited out and the local anesthetic drawn up without changing the filter straw or needle. For the epidural trials a 20 mL local anesthetic syringe was used and the desired concentration of epinephrine was 5 µg/mL. For the spinal trials a 5 mL syringe was used and the desired concentration of epinephrine was 100 µg/mL in two mL. The actual concentrations of epinephrine were measured with high-performance liquid chromatography.

Result
Epinephrine concentrations in the 2 mL spinal local anesthetic solutions prepared by measuring epinephrine averaged 196 µg/mL. Average concentrations for each anesthesiologist ranged from a low of 156 µg/mL to a high of 258 µg/mL. Two mL spinal solutions prepared by the “wash” method averaged 121 µg/mL and ranged from 103 µg/mL to 169 µg/mL. The desired concentration was 100 µg/mL.

Epinephrine concentrations in the 20 mL epidural local anesthetic solutions prepared by measuring epinephrine averaged 8 µg/mL. Average concentrations for each anesthesiologist ranged from a low of 4.9 µg/mL to a high of 11.5 µg/mL. Twenty mL epidural solutions prepared by the “wash” method averaged 6.1 µg/mL and ranged from 4.3 µg/mL to 7.5 µg/mL. The desired concentration was 5 µg/mL. In both cases, the differences between measured and “wash” groups was statistically significant. In all cases the standard deviation was a fairly large portion of the average epinephrine concentration, between 20% and 38%.
Better accuracy in the “wash” groups was most likely due to luck. Any given combination of needles, filter straws, and syringes has a given dead space in which epinephrine will remain when it is squirted out of a syringe. The 20 mL syringes used in this study had a manufacturer’s specified dead space of 0.15 mL and the filter needles 0.11 mL for a total of 0.26 mL. A larger or smaller total dead space volume would result in a larger error in epinephrine concentration on the high or low side respectively.

**Conclusion**

Epinephrine concentrations were greater than desired in all but one measured solution and all but one “wash” solution but the errors in the measured group were greater. There was a significant difference in accuracy between anesthesiologists. No epinephrine concentrations were clinically significantly low.

**Comment**

I would like to caution the reader not to see this study as an argument for using the “wash” technique for adding epinephrine to local anesthetics. While the accuracy was better in the “wash” groups I think the authors got it right when they labeled this result “… the fortuitous result of the specific combination of materials used in preparation of the study solutions…” Unless you walk around knowing the dead space volume in all the needles and syringes you use and your hospital doesn’t change suppliers every time there is a price change you’ll never have any idea how much epinephrine you are adding to the local anesthetic. While there are a few circumstances where the concentration of epinephrine isn’t critical I think it is always a good idea to know how much of a drug I’m giving.

An important question unasked in this little study was why the error in the measured groups was so large. Some ended up with epinephrine concentrations over 2.5 times as high as desired. Using a 0.5 mL insulin syringe and 1 mg/mL epinephrine it shouldn’t be that hard to draw up 100 µg or 200 µg. The only sources for this error I can think of are sloppy work, not doing the math correctly to know how much epinephrine I need to draw up, or uncorrected poor vision and it doesn’t seem like any of those explanations should apply. I’d really be interested in seeing a follow up with a clinically realistic but carefully controlled measurement procedure.

When I add epinephrine to my local anesthetic I’m going to continue to measure it … carefully.

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RECURRENT OF CARDIOTOXICITY AFTER LIPID RESCUE FROM BUPIVACAINE-INDUCED CARDIAC ARREST

Anesth Analg 2009;108:1344-1346

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Abstract

Purpose The purpose of this report was to describe a case of local anesthetic induced cardiac arrest which resolved with intralipid administration and reoccurred 40 minutes after intralipid infusion was ended.

Background There are multiple case reports of intralipid being used successfully to treat systemic local anesthetic toxicity. Some sources recommend treatment of local anesthetic toxicity with a bolus of 1.5 mL/kg 20% intralipid. A second bolus can be administered if needed. The bolus should be followed with an infusion of 0.25 mL/kg/min to 0.5 mL/kg/min for 20 to 30 minutes.

Methodology A healthy, 33 year old, 72 kg man was scheduled for debridement of an upper extremity. An infraclavicular brachial plexus block was performed using an insulated needle and nerve stimulation. A median nerve motor response was attained at a current of approximately 0.5 mA. The block was initiated with 30 mL of 0.375% bupivacaine injected over 3 minutes with aspiration every 5 mL. Immediately after injection was completed the patient complained of “a dry sensation in his throat and eyes.” Seizure and apnea occurred immediately following this complaint.

Result The patient was ventilated with 100% oxygen by facemask and seizures were treated with two doses of sodium pentothal totaling 250 mg. ECG showed a narrow complex tachycardia at 160 bpm. The QRS complexes quickly widened and asystole ensued. Just prior to asystole, an infusion of 150 mL intralipid was begun and completed over less than two minutes. Endotracheal intubation, chest compressions, and epinephrine 1 mg IV were all performed at the onset of asystole.

Within 3 minutes of the intralipid infusion, a narrow complex rhythm with a rate of 130 bpm returned. At this point BP was 160/120 and \(S_P O_2\) was 100%. ABGs showed a respiratory acidosis with a pH <6.8. As appropriate treatment continued, an additional 350 mL of intralipid was infused over 30 minutes. When the second dose of intralipid had been infused the surgical procedure was performed. Anesthesia consisted of midazolam 5 mg and isoflurane 0.5% (end tidal concentration). Ten minutes after the intralipid infusion was completed, the patients heart rate increased to 140 and multifocal PVCs and runs of ventricular tachycardia were observed. Since no more intralipid was available the arrhythmias were treated with amiodarone. The patient survived to hospital discharge several days later.

Conclusion The recurrence of bupivacaine local anesthetic toxicity after initial successful treatment was likely due to multiple factors. These factors may have included: 1) a decrease in the circulating concentration of intralipid due to redistribution 2) the 3.5 hour elimination half life of bupivacaine, 3) delayed bupivacaine elimination due to reduced liver perfusion during cardiac arrest, and 4) ion trapping of bupivacaine intracellularly during acidosis with release back into the circulation with return of normal arterial pH.

Systemic cardiovascular toxicity may reoccur after successful treatment of toxicity from some local anesthetics. A single 500 mL bag of 20% intralipid is insufficient for many adult patients.
Comment

I was years into my anesthesia career before I read a case report of successful resuscitation following bupivacaine induced cardiac arrest. Case reports of quick and successful treatment of local anesthetic toxicity with 20% intralipid are all the more impressive when viewed in contrast to those many years. (For more on treating local anesthetic toxicity with intralipid, search on the keyword, “intralipid,” at AnesthesiaAbstracts.com)

This report raises an interesting question. Following ACLS protocol the patient was given a mg of epinephrine IV following cardiac arrest. When the intralipid bolus quickly resulted in a perfusing rhythm that epinephrine was probably the cause of the blood pressure of 160/120. Post arrest cardiac enzymes lead the authors to believe that this patient had experienced myocardial damage. While perhaps not the cause of the heart damage, the hypertensive response to epinephrine was certainly undesirable. I wonder if we should hold off on the epinephrine in cases like this until we see if the intralipid is going to quickly restore a normal rhythm.

I really wish anesthesia providers would rethink our use of bupivacaine. It is the only local anesthetic we use that has selective myocardial toxicity. Only tetracaine (commonly used only in small doses for spinal anesthetics), dibucaine (which we don’t use), and cocaine have lower margins of safety than bupivacaine. I’m not saying that we should ban the use of bupivacaine. Small doses for spinal anesthetics or very dilute concentrations rarely result in significant complications. But in this case report the authors appeared to have done everything right. They injected only 5 mL at a time, they aspirated, and the total dose amounted to only 1.56 mg/kg. (Published “Maximum Allowable Safe Dose” is about 2 mg/kg.) Despite proper technique, the patient suffered a cardiac arrest. I believe ropivacaine and other options to be safer choices.

Michael Fiedler, PhD, CRNA

The “sources” recommending specific doses of 20% intralipid for treatment of local anesthetic toxicity mentioned in this article were the Association of Anesthetists of Great Britain and Ireland (AAGBI) and published articles cited in original the case report.