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LETTERS TO THE EDITOR

The Relationship Between International Normalized Ratio and Development of Hemorrhage With Placement of Ventriculostomy

To the Editor:

We read with great interest the article by Bauer et al.,1 in which the authors reviewed the records of 71 patients who underwent ventriculostomy after traumatic brain injury (TBI). The authors analyzed the risk of hemorrhage from ventriculostomy placement for the following international normalized ratio (INR) groupings: normal INR (0.8–1.2), slightly elevated INR (1.21–1.4), and moderately elevated INR (1.41–1.6). The authors reported no significant difference in hemorrhage risk among the groups and concluded that patients with INRs of 1.2–1.6 were suitable candidates for neurosurgical emergent ventriculostomy. We would like to comment on this issue. We suggest that although a moderately elevated INR may not exclude patients from undergoing ventriculostomy, it does not mean that it is unnecessary to correct elevated INRs. Several authors have reported a relationship between mild coagulopathy and the progression of intracranial hemorrhage and poor prognosis in patients with TBI.2,3 and Lustenberger et al.4 reported that coagulopathy occurred as late as 5 days after injury and with a prolonged duration (>72 hours) in 30% of patients with severe TBI. We think that coagulopathies should be treated as soon as possible, regardless of whether they are the cause of a poor outcome or the consequence of the severity of the injury, because patients with severe TBI and coagulopathy may require subsequent surgeries for progressive hemorrhage.

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Pitfalls in Computed Tomography Evaluation for Mild Head Injuries in Children

To the Editor:

We read with great interest the article by Nakahara et al.,1 in which the authors reviewed the records of 278 patients with mild head injury. The authors found that, in 8 of the 278 patients, radiographs were negative, but computed tomography (CT) scans were positive for linear fractures. The authors also discussed the possible reason why most of these eight patients were pediatric patients, as follows: (1) in children, the skull is deformed more easily on impact than in adults; (2) compared with adults, the skull in children is thinner, and the sutures are unfused; and (3) ossification centers and synchondroses may be apt to be overestimated as the fracture line, because of (1) the thin skull and (2) the unclear delineation between the diploic veins and the outer table of the skull. Special attention should be paid to the diploic veins when CT scans in children are evaluated, especially in those younger than 10 years.

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To the Editor:

We read with interest the article by Maxwell et al.1 describing the largest published randomized controlled trial of airway pressure release ventilation (APRV) to date. We congratulate the authors for demonstrating similar efficacy of APRV compared with low tidal volume ventilation, the only strategy for which level one evidence exists. As long time users of the technique (>25 years), we have some additional insight we would like to offer.

Physiologically Relevant Application of Airway Pressure Release Ventilation

Generally considered a form of “open-lung ventilation,” APRV was

Kyutoku and Kawamoto2 reported that in patients younger than 10 years, the CT absorptivity of the outer and inner tables is lower and that of the diploë is higher than those in adults. We consider that in children, diploic veins may be apt to be overestimated as the fracture line, because of (1) the thin skull and (2) the unclear delineation between the diploic veins and the outer table of the skull. Special attention should be paid to the diploic veins when CT scans in children are evaluated, especially in those younger than 10 years.

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developed as a means of optimizing alveolar volume and functional residual capacity without the extreme elevations in peak airway pressure often seen with assist control and synchronized intermittent mandatory ventilation (SIMV) with even moderate levels of positive end-expiratory pressure.²⁻⁴ Clinical benefits of APRV have been attributed primarily to spontaneous respiration occurring between mechanical breaths and improving recruitment.⁵

Multiple studies of APRV have been published, but few apply the method as originally described and may not achieve open-lung ventilation, the ultimate goal.² Maxwell et al. set initial P$_{\text{High}}$ to equal P$_{\text{plateau}}$ observed during SIMV, resulting in higher mean airway pressures during APRV. Opening pressure of the lung was not considered, and airway pressures likely were unnecessarily high. Most studies have applied APRV at mean airway pressures similar to the comparison mode.⁶ In our opinion, the most physiologically relevant way is to set P$_{\text{High}}$ while monitoring opening pressure using pressure-volume loops. Tidal volume can be set at 6 mL/kg by limiting release time (T$_{\text{Low}}$) or setting P$_{\text{Low}}$ at a level that prohibits further exhalation.

Many clinicians use APRV as a rescue mode for the treatment of acute respiratory distress syndrome (ARDS). No study supports (or discourages) the use of APRV in that way, but APRV should not be reserved for the rescue setting. The benefits of spontaneous breathing during APRV may be greatest in patients with ARDS but apply to all patients requiring ventilatory support. Varpula et al. compared APRV and SIMV in patients with acute lung injury with PaO$_2$/FiO$_2$ <200 mm Hg. Tidal volumes were 8 mL/kg to 10 mL/kg. Survival was >80% in both groups. The study was conceived before ARDSNet was published, so one can only theorize the potential survival if lower tidal volumes were applied.⁷ Maxwell et al. examined patients at high risk of pulmonary morbidity, but it is possible that the patients in the study would have done well regardless of ventilator strategy (30 of 32 patients in the SIMV group were weaned from ventilatory support within 72 hours). Fewer than half of patients had acute lung injury or ARDS, so a benefit in treating ARDS cannot be fully discerned. Also, uneven randomization resulted in higher APACHE II scores in the APRV group. The investigators used the widely accepted “drop and stretch” weaning strategy. We prefer to decrease rate and P$_{\text{High}}$ separately, as the functions of optimal lung volume (P$_{\text{High}}$) and the patient’s ability to breathe spontaneously (T$_{\text{high}}$) are not always synchronized. Despite the significantly higher mean airway pressures during APRV, extubation was achieved in a comparable amount of time with both modes, indicating that mild hyperinflation is not a significant detriment.

For APRV adherents, the present study demonstrates noninferiority of APRV when compared with low tidal-volume SIMV strategy in patients with trauma. Further trials examining APRV as either initial or rescue therapy should incorporate a more “physiology-driven” approach to an open-lung ventilation concept to help identify patients who would benefit the most from APRV therapy. Individualized ventilator setup focusing on pulmonary mechanics, opening pressures, and gas exchange is recommended.

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Reply to Letter to the Editor, “Physiologically Relevant Application of Airway Pressure Release Ventilation”

In Reply:

We would like to thank Dr. Evans for his kind and insightful comments regarding our article. When airway pressure release ventilation (APRV) became available as a standard ventilator mode, our initial experience was favorable, and we were excited to trial the therapy in a clinically relevant setting, i.e., trauma patients intubated for >72 hours who would be at risk for developing pulmonary complications such as pneumonia or acute respiratory distress syndrome (ARDS). Perhaps an ambitious effort for a single institution, we set out to compare APRV with the gold standard, the ARDS Network recommendations for low-tidal volume ventilation. Although we did not replicate the ARDS Net study per se, we used the P$_{\text{plateau}}$ generated from the 6 mL/kg tidal volume as a way to have equivalent starting pressure for both arms of the study. Our protocol, typical for a trauma or surgical intensive care unit, allowed the patients in the synchronized intermittent mandatory ventilation arm to be rapidly weaned to continuous positive airway pressure and pressure support, which differed from the ARDS Net study. We agreed that a potentially interesting approach to the experimental design for such a study would be based on the open-lung ventilation strategy now that this study has been completed. However, uniform interpretation of p-v loops and application of such protocol may present some
interoperator variation and technical difficulties with standardization of the approach. Clearly, future studies investigating APRV and ARDS should be multiinstitutional in nature. We do not believe that APRV should be used as a rescue mode either. We feel that this modality is an excellent recruitment strategy for ventilating trauma patients with multiple factors leading to atelectasis and consolidation. Our study showed that APRV is safe for general use in the trauma patient population and still has unproven benefit as a recruitment strategy and in increasing functional residual capacity. Our respiratory protocols now include the 3100B high-frequency oscillating ventilator (Sensormedics, Yorba Linda, CA) for patients with refractory ARDS as the next phase in managing patients with severe respiratory failure and oxygen indices >13.

Finally, we feel that the higher mean airway pressures seen in APRV are beneficial because they ultimately translate into decreased peak pressure and barotrauma. The elevated mean airway pressure also decreases the shear forces seen in other ventilator modalities that function below the lower inflection point while simultaneously increasing the time available for recruitment. Indeed further studies are warranted in this area and should include the open lung theory in development of the experimental design.

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Tourniquets on the Battlefield: Could N-Acetylcysteine Be Useful?

To the Editor:

We read with great interest the study of Koca et al. in the March issue of the Journal of Trauma. They assessed the protective effect of N-acetylcysteine (NAC) on occurrence of oxidative stress resulting from tourniquet-induced ischemia-reperfusion period in routine arthroscopic knee surgery. First, the use of tourniquets is also well established in military setting.2 Limb injuries caused by shrapnel or bullets are common in the combat scenario. Hemorrhage from injured limbs, leading to rapid exsanguination, continues to be a leading source of battlefield death. Although definitive care is usually provided in a hospital setting, immediate bleeding control should be achieved as soon as possible to prevent rapid hemodynamic deterioration. Ongoing hostile fire and other extreme battlefield conditions pose a significant challenge for the immediate caregiver. Combat soldiers are regularly equipped with tourniquets and trained to identify extremity hemorrhage and use the tourniquets routinely to stop the bleeding. Kragh et al.3 showed in the current war that emergency tourniquet use improves survival rates in patients with major limb trauma.

Beyond lies the question of how to manage tourniquets once they are applied. Two key points in time represent either end of a spectrum of risk. At one end, removing a tourniquet at 2 hours has minimal risk of ischemic complications and hemorrhage may have been controlled. Beyond 6 hours, the risk of arrhythmias and crush syndrome is so high that amputation above the level of the tourniquet is mandatory. Between these time points, the likelihood of serious complications including death increases with time and the chance of salvaging the limb decreases toward zero. The main problems are hyperkalemia and renal failure.

Second, there are few interventions that have been shown to consistently prevent acute kidney injury. Measures such as adequate hydration, maintenance of adequate circulating blood volume, and mean arterial pressure, to maintain renal perfusion pressure, are still the mainstay of prevention. Among the remaining pharmacologic options, NAC has the strongest evidence in prevention. The use of NAC has been shown to decrease the incidence of contrast nephropathy.5 Considering its safety, low cost, and possible benefit, NAC can be used in high-risk patients to prevent contrast nephropathy.

Finally, we would like to know the authors thoughts, based on their study and exceptional experience in the military medical academy of Ankara, concerning the potential role of NAC in military tourniquet use, to avoid both renal failure and amputation.

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Cervical Spine Immobilization in the Tactical Environment

To the Editor:

We read with great interest the study of Lustenberger et al., reporting a low rate of unstable cervical spine fracture after penetrating neck injury. The disability and socioeconomic costs associated with spinal injuries are so devastating that there is considerable anxiety surrounding both the potential for missed injuries and the prehospital management of patients who may have unstable spine fracture. The study of Lustenberger et al. seems to demonstrate that most victims of penetrating trauma to the neck do not require immobilization of their cervical spine. In contrast, Isiklar and Lindsey retrospectively reviewed 37 patients who sustained low-velocity gunshot wounds to the spine column. Of the 12 patients with penetrating injuries to the cervical spine, 3 had unstable injuries. This study points out a significant number of unstable cervical spine injuries secondary to low-velocity penetrating trauma. The conflicting results in the literature provide no definitive guidance, but the work of Lustenberger et al. agrees with the growing body of evidence that most victims of penetrating trauma to the neck do not require immobilization of their cervical spine.

Moreover, we would like to highlight that the rare decision of prehospital cervical spine immobilization after penetrating neck injury should be based both on the patient’s condition and the tactical situation. The practice of many trauma specialists is to only immobilize patients who have a neurologic deficit noted on examination or if the patient is unresponsive. But exposure of neck wounds and examination or if the patient is unresponsive may not be inclined to focus on patient and provider safety rather than immobilizing the spine.

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Electronic Scooters: Sweet Tooth for Danger?

An 8-year-old girl was riding a friend’s Sweet Pea Pocket Mod when she lost control and ran over her 4-year-old sister’s leg. The x-rays revealed a spiral fracture of the left tibia. She was immobilized in a short-leg walking cast for 4 weeks.

Innovative toys and activities for children may lead to unanticipated trauma. Examples include the Heelys roller shoes, Nintendo’s Wii, and child participation in mixed martial arts. In 2000, Razor introduced the scooter to a new generation of children and adolescents. Then in 2009, they presented a line of electric scooters with seats, including the Sweet Pea Pocket Mod (Fig. 1). It weighs 57 lbs and can travel 10 miles at a speed of up to 15 miles per hour on a single charge.

Reading this product’s owner’s manual exposes some concerns. It does not recommend parental supervision and this is not an integrated feature. However, the “on/off” switch below the seat can easily be engaged by a child much younger than the suggested minimum age of 13 years. There is no parental control mechanism to lock it in the “off” position. This omission potentiates danger, especially when the rider is accustomed to accelerating and braking with her feet instead of the handlebar controls.

Parents may give pause when reading the first two paragraphs in the manual discussing safety. Risk of “serious injury or death” is mentioned three times. It states the scooter should be used in “controlled environments, not on public streets or sidewalks.” Parents may not be inclined to haul a 57-lbs scooter to an open area every time their daughter wants to ride.

The product description appeals to girls younger than 13 years when it states the Pocket Mod “gives every young girl with an eye for fashion and a passion for fun a set of wheels.” Younger girls might appreciate the “handlebar streamers and sporty bell” more than their teenage sisters.
We advise parents to read the owner’s manual before considering an electronic riding vehicle for their child. Manufacturers should install a parental control key or switch to limit underage and unsupervised use. Physicians must remind parents that research before and supervision after the purchase of electronic toys can prevent injury and teach responsibility to children of all ages.

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To the Editor:

We read with great interest the article by Honeybul et al.,1 in which the authors reviewed the records of 74 patients with traumatic brain injury who had decompressive craniectomy (DC) for intractably raised intracranial pressure (ICP). They reported that the discrimination of the Corticosteroid Randomization After Significant Head Injury (CRASH) prediction model was excellent. We agree with the authors and wish to provide further comment. In a study by Cooper et al.,2 DC decreased ICP but was associated with more unfavorable outcomes. We consider that this result shows that indication for DC cannot be decided only by the threshold of ICP and cerebral perfusion pressure (CPP). Therefore, another tool to determine whether DC should be performed is required, and we consider that the CRASH prediction model could be used as one of these tools. Furthermore, we wish to provide an additional helpful index. Secondary brain injury is not always associated with pathologic changes in ICP or CPP; therefore, maintenance of adequate brain tissue oxygen (PbtO2) is recognized as a primary objective.3 Spiotta et al.4 reported that PbtO2-based therapy results in less mortality and a more favorable outcome 3 months after injury compared with only ICP and CPP-based therapy. In addition, DC for severe traumatic brain injury can reduce ICP but it also increases PbtO2.5 We suggest that multimodal use, including ICP/CPP and PbtO2 monitoring, and the CRASH prediction model, may be helpful for clinicians to determine the optimal timing of DC for patients expected to benefit from surgery and also to avoid excessive/ineffective procedures for patients who are not expected to benefit from surgery.

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Decompressive Craniectomy for Diffuse Cerebral Swelling After Trauma

To the Editor:

We read with great interest the article by Honeybul et al.,1 in which the authors reviewed the records of 74 patients with traumatic brain injury who had decompressive craniectomy (DC) for intractably raised intracranial pressure (ICP). They reported that the discrimination of the Corticosteroid Randomization After Significant Head Injury (CRASH) prediction model was excellent. We agree with the authors and wish to provide further comment. In a study by Cooper et al.,2 DC decreased ICP but was associated with more unfavorable outcomes. We consider that this result shows that indication for DC cannot be decided only by the threshold of ICP and cerebral perfusion pressure (CPP). Therefore, another tool to determine whether DC should be performed is required, and we consider that the CRASH prediction model could be used as one of these tools. Furthermore, we wish to provide an additional helpful index. Secondary brain injury is not always associated with pathologic changes in ICP or CPP; therefore, maintenance of adequate brain tissue oxygen (PbtO2) is recognized as a primary objective.3 Spiotta et al.4 reported that PbtO2-based therapy results in less mortality and a more favorable outcome 3 months after injury compared with only ICP and CPP-based therapy. In addition, DC for severe traumatic brain injury can reduce ICP but it also increases PbtO2.5 We suggest that multimodal use, including ICP/CPP and PbtO2 monitoring, and the CRASH prediction model, may be helpful for clinicians to determine the optimal timing of DC for patients expected to benefit from surgery and also to avoid excessive/ineffective procedures for patients who are not expected to benefit from surgery.

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In Reply:

We thank Dr. Takeuchi et al. for the interest they have shown in our work and they make a fundamental point with which we fully concur. Interpreting the intracranial pressure (ICP) in isolation has very significant limitations, and only by using accurate prognostic data to assess the severity of the primary brain injury, in combination with integrated multimodal monitoring as a measure of secondary injury, can the effectiveness of surgical intervention be truly evaluated. We also agree that the ICP should not be the only indicator for performing a decompressive procedure. However, we cannot agree with the conclusions of the DECARA investigators that the procedure worsens outcome because this statement is not really supported by closer examination of their data.1 The patients who received a decompressive craniectomy had a more severe traumatic brain injury than those in the standard care group. More patients in the surgical group had bilateral nonreactive pupils (27% vs. 12%; \( p = 0.04 \)), and radiologic findings as adjudged by the Marshall grading were more severe in the surgical group (grade III and nonevacuated hematoma: total 77% vs. 67%). Both these factors are significant prognostic factors for patients with severe traumatic brain injury, and when pupil reactivity was corrected for, there was no significant difference between the two groups. A further limitation of this trial is that the standard care group was not truly representative of those patients who require a decompressive procedure. Although the initial ICPs were 20 mm Hg to 25 mm Hg, they subsequently fell below 20 mm Hg over the following 3 hours to 6 hours. Comparing these patients with those who have had an aggressive surgical decompression will be misleading. Although it can be accepted that the DECARA trial has demonstrated that early decompression for relatively mild and transient intracranial hypertension does not prevent secondary brain injury and improve outcome, the question remains as to the role of decompressive surgery as a lifesaving procedure for those patients whose ICP continues to rise beyond 25 mm Hg and above.2 The statements made by Takeuchi et al. regarding the use of multimodal monitoring and an accurate prediction model to optimize timing and effectiveness of surgical intervention are pertinent.

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Triage Protocol Modifications to Reduce Undertriage

To the Editor:

We would like to thank a French team1-3 for their interest in our recent article published in the Journal of Trauma titled “Predictive factors for undertriage among severe blunt trauma patients: what enables them to slip through an established trauma triage protocol.”4 Tourtier et al.5 suggested utilization of the shock index (SI), with better ability to identify early shock status than blood pressure or heart rate alone, to reduce undertriage. Although the SI may improve the accuracy of the physiologic component of the triage protocol, our data did not support or refute this. Only 3 of 53 undertriaged patients showed abnormal SI (≥0.9) (Table 1); however, we used physiologic data on hospital arrival because of largely missing data at the scene. In our study, the undertriaged patients were defined as those transferred from another hospital; treatment before transfer might have modified the physiologic status.

Physiologic data, such as heart rate and blood pressure, are time dependent; it is sometimes difficult to identify patients with severe injuries based only on the physiologic status at the scene. The SI, even with better sensitivity, is not spared from this limitation. Cannon et al.5 showed increasing SI by 0.3 is a better predictor of mortality than the SI at the scene or at emergency department. Therefore, integrating into the protocol changing patterns of physiologic parameters during transfer might be an option to reduce undertriage.

To improve the triage decision at the scene, it is preferable to add new information, which is predictive of inju-
rries that are likely to slip through the triage protocol. As suggested by Tazarourte et al., ultrasound at the scene could also provide valuable information to identify severe chest and abdominal injuries, which might otherwise undertriage. Our study showed 15 and 6 of 53 undertriaged patients had severe chest and abdominal injuries, respectively; the focused assessment with sonography in trauma might have identified such injuries at the scene. However, ultrasound cannot provide additional information for isolated head and pelvic injuries that are likely to be undertriaged as shown in our study. Possibly a different type of information, such as detailed crash data may improve prediction of such injuries.

As stated by Raynaud et al., undertriage reduction should be balanced against overtriage increase. To what extent overtriage should be accepted depends on the local situations. In our study, we defined patients having any injuries with Abbreviated Injury Scale score ≥3 as severely injured patients who should be treated in a critical care medical center, given the treatment ability of non-critical care medical center hospitals. Based on this definition, overtriage rate was 24.5%; based on the commonly used definition, Injury Severity Score ≥16, the rate was 47.7%. Mizobata et al. reported that undertriage reduced from 23.4% to 14.8% after introducing triage guidelines based on the definition of Injury Severity Score ≥16, whereas overtriage increased from 37.6% to 55.9%. We believe these overtriage rates are acceptable in the study area.

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Proximal Splenic Arterial Embolization May Also Result in Pancreatic Necrosis

To the Editor:

We read with interest the review article by Schnüriger et al., which reviewed outcomes of proximal versus distal splenic artery embolizations (SAEs) after trauma. This meta-analysis discussed both major and minor complications. We have encountered two cases of postproximal SAE pancreatic necrosis, not reported in this review, which resulted in splenectomy and distal pancreatectomy. This complication has been reported in the literature once but was managed nonoperatively.

In both of our patients, the initial abdominal computed tomographic scan images of the pancreas were interpreted as normal. Both patients developed fever and peritoneal signs, which led to laparotomy, splenectomy, and ultimately distal pancreatectomies with a diagnosis of distal pancreatic necrosis. The splenic artery in one of the cases was tortuous, and the coils were placed more centrally.

The distal pancreas is sensitive to hypoxia and ischemia. The vascular supply to the pancreas is well documented with branches from the splenic artery and primarily the dorsal pancreatic artery and greater pancreatic artery, which flow into the transverse pancreatic artery to feed the peripheral/distal pancreas. Proximal embolization may lead to ischemia, enzyme elevation, pancreatitis, and, if severe enough as in our cases, frank pancreatic necrosis.

We hope to draw attention to the potentially life-threatening complication of pancreatic necrosis after proximal SAE. Because splenic injuries continue to involve nonoperative management with SAE, related morbidities must be carefully documented to thoroughly understand the ramifications of the intervention.

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Preliminary Experience in Acute Pain Control for Nonoperated Hip Fracture

To the Editor:

In our center, 712 patients were admitted for hip fractures from March 2009 to August 2010. Seventy-one patients were managed conservatively; 16 required referral to the acute pain service for pain control. Operative intervention remains the mainstay of treatment in our institution. However, 65% of these patients are above the age of 80 years and up to 40% have three or more comorbidities. Hence, the “Conservative Hip Fracture Management Pathway” was adopted.

The Conservative Hip Fracture Management Pathway is a methodical workflow targeting nonsurgical candidates or patients who do not consent to operative intervention. It aims to reduce the occurrence of complications, to improve functional capacity, and hence reduce overall length of hospital stay. We report our preliminary experience in 16 nonoperated hip fracture patients from March 2009 to August 2010 who were enrolled into this pathway.

Numerous nerve blocks for hip fractures exist, including continuous femoral nerve block infusion and remaining 12 received other modalities.

In the group with continuous femoral nerve block infusion, all were females. Average age was 71.3 years ± 13.0 years old. The procedure was done under ultrasound guidance with the aid of a peripheral nerve stimulator. The femoral nerve was isolated at a mean of 0.40 mm ± 0.18 mm, and the catheter was placed at 9.8 cm ± 3.3 cm at skin. A bolus of 20 mL of either 1% ropivacaine or 0.25% bupivacaine was given initially followed by infusion of either 0.2% ropivacaine (3 patients) or 0.125% bupivacaine (1 patient) at 4 mL/h and PCA bolus of 5 mL every 15 minutes. Median pain scores (Numerical Rating Scale) on postblock day 1 for rest pain was 3.00 (0.75–4.50) and on movement, it was 5.50 (5.00–6.00). By postblock day 4, three patients (75%) reported a pain score at rest of 0 and one (25%) reported no pain at movement and during transfer. Two patients (50%) were able to sit up in bed and one (25%) achieved wheelchair mobilization. One (50%) managed to sit up in bed and one (25%) managed to achieve wheelchair mobilization on day 5.

In the group without continuous femoral nerve block infusion, six (50%) were females. Average age was 80.2 years ± 6.6 years old. Eight (66.7%) received only oral analgesia; two (16.7%) received fentanyl patch; one (8.3%) received PCA fentanyl, and the remaining one (6.7%) received parenteral opioid. There was a steady decrease in pain score at rest and by fourth day postcommencement of analgesia, all 12 patients (100%) reported a pain score of 0 at rest; 8 (66.7%) reported pain on movement and transfer; 8 (66.7%) managed to sit up in bed, and 2 (16.7%) achieved wheelchair mobilization. Seven (58.3%) managed to sit up in bed and three (25%) managed to achieve wheelchair mobilization on day 5.

Emergency Medical Services and Emergency Department Thoracotomy

To the Editor:

We appreciate the in-hospital prospective on the role of emergency department thoracotomy (EDT) in the...
management of critically ill trauma patients and the challenge of determining the patient population that is most likely to benefit from this intervention. In their article on EDT in the February 2011 issue of the Journal of Trauma, Moore et al. argue that the National Association of EMS Physicians/American College of Surgeons-Committee on Trauma guideline for termination of resuscitation is “too restrictive,” implying that more patients in traumatic cardiac arrest should be transported to trauma centers for potential EDT. Although the National Association of EMS Physicians guideline for termination of resuscitation in trauma is currently being updated, we believe that even in its current form, it is inappropriate to suggest that the findings of this study render it invalid.

Moore et al. report on 56 patients who had EDT that survived to hospital discharge. Unfortunately, the authors do not provide the reader with a denominator to put this number in perspective. The reader is left wondering how many patients underwent EDT in order for there to be one successful outcome. In their data set of survivors of EDT, only 1 of 56 (1.8%) and 3 of 56 (5.3%) of the patients with more than 10 minutes of CPR in the field or asystolic arrest, respectively, survived to discharge with only mild or no neurologic deficits. The single patient who survived after 15 minutes of CPR was reported to have moderate neurologic deficits.

Beyond the analysis of these results from the perspective of the individual rather than the community, the authors also make incorrect assumptions about EMS systems. The authors write that, “...ground EMS services do not monitor cardiac activity in the field.” Although it may be true that many rural EMS systems do not have advanced life support capabilities, virtually all US urban EMS systems have monitoring capabilities with 12 lead electrocardiograms and it is the standard of care for even basic life support providers to carry an automated external defibrillator that has the capability to determine whether a patient is in a shockable rhythm. Furthermore, it is within the scope of practice for many EMS providers to perform needle thoracostomy and monitor end-tidal CO₂. We expect that in coming years, increased availability of prehospital ultrasound and point-of-care testing will further refine ability to better define subpopulations that may or may not benefit from further resuscitative efforts or specific interventions.

Compared with the inpatient and emergency department settings, EMS operates with a different set of medical personnel, operational parameters, and duty to both patient and the community that as a whole provides a different lens through which to view and interpret research findings from the in-hospital environment. The uniqueness of the practice of clinical medicine in the EMS environment and the skills required to provide medical direction for EMS has led to the recent recognition of the creation of EMS as an Accreditation Council for Graduate Medical Education-approved subspecialty within the house of medicine.

EMS must also take a community approach to delivery of healthcare services, finding the appropriate balance between maximization of care for the individual and maximization of care for the general population. Aside from the risk to EMS professionals and the public in running ambulances with warning lights and sirens, committing multiple resources to bring pulseless patients to trauma centers negatively impacts timeliness and resource availability needed to care for living patients with time critical illnesses.

We maintain that an evidence-based guideline for field termination of resuscitation of traumatic cardiopulmonary arrest remains appropriate, even with the perspective provided by Moore et al. in their study. We believe that further research on the utility of EDT should consider the EMS environment, its management capabilities, the entire population of patients in traumatic cardiopulmonary arrest, and finally the needs of the community as a whole.

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REFERENCES

To the Editor:

W e read the interesting article by Elgafy et al. Management of femur neck nonunion is a tough challenge for an orthopedic surgeon. As described by the authors, two sorts of treatment can be offered to patients with femur neck nonunion; either joint conservation or reconstruction (hemiarthroplasty or total hip arthroplasty). Fibular grafting with revision internal fixation and intertrochanteric osteotomy are the joint preserving procedures, but with a doubtful outcome. In young patients, replacement surgery is not the first procedure of choice for femur neck nonunion.

As highlighted by the authors, the most important predictor of failure of internal fixation of displaced femur neck fracture is varus reduction and perceived difficulty in obtaining reduction. They recommended that valgus intertrochanteric osteotomy is the preferred method if there is femur neck nonunion with varus malalignment. However, the authors prefer nonvascular fibular graft with screw fixation for nonunion without varus...
alignment. Although the article describes the outcome of fibular allograft and autograft fixations in the femur neck nonunion, we are concerned about the fixation technique that the authors used for their patients.

The authors used two or three cannulated screws along with the nonvascularised fibular graft. Among 13 patients with autogenous fibular bone graft fixation, 4 had nonunion, and they all went for arthroplasty. The success rate for this procedure was 69.2%, and the mean time to union with autograft was 4.8 months. The fibular osteosynthesis had been first reported from our institute by one of our senior professor (Prof. ON Nagi). This procedure is one of the most commonly performed surgeries for femoral neck nonunion in young patients in my institute. We have evolved over time; and as per our experience, the cannulated screw fixation does not provide adequate stability to the fracture site. The cannulated screw is a round, cylindrical, metallic implant, and the threads purchase the bone, providing stability. In nonunion of the neck of the femur, the bone is relatively osteoporotic because of disuse for a long time. The purchase of the screws in this weakened bone is poor. Hence, a shear force is exerted over the fibular graft, and we believe this is the cause of failure of fibular autograft fixation in these cases. The fibular autograft cannot serve both the purposes of fixation and osteogenesis adequately.

We believe that an angle fixed device along with fibular graft is a better method of fixation. An extra cannulated screw can be put if space is available. Till date, we have operated 20 cases of the femur neck nonunion with angle blade plate and fibular autograft. Other than one nonunion, we have achieved union in all the cases (95% success rate) after a mean period of 4 months. In this fixation, the sole purpose of fibula is “osteogenesis”. The angular blade plate (of angle 120 or 130) provides rigid stability because of its construct and fixations to the femoral shaft. The cannulated screw is supplementary to the fixation to form a rigid construct. The inherent biological activity of autogenous fibular graft and its capacity to activate the surrounding host tissues to relevant biological activity promote osteogenesis at the nonunion site. As described by the authors, it also supports the ingrowth of host tissue. Initial radiographs show union of the fibular graft at both ends, and subsequently, ossification at the fracture site is observed. With time, the new bone gets consolidated, and the graft is completely incorporated.

The Figure 1 is the radiograph of a patient after 5 years of fibular autograft fixation. The index case was operated with the above mentioned procedure after failure of a fibular graft with cannulated screw fixation for his neck of femur nonunion. Thus, the technique of angle blade plate and fibular graft fixation broadens the indications of hip preserving procedures in cases of femoral neck nonunion. We have observed successful outcome even in mild varus alignment and in a gap of 1.5 cm. However, in these cases, the nonunion site should be freshened first and then both the surfaces should be collapsed. Further procedure involves fibular grafting and angle blade plate fixation. We believe that fibular graft with angle blade device may be a suitable construct for femoral neck nonunion that provides a better biology and mechanical strength.

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**REFERENCE**

**Reply to the Letter to the Editor**

**In Reply:**

We read with interest your letter regarding our recently published article titled “Revision internal fixation and nonvascular fibular graft for femoral neck nonunion.” In your letter, you expressed a concern regarding the fixation technique that we used. This involved the use of two or three cannulated screws for the internal fixation along with the nonvascularized fibular graft.

We thank you for the letter and sharing your institution’s experience in such a difficult problem in young adults. We agree with your assessment that a fixed angle device is a biomechanically stronger method of fixation than cannulated screws. However, the use of the fixed angle device is a more challenging technique and we preserve using the fixed angle device in cases with varus malalignment that requires valgus intertrochanteric osteotomy.

In our patients, we did not encounter any problem with the screws’ purchase likely related to the young age and early diagnosis and management of the nonunion. The average age at the time of the index procedure in our study was 46 years (range, 24–58 years). The mean time interval from...
injury to index procedure was 8 months (range, 4–22 months). Certainly, in cases with late diagnosis, inadequate purchase of the screws may be encountered secondary to disuse osteopenia and in such a case, one has the option of using a fixed angle device for the internal fixation or electing to use hemiarthroplasty or total hip arthroplasty.

We think that femoral neck non-union in young adults represents a challenging problem. Treatment with fibular bone graft and revision internal fixation with cannulated screws or fixed angle device, before proceeding to a hemiarthroplasty or total hip arthroplasty, is an alternative option in selected patients.

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REFERENCE

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ERRATUM

Prehospital Hypotension in Blunt Trauma: Identifying the “Crump Factor”: Erratum

In a manuscript in the May 2011 issue of The Journal of Trauma it should have been noted that the paper was presented as a Podium Paper at the 2008 Western Trauma Association meeting February 24–March 1, 2008 at Olympic Valley, California.

REFERENCE