to find the patient lying on the floor, high up in a steeply banked seating area under a row of folding seats. Cardiopulmonary resuscitation had been commenced and his trachea had been intubated by a paramedic. Cannulation of a peripheral vein proved impossible and central venous access was not a feasible option in the circumstances. The decision was therefore made to give adrenaline 2 mg via a tracheal tube, which was given in 20 ml (two “minijet” syringes). A pulse became palpable and the patient’s colour was restored quite rapidly after this and he became stable enough to be transferred to the ITU of a local hospital, where he recovered sufficiently to be discharged home 2 weeks later.

This experience suggests that there is still a place for keeping intra-tracheal adrenaline in the resuscitation guidelines even if only as a final resort for those involved in atypical arrest situations.

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Blind nasotracheal intubation and trauma victims

Sir,—We read with interest the paper by McHale and colleagues [1]. Blind nasotracheal intubation may indeed be a difficult technique to master and we use oral intubation with inline stabilization as a first-line approach in the emergency situation. However, we endorse the 1993 Advanced Trauma Life Support (ATLS) guidelines which allow the exercise of personal discretion in deciding the most appropriate route for intubating the trachea of a trauma victim. However, we feel that the results of McHale and colleagues do not support their conclusions and recommendations. Their design tests first the ability of the ATLS programme to teach a specific psychomotor skill and second the ability of 15 individuals to retain and perform that skill. It does not test the effectiveness of the technique of blind nasotracheal intubation. If the ability of the same 15 individuals to intubate via the oral route had been assessed under the same experimental conditions, it is conceivable that the success rate of correct tracheal tube placement would have proven to be as low as that described for intubation via the nasotracheal route. We feel that the authors are not justified in concluding from their results that “in British hospitals, blind nasotracheal intubation should not be recommended as the first-line management in securing the airway of patients with suspected or proven cervical spine injury” [2]. The North American Emergency Medicine literature is replete with studies attesting to the safety and effectiveness of blind nasotracheal intubation in securing the airway of trauma victims.

In the absence of any supporting scientific or clinical data, the authors then proceed to make a number of recommendations regarding the management of the airway in injured patients. Specifically, the authors appear to be advocating the use of the laryngeal mask airway (LMA) in the hospital setting by non-anasthetists unable or unwilling to intubate the trachea of trauma victims in their care. We are currently involved in a number of studies designed to evaluate the role of the LMA in the airway management of the injured patient. We are unaware of any clinical data which would support its use in hospitals as a first-line airway adjunct in trauma resuscitation. The LMA may have theoretical advantages in this group of patients under specific circumstances. However, as yet there are inadequate clinical data to support such a sweeping recommendation.

The reality is that any hospital in the UK that receives the severely injured patient is likely to have an anaesthetist available in the accident and emergency department within a few minutes. The pre-eminent role of the anaesthetist in trauma resuscitation in the UK (in stark contrast to the USA) is likely to persist and render the considerations and concerns expressed by McHale and colleagues of solely theoretical interest.

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Sir,—Thank you for the opportunity to reply to Drs Sasada and Gabbott. Unfortunately they seem to have missed the point of our findings.

A working party from the Royal College of Surgeons reported in 1988 that the management of trauma patients that reach hospital in this country is suboptimal [1]. Since 1988, Advanced Trauma Life Support (ATLS) courses have been established in the UK. Although these courses are attended by anaesthetists, the majority of doctors taking these courses are from surgical specialties or accident and emergency departments. The first manual which accompanied the course from 1988 to October 1993, taught that in the breathing but unconscious patient with either proven or suspected cervical spine injury, intubation should be performed using a blind nasotracheal technique [2]. This was the teaching at the time of our study. As Drs Sasada and Gabbott have noted, our study was indeed designed to test the ability of the ATLS course to teach this skill and the ability of the individuals to retain the skill. Our results suggest that the technique was either inadequately taught or learned, but certainly not a successful method of oxygenating the lungs of these particular patients. We have no doubt that blind nasotracheal intubation is an effective method for providing oxygen to the lungs, but only if the nasotracheal tube is inserted into the correct site. We dispute the assertion that “the North American Emergency Medicine literature is replete with studies suggesting that this route is superior to our own, and note that none has been quoted. Further, Drs Sasada and Gabbott report that the method used in the Maryland Shock Trauma Centre is in fact oral intubation with inline cervical stabilization; presumably because its success rate is higher than blind nasal intubation.

We felt that it was important to make alternative suggestions for oxygenation of the lungs of trauma patients having criticized one particular technique. The available options are bag-and-mask ventilation, insertion of a laryngeal mask airway, blind nasal intubation, orotracheal intubation with inline cervical stabilization, fibreoptic nasal intubation or creation of a surgical airway. We concluded that it was up to the individual doctor to decide, in the light of his own experience, what is the most effective method of airway control.

We agree that in the UK there is likely to be an anaesthetist readily available to manage the airway. However, the enthusiasm of some ATLS course graduates to practice their new skills [McHale S., unpublished observations] could lead some to attempt a technique with which they are unlikely to be successful.

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Plasma endotoxin-like activity during cardiopulmonary bypass

Sir,—The recent short communication by Inaba, Kochi and Yorozu [1] concluded that methylprednisolone suppressed increases in plasma concentrations of endotoxin-like substances induced by cardiopulmonary bypass, as measured by a conventional limulus amoebocyte lysate (LAL) assay.

In their discussion, the authors failed to mention an earlier study by Andersen and colleagues [2], which showed that concentrations of endotoxin increased significantly after administration of methylprednisolone during cardiac surgery involving cardiopulmonary bypass, and concluded that steroids delayed the clearance of endotoxin from the circulation. This earlier study also used a conventional LAL assay for endotoxin.

[Reference citations are not visible in the text.]

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