be considered in those patients presenting to an A&E department with features suggestive of migraine.

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The authors reply
The letter from Gupta and Moalypour further illustrates that carotid artery dissection is not as uncommon as had been thought.1 The range of symptoms recognised as being due to the condition and the diagnosis will no doubt increase as doctors become aware of it and imaging techniques and access to them improve. At present we are still learning the true incidence and the natural history of this challenging condition.

Minor injuries units

EDITOR,—Mabrook and Dale's paper on the minor injuries unit in Horsham will doubtless be cited as further evidence of the viability of such facilities.2 However, closer inspection of their data suggests otherwise.

Firstly, we know that 50% of patients attending minor injuries units could have either self treated or seen their general practitioner (GP).3 This implies that only 3472 of the Horsham patients had a significant injury. Of these, 1342 had to be seen by the accident and emergency (A&E) consultant, 234 were referred to the major A&E unit, and 93 were referred to the ear, nose, and throat and ophthalmology departments. This leaves just 1803 appropriate patients who were treated by emergency nurse practitioners (ENPs) during the 12 month period. This equates to 3.5 patients per nurse per working day. Hardly an efficient use of experienced nurses.

Secondly, the paper talks about quality but fails to say whether the ENPs accurately managed soft tissue injuries or whether the antibiotics prescribed were appropriate. Nor are we told how many ENP patients later self referred to their GP or to the major A&E unit. Nor does the planned readmission rate reported (23%) suggest a particularly efficient or confident department.

As the pressure to close small and medium sized A&E units continues, more and more communities will be offered minor injury units instead. The public should understand that such units are both underused and inefficient of resources.

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The authors reply
The aim of the paper was to evaluate whether an experienced trained nurse can treat minor injuries and allocate them to a minor injury unit and not to justify the existence of such units. However, if healthcare trusts decide to commission a unit the year's study has shown that ENPs can be used to provide a successful alternative service.

The points raised by Mr Leaman are arguments that can be used against the existence of minor injuries units, which might well be valid, however this is not what the authors intended to raise in this paper.

All patients who attended the unit were initially assessed and treated by the ENPs. Patients who were referred to the consultant were patients who required follow up and would have been referred even if they had been treated by a casualty officer. It is true that a high number of patients were reviewed in the unit. This is because the consultant has an interest in the management of the common fractures that do not require orthopaedic intervention.

In order to monitor the ENPs' work during the year of evaluation, all the patients' notes were reviewed by the consultant to ascertain whether patients had been diagnosed correctly and treatment had been carried out according to protocols set. Reviews of patients served to monitor the effectiveness of the treatment given by the ENPs.

As to not having kept a record of how many patients treated by the ENPs then self referred to a GP or an A&E department... Is it possible to keep records of this without a national integrated monitoring system? Patients self refer for second opinion all the time regardless of where they have been initially treated.

Risk of fire outweighed by need for oxygen and defibrillation

EDITOR,—We read with interest that Cantello et al from St George's Hospital have reported part of an experiment we conducted (at the same institution) examining ambient oxygen concentrations in the cardiac catheterisation unit, and the use of cardiopulmonary resuscitation.1 Unfortunately, it is unclear exactly where their gas samples were taken. They state that “the oxygen level below the manikin on the trolley surface did rise from 22% to 28%.” 28% is far below the “reservoir valve” but does not define sampling points nor the time course of the experiment.

This lack of detail may be responsible for Dr Ward's supposition (in his comments attached to the letter) that Cantello et al measured 28% oxygen concentrations at the axilla. This would be, indeed, a potential hazard as this is a standard paddle position during defibrillation.

We demonstrated a risk of raised oxygen concentrations in areas where oxygen (which is heavier than air) can pool, notably the axilla, when a disconnected ventilation device (Waters' bag, self inflating bag, and intensive care ventilator) is left resting on the pillow. Oxygen concentrations were not raised if the breathing systems were left connected to the manikin or were removed to a distance of greater than 1 m behind the head. The aetiology of a United Kingdom “that the breathing system be disconnected from and distanced from the patient” is sound if the distance be specified as greater than 1 m.

Although ENPs' positions are not supported by the findings of Cantello et al we feel that, in the case of a disconnected and not adequately distanced breathing system, they do apply. It is a simple thing to move the source of oxygen away from the patient in the accident and emergency department. In the intensive care unit the problem of breathing systems may present more complex problems, it may be safer to leave the patient attached to the ventilator.

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The authors reply
We were pleased to find that McAnulty and Robertshaw's work regarding oxygen concentration during simulated cardiopulmonary resuscitation confirms our research, and are grateful for the opportunity to discuss our methodology in greater detail.

With the manikin and ventilation bag set up as explained in our previous letter, the oxygen level recorded at the sternum and apex paddle positions and also at the mouth remained at 21%. Each position was observed for 10 minutes and the oxygen reading was noted to stabilise after two minutes. Therefore, with the anaesthetist raised above the trolley around the manikin's anterior, there is no change in oxygen concentrations.

Referring to both our earlier letter on the subject and supporting letter by Dr Ward,1 the real debate is not how far to remove the ventilation bag before defibrillation but whether one should remove the oxygen at all before defibrillation. Which is the greater risk? The risk of fire due to a defibrillator over an oxygenated area, or the risk of dislodging the endotracheal tube, reducing oxygen flow to a patient in dire need, and delaying life saving defibrillation?

As a training issue, we concur with Dr Ward, “awareness of the problem is likely to reduce the incidence [of fire],” and Robertshaw and McAnulty, “it is most important to avoid dislodging correct placement of paddles and electrolyte pads before defibrillation...” 2

In summary, the risk of fire is remote in properly performed defibrillation; the risks to the patient caused by taking the time to remove the oxygen, and the possibility of dislodging the endotracheal tube before defibrillation are too great. Whether in the accident and emergency department or the intensive care unit, we reaffirm our belief that oxygen should not be removed before defibrillation.


Child Protection Register—time for change

EDITOR,—The Child Protection Register neither protects children nor is it a good register. It is used in accident and emergency (A&E) departments across the country several thousand times a day as an investigation. What is the sensitivity and specificity of this test, what positive or negative predictive value has it got?

Many, if not all, A&E department clinicians

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Risk of fire outweighed by need for oxygen and defibrillation.

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