LETTERS TO THE EDITOR

Intramuscular or intravenous adrenaline in acute, severe anaphylaxis?

EDITOR,—The consensus guidelines on the emergency medical treatment of anaphylactic reactions by the Project Team of the Resuscitation Council (UK) are an excellent guide for first medical responders, whether general practitioners or emergency department staff.[1] They are pragmatic, safe, and emphasise the importance of first line treatment with oxygen, adrenaline (epinephrine) and fluids, and as Hughes and Fitzharris in their *BMJ* editorial suggest, rightly deserve to " . . . adorn the walls of emergency departments, general practitioners' surgeries, and outpatient clinics . . .".[2]

The guidelines usefully remind us that a panic attack or a vasovagal syncopal episode may be confused with anaphylaxis with the danger of inappropriate treatment. Additional differentiating features not mentioned in the text that suggest a faint rather than anaphylactic collapse are the rapidity of onset, maintenance of a central pulse, and prompt response to the recumbent position.[3 4]

It is refreshing to see the debate over the delivery of adrenaline move forward a stage, with the subcutaneous route no longer recommended as the absorption is delayed and variable, at least in well children with a history of systemic anaphylaxis, when compared with the intramuscular route.[5] The guidelines thus quite correctly favour the early administration of intramuscular adrenaline at a dose of 0.5 ml of 1:1000 for adults, to all patients with clinical signs of shock, airway swelling, or definite breathing difficulty. Intramuscular adrenaline given early, or when venous access is difficult and if the patient is unmonitored, is safe and effective even in less experienced hands.

Now is the time to also reappraise some of the perennial dogma that limits the use of intravenous adrenaline in acute, severe anaphylaxis. The introduction's criticism of the inappropriate use of intravenous adrenaline, singling out accident and emergency departments is unjustified. It is presumably based on rare, outdated reports that include a questionnaire circulated to junior resident staff showing that in the 10% who responded there was misunderstanding about the dosage, route, and dilution of adrenaline,[6] and a purely anecdotal letter castigating the use of intravenous adrenaline as it is "…a hazardous procedure rarely warranted in anaphylactic reactions"![7] As has been pointed out before, published reports on the apparent dangers of intravenous adrenaline consistently fail to emphasise that other causes such as hypoxia, hypotension, or the direct actions of the inflammatory mediators themselves released during anaphylaxis may be responsible for the cardiovascular complications. In addition, adverse outcomes to adrenaline occur when it has been given too rapidly, inadequately diluted, or in excessive dosage.[8]

Also, the statement that intravenous adrenaline should only be reserved for profound shock or special indications, for example during anaesthesia, is perplexing. Seventy five per cent of anaphylactic deaths are due to asphyxia from upper airway oedema and hypoxia from severe bronchospasm, many within the first hour of onset of symptoms and only 25% from circulatory collapse.[8] In addition, there is nothing unique about anaphylaxis during anaesthesia other than the standard mode of delivery of all drugs is intravenously, and thus unexpected anaphylactic reactions may be rapid and catastrophic.

However, many of the same drugs are also used regularly in emergency medicine departments and intensive care units to facilitate interventional airway care. Continuous electrocardiographic, blood pressure, and pulse oximetry monitoring are now routine in all these areas and are a requirement for the safe use of intravenous adrenaline. As Hughes and Fitzharris point out, the guidelines are somewhat vague in empirically recommending repeated intramuscular adrenaline every five minutes, or intravenous adrenaline as slowly as seems reasonable in the absence of clinical improvement or if deterioration occurs, particularly with profound shock.[2]

Emergency medicine has come a long way in the last decade and now includes a much higher level of senior supervision, routine access to the minimum standards of monitoring suggested above, and widespread collective expertise in managing anaphylaxis. It is now time to consider that in any monitored area, whether the emergency department, intensive care unit, or high dependency unit, in experienced hands low dose, high dilution intravenous adrenaline is the optimal care for a patient with severe or rapidly progressive, life threatening anaphylaxis whether from shock or acute respiratory distress. Administration of 0.75–1.5 µg/kg of 1:100 000 adrenaline intravenously at 1 ml or 10 µg per minute reverses all the life threatening effects of anaphylaxis and inhibits further mast cell mediator release by raising intracellular cyclic AMP.[8] The 1:100 000 dilution, as the guidelines suggest, not only allows precise titration to response, but avoids inadvertent rapid or excessive dosage. In addition, the therapeutic response to the adrenaline is immediate and assured.

The Project Team of the Resuscitation Council (UK) must be congratulated on an important document that will undoubtedly improve first responders' knowledge and outcome, and indeed be of benefit to us all.

ANTHONY F T BROWN
Staff Specialist and Clinical Associate Professor, Faculty of Medicine, University of Queensland, Department of Emergency Medicine, Royal Brisbane Hospital, Queensland 4029, Australia
e-mail: brownaft@health.qld.gov.au

1 Project Team of the Resuscitation Council (UK). Emergency medical treatment of anaphylactic reactions. *J Accid Emerg Med* 1999;**16**:243–7.
2 Hughes G, Fitzharris P. Managing acute anaphylaxis. *BMJ* 1999;**319**:1–2.
3 Canada Communicable Disease Report. Anaphylaxis: statement on initial management in non-hospital settings. *Can Med Assoc J* 1996; **154**:1519–20.
4 Department of Health, Welsh Office, Scottish Office Department of Health, DHSS (Northern Ireland). *Immunisation against infectious disease*. London: HMSO, 1996: 37–43.
5 Simons FE, Roberts JR, Gu X, *et al.* Epinephrine absorption in children with a history of anaphylaxis. *J Allergy Clin Immunol* 1998;**101**:33–7.
6 Gupta S, O'Donnell J, Kupa A, *et al.* Management of bee-sting anaphylaxis. *Med J Aust* 1988; **149**:602–4.
7 Roberts-Thomson P, Heddle R, Kupa A. Adrenaline and anaphylaxis. *Med J Aust* 1985; **142**:708.
8 Brown AFT. Therapeutic controversies in the management of acute anaphylaxis. *J Accid Emerg Med* 1998;**15**:89–95.

Medical treatment of anaphylaxis

EDITOR,—We have read with grave concern the project team's recommendations for the medical treatment of anaphylaxis[1] and believe very strongly that the advice against using intravenous adrenaline (epinephrine) is potentially very dangerous. We also find the omission of reference to guidelines for the management of anaphylaxis in the accident and emergency (A&E) department published in the same journal[2] as very regrettable if deliberate, or puzzling if the project team had no knowledge of their existence.

The project team's guidelines have also failed to emphasise the relevance of grading the severity of anaphylaxis and that its treatment should be directed to the severity of the attack encountered.

We agree that the project team's guidelines should be used by the inexperienced and invariably pre-hospital responders. We also agree that the subcutaneous route is unreliable and should be abandoned. However, to suggest that A&E seniors or supervised trainees and well supported juniors lack clinical credibility to administer high dilution intravenous epinephrine carefully titrated against response in the fully monitored patient in the resuscitation room is insulting to the specialty of A&E. It also shows that in spite of having A&E representation the project team fails to understand fundamental principles of A&E involvement in the management of the critically ill.

To suggest that patients with clinical signs of shock should be administered intramuscular epinephrine as epinephrine can be rapidly absorbed is in physiological terms most bizarre advice.

We conclude that the project team's guidelines need urgent revision as they will lead to patients dying due to failure to urgently administer intravenous epinephrine. We will continue, as we hope the majority of A&E departments will do similarly, to use the published A&E guidelines[2] and we believe that they are currently the best available guidelines for treating anaphylaxis in the A&E department.

M GAVALAS
Consultant in Emergency Medicine
C WALFORD
Consultant in Primary Care/Accident and Emergency, University College Hospital, Grafton Way, London WC1E 6AU
A SADANA
Consultant in Emergency Medicine
C O'DONNELL
Consultant in Emergency and Intensive Care Medicine, Whipps Cross Hospital, Leytonstone, London E11 1NR

1 Project Team of the Resuscitation Council (UK). Emergency medical treatment of anaphylactic reactions. *J Accid Emerg Med* 1999;**16**:243–7.
2 Gavalas M, Metcalf S, Sadana A. Guidelines of the management of anaphylaxis in emergency department. *J Accid Emerg Med* 1998;**15**:96–8.

Professor Chamberlain replies on behalf of the Anaphylaxis Project Team

We are grateful to Dr Brown for his kind general remarks about our consensus guidelines, and also for giving us the opportunity to clarify one sentence in our introduction. We said "There has been a vogue for inappropriate use of intravenous epinephrine (adrenaline), both by paramedics and in accident and emergency departments, when epinephrine

(adrenaline) should have been given intramuscularly". We believe this to be true, but it was most certainly not our intention to condemn all use of intravenous adrenaline by experienced medical practitioners either in emergency departments or elsewhere. In retrospect we see that we should have been more explicit on this point. We agree totally with Dr Brown's statement that adverse outcomes for adrenaline occur when it has been given too rapidly, inadequately diluted, or in excessive dosage. We also recognise that emergency medicine has progressed a long way in the last decade with a much higher level of senior supervision and greater possibilities for treatment under monitored conditions.

Our guidelines were intended specifically for those first medical responders who are inexperienced in the management of this emergency. They are unlikely to have monitoring facilities available. For these, cautious recommendations are appropriate with intravenous use of adrenaline restricted to emergencies judged to be immediately life threatening.

The examples that we gave for indications for intravenous adrenaline were clearly not intended to be comprehensive, and experienced physicians in monitored areas will appropriately make their own decisions. We did mention the value of infusions of 1:100 000 adrenaline which permits titration of dose against need. It may also be worth emphasising that in asphyxia from upper airway oedema and hypoxia from severe bronchospasm, there are additional priorities—not replacing parenteral adrenaline—such as oxygenation and nebulised inhaled bronchodilators. Dr Brown's comments do, of course, add to our own guidelines by making sound recommendations for expert management that was outside the remit of our article.

The concerns of Mr Gavalas and his colleagues were similar, but there seems also to be an element of misreading of our document. They say that they "believe very strongly that the advice against using intravenous adrenaline (epinephrine) is potentially very dangerous". We must reiterate that we did not advise against its use, but urged only that it should be used in the most serious cases and by experienced clinicians.

Our guidelines stated in paragraph 4.4 that:

"Intravenous epinephrine (adrenaline) in a dilution of at least 1:10 000... is hazardous and must be reserved for patients with profound shock that is immediately life threatening and for special indications".

We also added in paragraph 5.2:

"The use of epinephrine (adrenaline) by the intravenous route in the special circumstances given in paragraph 4.4 should usually be reserved for medically qualified personnel who have experience of it, who know that it must be administered with extreme care, and who are aware of the hazards associated with its use".

The footnote to the legends state very clearly:

"Consider slow intravenous (IV) ephinephrine (adrenaline) 1:10 000 solution. This is hazardous and is recommended only for an experienced practitioner who can also obtain IV access without delay".

There are some practitioners who have made the habit of always using adrenaline intravenously, while others have preferred the subcutaneous route, and many are afraid to give it at all. We believe that we have given the correct emphasis—that intramuscular adrenaline is the norm for the emergency treatment by first medical responders, with IV adrenaline reserved for special and life threatening situations. This is far from advising against its use!

There is one charge to which we must plead guilty. Of course we were aware of the previous paper published in the *Journal of Accident & Emergency Medicine* in 1998, and it was indeed our intention to reference it together with the other specialist recommendations. That omission was not deliberate, and one of us (DAC) must take responsibility for that important last minute oversight.

We do not accept that the guidelines need urgent revision. Neither does the Project Team with its wide representation accept that the recommendations as they stand are insulting to the specialty of accident and emergency. We are conscious that we all have the same aims: better and safer treatment of an important medical emergency.

Future inpatient management of patients with minor head injuries

EDITOR,—We read, with interest, the letter from Pau and Buxton, regarding the need for neurosurgical referral of patients with an admitting diagnosis of minor head injury.[1] We agree that these patients are a low risk group. We performed an audit of patients admitted to our observation ward with the primary diagnosis of minor head injury. From October 1997 to July 1999, 668 such patients were admitted under our care. Of these patients, only two were subsequently transferred to the regional neurosurgical unit after the finding of intracranial haematoma on computed tomography. This finding, and our general experience, leads us to agree that patients with an admitting diagnosis of minor head injury do not require neurosurgical referral, in the first instance. However, we suspect, given that only 12% of responding accident and emergency (A&E) departments (71% response rate) in the UK have on-site neurosurgical facilities,[2] this practice is not widespread anyway.

Pau and Buxton's conclusion is in keeping with the recommendations of the *Report of the Working Party on the Management of Patients with Head Injuries*.[2] One of the logistical concerns raised by this report, and highlighted in Pau and Buxton's letter, is the fact that observation wards have a finite capacity. Our current practice is that, when our observation ward is full, these patients are admitted under the general surgeons. *The Way Ahead* document recommends that A&E observation wards should be within, or immediately adjacent to, the A&E department.[3] We feel that it is inevitable, when the only specialty admitting patients with isolated minor head injuries is A&E, that problems will arise when the observation ward is full. It goes against the spirit of the report of the working party to then admit these patients under other specialties, just because the observation ward is full. If these patients are to be admitted to wherever there are beds within the hospital, but still under the care of A&E, we fear a major step backwards in the standards of our practice if, when a seriously ill or injured patient arrives in the A&E department at 5 am, the only A&E doctor on duty is at the far end of the hospital, assessing a head injured patient.

The recommendations of the report have provoked widespread debate and polarity of views within the specialty. It probably represents a watershed in the management of A&E departments that do not currently accept inpatients under the care of A&E consultants. We do not yet know how we will resolve this issue locally, but would welcome other views on ways of managing the patient.

G MCCARTHY
G QUIN
Accident and Emergency Department, Royal Gwent Hospital, Gwent Healthcare NHS Trust, Cardiff Road, Newport NP9 2UB

1 Pau H, Buxton N. Management of minor head injuries by non-specialists. *J Accid Emerg Med* 1999;**16**:390.
2 Royal College of Surgeons. *Report of the working party on the management of patients with head injuries.* London: Royal College of Surgeons, 1999.
3 British Association for Accident and Emergency Medicine. *The way ahead.* London: British Association for Accident and Emergency Medicine, 1998.

Cycle helmets

EDITOR,—I read with horror the British Medical Association's Board of Education and Science conclusion that legislation to make helmets for cyclists compulsory would reduce the number of cyclists and not be in the interests of health.[1]

As an accident and emergency consultant for over 20 years, I have seen far too many head injuries in cyclists, some fatal. The main factor in causing most accidents is human error. I agree, as the article suggests, that cycling proficiency should be taught in all schools and the driving test modified for awareness of cyclists to other road users. The government, as suggested, should subsidise cycle helmets and promote them through the media by advertising. The car driver is not interested, but we need better cycle routes nationwide.

Our government is committed to reduce injuries from accidents (see *Our Healthier Nation*). The main cause of death in children is trauma. But I have personally discussed at length the benefits of wearing a helmet with children who, unhelmeted, have suffered a fractured skull in a cycle accident. Many children still remain unconvinced! Parental control is weak. So voluntary action to increase the wearing of cycle helmets in this age group is unlikely to succeed.

My answer is that the compulsory wearing of cycle helmets is needed now. This should be introduced as part of an overall cyclist's programme—teaching, better routes, helmet subsidies, and increased awareness through publicity. The number of cyclists may well reduce initially but more importantly head injuries will become fewer too! However, in the future, the population of cyclists naturally will then increase as cycling at last becomes safer and so even more enjoyable.

There has been successful government legislation concerning road traffic accident prevention and injury protection—for example, the breathalyser laws, compulsory crash helmets for motorcyclists, and the seat belt laws. These are still supported by publicity. The legislation could be expanded.

We as an emergency specialty, however, have a responsibility in accident prevention and injury protection. We all need to be active, as I have been, in the local press recently highlighting, for example, the need for cyclists to wear helmets. We recognise this problem only too well.

Our department has a nurse who travels round schools talking about accident prevention and injury protection—particularly cycle

helmets. Such programmes need to be national not local. Government money is available from the Health Improvement Programme. Have you put a bid in? We have.

Our specialty needs to wake up to its responsibility of prevention and be active. Prevention is the best treatment for any condition.

A FRASER-MOODIE
Consultant in Accident and Emergency, Derbyshire Royal Infirmary NHS Trust, London Road, Derby DE1 2QY

1 Carnall D. Cycle helmets should not be compulsory. *BMJ* 1999;**318**:1505.

Playing in the back seat

EDITOR,—We read with interest the letter of fatal intra-abdominal injury associated with incorrect use of a seat belt and would like to present another aspect of seat belt misuse.[1] A pair of 7 year old identical twins presented to the accident and emergency (A&E) department after being involved in a head-on road traffic accident. Both had been restrained in four point child seats in the back of the car.

In the department, one was complaining of abdominal pain, the other none. On examination of the twins, both were haemodynamically stable. Twin 1 had bruising to her abdomen, the other none. This bruising was linear in nature across her abdomen, "the seat belt sign". She had no focal tenderness.

In view of the seat belt sign, twin 1 was admitted and an abdominal ultrasound was performed at this stage. The ultrasound showed a small amount of fluid in the pouch of Douglas, but no other free fluid or lacerations of visceral organs.

Overnight she remained painful, but haemodynamically normal. A further ultrasound was performed the next day which showed free fluid within her abdomen.

A laparoscopy was performed that confirmed turbid fluid and this was converted to a laparotomy. Three lesions were found in her jejunum, one full thickness and two partial. A 15 cm resection of her jejunum was performed with anastomosis. She made an uneventful recovery.

On detailed questioning while in the A&E department, she stated that she had been playing a game with her twin sister and had wriggled out of her shoulder straps. This had converted her four point child seat into a lap belt. Twin 2 had remained properly restrained in her child seat.

Lap belt use has been recognised as a mechanism of blunt injury to the small bowel. The seat belt sign is associated with an increased likelihood of intestinal injury.[2] This lap belt injury in children usually occurs when children are to large for their safety seats and too small for adult seat belts.[3]

This case highlights the importance of the seat belt sign, but more importantly that children within their own safety seats need to be restrained properly within them. Incorrect usage results in injury similar to adult lap belts.

MARK G JENKINS
Specialist Registrar in Accident and Emergency
M RAO
Senior House Officer in Surgery, Altnagelvin Hospital, Londonderry, Northern Ireland BT47 6SB

1 Dawson LK, Jenkins NH. Fatal intra-abdominal injury associated with incorrect use of a seat belt. *J Accid Emerg Med* 1998;**15**:437–8.
2 Chandler CF. Lane JS. Waxman KS. Seat-belt sign following blunt trauma is associated with increased incidence of abdominal injury. *Am Surg* 1997;**63**:885–8.
3 Hendey GW, Votey SR. Injuries in restrained motor vehicle accident victims. *Ann Emerg Med* 1994;**24**:77–84.

Oesophageal rupture

EDITOR,—Drs Onyeka and Booth present an interesting case of tension pneumothorax associated with Boerhaave's syndrome.[1] I have previously described a similar case in a 47 year old man who survived his ordeal in 1996.[2] As the authors note, awareness of the condition is the mainstay of diagnosis.

Aside from these two cases, tension pneumothorax has been described in association with rupture of a Barrett's oesophagus[3] and after rupture of an oesophageal diverticulum.[4] It is worth noting that by far the commonest cause of oesophageal rupture is after endoscopy. Regardless of the cause of rupture I agree with the authors that a gastrografin oesophogram is the diagnostic procedure of choice.

Conservative management of oesophageal rupture is now a well established option in iatrogenic rupture and has been described in Boerhaave's syndrome,[5 6] although as the authors note, immediate surgery comprising drainage and repair is the mainstay of curative treatment.

STEVEN DOHERTY
Emergency Physician, Tamworth Base Hospital, PO Box 83, Tamworth, NSW 2340, Australia

1 Onyeka WOC, Booth SJ. Boerhaave's syndrome presenting as tension pneumothorax. *J Accid Emerg Med* 1999;**16**:235–6.
2 Doherty S. Boerhaave's syndrome complicated by tension pneumothorax. *Emergency Medicine* 1996;**8**:28.
3 Matsumoto MA, Rockoff SD, Aaron BL. Tension pneumothorax. Rare presentation of ruptured Barrett's oesophagus. *Chest* 1993;**103**:1604–6.
4 Rigg KM, Walker RW. Tension pneumothorax secondary to ruptured oesophageal diverticulum. *Br J Clin Pract* 1990;**44**:528–9.
5 Smyth AR, Wastell C. Spontaneous rupture of the oesophagus (Boerhaave's syndrome): delayed diagnosis and successful conservative management. *J R Soc Med* 1989;**82**:498.
6 Troum S, Lane CE, Dalton ML. Surviving Boerhaave's syndrome without thoracotomy. *Chest* 1994;**106**:297–9.

Carbon monoxide poisoning

EDITOR,—I read with interest the article by Turner *et al*, providing an update on carbon monoxide poisoning.[1] The authors correctly point out that hyperbaric therapy remains controversial, and that no controlled clinical trial had been conducted comparing hyperbaric oxygen (HBO) with normobaric oxygen (NBO). Since the date of acceptance of their paper a prospective, blinded, randomised trial comparing NBO with HBO has been published.[2] This trial also included severely poisoned patients and incorporated sham treatments. This study found in 191 patients that three days of HBO (2.8 atmospheres for 60 minutes) offered no advantages compared with three days of NBO (100 minutes of 100%). Another study is continuing in the United States and the interim results[3] have found no difference in the incidence of persistent neurological sequelae between those treated with HBO compared with NBO, although there is an increased incidence of delayed sequelae in one of the blinded treatment arms.

The authors also recommend careful neurological and cognitive re-examination. It is worth highlighting that cognitive testing in carbon monoxide poisoning is far from standardised. Many studies utilise different screening tests, different time intervals to re-screening, and different HBO regimens. This lack of standardisation makes it difficult to compare studies and no doubt contributes to our inability to provide definitive recommendations in the management of carbon monoxide poisoning.

STEVEN DOHERTY
Emergency Physician, Tamworth Base Hospital, PO Box 83, Tamworth, NSW 2340, Australia

1 Turner M, Hamilton-Farrell MR, Clark RJ. Carbon monoxide poisoning: an update. *J Accid Emerg Med* 1999;**16**:92–6.
2 Scheinkestel CD, Bailey M, Myles PS, *et al.* Hyperbaric or normobaric oxygen for acute carbon monoxide poisoning: a randomised controlled clinical trial. *Med J Aust* 1999;**170**:203–10.
3 Weaver LK, Hopkins RO, Larson-Lohr V, *et al.* Double blind, controlled, prospective, randomised clinical trial in patients with acute carbon monoxide poisoning: outcome of patients treated with normobaric or hyperbaric oxygen—an interim report. *Undersea Hyperb Med* 1995;**22**(suppl):14.

The authors reply

We also read with interest the paper by Scheinkestel *et al*, which was published after acceptance of our article.[1] Scheinkestel's paper was accompanied by a detailed editorial which documented a number of criticisms that preclude implementation of its findings until further data are forthcoming.[2] Recommendations from poison information centres, as described in our article, have not been changed and still provide useful guidance on selecting patients for hyperbaric oxygen.

We believe that careful neurological examination, including specific testing of cognitive function, is vital in the management of patients. Physicians should use tests with which they are familiar and apply them serially to the same patient. Standardisation of formal cognitive testing for trials was beyond the scope of our clinically orientated article.

1 Scheinkestel CD, Bailey M, Myles PS, *et al.* Hyperbaric or normobaric oxygen for carbon monoxide poisoning: a randomised controlled trial. *Med J Aust* 1999;**170**:203–10.
2 Moon RE, DeLong E. Hyperbaric oxygen for carbon monoxide poisoning. *Med J Aust* 1999;**170**:197–9.

Evidence based and guideline based medicine

EDITOR,—Evidence based and guideline based medicine is justifiably emphasised in current accident and emergency (A&E) medicine practice. At St James's University Hospital in Leeds, the A&E trainees participate in regular evidence based critical appraisal sessions as part of education development, and such skills are assessed in our Faculty of Accident and Emergency Medicine exit examination.

One source of valued literature is Sackett *et al*,[1] particularly their appraisal cards on the validity, importance, and applicability of a particular type of study. Lacking a photographic memory, I put forward simple acronyms that have helped me to facilitate timely and efficient appraisal for everyday use when selectively scanning relevant journals. Preceding these specific acronyms is a "stand-

ards" acronym that follows Crombie's suggestion that standard questions should be used as a filter for all papers.[2]

I hope they are of use to fellow practitioners of evidence based medicine, and further suggestions will be gratefully received.

Standards
Stated aims?
Tests and measures appropriate?
Arithmetic (do the numbers add up?)
Null findings (were they considered?)
Design appropriate?
Appropriate statistics?
Relevance to your practice?
Different results from previous reports?
Sample size/power adequate?

Diagnosis
Diagnostic test needed?
Independent blind comparison?
Appropriate population?
Gold standard used regardless of test result?
Numerogram (2×2 table) constructable?
Odds (pre-test and post-test)/likelihood ratios important?
Sensitivity and specificity important?
Inferences possible?
Safe, cheap, and helpful?

Prognosis
Prospective study?
Representative sample?
Objective and blinded outcome criteria?
Groups adjusted for prognostic factors?
Numbers recruited and followed up adequate?
Outcomes likely?
Study findings precise?
Inferences possible?
Similar patients to your own?

Guidelines
Guidelines needed?
User friendly?
Identified risks and benefits?
Decision options clear?
Evidence based decisions?
Large variations in current practice?
Implementable?
NHS benefit?
Economical?
Safe?

Therapy
Trial ethically approved?
High recruitment and follow up?
Equal treatment and assessment in each group?
Randomised and how?
Appropriate population?
Potential benefits for patients?
End points applicable?
Absolute risk reduction/number needed to treat?

Systematic review
Systematic search strategy?
Randomised and relevant trials?
Each trial assessed?
Valid results in all trials?
Inconsistent populations or results?
Evidence of benefit via odds ratios/number needed to treat?
Was hypothesis satisfied by the review results?

THOMAS CARRIGAN
Specialist Registrar in Accident and Emergency,
On behalf of the St James's Accident and Emergency Medicine Trainees,
St James's University Hospital,
Beckett Street,
Leeds LS9 7TF

1 Sackett D, Richardson W, Rosenberg W, *et al. Evidence-based medicine: how to practice and teach EBM*. Edinburgh: Churchill Livingstone, 1997.
2 Crombie IK. *The pocket guide to critical appraisal*. London: BMJ Publishing Group, 1996.

Weekly web review

EDITOR,—A noteworthy web site exists for emergency department evidence based medicine enthusiasts. The Weekly Web Review in Emergency Medicine (www.wwrem.com/) is dedicated to the critical analysis of current clinical literature on topics relevant to the practice of emergency medicine. It has a useful rating system for each of the articles reviewed and an archive database. It dovetails with this journal's journal scan and has useful links, notably with the Centre for Evidence Based Medicine at Oxford. I recommend it as a useful adjunct to any emergency physician's continual professional development.

GAVIN LLOYD
Consultant in Accident and Emergency,
Accident and Emergency Department,
Bristol Royal Infirmary,
Bristol BS2 8HW