

PostScript

LETTERS

Diagnostic errors in an accident and emergency department

I commend the author for comprehensively investigating a complex area but found some important pieces of information missing in the study.¹ Firstly, there is no information regarding the total number of patients seen in the accident and emergency (A&E) department during the study period. This information would put into better perspective the number of patients (934) who had recorded diagnostic errors and would allow for more scientifically valid comparison of the findings of this study by other A&E departments. Secondly, there is no record of the number of cases where there was dispute over the diagnosis between A&E clinician and radiologist. Furthermore, it seems the author *alone* made the final decision regarding the diagnosis in such cases. This is a very subjective method of diagnosis with little scientific validity. Moreover, there is no information as to the specific diagnosis made and subsequent management of this group of patients. The management of this subset of cases is a dilemma for A&E clinicians and more information from the author on their management will be informative. Finally, hospital policy for reporting A&E radiographs changed during the study period. Did this have any effect on the number of diagnostic errors recorded? Data comparing the number of diagnostic errors before and after the change of policy to immediate reporting of radiographs would provide useful scientific evidence for radiologists to decide whether to give priority to A&E radiographs.

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Author's reply

I thank Dr Wakai for his comments. The total number of new patients seen over the four year period in which this study took place was 244 442. I have no record of the number of cases where there was dispute over the diagnosis between A&E clinician and radiologist, but the number was very small, and usually related to a radiology trainee, rather than a consultant radiologist. The subsequent management of patients in whom diagnostic errors had been made was left to the individual consultant and I have no specific data on this but it obviously varied with the severity of the diagnostic error and the circumstances in which the error was discovered. Clearly, if the diagnostic error was discovered when the patient reattended the A&E department, or a follow up clinic, it was dealt with there and then, but if an error was discovered by a radiological report, probably

most patients were sent an appointment to reattend one of the A&E clinics, though some patients would have been telephoned and asked to return immediately. For very minor errors, for example, minor avulsion fractures, the GP would have been informed that the patient would not have been advised to return.

The change in radiological reporting that occurred part of the way through the study was, of course, only one change that occurred over the four year period. There were also changes in staffing and as the idea behind the original collection of data was for continual quality improvement, the results of the study each six months led to changes in teaching, etc. For what it is worth, the incidence of diagnostic errors appeared to fall for the 12 months after the introduction of hot reporting, but subsequently rose again. It is difficult to attribute this completely to the change in radiological reporting. In addition, as the study notes, it proved very difficult to obtain details of every diagnostic error and the data are certainly incomplete. I am not sure that conclusions on the effectiveness of changing the radiological reporting system based on incomplete data would be scientifically valid.

Dr Wakai rightly states that diagnosis based on the opinion of a single person is not valid. To this must be added the difficulties in defining diagnostic error and the incompleteness of the data.

With a relatively low incidence of diagnostic errors, a study to accurately determine the incidence of these and to draw scientifically valid conclusions about their types, causes, etc, would require 100% follow up of many thousands of patients with all potential diagnostic errors being submitted to a panel to determine the exact diagnosis. Such a study would be very expensive and has never been done.

My study was, I hope, more than just "one consultant's experience of diagnostic errors he has encountered", as I actively tried to seek out all diagnostic errors as part of a quality improvement exercise. It must be regarded as a best attempt at determining all diagnostic errors for audit purposes but with no additional resources allocated. As such, I hope that it will be useful when discussing quality of service in A&E departments, but it did not

accurately define every diagnostic error that occurred over the four year period.

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Prehospital rapid sequence intubation

We read with interest the recent paper by Mackay and colleagues regarding the safety of prehospital rapid sequence induction by emergency physicians¹ and would like to make two observations. Despite grading more of the patients as Cormack-Lehane 1 and 2 (95% compared with 81.5% in the emergency physician group) the anaesthetists were still using the gum elastic bougie more often (60.4% versus 51.0%). The use of the Cormack-Lehane scoring system is not necessarily predictive of intubation difficulty. Pre-hospital evaluation of intubation in France has showed that glottic exposure alone is an incomplete reflection of the difficulty encountered. In fact using a seven point scoring system, the influence of glottic visualisation was only moderate when assessing the subsequent degree of difficulty of intubation.² Given that this is the case then should the use of an aid to intubation, such as the gum elastic bougie be part of the standard operating procedure for prehospital intubation? This may further reduce the number of repeat attempts at intubation, which the authors themselves comment as probably being under-reported in the study.

The authors also state that the laryngeal mask airway is not routinely carried. This is surprising given that, as an airway adjunct, while not providing protection from gastric aspiration, it may be available to provide oxygenation in circumstances where the provision of a definitive airway may be difficult. Its potential role in the prehospital setting should not be overlooked.³

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- 3 Sasada MP, Gabbott DA. The role of the laryngeal mask airway in pre-hospital care. *Resuscitation* 1994;28:97-102.

Authors' reply

We thank the authors of this letter for their comments. While we accept that simply grading the view at laryngoscopy is not the only factor predicting difficulty of intubation, it is convenient and well understood and may reflect potential problems.¹ We agree that a gum elastic bougie should be used as a routine to aid prehospital intubation.

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A laryngeal mask airway may certainly have a role as a backup device, but is not always easy to insert, particularly in the multiply injured patient requiring cervical stabilisation.² Comparative studies are required to determine the best approach to a failed prehospital intubation.

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Intranasal naloxone for life threatening opioid toxicity

Heroin overdose is a major cause of death in Western countries. Many lives are saved by the administration of naloxone by emergency department and ambulance staff. In Australia, there have recently been calls by drug and alcohol dependence agencies and coroners for the extension of this treatment to other emergency service and community workers. Parenteral administration of naloxone however has some problems. It entails administration by way of an injection, mandating training of personnel and secure storage of equipment. There is also risk of transmission of blood-borne diseases such as hepatitis C to the treating person by way of needlestick injuries.

Currently available pharmacology data suggest that naloxone has high bioavailability through the nasal mucosa, with onset of action and plasma bioavailability curves that are very similar to the intravenous route.¹ Work in the field of drug addiction has shown that intranasal naloxone is effective in detection of opioid dependence² and is as effective as parenteral naloxone for the reversal of opioid effects.³ To date, the intranasal administration of naloxone for the emergency treatment of opioid overdose has not been reported in the literature.

Six cases of isolated acute heroin overdose were treated with intranasal naloxone, in addition to ventilatory support, in the Department of Emergency Medicine of Western Hospital, Melbourne, Australia. All patients had return of adequate spontaneous respiration within two minutes, with a median of 50 seconds (table 1). Doses used ranged from 0.8 to 2 mg and were at the treating doctor's discretion.

If intranasal administration of naloxone could be shown in larger series to be effective and practical, there is the potential to extend

this treatment to a wide variety of community workers without the risk of needlestick injury and with minimal training. This may well translate into an increase in lives saved.

A prospective clinical trial comparing the effectiveness and safety of the intranasal route for administration of naloxone to the intramuscular route in the prehospital setting is planned to begin in December 2001.

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Anti-D immunoprophylaxis within the accident and emergency department

The debate on anti-D prophylaxis rages on. Recently the subject was discussed in a green top guideline from the Royal College of Obstetricians and Gynaecologists.¹ There are still approximately 50 deaths per annum attributable to rhesus isoimmunisation in the UK. In reviewing the reasons why these deaths still occur, the Consensus Conference on Anti D in 1997 admitted that the 1991 Recommendations are not being adhered to by all units and that a substantial proportion of accident and emergency (A&E) departments did not administer anti-D when appropriate (Consensus Conference on Anti-D Prophylaxis, Edinburgh, UK 8–9 April, 1997).

The conference discussed but did not conclude on the need for anti-D prophylaxis where threatened miscarriage and resolution occurs in the first trimester, or when spontaneous miscarriage occurs at this time without instrumentation. The College guidelines go further in advocating non-use of anti-D when pregnancy bleeding occurs in the first trimester with a viable fetus and supports the use of anti-D when "bleeding is heavy or repeated, when abdominal pain is present or when gestation approaches 12 weeks".

There is a need here for more precision. Many SHOs in A&E have limited gynaecological experience and under the new guidelines will be expected to determine which patients require anti-D.

Furthermore, the present recommendation for non-use of anti D is based largely on two observational studies, (Grade C recommendation). In this era of evidence based medicine is this sufficient basis for a change in policy?

In the past anti-D immunoprophylaxis was routinely given to all rhesus negative women

with early pregnancy bleeding. This has not been shown so far to be significantly associated with adverse side effects and the cost implications are not prohibitive.

Perhaps the way forward is shown in a more recent RCOG guideline, on the management of early pregnancy loss.² The same dilemma is dealt with in a caveat "if there is clinical doubt then anti D should be given". Until more conclusive information is to hand, rather than obfuscating the issue, a return to a policy of administering anti-D to all rhesus negative women with early pregnancy bleeding seems a more plausible option.

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Teaching and learning

We read with interest the paper by Dr Lockey describing the different learning approaches that may be taken by students.¹ We are aware that the field of educational psychology is woolly and littered with many definitions and it may be difficult to give a brief overview of learning approaches. The author has made a valid point in suggesting that as doctors we are expected to teach but are rarely trained in the teaching process. The author goes on to describe how there are essentially two learning approaches adopted by students: "surface" and "deep". We are then told how deep learning is superior to surface and that as educators we should attempt to promote deep learning.

This is fine. However, Dr Lockey has made an important omission in his paper. The author has failed to describe a third and very important learning approach. That is the "strategic" approach as described by Miller and Partlett.²

The strategic learner is a success driven person who approaches the learning process as a game where a high mark is the end point. These people will focus only on what they perceive to be relevant to exam success and disregard additional information. They may attempt exam prediction or even attempt to obtain inside information from authority figures. This approach results in poor long term recall and patchy subject knowledge. McManus *et al* have shown that medical students with the most clinical experience do not perform best in final exams but deep and strategic approaches do correlate well with exam success.³ The worry here is that as medical students these people may flourish in exams but as clinicians lack the knowledge base or understanding to work safely or effectively.

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References

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Table 1

Patient	Dose IN naloxone	Time to spontaneous respiration
1	0.8 mg	40 seconds
2	1.6 mg	2 minutes
3	1.6 mg	30 seconds
4	2 mg	1 minute
5	1.6 mg	90 seconds
6	0.8 mg	30 seconds

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Factitious hypoglycaemia in hypotension

Capillary blood glucose evaluation is routinely performed on patients presenting to the accident and emergency department. However, the limitations of this test are not widely known. We recently cared for a shocked patient who was hypoglycaemic (capillary glucose 1.3 mmol/l, venous laboratory glucose 2.3 mmol/l) on presentation. He was treated with repeated boluses of intravenous glucose and a single dose of intravenous glucagon (1 mg) as capillary blood samples remained hypoglycaemic. With continued resuscitation a further venous glucose sample revealed his formal blood sugar to be increased (30.8 mmol/l) while capillary levels were still in the hypoglycaemic range (1.8 mmol/l). We were unaware of the possibility of inaccuracy in this situation and discussion with colleagues revealed a similar lack of awareness.

Atkin *et al*¹ showed in a prospective study of hypotensive (systolic blood pressure <80) patients in the emergency department that

32% of patients were incorrectly diagnosed as hypoglycaemic by finger stick measurements. Indeed, on laboratory measurement of venous samples, two patients were hyperglycaemic. They recommended that venous blood samples measured with glucose reagent strips should be the preferred method of bedside blood glucose estimation in hypotensive patients as these results were comparable to laboratory values. The reason for the discrepancy between capillary blood glucose measurements and venous blood glucose measurements remains unclear. It has been proposed that, in the shocked patient, both peripheral vasoconstriction causing shunting of blood from the periphery and continued peripheral consumption lead to decreased capillary blood glucose concentrations.

While the risks of hypoglycaemia are widely appreciated, it is becoming increasingly recognised that hyperglycaemia is not desirable and may indeed worsen outcome.² The mechanism involved is uncertain but is probably related to increased cellular lactic acid production.³

Hypotension is frequently encountered in acutely ill patients and the limitations of a routinely used test need to be recognised and highlighted.

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CORRECTION

An editorial error occurred in this article by Dr Wallace and others (2002;**19**:202–5). In the flowchart, along the staggered overdose pathway, all doses should be described on a dose/kg/day and not a dose/kg basis. Also, patients who present after a paracetamol overdose with an unknown quantity of paracetamol should definitely be treated as though they may have taken a potentially hepatotoxic dose. The correct version of the flowchart is available on the journal web site (www.emjonline.com).