

PostScript

LETTERS

Delayed lethal arrhythmia after an electrical injury

Bailey *et al* are to be congratulated on conducting a prospective study of electrical injury.¹ The "abnormal" ECGs were mostly non-specific abnormalities, and only occurred in 11% those who were deemed to have a risk factor. As a result, I searched the literature for cases of delayed lethal arrhythmias.

Bailey reported on a 16-year-old subject with a past history of palpitations who was assessed for painful burns on both hands after a 750 V DC shock.² She was found dead 10 h later. No mention of an ECG was made. The coroner concluded that she had died from arrhythmia not necessarily directly related to the electric shock.

The patients reported by Jensen to have delayed dangerous ventricular arrhythmias also had delayed presentations (up to 2 months) and had initial ECG abnormalities.³ The other patient, a 43-year-old electrician, was exposed to a 3000 V DC electrical injury. Twelve hours later while playing football, he collapsed and was resuscitated from ventricular fibrillation. Unfortunately, there was no intervening ECG.

Sharma *et al* describe the case of a 24-year-old man who had an ECG 45 min after contacting a 220–240 V alternating current switch.⁴ This showed low voltage and a first degree atrioventricular (AV) block (PR interval of 0.3 s). After 2 h, he developed a Mobitz type I Wenckebach AV block. The next day, he had complete AV block. Six hours later, he developed ventricular fibrillation and needed defibrillation. He was observed for 6 more weeks without problems.⁴

So when assessing patients after an electric shock, these reports confirm that one can be confident that if the patient is asymptomatic and has a normal ECG, cardiac monitoring is not required.⁵ This is reassuring for both patients and staff. Delayed lethal arrhythmia must be exceptionally rare.

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Skin turgor: author's response

I was delighted to read the critique of the recent Best Evidence Topic (BET) summary on the reliability of skin turgor as a method for assessing dehydration in children,¹ and would like to accept your invitation to respond. My contribution, as third author for this paper, was to check and update the search strategy and to review the final manuscript.

While I am sorry that the BET did not provide Dr Smith with the information he desired regarding the diagnostic accuracy of skin turgor in assessing dehydration in children, I fear that he may perhaps be looking in the wrong place for this answer. Just as we would not criticise a paper published within the Emergency Casebook for not being a randomised controlled trial, it is perhaps wrong to criticise a BET for not being a thorough systematic review of the assessment of dehydration in children.

BETs were designed to "bring the evidence one step closer to the bedside, by providing answers to very specific clinical problems, using the best available evidence" (<http://www.bestbets.org/home/betsintro.html>). The BET in question asked a very specific three part question regarding the interobserver reliability of skin turgor, as designed by Drs Fayomi and Maconchie. If skin turgor cannot be reliably measured by emergency physicians, it is perhaps of dubious value as a diagnostic test.

The search strategy was also perhaps unfairly criticised. All of the papers cited by Dr Smith were identified using the reported search. None of these papers assessed inter-observer reliability and therefore did not answer the three part question that had been posed. All of these papers were also included in the systematic review that we cited.² It may be of further interest to Dr Smith that, for the detection of 5% dehydration, abnormal skin turgor carried a pooled sensitivity of 58% (95% confidence interval (CI) 40% to 75%) and specificity of 76% (95% CI 59% to 93%) in this well designed systematic review, although the BET in question did not seek to report on diagnostic accuracy.

Finally, Dr Smith states that the usefulness of BETs has been debated and urges caution in the interpretation of their conclusions. While it is true that caution should be exercised in the

interpretation of all medical literature, I should like to pass comment for the reader who will infer from this a criticism of BETs as a concept.

Where else within the emergency medicine literature can one easily access an up-to-date concise summary of the best available evidence for topics ranging from the use of Buscopan in oesophageal food impaction to the sensitivity of a normal chest x ray for excluding aortic dissection; from the use of oxygen in acute myocardial infarction to the prognostic effect of clopidogrel in head injury? Truly, BETs have revolutionised our approach to emergency medicine both within this country and (increasingly) internationally, as demonstrated by recent publications from the USA and Australia.

Much of the beauty of BETs is in their simplicity. Let us not overcomplicate the issue.

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CORRECTION

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Redmond AD. Debriefing. *Emerg Med J* 2007;24:605.

The journal apologises for an error that has occurred within the last sentence of this letter. The letter should read as follows.

In the article by Doy *et al*¹ reference is made to "critical incident stress debriefing". It was suggested there was some disagreement as to its effectiveness, but nevertheless the article appeared to be recommending its use. I would, however, refer readers to the National Institute for Health and Clinical Excellence guidelines on post-traumatic stress disorder (www.nice.org.uk). These guidelines state that for individuals who have experienced a traumatic event, the systematic provision to that individual alone of brief, single session interventions (often referred to as debriefing) which focus on the traumatic incident should **not** (their bold type) be routine practice when delivering services.