

PostScript

LETTERS

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The impact of NICE guidelines for the management of head injury on the workload of the radiology department

We performed an audit looking at the impact of NICE head injury guidelines published in June 2003¹ on the workload of the radiology department. This was performed retrospectively in the emergency department (ED) of

Barnet District General Hospital over a three month period between 1 June and 31 August 2003. The hospital serves a population of about 250 000 and the ED sees 60 000 patients per annum.

Case notes were retrieved and electronic medical records were reviewed to determine whether the skull radiograph or CT scan of the head had been done using the existing guidelines for head injury and whether they would have been indicated if complying with the new NICE guidelines for head injury. The existing guidelines were as recommended by the Royal College of Radiologists.² It was also checked whether patients had CT scans of their head carried out subsequently at Barnet. "Head injury" for the purpose of audit was defined as any trauma to the head, other than superficial injuries to the face. Patients' age, sex, time of arrival, day of arrival, Glasgow coma scale (GCS), risk factors, and disposal were noted.

A total of 16 008 patients attended the ED during the period reviewed. Altogether 520 (3.25%) attended with head injury. A total of 316 (61%) were male and 204 (39%) female. There were 39 (7%) infants, 105 (20%) between age of 1 and 5 years, 72 (14%) between age of 6 and 15 years, 222 (43%) between age of 16 and 64 years, and 82 (16%) between age of 65 and 97 years.

A total of 372 (72%) patients with head injury attended ED between Monday and

Friday. Out of these 173 attended between 8 00 am and 5 00 pm and 199 between 5 00 pm and 8 00 am. A total of 148 (28%) patients with head injury attended on Saturday and Sunday. Out of these 61 attended between 8 00 am and 5 00 pm and 87 between 5 00 pm and 8 00 am.

Of 520 patients with head injury 59 (11.3%) patients had skull radiographs and 12 (2.3%) had CT scan of head done within existing head injury guidelines. Three patients had both CT scan and skull radiographs done. To comply with the new NICE guidelines for head injury no patient would have required skull radiography and 26 patients would have required CT scan of head including the 12 who had scan within existing guidelines (see table 1).

The study suggests that in a medium size semi-urban area district general hospital the number of CT scans of the head for head injury would increase from four to eight or nine a month. Most of these would not happen during normal working hours. This will increase the on-call workload by about two to three scans per month particularly as NICE expects them to be done within one hour of request for many indications. As skull radiographs are only indicated when CT is unavailable there would be a saving of about 20 skull radiographs per month.

Table 1 Twenty six patients who would have required CT scan of head to comply with the NICE guidelines for head injury

Age	Arrival day	Arrival time	GCS	Risk factors	Radiography of skull done	CT done	Disposal
38	Mon-Fri	8 am-5 pm	15	Dangerous mechanism of injury and LOC	Yes	No	Discharged
43	Mon-Fri	8 am-5 pm	15	Dangerous mechanism of injury and amnesia	No	No	Discharged
1	Mon-Fri	8 am-5 pm	15	Dangerous mechanism of injury and age under 5 years	No	Yes	Admitted
49	Weekend	5 pm-8 am	15	Dangerous mechanism of injury and LOC	Yes	No	Discharged
18	Mon-Fri	8 am-5 pm	15	Dangerous mechanism of injury and LOC	No	Yes	Discharged
30	Weekend	5 pm-8 am	15	Dangerous mechanism of injury and amnesia	No	No	Discharged
19	Weekend	8 am-5 pm	15	Dangerous mechanism of injury and coagulopathy	No	No	Transferred*
19	Mon-Fri	5 pm-8 am	15	Dangerous mechanism of injury and amnesia	Yes	No	Admitted
7	Weekend	5 pm-8 am	15	More than one episode of vomiting	No	No	Admitted
46	Weekend	5 pm-8 am	15	Dangerous mechanism of injury and LOC	Yes	No	Discharged
11	Weekend	8 am-5 pm	15	Dangerous mechanism of injury and LOC	No	No	Admitted
18	Weekend	8 am-5 pm	15	Dangerous mechanism of injury and LOC	Yes	No	Discharged
2	Weekend	5 pm-8 am	15	Dangerous mechanism of injury and age under 5 years	Yes	No	Admitted
56	Mon-Fri	8 am-5 pm	15	Dangerous mechanism of injury and LOC	No	No	Observation in A&E
80	Mon-Fri	5 pm-8 am	15	Amnesia and age greater to 65 years	No	Yes	Admitted
6	Mon-Fri	8 am-5 pm	15	Dangerous mechanism of injury and LOC	Yes	No	Admitted
27	Mon-Fri	8 am-5 pm	15	Dangerous mechanism of injury and LOC	No	No	Admitted
76	Weekend	5 pm-8 am	14	GCS 13 or 14 at two hours after injury	No	Yes	Admitted
39	Mon-Fri	5 pm-8 am	14	GCS 13 or 14 at two hours after injury	Yes	Yes	Admitted
72	Mon-Fri	8 am-5 pm	14	GCS 13 or 14 at two hours after injury	No	Yes	Admitted
43	Mon-Fri	8 am-5 pm	13	GCS 13 or 14 at two hours after injury	Yes	Yes	Admitted
37	Mon-Fri	8 am-5 pm	13	Dangerous mechanism of injury and LOC	No	Yes	Admitted
26	Mon-Fri	5 pm-8 am	13	GCS 13 or 14 at two hours after injury	Yes	Yes	Admitted
95	Mon-Fri	5 pm-8 am	12	GCS less than 13	No	Yes	Admitted
43	Mon-Fri	5 pm-8 am	11	GCS less than 13	No	Yes	Admitted
27	Mon-Fri	5 pm-8 am	10	GCS less than 13	No	Yes	Admitted

LOC, loss of consciousness. *Transferred to specialist centre.

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Vasopressin or adrenaline in cardiac resuscitation

The best evidence topic report (BET) by Hogg and Mahu¹ raises a number of concerns, both with the article itself and the BETs process as a whole. The relative efficacy of adrenaline and vasopressin in the management of cardiac arrest is an important subject of relevance to all who work in emergency medicine. For this BET to only include those papers directly comparing vasopressin and adrenaline is to dismiss a large amount of research published in this area. A very brief search on Medline reveals a large number of articles looking at this subject, including two recently published reviews comparing adrenaline and vasopressin,^{2,3} not mentioned by the authors. Surely a topic such as this should be subject to a formal literature review and meta-analysis, not the "shortcut review" method advocated by the BETs methodology.

One of the stated aims of the BET methodology is to summarise the highest level of evidence available.⁴ In response to previous criticisms Professor Mackway-Jones was keen to emphasise that those undertaking BETs "go to great lengths to ensure that the search strategies used are highly sensitive".⁵ It would appear from this article that this has not been the case on either count.

Given the above concerns could the journal editors confirm whether the BETs are subject to the same peer review process as other articles in this journal.

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- 2 **Ong ME, Lim SH, Anantharaman V.** Intravenous adrenaline or vasopressin in sudden cardiac arrest: a literature review. *Ann Acad Med Singapore* 2002;**31**:785-92.
- 3 **Nolan JP, De Latorre FJ, Steen PA, et al.** Advanced life support drugs: do they really work? *Curr Opin Crit Care* Jun 2002;**8**:212-18.
- 4 **Mackway-Jones K.** Towards evidence based emergency medicine: best BETs from the Manchester Royal Infirmary. *Emerg Med J* 2003;**20**:464.
- 5 **Cocks RA.** Best evidence topic reports: fracture of the clavicle. [Author's reply]. *J Accid Emerg Med* 1999;**16**:389.

Authors' reply

We read with interest the comments on our best evidence topic review on vasopressin or adrenaline in cardiac resuscitation and are happy to explain the process entailed in producing the BET.

This literature search was first conducted in March 2002. Our initial and specific question was: Is vasopressin more effective than adrenaline in achieving return of circulation and long term survival, in human cardiac arrest?

A full and sensitive search strategy was compiled. The search strategy was checked by two additional independent doctors who cross checked their own strategies to maximise the sensitivity. All titles and abstracts were appraised initially by the two authors and before publication by the third independent author. The relevant original studies and review articles were sourced in full text (18 in total). All review articles were cross referenced.

These 18 papers were reduced to four potentially relevant papers. This included the two published studies. The search strategy and all four papers were reviewed by the Manchester Royal Infirmary Emergency Medicine journal club. A consensus decision was taken to exclude from the analysis one study reporting the effects of intravenous vasopressin on coronary perfusion pressures in 10 patients¹ and a second that reported the effects of intravenous vasopressin administered in refractory cardiac arrest, in eight patients.² The first study did not use return of circulation as an outcome, and the second did not compare the effects of vasopressin and adrenaline. The decision to publish this review was taken four months before publication. At that point it was reviewed by Professor Mackway-Jones, the first author repeated the search and a third independent author checked the relevant articles and search strategy.

To directly respond to your points

- The BET addresses a specific question. We did not aim to present a vague representation of all literature on vasopressin but asked the question—Is vasopressin better than adrenaline in a human cardiac arrest?
- Dr Locker wonders why we did not include two recent reviews addressing this question. A less superficial appraisal of these papers would have revealed they found the same four studies. We have cross referenced all the review articles published on this subject and can find no further studies.
- We do not pretend that these reviews are systematic reviews, (we used Medline only and do not search for unpublished data), but we do openly lay out our methodology. It would be possible for another doctor, anywhere in the world, to repeat this search exactly if they so required.
- At present, a meta-analysis would appear wholly inappropriate to answer this question. There are only two small studies, the results of which are clearly laid out in the table. The reader is capable of drawing their own conclusions from this table. When more relevant data are available (there are at least two ongoing studies), a meta-analysis may be of use.

In conclusion, this BET is an accurate and reproducible formal review. It addresses the

question posed by the authors and clearly summarises the relevant published literature.

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- 2 **Lindner KH, Prengel AW, Brinkmann A, et al.** Vasopressin administration in refractory cardiac arrest. *Ann Intern Med* 1996;**124**:1061-4.

Vasopressin—the continuing evidence. 2 February 2004

On the 8th of this month, the large multicentre European Resuscitation Council study comparing the effects of adrenaline and vasopressin in out of hospital cardiac arrest was published. This was a multicentre study conducted between 1999 and 2002 in Austria, Germany, and Switzerland. Patients with an out of hospital cardiac arrest requiring cardiopulmonary resuscitation and intravenous vasopressor therapy were included. The study shows no statistically significant benefit in primary (overall survival to hospital) or secondary (overall survival to discharge) end points. A subset analysis demonstrated that more patients in asystolic arrest survived to hospital after the administration of vasopressin as compared with adrenaline. Notably, the survival to discharge did not reach clinical significance. The sub-analysis further showed that of those who did not respond to two doses of research drug and went on to be given adrenaline, significantly more patients in the vasopressin group reached both end points.

This is the largest study addressing the question whether vasopressin should be used in an arrest. As normal practice, the Best BET comparing these two entities has been updated to include this evidence.

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BETs—Should they be published in the journal?

I agree with Dr Locker's concerns regarding the publication of BETs in a peer reviewed journal. BETs are useful for introducing people to the theory of literature searching, and appraisal of published evidence, ideal skills for SPRs working towards their clinical topic review. However, this does not necessarily warrant their publication in a peer reviewed journal. They occupy valuable space within a journal that is only published bimonthly, which could instead be used by studies with more rigorous methodology. If the *EMJ* is to become a leading worldwide journal in the field of emergency medicine, should it be including BETs within its pages? I don't see the *Lancet* or the *BMJ* publishing six to seven pages of Medline searches each edition. Although Dr Hogg does explain that she has carried out a rigorous search, and had this checked, this in itself does deviate from the initial aims of BETs as something a clinician could do in a short period of time.

With the advent of nearly universal internet use is the BestBETs web site not the best place for them to reside?

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BestBETs reply from the BestBETs group

Two critical letters from the Sheffield Deanery have been posted on the *EMJ* online web site. The first was written about a particular BET (vasopressin versus adrenaline (epinephrine) in cardiac arrest) and challenged the completeness of the search—claiming that a brief search of Medline had uncovered recent reviews that had been missed. It also called into question the whole peer review process for BETs. The letter was erroneous, and a robust defence of the search and selection strategy for the BET in question was posted by the author (Kerstin Hogg) pointing out that finding papers was a first step and that the alleged “missed” papers had in fact been found and added nothing in that they had found no additional primary papers. The issue of the peer review process was left as the argument that the review process for this BET had failed (which underlay the complaint) had been disproved. Dr Webster raises a number of points in addition to those raised by Dr Locker earlier in this correspondence. I feel the BestBETs team needs to answer three of them:

Methodological quality

The BETs are not of low methodological quality—rather they are of lower methodological quality than a systematic review. They are in fact of much higher quality than almost all of the literature reviews contained in the original articles that are mooted to replace them. They have a clear aim (three part question), a clear methods section (search strategy) and clear results (evidence table), discussion (comments) and conclusions (clinical bottom line). The main problem (in that the method is not overt) lies in the selection strategy for the papers (that is, why were particular papers selected and why were particular papers rejected). The BestBETs web editors group is currently working towards posting selection algorithms on the BestBETs web site—but in the mean time the method is set out in the original paper in *JAEM* that is referenced in every BestBETs set published. My opinion is that it would take a huge amount of space to publish an overt selection section individually for each BET (listing the papers rejected and why) for no real gain.

Waste of space

Best BETs are also published in *Archives of Disease in Childhood* and *Interactive Cardiovascular and Vascular Surgery*. *Emergency Nurse* is also likely to begin publication this year. Journals such as *BMJ* publish POEMS and extracts from *Clinical Evidence*, and the ACP Journal Club is entirely based on such articles. Best BETs have been reviewed both on the ACP Journal Club and *Annals of Emergency Medicine* and are regarded as the best way to answer real time questions in the specialty. Furthermore, at the time of writing four BETs are in the top 10 hits (January) so

we are clearly giving most readers something they want.

Review process

The BestBETs are specialist articles (shortcut reviews) and require peer review by expert reviewers. BestBETs are all dual authored—one reporter and one checker. This is the first review. The BestBETs team have set up a system whereby all BETs in emergency medicine (whether generated internally or externally) are brought to the weekly evidence based journal club at the Manchester Royal Infirmary for group review. At this meeting the construction of the question, the search strategy, the data extraction, and the veracity of the clinical bottom line are discussed. This is the second review. Internally generated BETs are sent back for reconstruction by their original author and brought back for further discussion once this has been done. Externally generated BETs are allocated to a club member and brought back for further discussion once required changes have been made. This is the third review. Each BET is then allocated to a web editor who independently reviews the search strategy, paper selection, data extraction, and clinical bottom line. This is the fourth review. Finally, just before publication, the search is rechecked and rerun by an information officer (as MESH terms may have changed) and any new papers obtained. The table and conclusions are updated by a senior editor and the article submitted to *EMJ*. This is the fifth and final review. I suspect this process stands up well to the peer review process (judging from what we see on the hanging committee). I would term it specialist peer review and I believe it to be eminently defensible.

In summary the BestBETs are not low quality—but lower quality than the highest possible quality (systematic reviews). If this were a reason not to publish then *EMJ* would be easy to edit (blank pages every month) as we very rarely receive papers of the highest quality. They are popular with the readership. The peer review system is probably more rigorous than that for other articles.

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Good BETs?

As editors we seldom get feedback on the *EMJ*. It is good to have questions raised about the journal and its contents. Avid readers of the letters section in emjonline may have been following correspondence that has questioned the “BETs” section and whether it is a good use of journal space and the editorial processes that underpin the BETs process.

We have to declare a conflict of interest in that one of the editorial team is heavily involved in BETs process. However, we, as the editors of the journal, have long formed the opinion that BETs is one of the “jewels in the crown” of the *EMJ*. Initially started using authors from Manchester, the system now allows online submission from any one with the enthusiasm to undertake work required to produce a BET. The editors have been impressed by the hard work and professionalism that goes into the production of BETs and we do not have concerns about the rigour of this process.

Emergency medicine is a practical subject and the BETs provide an approach that makes us think about the evidence base behind the decisions we make and the work we do. Admittedly more often than we would like the “bottom line” is that there is not a good evidence base. However, this is not the fault of the BETs process, it is a sign of the relative paucity of research in the fundamentals of our practice. Don't shoot the messenger.

As to the question “are they worth the space?” I think a visit to the “top ten papers” section in emjonline shows the popularity of these articles. The fact that a number of other journals have copied this methodology verifies the appeal of this approach in medical journals.

The *EMJ* owes a great debt of gratitude to Kevin Mackway-Jones and his team for working with us in conceiving and producing this section. He replies on behalf of his group in the letter above.

We thank the readers for raising this issue and we agree that it is healthy to review our practice. We intend to carry out a full readership survey in the near future and if our readers tell us that BETs is no longer read then we would consider these views. However, we try to practice “evidence based editing”. The evidence we have at present is that this format is useful, popular, and well read. Your comments would be welcome through emjonline letters.

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Emergency contraception

We read this paper with great interest, but were very disappointed that the authors seemed unaware of the availability of free oral emergency contraception from selected pharmacies in Lambeth, Southwark, and Lewisham since April 2000.¹ This has been widely publicised, and local accident and emergency (A&E) departments were informed when it started.

In April to June 2000 there were 1003 consultations under this scheme, as it became more widely known the figure for April to June 2001 rose to 1846 consultations. We feel that in a deprived area this is more likely to have accounted for the decrease in supply at A&E, (as well as family planning and GP surgeries) than the expensive over the counter purchase route.

A snapshot audit in October 2003 showed that A&E supply is now only 3% of the total in the area.

A recent audit by the Lambeth, Southwark and Lewisham Clinical Governance Resource Group produced evidence of long waits at A&E departments, and a feeling by some A&E staff that this was not appropriate work for their department. The skills of community pharmacists are indeed underused, and we hope for the support of A&E departments as we mainstream this work.

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BOOK REVIEW

Paediatric education for prehospital professionals

Edited by R Dieckman, D Brownstein, and M Gausche-Hill. Published by Jones and Bartlett, 2000, pp 344. ISBN 0-7637-1219-1

This book from the USA is a companion to the course of the same name.

The book is crammed with gems that have clearly come from years of paediatric experience and the photography and drawings are outstanding. The chapters are fairly uniformly constructed and most of them contain a section on when to transport. The text is comprehensive and some of our paediatric and emergency medicine trainees would learn much from it. I personally found the

“Tips” and “Blips” in the margins irritating but they may appeal to some. The highlighted “Controversy” points were a different matter—an excellent way of emphasising the confusion and ignorance surrounding the prehospital care of children, they were topical and up to date.

I totally agree one should analyse the overall severity of illness of the child on arrival, but I was very disappointed in the patient assessment triangle or “PAT”, which receives much emphasis throughout the book. It seems to be nothing more than a rapid initial assessment of ABCD, which is certainly taught as part of the approach to any child on most of the major resuscitation courses in the UK. To give it a special new name left me mystified.

Treatment is divided into basic life support (BLS) and advanced life support (ALS). The BLS was clear and well presented at ambulance technician level, but some of the ALS interventions would be in the realm of the paediatric intensivist in the UK. I can only assume American paramedics have good “on line” medical support if they are really giving inotropes and adenosine to children and babies in the field! I was disappointed that a number of less high powered problems I have trouble dealing with in practice (like when to move in cardiopulmonary arrest when there are only

two of you and how to best protect the neck of a hysterical head injured toddler) were neatly avoided. Maybe they don’t know either!

I was worried from the start that the text might confuse rather than clarify matters for British readers because the UK and North American EMS systems are so different. Generally this was not the case, but there were certainly some glaring discrepancies as well as a lot of minor ones. For example, we would almost *never* use neat 50% dextrose in *any* child to treat hypoglycaemia—neither would we usually head straight for adrenaline if nebulised salbutamol failed to improve asthma.

In conclusion, I thought the book beautifully presented (except for the paper, which was poor quality) and the BLS sections good value for all. I would recommend that UK paramedics follow the ALS sections only under the guidance of a doctor experienced in UK prehospital paediatrics who could “pick out the wood from the trees” for them—defeating the object of using it for personal study. Prehospital doctors, however, would, I’m sure, find it a useful read providing they had enough paediatric background knowledge to recognise the transatlantic differences.

F E Jewkes