PostScript

LETTERS

Screening for hypertension in the emergency department

I read with interest the paper by Fleming *et al*¹ concerning screening for hypertension in the emergency department. The important public health issues and current emphasis on screening are well illustrated in this paper. However, there is increasing debate concerning the appropriateness of routine enquiry, as debated in the commentary by Lee.² If screening for a condition is warranted it should, at least approximately, fulfil the Wilson criteria.³ The diagnosis of hypertension fails to meet these criteria in a number of important regards.

Firstly, the endpoint of screening is to establish the diagnosis so that prevention of an adverse endpoint is achieved on a population basis. In this paper only 2.5% of the patients' general practitioners were directly informed of the diagnosis, and there are no data on the clinical results for these patients. The screening has become an end (to achieve diagnosis), not a means. Secondly, systematic testing of a population for hypertension should be performed on a continuous and total basis, and this paper¹ reveals that this is difficult to achieve. Lastly, the case-finding needs to be economically balanced in relation to diagnosis and treatment and possible total health care expenditure, and this is not discussed.

Although universal screening for hypertension in the emergency department may not be appropriate for the reasons stated above, or desirable for reasons related to service configuration, targeted screening for an essentially asymptomatic disease will fail almost by definition. This is not to say, however, that opportunistic detection in the emergency department is not appropriate.

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Simpler thrombolysis decisions in patients with left bundle branch block

I read the article by Reuben and Mann¹ with considerable interest. This is a useful way of presenting the Sgarbossa criteria, which have high specificity for acute myocardial

infarction (AMI). Unfortunately, these criteria are far too insensitive to exclude MI, being present in only 20% of patients with left bundle branch block (LBBB) and confirmed AMI.^{2,3} It is therefore important that less experienced practitioners do not assume that thrombolysis should be withheld if these electrocardiographic (ECG) features are absent.

In fact the simplest thrombolysis decision is that all patients with a history strongly suggestive of AMI and LBBB should receive thrombolysis, unless significant contraindications exist. Even the well rehearsed medical myths relating to the age of the LBBB are of minimal relevance, as described in an elegant editorial cited by the authors.⁴ Patients with LBBB have the most to gain from thrombolysis,⁵ but they are still failing to receive this highly effective therapy in too many cases.

In summary, therefore, where Sgarbossa criteria are seen on the ECG, AMI is highly likely. However, even in the absence of these features patients with a convincing history of AMI and LBBB should still receive thrombolysis, unless there are compelling reasons not to.

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Response to Mucci *et al*'s study: "Cranial computed tomography in trauma: the accuracy of interpretation by staff in the emergency department"

We read with interest Mucci *et al*'s¹ study of the accuracy of interpretation of cranial computed tomography (CT) scans in trauma by emergency department (ED) staff. It is a topical subject that needs exploring, but we have questions about the design of their study.

Firstly, the study was underpowered with only 100 scans examined. The quoted sensitivity of 86.6% has too low a 95% confidence interval (83.4% to 89.9%) to propose trusting the reliability of interpretation of the CT scans by the ED staff. If these figures were translated into one scan been examined by only one reader (which is more comparable with real life practice) the corrected 95% confidence interval would be 68.7% to 94.0%. Is an error rate of more than 15% really acceptable?

Secondly, we are concerned that multiple readers interpreted the same cases. The authors allude to Robinson *et al*'s² findings about interobserver variation, but the latter only studied the variability between radiologists interpreting plain radiographs. Can Robinson's findings be extrapolated to interobserver variation of ED staff interpreting CT scans?

Thirdly, we are intrigued by the high proportion of "abnormal" scans. Does the fact that skull radiographs are routinely done at their hospital suggest that only those patients who are more injured are scanned, thus increasing the chance of having serious pathology which is easier to identify on CT? If they scanned more patients would increasingly subtle abnormalities have been harder to detect?

Finally, we chuckled at the lack of conflicting interests. Would it be churlish to suggest radiologists would welcome any study that would reduce their out of hours workload?

Overall, we welcome Mucci *et al*'s paper but suggest that they have placed too much significance on an underpowered study with considerable interobserver variability. Further, larger studies are required (and are being performed) to answer this question more thoroughly.

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Emergency care practitioners should not be compared with paramedics

I enjoyed the article by Cooper *et al*¹ and was delighted to see some evidence being published outlining the role of the emergency care practitioner (ECP). However, I have