Reviewer's report

Title: A simple statistical model for prediction of acute coronary syndrome in chest pain patients in the emergency department

Version: 1 Date: 14 December 2005

Reviewer: Richard J McManus

Reviewer's report:

General

Thank you for the opportunity to review this paper which describes the building of a simple model for predicting ACS amongst chest pain patients in the ED. The work differs from other models that have mostly concerned prediction of AMI. The authors rightly state that prediction of ACS is the more important decision with respect to admission/discharge as opposed to treatment/no treatment with thrombolysis or angioplasty. This is an interesting study which merits publication but before this there are some issues to clear up.

------------------------------------------------------------------------

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

1. Background para 1: “over admission” does not necessarily mean poor quality of care unless there is a better alternative.
2. Methods para 3: I would head this “reference standard”. This would make it clearer that it is this section describes the standard that was used. Were discharge diagnoses ALL reviewed for accuracy or just ones where there was ambiguity? (eg what if someone was discharged but the represented with AMI.). There is a typo on line 2 – diagnose should be diagnosis.
3. ECG – I would use sub heading of machine read and expert read and give an introductory sentence explaining that two methods of ECG analysis were used.
4. Citations should be after punctuation in Vancouver ie . 1
5. The explanation of the validation method used needs to be easier to understand. My reading of Baumann’s method is that the random sampling is used for both learning and test sets. I may be wrong about this but most readers will need a better explanation of what went on. This is key because the results from the model are not particularly impressive, despite being tested on “patients” drawn from the same pool as used for the learning set.
6. Discussion para 2: I think the role of the ECG in ACS is limited to people with chest pain. This sounds picky but if you are asymptomatic then the ECG won’t necessarily be much good at differentiating ACS.
7. The discussion needs to address head on the fact that in the setting the model was developed from (the ED), it did not perform any better than standard care. This is must be tackled or if the authors disagree with this they should justify why at the sensitivity chosen as similar to current practice, the predictive value if not really any better than current practice. Suggesting that the model might work better in settings such as primary care with low prevalence is rather optimistic as the model was not developed from data collected in such a setting and so (as the authors point out), the model needs to be tested elsewhere to check on its usefulness. Another possibility not mentioned might be in assisting inexperienced junior medical staff in ED (say in their first month in the ED) and thus improving quality control.

------------------------------------------------------------------------
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)
8. References need to be checked – eg ref 30 Jama should be JAMA.

Discretionary Revisions (which the author can choose to ignore)
9. Figures: is it possible to plot the two ROC curves together so that they can be compared?

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:
I have received research funding and funding to attend a conference from sanofi-aventis and Merck. I'm not aware of any potential loss or gain to these companies from this paper. I have no other competing interests