Reviewer's report

Title: A Comparative Analysis of Multi-Level Computer-Assisted Decision Making Systems for Traumatic Brain Injuries

Version: 1 Date: 6 March 2008

Reviewer: Rory Wolfe

Reviewer's report:

Major Compulsory Revisions

1) Simple description of the datasets being used needs to be expanded and would best be presented in a table. For example what proportion of people in the datasets had each outcome of interest (I couldn't find the % rehab versus % home)?

2) The Background is too long and unclear

3) The authors repeatedly state that logistic regression "does not assume a linear relationship". This is incorrect. This model assumes a linear relationship between the log-odds of outcome and any nominal variable included as a covariate. For this reason it is essential to explore whether this assumption is reasonable before simply including nominal variables in logistic regression models.

4) There is an important distinction to be made between (1) variables such as GCS, ISS etc which can take integer values in a restricted range because they are formed by addition of a number of sub-scale scores, and (2) variables such as blood pressure and age which are fundamentally continuous but due to the limitations of measurement accuracy may be recorded to the nearest integer or to one decimal place etc. This distinction is important in the context of point 3 above.

5) The presentation of references is unacceptable.

6) There are a very large number of grammatical errors.

7) There are no page numbers.

8) The rationale for including SVM, Neural networks and AdaBoost is not well made. In the end these appear to be ignored anyway because they don't provide "rule-like" predictions.

9) The case for "rule-like" predictions being essential is not well made. If a prediction/decision algorithm can be implemented in software then it doesn't matter how complex it is surely?

10) In Step 3 logistic regression is used to identify significant variables. Since this method is being used the predictive performance of logistic regression should
also be included in the comparisons.

11) The "Direct MLE" is insufficiently explained yet is crucial to the methods. In particular how does it differ from forward or backward elimination in the absence of a stepwise process? What models are compared when "calculating the log likelihood ratio"?

12) A rationale for >85% accuracy is almost completely absent. Also, the rationale for considering 75%-85% accuracy is particularly unclear and unconvincing.

13) The Discussion section makes a big comment about missing data but I couldn't see any mention of this aspect in the results section.

14) The Discussion section needs to be completely rewritten. There is no discussion of limitations. Point 1 in the discussion section (only the second half of it in which the comparison of the 5 algorithms is discussed) and point 8 are the only points that contain valid and useful reflections on the results presented in the paper. Point 9 in the Discussion might also be reasonable but it needs to be rewritten.

Minor Essential Revisions

1) The description of dichotomising ICU into >2 or <=2 is particularly unclear.

2) Table 2 - the text refers to "4 categorical and 6 numerical attributes" but the table only lists 5 variables??!

3) Step 2 - should discuss sensitivity and specificity here since both are used in the results and discussion sections.

4) Table 3 - too many decimal places for most columns. Why present coefficients and odds ratios? And then why in Table 4 is such a different set of columns chosen for presentation?!

5) There are too many small tables and these should be combined.

6) Tables 7-12. It is unclear what is meant by "Total testing". Also, for C4.5 in Tables 9, 10 and 12 it seems that only prediction of Rehab, Rehab, ICU<=2 respectively is possible. This needs to be explained.

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 limited interest

Quality of written English: Not suitable for publication unless extensively edited

Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:

I declare that I have no competing interests