Reviewer's report

**Title:** Does Interhospital Transfer Improve Outcome of Acute Myocardial Infarction? A Propensity Score Analysis from the Cooperative Cardiovascular Study

**Version:** 1  **Date:** 14 December 2007

**Reviewer:** jay brophy

**Reviewer's report:**

This large observational study has several interesting findings. However, I do have a number of issues that the authors may want to consider. The provision of comments is complicated by the authors not having provided page numbers.

1. The paper never states what is the primary study hypothesis. Is this meant to be a purely descriptive study? This is acceptable but should be stated.

2. A limitation, in my opinion, the authors' over enthusiasm and misunderstanding of propensity methods which subtracts from the important substantive messages in this paper. While there may be infrequent situations where propensity scores out perform conventional regression techniques (such as with rare outcomes where conventional modeling of the outcome may give unstable estimates), neither approach can adjust for the major potential bias of observational research, namely unmeasured confounding or selection bias. Contrary to what the authors assert multivariable regression modeling will provide accurate adjustments in large datasets providing sufficient outcomes have occurred. Propensity scores are often over hyped and several publications exist demonstrating empirically the lack of improvement with propensity scores (cf Sturmer AJE 2005, Stukel JAMA 2007). In the present example, I doubt that propensity scores improves the analysis compared to regular repression techniques. It would be interesting if the authors would repeat their analysis using standard techniques to test this opinion. This is not to imply that their chosen analysis is wrong or inferior but only that the same limitations of residual confounding due to unmeasured covariates still exist.

3. More details are required concerning the methods. What is a "structured medical record review"? What was the random sampling process? What measures of validity exist in the assessment of aspirin and thrombolytic indications?

4. Why do the numbers of excluded and retained patients in the methods not add up to the total number of patients? The same comment applies to the number of hospitals.

5. The authors' interpretation of the C statistic is overly optimistic; remember that chance alone gives a C statistic of 0.5. In my opinion 0.57 = weak and 0.68 fair ability.

6. Why were only the variables in Table 2 used to construct the propensity score,
or is this a simple typo? Why was diabetes and history of CABG not selected as covariates for the propensity score? The basic idea of propensity is to make the treatment groups as similar as possible, like randomization, in order to assess the main effect of transfer on mortality and why exclude these mortality confounders.

7. The conclusion that transferred patients, even after adjustment, have a lower mortality may be true but the present analysis is not convincing. The mean time until transfer is not reported and it is well known that mortality following AMI is a function of time. Patients with a given propensity score should be matched not only on the score but also conditional on being alive at the time of transfer. Again thinking of propensity as a method of randomization, a patient with a propensity score X who dies at day 1 is not necessarily comparable to a patient with score X as determined when transferred on day 4,5,6 or 7.

8. The authors mention the confusion as to how to attribute mortality for transferred patients but then do not state how they handled this situation in their urban versus rural analysis.

9. The analysis examining improved mortality as a function of subgroups (Table 5) seems incomplete. The proper analysis would have been to include multiple interactions terms in a multivariable model and to perform statistical testing.

10. The authors could provide a more complete discussion of their results. For example, is there a transfer paradox whereby those at lower risk (younger, less comorbidities) are being transferred? According to Table 5, patients transferred from low tech hospitals have only 55% the probability of dying of those not transferred. This needs some discussion and comment. Also requiring comment is the essentially non-benefit of increased technology and the large benefits of applying evidence based medical therapies. Does this not raise the question as to why so many patients systematically receive invasive interventions following AMI? Finally some comment about the low rates of smoking counselling is required.

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

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

'I declare that I have no competing interests'