Reviewer’s report

Title: Changes in Practice Patterns Affecting In-Hospital and Post-Discharge Survival Among Acute Coronary Syndrome Patients

Version: 1 Date: 8 September 2006

Reviewer: Mauricio G Cohen

Reviewer’s report:

General
In this manuscript Stommel and colleagues present a comparison between three different cohorts of ACS patients who were treated in different times at institutions located in the state of Michigan. They conclude that patients treated more contemporarily have better outcomes due to better adherence to recommended therapies according to AHA/ACC guidelines.

In general the paper is difficult to follow. It reads more as a rough draft, rather than a crisp, clean, and mature manuscript ready for publication.

There are conceptual problems that need to be corrected before the study can be considered viable to answer the research question.

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)
1. The high use of thrombolytic therapy of approximately 60% in the cohorts MICH I and MICH II contrasts starkly with the low use of 11% in the HARP-GAP cohort. Are the authors sure that these are comparable populations in terms of type of ACS presentation? They should clarify whether or not these patients presented with or without ST elevation in Table 1.
2. How many of the PCI procedures were for primary reperfusion of ST elevation myocardial infarction? What was the timing of PCI?

It would be useful to discriminate whether the cohorts received thrombolysis or primary PCI as reperfusion strategy. It has been shown that primary PCI is associated with improved outcomes compared with thrombolytic therapy. Therefore the improved in-hospital outcomes in the more contemporary cohort could be attributed to the use of primary PCI.

3. The depiction in the tables and text of “most” invasive procedure is conceptually inappropriate. I would suggest that they present actual rates of cardiac catheterization, PCI, and CABG. This way the reader could understand how many of these patients underwent catheterization, which is a diagnostic procedure, as opposed to PCI and CABG that are therapeutic procedures.

Likewise, in page 9, the authors utilize the term “invasive procedure” indistinctly in their interaction analysis with age. Again, I remind the authors, that there is an important difference between an invasive diagnostic and an invasive therapeutic procedure.
4. The Charlson comorbidity index should be presented as median or average for each group instead of categories. Likewise, the comorbidity index should be analyzed as a continuous variable in the multivariable model.
5. In describing medications use, the authors should only refer to those patients in whom the medicines are indicated. For example, ACE-inhibitors are not indicated for everyone with ACS, but for those with hypertension or LV ejection fraction ≤ 40%.
6. The description of extent of coronary disease as means (SD) is inappropriate. The authors should categorize the number of diseased vessels and present the proportion of patients with 1-, 2-, 3-vessel, and left main disease.
7. The table with the multivariable models should be reformatted ordering the variables by order prognostic relevance. For example, the authors should be able to indicate whether age is a stronger predictor than use of beta-blockers? It should be worthwhile presenting the variables associated with mortality from hospitalization to one year. It should be highlighted that being treated in 93-95, 97, 02-03 was independently associated with in-hospital and long-term outcomes.
8. What was the c-index of the model? I suggest that the authors review the following article: Harrell Jr FE, Lee KL, Mark DB. Multivariable prognostic models: issues in developing models, evaluating assumptions and adequacy, and measuring and reducing errors. Stat Med 1996;15:361- 87.

9. An interesting finding of this study (if true) is that the curves do not separate after hospital discharge in the different cohorts? Does this mean that physicians after all these years have not done a good job at improving care after discharge in ACS/AMI patients?
In summary, this paper needs substantial work and some of the analyses need to be redone. As is, this manuscript lacks scientific validity.

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

- The description of the methods is very long and redundant. In page 6, the authors could refer to an appropriate reference for a more detailed description of the GAP project.
- The presentation of the tables does not follow standard publication guidelines. I would recommend that the authors review the Manual of Style by the American Medical Association.
- In page 8, third paragraph, in the sentence that starts with Table 2..., the authors should change “Table 2 presents the results from an analysis employing the Cox proportional hazard model” for “Table 2 presents adjusted survival rates”. The following sentence “In addition to the study cohort, the model…” should be deleted as this is described in the methods section.
- The first paragraph of the discussion should summarize the major findings of the study.
- What is the unit used in the X axis in the Kaplan-Meier curves?
- In order to better examine the shape of the survival curves in Figure 2, the scale of the y-axis should be changed from 0-1.00 to 0.75-1. This would highlight better the timing of separation.
- What is “Time 0” in Figure 2? Admission or discharge time?

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Discretionary Revisions (which the author can choose to ignore)

**What next?:** Reject because scientifically unsound

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

**Quality of written English:** Acceptable

**Statistical review:** No

**Declaration of competing interests:**

I declare that I have no competing interests