Reviewer's report

Title: Are patients with non-ST elevation myocardial infarction undertreated?

Version: 1 Date: 19 November 2006

Reviewer: CHRISTIAN Terkelsen

Reviewer's report:

General

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

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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)

Regarding the manuscript: "Are patients with non-ST elevation myocardial infarction undertreated?"

The study describes long-term mortality in a cohort of MI-patients treated at a highly specialised centre. The authors find that mortality among nonSTEMI patients is higher than among STEMI-patients. After correction for baseline covariates, however, mortality is comparable. The authors conclude that more aggressive treatment may improve outcome in nonSTEMI patients.

The findings are important and should tell us that it is time to focus further on treatment in nonSTEMI-patients who still have high mortality.

There are some minor comments:

1. The authors state that it is an “unselected” cohort. However, the proportion of patients with STEMI were higher than expected (71% versus 40% in the general population). The reason was that the cohort included patients transferred from other hospitals. Selection bias may therefore play a role, as it is evident that the patients transferred from local hospitals to invasive centers are selected, i.e. younger with less comorbid illness and tending to have a more favourable outcome per se. In conclusion the cohort presented is highly selected.

2. Page 3, background: The authors state that the study by Terkelsen CJ et al “had an important limitation since….only 55% of the STEMI patients were treated with reperfusion therapy”. This is not true. In STEMI patients eligible for reperfusion therapy (having symptoms of 12 hours or less) the proportion receiving reperfusion therapy was 143/205 = 70%, which is consistent with observations from other unselected cohorts of STEMI-patients. In the present study, however, 94% received reperfusion therapy (PCI, thrombolysis, CABG). This again is explained by the fact that the present cohort is selected and not representative for the general population.

3. The mortality rates in the present study was rather low, i.e. 12% in the STEMI-population. These patients were also younger than expected, i.e. 63 years in mean. This indicates that the elderly STEMI-patients with severe comorbid illnesses were not transferred from the local hospitals to the invasive center.

4. Page 7, the authors state that “Results of registries are suggested to be more externally valid than randomized trials because they include ALL patients and are done in a real world setting”. This is far from true. Also registries have several limitations. Thus, the registries mentioned were hampered by numerous limitations: a. they were based on voluntarily reported cases (selection bias), b. one registry excluded patients who died within 24 hours of admission thus underestimating the true mortality, c. the registries lag endpoint committees and are thus mixing STEMI and BBBMI patients thereby overestimating the mortality in STEMI-patients, d. the registries often requires informed consent from the patient i.e. excluding high-risk patients who were not able to give consent, e. a high proportion of patients were included in the registries at academic centers with revascularisation facilities i.e. known to have lower mortality.
5. The authors do not give separate data on BBBMI patients. These are known to have extremely high risk and if included in the STEMI-population this may explain that the mortality among nonSTEMI-patients were not lower than among STEMI-patients (overestimation of mortality in STEMI-patients).

6. Table 2. How come that the proportions do not sum up to 100%. For instance 12% of STEMI-patients were treated conservatively, however, 83% were treated with PCI, 7% with thrombolysis and 4% with CABG = 106% in total.

In conclusion the study provides additional information indicating that even at highly specialised centers mortality is high among nonSTEMI-patients. The authors need, however, to revise some of the manuscript to acknowledge that their study is by no means unselected. We agree that more studies are needed in nonSTEMI-patients to further evaluate whether a more aggressive reperfusion strategy in these patients.

Discretionary Revisions (which the author can choose to ignore)

What next?: Accept after minor essential revisions
Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: No
Declaration of competing interests:
I declare that I have no competing interests