Reviewer's report

Title: Trends and inequalities in short-term acute myocardial infarction case fatality in Scotland, 1988-2004

Version: 1 Date: 7 September 2010

Reviewer: Anton Kunst

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Major Compulsory Revisions

1. I suggest that authors to present the interactions between age-sex-year-deprivation with much less detail. The results presented in tables 3 and 4 are too numerous and too detailed for getting an overview. The reader gets overwhelmed by the large number of results, and it seems arbitrary to identify the main patterns in these tables (also given the wide 95% CI's). My specific suggestion is to formulate a priori the specific questions to be addressed, or hypotheses to be tested, and then to perform and present some specific analyses, each of which is focussed on a separate question or hypothesis.

2. Focus on socioeconomic inequalities instead of adding gender inequalities as well. The paper would be a stronger focus if all analyses and results are interpreted from the perspective of socioeconomic inequalities. Gender inequalities are a topic on its own, of interest in a separate paper. In a paper on socioeconomic inequalities, gender is important only in as far as it confounds or modifies the effect of socioeconomic factors. This would imply, amongst other things, that in the Discussion section the results for Day1-27 case fatality would be re-interpreted from a socioeconomic perspective.

3. Re-evaluate the conclusion on the persistence and importance of socioeconomic factors. There is a tendency to stress that magnitude and importance of the inequalities observed. None the less, according to the key results in table 1, socioeconomic inequalities in Day0 case fatality are exceptionally small (6% odds difference) while those for Day1-27 (>10% odds difference only for the lowest 2 groups out of 7) are modest as compared to the magnitude of socioeconomic inequalities that is generally observed for health outcomes. In addition, according to the author's interpretation of table 2, deprivation did not contribute to geographical inequalities. These findings do not seem to support statements in the discussion such as “reducing case fatality rates in the most disadvantaged populations is key to reducing total AMI mortality in Scotland ...”

4. In the Discussion section, reconsider the role attached to the known risk factors for AMI incidence. There is strong tendency in the Discussion section to attribute inequalities in case fatality rates to inequalities in risk factors such as lifestyles. However, in the midst of page 12, no references are given to support
the assumption that population trends and differences in case fatality rates are strongly determined by these risk factors. The only references given further down the text, for Day1-27, finds that most risk factors such as smoking and overweight are NO determinants of case fatality (ref 53). If positive evidence is not available from the epidemiological literature, the section should be fundamentally rewritten.

5. Omit figure 1. This figure is not informative to a reader who is not familiar with Scottish topography. To such a reader, there is no message emerging from these regional differences. The only pattern that seems to be illustrated here, higher case fatality rates in rural regions, is not directly visible in this figure, and moreover is already documented in reference 5.

Minor Essential Revisions

6. Explain why logistic regression was applied instead of loglinear regression. For this type of “count” outcome, loglinear or Poisson regression is more common. In addition, the reader is now confronted with Odds Ratios instead of Rate Ratios. The latter would be much more straightforward to interpret.

Discretionary Revisions

7. Restructure the long paragraph on page 9. Some sentences may be moved to the Methods section, whilst other sentences with detailed results may be omitted. Moreover, for the Discussion section and final conclusions, table 2 is much less informative than tables 1 and (new, concise versions of) tables 3 and 4.

8. Discuss in more detail the limitations inherent to the use of an area-level measure of socioeconomic position. Often, such a measure underestimates the magnitude of socioeconomic inequalities as compared to what the individual-level measures can reveal. Is there evidence from Scotland or England for a similar effect in case of AMI case fatality? If so, how much is the magnitude of underestimation?

Level of interest: An article of importance in its field

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’