Reviewer's report

Title: The impact of social deprivation on mortality following acute myocardial infarction, stroke or subarachnoid haemorrhage: A record linkage study

Version: 4 Date: 31 March 2015

Reviewer: Brendan Smith

Reviewer's report:

Thank you for the continued opportunity to review this manuscript which examines the relationships between social deprivation on mortality after acute myocardial infarction, stroke and subarachnoid hemorrhage. The manuscript as presented is improved in this latest version, including details that resolve many of my initial comments.

Major Compulsory Revisions

However, there are a number of essential revisions suggested by both of the reviewers which were not adequately address in this latest version of the manuscript, including:

1) Ambiguity remains around the answer to Reviewer #2’s comment #1 (or #23 as listed in the response to reviewers). This relates to the issue of my comments #5 and #6. While an interaction analysis has been added, the results from Table 2 continue to be interpreted by the authors stating whether the association between social deprivation and mortality is statistically significant, and not whether the association is consistent across levels of the variable. I would agree with Reviewer #2’s initial comment that the authors need to clarify their intent and make this consistent with their secondary objective.

2) Reviewer 1, Comment #18 – To clarify my original comment, it would be useful to include a typical “Table 1” in your study. While the ultimate format is up to the authors’ discretion, it would be useful to have social deprivation quintile as columns including “admissions (% mortality)” and patient comorbidities and risk factors as rows with separate sections for AMI, stroke and SAH as in the current Table 1. At minimum a table with admissions and % mortality by AMI, stroke and SAH for all comorbidities and risk factors. This is not an overly complicated exercise, and would help readers interpret your results by providing basic information. For example, how many of your admissions are taking place in community hospitals and how whether that number impacting your results in Table 2 for AMI, where a large OR is observed, but it did not reach statistical significance.

3) Reviewer 1, Comment #19 – There may be a discrepancy in language between authors and reviewer, so I leave it to the Editor to decide the preference for this journal. In my opinion there is a difference in reporting that mortality is 30% higher in deprivation quintile V compared to quintile I, instead of a 30%
increased odds of AMI mortality in deprivation quintile V compared to quintile I. It is not really a matter of reporting an odds ratio or a % increase or decreased odds. Either way, in my opinion, these should be reported with 95% confidence intervals to help the reader interpret your findings (in the tables, text and abstract).

Minor Essential Revisions
Reviewer 1, Comment #4 – In the clarified analyses it would seem that the key risk factors are conceptualized as effect modifiers of the relationship between social deprivation and mortality following AMI, stroke, SAH.

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

**Quality of written English:** Needs some language corrections before being published

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

**Declaration of competing interests:**
I declare that I have no competing interests.