Reviewer's report

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

Version: 3 Date: 13 January 2015

Reviewer: Brendan Smith

Reviewer's report:

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

1. Overall, a more in depth background section providing additional context for the reader would improve the manuscript. It would be helpful to introduce issues such as greater specificity of the severity and scope of the problem (e.g. high mortality) and what mechanisms are proposed to explain the hypothesized deprivation inequalities in mortality for the 3 conditions.

2. Reading the background I am also unclear what the proposed study will add to the debate of social deprivation and mortality following acute myocardial infarction, stroke or subarachnoid haemorrhage. A few sentences discussing a specific contribution would strengthen the paper.

3. A significant limitation to the paper was the second objective of “determine how any increased mortality for deprived groups may be affected by factors such as patient demographics, timing of admission and hospital size”. Including a greater discussion and rationale in the background for the second objective would support investigation of the proposed associations.

3 a) Continuing on with the second objective, in the “methods” section under “risk factors”, it is not clear how patient demographics, timing of admission and hospital size are hypothesized to influence the association between social deprivation and mortality. For example, are they considered confounders of the relationship or mediators?

3 b) In the “Risk factors” section it was described that to assess the impact of risk factors on the relationship between social deprivation and mortality (i.e. to compare the least and most deprived cases using the least deprived quintile as the reference group). As written, I am not clear what methodology has been used. In particular when in the “Methods and analysis” section, paragraph 2 it describes that logistic regression was used to assess whether “increased mortality for deprived groups may be affected by key risk factors including patient demographics...”. I am not clear how this produced the results presented in table 2.

3 c) In the “Results” section, “effect of factors on the increased mortality with social deprivation” section, – The results presented in this section and in Table 2 are not clear. As mentioned in the methods section, it remains unclear what regressions are determining the presented odds ratios. Are these the odds of
mortality given social deprivation stratified by each level of the risk factors?

3 d) In the discussion section, paragraph 1, sentence 2: Based on the analysis, I am not able to evaluate this sentence based on the evidence provided.

4. In the “patient comorbidities” section, it is unclear how the factors discuss play into the analysis in this study. Specifically, they are not mentioned again throughout the manuscript, with the exception of appearing as a limitation in the “discussion” section, final paragraph. This is extremely important potential source of confounding that should be quantified in this study. Further the sentence “This methodology is described elsewhere [27]” is not appropriate. Please describe any methodology used to adjust for patient comorbidities in this study. Were any other confounders considered besides age and sex?

5. In the “Methods of analysis” section, paragraph 2 the analytical approach described uses logistic regression to achieve the study outcomes. Please describe the rational for not using a time to event analysis (e.g. cox proportional hazard model). It is assumed that the time to event is available given that mortality at different time points is being assessed.

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

1. In the “Methods” section, “study design”: a few details could be added if appropriate for this data linkage study. Mainly: 1) what percentage of patients were able to be linked to other data sources (e.g. to the SAIL, ONS and AR)?; 2) what percentage of all deaths to study subjects were ascertained in the ONS and AR? 3) Was there any other missing data? For example, in the footnotes to Table 1 you have specified “Gender for 1 case was not recorded for AMI, WIMD score was available for 3.2% of AMI cases”.

2. In the “Methods” section, under “study design”, second paragraph: This section could be written with more detail to facilitate clarity for readers not familiar with the datasets used in the study.

3. In the “inclusion and exclusion criteria” section: it is stated that all emergency admissions to Welsh hospitals were selected for AMI, stroke or SAH. What is the coverage for your study for events (e.g. AMI mortality) that were not admitted to an emergency hospital (e.g. those that died in the community)? If a large amount this should be discussed in the limitations section.

4. In the “Mortality” section, can you provide a rational for choosing to look at 30 day mortality? Also, in the first sentence 365 days was mentioned as a secondary outcome but no results were discussed in the manuscript.

5. In the “social deprivation” section, it is not clear whether the domains of deprivation are based on individual characteristics, or whether they relate to community/neighbourhood level percentages.

6. In the “Methods of analysis” section, the first sentence reads “the main study outcome measures were percentage mortality rates at 30 days...”. Is the main study outcome not the increased odds of mortality in low compared to high
deprivation as would be suggested by the use of logistic regression analyses?

7. In the “Methods of analysis” section, paragraph 2, sentence 1: “any increased risk”, should read “any increased odds” given that a logistic regression has been used.

8. In the “Methods of analysis” section, paragraph 2, sentence 4: I don’t see Bonferroni correction as necessary given the statistical test described in this manuscript. Was there multiple statistical tests performed that were not outlined in the method section? If so, please provide a description of statistical testing undertaken.

9. In the “Results” section, it would be helpful to have Table 1 describe admissions and mortality rates by social deprivation for each of the risk factors and patient comorbidities. Could move any odds ratios to a second table.

10. In the “Results” section– The results from logistic regression analyses are not interpreted correctly. It is an increase odds, not a percentage. This continuous throughout the whole section. Ideally the odds ratio and 95% confidence intervals would be presented for each association discussed.

11. In the “Results” section– At some point in the manuscript it would be appropriate to perform a power analysis to determine whether there is enough sample size to find an association between social deprivation and mortality for the SAH outcome.

12. In the discussion section, paragraph 6, sentence 4: A clear rationale for why seasonal variations in the social inequalities in mortality would be expected. If, as suggested, that an increased population-based mortality during summer months was related to air pollution, would this not effect more and less deprived areas equally? Are there other potential mechanisms?

13. In the discussion section, paragraph 7, sentence 1: Results from a time trend analyses were not presented in the study. Please include this analysis in the manuscript if this is of interest to the authors.

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

1. There are a few acronyms that I do not believe were ever fully described in the text (e.g. NRES, NHS)

2. Sex and gender are not synonymous. One should be used consistently depending on how the relationship is being conceptualized.

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, but I do not feel adequately qualified to assess the
statistics.

**Declaration of competing interests:**

I declare that I have no competing interests