Author's response to reviews

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

Authors:

Kymberley Thorne (k.thorne@swansea.ac.uk)
John G Williams (j.g.williams@swansea.ac.uk)
Ashley Akbari (a.akbari@swansea.ac.uk)
Stephen E Roberts (stephen.e.roberts@swansea.ac.uk)

Version: 5 Date: 8 May 2015

Author's response to reviews: see over
Dear Editor,

We are grateful for ongoing reviewer comments for our article entitled “The impact of social deprivation on mortality following acute myocardial infarction, stroke or subarachnoid haemorrhage: A record linkage study”.

We are pleased that Reviewer 2 has no further comments and have addressed Reviewer 1’s additional comments as follows:

Reviewer 1

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.

As suggested, we have now amended the abstract and main text so that it relates directly to the secondary objective and the results of the interaction analysis. All edits or additions are in blue text.

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.

As suggested, have now added a new table (designated Table 2) reporting the number of admissions and 30 day mortality rates for all three conditions for the most and least deprived quintiles according to each risk factor in the study. We have also attached an extended table showing the data for all five deprivation quintiles but we prefer to use the table inserted in the paper as this is more reader-friendly. We would be happy for the tables to be swapped should the Editor prefer the extended table.

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).

In our revised manuscript we had followed the advice of Reviewer 2 on this matter and used the word “higher” rather than “increased”. However, we would be happy to oblige any editorial preferences. We have added 95% confidence intervals to all results throughout the manuscript, as suggested by the reviewer.

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.

We agree with this statement.

Reviewer 2

The authors have done an excellent job of responding to all of my comments. Congratulations on an excellent manuscript.

No amendments required.