Author's response to reviews

Title: Persistent socioeconomic inequalities in cardiovascular risk factors in England over 1994-2008: a time-trend analysis of repeated cross-sectional data

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Version: 3 Date: 22 August 2011

Author's response to reviews: see over
Dear Victorino Silvestre and Prof Young-Ho Khang

MS:  5438487145395786


Shaun Scholes, Madhavi Bajekal, Hande Love, Nathaniel Hawkins, Rosalind Raine, Martin O’Flaherty and Simon Capewell

Thank you very much for inviting a revised manuscript.

We attach, as suggested, a point-by-point response to the issues raised.

We are very grateful for your constructive comments and have incorporated them into the revised manuscript. All amendments are shown as “underlines” to aid visibility.

We hope this manuscript is now acceptable for publication in BMC Public Health

With Best Wishes

Yours Sincerely

Shaun Scholes, Madhavi Bajekal, Hande Love, Nathaniel Hawkins, Rosalind Raine, Martin O’Flaherty and Simon Capewell
Editor's comment:

“As Dr Harper indicated, I cannot see the real rationale for using both logistic and log-binomial regressions. I also believe that prevalence ratio from log-binomial regression should be presented rather than OR from logistic regression considering that PR is the measure of interest and OR cannot be a good approximation of PR where the outcome prevalences are high.”

Thank you. We have taken this suggestion on board. The revised manuscript now only shows the results using log-binomial regression.
Reviewer’s report

**Title:** Persistent socioeconomic inequalities in cardiovascular risk factors in England over 1994-2008: a time-trend analysis of repeated cross-sectional data

**Version:** 2 **Date:** 13 July 2011

**Reviewer:** Kyunghee Jung-Choi

**Reviewer’s report:**
My previous comments were correctly taken for revision. There is a question which still needs some clarifications. Were the regression analyses for inequalities adjusted for age? As the authors noted, the two age groups (16-54, >=55 years) are too broad. The results should be presented with proper method of age adjustment.

Thank you. Sample sizes in the Health Survey for England are too small to present results by finer age bands (e.g. 25-34), particularly for those risk factors (cholesterol, diabetes and physical activity) which are only measured in intermittent years.

In the revised manuscript we show the results for the two broad age bands – but we have now adjusted for age in the regression modelling (in addition to using an age-standardised weight). Our revised text in the methods section (Page 9) is as follows:

“Secondly log-binomial regression models were used to estimate annual change in prevalence ratios (PR) using the specific cardiovascular risk factor as the dependent variable with survey year and age as continuous independent variables.”

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a Statistician

**Declaration of competing interests:**
I declare that I have no competing interests
Reviewer's report

Title: Persistent socioeconomic inequalities in cardiovascular risk factors in England over 1994-2008: a time-trend analysis of repeated cross-sectional data

Version: 2 Date: 17 July 2011
Reviewer: Sam B Harper

Reviewer's report:
1. This is an excellent revision of an initial paper looking at secular trends in cardiovascular risk factors in England. The authors have done a nice job of responding to comments and suggestions. I have only a few minor issues with this version.

2. p5. The comment that “using relative measures alone fails to allow monitoring of changes in overall population health or changes in risk factor levels across groups” seems a little confusing. Maybe consider revising as “absolute risk factor levels”?

   Thank you. Our revised text (Page 5) reads as follows:

   “using relative measures alone fails to allow monitoring of changes in absolute risk factor levels across groups”.

3. p8. The revised text describing the IMD measurement is much better, but I would still like to see the authors address the issue of its retrospective validity. How well does it measure, or, more precisely--given the authors state that it was created in 2004)--how does it measure deprivation in 1994? It was still not clear to me. Furthermore, as written it still seems as though LSOAs were classified in 2007 and this classification assumed to apply in all previous periods. Is that correct? Wouldn't this lead to some misclassification by deprivation status in earlier years?

   Thank you. We have added a paragraph in the limitations section to discuss this issue. The text (Page 24) is as follows:

   “One limitation of our study was that Health Survey for England respondents were assigned to the 2007 IMD measure (based on 2005 data) rather than assigned to a deprivation quintile compiled on data around the time of interview. This leads to the question of whether IMD 2007, which expresses the relative position of LSOAs to the average for England as a whole in 2005, is an accurate marker of deprivation across all survey years. A study using a comparable area-based indicator over 1991-2001 showed declines in absolute levels of deprivation accompanied by continuity in the relative deprivation status of wards [54]. Since 2001, the 2004, 2007 and 2010 IMD measures have retained broadly the same methodology, domains and indicators [19]. Our analyses showed reassuring stability in the relative
position of LSOAs. Agreement between the 2004 and 2010 quintiles was 76% (kappa statistic = 0.70), indicating a good level of agreement.

4. p10. I thank the authors for clarifying the presentation of models with quadratic trends, but I still find the text confusing. As written, the sentence “Quadratic trends were examined for risk factors that had a (mostly) continuous data series and are shown only if statistically significant at the 1% level.” implies that quadratic trends were fit for all models but only shown when crossing the magic p-value threshold. But the authors’ response suggests that they were dropped from models without significant p-values. If so, then I would revise this sentence to make sure readers know that in the cases where quadratic terms are not shown, they were not in the model.

Thank you. Our revised text (Page 10) now reads:

“Quadratic terms not significant at the 1% level were removed from the model leaving just the linear trend.”

5. p11. I think it is great that the authors used binary regression, but, given that we are likely to be more interested in prevalence ratios than the ratio of prevalence odds, why even bother with the logistic regression analyses at all? There are already a huge number of models and estimates presented, and I can’t see any reason (other than computational convenience and familiarity) for the use of the estimates from the logistic models. The paper would be better off (shorter, clearer) without them.

Thank you. The revised manuscript now only shows the results using log-binomial regression.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: I declare that I have no competing interests
Reviewer's report

Title: Persistent socioeconomic inequalities in cardiovascular risk factors in England over 1994-2008: a time-trend analysis of repeated cross-sectional data

Version: 2 Date: 21 July 2011

Reviewer: Bruna Galobardes

Reviewer's report:

The authors have covered a significant number of the questions raised. I do have, however, remaining concerns about two important aspects.

Removing the health domain from IMD:

The authors argue there is no need to remove the income domain from the IMD citing an article (ref number 50) that found little difference in health outcomes using census data (as stated in title). The obvious answer to this is that this may not apply to other outcomes and should, at least, be investigated. For my own knowledge I searched the article and I was reassured just by reading the abstract (which made me wonder if the authors have only read the title):

“Removing the health domain had little, practical, effect on measured socioeconomic inequalities in census measures of health. These findings may not hold for other measures of health, and in the context of socioeconomic inequalities in health, removing the health domain from IMD 2004 probably represents best practice.”

I would encourage the authors to do what is best practice.

The second argument used to defend no need to remove the health domain has no bearing in this issue and should be deleted: “Furthermore, such “mathematical coupling” can also occur with individual-based markers such as occupation or income being a consequence as well as cause in any association with health [33]”. What the authors are referring to is reverse causality, a separate and very different issue. Reverse causality has been investigated in cardiovascular disease and it has been found to explain little of the socioeconomic gradient.

Thank you. We have deleted the second argument from the manuscript. We have been able to investigate the impact of excluding the health domain from the IMD 2007 measure. Our revised text (Pages11-12) reads as follows:

"Sensitivity analyses – IMD minus the health domain

The IMD includes a health component which may lead to overestimation of the association between area-based deprivation and risk factors. We created an ‘IMD-minus-health’ quintile variable by standardising and exponentially transforming the six non-health domains and computing a non-health score by reallocating the health domain weight across the other domains in proportion to their original weights [18-19, 22] and matching this variable to
the survey data. Our analyses showed that excluding the health domain had little practical effect on the magnitude of absolute and relative inequalities with no systematic pattern in the differences, and thus the results of using the full IMD are presented here. (Results from the IMD measure excluding the health domain are available on request)."

Merits of using area-based rather than individual measures of SEP:

Whilst stating correct facts such as that contextual measures of SEP show associations with health outcomes there is a conceptual error in considering contextual level “better” than individual. It all depends on the level of inquiry and the authors are interested in individual, not contextual, level of socioeconomic position (SEP). The Health Survey of England (HSE) has individual level indicators of SEP that are not being used. There is a principle here, and I think it is important, about using the best data available. There is no basis or need for using area-level deprivation as a surrogate for individual-level SEP when individual indicators are available. If the authors were interested in area level inequalities, then they have the excellent opportunity to use appropriate multilevel models to investigate the effect that area deprivation has beyond the individual SEP. That would be very interesting, but it is a different question and would require different and appropriate statistical methodology.

Thank you. We argue that using an area-based measure enables us to monitor similarities in trends in both risk factors and CHD mortality rates. Our revised text (Pages 22-23) is as follows:

“We chose IMD, a well-established marker of assigning socioeconomic circumstances based on area of residence for three main reasons. Firstly studies continue to show contextual associations between neighbourhood and health even after controlling for individual-level markers [48]. Residential deprivation is powerfully linked to health due to the influence of both composition (characteristics of individuals who live there) and context (features of the location itself) [49]. Area-based measures therefore may contribute additional socioeconomic information over and above that obtained from individual-level measures. Secondly area-based measures are particularly useful proxy measures of individual social position in older age groups [50-51]. Occupational-based schemas such as the UK National Statistics socioeconomic classification (NS-SEC) are not recommended for studying inequalities at older ages because of the large proportion that cannot be accurately classified. Third, stratifying Health Survey for England respondents by IMD enables us to examine whether recent changes in the magnitude and/or direction of socioeconomic gradients in CHD mortality may be explained by similar changes in its key risk factors. Area of residence (postcode) is recorded on death certificates and so area-based measures of deprivation can, to some extent, circumvent the difficulties in attributing socioeconomic status to older people and to women [52-53]. Using identical stratifying variables to monitor changes in inequalities in both cardiovascular risk factors and death rates can shed important light on the possible potential drivers of longevity and inform discussions on possible future trends.”
**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

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