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:** 1  **Date:** 13 May 2011

**Reviewer:** Bruna Galobardes

**Reviewer's report:**

This manuscript investigates the trends in inequalities in cardiovascular risk factors between 1994 and 2008 in England. This is a well written paper in a relevant area of research using data from the Health Survey of England.

I have some comments about specific aspects of the methodology:

1) In page 5, the authors state including risk factors irrespective of medication use because they are not interested in the underlying reason generating the inequalities.

Lipid lowering drugs were included in their assessment of total cholesterol (stated in Table 1). People under treatment are still at higher risk of developing cardiovascular disease compared to those who are not, therefore I would argue that to fully account for the inequalities in risk factors treatment should be included in the definition.

2) Measure of socioeconomic position (SEP):

Page 6 – The authors use the full index of Multiple Deprivation which, as they report, includes a health domain (years of potential life lost, Comparative Illness and Disability Ratio, Measures of acute morbidity and proportion of adults under 60 suffering from mood or anxiety disorders). This results in a circular argument as health is in both sides of the equation. It is best practice to exclude the health domain from the indicator when the outcome of interest is health related.

The main reason why the authors use an aggregate measure of SEP rather than the individual level (available in the study and their level of interest), is because these analysis are part of a larger project for which individual level will not be available for other outcomes. This is debatable given that this is submitted as an independent piece of research. I would even argue that carrying these analyses with the appropriate individual level of SEP may actually provide valuable information for the larger project because they will have an estimate of how much of the inequalities are underestimated because of the inevitable use of aggregate measures in the other CVD outcomes.

3) Analysis:

I am not clear from how the IMD quintiles are entered in the model, as an ordinal or as a categorical variable.
In order to report the inequalities in each risk factor the authors calculate a Relative Index of Inequality (RII) and then test whether this changed between the 90s and the 2000s. I believe the calculation of this RII is unnecessary in this case. Given that the authors are using quintiles of deprivation, a simple ordinal variable could have been used to summarize, in one value, the change in risk factor by deprivation level (although whether an ordinal variable provides a good description needs to be tested, more on this below). The RII is useful when comparing unequal groups as it weights for the different proportions of population in each group. However, by definition quintiles have equal proportions in each group. Furthermore, the RII, as an ordinal variable assumes linearity across groups. Thus, categorising deprivation quintiles in a 5 ordinal level variable should give to the same results and would appear more straightforward analysis. The authors should test though, whether the linearity assumption holds and otherwise analyse quintiles of deprivation as a categorical variable (but the RII is not a solution to this as the assumption of linearity is also true for the RII).

I am also not clear why there is a need to test the 90s versus the 2000s, given that annual changes are available (unless there is an a priori reason why risk factors trends should be different between the two decades rather than change continuously over time). Whether deprivation changes over time is captured in the interaction test between quintiles of deprivation (ordinal if linearity holds or as a categorical variable otherwise) with year of survey. If I understand it correctly, this is the main analysis the authors have already done, therefore I am clear why there is a need of grouping years into decades. It is important to report this interaction though, because as it is in Tables 1 and 2, the authors provide a p-value (for the trend) within each quintile but these do not report whether there is a difference in trend by quintiles of deprivation.

4) Reporting of results
I am of the opinion that trends are best shown with a graph. The authors do provide these but as supplementary material. I would suggest to bring these to the main paper and provide the yearly and quintile data in supplementary tables.

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'