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”?

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?

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.

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.
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.