Reviewer's report

Title: Racial disparities in infant mortality: What has birth weight got to do with it and how large is it?

Version: 2 Date: 3 September 2010

Reviewer: Enrique F Schisterman

Reviewer's report:

The authors have undergone an extensive revision in order to put their work into the context of other work in this area. The paper is much improved, however, not all of the points have been sufficiently addressed in the revised manuscript, and some points require additional discussion or clarification as mentioned below. In addition, the paper is very long and would benefit from some tightening of the language throughout the manuscript--several sections could be shortened without losing any important points.

Major Compulsory Revisions

1. The authors have discussed several of the assumptions of their effect decomposition model and how they compare with counterfactual methods. But a few points deserve further clarification. In particular, how do the conditional independence assumptions differ between the counterfactual vs. non-counterfactual approaches? While a specific DAG does not have to be specified, appropriate conditional independences still must be described. How was this assumption tested, or is there evidence that the relationships described were met? The proposed model also depends on several parameters -- how was model fit assessed. Lastly, as mentioned in the discussion section, strong unmeasured confounding assumptions are required. The statement made that "Given this view, there are no unmeasured effects of race, on birth weight or infant mortality by definition" is quite strong and requires further explanation. While the authors are careful to state that it is unlikely that dividing the population into "normal" and "compromised" subpopulations will account for all of the unmeasured confounding, the fact that this analysis does not adjust for confounding of observed covariates is a significant limitation. The authors need to explain more of the potential impact of this unmeasured confounding.

2. Can the authors explain in more detail how the pediatric paradox is associated with the compromised subpopulation -- the paradox usually refers to the low birth weights, but the compromised population here includes weights at both the high and low ends of the spectrum.

3. Page 8 refers to the use of z-scores in order to break the association between race and birth weight. However, in the cited reference (Schisterman et al. 2009), while the use of birth weight z-scores does break the association between birth weight and the exposure, adjustment for z-scores only allows for estimation of
total effects (not effect decomposition). Can the authors describe how this fits in with the proposed framework?

Minor Essential Revisions

1. Some of the statistical notation is cumbersome and should be revised for improved clarity. For example, in eq 17 the mean bar should just above the P, rather than the entire term. Also, eq 16 should complete the last steps required to define the probability of infant death as \( \exp(A+Cx\ldots) / (1+\exp(A+Cx\ldots)) \), rather than leaving as \( \exp(A+Cx\ldots)\cdot(1-Pr(Y)) \).

Minor Issues Not for Publication

1. Please check the reference for Geneletti 2007 – no journal is specified.

2. Abstract, Results: "Model-correction this effect indicate..." should read "Model-correction for this effect indicates..."

3. Model Fitting: Remove s in "...confidence intervals s are..."

4. Conclusions, first sentence: delete "appears to be correct"

Level of interest: An article of limited interest

Quality of written English: Acceptable

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.