Reviewer's report

Title: Primary prevention of lead poisoning in children: A cross sectional study to evaluate state specific lead-based paint risk reduction laws in preventing lead poisoning in children

Version: 4
Date: 1 September 2014

Reviewer: Joseph Braun

Reviewer's report:

I have reviewed the responses from the authors and selected portions of the text. The authors have adequately address some of my comments, but I still have a few major concerns related to ecological confounding, the dust Pb measurements, and missing data. I believe the presentation of the results could be simplified to improve the manuscript's interpretation.

Major Compulsory Revisions

Comment 1: I appreciate the authors adding a description of why they chose to study MS. While the efforts to adjust for so many confounders are commendable, I believe it would still be worthwhile to mention the potential for ecological confounding in the discussion.

Comment 2: I am still not convinced that the dust Pb measurements add much to the manuscript. The authors state that it was pertinent to have these measures since they are related to the risk of Pb poisoning and detection of subsequent cases. However, since dust Pb data were collected at different times relative to the identification of a contaminated dwelling in the different states, they do not represent the same construct and their measurement error in measuring is correlated with the primary effect of interest (state of residence). Thus, I strongly encourage the authors to drop Tables 5 and these analyses.

Comment 3: There appears to be a substantial amount of missing data for the different covariates since the unadjusted model contains 292 addresses and the final model contains 115. Could the authors please comment on how this might affect their results? I suggest comparing characteristics of the residences and participants with and without complete data.

Minor Essential Revisions

Comment 1: The manuscript has a large number of tables and many seem unnecessary or overly complicated. I strongly suggest dropping Table 3 since there is not strong justification for providing this level of detail for one single covariate. Tables 4 could be simplified by dropping the p-value column.

Table 6 is confusing. It is not clear if the ORs displayed are for the effect estimate of MA/OH vs. MS or the actual covariate. The last column heading says they are
the former, but the footnote says they are the latter. I suggest simplifying the table by presenting the unadjusted and adjusted ORs. The effect estimates for the covariates is not as interesting for this particular paper.

Comment 2: The paragraph related to gentrification contains opinion and non-referenced observations (e.g., “urban blight” and “changing structure of inner city neighborhoods”) and does not seem directly relevant to the study at hand. I strongly suggest deleting this paragraph or substantially rewriting this paragraph so it does not have an editorial tone and only contains statements/evidence relevant to this study.

Comment 3: The first sentence of the methods is confusing as written.

Comment 4: Please display the odds ratios in Figure 1 on the natural log scale.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.