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: 1 Date: 27 May 2014

Reviewer: Joseph Braun

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The authors used data from the CDC Childhood Blood Lead Surveillance System database to determine if state lead laws reduce the incidence of elevated blood lead levels in children. Specifically, they identified housing that was occupied by a child with elevated blood lead levels (index) and then determined if any children subsequently living in that house had elevated blood lead levels. They compared whether the number of subsequent children with EBLs was higher in MA/OH compared to MS, since MA/OH have stricter laws for preventing lead poisoning.

This is an important question since there has been little recent research into the effect of laws aimed at preventing lead-poisoning. Using pre-existing, programmatic data can efficiently address this question, although the lack of potentially important confounders in these databases is a limitation. My main concerns with this study are the selection of Mississippi (MS) as a control state, presentation of the methods and results, definition of subsequent cases, and lack of control for key confounders (detailed below).

Major Compulsory Revisions

1. The justification for using MS as a control state seems somewhat arbitrary. Are there other states that meet the criteria set forth by the authors (i.e., no lead laws)? This is especially problematic given the ecological nature of the exposure in this study (state lead laws) and the fact that there are profound differences in the features of the states being compared.

2. Related to this point is the concern for ecological confounding. Differences in the rate of blood lead screening may be driving the relationship observed in this study. In fact, this is suggested by Table 2, which shows that the rate of screening was higher in MA compared to OH or MS. Thus, the observed relationship may not be related to the state laws, but the presence of more aggressive screening that is better at detecting children at-risk for EBLs.

The potential for confounding related to temporal changes in housing characteristics, sociodemographics, and neighborhoods is a potential concern with this type of data. The authors briefly mention changes in housing stock over time on Page 18, but could adjust for the time elapsed between the index and subsequent case in their statistical models. Other confounders worth considering would be individual level characteristics that are associated with lead poisoning, including child sex, age, and race/ethnicity.
3. Did the authors consider examining continuous blood lead levels of children who lived in houses after remediation by state to see if interventions may be effective at reducing blood lead levels among children who do not meet the programmatic definition of lead poisoned? Furthermore, is it possible to see if more children have blood Pb’s greater than CDC’s current EBL definition of 5 μg/dL in MS compared to OH/MA? In addition, the different programmatic definitions used to identify the index case should be noted in the discussion.

4. The authors should clearly articulate in the manuscript and abstract that the unit of study is the state, and that comparisons are being made among a set of children who are living in homes that were previously occupied by a child who had an EBL.

5. The dust Pb measurements don’t seem relevant to the manuscript and distract from the main findings. Furthermore, given that they were collected at different times relative to the abatement, they aren’t very comparable.

6. Page 18-Is it really a good thing that these neighborhoods are being gentrified? This drives people out of these neighborhoods and increases the cost of living for the remaining residents.

Minor Essential Revisions

1. Abstract: Please define the threshold(s) used to designate EBLL. Also, please state the sample size and years/place of study.

2. The power calculations are unnecessary since it sounds like they were done post hoc. If they were done before the study was conducted, then the authors need to define the OR that they had sufficient power to detect.

3. The authors mention “target number” throughout the manuscript. I suggest defining this or changing the term to something less “jargon-y”.

4. Page 11-I suggest changing “Summary Level 050” to something that sounds less like jargon.

5. Did the authors conduct analysis of MA vs. MS and OH vs. MS to see if the results were driven by one state?

6. Table 3 seems unnecessary since it is related to one covariate.

7. Could Table 2 be merged with Table 4?

8. I suggest dropping Table 5 since these data are not directly relevant to the manuscript’s primary question.

9. Table 6 is confusing. It is not clear what the sample size numbers refer to. Is this the N in the model or the n with that characteristic? Are the presented beta coefficients from a single model or different models with each term run one-at-a-time? The term ‘Slope Parameters’ could be confusing. I suggest the following

   a. Clearly designate the variables included in the final model.

   b. Change “slope parameters” to effect estimates and only present the OR and its 95% CI for all the variables.
c. Include the OR for the effect estimate of OH/MA vs. MS.
d. Specify which covariates are modeled as continuous variables.
10. Figure 1 could be dropped if the OR for the state effect estimate is presented in Table 6.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

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

Declaration of competing interests:
I declare that I have no competing interests.