Reviewer's report

**Title:** Area-level poverty and preterm birth risk: A population-based multilevel analysis

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**Reviewer:** Robert Goldenberg

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In this study, the authors attempt to distinguish between individual and area level (poverty) factors associated with preterm birth. They used a large statewide Missouri data set collected over a 10 year span.

The paper is generally well-written and the analysis seems appropriate.

**Specific Comments:**

At best, the authors have found a weak association between county level poverty and PTB after adjusting for individual characteristics, and they should not make too much of this weak association. The last sentence of the Abstract is therefore not appropriate. There is nothing in this paper to suggest that enhancing access to preventative health services in areas with high poverty rates will change anything. Similar comments in the Discussion should be removed as well.

There are probably available many other county level data other than poverty such as crime rates, toxic waste dumps, etc. that could have been studied as well. There is nothing wrong with just focusing on poverty, but it may not totally explain their findings. This should be addressed in the Discussion.

Also, while they discuss this issue, many papers dealing with this topic use smaller geographic areas such as zip code or census tracts because the counties are relatively large and the percent of poverty, even in the "poorest" counties are relatively low. This should be addressed. While it probably does not make much difference in the outcome, the composite variable of medical risk seems highly
heterogeneous. Why include infant > 4000 gm for example.

There are some misspellings - "poverty" on Page 11, "which" on Page 16. Also some references are either wrong or incomplete - see numbers 4, 26, 23.

In the Discussion, 1st paragraph they acknowledge that the OR's for poverty are modest - but then go on to argue that this is "important." I am not sure. On the top of Page 15 they say "our results suggest that county environments do in fact independently influence birth outcomes." I think "may" is more consistent with a weak association.

On Page 15, they talk about the rising incidence of PTB. Ananth et al have shown that this rise is all do to indicated PTB and physician choice. This should be referenced, and again there is no data to support the contention that "targeting preventive health care strategies" has any benefit on the PTB rate. Did the authors consider looking just at spontaneous preterm birth since this is the outcome most likely associated with poverty.

In the first full paragraph on Page 17, country level poverty did not "increase" the risk of PTB. At best, it was "weakly associated" with an increase in PTB, and the last sentence of that paragraph has no basis in science, as does the last sentence in the Conclusion.