Reviewer's report

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

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Reviewer: Kenneth Schoendorf

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Comments on BMC Public Health ms – Area-level poverty and preterm birth risk: a population-based multilevel analysis

General comments
- This analysis links birth certificate and Census data to examine the association between county-level poverty and preterm birth (< 35 weeks) in Missouri during the 1990s. A moderate association was reported after adjustment for a number of individual-level factors. Understanding potential causes behind the seemingly intractable preterm birth problem in the US is undeniably of great public health import, as is understanding the interplay between societal and medical factors. This manuscript addresses those issues in a reasonable fashion, including appropriate treatment of the chosen community-level exposure. However, it does so in a perhaps overly straight-forward fashion that does not add substantially to the extant literature. Further exploration of some of the themes introduced in the analysis may have shed more light on the topic. Some thoughts to consider are:

- the potential for interaction between specific individual-level characteristics and area-level poverty – For instance, is the risk to someone otherwise in a high-risk group (e.g., poor, low education, “black race”) ameliorated by residence in a low-poverty area?

- relating the simple and uni-dimensional county-based poverty measure used in this analysis to other area measures, such as segregation, income inequality, geographic availability of obstetric care, etc.

Essential Revisions
1. Sample size – a likely easy fix, it is important to present the data accurately. Methods, para 2, 1st line refers to 711,015 births from 1978-98. However, the study used only births from 1989-98, had additional exclusions, yet had a reported sample size of 634,994.

2. Cleaning of gestational age data – the potential problems inherent in gestational age reporting on birth certificates are paid lip service in the Discussion, but are not addressed. Defining preterm as <35 weeks, instead of 37 weeks, does not alleviate the systemic problems described in the Alexander reference and elsewhere. The gestational age data should be handled appropriately according to some accepted method (e.g., Alexander, Zhang).
3. Construction of medical risk factor variable – the creation of the variable should be described more fully. One assumes that having any one of the conditions is the determining factor, but that should be stated. It may be worth considering if all the risk factors actually carry the same weight. For instance, history of previous preterm birth likely carries a much higher risk of subsequent preterm birth than does anemia. Also, the limitations of these data obtained from birth certificates could be more clearly outlined.

4. Use of paternal education – This is undoubtedly a potentially important factor in determining individual-level social position. However, on birth certificates, paternal data is often missing in a large and non-random fashion. Some information concerning the percentage missing (perhaps for all variables but certainly for this one) should be provided.

5. Policy and intervention relevance – the Introduction (end of 1st paragraph) and the Conclusion (last paragraph) both suggest that the results of this analysis will be useful in guiding interventions and policy to reduce preterm birth. Unless the study’s results can be used to more fully expound on these claims, they do not hold sufficient weight. Some specific examples would be useful. This is especially important given the statements in paragraph 3 of the Discussion, suggesting that, for preterm birth, individual-level interventions may be more beneficial. In addition, the ability of such research to constructively guide public health or larger societal interventions may be strengthened by the use of more complex area-level measures, as mentioned in the comments above.

6. Relationship of this analysis to other contextual/multi-level analyses of birth outcome in the U.S. – perhaps this is my own bias, but the manuscript seems to overstate the uniqueness of the study design in relation to previous analyses examining area-based measures and birth outcome. Numerous previous papers have addressed similar issues, including some used as references in this manuscript. It is true that “these studies vary significantly with respect to sample size, study design, and methodologic strategies.” [Discussion, para 2] However, many previous studies are sufficient in all those categories; the differences may exist but do not render them worthy of dismissal. Similarly, it is also true that “…prior studies have been limited by design constraints and the inability to thoroughly account for potential confounding risks.” [Conclusions, para 1] Given the sources of data and the nature of the problems being studied, this statement is as true of this analysis as it is of many of the others. It is inherent in this type of research and is necessary to recognize, but is not solved in this analysis.

7. Minor editorial comment – The Results-Individual-level measures section (para 1) repeats “marital status” and “presence of medical risk factors”.

Discretionary revisions

8. Given the unsurprisingly stability of the models, showing all 5 seems unnecessary. It seems as though showing 1, 3, and 5, at a maximum, would demonstrate the point just as well.
Level of interest: An article of insufficient interest to warrant publication in a scientific/medical journal

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 I have no competing interests.