

Reviewer comments from Journal of Epidemiology and Community Health

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1 Reviewer 1

The authors present a well written manuscript that explores the association between maternal physical exercise and the risk of preterm birth in Wuhan, China using a case-control study design. It is estimated China had the second largest proportion of PTB internationally in 2014, only behind India. Additionally, only 11% of pregnant women in China meet the international guidelines for physical activity during pregnancy, a significantly lower proportion than that in Western countries. Therefore this study is highly relevant.

1. Last part of the last statement of the "What this study add?" is not fully supported with the data provided in the manuscript - "[...] but overwhelming physical exercise (more than 150 minutes per day) may increase the risk of preterm birth."
2. It is unclear for the ratio for the matched controls to the cases based on birthdate.
3. The environmental variable was actually abnormal smells in the home. Why were abnormal smells outside of the residence not considered. If there were perceived outdoor exposures individuals might tend to exercise outside less.
4. Since 28.9% of the respondents had to be excluded from analysis, sensitivity analysis were needed which the authors provide.
5. Unclear results presented: On line 47 family history of PTB was listed at 7% and 3%. But the table is not given in percent. Perhaps the table should provide percent number. The units listed are mean (sd). This might be the same issue with maternal disease as well.

2 Reviewer 2

This manuscript examines the association between maternal physical exercise and preterm birth in a sample of women who delivered during 2011-2013 in Wuhan, China. Given the size of Wuhan, the authors should give the reader more information on how the cases were selected, as it's highly unlikely to be true that (as the manuscript states), the sample constitutes all women with preterm or low birth weight babies in the seven core urban districts of Wuhan during the study period (ie n=2,393). The manuscript does not include the preterm birth rate in Wuhan or China in general. Based on a recent Lancet article, the PTB rate in China is 7.3% ([https://doi.org/10.1016/S0140-6736\(18\)32702-8](https://doi.org/10.1016/S0140-6736(18)32702-8)). The population of Wuhan is estimated at 11 million, and a recent estimate of the crude birth rate for China as a whole is 12.4/1000. This would suggest about 136,000 annual births in Wuhan, and if 7.3% were preterm, this would yield about 10,000 per year. The present study is a two year study, and admitted not for the entire population of Wuhan, but the authors need to give a better explanation of how the cases were identified, as all indications are that some sampling was done, and likely some exclusions were made.

1. Additionally, on p 8, line 34, the authors indicate that full-term was defined as 38-42 weeks gestation, while preterm was defined (with no lower limit) as less than 37 weeks gestation. So what about babies delivered at 37 weeks? Were these ignored, and if so, with what rationale?
2. Turning to measurement of physical activity, the methods (p 8) do not clarify at what time this measurement was taken. It is asked of women at 7 or more months gestation (roughly, 28-30 weeks gestation), so again readers need to know the lower limit of gestational age used to define preterm birth.
3. Outdoor sports are one form of physical activity, but one wonders whether women may have exercised in other ways that should also be documented. Some discussion of measurement as a limitation of the study is in order.
4. A few other comments on variables and measurement are also in order. The choice to categorize maternal age as ≤ 21 , 22-28, and > 28 should be justified, as this is a very different classification than what is typically used in perinatal epidemiology. Family history implies much more than history of preterm or low birth weight birth, and it doesn't make sense to use newborn gender as a proxy for genetics (p 9 line 16). Collapsing the long list of health conditions into a single measure of comorbidity also likely gives more prominence to some conditions less likely associated with preterm birth). It would be important to capture conditions occurring in pregnancy that are known to be associated with preterm birth, and examine causal mediation in relation to physical activity.
5. The authors should also examine their categorization of BMI. This is indicated as ≤ 18.5 , 18.5-24, and > 24 . Its fine to group overweight and obese together, but in most reports from developed countries, we use 18.5-24.9 and ≥ 25 to separate normal BMI. In the interest of having results comparable to the general literature it might be helpful to classify prepregnancy BMI accordingly.
6. Given that almost 30% of study subjects did not report minutes of exercise per day (p 11, line 3), this should be indicated in Table 1. Other variables with missing data should also be indicated in Table 1. There is no mention that multiple imputation or similar techniques were used to better understand the implications of missingness for the study findings.
7. Table 2 should show the crude and the adjusted odds ratios, and format in a more traditional manner (rather than having widely spaced separate columns for the lower and upper limits of the 95% CI). The title should emphasize what's being studied rather than the method, perhaps 'Table 2: Crude and Adjusted Credible Intervals for Preterm Birth, Wuhan China Study'. The methods can be notated at bottom. The sample included should be indicated, here and in tables that follow. Similarly for Table 3.
8. Given that the results in Table 4 are for a sample that excludes about 30% of the study subjects, how comparable are these results to those using logistic regression methods?
9. Its interesting what variables are not included in this study. Does the mother work outside the home? How much physical activity is involved in the work place. Did the mother experience adverse health events during pregnancy, including gestational diabetes, pregnancy-induced hypertension or pre-eclampsia, or placenta ischemic disease? Did delivery occur early for a medical indication? All of these are known to be associated with preterm birth. Also not indicated is whether all pregnancies studied involved singletons; if not, plurality should be a covariate.

All in all, this is an interesting paper, but as written it raises more questions about study design, methods, and presentation of findings than it conveys in terms of those findings.