PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Does Neighborhood Socioeconomic Status Predict the Risk of Preterm Birth? A Community-based Canadian Cohort Study
AUTHORS	Adhikari, Kamala; Patten, Scott; Williamson, Tyler; Patel, Alka; Premji, Shahirose; Tough, Suzanne; Letourneau, Nicole; Giesbrecht, Gerald; Metcalfe, Amy

VERSION 1 – REVIEW

REVIEWER	Reviewer name: Wei Tu Institution and Country: Georgia Southern University, USA Competing interests: NA
REVIEW RETURNED	17-Aug-2018

GENERAL COMMENTS	Using data from two cohort studies in Alberta, Canada (n=5,297), this study developed a regression model to examine if neighborhood socioeconomic status (SES) could help better predict the risk of preterm birth (PTB). The conclusion is that, while the combination of individual and neighborhood level indictors did improve the overall prediction of PTB compared to the models using only the individual level predictors, the predictive model had poor detection rates for PTB. This paper hinges upon a broader and significant topic related to the neighborhood SES effect on birth outcomes, so the theme of the papers fits well with the scope of this journal. The methods used for data collection and processing and model building and validation procedures are standard and results were overall well explained. I think that this manuscript should be accepted for publication although I do have a few comments/suggestions for the authors to consider. I had trouble understanding the following two sentences"…Furthermore, in our study, reduction of some of the neighborhood variance after the inclusion of neighborhood SES would have reduced the predictive role of the neighborhood random effect (21). However, the multilevel model simultaneously improves the prediction of PTB through the addition of the regression coefficient for the neighborhood SES variable…" (Paragraph 1, P11). 1. What exactly is "the predictive role of the neighborhood random effect?" 2."…the addition of the regression coefficient for the neighborhood SES variable", Are you trying to say that the regression coefficient has increased? If so, how is this related to the improvement of the predicting power of the model? "Furthermore, as neighborhood variation in PTB (as measured by ICC) corresponds to the predictive accuracy (as measured by AUC)(21)– when the ICC is high the AUC is also high, the information about the variation in PTB at neighborhood level offers some understanding about the ability of neighborhood level factors to predict PTB(21).

	However, previous research has emphasized identifying neighborhood level risk factors associated with PTB or causal effects, which is difficult to establish due to the potential challenges…"(Paragraph 2, P12). ICC measures the share of the variance in the outcome variable that are explained by the neighborhood level variance, not neighborhood variation (this should be measured by the neighborhood variance), thus the following interpretation between the relationship of AUC and ICC needs to be rewritten. I am also wondering how to interpret the model results of neighborhood level variable (e.g., the index) for women living at the same neighborhood as they all share the same value for the variable. It will also be important to add some discussion on how would the MAUP (http://gispopsci.org/maup/) and UGCoP (http://www.meipokwan.org/UGCOP.html) impact the selection of neighborhood level variable and then the model performance. Several minor issues: 1. There should be a % sign followed by ICC (next to the last sentence, p8).So ICC should be 5.72%, not 5.72. 2. MOR should be interpreted in the results. 3. Perhaps the neighborhood level median personal income can be ignored in the text since the results were not reported.

REVIEWER	Reviewer name: Daphne McRae Institution and Country: University of British Columbia Competing interests: Since May of 2018 I have worked as an independent consultant for the Midwives Association of British Columbia.
REVIEW RETURNED	26-Oct-2018

GENERAL COMMENTS	This a well conceptualized, clearly explained study. The authors have addressed an important clinical issue, with population and health equity implications, using a thorough and systematic analytical approach. The three models developed support the conclusions drawn as do the numerous measures of model discrimination. However, clarification of the following issues would strengthen the paper: 1) In the introduction there is very little explanation of the mechanisms thought to link neighbourhood low SES to preterm birth (PTB). The fact that the rate of PTB in low SES neighbourhoods is higher than the rate in high SES neighbourhoods could be explained by residents with higher individual-level risk factors living in low SES neighbourhoods. Is there something about the neighbourhood itself that increases the risk of PTB? The authors alluded to this (stress, environmental factors), but expanding on this explanation would strengthen the rationale for the study. 2) It would be helpful for the authors to provide justification for the retention of variables in the models that had a p-value <0.1 rather than the conventional 0.05. 3) Could the authors conduct a sensitivity analysis including the variables "previous PTB" and "prenatal care visits" in the models, using the "All Our Families" dataset? If so, this could alleviate doubts about the absence of critical variables in the models, particularly previous PTB.

	4) In the conclusion the authors state that the predictive performance of the model was too low to consider its application in clinical practice, but then continue to say that knowledge about neighbourhood context may help healthcare providers in identifying women most at risk of PTB. I think a conclusion that would better align with the results would emphasize the contribution this study makes to the literature and its research implications (i.e. pg.14, line 3 suggests that the predictive model developed is an important first-step, what would the second-step be?). Overall, this is a robust study. I recommend accepting it with minor revisions.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Wei Tu

Institution and Country: Georgia Southern University, USA

Please state any competing interests or state 'None declared': NA

Please leave your comments for the authors below

Using data from two cohort studies in Alberta, Canada (n=5,297), this study developed a regression model to examine if neighborhood socioeconomic status (SES) could help better predict the risk of preterm birth (PTB). The conclusion is that, while the combination of individual and neighborhood level indictors did improve the overall prediction of PTB compared to the models using only the individual level predictors, the predictive model had poor detection rates for PTB. This paper hinges upon a broader and significant topic related to the neighborhood SES effect on birth outcomes, so the theme of the papers fits well with the scope of this journal. The methods used for data collection and processing and model building and validation procedures are standard and results were overall well explained. I think that this manuscript should be accepted for publication although I do have a few comments/suggestions for the authors to consider.

1) I had trouble understanding the following two sentences"…Furthermore, in our study, reduction of some of the neighborhood variance after the inclusion of neighborhood SES would have reduced the predictive role of the neighborhood random effect (21). However, the multilevel model simultaneously improves the prediction of PTB through the addition of the regression coefficient for the neighborhood SES variable…" (Paragraph 1, P11). 1. What exactly is "the predictive role of the neighborhood random effect?" 2."…the addition of the regression coefficient for the neighborhood SES variable", Are you trying to say that the regression coefficient has increased? If so, how is this related to the improvement of the predicting power of the model?

Response: In the original manuscript, we were trying to explain observed unchanged discriminatory accuracy between the multilevel model, with and without neighborhood SES variable in our study. Since our explanations are speculative, they have been removed.

2) “Furthermore, as neighborhood variation in PTB (as measured by ICC) corresponds to the predictive accuracy (as measured by AUC)(21)– when the ICC is high the AUC is also high, the information about the variation in PTB at neighborhood level offers some understanding about the ability of neighborhood level factors to predict PTB(21). However, previous research has emphasized identifying neighborhood level risk factors associated with PTB or causal effects, which is difficult to establish due to the potential challenges…”(Paragraph 2, P12). ICC measures the share of the variance in the outcome variable that are explained by the neighborhood level variance, not neighborhood variation (this should be measured by the neighborhood variance), thus the following interpretation between the relationship of AUC and ICC needs to be rewritten.

Response: Thank you very much for indicating this. We have rewritten interpretation for ICC: “the share of the variance in PTB that are explained by the neighborhood level variance.”

3) I am also wondering how to interpret the model results of neighborhood level variable (e.g., the index) for women living at the same neighborhood as they all share the same value for the variable.

It will also be important to add some discussion on how would the MAUP (http://gispopsci.org/maup/) and UGCoP (http://www.meipokwan.org/UGCOP.html) impact the selection of neighborhood level variable and then the model performance.

Response: Thank you very much for your comment. Use of area-based variables such as neighborhood level variable, where women living in the same neighborhood share the same value for the variable, can be a methodological problem. Individuals who live in the same neighborhood (defined by dissemination area in our case) may experience different contextual influences from many other areal units besides their home neighborhoods. Similarly, the timing and duration in which individuals experienced these contextual influences is also uncertain. Thus, it is hard to interpret neighborhood influences for women living at the same neighborhood, creating an uncertainty in the performance of the model that contains the neighborhood level variable. We have acknowledged these issues in the limitation section. We have also indicated that we used the smallest area-level unit (dissemination area) to define neighborhoods and used multilevel analysis to account for the neighborhood variation. These are known as suitable approaches.

4) Several minor issues: 1. There should be a % sign followed by ICC (next to the last sentence, p8).So ICC should be 5.72%, not 5.72. 2. MOR should be interpreted in the results. 3. Perhaps the neighborhood level median personal income can be ignored in the text since the results were not reported.

Response: We have revised these as suggested. Regarding the neighborhood level median personal income, the tables contain results associated with neighborhood level median personal income; thus, instead of ignoring the results in the text, we briefly interpreted them.

Reviewer: 2

Reviewer Name: Daphne McRae

Institution and Country: University of British Columbia

Please state any competing interests or state ‘None declared’: Since May of 2018 I have worked as an independent consultant for the Midwives Association of British Columbia.

Please leave your comments for the authors below

This a well conceptualized, clearly explained study. The authors have addressed an important clinical issue, with population and health equity implications, using a thorough and systematic analytical approach. The three models developed support the conclusions drawn as do the numerous measures of model discrimination.

However, clarification of the following issues would strengthen the paper:

1) In the introduction there is very little explanation of the mechanisms thought to link neighbourhood low SES to preterm birth (PTB). The fact that the rate of PTB in low SES neighbourhoods is higher than the rate in high SES neighbourhoods could be explained by residents with higher individual-level risk factors living in low SES neighbourhoods. Is there something about the neighbourhood itself that increases the risk of PTB? The authors alluded to this (stress, environmental factors), but expanding on this explanation would strengthen the rationale for the study.

Response: The link between the neighborhood itself (including neighborhood SES) and PTB has been expanded upon in introduction

2) It would be helpful for the authors to provide justification for the retention of variables in the models that had a p-value <0.1 rather than the conventional 0.05.

Response: As the focus of our model was prediction or estimation (not hypothesis testing), it is reasonable to include a predictor with a p-value higher than 0.05 as long as it improves the predictive ability of the model. In our case, few variables met the initial criteria (i.e., p<0.25 in bivariate analysis) to be potential candidate variables considered for inclusion in the full multilevel model. We decided to use a p-value threshold of <0.1 instead of <0.05 to increase the chance of their retention in the final model, and to allow us to assess their predictive ability. We have now provided a justification for using p-value<0.1 in the manuscript (supplementary file).

3) Could the authors conduct a sensitivity analysis including the variables "previous PTB" and "prenatal care visits" in the models, using the "All Our Families" dataset? If so, this could alleviate doubts about the absence of critical variables in the models, particularly previous PTB.

Response: A sensitivity analysis, including these two variables in the final models, was performed. The model performance did not change: AUC increased by 2% for the logistic regression model, but did not change for the multilevel model. We have described this in the methods and results sections.

4) In the conclusion, the authors state that the predictive performance of the model was too low to consider its application in clinical practice, but then continue to say that knowledge about neighborhood context may help healthcare providers in identifying women most at risk of PTB. I think a conclusion that would better align with the results would emphasize the contribution this study makes to the literature and its research implications (i.e. pg.14, line 3 suggests that the predictive model developed is an important first-step, what would the second-step be?).

Response: We agree with your comment. The conclusion has been revised: the contribution of this study to the literature and the future research direction and implications have been added.

5) Overall, this is a robust study. I recommend accepting it with minor revisions.

Response: Thank you!

VERSION 2 – REVIEW

REVIEWER	Reviewer name: Wei Tu Institution and Country: Georgia Southern University, USA Competing interests: N/A
REVIEW RETURNED	14-Dec-2018

GENERAL COMMENTS	I thank the efforts that the authors have made in addressing the questions of the reviewers and I think that it is ready to be published.

REVIEWER	Reviewer name: Daphne McRae Institution and Country: Postdoctoral Research Fellow, School of Population and Public Health, University of British Columbia, Canada Competing interests: Since May of 2018 I have worked as an independent consultant for the Midwives Association of British Columbia.
REVIEW RETURNED	21-Dec-2018

GENERAL COMMENTS	The authors have done a good job addressing all of the comments in the initial review. They have adequately expanded on the study rationale in the introduction and avoided any overstatement of clinical implication in the conclusion, choosing to focus on future research needs. The sensitivity analysis shows diligence in using all available data and further reinforces their conclusions. The addition of the MOR interpretation in the text adds greater clarity to the results. I recommend accepting this paper as is.