Reviewer's report

Title: Variations in Preterm Birth Rates by Level of Prenatal Care and Ethnicity in New Mexico

Version: 2 Date: 31 October 2011

Reviewer: Jason L Salemi

Reviewer's report:

Major Compulsory Revisions:
- Although the authors appear to have attempted to address my initial comment, I still feel as though the title implies a focus on specific factors, but only because those factors had a statistically significant association with risk of PTB in the analysis. It is a completely exploratory study and the title should reflect this.

- Methods: The authors have contradictory statements in the Methods and Discussion section regarding their key outcome variable: the determination of gestational age. In the Methods section, they state it is “based on last menstrual period…” and “…was consistent with the physician’s best estimate of the due date.” However, in the discussion they state, “This study used the physician’s estimated gestational age…” and “…because the calculated gestational age was not available for many records.” This does not give much confidence to the reader that there was an established method of determining gestational age and, thus, preterm birth.

- Methods: In my first review, I asked whether the authors took into account the inherent clustering of the data (e.g. Generalized Estimating Equations to account for within-mother dependency). The authors responded that this is an exploratory study rather than hypothesis testing, but that does not preclude the necessity to apply the appropriate statistical test. Additionally, the authors stated that it was “difficult” to model the correlation structure and took the “simplest” approach and treated multiple outcomes as independent. This is not, in my opinion, adequate justification. They have enough statistical power to use an unstructured correlation structure and the “simplest” solution does not mean it is valid. The sentence provided in the paper, “Since our study was exploratory in nature, we did not limit the analysis to one record per unique woman…” does not make sense to me. I do not think this is necessary, but if multiple outcomes per woman are allowed and since we are researching maternal characteristics, an appropriate analytic strategy should be adopted, or at least a reasonable explanation why it is not being used.

- Methods: Also in my first review, I asked whether the authors considered alternative ways of determining/categorizing adequacy of prenatal care, such as the GINDEX or R-GINDEX (Greg Alexander). The authors stated that they, “did not explore alternate measures of prenatal care, though this would indeed be interesting.” I do not feel that is sufficient. If the title is correct and adequacy of

prenatal care is a primary factor in this study, and this journal has a recently published paper (Heaman, 2008) that concurs with the 1996 Alexander and Kotelchuck paper that states, “As these indices are conceptually distinct in their measurement approach, they are likely to yield different patterns of prenatal care use in a population and cannot be used interchangeably. Recommendations for their use are provided.” I feel, since it is such an integral variable in the paper, that there should at the very least be a discussion providing the rationale for choosing the Kotelchuck index and if other indices might yield different results.

Minor Essential Revisions:

- Lacking citations for the sentence on page three beginning, “In observational studies…” There is a reference for the randomized studies, but not for the initial claim regarding the findings of observational studies.

- Background, 3rd paragraph: I do not like the last sentence. Why is it important? Is this the focus of the paper? I thought the focus was on maternal race-ethnicity AND prenatal care.

- Methods: I repeat my first recommendation that the paper would benefit from a clearer understanding of how many and what percentage of records were excluded for congenital anomaly outcomes and for multiple gestation. Simple numbers as to how many were excluded for each reason would be sufficient.

- Methods: How would someone that achieved high school graduation be treated? I imagine they would be in the 9-12 years group. I think, however, that there is sufficient heterogeneity between someone completing 9, 10, or 11 years of schooling and someone that makes it through and actually graduates from high school. Perhaps “high school grad (12 years)” could be an additional category to differentiate it from those starting but not completing high school?

- Methods: There is a statement that begins, “Based on clinical interest, the interaction between...”. I think a stronger argument could be made here, despite the study being exploratory. What reason is there to drive this interaction term? Why would you think there would be a different association between adequacy of prenatal care and preterm birth by maternal “risk group”? “Clinical interest”, to me sounds like a weak argument.

- Results: The authors keep referring to “risk” in the results section; however, this is a cross-sectional study with logistic regression being done. I would suggest that “odds” be used in place of risk.

- Table 1: Why are all of the factors in the model not listed in this table (smoking, alcohol consumption)? I suggest inclusion of all factors for the “main effects” model. This will provide the main effects of “high/normal risk” pregnancy and adequacy of prenatal care separately. Then a figure could highlight the effect modification and show the odds of preterm birth stratified by risk-PNC groups. Also, as mentioned earlier, unadjusted ORs and 95% CIs would be beneficial to reveal the impact of adjustment on each factor. If space becomes a factor, the p-values not necessary since confidence intervals are provided.
- Tables and Figures: I do not feel that a separate figure for each factor in Table 1 is needed. This is redundant information. If there is one figure highlighting the primary factor(s) of interest, that is great, but there is just too much redundancy as provided. If figures are included (i.e., figure 1), suggest adding values of each point on the graph and slope, p-value of trend line.

- Discussion, 2nd paragraph: As mentioned in my initial review, you interpret the finding, among high risk pregnancies, that intensive care decreases the risk for PTB. However, you do not discuss the finding that the exact opposite happens among normal-risk pregnancies. This is a big finding – they have 4-fold measures of association in the normal risk subset, but the authors seem to shy away from any plausible explanation as to why this might be happening. They stated that they intentionally included an interaction term in the model to see what happens – they should offer something aside from “other factors must be at play here” as stated in their response to my comment.

Discretionary Revisions:

- Background, 3rd paragraph: suggest changing “national increase” to “overall increase”
- Background, last sentence: suggest changing “correlate with” to “are associated with”
- Methods, 2nd paragraph: suggest changing “and because it has been” to “and because maternal education has been”
- Discussion, 2nd paragraph: For the statement, “Having a high risk pregnancy increased the risk of PTB, similar to findings from numerous studies”, there is one reference although multiple studies are alluded to.
- Table 1: Two categories are listed as “Normal Risk Pregnancy and Level…”. I believe the second should begin “High Risk”.

Level of interest: An article of limited interest

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 that I have no competing interests.