

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	A Cohort Study of Early Literacy and Childbearing Over the Reproductive Lifecourse
<b>AUTHORS</b>	Seymour, Jane; Frasso, Rosemary; Shofer, Frances S.; Bennett, Ian

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Tzu-I Tsai National Yang-Ming University, Taiwan
<b>REVIEW RETURNED</b>	23-Aug-2016

<b>GENERAL COMMENTS</b>	<p>The paper examined the association between early-life literacy and childbearing across women's reproductive lifecourse in the US. The explaining factor was measured in 1980, and the cumulative childbearing as the outcome was assessed in 2010.</p> <ol style="list-style-type: none"><li>1. Despite grand multiparity has been considered as a risk factor for adverse maternal outcomes in the past, grand multiparity no longer need to be considered an obstetrical risk in the presence of good perinatal care, especially in high-income developed country. Large family size maybe also due to cultural or religion reasons. Literature should discuss what race/ethnicity or cultural group prefer large family size, and what this study found.</li><li>2. There was a possibility that women who had grand multiparity were less likely to have an abortion. The maternal risk of grand multiparity might not higher than the risk of abortion. The data could not identify women who had ever had abortion and take the numbers of abortions into account.</li><li>3. What are the risk factors of maternal morbidity and mortality? The authors addressed unintended pregnancies, teenage pregnancy were related to risks of childbearing. But grand multiparity is not necessary the case of unintended pregnancy or teenage pregnancy. The information cannot support grand multiparity is an obstetrical risk in a developed country.</li><li>4. What accounts for the association of early literacy and maternal outcomes? This paper would be strengthened by a clarification of, and the roles played by early literacy on perinatal outcomes. The paper needs theoretical base or causal pathway to support how the relationship between early literacy and perinatal outcome is causal. What are hypothesizing pathways?</li><li>5. Since this is a longitudinal study, in additional to cumulative parity, it would be interesting to reveal the trajectory of each birth of a woman's age, as well as compare literacy effects on birth trajectory.</li><li>6. The reading grade level in 1980 was the explaining factor. Since this paper only collected the educational level of women aged 14-22 at a single point (in 1980), the education might not represent their highest reading level in the survey year. In particular, the study participants were across various education stages in the survey year. Need more literatures to support early literacy instead of</li></ol>
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	<p>educational attainment is a sensible and good indicator in this issue.</p> <p>7. There were several limitations of this study. What future research needed to help us clarify or advance our understanding, eliminate the adverse impacts of low early literacy on maternal health?</p> <p>8. Much more detail about the practical implications of this work is needed.</p>
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<b>REVIEWER</b>	<p>Yhenneko J. Taylor          Director, Outcomes Research and Evaluation          Carolinas HealthCare System          Charlotte, NC, U.S.A</p>
<b>REVIEW RETURNED</b>	20-Sep-2016

<b>GENERAL COMMENTS</b>	<p>The authors address a very useful topic with a very robust set of data. However, several limitations to their approach can be addressed to improve the strength of their findings.</p> <p>Statistics</p> <ul style="list-style-type: none"> <li>• It would be useful to state the study hypothesis at the end of the introduction.</li> <li>• It would be useful if the authors would conduct a sensitivity analysis to determine the impact of excluding women who had missing parity data in 2010. It is possible that using parity data from 200 or 2008 for only a subset of women may bias the results. How many women had missing parity data in 2010?</li> <li>• It is unclear what is meant by a forced entry logistic regression model. Did the authors use a selection procedure to select the final variables included in the model? It seems like many important confounders are excluded from the model, e.g. marital status, occupation, income, health status. The authors seem to focus on variables from only one part of the life course without mention of several other factors that may influence parity.</li> <li>• It would be useful to include a test for trend if the authors want to be able to state that there is an increasing association between RGL and odds of grand multiparity.</li> <li>• Did any respondents have children at the time of RGL assessment? If so, how did the analysis control for this?</li> </ul> <p>Results</p> <ul style="list-style-type: none"> <li>• It is not clear what is meant by “poor white supplement” in the first sentence in the Results section.</li> <li>• Table 1 compares those with missing parity data to the included sample. It would be useful to understand comparisons to everyone who was excluded (including those with missing ASVAB scores). Also, in Table 1, the number with missing parity is stated as 659 vs. 695 in the text.</li> <li>• Table 2 repeats many of the same statistics as Table 1. The authors could provide new information by stratifying Table 2 by the outcome of grandmultiparity.</li> <li>• The footnote in Table 3 should indicate whether the p-values are for comparisons between all RGL level or just the lowest and highest. The text reads as if comparisons were made only between the lowest and highest categories of RGL.</li> </ul> <p>Discussion</p> <ul style="list-style-type: none"> <li>• The authors state in the discussion that the association is in a “stepwise manner.” The authors did not conduct trend tests to suggest this. The authors should clarify if something else is meant by this phrase.</li> </ul>
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	<ul style="list-style-type: none"> <li>• The authors mention controlling for “identified confounders.” The process for selecting confounders is vague in the methods section. Multiple unmeasured confounders may impact the results.</li> </ul> <p>Limitations</p> <ul style="list-style-type: none"> <li>• The authors should address sources of unmeasured confounding.</li> </ul> <p>References</p> <ul style="list-style-type: none"> <li>• Reference number 15 should be revisited. The authors state in the introduction that “childbearing...is one the leading causes of morbidity and mortality.” This statement may need to be qualified to reflect the setting (low and middle income countries) or deleted altogether.</li> <li>• Some mention of what factors we already know influence parity (other than literacy) may be helpful.</li> </ul>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer Comment	Author Response
<b>Reviewer 1</b>	
Despite grand multiparity has been considered as a risk factor for adverse maternal outcomes in the past, grand multiparity no longer need to be considered an obstetrical risk in the presence of good perinatal care, especially in high-income developed country.	We respectfully disagree on this point and have referenced the persistent risk associated with grand multiparity in the US which is a high income country. We have also added referenced introduction and discussion which supports the larger point that there is morbidity and mortality associated with childbearing which occurs with every pregnancy and so is cumulative additional risk for women with more births – a part of the lifecourse frame that we take in this analysis. We point out that in countries like the US with very high rates of unintended pregnancy discussions of this risk is relevant to public health.
Literature should discuss what race/ethnicity or cultural group prefer large family size, and what this study found.	We have added comments to this effect and appreciate the suggestion.
There was a possibility that women who had grand multiparity were less likely to have an abortion. The maternal risk of grand multiparity might not higher than the risk of abortion. The data could not identify women who had ever had abortion and take the numbers of abortions into account.	We appreciate the reviewer’s hypothesis but disagree. It is widely documented that in countries such as the US where safe abortion is available, the risk of mortality and morbidity associated with abortion is much lower, by around an order of magnitude, than carrying that same pregnancy to term. In addition, the purpose of the current analysis was to look at the association of literacy with total parity in the United States and not what specific mechanisms might be related to it. We discuss the general point that mechanisms are beyond the scope if this paper.
What are the risk factors of maternal morbidity and mortality? The authors addressed unintended pregnancies, teenage pregnancy were related to risks of childbearing. But grand multiparity is not necessary the case of unintended pregnancy or teenage pregnancy. The information cannot support grand multiparity is an obstetrical risk in a developed country.	The Background now more explicitly notes maternal factors associated with high parity. As mentioned above we have referenced the support for risks of pregnancy in the US including the added risk of grand multiparity.

<p>What accounts for the association of early literacy and maternal outcomes? This paper would be strengthened by a clarification of, and the roles played by early literacy on perinatal outcomes. The paper needs theoretical base or causal pathway to support how the relationship between early literacy and perinatal outcome is causal. What are hypothesizing pathways?</p>	<p>The Discussion now notes that this paper was not designed to explore causal pathways, but rather associations and so we make no claims of such mechanisms. As noted in the Methods section, covariates were selected using Andersen's Behavioral Model and the study was designed using the lifecourse conceptual framework. We agree with the point that additional investigations are needed to explore such causal mechanisms.</p>
<p>Since this is a longitudinal study, in addition to cumulative parity, it would be interesting to reveal the trajectory of each birth of a woman's age, as well as compare literacy effects on birth trajectory.</p>	<p>We agree that the analytic approach suggested by the reviewer is appropriate to this type of data and could provide additional value. It is our view that the approach we have taken is also appropriate and has appeal in its simplicity.</p>
<p>The reading grade level in 1980 was the explaining factor. Since this paper only collected the educational level of women aged 14-22 at a single point (in 1980), the education might not represent their highest reading level in the survey year. In particular, the study participants were across various education stages in the survey year. Need more literatures to support early literacy instead of educational attainment is a sensible and good indicator in this issue.</p>	<p>A significant result of the current paper is providing evidence that literacy is a social determinant of childbearing independent of educational attainment. We have controlled for educational attainment at time of literacy assessment for this purpose. As pointed out by the reviewer this is not a perfect measure and we have discussed this in the limitations section. This is an important point as there has been relatively little attention paid to literacy in much epidemiologic and demographic literature. We have added text to that effect in the background along with referenced studies from the health and literacy literature which support this independence of literacy from educational attainment.</p>
<p>There were several limitations of this study. What future research needed to help us clarify or advance our understanding, eliminate the adverse impacts of low early literacy on maternal health? Much more detail about the practical implications of this work is needed.</p>	<p>In the Discussion we more explicitly note the practical implications, need for intervention studies, and importance of considering literacy as a risk factor when creating policy to reduce maternal risk.</p>
<p><b>Reviewer 2</b></p>	
<p>It would be useful to state the study hypothesis at the end of the introduction.</p>	<p>The study hypothesis is now explicitly stated at the end of the Discussion section.</p>
<p>It would be useful if the authors would conduct a sensitivity analysis to determine the impact of excluding women who had missing parity data in 2010. It is possible that using parity data from 200 or 2008 for only a subset of women may bias the results. How many women had missing parity data in 2010?</p>	<p>In the Results section we note that the final analysis was re-run using only those women with complete parity data in 2010 and did not vary from the reported results which included those whose parity was missing in 2010 and . Results did not change.</p>
<p>It is unclear what is meant by a forced entry logistic regression model. Did the authors use a selection procedure to select the final variables included in the model?</p>	<p>We have clarified language in the Methods to note that confounders included in models were identified <i>a priori</i>. There was no statistically based selection procedure for selecting covariates - they came from our conceptual model.</p>
<p>It seems like many important confounders are excluded from the model, e.g. marital status, occupation, income, health status. The authors seem to focus on variables from only one part of the life course without mention of several other factors that may influence parity.</p>	<p>We now explicitly note that this study focused on time-invariant confounders measured in early-life. In the Discussion section we note that future studies should consider the effect of time-varying confounders.</p>

It would be useful to include a test for trend if the authors want to be able to state that there is an increasing association between RGL and odds of grand multiparity.	We appreciate this suggestion and have added a Cochran-Armitage test for trend. The results support our findings that RGL is associated with grand multiparity.
Did any respondents have children at the time of RGL assessment? If so, how did the analysis control for this?	All models now control for whether or not a woman was parous prior to 1980.
It is not clear what is meant by "poor white supplement" in the first sentence in the Results section.	In the Methods section we clarified language explaining this supplemental cohort which was dropped after the first few years of the NLSY (in the early 1980's).
Table 1 compares those with missing parity data to the included sample. It would be useful to understand comparisons to everyone who was excluded (including those with missing ASVAB scores).	We have added an additional column to Table 1 with covariate information for those missing ASVAB.
Also, in Table 1, the number with missing parity is stated as 659 vs. 695 in the text.	We have corrected this error.
Table 2 repeats many of the same statistics as Table 1. The authors could provide new information by stratifying Table 2 by the outcome of grand multiparity.	We appreciate this suggestion and Table 2 is now stratified by grand multiparity.

The footnote in Table 3 should indicate whether the p-values are for comparisons between all RGL level or just the lowest and highest. The text reads as if comparisons were made only between the lowest and highest categories of RGL.	The footnote in Table 3 has been clarified to explain that the chi-square and Kruskal-Wallis p-values compare all reading grade levels.
The authors state in the discussion that the association is in a “stepwise manner.” The authors did not conduct trend tests to suggest this. The authors should clarify if something else is meant by this phrase.	As noted above, a test for trend is included and the results support our description of the association being in a stepwise manner.
The authors mention controlling for “identified confounders.” The process for selecting confounders is vague in the methods section. Multiple unmeasured confounders may impact the results.	We appreciate this important point and have clarified that we were interested in time-invariant confounders measured in early-life and note potential unmeasured confounders that could bias results. This is included in our discussion of limitations along with a statement of the reason for limiting our controls to these variables.
The authors should address sources of unmeasured confounding.	As noted above, in the Conclusion we more clearly state potential unmeasured confounders.
Reference number 15 should be revisited. The authors state in the introduction that “childbearing...is one the leading causes of morbidity and mortality.” This statement may need to be qualified to reflect the setting (low and middle income countries) or deleted altogether.	We agree that this is a confusing point that is tangential to the overall direction of the study and this reference has been deleted.
Some mention of what factors we already know influence parity (other than literacy) may be helpful.	We appreciate this suggestion to expand our background – we have done so and factors that are associated with high parity are more clearly noted in the Background.

#### VERSION 2 – REVIEW

<b>REVIEWER</b>	Yhennoko Taylor Carolinas HealthCare System USA
<b>REVIEW RETURNED</b>	07-Nov-2016

<b>GENERAL COMMENTS</b>	The authors have done a good job addressing all reviewers comments. One suggestion would be to change the label "Non-Black, non-Hispanic" to "Other, non-Hispanic" to be consistent with other reporting of race and ethnicity.
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