PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Can Antipoverty Programs Save Lives? Quasi-experimental evidence from the Earned Income Tax Credit in the United States
AUTHORS	Muennig, Peter; Vail, Daniel; Hakes, Jahn

VERSION 1 - REVIEW

REVIEWER	Melvin Livingston Emory University, USA
REVIEW RETURNED	06-Mar-2020

GENERAL COMMENTS	The presented paper is an important contribution to the evidence base for the health effects of EITC (and income supplementation programs more broadly). A particular strength of the paper is the ability to identify probable EITC receipt at the individual level. The following comments are meant to help clarify the results and identification strategy. 1. “The hypothesis that EITC might reduce premature mortality is supported by previous research.” While this may be true, the cited articles do not consistently say this. Larrimore 2011 seems to find no short term effects on EITC on health; Schmeiser 2009 finds increase in obesity risk with increased EITC, etc. From the cited articles the supporting evidence seems mixed. 2. Is there a reason dollar amounts are expressed in 2015 dollars and not 2011 dollars (the end of follow-up)? It makes some comparisons a little more difficult than seems strictly necessary. 3. I am both simultaneously happy and somewhat concerned about “In interpreting these results, it is important to consider that our final, statistically-significant models were not pre-specified. Rather, they were re-specified in response to reviewer comments over various revisions of this manuscript.” Its great to acknowledge this, but it would nice to know what the original pre-specified approach was and why it was changed. 4. My understanding of how the sample flows into the analysis is that there are many cohorts from NLMS included (new cohort each year). If after adjustment for inflation, the eligibility for federal EITC is changing over time, and thus by cohort, couldn’t a 100 dollar increase (in 2015 dollars) mean something different to different cohorts? For example, if early cohorts had more restrictive inclusion criteria (say you had to be poorer than later cohorts), then the $100 may be a different proportional increase in their income compared to

	later cohorts even if the $100 is itself inflation adjusted. Doesn't this have implications for the proportional hazards assumption made by the specified Cox regressions? It would be helpful if explicit PH assumption tests were presented. 5. The overall identification strategy seems to hinge on whether those who are eligible for EITC are exchangeable with those who are not. This is somewhat strengthened by limiting the control group to those whose income is twice the maximum allowable income for federal EITC, but that still allows quite a bit of potential variation. The authors do additionally control for income in the models, but I'm not sure if it is sufficient. As one example, income is only measured at entry into the study, and income growth is likely not homogenous across "baseline" income. I'd request a sensitivity analysis further reducing the level of allowable income in the control group to see how results change with a "tighter" control group. 6. Likely just needed clarification, but where these restriction on income eligibility among the controls conditional on their martial status/# of children, etc? Or was the maximum federal eligibility criteria (married, 3 or more children) used for everyone? If the latter, that's problematic. 7. How was variation in refundability for state level EITC accounted for? It would seem inappropriate to count a $200 refundable credit the same as a $200 non-refundable credit. 8. Confidence intervals in addition to p-values would provide easier to interpret results.

REVIEWER	Daniel Kim Northeastern University, USA
REVIEW RETURNED	20-May-2020

GENERAL COMMENTS	I commend the authors for tackling an important research question using a rich dataset, and I enjoyed reading this paper. The findings are very interesting. I have a few queries about the study and these findings, as detailed to the authors below: 1. It is slightly difficult for me to get a good handle on the impact of a $100 EITC increase, since the EITC amount varies by family size, etc. Can the authors given an estimated average effect (across all recipients) according to family size? Is there any evidence that the estimated effect varies by family size? 2. Did the authors explore any possible non-linear effects between the state EITC and mortality? For example, it is mentioned that income was log transformed. Was or could the state EITC also be considered for log transformation? 3. It was not entirely clear to me whether the surveys included all surveys from 1986 to 2011. How might the causal effect on mortality up to 2011 be affected by this, such as if the survey was taken as late as 2010? In addition, for those who took the survey as early as 1986, how potentially problematic is it in terms of possible misclassification if the respondents moved subsequently?

	4. In the second paragraph of the Discussion, the authors mention several study limitations including the possibility of state-level confounding by other social welfare programs. How much of an issue could such bias pose, and did/can the authors control for any such potential confounders? 5. I found the Results section and Discussion section relatively short compared to the rest of the paper, and would suggest adding some text. For example, I think the Results could be expanded slightly by reporting any evidence of modification of effects by family size. Likewise, I think the Discussion section could also be lengthened such as by expanding on the plausibility of effects (including based on subsequent moves to a different state and based on the length of follow-up considering chronic diseases as a causes of death, etc.) and briefly discussing how these effects compared to those from related studies. 6. There is a positive association reported for the Federal EITC with mortality, which is significant for the maximum follow-up analysis. Can the authors at least mention this finding and offer any kind of plausible explanation for this (presumably unexpected) finding?

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Melvin Livingston
Institution and Country: Emory University, USA
Please state any competing interests or state 'None declared': None Declared

The presented paper is an important contribution to the evidence base for the health effects of EITC (and income supplementation programs more broadly). A particular strength of the paper is the ability to identify probable EITC receipt at the individual level. The following comments are meant to help clarify the results and identification strategy.

1. "The hypothesis that EITC might reduce premature mortality is supported by previous research." While this may be true, the cited articles do not consistently say this. Larrimore 2011 seems to find no short term effects on EITC on health; Schmeiser 2009 finds increase in obesity risk with increased EITC, etc. From the cited articles the supporting evidence seems mixed.

Response. We agree, and have amended the text to reflect the range of findings in the literature. Thank you for catching this. Please see the highlighted copy for relevant changes. We have also updated the references.

2. Is there a reason dollar amounts are expressed in 2015 dollars and not 2011 dollars (the end of follow-up)? It makes some comparisons a little more difficult than seems strictly necessary.

Response. Yes, there are two reasons for this. The primary reason is that 2015 was the year that the variable was created and standardized throughout the dataset after correcting for inflation. The second reason is that 2011 dollars are increasingly difficult to relate to. One dollar then is worth $1.16 today. On the other hand, the difference between 2015 and 2020 is only 8 cents. By using inflation-adjusted (constant) 2015 dollars, we remove inflation effects while also presenting a value for which the reader can relate. We have added an explanation. Please see the highlighted copy, intro to methods section.

3. I am both simultaneously happy and somewhat concerned about “In interpreting these results, it is important to consider that our final, statistically-significant models were not pre-specified. Rather, they were re-specified in response to reviewer comments over various revisions of this manuscript.” Its great to acknowledge this, but it would nice to know what the original pre-specified approach was and why it was changed.

Response. Thank you for calling our attention to this. We now mention the changes made from the original specification to the opening of the results section.

4. My understanding of how the sample flows into the analysis is that there are many cohorts from NLMS included (new cohort each year). If after adjustment for inflation, the eligibility for federal EITC is changing over time, and thus by cohort, couldn’t a 100 dollar increase (in 2015 dollars) mean something different to different cohorts? For example, if early cohorts had more restrictive inclusion criteria (say you had to be poorer than later cohorts), then the $100 may be a different proportional increase in their income compared to later cohorts even if the $100 is itself inflation adjusted. Doesn't this have implications for the proportional hazards assumption made by the specified Cox regressions? It would be helpful if explicit PH assumption tests were presented.

Response. To correct for differences in cohort year, we adjusted for inflation (to constant $2015 dollars) and we use fixed effects for the year of the CPS interview. We now explain this in the highlighted changes in both the methods and first paragraph of the results sections of the paper.

5. The overall identification strategy seems to hinge on whether those who are eligible for EITC are exchangeable with those who are not. This is somewhat strengthened by limiting the control group to those whose income is twice the maximum allowable income for federal EITC, but that still allows quite a bit of potential variation. The authors do additionally control for income in the models, but I’m not sure if it is sufficient. As one example, income is only measured at entry into the study, and income growth is likely not homogenous across “baseline” income. I’d request a sensitivity analysis further reducing the level of allowable income in the control group to see how results change with a “tighter” control group.

Response. Thank you for noting this. We deploy a difference-in-difference model. Therefore, the identification of those who are eligible is achieved through variations of the state EITC policies. Many states offer no state EITC benefits at all, while those that do vary widely in generosity. It is this variation that we are using to identify the “state EITC” coefficient.

The inclusion of people with 1.0 to 2.0 times the Federal EITC threshold for their family size was to help identify the coefficient of the “Federal EITC” variable by including some zero values along with the range of benefit receipts. However, the Federal EITC was not the focus of this paper, and is included as a control. We have made changes throughout to clarify this for the reader. We have also expanded the results and conclusions to address many of these points.

6. Likely just needed clarification, but where these restriction on income eligibility among the controls conditional on their martial status/# of children, etc? Or was the maximum federal eligibility criteria (married, 3 or more children) used for everyone? If the latter, that's problematic.

Response. Yes, they were conditional. We calculated the state EITC benefits received using income, marriage, and number of children. The maximum income for inclusion in the regression sample also controlled for these variables and the Federal EITC income thresholds for various family situations. We now note this. Please see highlighted changes in the methods section of the attached paper.

7. How was variation in refundability for state level EITC accounted for? It would seem inappropriate to count a $200 refundable credit the same as a $200 non-refundable credit.

Response. Thank you for noting this. This is another point that we did not mention, but now do. Please see the highlighted changes under the Strengths and limitations section of the Discussion.

8. Confidence intervals in addition to p-values would provide easier to interpret results.

Response. Thank you for this important suggestion. We have added confidence intervals throughout.

Thank you also for catching the many important details we overlooked in writing up our paper.

C. Reviewer: 2
Reviewer Name: Daniel Kim
Institution and Country: Northeastern University, USA
Please state any competing interests or state 'None declared': None declared.

I commend the authors for tackling an important research question using a rich dataset, and I enjoyed reading this paper. The findings are very interesting. I have a few queries about the study and these findings, as detailed to the authors below:

1. It is slightly difficult for me to get a good handle on the impact of a $100 EITC increase, since the EITC amount varies by family size, etc. Can the authors given an estimated average effect (across all recipients) according to family size? Is there any evidence that the estimated effect varies by family size?

Response. Thank you. Adding dose-response effects by family size would produce very useful information. As you know, the benefits increase as family size increases both with respect to state and federal generosity. We originally did not break down the analysis by family size because we did not have the statistical power to do so. The standard errors for the "state EITC" coefficient rise above 0.025 in several of the models, which creates unhelpfully broad confidence intervals.

We present the analysis here in response to your comment. We unfortunately cannot include this in the paper as a sensitivity analysis due to the extraordinarily long clearance times required by the Bureau of the Census (up to one year). Please understand that we began this paper in 2015, and every time a reviewer asks for new analyses can cause significant delays to the next draft for review. However, we can present an overview of the results without detail and with the understanding that they will not be mentioned elsewhere. For families with no children, the HR is close to 1.0 ($p > 0.45$ for all follow-up periods). As family size increases, the HR becomes smaller, but remains ns for family size of 1. It becomes statistically significant at a family size of 2 (HRs are at or below 0.95, $p < 0.05$). The small sample size for family size 3 or more is underpowered and not significant.

The logic behind showing the change in mortality by $100 increments for all recipients is that it helps guide policymakers on the potential health benefit per unit increase in cash assistance. We now include these explanations (please see highlighted changes, particularly in the opening paragraph of the Results).

2. Did the authors explore any possible non-linear effects between the state EITC and mortality? For example, it is mentioned that income was log transformed. Was or could the state EITC also be considered for log transformation?

Response. We considered that possibility, but decided to use the linear measure per $100 of credit per the advice of an earlier reviewer. This decision was made primarily because the amounts received have a much smaller dollar range than personal incomes, and because EITC receipts are designed to have a non-skewed distribution. For each family size increment, EITC benefits increase as income increases until a maximum level of income is garnered, and then the benefits decrease as income increases beyond that maximum. Thus, there is little skewness in the distribution of this variable, and the maximum amounts of benefits are received are near the middle of the income distribution in our sample.

3. It was not entirely clear to me whether the surveys included all surveys from 1986 to 2011. How might the causal effect on mortality up to 2011 be affected by this, such as if the survey was taken as late as 2010? In addition, for those who took the survey as early as 1986, how potentially problematic is it in terms of possible misclassification if the respondents moved subsequently?

Yes, we included CPS samples from all years during the 1986-2011 period. We now mention this explicitly in the text (Methods section, data subheading, second paragraph). The NLMS includes all years of Census data and is linked to the NDI. Because the NDI is a national survey, we can capture a death even if participants moved from the state in which the survey occurred.

We explore only state EITC receipts for the year of survey interview. If there are two people interviewed in 1986, one of whom moved out-of-state in 1987 and one who stayed in the state in 1987, their “state EITC” records look the same. The interstate mobility of families makes the test more conservative; mobility from a state with or without EITC to another state weakens the signal in the data. Our choice to show three different follow-up periods is in part a recognition of that additional noise. Note that the hazard ratio has its highest deviation from 1.00 for the shorter follow-up, which has less “shuffling” of people between states. We now mention this in the expanded Strengths and limitations section of the Discussion.

4. In the second paragraph of the Discussion, the authors mention several study limitations including the possibility of state-level confounding by other social welfare programs. How much of an issue could such bias pose, and did/can the authors control for any such potential confounders?

Response: This is a common statistical difficulty when looking at the effects of social safety net policies. Previous reviewers recommended using state-level fixed effects that would pick up at least some of the covariation caused by state legislatures (Methods, Model specification sub header). We have also greatly expanded the discussion of the limitations of the study (Discussion).

One concern might be, for instance, that a state permitted fracking and this greatly increased state revenues. As a result, legislators might have decided that they can now afford EITC. However, one would expect employment to change in such a scenario (along with income). The inclusion of employment, age, income, etc. should reduce potential confounding by state-level changes in wealth that could also translate into the state seeing EITC as an affordable policy.

5. I found the Results section and Discussion section relatively short compared to the rest of the paper, and would suggest adding some text. For example, I think the Results could be expanded slightly by reporting any evidence of modification of effects by family size. Likewise, I think the Discussion section could also be lengthened such as by expanding on the plausibility of effects (including based on subsequent moves to a different state and based on the length of follow-up considering chronic diseases as a causes of death, etc.) and briefly discussing how these effects compared to those from related studies.

Response. Thank you. We have done so. Please see the extensive changes to both the results and discussion section. As mentioned in the response to your first comment, we do not have adequate statistical power to show results by family size, even when children were grouped. We do not have permission from NLMS to show the trend until the new analysis is cleared by the Bureau of the Census. Doing so could delay our submission by up to a year. We do expand on the plausibility of effects and length of follow up and some of the other issues you raise (Strengths and limitations sub header). Thank you for these suggestions.

6. There is a positive association reported for the Federal EITC with mortality, which is significant for the maximum follow-up analysis. Can the authors at least mention this finding and offer any kind of plausible explanation for this (presumably unexpected) finding?

Response. Thank you. We have done so. Please see final paragraph of the Results, and the 3rd paragraph of the Discussion.

VERSION 2 – REVIEW

REVIEWER	Daniel Kim Northeastern University, United States
REVIEW RETURNED	03-Jul-2020

GENERAL COMMENTS	I thank the authors for responding thoroughly to each of my previous comments.