Referee Report: JHLTHEC-D-22-01070, *Journal of Health Economics*

“Mothers as Insurance: Family Spillovers in WIC” (Bitler et al.)

This paper uses a clean regression discontinuity (RD) design to investigate the spillover impact of a young child’s losing access to WIC on nutrition and health outcomes for the rest of the family. They examine the impact of a child aging out of assistance at age 5 and show that such a loss impacts not the affected children, but most notably household adult women (e.g., mothers) who reduce their caloric intake in response.

The findings of the paper are very policy-relevant, and I am impressed by the clean design and the focus on spillover effects in a home. I think the key contribution of this paper, relative to other papers on losing access to WIC, is the detailed data that allows for an in-depth exploration of spillover effects in a home across a variety of outcomes (particularly when assessing “upward spillovers” to mothers). I am also impressed by the way that the authors have thought carefully about substitution effects through, for example, access to school lunches. I am optimistic about this paper and only have a few suggestions for improvement.

**Major Comments**

1. **Regression Discontinuity Robustness.** The main result that the authors sell (which I also view as the most interesting contribution of this paper) is that adult women reduce their caloric intake after the loss of WIC. I have some concerns about this particular RD, as well as the spillover effects on food insecurity.
   1. First, the point estimate here is 768 calories/day, roughly 40% of the average caloric intake in this group (Table 2, column 4). This would suggest that the women in these homes are skipping, on average, just over a meal a day following the loss in WIC. Is this a realistic estimate?
   2. To that end, Figure 2 panel 3 (the picture of the RD) raises concerns about the identification of this point estimate. There appears to be an outlier on the left of the discontinuity in food consumption just shy of the cutoff (the bin between 0 and 60 days before the 5th birthday). Given that the RD is a local linear regression on either side of the cutoff, the point estimate of the RD could be heavily influenced by an outlier here, potentially artificially inflating the true effect.
   3. Do the authors think that this outlier has economic content? E.g., is there a case that women are preemptively consuming more calories/day in the month leading up to the loss of WIC (either through anticipation effects, or something as innocuous as celebrating a child’s fifth birthday)? If the outlier doesn’t have economic content, I would like to see a “donut RD” dropping a few bins just around the cutoff. The concern in doing so, obviously, is that we are dropping children/families with the most weight (and the most internal validity), but I think in this case, there is reason to be concerned that the point estimates may not be robust to this outlier. Even a small donut hole (1-3 bins) should be enough to rule out this concern.
   4. I have a similar concern about the result for food insecurity (Figure 2, panel 1). The point estimates are still larger than I would expect here (Table 2, columns 1 and 2), but perhaps are less extreme than the calories finding. But there is also an outlier just to the right of the cutoff—this could just be an example of a (very?) short-term effect before households recalibrate following the loss of WIC benefits, but could also be artificially inflating the RD effect. Could the authors comment somewhat on the timing/duration of the effect here?
   5. Can the authors add the spillover caloric consumption outcome (Figure 2, panel 3) to the robustness check for Table A.6 in the appendix?
2. **Introduction**. I think that the introduction could benefit from a greater discussion of the interpretation/magnitude of the results found. The second paragraph on page 3 only mentions that the results are “fewer calories” consumed by adult household women. If the authors’ point estimate is true, then this effect is huge and should be highlighted more in the introduction! If, on the other hand, this point estimate is not very robust (see comment 1), I would like more discussion of that up front as well.
   1. In addition, the authors may benefit from fitting their work into a larger literature on health spillovers within families, rather than simply the work on the relevance of WIC for children. This is a paper with a clean identification of how relationships form health behaviors, including nutrition, and thus makes an important contribution about how policy shapes health behaviors within a social network, including a nuclear family. I’m thinking about papers like Fadlon and Nielsen’s work on how families respond to acute health events (2019, 2021; see also Dalton et al., 2020), and Ag¨uero and Beleche’s (2017) work on how community health events such as infectious disease outbreaks affect household decisions.
3. **Role of measurement/classification error.** If I understand the paper correctly, the authors use an external data set to show the “first stage” of the RD but do not actually have data on WIC participation among their analytical sample. Hence, the RD estimates conflate children who turn 5 and lose coverage with children who turn 5 but were not covered at the time. The authors get around this somewhat through their choice of analytical sample. Is there reason to believe that there is still a nontrivial percentage of near 5-year-olds who aren’t on WIC due to any barriers to access at the household level? How would this affect the results?
4. **Heterogeneous treatment effects.** What is the frequency of households with more than one child covered by WIC (e.g., more than one under 5 years old)? Do results differ from households with added social protections versus households who lose all access to WIC? This can be a quick analysis, unless the authors are interested in discussing this finding more in the paper.

**Minor Comments**

1. **Medicaid.** The authors don’t emphasize the Medicaid outcomes, but I am confused whether they should be part of the set of outcomes. Individuals who are enrolled in Medicaid are automatically considered income-eligible for WIC, so Medicaid enrollment may be a strong mediator in WIC participation (particularly in the case where not all 4.9 year olds in the analytic sample are enrolled in WIC). Even if losing access to WIC doesn’t affect underlying Medicaid eligibility, the potential endogeneity of enrollment here strikes me as concerning. Does it make sense to keep Medicaid enrollment as an outcome here?
2. **Clarifying some points.** 
   1. I would like some clarification that the first-stage of the RD (showing the drop in WIC participation) doesn’t come from the analytical sample but an external data source. I had to read the section twice to check that, might be helpful to clarify for readers.
   2. I thought the comment on page 4, paragraph 2 about “only 1% of spillover papers ask how programs for children impact parents” seemed like an overstatement, until I read the cited paper. Very interesting! The authors may still consider that leaning on the 1% statistic in that sentence may lead readers (who may not look at the cite) to draw different conclusions. Not suggesting any changes here, just a thought.