Reviewer’s report

Title: The home environment and childhood obesity in low-income households: Indirect effects via sleep duration and screen time

Version: 2 Date: 17 September 2014

Reviewer: Benjamin Gibbs

Reviewer’s report:

Major

The study has merit but I am left with a couple puzzling aspects of the data and findings. The study is in Chicago of mostly black residents, focused on the mechanisms of obesity among this group. This is indeed a sensitive topic, but it is avoided altogether, not to mention the uniqueness of this problem in such a place as Chicago. The fact that so many other reasonable factors that were measured in this study prove to not hold statistical significance either implicates the small sample size or the generalizability of the data. Thus, strong conclusions are hard to make in this study beyond mostly black, urban residents of Chicago. There is room for this kind of work, of course, but to not address this issue in the manuscript suggests the authors are perhaps unconcerned about these limitations for researchers seeking to understand obesity in other locales, with other populations.

The biggest result is that obese kids sleep less. Yet in the descriptive statistics it appears that normal weight kids have more screen time, am I reading this right? This seems strange to me.

Given that causality cannot be established, I think it is valid to ask, how exactly does screentime influence obesity when other factors (like diet and activity) are accounted for? The punchline of the paper is just not clear to me.

Minor Issues

Cutoffs for obesity rates should be justified.

Is the data nested in homes? (More than one case collected out of homes?). If so, how many households are in the sample? How does the nested nature of the data compromise assumptions of the analysis.

There appears to be missing data on wear time. What happened with these cases? Listwise deletion? I suggest either multiple imputation to recover these cases and see if results change or at least create a dummy variable of the missing cases and see if it is predictive of other measures in the model (in other words, are these missing cases patterned?)

Also, I am not quite sure the authors gain much by only looking at low-income. Surely there are different forms of disadvantage that extend beyond income. I need more logic and evidence that a target population suffers less from bias than
studies of the whole.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests