Reviewer's report

Title: Preschool children's preferences for sedentary activity relates to parent's restrictive rules around active outdoor play

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Reviewer: Kristen Copeland

Reviewer's report:

The manuscript is a cross-sectional study attempting to assess the effect of parenting practices on children's physical activity. As it is a cross-sectional study, causation cannot be determined. Only correlations can be determined from a cross-sectional study. There are several statements in the manuscript and abstract that imply causality that need to be softened. For the current topic, it is likely that parenting practices are influenced by child temperament and behavior, and as such the relationship between child preferences for PA/actual PA and parenting practices is bidirectional. This bidirectional relationship is, in fact, noted in the title of one of the quantitative studies the authors list (reference 7). The fact that reference 7 found a bidirectional relationship between parenting practices and childhood habits in a longitudinal study, a more robust design than a cross-sectional study, makes me question what this study is adding to the literature.

Furthermore, the authors lack an objective measure of actual child PA. Parents are notoriously bad reporters of their children's PA levels. This would particularly be expected to be true for children who are in child care, and thus outside their parents’ view. For this study the authors rely on parent report of child PA and preference for PA from the same survey with which they are using to assess parenting practices (Vaughn et al 2013). Thus parents are reporting both the exposure and outcome variables. If anything, this suggest internal consistency of parent report, which is something the authors of the original survey noted in their 2013 paper. (Validation was assessed by exploratory factor analysis, internal consistency of items in parent survey, and association with parent-reported PA and accelerometry) In the original 2013 paper Vaughn and colleagues used accelerometry to objectively measure children's PA, nd found some associations between child PA and parenting practices. But the best correlation between parenting practices and child PA was in the parent report of child PA. This is a quote from the original paper : "Overall, the associations between parenting practices and accelerometer measured physical activity were less consistent than with parent reported outside and TV time." Again, it is not clear what this paper is adding to the literature as it is reporting internal consistency of what parents report for parenting practices for PA and children's actual PA on the same survey.

The addition of the authors' iPAD tool to assess children's preferences for PA is new- and correlation between what parents report for parenting practices and what the child reports for preferences related to PA is interesting. There is only one bivariate association that is significant - rules around outdoor pay and child PA preference—out of 16 bivariate associations examined in Table 3. What does this finding mean? It would seem to make sense that children who like to play outside would have parents that have rules about them playing outside. But the question is, what is the next step/intervention? If the relationship between parenting practices and childhood
preferences is bidirectional, where to intervene- on the child or the parent? And if a case can be made that parenting practices influence childhood preferences (which cannot be made from a cross-sectional study), how would the authors propose to change parenting practices on a broad scale? Or even an individual scale? Is there a feasible or effective intervention in which parents are convinced to start allowing their children to play outside when it is cold or when parents think it is unsafe?

The associations the authors report between child PA and demographics (both parent and child) have been reported previously. It is not clear what the study adds to the literature. The findings do not warrant the amount of discussion they currently have in the results and discussion sections.

A few other comments by section:

The introduction and conclusion are selective in citations. There are several citations listed when only one may be needed, and only a single citation when it would be appropriate to list several others. The authors cite several review papers, but not the key sentinel papers for the concepts they are describing. There are papers in the literature that discuss societal values and parents' view of safety of neighborhoods as influences in their preschool-aged children's physical activity that are not cited. The conclusion strays off into citing dopamine mechanisms for enjoyment of screen time, which is not relevant to the study.

More detail about the questionnaires used is needed in the methods section- specifically what are the relevant questions. It was not clear to me that "rules about outdoor play" was negative until late in the discussion section when the question stems were listed.

How the key instruments in the study were validated and the relevant measures (e.g., Cronbach alpha) should be cited in the methods section. For the Vauhgn paper, the instrument was assessed for internal consistency, but it's not clear how it was assessed for construct validity. Same for the IPAD tool of the authors.

The authors used a convenience sample of child care centers - what was the response rate from parents within centers? What percent of Australian children are in child care? Without this information, it is not clear to whom the results might be generalizable. Why did the authors have difficulty assessing the population as stated in the discussion section?

Do the screen-time habits assessed by the 2013 survey include tablets and phones?

The bivariate association between ethnicity and parent reported outdoor time seems likely related to neighborhood safety.

It is not clear what the measure of physical activity preferences is for Table 4 and the final multivariate analysis. Is this based on parent report or the child Ipad activity? If it is parent report, it does not seem different from the original paper by Vaughn and colleagues to validate their instrument, except that that paper used an objective measure of physical activity.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Unable to assess

Are the conclusions drawn adequately supported by the data shown?
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No

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