Reviewer’s report

Title: Descriptive analysis of preschool physical activity and sedentary behaviors - a cross sectional study of 3-year-olds nested in the SKOT cohort

Version: 0 Date: 20 Mar 2017

Reviewer: Jill Hnatiuk

Reviewer's report:

Thank you for the opportunity to review the manuscript entitled, "Descriptive analysis of preschool physical activity and sedentary behaviours - a cross sectional study of 3-year olds nested in the SKOT cohort". This paper provides evidence on young children's physical activity and sedentary behaviour levels and patterns with comparison across different settings and times of day. I think it is a nice contribution to the literature, though I do offer some suggestions below for the authors to address prior to publication.

Introduction:

Page 4, line 76 - the authors mention that the physical activity recommendations are based on reducing the risk of overweight and obesity in children. However, this is only one health outcome on which the recommendations are based. Physical activity and sedentary behaviour recommendations take into consideration evidence on many other physical (e.g., skeletal, cardiovascular health) and psychosocial (e.g., cognitive, psychological) health outcomes. I recommend the authors modify this section to more accurately reflect this.

Methods:

Page 8, line 167 - 177 - Can the authors please comment on whether the procedures for excluding day sleep time have been validated? If so, can you please add this information to the manuscript, as it would be useful for other researchers to use a similar process to remove nap times from their dataset in future. At present, most work just excludes consecutive zero counts which likely doesn't appropriately address children's napping behaviours (so thus this contribution is needed).
Results:

Page 10, line 231 - Can the authors please clarify what is meant by 'long academic education', 'short academic education', etc. Is this post-secondary - i.e., university level or secondary school? Perhaps there is an opportunity to revise this statement so that it is easily interpreted by an international audience.

Discussion:

Page 17, line 376 - The authors suggest that they found high agreement between wake up times based on the IMI and parental questionnaire data. I would suggest adding this information to the results section and also reporting for child's bed time if you have it. It will further justify your decision to use the IMI method, particularly if this method hasn't been validated.

I think the discussion section could be strengthened by discussing the implications of this work, rather than just comparing to other studies. For example, given children were most active in DC, should we be focusing on that setting for promoting PA in children? Or should we focus on the home setting since PA levels were lower? What is it about the day care setting that facilitates PA? Better trained staff, other children around? I understand it's not possible to conclusively tell from this study, but drawing on other research in this domain to explain the findings would strengthen the paper.

On a similar note, I found it particularly interesting that the most and least active children were more homogenous in their behaviours across settings, while those in the middle tended to vary to a greater degree. Can the authors comment on how we can use this information to develop programs to optimise physical activity for all children?

Can the authors also discuss the implications that may result from such a small number of the least active children meeting PA recs (e.g., 7% of girls)? Although in this sample it would only be approximately 50 children, if you extrapolate to the population, it is thousands of children not meeting recommendations at a very early age, which of course may have substantial implications for health service use in future years if this pattern of PA is predictive of later activity levels.

Page 15, line 350 - Although the authors provide a justification for not controlling for the individual day care centre that the child attended, it is a factor that has been previously shown to influence PA levels of children. I am wondering if the results would be the same if they did so. Perhaps looking at how similar children at each centre were in their behaviour as an initial step would help to determine whether or not it is relevant to control for this variable.
Minor comment:

Page 5, line 106- I believe this sentence should be greater than 37 weeks gestation (not less than)?

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

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

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

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