Dear Matilda van den Bosch and Cecil Konijnendijk

Re. Manuscript UFUG\_2018\_555\_R1: “The effects of meteorological conditions and daylight on nature-based recreational physical activity in England.”

Thank you for the opportunity to resubmit a second revised version of our manuscript for consideration for publication in Urban Forestry and Urban Greening. We are extremely grateful to yourselves and the reviewers for their further comments on this revised submission. We have revised the manuscript accordingly and our responses are provided below in blue typeface.

We hope you agree that our revisions adequately address these remaining points and that the quality of the manuscript has once again been improved. We look forward to hearing from you in due course pending consideration of this revised manuscript.

Yours sincerely,

The authors

**Reviewer 1:**

Concerns have been largely addressed, however a few remain.

We are pleased we have addressed the majority of the reviewer’s concerns and have attempted to address these few remaining issues below.

What is the effect size of meteorological conditions? Barely allude to age, further afield, SES being larger effect sizes. That R2 is so low, only .01 in unadjusted model, suggest climate has very little meaningful effect.

The reviewer is correct in stating that little variance is explained by the minimally-adjusted models. While not formally specifying effect size estimates (e.g. partial eta-squared) for the variables included in models (indeed these are not possible for smoothed terms), we do state in section 3.3 that:

“Sex and visits “further afield” were generally the strongest and most consistent predictors across these stratified models”

And in the limitations section (4.3) that

“The models did not explain much variance in MET-minutes. However, models with log-transformed MET-minutes explained up to twice the variance of untransformed models (Tables S5 and S6) and key relationships between meteorological conditions/daylight hours held”

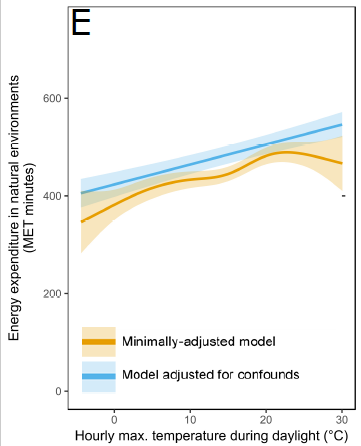
We also note that while visits further afield had a consistently large effect across environments, age and SES (to which the reviewer refers) did not.

Nonetheless, we have now more clearly articulated the reviewer’s concern regarding the meaningfulness of the influence of meteorological conditions in the limitations section and hope this addresses the concern that we did not pay this enough attention in the previous version of the manuscript:

“For example in the minimally-adjusted model, meteorological conditions and daylight hours only explained 1% of the variance in energy expenditure. Therefore, while statistically significant, these only appear to play a small role in determining how much energy an individual might expend at a particular natural environment, with other factors such as sex, or whether the visit was further afield, playing a larger role. Future prospective work could investigate how changes in weather or daylight hours might affect energy expenditure within the same individuals over multiple visits, and thus better illuminate the impact of these on energy expenditure where this cross-sectional work could not. In spite of this, we note that models with log-transformed MET-minutes explained up to twice the variance of untransformed models (Tables S5 and S6) and key relationships between meteorological conditions/daylight hours held.”

I do not see Figure 3 in the resubmission so I cannot speak to whether this is now more clear.

We apologise for this. Figure 3 was in the PDF document we viewed before confirming submission of the last revised manuscript. Hopefully the figure is viewable in this newly revised version. Essentially, we placed a legend at the bottom of the graphs denoting that the orange line referred to a minimally-adjusted model, and the blue line to a model adjusted for confounders. For example:



Check VIF scores. That VIF is 4.72 could be considered not appropriate, recommend running sensitivity analyses with work status and age group separated, if the authors want to include both in the main model of the study.

The reviewer rightly has some concerns about the inflated variance of work status/age and the influence this could have on our results. The addition of eight further regression models (i.e. with and without age or work status included for each of the four stratifies models) may detract from the narrative of the paper and is perhaps outside of its scope.

Instead of this, and in retrospect, instead of detailing the generalised variance inflation factors (GVIF) we referred to in the caption of supplementary table S6, we perhaps should have drawn the reader’s attention to the GVIF1/(2\*df) which additionally adjusts for degrees of freedom of the term (i.e. it calculates a variance inflation factor that accounts for the fact that some categorical predictors have more levels than others).

Using this calculation, the reader can see that the apparent collinearity between work status and age is mostly accounted for by the fact that the work status regression term in particular has a larger number of degrees of freedom associated with it (4) compared to other variables in the models.

To make this more transparent to the reader, we have added a further supplementary table (S7) that details these adjusted generalised variance inflation factors for each regression term in each of the four stratified models.

We hope the addition of this new table satisfies the reviewer’s concern that the apparent multicollinearity is not an issue to be too concerned with.

Fox & Monette (1992) doesn't indicate what VIF values are appropriate, recommend other source

The reviewer is correct that the Fox and Monette reference did not support our point in Table S6 that GVIF’s can be inflated in categorical variables with more than three levels (they do not state arbitrary rules of thumb as the reviewer points out). However, this caption has now been removed in favour of the added supplementary table of GVIFs adjusted for degrees of freedom that we described in our response to the reviewer’s last comment. This adjusted GVIF was first introduced in the Fox and Monette article previously cited (Table 2 on page 182 of their original article), so we have maintained reference to this in the caption of the new Table S7.

**Reviewer 2:**

The authors have addressed my comments and questions very well and the manuscript has improved substantially.

We thank the reviewer for their positive appraisal of our revised manuscript.