Author’s response to reviews

Title: Feasibility of an incentive scheme to promote active travel to school: a pilot cluster randomised trial

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Response letter

Dear Editor of the BMC Pilot and Feasibility Studies Journal

Thank you for your comments and for the chance to resubmit our manuscript to your journal. Please find below our response to each of the points raised, and attached the manuscript in which we have made changes as suggested. Both our responses below as well as the changes in the manuscript are in green.

We look forward to hearing from you

Many thanks

Kind regards

Samuel
The authors have done well to address the reviewers’ comments as raised previously. However, I think there are still some points that need to be addressed before the manuscript can be published in PAFS. Notably, the authors say in their response to reviewer 2 that they do not look at behaviour change, but surely in order to progress for a definitive trial we need to have some idea that the intervention is producing a change in the target outcome in the right direction, and what the magnitude of that change is (and also to inform the decision of what the outcome measures should be for a definitive trial). Therefore, in response to the authors’ response to reviewer 4 – I think they should report physical activity outcome data in this paper.

Our response: We have now added data on the ‘potential effectiveness’ of the intervention as requested, in lines 243-259 (methods: data analysis), lines 413-428 (results), lines 523-533 (discussion), and lines 608-609 (acknowledgements). However, we advise caution when interpreting these results as the study was not powered for an effectiveness analysis. To explain the amount of missing data in the regression analysis developed for the effect size estimations, we have also provided additional information on the return of valid ATS reports, on lines 333-344 and highlighted text on table 2 (lines 350-351 - previously we had just included the rates of return, irrespective of whether reports were completed or not).

Other (many minor) points for consideration are listed below (some of which were raised previously).

Abstract: ln 39 – it is not clear what the draw sessions consisted of … please could there be more description of the intervention?

Our response: Please see highlighted text in lines 35-39. In addition to information that was already in the abstract, we have added that “Before each draw session, the researcher prepared the tickets and placed them into an opaque bag, from which one was randomly picked by the teacher at the draw session”.

Ln 41 is it objectively measured MPVA?

Our response: Yes, please see highlighted text in lines 44-45. We have clarified that it is “accelerometer-assessed MVPA”.

Ln 46 – more clarity around the time frames is needed.

Our response: We have clarified in lines 45-46 that the first timeframes is “during the journey to school based on the times reported by the parent”.

Ln 47-48 is the final sentence of the results needed?
Our response: Thank you for your suggestion. The sentence has been removed.

Ln 50 school recruitment does not emerge as a problem in the abstract results.

Our response: We say in line 40 of the abstract that 4 out 123 schools (3.3%) contacted replied positively. We regarded this as a problem since many more schools would be needed in an evaluation trial.

Key words: Is active travel to school a key word? Should it just be active travel?

Our response: Thank you, we have amended as suggested.

Background: ln 65-67 Is the citation in wrong place?

Our response: We are not sure which citation this refers to. Both the Chillon et al and the Villa-Gonzalez citations seem to be in the right place for us.

Ln 68 less costly than what? It is not clear

Our response: Please see line 70. We have replaced this by ‘relatively inexpensive and easy-to-deliver’; this sentence comes after another in which we pointed out that some interventions require considerable time and resources from schools.

Method: ln 177 is ‘was to be’ necessary?

Our response: please see lines 178-180. We have reworded this sentence to clarify that the questionnaire was taken home by the child, and also returned by the child.

What about the option for parents to report by SMS in non-accelerometery weeks?

Our response: Please see lines 185-187. We have added that “Children whose parent was a SMS-respondent only returned the parental ATS report in the two accelerometer weeks; in the remaining non-accelerometer weeks, those children did not return parental ATS reports.”
Ln 181-182 the difference between accelerometry weeks and non accelerometry weeks needs to be made clearer, was it just the voucher in the accelerometry weeks?

Our response: Please see changes in lines 183-187; the thank you voucher was one difference (as explained in line 190). Other differences have been explained in lines 138-146: the fact that all children were assessed at once at baseline, and that different subsamples were assessed in each post-baseline week; everybody had to complete the parental paper ATS report which included questions about times of the journey to school (and pauses therein).

Ln 202 – is it necessary to reference the completed TIDieR checklist – all elements of TIDier should be in the description of the intervention in the manuscript – is this currently the case? There may be some missing.

Our response: Please see lines 193-219. We have now added all the details about the intervention from the TiDieR checklist to the manuscript.

Ln 209-10 bar charts are a way of presenting data not a form of analysis. Please remove this sentence.

Our response: This sentence has been removed.

Results: Ln221-222 Should it read ’19.7% of pupils eligible for free meals?

Our response: Thank you, we have now removed the word ‘were’ (line 266). In lines 264-268, for each instance where we report a median we have now added an LQ and UQ, to clarify that the parenthesis that follows the median refers to the inter-quartile range.

Ln229 – I still don’t think the authors should use unexpectedly unless you explain its use in the manuscript.

Our response: We have removed this.

Ln233-234 what is the percentage of children that were retained for the 9 weeks of the trial in the control group Ln 236 Again I am not sure we need the completed consort check list referenced as additional material?
Our response: Please see lines 277-278, we have added percentages. We thought that, in addition to the CONSORT flowchart, the CONSORT check list was required by your journal; this is discussed here https://www.biomedcentral.com/getpublished/writing-resources/reporting-guidelines Assuring that the above comment only refers to the reference to the check list in the text, we have now omitted this reference from the text, although we are still including this as an additional file as it is our understanding that the journal requires so.

Consort Figure – I still don’t think ‘not as suitable as the schools selected’ is meaningful in the Consort diagram – should there be a footnote (this diagram, like the tables should be fully understandable as stand alone items).

Our response: Please see Figure 1 (CONSORT flowchart). We have replace this with “Had higher ATS levels according to head teacher or existing data (n=2)”.

I still think the CD needs to report how many children were approached in each group?

Our response: Please see the CONSORT diagram; that information has now been added.

Why were 14 control participants included in the analysis when 2 were lost to follow up (The CONSORT diagram typically refers to the before and after analysis) Ln 246-248 Is the difference between the groups in terms of transport to school at baseline a likely confounder?

Our response: A before and after analysis, in the context of effect size estimation, was not among our initial objectives as we knew that the study was not powered to do so. As a pilot study feasibility outcomes were our focus. In this perspective, all participants, including the two dropouts, were included in the ‘main analysis’, based on the data they had provided however limited (aspects such as materials returned, or number of children recruited, were our main outcomes of interest). At the editor’s request, we now provide data on the effectiveness of the intervention (lines 413-428). However, we are still assuming that the assessment of feasibility aspects is our ‘main analysis’, and as such, we are including all 29 participants on the ‘analysis’ boxes on the CONSORT flowchart. We also included baseline levels of ATS and of MVPA data as potential confounders in the regression models that we developed to estimate effect size.

Ln 258-263 Are the differences in format between the intervention and control groups confounding factors? I.e. the presence of the teacher in one school is mentioned in the discussion, but not the timing of the sessions.

Our response: We now discuss this in lines 476-479.
Ln 275 Is it 90 days with missing data rather than 90 days missing?
Our response: Thank you, we have rephrased as suggested in line 348..

Ln 286 – it should be made clear that this is objectively measured MVPA from the accelerometer
Figures 2 and 3 – it is not clear what distribution of ATS trips means – these figures should be
given a more descriptive title (see previous comment about figures and tables being stand alone).
Our response: We have clarified that it is objectively measured MVPA (lines 353-354), and
explained what ATS trips and non-ATS trips are in lines 121-122. We now refer to weekly
percentage of ATS trips, rather than distribution of ATS trips (lines 357-358).

Ln 301 – it is not clear what the less than 1 minute wear time exclusion criterion is – is on that
day?
Our response: We have clarified in lines 370-372 that it is accelerometer recordings with less
than a 1 minute duration (in any time period) and gave the following example: “if the child wore
the device for 10 minutes consecutively and then for 20 seconds, only the 10 minute recording
was included”.

Ln 304-05 – ‘and with very similar numbers in both groups’ is misleading – it suggests that the
number of participants contributing to the findings is similar in both groups, and I don’t think
this is what the authors are trying to infer?
Our response: We have rephrased to clarify that it is ‘on average and equally in both schools’
(line 375).

Table 3 – need to specific what tests were done (i.e. what U and Z refer to) (tables need to be
stand alone)

Our response: We have now added in table 3 that it refers to the Mann-Whitney U test, and have
omitted z scores to avoid confusion.
Discussion: In 353 could the authors remind the reader of what the level of participant recruitment was. Will the recruitment procedures be reviewed in light of these findings?

Our response: We have now added the percentage of participants recruited is 33.0% (line 445). In the next sentences, we then discuss how our recruitment strategies could have contributed to this low rate, and suggest alternatives.

Ln362 this suggests the current study was with low income families — is this the first reference to level of deprivation (it should be mentioned throughout the manuscript, including in the abstract)?

Our response: We now refer to this in the abstract (line 31) and in lines 295-299 when characterising our sample.

Ln380 Why is the difference in car availability between the groups not highlighted in the results?

Our response: This is now presented in lines 295-296.

Ln390-391 – where is it reported that the majority of data used in MVPA analysis was from participants in the intervention school? It is perhaps implied implicitly, but I think it needs to be made more explicit in the results.

Our response: Details about this discrepancy are presented in lines 399-3403 in the results section.

Ln396-398 – I do not understand this sentence.

Our response: This sentence means that both parental and child reports were valid. We have removed the second part of the sentence (lines 490-491), as the issue of small MVPA difference between active and non-active trips is discussed in the next paragraph (starting in line 493).

Ln 432 – is this the first reference to IMD quintile – deprivation needs to be mentioned throughout the manuscript.

Our response: We now refer to this in the abstract and in lines 295-299 when characterising our sample.
Lm447-449 – does this sentence contradict ln 159-60 which talks about selection of schools based on likely scope for change?

Our response: We are not sure how this contradicts the fact that we selected our two schools, among other reasons, because they seemed to have greater scope for change (i.e. lower ATS rates) than the other two which were not selected, according to the head teacher and to previous data. It turned out that that two selected schools too had quite high levels of ATS, for example compared with national rates which are around 50%. However, at the point of selecting the two schools we did not know our sample, and based on data we had available at that time, our two selected schools had potentially more scope for change (i.e. lower rates of ATS as per publicly available data) than the two schools which we chose not to select.