PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Is the physical activity environment surrounding primary schools associated with students' weight status, physical activity or active transport, in regional areas of Victoria, Australia? A cross-sectional study.
AUTHORS	Jacobs, Jane; Crooks, Nic; Allender, Steven; Strugnell, Claudia; Backholer, Kathryn; Nichols, Melanie

VERSION 1 – REVIEW

REVIEWER	Carlton, Troy Catawba College, Sport & Health Sciences
REVIEW RETURNED	03-Jan-2021

GENERAL COMMENTS	1. Is the research question or study objective clearly defined? NO -I feel there needs to be a definitive research question at the end of your background section. Or at least, an expansion of your objective statement. You say the purpose "is to investigate whether the physical activity environment surrounding public schools is associated with…." That is a fine primary objective but there needs to be follow up concerning "to what degree" each of those variables influences physical activity and weight. Does this make sense? For example, if you did find association (which I can see that you did), your inquiry does not end there, rather, you need to address your next steps in discovering what particular variables are pertinent and what is their strength in its association with PA behavior and weight status. As a reader, I want to know a little more specifics about your exact aims and what direction you plan on going with regards to all of your measurements. Additionally, I would like to see a sentence or two following the research question on the impact that this research would have on. In other words, what is the overall contribution to knowledge if a study like this were carried out? Otherwise, the study objective is clear. 2. Is the abstract accurate, balanced and complete? NO -Keywords: add "school environment" and "children" -Need to abbreviate physical activity to "PA" throughout the paper as is the industry standard. 3. Is the study design appropriate to answer the research question? NO Instrumentation? Especially for the Physical Activity Environment component. Where can one retrieve this survey instrument? How many schools? Need to elaborate more on the use of Arc GIS. Only a short sentence was provided. 4. Are the methods described sufficiently to allow the study to be

	repeated? NO -I strongly suggest adding a table that clearly lays out the variables of interest (both IV and DVs), what is the level of data (e.g., ordinal, ratio, etc.) and your analysis plan. This gets a little confusing as to how all of these data are coming together. -Lines 231-249. Is there a reason why these questions are included in the manuscript? This seems completely random why there is a Q&A section at the end of your methods section. Maybe this is an appendix item? Consolidate the answers and leave out the questions, unless this was mandated by the journal publisher. 16. Does this paper require specialist statistical review? YES -The GIS software "Arc GIS" was mentioned as a data collection method for the environmental components. This is a very specialized software that I am unfamiliar with and so I recommend any scholar with experience using Arc GIS, particularly when it comes to examining street connectivity, walkability, etc. to review their methods and statistical analysis.

REVIEWER	Silverwood, Richard London School of Hygiene and Tropical Medicine, Department of Medical Statistics
REVIEW RETURNED	24-Mar-2021

GENERAL COMMENTS	This study examines the associations between the physical activity environment surrounding regional primary schools and physical activity behaviours and weight status in Victoria, Australia. I have conducted my review of the manuscript with a particular emphasis on the statistical methods and analyses used. Major comments We are told that the schools in the final analysis "included 1,794 students with complete measures". Were there therefore students in these schools who participated in the study but were excluded from the analysis on the basis of incomplete measures? The extent of such exclusions should be reported and the potential consequences of excluding these students from the analysis considered. Outcome data were collected at the student level but aggregated to the school level for analysis, leading to the analysis of only 39 data points (schools). Did the authors consider instead conducting their analysis at the student level while accounting for the clustered nature of the exposures, for example by using multilevel modelling? Justification for their chosen aggregated approach should be provided. I would have assumed that socioeconomic position (SEP) was an important confounder to consider in these analyses. The authors have access to a seemingly continuous school-level measure of SEP, which they chose to dichotomise. This approach throws away a lot of information and minimises the opportunity for confounder control. The authors should revisit their specification of this variable in the models, instead retaining it as continuous variable or using a greater number of categories (e.g. quintiles, deciles). (I acknowledge that a dichotomised version is helpful when presenting stratified descriptive statistics as in Table 1 could be retained for this

	purpose.) We are told that “geographical location (according to remoteness) is included in the ICSEA calculation, and therefore was not adjusted for separately” (line 223). Presumably the logic here is that remoteness may be colinear with SEP? This seems unlikely given the number of other factors incorporated in the ICSEA (and highly unlikely under the current dichotomous SEP variable). If the authors consider remoteness to be a potentially important confounder then I would invite them to reconsider this decision (or provide furher justification if I have misunderstood their reasoning). Three of the four outcome variables are proportions (so bounded at 0 and 1) but are analysed using linear regression. The authors should note whether the distributional assumptions underlying linear regression were met (and if not, should revisit their analysis approach as per the above comment). The three exposures are presumably correlated. The authors should consider the addition of mutually adjusted analyses to try to disentangle the associations further. The number of exposures (3 plus one composite) and outcomes (4) means that the extent of multiple testing requires further consideration. This could be handled formally through p-value adjustment and certainly requires some discussion. The authors note in the Discussion that “more recreational facilities and higher walkability scores were found in areas surrounding schools classified as lower compared to higher SEP”. I am not familiar with the relevant literature, but this struct me as counterintuitive – if that is the case then further discussion is required. The authors note in the Discussion that “Our finding of the significant association between the total physical activity environment score and the proportion of students with high weight status was in the opposite direction to our hypothesis and may reflect type II error”. Firstly, given that an association was identified is this not really a type I (false positive) error rather than a type II (false negative) error? Secondly, neither a type I nor type II error can really explain the identification of an association in the opposite direction to the (assumed) true association. Further discussion, particularly around the possibility of confounding, is required here. Minor comments Throughout: The word “region” or “regional” (e.g. “regional primary schools”, “regional areas”) is used in a way that I am unfamiliar with. Line 83 (“Compared to children living in metropolitan areas, regional and rural children…”) suggests it might mean non-metropolitan but non-rural areas? Perhaps this could be clarified at first use. Line 32: Results are reported as “beta = X” in the abstract. It is not clear what “beta” represents here. Better to report results on a more intuitive scale and/or with appropriate units. Some indication of what they are adjusted for is also required.

	It was not completely clear whether the included schools were sampled or comprised the entire population of schools in the regions studied. We are told that the region includes "142 government, Catholic and independent primary schools" (line 116) and that "all government and independent primary schools in each study region were invited to participate" (line 122), but also that "data were collected from 66% (85/129) of schools" (line 252). I would have expected the denominator here to be the 142 schools described previously – unless the difference is the Catholic primary schools, which it is implied may have been excluded? This requires clarification. Table 1: "Mean (SD) proportion meeting PA guideline 5 school days" and "Mean (SD) proportion using AT to or from school" are listed in the exposures section, but presumably these should really be outcomes? Table 2: As above, "beta coef" is reported but should be made clear from the column header, table title or table footnote what this actually means. Table titles should include the number of students and/or schools included.

VERSION 1 – AUTHOR RESPONSE

Reviewer's comment	Author response	Update in manuscript (marked-up version)
Reviewer 1		
Is the research question or study objective clearly defined? NO -I feel there needs to be a definitive research question at the end of your background section. Or at least, an expansion of your objective statement. You say the purpose "is to investigate whether the physical activity environment surrounding public schools is associated with…." That is a fine primary objective but there needs to be follow up concerning "to what degree" each of those variables influences physical activity and weight. Does this make sense? For example, if you did find association (which I can see that you did), your inquiry	Thank- you for this feedback. We have aimed to make a clearer statement of the objectives of the study at the end of the Background section. Additionally the contribution that this study makes to the current knowledge is also included.	Please see last paragraph of the background: pg 6-7 lines 121 – 125. Updated text: *In this study we aimed to quantify the relationships between physical activity environments surrounding primary schools and i)students' weight status ii) PA levels iii) active transport, in regional areas of Victoria, Australia. Findings from this study may aid in the prioritisation and targeting of policies and programs to improve PA environments around schools, so that all children have the opportunity to engage in PA, regardless of where they live.*

does not end there, rather, you need to address your next steps in discovering what particular variables are pertinent and what is their strength in its association with PA behavior and weight status. As a reader, I want to know a little more specifics about your exact aims and what direction you plan on going with regards to all of your measurements. Additionally, I would like to see a sentence or two following the research question on the impact that this research would have on. In other words, what is the overall contribution to knowledge if a study like this were carried out? Otherwise, the study objective is clear.		
Is the abstract accurate, balanced and complete? NO -Keywords: add "school environment" and "children" -Need to abbreviate physical activity to "PA" throughout the paper as is the industry standard.	Thank you for this feedback, these changes have been made	' School environment' and 'children' added to Keywords (pg 4, line 74) Physical activity has been updated to 'PA' throughout manuscript
Is the study design appropriate to answer the research question? NO Instrumentation? Especially for the Physical Activity Environment component. Where can one retrieve this survey instrument? How many schools? Need to elaborate more on the use of Arc GIS. Only a short sentence was provided.	The addition of Table 1 (as recommended in comment below) now includes all data sources used to inform each variable. The number of schools included out of all possible schools has been updated (with the updated analysis approach) and is included in the first paragraph of the results section The use of Arc GIS has been clarified and more detail included regarding the process	Table 1 has information on source of data – pg 10, line 197. Updated number of included schools on page 16, lines 331-333 <u>Updated text:</u> '*Data were collected from 65% (84/129) of eligible schools for two large-scale systems-based obesity prevention interventions'* Additional information on the use of ArcMap has been included on page 13, lines 258-266 <u>Updated text:</u> *School neighbourhoods (street network buffers) were imported from the AURIN results into ArcMap (ArcGIS Desktop, version 10.7.1 ESRI, Redlands, CA). The FOI data, which included greenspace and recreation facility locations projected as polygons, were also imported into ArcMap. Within ArcMap, the intersect tool was used to produce an attribute table including all recreational facilities and greenspaces that were within or intersected with the 1km*

		walkable neighbourhood around included primary schools. Duplicates were removed within school neighbourhood (where a polygon intersected with the buffer multiple times). This table was exported to Stata SE Version 15 (StataCorp, College Station, TX) for analysis.
Are the methods described sufficiently to allow the study to be repeated? NO -I strongly suggest adding a table that clearly lays out the variables of interest (both IV and DVs), what is the level of data (e.g., ordinal, ratio, etc.) and your analysis plan. This gets a little confusing as to how all of these data are coming together.	Thank you for this suggestion. Table 1 has been included outlining the variables included, level of data and data source.	Table 1 titled *'Description of independent, dependent and control variables used in the analysis'* has been included on page 10 which includes this information
Lines 231-249. Is there a reason why these questions are included in the manuscript? This seems completely random why there is a Q&A section at the end of your methods section. Maybe this is an appendix item? Consolidate the answers and leave out the questions, unless this was mandated by the journal publisher.	The questions were mandated by the journal to be included at the end of the Methods section. However, the responses have been updated to a paragraph rather than Q&A to improve flow of the paper	The patient and public involvement statement has been updated <u>Updated text:</u> *The wider trials from which the baseline data are drawn upon for this manuscript involved extensive collaboration with numerous community-based organisations (e.g. health services, primary care partnerships and local councils). Key local agencies contributed to recruitment and student level data collection.* *The outcome measurements (weight and health behaviours) were developed in conjunction with community-based organisations (e.g. health services, primary care partnerships) due to an absence of locally available data on the prevalence of childhood obesity and associated modifiable behaviours.*
Does this paper require specialist statistical review? YES -The GIS software "Arc GIS" was mentioned as a data collection method for the environmental components. This is a very specialized software that I am unfamiliar with and so I recommend any scholar with experience using Arc GIS, particularly when it comes to examining street connectivity, walkability, etc. to review their methods and statistical analysis	As above, we have added further detail on the spatial analysis component of the work. We are happy to address any further specific comments if required.	n/a
Reviewer 2		
We are told that the schools in the final analysis "included	Thank you for this comment. The first	Please see the first paragraph of the Results section, page 15-16 lines 317-

1,794 students with complete measures". Were there therefore students in these schools who participated in the study but were excluded from the analysis on the basis of incomplete measures? The extent of such exclusions should be reported and the potential consequences of excluding these students from the analysis considered	paragraph of the results has been updated to reflect the number of included schools and students and the extent of school and student exclusion (approx. 6% of students). The demographics of the excluded students are also included in this paragraph, along with potential consequences of these exclusions	330 for updated exclusions. Updated text: *Data were collected from 65% (84/129) of eligible schools for two large-scale systems-based obesity prevention interventions, with 79% (3,476/4,386) of eligible students within those schools participating in the study. Of these eligible students, 2,269 were in years 4 and 6. For this analysis, three special development schools were excluded due to not being assigned an ICSEA score and one further school did not have complete data on any year 4 or 6 students. This resulted in 80 schools being in the final analysis. These schools included 2,144 students with complete measures (94% of eligible year 4 and 6 students). There was some variation in gender and year level within the excluded students (n=72 boys, n=53 girls; n=74 year 6s, n=51 year 4s).* Limitations section in the Discussion has been updated to include potential consequences of excluding these students from analysis. See pg 24, lines 455-458 Updated text: *Additionally, the exclusion of Special Development schools due to these schools not being assigned a school-level SEP measure may impact the generalisability of the results, in particular regarding applicability of the results to students attending these schools.*
Outcome data were collected at the student level but aggregated to the school level for analysis, leading to the analysis of only 39 data points (schools). Did the authors consider instead conducting their analysis at the student level while accounting for the clustered nature of the exposures, for example by using multilevel modelling? Justification for their chosen aggregated approach should be provided.	Thank you for this suggestion. We have now updated the analysis to be conducted at the student level while accounting for clustering at the school level using multilevel models. This substantial change has resulted in a stronger analysis and allowed for the inclusion of a greater number of students and schools. The methods have been updated to reflect this change	Please see updated methods page 14, lines 276-287 Updated text: *Multilevel mixed effects logistic regression were fitted to test the association between independent variables: i) the count of recreational facilities within, or intersecting with, the 1km walkability buffer, ii) the count of greenspaces within, or intersecting with, the 1km walkability buffer, iii) the school walkability score, and each of three dependent variables; i) weight status (overweight or obese), ii)adherence to PA guidelines (yes/no) and iii)use of active transport(yes/no) as separate regressions. Multi-level linear regression models were fitted to test the associations between all three PA environment independent variables and the dependent variable of BMI z-score. For all models, clustering was accounted for at the school level. Initial models (model 1) did not include any*

		adjustment for covariates Please see updated Table 3 (formerly Table 2) with the results of the new analysis approach on pg 21-22
I would have assumed that socioeconomic position (SEP) was an important confounder to consider in these analyses. The authors have access to a seemingly continuous school-level measure of SEP, which they chose to dichotomise. This approach throws away a lot of information and minimises the opportunity for confounder control. The authors should revisit their specification of this variable in the models, instead retaining it as continuous variable or using a greater number of categories (e.g. quintiles, deciles). (I acknowledge that a dichotomised version is helpful when presenting stratified descriptive statistics as in Table 1 could be retained for this purpose.)	Thank you for this suggestion. We have now included the SEP measure (ICSEA) as a continuous variable in the models (Table 3) while retaining the dichotomised version for the descriptive statistics (Table 2)	Description updated in Methods section on pg 12, line 250-253 Updated text: *ICSEA scores for each participating school were included as a continuous variable in the regression models, but were dichotomised for comparison of descriptive statistics, categorising school into either equal to or above (≥1000) or below (<1000) the national mean*
We are told that "geographical location (according to remoteness) is included in the ICSEA calculation, and therefore was not adjusted for separately" (line 223). Presumably the logic here is that remoteness may be colinear with SEP? This seems unlikely given the number of other factors incorporated in the ICSEA (and highly unlikely under the current dichotomous SEP variable). If the authors consider remoteness to be a potentially important confounder then I would invite them to reconsider this decision (or provide further justification if I have misunderstood their reasoning).	This is a valid point. However the measure of SEP (ICSEA) has 4 inputs, one of which is geographical location (remoteness). Correlation analysis shows that the remoteness and ICSEA are collinear (pairwise comparison <0.05) in this sample, which is why remoteness was not included as a confounding factor.	Clarification of this is included on page 14, line 292-296 Updated text: *Geographical location (according to remoteness) is a direct input into the calculation of ICSEA. Correlation analysis shows that the two variables are collinear in this sample (pairwise correlation p<0.05) and therefore remoteness was not adjusted for in any of the models.*
Three of the four outcome variables are proportions (so bounded at 0 and 1) but are analysed using linear regression. The authors	With the updated analysis approach these outcomes are now included as dichotomous variables,	The updated analysis approach in included on pg 14, lines 276-287 Updated text: *Multilevel mixed effects logistic regression*

should note whether the distributional assumptions underlying linear regression were met (and if not, should revisit their analysis approach as per the above comment	assessed using multi-level logistic regression models	*were fitted to test the association between independent variables: i) the count of recreational facilities within, or intersecting with, the 1km walkability buffer, ii) the count of greenspaces within, or intersecting with, the 1km walkability buffer, iii) the school walkability score, and each of three dependent variables; i) weight status (overweight or obese), ii)adherence to PA guidelines (yes/no) and iii)use of active transport(yes/no) as separate regressions. Multi-level linear regression models were fitted to test the associations between all three PA environment independent variables and the dependent variable of BMI z-score. For all models, clustering was accounted for at the school level. Initial models (model 1) did not include any adjustment for covariates*
The three exposures are presumably correlated. The authors should consider the addition of mutually adjusted analyses to try to disentangle the associations further	Mutually adjusted analyses have now been included in Table 3 (Model 3) to investigate the associations between each of the independent variables and each of the dependent variables.	Table 3, Model 3 shows the results of the mutually adjusted regressions (pg 21-22). The statistical analysis description has been updated – see pg 14, lines 291-293 <u>Updated text:</u> *A third regression model (model 3) included all independent variables related to the PA environment.*
The number of exposures (3 plus one composite) and outcomes (4) means that the extent of multiple testing requires further consideration. This could be handled formally through p-value adjustment and certainly requires some discussion.	Thank-you for this suggestion. All associations were robust to a p-value of <0.01. We have alluded to the issue of multiple testing and the strength of the p-values in the discussion.	Please see Discussion, pg 25, lines 479-481 <u>Updated text:</u> *Additionally, although multiple tests have been conducted, consistent associations with active transport and the PA environment are found, and results remain consistent with adjustment of the p value threshold to <0.01.*
The authors note in the Discussion that “more recreational facilities and higher walkability scores were found in areas surrounding schools classified as lower compared to higher SEP”. I am not familiar with the relevant literature, but this struck me as counterintuitive – if that is the case then further discussion is required.	The findings that lower SEP areas have better PA environments compared to higher SEP schools remains, in the updated analysis approach. These findings are now discussed further in relation to the current literature.	Please see pg 24, lines 463 – 470 discussing these results in relation to other literature. <u>Updated text:</u> *However, in this study, more recreational facilities and greenspaces and higher walkability scores were found in areas surrounding schools classified as lower compared to higher SEP. There have been similar findings in other studies examining associations between area-level SEP and greenspace, recreational facilities, and walkability where lower SEP areas have had more facilities or higher walkability scores compared to higher SEP areas. These results highlight the complexity of these relationships, with factors such as quality and accessibility also playing important roles.*

The authors note in the Discussion that “Our finding of the significant association between the total physical activity environment score and the proportion of students with high weight status was in the opposite direction to our hypothesis and may reflect type II error”. Firstly, given that an association was identified is this not really a type I (false positive) error rather than a type II (false negative) error? Secondly, neither a type I nor type II error can really explain the identification of an association in the opposite direction to the (assumed) true association. Further discussion, particularly around the possibility of confounding, is required here.	In the updated analysis this specific association is no longer current. Therefore the paragraph discussing this finding has been removed	Removal of paragraph on pg 25, lines 492-495 due to being no longer relevant to the findings
Throughout: The word “region” or “regional” (e.g. “regional primary schools”, “regional areas”) is used in a way that I am unfamiliar with. Line 83 (“Compared to children living in metropolitan areas, regional and rural children…”) suggests it might mean non-metropolitan but non-rural areas? Perhaps this could be clarified at first use.	Thank you for highlighting this point. This is a common usage in Australia for areas outside of major cities (but not necessarily ‘remote’), which we acknowledge may not be clear to international readers. We have updated a paragraph in the background section to used consistent terms. We have also clarified these terms in the methods section to reduce confusion.	Updated paragraph on page 5-6, line 94-105. <u>Updated text:</u> *Limited research has been conducted on the PA environment outside of major cities in Australia and internationally. Australian data indicates that overweight and obesity prevalence has significantly increased outside of major cities areas since 2010, whereas it appears to have plateaued in major cities. Compared to children living in major cities, children living outside of major cities have been reported to be more physically active overall, although have lower levels of active transport. Several major city based studies have reported that key determinants of whether a child uses active forms of transport to and from school include distance to school, population density, and street connectivity, which are all aspects of the environment that are likely to differ between major cities and regional and remote areas.* Updated methods sections page 12, lines 238-243 <u>Updated text:</u> *Remoteness classification for each school was determined according to the five categories of the Accessibility/Remoteness Index for*

		Australia (ARIA+); Major Cities, Inner Regional, Outer Regional, Remote and Very Remote. Classification is based on a continuous variable derived from the area's access to services, measured as distance by road, and the population of the closest centre. All schools included in this study fall into the Inner Regional and Outer Regional categories.
Line 32: Results are reported as "beta = X" in the abstract. It is not clear what "beta" represents here. Better to report results on a more intuitive scale and/or with appropriate units. Some indication of what they are adjusted for is also required.	Thank you for this. The results have been significantly reworked in response to the suggested alternative analysis approach. As such results are now reported as Odds Ratios, which are more intuitive to interpret for the reader. The variables that have been adjusted for has also been included in this section	Updated Results in the Abstract – Page 2, lines 36-45 Updated text: *When adjusted for student and school demographics, students had significantly increased odds of using active transport to or from school when the school neighbourhood was more walkable (OR 1.21 (95%CI 1.09, 1.35), had a greater number of greenspaces (OR 1.35 (95%CI 1.20, 1.53)) and a greater number of recreational facilities (OR 1.18 (95% CI 1.07, 1.31)). A higher cumulative PA environment score was also associated with a higher proportion of children using active transport (OR 1.33 (95%CI 1.28, 1.51)).*
It was not completely clear whether the included schools were sampled or comprised the entire population of schools in the regions studied. We are told that the region includes "142 government, Catholic and independent primary schools" (line 116) and that "all government and independent primary schools in each study region were invited to participate" (line 122), but also that "data were collected from 66% (85/129) of schools" (line 252). I would have expected the denominator here to be the 142 schools described previously – unless the difference is the Catholic primary schools, which it is implied may have been excluded? This requires clarification.	Thank you for highlighting this oversight in our description. It is correct that the difference is the Catholic schools which did not grant opt-out consent process in 2015. We have included a more detailed explanation of school recruitment and inclusion to make this clearer in the 'participants' section of the Methods. The Results section has also been updated regarding the school inclusions	See Methods section, pg 8, lines 148-156. Updated text: *In brief, during the 2015 (South-West region) and 2016 (Goulburn Valley region) data collection periods all primary schools (Government, Catholic and Independent) in each study region were invited to participate. In participating schools, all students in year 2 (aged approximately 7-8 years), year 4 (aged approximately 9-10 years) and year 6 (aged approximately 11-12 years) were invited to participate via an opt-out recruitment approach. Catholic schools data were not included in 2015 as approval to use passive (opt-out) recruitment processes were not granted by Catholic schools in that year, and evidence shows that opt-in consent can result in up to 5% lower overweight and obese prevalence detection* See Results section, pg 16, lines 331 – 337 Updated text: *Data were collected from 65% (84/129) of eligible schools for two large-scale systems-based obesity prevention interventions, with 79% (3,476/4,386) of eligible students within those schools*

		participating in the study. Of these eligible students, 2269 were in years 4 and 6. For this analysis, three special development schools were excluded due to not being assigned an ICSEA score and one further school did not have complete data on any year 4 or 6 students. This resulted in 80 schools being in the final analysis.
Table 1: "Mean (SD) proportion meeting PA guideline 5 school days" and "Mean (SD) proportion using AT to or from school" are listed in the exposures section, but presumably these should really be outcomes?	Thank-you for highlighting this error – it has been corrected	See updated Table 2 (formally Table 1) – pg 19
Table 2: As above, "beta coef" is reported but should be made clear from the column header, table title or table footnote what this actually means.	The updated Table 3 now has this information (Odds Ratio and mean change in BMI z-score)	See updated Table 3 (formally table 2) - pg 21-22
Table titles should include the number of students and/or schools included.	The titles of Table 2 and 3 have been updated to include the number of schools and students	Titles of Table 2 (pg 18-19) and Table 3 (pg 21-22) have been updated **Updated titles:** Table 2: Descriptive statistics of schools (n=80) and students (n = 2,144) Table 3: Associations Between Physical Activity Environment, Weight and Behavioural Outcomes (students n = 2,144)

VERSION 2 – REVIEW

REVIEWER	Carlton, Troy Catawba College, Sport & Health Sciences
REVIEW RETURNED	05-Jun-2021

GENERAL COMMENTS	No further comments.