PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Toward a Demographic Risk Profile for Sedentary Behaviors in Middle-Aged British Adults: A Cross-Sectional Population Study
AUTHORS	Patterson, Freda; Lozano, Alicia; Huang, Liming; Perkett, Mackenzie; Beeson, Jacqueline; Hanlon, Alexandra

VERSION 1 – REVIEW

REVIEWER	Sofie Compernolle Ghent University, Belgium
REVIEW RETURNED	03-Oct-2017

GENERAL COMMENTS	This paper discusses demographic predictors (both individual and clusters) of sedentary behavior in a large sample of British middle-aged adults. While the study is interesting and innovative - especially the identification of hierarchical clusters - some issues need to be further explained/developed in order to be published. Abstract: - The abstract is somewhat misleading as overall daily sedentary time was not measured. Please clearly indicate which sedentary behaviors that were assessed, and indicate how overall daily sedentary time was computed in the methods section. - Please add somewhere in the abstract that the study was conducted among middle-aged adults - Please clarify that all measures were self-reported in the methods section of the abstract. - It is unclear what is meant with 'the most proximal sedentary behavior' (last sentence of the abstract). I would also replace 'the evaluation of targeted interventions' into 'the development of targeted interventions' Introduction - Please use the recent definition of sedentary behavior, published by Tremblay et al. in IJBNPA (Sedentary Behavior Research Network (SBRN) – Terminology Consensus Project process and outcome) - De Rezende et al. published an overview of systematic reviews regarding sedentary behavior and health; including this recent publication would be of added value: Sedentary behavior and health outcomes: an overview of systematic reviews (de Rezende et al., 2014; PloS One) - Line 91-92: This sentence is too strong, as there are PA interventions that have shown long-term effects - Line 98: e.g. --> The sentence ends with a comma... - Line 105-110: Please refer to the recent review by O'Donoghue regarding correlates of sedentary behavior, instead of only reporting single studies. The results of this review provide a complete

	overview of what is known on correlates of sedentary behavior. - Line 114: The range of self-reported tools was indicated as an important limitation. However, this limitation was not addressed in the current study. Why not? Methods: - Line 151: Is it correct that you only asked for hours? And no minutes? This is a really important study limitation! Needs to be clearly identified in the limitation section! - Line 153: Again, I'm not in favor of using the term 'total sedentary time', as this is not what was measured... - Line 158-162: Please provide information on why these socio-demographic factors were chosen. Why was family situation/having children not included? Results - Line 258: -0.51 fewer hours? I expect the negative sign should be deleted? Discussion - Line 298: The current study queried about recreational sedentary behaviors – This is not entirely correct, as driving might also include driving as part of someone's job. This was also mentioned in line 322: 'daily driving time could be related to blue-collar service jobs' - Line 325: 'non-work specific sedentary behavior was 4.60'. Again, it is not possible to report on 'non-work specific sedentary behavior', as no information is available on if the reported time spent sedentary was part of the job or not - Limitations: As only middle-aged adults were included in this study, the results of the current study are not generalizable to adults in general – I suggest to include this as a limitation.

REVIEWER	Célina Roda ISGlobal, Spain
REVIEW RETURNED	03-Dec-2017

GENERAL COMMENTS	General comments Thanks for the opportunity to review this study. Using data from the UK biobank, the authors have investigated the sociodemographic characteristics associated with sedentary behaviors in adults. Although this study has some limitations (e.g. cross-sectional, self-reported), it helps in the identification of target groups for interventions to limit sedentary bahaviors. The following are some suggestions for the authors to consider for improving clarity. More details on the study population are needed, and generalization of results should be discussed. The authors have analyzed the data using 2 diffferent approaches (regression analysis and CART). The added value of each approach and their complementarity should be explained. Abstract Some characteristics of the study population should be given (e.g. age, sex, etc.) Strengths and Limitations The CART analysis is not an innovative approach, it has been in use for 30 years (Breiman et al ; 1984). Please rehrase the corresponding bullet point. Only self-reported data is used in this study, please rephrase the

	corresponding bullet point. Introduction Generally fine but I think some references related to the statistical approach have been missed. A reference of the method CART (e.g. Breiman et al. 1984), and some examples of application should be given (e.g. Roda et al. 2016, King et al. 2015). The aim of the study is mentioned twice, please rephrase. Replace « television watching » by « television viewing ». Material & Methods Did you collect information on sedentary behviors during a week day and a weekend day separately? Did you collect information on disability? Inclusion of disabled persons may biased the results. It is unclear if the authors examined the goodness-of-fit of the linear models. Please specify it. Did you consider all covariates in the CART analysis (e.g. sex, age, etc.)? Results The comparison of included and excluded subjects should be presented in a Table. In the text, the confidence intervals at 95% should be given instead of standard errors. So, the reader can judge easily the significance of the results. The associations of sedentary behaviors with physical activity are not presented. Could you specify them? Please specify the standard deviations of means in all nodes of the tree, and in the text when mentioned. CART analysis seems to be performed on 207,369 individuals, and not on 345,616 individuals. Please explain this difference? Discussion Please rephrase the second sentence by something like « interventions to limit sedentary behaviours ». Several factors, such as age, season, residence area are not discussed. Please discuss the potential generalization of the results. Tables Table 1. Rephase the title « Study sample characteritsits » Please check the sum of percentages. Please specify the abbreviations (N, M, SD) in the footnote.

REVIEWER	RUTH PICKERING UNIVERSITY OF SOUTHAMPTON, UK
REVIEW RETURNED	22-Dec-2017

GENERAL COMMENTS	I thought this was an interesting paper, but the exclusion of sedentary behaviour during work does weaken it, though this is mentioned as a limitation. There were some parts that I felt could be better described. 1 On page 7, line 27/165, where they say METS was included as a controlling variable - in this in the linear regressions (Table 2), the CART analysis or both? If its in the linear regressions why aren't parameters estimates for METS reported in Table 2? Is it possible to go into CART so that it is not considered as a variable for splitting the dataset into groups?

	2 On page 7, line 51/175 the analytic sample is 345616 out of the total of 502656 records from the Biobank, with the exclusions due to missing data. No missing data methods have been considered, and this is a further limitation that should be discussed. 3 Page 8, line 43/195, they say validation of CART was performed: 60% training and 40% validation, and the resulting trees (Figures 1a to 1d) are thus based on a sample size n=207369. The validation results should also be reported. Did they confirm the trees in Figures 1a-d? 4 In Table 2, regression parameter estimates are labelled as beta, but this often means that they have been standardised. I don't think this is the case here as they describe the differences between levels of the categorical variables in terms of hours/day, but I don't think they interpret the regression parameters associated with age. Since age is a significant predictor of the individual sedentary activates but in opposite directions its impact cancels out in total sedentary behaviour. Please clarify whether the parameters for continuous predictors (age and METS) were standardised. 5 Rather than present Wald statistics for each level of the categorical variables, I think it would be better to present ANOVA type test P values for equality across all levels of the categorical variables. The significance of contrasts between each level and the reference level can be judged from the confidence intervals. 6 The title of table 2 might be better described as "Regression results" rather than "associations". 7 The titles of Figures 1a-1d. I think these should be described as Results of the CART analysis, or tree structure, or something better than associations.

REVIEWER	Richard Woodman Flinders University, Australia
REVIEW RETURNED	26-Dec-2017

GENERAL COMMENTS	A major limitation of the study, as the authors note in the discussion is that the survey relied on self-report and the latter is subject to reporting bias - which may differ according to the very demographics being studied. In addition, there is no mention of whether computer related time was completely sitting of sitting/standing with desk stands for the computers. Given the large sample size, I would suggest the authors stratify their analysis by employment status to try to disentangle the possible influence of work related sedentary behaviour from non-work related sedentary behaviours. The use of a training and validation sample is mentioned in the methods but the results do not differentiate between the 2 sets. Were the findings from the training set validated? There is a large amount of missing data (31%). Were any sensitivity analyses performed such as using multiple imputation? How might results have been affected had all those that were missing data had small/large amounts of sedentary behaviour? Page 8 lines 178-9. Why were all analyses adjusted for female, white, college attendees, and employed? I thought these were the pre-specified predictors (page 7 lines 159-161). What is the rationale for adjusting for physical activity? Since the predictors of interest are mostly non-modifiable (age, gender), these

	would not be able to be influenced by physical activity and therefore cannot be confounders. Was the idea to study only the direct effects of the pre-specified variables – after the influence of physical activity was removed – in which case physical activity is being considered as a mediator of some of the effects of the demographic variables? How does the decision tree analysis add to the results of the MV regression? For example, how much additional increase in explained variability was there with each variable – both for the MV regression and the decision tree analysis? This could be shown in the text and/or columns. Please explain the use “data-driven” for readers. Table 1 lists sample characteristics, all of which were included in the MV regression. Only 3 of these variables were included in the decision tree analysis, so is the latter similar to a stepwise regression approach with only highly predictive variables included? To what extent do these data truly inform given the lack of contextual data. For example, a working/non-working college graduate may well watch less TV than a retired non-college educated individual, but what is the reason – a real difference in real sedentary time-behaviour or a difference related to generational differences. Is there any evidence from the study data to support such hypotheses? Were there perhaps inverse correlations between those that watched less TV and those that spent more time with computers?

VERSION 1 – AUTHOR RESPONSE

Reviewer #1

Abstract

1. The abstract is somewhat misleading as overall daily sedentary time was not measured. Please clearly indicate which sedentary behaviors that were assessed, and indicate how overall daily sedentary time was computed in the methods section.

This point is well taken, and we have revised the abstract so that it clearly states that the definition of overall sedentary time for this study was the sum of time spent in each of the measured sedentary behaviors. Specifically, the abstract now states: “Data-driven decision tree models were generated for each of the sedentary behavior outcomes of hours per day spent television viewing, recreational computer use and driving; the sum of time spent in these sedentary behaviors (“overall”) was also generated.” [Page 2, lines 53-56].

2. Please add somewhere in the abstract that the study was conducted among middle-aged adults.

The mean age of the sample has been added to the abstract (“mean age of 56.6 (SD=8.1) years”; Page 2, Line 62). We have also added that the sample were “middle-aged” in the strengths and limitations bullet points (page 3, line 198).

3. Please clarify that all measures were self-reported in the methods section of the abstract.

This has been added (“All data were self-reported”; Page 2, Line 58-59)

4. It is unclear what is meant with 'the most proximal sedentary behavior' (last sentence of the abstract). I would also replace 'the evaluation of targeted interventions' into 'the development of targeted interventions'

We have simplified this sentence as recommended. It now reads: “The development of targeted interventions to reduce sedentary behavior in different demographic sub-groups is needed.” [Page 3, line 193-194].

Introduction

5. Please use the recent definition of sedentary behavior, published by Tremblay et al. in IJBNPA (Sedentary Behavior Research Network (SBRN) – Terminology Consensus Project process and outcome). De Rezende et al. published an overview of systematic reviews regarding sedentary behavior and health; including this recent publication would be of added value: Sedentary behavior and health outcomes: an overview of systematic reviews (de Rezende et al., 2014; PloS One)

As recommended, these citations have been added. [Page 4, line 252 and line 254]

6. Line 91-92: This sentence is too strong, as there are PA interventions that have shown long-term effects

Agreed. We have softened the language in this sentence to read as follows: “Given that interventions to address other, “established” behavioral cardiovascular risk factors such physical inactivity, tobacco use, and poor dietary behaviors may have limited long-term efficacy.” [Page 4, lines 257-259]

7. Line 98: e.g. --> The sentence ends with a comma...
Thank you. We have superscripted the e.g., as it should have been from the outset so the sentence now reads: “In addition to quantifying the independent relationship between sedentary behavior time with disease outcomes, research has also been dedicated to identifying predictors of sedentary time. e.g., 6”

8. Line 105-110: Please refer to the recent review by O'Donoghue regarding correlates of sedentary behavior, instead of only reporting single studies. The results of this review provide a complete overview of what is known on correlates of sedentary behavior.

This has been added. Please see page 5, lines 289-291.

9. Line 114: The range of self-reported tools was indicated as an important limitation. However, this limitation was not addressed in the current study. Why not?

This is an excellent point. The nature of the UK biobank publically available dataset is such that the data are self-report (although in 2019, summary actigraph data for physical activity and sedentary behavior is expected for a sub-set of the cohort). In response to this concern we have noted that self-report data in smaller community samples have been widely used to assess demographic differences in sedentary behaviors. By using a large population study (albeit also using self report measures) we hope to have advanced the current state of knowledge. [Page 5 Line 295-296]

Methods

10. Line 151: Is it correct that you only asked for hours? And no minutes? This is a really important study limitation! Needs to be clearly identified in the limitation section!

Yes, the UK Biobank queries sedentary behavior in terms of hours per day and not minutes per day. The questions are as follows:

"In a typical DAY, how many hours do you spend watching TV? (Put 0 if you do not spend any time doing it)"

"In a typical DAY, how many hours do you spend using the computer? (Do not include using a computer at work; put 0 if you do not spend any time doing it)"

"In a typical DAY, how many hours do you spend driving?"

This hour/day assessment of sedentary behavior time has been added as a study limitation: “Moreover, survey respondents provided typical hours of time in sedentary behavior and not minutes, thus the precision of the self-report is limited.” [Page 16, lines 931-932]

11. Line 153: Again, I'm not in favor of using the term 'total sedentary time', as this is not what was measured...

Agreed. We have clarified that we are using the term “overall” sedentary time and operationalizing this as the sum of self-reported time (in hours) spent watching television, recreational computer use, and driving. We hope this clarity in how we are defining the term helps resolve this concern.

12. Line 158-162: Please provide information on why these socio-demographic factors were chosen. Why was family situation/having children not included?

We agree that considering family situation and number of children would have been an important demographic characteristic to have included; unfortunately this variable was not available in the dataset. We have added this as a limitation to the study. [Page 16, lines 934-935]

Results

13. Line 258: -0.51 fewer hours? I expect the negative sign should be deleted?

This is no longer relevant since the linear regression modeling has been removed as per an earlier concern.

Discussion

14. Line 298: The current study queried about recreational sedentary behaviors – This is not entirely correct, as driving might also include driving as part of someone’s job. This was also mentioned in line 322: ‘daily driving time could be related to blue-collar service jobs’. Line 325: ‘non-work specific sedentary behavior was 4.60’. Again, it is not possible to report on ‘non-work specific sedentary behavior’, as no information is available on if the reported time spent sedentary was part of the job or not.

We agree and in response to this comment have removed all categorization of sedentary behavior in terms of work versus recreational sedentary behavior.

15. Limitations: As only middle-aged adults were included in this study, the results of the current study are not generalizable to adults in general – I suggest to include this as a limitation.

This limitation has been added. [Page 16, line 930-932]

Reviewer: 2

1. More details on the study population are needed, and generalization of results should be discussed.

This point is well taken and relates to reviewer 1 concerns also. In response we have added descriptive statistics about the sample to the abstract and have noted that since the sample was comprised of mostly middle age and White adults, the generalizability to adults who are younger and/or non-White is not known. [Page 2, lines 61-62; Page 16, lines 957-958]

2. The authors have analyzed the data using 2 different approaches (regression analysis and CART). The added value of each approach and their complementarity should be explained.

In reflecting on this comment, we decided to remove the regression analysis since using the CART approach was our primary goal. The decision tree analysis allows us to identify data-driven patterns that can reveal complex interactions between predictors, and thus capture profiles of high or low risk groups with relatively high accuracy. All reference to the regression analysis has been removed throughout the manuscript.

Abstract
3. Some characteristics of the study population should be given (e.g. age, sex, etc.)

Thank you. Characteristics of the study population have been added to the abstract.

4. The CART analysis is not an innovative approach, it has been in use for 30 years (Breiman et al ; 1984). Please rephrase the corresponding bullet point.

Thank you for your comment. This bullet has been rephrased.

5. Only self-reported data is used in this study, please rephrase the corresponding bullet point.

Duly noted. This information has been updated in the corresponding bullet.

Introduction
6. Generally fine but I think some references related to the statistical approach have been missed. A reference of the method CART (e.g. Breiman et al. 1984), and some examples of application should be given (e.g. Roda et al. 2016, King et al. 2015).

As suggested, we have added the Breiman et al citation and the examples suggested.[Page 6, line 329]

7. The aim of the study is mentioned twice, please rephrase.

This section has been rephrased so that the aim of the study is provided once. [Page 6, lines 324-329]

8. Replace « television watching » by « television viewing ».

This has been done. [Page 6, line 323]

Material & Methods

9. Did you collect information on sedentary behaviors during a week day and a weekend day separately?

There was no distinction between weekend and weekday sedentary in touchscreen questionnaire, as participants were asked to specify hours in a typical day for each sedentary behavior. This has been added as a study limitation. For more information, the UK Biobank survey questions used in this study are as follows:

"In a typical DAY, how many hours do you spend watching TV? (Put 0 if you do not spend any time doing it)"

"In a typical DAY, how many hours do you spend using the computer? (Do not include using a computer at work; put 0 if you do not spend any time doing it)"

"In a typical DAY, how many hours do you spend driving?"

10. Did you collect information on disability? Inclusion of disabled persons may biased the results.

No information on disability status was collected. This has been added as a study limitation. [Page 15, line 983]

11. It is unclear if the authors examined the goodness-of-fit of the linear models. Please specify it.

This is no longer relevant since the linear regression modeling has been removed as per an earlier concern.

12. Did you consider all covariates in the CART analysis (e.g. sex, age, etc.)?

Yes, all demographics (age, sex, race, education, employment status, shift work, residence, season of assessment) and categorized total physical activity were considered in all decision tree models for all sedentary behavior outcomes.

Results

13. The comparison of included and excluded subjects should be presented in a Table.

The comparison of included and excluded participants has been provided in Table 1.

14. In the text, the confidence intervals at 95% should be given instead of standard errors. So, the reader can judge easily the significance of the results.

This is no longer relevant since the linear regression modeling has been removed as per an earlier concern.

15. The associations of sedentary behaviors with physical activity are not presented. Could you specify them?

This is no longer relevant since the linear regression modeling has been removed as per an earlier concern.

16. Please specify the standard deviations of means in all nodes of the tree, and in the text when mentioned.

This is no longer relevant since the linear regression modeling has been removed as per an earlier concern.

17. CART analysis seems to be performed on 207,369 individuals, and not on 345,616 individuals. Please explain this difference?

CART analysis was performed on all 415,666 individuals. However, 60% of the full sample (N=249,399) was used as the training data to train the trees with cross validation, and the remaining 40% of the sample (N=166,267) was used to evaluate the performance of the trees.

Discussion

18. Please rephrase the second sentence by something like « interventions to limit sedentary behaviours ».

Done. [Page 11, Line 960]

19. Several factors, such as age, season, residence area are not discussed. Please discuss the potential generalization of the results.

Age was included as a predictor of interest in the analysis. Consideration of season and area of residence was outside the bounds of the research question, but we have added consideration of environmental factors as an area for further research. [Page 15, lines 1101-1102]

Tables

20. Table 1. Rephrase the title « Study sample characteristics »

The title for Table 1 has been updated.

21. Please check the sum of percentages.

Thank you for your comment. The sum of percentages has been checked for all entries, although some entries may not sum up to 100% due to rounding. A second decimal place was taken in this version of the paper.

22. Please specify the abbreviations (N, M, SD) in the footnote.

These abbreviations have been included as footnotes in Table 1.

Reviewer 3

1. On page 7, line 27/165, where they say METS was included as a controlling variable - in this in the linear regressions (Table 2), the CART analysis or both? If it's in the linear regressions why aren't parameters estimates for METS reported in Table 2? Is it possible to go into CART so that it is not considered as a variable for splitting the dataset into groups?

This is no longer relevant since the linear regression modeling has been removed as per an earlier concern.

2 On page 7, line 51/175 the analytic sample is 345616 out of the total of 502656 records from the Biobank, with the exclusions due to missing data. No missing data methods have been considered, and this is a further limitation that should be discussed.

Thank you for your comment. We have included a comparison of participants who were included and excluded in the study in Table 1. Although significant differences between participants with complete and incomplete data were observed for all study variables, this is due to our large sample size and high power . As a result, Cohen's d were calculated and were deemed either very small (<0.20) or small (0.20-0.49) in size for all study variables, indicating negligible differences between participants with complete and incomplete data. Thus, all analyses in this study reflect data from participants with complete information for all study variables.

3 Page 8, line 43/195, they say validation of CART was performed: 60% training and 40% validation, and the resulting trees (Figures 1a to 1d) are thus based on a sample size n=207369. The validation results should also be reported. Did they confirm the trees in Figures 1a-d?

Cross validation was run on the training dataset by the package 'rpart' in R automatically. That is, we did not do it explicitly. As such, 40% of the sample was used to evaluate the trees trained on the training dataset. R-squared for both the training and testing sets was reported in the revised manuscript.

4 In Table 2, regression parameter estimates are labelled as beta, but this often means that they have been standardised. I don't think this is the case here as they describe the differences between levels of the categorical variables in terms of hours/day, but I don't think they interpret the regression parameters associated with age. Since age is a significant predictor of the individual sedentary activates but in opposite directions its impact cancels out in total sedentary behaviour. Please clarify whether the parameters for continuous predictors (age and METS) were standardised. Rather than present Wald statistics for each level of the categorical variables, I think it would be better to present ANOVA type test P values for equality across all levels of the categorical variables. The significance of contrasts between each level and the reference level can be judged from the confidence intervals. The title of table 2 might be better described as "Regression results" rather than "associations".

Based on earlier reviewer feedback, the regression models have been removed from the analysis. Thus, these points were not considered.

7 The titles of Figures 1a-1d. I think these should be described as Results of the CART analysis, or tree structure, or something better than associations.

Thank you, the titles of Figures 1a-1d have been modified using these suggestions.

Reviewer: 4

1. There is no mention of whether computer related time was completely sitting or sitting/standing with desk stands for the computers. Given the large sample size, I would suggest the authors stratify their analysis by employment status to try to disentangle the possible influence of work related sedentary behaviour from non-work related sedentary behaviors.

We did not complete this suggestion to stratify the analysis by employment status to disentangle work versus non-work related sedentary behavior because the survey question querying computer use specifically queries recreational computer use, thus computer use was not work-related. We have stated that not being able to fully distinguish work and non-work related sedentary behavior is a limitation for this study and an important area for future work.

2. The use of a training and validation sample is mentioned in the methods but the results do not differentiate between the 2 sets. Were the findings from the training set validated?

This point was very well taken. The tree model was fit with cross validation on the training set. In other words, the tree chosen was optimal based on cross validation, given the criteria that every split must increase the overall R-squared at least by 0.01. In addition, the tree model fit was evaluated using the testing set.

3. There is a large amount of missing data (31%). Were any sensitivity analyses performed such as using multiple imputation? How might results have been affected had all those that were missing data had small/large amounts of sedentary behavior?

Thank you for your comment. Please see Reviewer 3 Comment 2 for more information.

4. Page 8 lines 178-9. Why were all analyses adjusted for female, white, college attendees, and employed? I thought these were the pre-specified predictors (page 7 lines 159-161).

Age, sex, education, race, and employment status were all predictors of interest, and as such, this sentence has been removed.

5. What is the rationale for adjusting for physical activity? Since the predictors of interest are mostly non-modifiable (age, gender), these would not be able to be influenced by physical activity and therefore cannot be confounders. Was the idea to study only the direct effects of the pre-specified variables – after the influence of physical activity was removed – in which case physical activity is being considered as a mediator of some of the effects of the demographic variables?

Given that the regression models were removed from the study this point is not fully relevant anymore. Nevertheless, it is important to note that physical activity was included in the decision tree models so that the demographic risk profile for sedentary behavior, independent of physical activity, could be identified. This has been clarified in the text. [Page 7, line 380-381]

6. How does the decision tree analysis add to the results of the MV regression? For example, how much additional increase in explained variability was there with each variable – both for the MV regression and the decision tree analysis? This could be shown in the text and/or columns.

Please refer to our response to Reviewer 2 Comment 2 for more information.

7. Please explain the use “data-driven” for readers.

By data-driven analyses examine relationships between variables without explicit prior hypotheses. For clarity, the term data-driven was removed.

8. Table 1 lists sample characteristics, all of which were included in the MV regression. Only 3 of these variables were included in the decision tree analysis, so is the latter similar to a stepwise regression approach with only highly predictive variables included?

Please note that all demographic variables and categorized total physical were considered as predictors in the decision tree analysis. However, those that appear in the decision trees were those that were considered “important” predictors for each outcome.

9. To what extent do these data truly inform given the lack of contextual data. For example, a working/non-working college graduate may well watch less TV than a retired non-college educated individual, but what is the reason – a real difference in real sedentary time-behaviour or a difference related to generational differences. Is there any evidence from the study data to support such hypotheses? Were there perhaps inverse correlations between those that watched less TV and those that spent more time with computers?

The rationale for this research question came from a call by the American Heart Association for a

greater understanding of the demographic determinants of different sedentary behaviors – this population study, we believe goes a long way in answering this call. However, the point is well taken that understanding the role of environmental/contextual factors on sedentary behavior is a crucial area for further inquiry. We thank the reviewer for the comment.

VERSION 2 – REVIEW

REVIEWER	Sofie Compernolle Ghent University, Belgium
REVIEW RETURNED	21-Feb-2018

GENERAL COMMENTS	The authors did a great job revising the manuscript. They answered the questions appropriately, and adapted the manuscript accordingly. Just two small comments: - Abstract: The first sentence in the conclusion starts with 'Daily h/d' – Maybe it would be better to say 'Daily time' to avoid repetition. - Introduction: 'While studies have generally shown older adults (>60 years) to be more sedentary than younger adults…'. This is indeed true, however, this was not examined in reference 9. I would suggest to refer to a review that includes different studies examining age as a correlate in order to strengthen this statement.

REVIEWER	Célina Roda ISGlobal, Spain
REVIEW RETURNED	22-Feb-2018

GENERAL COMMENTS	Abstract "hierarchical clusters of demographic factors" is confusing. The hierarchy of demographic factors is identified, right? Please rephrase the sentence. "Sex, …, as well as physical activity were the predictors" should be replaced by "considered as predictors". Bullet-points of strengths and limitations Please delete "Data-driven" in the second bullet-point. Introduction Line 101 page 4: Please put into brackets e.g., 8 and place it before the end of the sentence or cite the author names instead of. It would be more useful if the authors comment the results of the review of O'Donoghue instead of referring the reader to this review. "capture what combination or clustering of demographic factors" is unclear because individuals are clustered according to the combination of demographic factors. Please rephrase. Please delete "data-driven". "to identify individual and hierarchical clusters of demographic characteristics" is unclear and repeated several times throughout the manuscript. As mentioned above, rephrase. Methods While the period of data collection is important to mention, the period of analysis is useless. "Data analysis for the current study was conducted in 2018" can be removed. Please replace "METS" by "METs". Statistical analysis:

	"502,243 participants" is mentioned. Isn't "502,543" more accurate? Please check the numbers. The first paragraph is not really related to the statistical analysis but more to data management and study sample definition. Please rename this subsection or move this part in more appropriate (sub)-sections. Only the comparison study of excluded and included participations is relevant here and should be kept in the statistical analysis section! Please add the abbreviation "SD". Results Table 1. Precision of 2 decimals is not relevant, please round the percentages and check that all totals are equal to 100%. The abbreviation METs should be defined in the footnote of the table. Line 228 page 11: Please delete "hours/day" in the subtitle. Modalities of variables seem to be combined (e.g. "no college" and "prefer not to answer") – that is due to the CART algorithm that creates binary splits. This characteristic of CART needs to be presented when the approach is presented in the Methods section. Since all results are now from CART analysis, it is not necessary to specify "In the data-driven decision..." in all subsections Discussion "All models were adjusted for physical activity levels". There is no adjustment for in CART analysis. Several factors are considered as potential predictors of the outcomes. Please change "total sedentary time" by "overall sedentary time" as made earlier in the text. "That adults who had not attended college and were retired were the highest risk group for television viewing by accruing a full 1.55 more hours per day of television time than those who went to college and were employed or unemployed (Figure 1b)". Is the factor "1.55" accurate? Please check. General comments The R-squared coefficients seem low. Is that common in CART analysis? How the authors interpret that? What is the association between college attendance and employment status? How the authors interpret the difference in hierarchy of demographic factors found for each sedentary behaviour? Besides demographic factors, physical activity was also considered as a potential predictor but not retained in the trees, how do you interpret that?

REVIEWER	RUTH PICKERING UNIVERSITY OF SOUTHAMPTON, UK
REVIEW RETURNED	06-Mar-2018

GENERAL COMMENTS	The paper describes CART modelling of a large UK datasets into groups with similar amounts of recreational sedentary data. I've got a couple of points that the authors could address. 1 Introduction , page 4, line 17, they mention more than 10 hours of riding in a car, on this line. Is this in a typical week? 2 When reading the Discussion I wasn't sure whether the hours of driving reported was just recreational or all driving. In the Abstract,

	page 2 line 22/3, they do say "recreational computer use and driving". In the methods, page 6, line 55, it is "using a computer for recreation, and, driving on a typical day". So trying to check this point I wasn't sure whether the adjective "recreational" applied just to computer use or to driving as well. This would be easily clarified by specifying "recreational driving " on page 6, line 55. In relation to my previous point, does "driving" include riding in a car as a passenger - as specified in the research reported in reference 6. 3 Table 1, percentages are reported here to two decimal places, in the text referring to these results percentages are rounded to one decimal place. The table would be clearer with only 1 decimal places, in fact I thought not much would be lost by reporting all percentages with no decimal places. 4 Reporting R-squared for training and testing samples, for example page 11, line 47/8. In all cases the R-squareds are low, and I believe this is interpreted as the percentage of variation in the hours in sedentary behaviour in question explained by the groups found. In this case only 5%. This should be mentioned in the Discussion. It is interesting that in all cases the R-squared is identical in training and testing sets, I wasn't anticipating that. But perhaps given the very large sample size and if the allocation to training or testing was random, it isn't a surprise. On page 9, line 3/4. They should specify how this allocation was done - hopefully at random. 5 Discussion, page 13, line 44-47, The mean hours quoted on these lines aren't the same as those reported in Fig 1a I believe, but in my copy the precision of the figures wasn't high enough for me to read the numbers clearly and I may be wrong about this. 6 I previously commented on the statement about "controlling" for physical activity level - in the regression models or CART or both, and if in CART how was the controlling done. They replied that since the regression models are removed my query no longer applied, and I assumed this to imply that the controlling had only been in the regression models. But in the Discussion, page 13, line 33, they say that all models were adjusted for physical activity levels. Perhaps in the statistical methods, page 8, lines 40-49, they could be more specific about the variables included that could split the data into groups, and others in the models that controlled.

REVIEWER	Richard Woodman Flinders University Australia
REVIEW RETURNED	13-Mar-2018

GENERAL COMMENTS	I am happy with the revisions made

VERSION 2 – AUTHOR RESPONSE

Reviewer 1

1. The authors did a great job revising the manuscript. They answered the questions appropriately, and adapted the manuscript accordingly.

Thank you.

2. Abstract: The first sentence in the conclusion starts with ‘Daily h/d’ – Maybe it would be better to say ‘Daily time’ to avoid repetition.

Done

3. Introduction: ‘While studies have generally shown older adults (>60 years) to be more sedentary than younger adults…'. This is indeed true, however, this was not examined in reference 9. I would suggest to refer to a review that includes different studies examining age as a correlate in order to strengthen this statement.

Thank you for pointing this out; we have replaced this citation with Matthews et al., 2008, and Bowman 2006 and O’Donoghue et al 2016.

Reviewer 2

1. Abstract: “hierarchical clusters of demographic factors” is confusing. The hierarchy of demographic factors is identified, right? Please rephrase the sentence.

This has been rephrased to “this study aimed to identify the hierarchy of demographic characteristics associated with the sedentary activities of television viewing, recreational computer use and driving.”

2. “Sex, …, as well as physical activity were the predictors” should be replaced by “considered as predictors”.

Done

3. Bullet-points of strengths and limitations: Please delete “Data-driven” in the second bullet-point.

Done

4. Introduction: Line 101 page 4: Please put into brackets e.g., 8 and place it before the end of the sentence or cite the author names instead of.

We placed the e.g., in brackets.

5. It would be more useful if the authors comment the results of the review of O’Donoghue instead of referring the reader to this review.

The literature summarized in the introduction is concordant with the main findings presented by O’Donoghue. To balance the need to be succinct and comprehensive in a manuscript introduction while also recognizing the complexity of the literature, we referred the reader to this fuller literature review. Since summarizing the findings of O’Donoghue would be redundant in our introduction, we have removed the sentence.

6. “capture what combination or clustering of demographic factors” is unclear because individuals are clustered according to the combination of demographic factors. Please rephrase.

This has been rephrased to: “may oversimplify or not fully consider the hierarchy of demographic factors that combine to present a high-risk profile for different forms of sedentary behaviors.” [Page 5, lines 158-160]

7. Please delete “data-driven”.

Done

8. “to identify individual and hierarchical clusters of demographic characteristics” is unclear and repeated several times throughout the manuscript. As mentioned above, rephrase.

Based on other studies that have used decision tree modeling to predict behavioral outcomes, we have rephrased this to: “to identify the hierarchy of demographic characteristics.”

9. Methods: While the period of data collection is important to mention, the period of analysis is useless. “Data analysis for the current study was conducted in 2018” can be removed.

Done

10. Please replace “METS” by “METs”.

Done

11. Statistical analysis: “502,243 participants” is mentioned. Isn’t “502,543” more accurate? Please check the numbers.

Thank you for pointing this out, the analytic sample is indeed 502,543.

12. The first paragraph is not really related to the statistical analysis but more to data management and study sample definition. Please rename this subsection or move this part in more appropriate (sub)-sections. Only the comparison study of excluded and included participations is relevant here and should be kept in the statistical analysis section!

We have renamed this section “Generation of Analytic Sample”

13. Please add the abbreviation “SD”.

Done (Line 254)

14. Results: Table 1. Precision of 2 decimals is not relevant, please round the percentages and check that all totals are equal to 100%.

As requested, percentages have been rounded to one decimal place.

14. The abbreviation METs should be defined in the footnote of the table.

Done

15. Line 228 page 11: Please delete “hours/day” in the subtitle.

Done

16. Modalities of variables seem to be combined (e.g. “no college” and “prefer not to answer”) – that is due to the CART algorithm that creates binary splits. This characteristic of CART needs to be presented when the approach is presented in the Methods section.

This is absolutely correct and, as requested, we have clarified this in the methods section to say “Specifically, the Classification and Regression Trees (CART) growing method was used for all decision tree analyses, which split the data into binary segments that were as homogeneous as possible with respect to the outcome until no predictors could improve the homogeneity of the nodes given a complexity parameter of 0.01 for all decision tree models.” (Page 8, lines 258-262)

17. Since all results are now from CART analysis, it is not necessary to specify "In the data-driven decision..." in all subsections

The words “data-driven” have been removed from the manuscript.

18. Discussion: “All models were adjusted for physical activity levels”. There is no adjustment for in CART analysis. Several factors are considered as potential predictors of the outcomes.

As indicated, this sentence has been removed.

19. Please change “total sedentary time” by “overall sedentary time” as made earlier in the text.

Thank you for pointing this out. This change has been made throughout the discussion.

20. “That adults who had not attended college and were retired were the highest risk group for television viewing by accruing a full 1.55 more hours per day of television time than those who went to college and were employed or unemployed (Figure 1b)”. Is the factor “1.55” accurate? Please check.

The factor has been corrected to 1.57. (Page 14, Line 608)

21. The R-squared coefficients seem low. Is that common in CART analysis? How the authors interpret that?

This feedback is consistent with Reviewer 3’s comment 4. As we responded there, R^2 values of $\leq$5% are quite common in behavioral health research. Moreover, the outcome – hours of sedentary behaviors per day – is a complex and multilevel determined construct. The current study is seeking to only identify the hierarchy of demographic characteristics that relate to the different sedentary behaviors, so under this premise, we would not necessarily expect to explain a substantial proportion of the variance in time spent in each of the sedentary behaviors. We have added a comment in the discussion section to reference the low R^2 seen in the study and how future studies that consider how the demographic hierarchies identified in this study interact with other social and environmental determinants of sedentary behavior.

22. What is the association between college attendance and employment status?

As one would expect, college attendance and employment status were significantly associated (p<.0001). Specifically, participants that were employed were significantly more likely to be college graduates vs not (70% vs. 58%); additionally, those that were retired were less likely to be college graduates vs not (27% vs 39%). Given that testing this association is not directly related to the research question, it is beyond the scope of this manuscript to include this information in the text.

23. How the authors interpret the difference in hierarchy of demographic factors found for each sedentary behaviour?

This is a great comment and certainly one of the more interesting findings of this study that have not been previously reported. We interpret our results showing a different hierarchy of demographic factors for the sedentary behaviors as evidence that sedentary behavior is not a homogenous term and that different sedentary behaviors may have different determinants. As such, in designing interventions to address sedentary behavior, programmers should consider what the determinants of the specific sedentary behavior are. [Page 16, lines 655-657]

24. Besides demographic factors, physical activity was also considered as a potential predictor but not retained in the trees, how do you interpret that?

We thank the reviewer for highlighting this interesting part of our results. We interpret the fact that physical activity did not emerge as one of the stronger correlates of any of the sedentary behaviors as another signal that physical activity and sedentary behavior are independent constructs. In response to the reviewer's comment, we have emphasized this interpretation in the revised manuscript. [Page 16, Lines 649-652]

Reviewer 3

1. Introduction , page 4, line 17, they mention more than 10 hours of riding in a car, on this line. Is this in a typical week?

We appreciate this comment and have clarified that that the unit of measurement is indeed hours of riding in the car per week.

2. When reading the Discussion I wasn't sure whether the hours of driving reported was just recreational or all driving. In the Abstract, page 2 line 22/3, they do say "recreational computer use and driving". In the methods, page 6, line 55, it is "using a computer for recreation, and, driving on a typical day". So trying to check this point I wasn't sure whether the adjective "recreational" applied just to computer use or to driving as well. This would be easily clarified by specifying "recreational driving " on page 6, line 55. In relation to my previous point, does "driving" include riding in a car as a passenger - as specified in the research reported in reference 6.

The UK biobank question for this item is as follows: "In a typical DAY, how many hours do you spend driving?" Thus, the question queries driving for any purpose – recreational or work related – and, it does not include time as a passenger in a car. To address this comment, we have specified in the methods that driving time is *all* driving time and as such includes driving for recreation and work (Page 6, Line 197), where possible, we have prefixed the work driving with the word "all," and we have noted in the discussion that time driving does not include time as a passenger and as such, is not a complete estimate of sitting for transportation time (Page 17, Lines 682-683).

3. Table 1, percentages are reported here to two decimal places, in the text referring to these results percentages are rounded to one decimal place. The table would be clearer with only 1 decimal places, in fact I thought not much would be lost by reporting all percentages with no decimal places.

Another reviewer also requested that the percentages be expressed to one decimal place. This has been done in the revised manuscript.

4. Reporting R-squared for training and testing samples, for example page 11, line 47/8. In all cases the R-squareds are low, and I believe this is interpreted as the percentage of variation in the hours in sedentary behaviour in question explained by the groups found. In this case only 5%. This should be mentioned in the Discussion. It is interesting that in all cases the R-squared is identical in training and testing sets, I wasn't anticipating that. But perhaps given the very large sample size and if the allocation to training or testing was random, it isn't a surprise. On page 9, line 3/4. They should specify how this allocation was done - hopefully at random.

The allocation to the training and testing data sets was indeed done by random assignment, this detail has been added to the methods section (Page 9, Line 255). The reviewer is indeed interpreting the R^2 correctly as the percentage of variance in the outcome explained by the predictors and covariates. It is important to remember that sedentary behavior is a complex behavior that is determined by multiple levels of predictors beyond demographic factors. The goal of this study was to respond to the American Heart Association call for greater clarity in the demographic profile for sedentary behavior. We would not expect just demographic factors to explain a large proportion of the variance in sedentary behavior. Moreover, R^2 values of $\leq$5% are common in behavioral research. In response to this reviewer comment, we note in the discussion that future studies that consider how demographic factors interact with social and environmental determinants of sedentary behavior are likely to explain a greater variance in sedentary behavior than the current study. (Page 17, line 686-688)

5. Discussion, page 13, line 44-47, The mean hours quoted on these lines aren't the same as those reported in Fig 1a I believe, but in my copy the precision of the figures wasn't high enough for me to read the numbers clearly and I may be wrong about this.

Thank you for pointing this out. These data have been corrected to be consistent with the figures.

6. I previously commented on the statement about "controlling" for physical activity level - in the regression models or CART or both, and if in CART how was the controlling done. They replied that since the regression models are removed my query no longer applied, and I assumed this to imply that the controlling had only been in the regression models. But in the Discussion, page 13, line 33, they say that all models were adjusted for physical activity levels. Perhaps in the statistical methods, page 8, lines 40-49, they could be more specific about the variables included that could split the data into groups, and others in the models that controlled.

We apologize for this confusion. As also requested by another reviewer, the statement about physical activity being controlled has been removed since the concept of controlling does not apply to CART modeling. As requested, a statement has been added to the methods section to state that separate decision tree models were generated for each study outcome, and that all study predictors and the physical activity variable was included in each model. (Page 8, line 256 – 258)

Reviewer 4

1. I am happy with the revisions made.

Thank you.

VERSION 3 – REVIEW

REVIEWER	Célina Roda ISGlobal, Spain
REVIEW RETURNED	16-Apr-2018

GENERAL COMMENTS	The manuscript has been improved since last version. Below are my minor comments: Table 1: please check the total of the percentages – should be equal to 100% (see the following variables: Race, Education, Season). Please round all percentages to 1 decimal (see “Shift-work” variable). Introduction / Methods Please add CART applications where the approach is presented in the Methods section and not at the end of the introduction. Methods – Generation of Analytic sample Add a line break after the subtitle Results – Figures The figures are blurred and I cannot check all numbers – please check them, and the quality of the Figures.

VERSION 3 – AUTHOR RESPONSE

Table 1: please check the total of the percentages – should be equal to 100% (see the following variables: Race, Education, Season). Please round all percentages to 1 decimal (see “Shift-work” variable).

The total of percentages do not always equal to 100% because of rounding. This has been noted at the table. The Shift-work variable has all percentages rounded to 1 decimal.

Introduction / Methods
Please add CART applications where the approach is presented in the Methods section and not at the end of the introduction.

This has been done.

Methods – Generation of Analytic sample
Add a line break after the subtitle

This has been done.

Results – Figures
The figures are blurred and I cannot check all numbers – please check them, and the quality of the Figures.

The clarity of the figures have been greatly enhanced and uploaded in both a MS Word and JPEG format.