PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	The relationship between bicycle commuting and perceived stress: a cross-sectional study
AUTHORS	Avila-Palencia, Ione; de Nazelle, Audrey; Cole-Hunter, Tom; Donaire-Gonzalez, David; Jerrett, Michael; Rodriguez, Daniel; Nieuwenhuijsen, Mark

VERSION 1 - REVIEW

REVIEWER	Klara Johansson Umeå University, Department of Public Health and Clinical Medicine, Unit of Epidemiology and Global Health
REVIEW RETURNED	01-Sep-2016

GENERAL COMMENTS	GENERAL COMMENTS: This is a well performed and ambitious study on an interesting and important topic, and addresses a question where we still lack knowlesge, and in a geographical setting where there is a lack of studies on active commuting. The article needs some further work however, mainly in clarifying to the reader what has been done and how. Many parts of the article are unclear, or clarification comes in the wrong place. I suggest the authors try to approach the article as if they were a reader, and try to understand what information the reader needs, and in what order, to understand and evaluate the article. Some of the confusions might be due to language mistakes. Some of the analyses are also unclear or not explained. I am very impressed by the wealth of environmental variables included in the study. They are very interesting, complex and relevant for the overall topic. However, it is not clear how they fit into the specific research question and the theorized association between bicycle commuting and stress. I can see that they might, and the authors give some hints, but it is not explained or discussed in full. Are they considered to be confounders? They might: for example people living for instance in greener areas might be more likely to cycle and also less stressed because there is green outside their home. But they are not discussed as potential confounders. Are they considered to be moderating variables? They might: people who live in greener areas might benefit more from cycling compared to cyclists in non-green areas - but interaction effects are not tested in these analyses, so authors probably don't consider them as moderating variables. Are they mediating variables? Well, no, because cycling does not make the area green. Could they be a way to increase cycling? Yes absolutely, but that is not a part of the research question here, and could be better addressed in another

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	paper. I suggest the authors either clarify the role of the environmental variables or else save them for a future paper. The sampling procedure needs to be more described (see detailed comment below). Step-by-step comments: ABSTRACT: My first question on reading the abstract was whether pedestrians were included in the non-bicycling category. In the manuscript itself, it is explained that they are excluded from analyses. This could be clarified in the abstract by inserting "(pedestrians excluded)". Introduction: The introduction is well written, but it could be better organised in order to lead up to the research question. The intro starts with physical activity, but PA is not the main focus of the article, only a co-variable. The main point of the article is bicycle commuting in relation to stress, so the intro should lead with either the potential positive effects of bicycle commuting or with the negative aspects of stress, and how active commuting could alleviate this. The importance of environmental aspects is mentioned, but not clarified how they fit into the theorised association here; this should be clarified in order to assess if the statistical models fit the question. Research question: In my opinion, the research question should be general, not specific. Thus, the geographic location should not be mentioned. That the study is done in Barcelona is an operationalisation, not a research question. Instead, it would be better to write something like "...in a dense urban setting" or something else that describes the Barcelona setting in a way that is relevant and generalizable to other similar settings. Methods: It is unclear how the participants were selected. The "locations" were randomly selected, but how were participants within locations selected and approached? And what represents a location? This is important to know in order for the reader to understand if the participants can be assumed to be independent of each other, or if they are clustered within locations, which in such case should be adjusted for, for instance by using multilevel models or robust confidence intervals. There is also no mention of the non-reponse rate. This information has to be included. NB: they authors refer to a previous publication for details. That is good, but the information I ask for above (selection of participants, non-response) needs to be included here, it is not enough to refer to previous publication. The inclusion of environmental variables is very impressive and represent a considerable work effort by the authors. They are very briefly described however, and it is difficult for a reader to assess what they represent. I understand this is a case of staying within word count, but if these variables could be explained in a more clear way, it would help the reader.

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	For "bicycle commuting propensity", readers are referred to a previous publication. But the variable has to be explained here. It can be explained more briefly than in the previous publication, with reference to the previous publication, but it is not ok to completely skip the explanation of a central variable that is not by itself intuitively understandable. The variable ">2 employed persons employed in household" is confusing. MORE than 2 employed people? In a nuclear family, there is often MAX two adults, so who else would be employed? Or should it be "at least 2 employed" instead of "more than 2 employed"? Or less than (which would be "<2 employed")? ANALYSES Due to lacking information in the methods, it's not possible to assess if the sample is clustered. If it is, this should be addressed either by using robust confidence intervals or mixed model (multilevel) analyses. It's great that the authors have tested assumptions of linearity! Many other papers miss this important step (including myself on occasion...) However, it seems to be misdirected here. Linearity should be tested for continuous variables, but the authors test linearity of "bicycle commuting" on "stress", even though the main analyses only use these variables as binary or categorical variables, in which case linearity is not assumed and does not need to be tested. The test of linearity reported here would make sense if the intention was to test the assumptions before performing a linear regression. But the GAM performed here, while interesting, seems to be a confusing side-track, but I could be mistaken and would be happy to be enlightened. (A linear regression would be doubtful anyway, since the stress index is a count variable and not a true continuous variable; poisson or ordinal regression could be other options; but since linear regression is pretty robust to violations of assumptions, and the GAM reported by the authors does indicate that the relationship could be linear, a linear regression could have been possible.) However, there are some continuous independent variables in the analyses, for instance minutes of PA, MVPA and VPA. The authors show that there is no association between these variables and stress, and thus correctly conclude that they cannot be mediators. But they enter them in the model as linear variables, but have not tested if the association with stress is linear. Here, a logistic GAM would make sense, to test if the relationship with minutes of PA, MVPA and PA on the log odds of being stressed increases equally for each minute, or if there is a non-linear relationship. Also age could be tested for linearity, and if the assocation is not linear, could be entered in analyses as age categories instead. GAM seems to be appropriate for testing for linearity from what I understand, though it should be written out in full as generalized additive models, not only the abbreviation. It could also be good to add a reference for the use of GAM to test linearity. If tests of linearity are performed in order to test if it is possible to enter variables as continuous variables in the analyses, the results of the test of linearity should be reported in the method section and not the results section, because it is the argument for why the main analyses were performed this way and not another way.

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	Authors have performed a lot of sensitivity analyses, which is good in that it strengthens the results. But it would be good to see some more theoretical resoning of why they have chosen the cut-offs and categorisations they have. RESULTS Good that the authors have excluded an extreme outlier, and mentioned it. It should be mentioned in Methods however, not in Results. Only the binary categorisation of "Stress" is presented in Table 1. It would be relevant to know more about the distribution of the stress scale. Since only 35% were categorised as stressed, even though the cut-off was above the median, indicates that a large number of respondents were actually ON the median. More information about the full scale makes it easier for the reader to assess the relevance of the cut-off chosen. The authors state that physical activity is not significantly related to the stress variable. This is interesting and unexpected. Since previous research indicates that physical activity does affect positively on stress, it could be worth doing some further analyses on this. Such as: testing the linearity of hypothesized relation, as indicated above; testing this relationship in some multivariate analyses to see if there is some negative confounding happening. Authors also state that because this association is not significant, mediation is not further explored. That makes sense, but, mediation IS in fact further explored in Table 3, at least a little bit, since MVPA is included as a control variable in some models. Since the OR:s change little with the inclusion of MVPA, it confirms their conclusion that MVPA is not a mediator, which is interesting (and unexpected) and could be discussed in the discussion. (It is currently mentioned, but not discussed.) Authors say they test interaction between bicycle commuting and sex on stress, and that this is non-significant; but no such test is presented. DISCUSSION At some point, it could be a good idea to mention in a short sentence that cyclists are vulnerable traffic users, at higher risk of traffic injury, and that public policy should endeavor to make cycling as safe as possible. The authors discuss the importance of environmental variables, but it is still unclear how the environmental variables fit into the research question here. I really like the environmental variables and I think they could be valuable contribution to the research, but the whole article is very unclear about how they fit into this specific research question. The authors mention the possibility of reverse causation but they don't make a note of what this would actually mean for their results. Some amount of reverse causation is highly plausible in this case, in that stressed persons might be less likely to bicycle. This is, in my view, strengthened by the result in Table 3, that non-bicycle commuters who want to bicycle are less stressed. This does not

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	detract from the findings here - the relationship between cycling and stress increases with more cycling for instance, indicating that at least part of the association found is in the direction theorized. I just want to see this discussed in the discussion in specific terms, not general. The authors also mention the possibility of self-report bias, but they don't discuss if such a bias might be systematic rather than just white noise. Table 1 The footnote is not understandable. Did the authors mean "except for" instead of "instead of"? * Reporting the IQR as one number instead of a range is confusing to me. Does it mean I as a reader should compute the plus/minus of half the number given, to get the 25th and 75th percentile? It would be more helpful to the reader to just write out the IQR as a range, for example age as median 36, IQR (29-43) Table 2 The effective sample size in this table is not mentioned. Each table needs to mention the n= It is unclear if this table is bivariate or if all variables are controlled for each other. Probably bivariate, but it is unclear. The environmental variables, it is unclear how they are modelled. Are they treated as continuous variables? Why does it say "median;IQR" in several places, but the table obviously reports OR:s and confidence intervals, is this leftover from copy+paste from Table 1 ? Table 3 This table is overall clear and well explained. It could be clarified that the variables in the table are not controlled for each other (though that is pretty clear, since they are all variations of the bicycle commuting variable, and the footnotes clarify which variables are controlled for, so this is just a suggestion and not essential). Minor: Language: Spelling is good, but there are several minor grammatical errors, in a few instances to the point of making sentences difficult to understand. What is "Bicing", is that the name of the bike-sharing system in Barcelona? There should be no hyphen in "statistically significant", it is not written *"statistically-signifiant".

REVIEWER	Masanori Ohta Department of Food and Health Sciences, International College of Arts and Sciences, Fukuoka Women's University JAPAN
REVIEW RETURNED	03-Sep-2016

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GENERAL COMMENTS	Comments for the authors
	The aim of the present study was to evaluate the relationship between commuting by bicycle in the working or studying adult population and perceived stress, using a cross-sectional study design. The authors demonstrate that stress reduction may be an important consequence of routine bicycle use. The authors' findings provide an important contribution to the field of public health and inform policy makers to consider the potential benefits of bicycle use. However, some major and minor issues need to be addressed. **Major comments** 1. In the Results section (page 9), there is no information regarding participant recruitment and response rate. Although the authors refer to the paper by Donaire-Gonzalez for the details of the study population, there is a discrepancy in the number of participants studied (present paper: N=788, Donaire-Gonzalez's paper: N=752). Please include a brief comment about recruitment in the Methods section and response rate in the Results section. 2. In Table 1, the data about "public bicycle stations around the home" are 4 (3) for non-bicycle commuters and 4 (3) for bicycle commuters. There is a significant difference between these two groups, although the median and IRQ are same. Please recheck these data. 3. In the Results section (page 10, line 14), the authors describe "higher levels of bikeability at home and work/study address compared with non-bicycle commuters", but there is no significant difference in "bikeability at home" according to Table 1. Please recheck the results. **Minor comments** 1. In the Methods section (pages 8, and 9), the abbreviations MEDEA and GAM should be defined at their first appearance. 2. In the Methods section (Other explanatory measures, page 8), NO2 should be expressed as NO_2. 3. Table S2 has not been cited in the text; please cite it at the appropriate location in the manuscript.

REVIEWER	Steven Lane Department of Biostatistics University of Liverpool UK
REVIEW RETURNED	05-Oct-2016

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GENERAL COMMENTS	Introduction DALY - could be included in full for non-health economist reader

REVIEWER	Hazel Inskip University of Southampton, UK I am an active bicycle commuter
REVIEW RETURNED	19-Oct-2016

GENERAL COMMENTS	This is an interesting paper, though the cross-sectional nature of it does make it difficult to interpret. The possibility of reverse causation is very strong but unmeasurable in a cross-sectional study. The authors note the reverse causation possibility briefly, but don't seem to consider it in their conclusions when they are making recommendations to decision makers. I think this needs more caution – it is too early on the basis of this cross-sectional study to make such recommendations. I have been asked to review the statistics and my comments are below, followed by some more general comments on the paper. The authors mention using a GAM. I assume this is a Generalised Additive Model, but this is never spelled out and it does seem rather a black box. I am not at all clear why they used this method nor what they were aiming to do in using it. This either needs more detail or needs to be dropped. The graph in the supplement needs explaining to a general reader. Without more explanation it is of little help to any reader, let alone someone who doesn't know what a GAM is. I was surprised that the authors used logistic regression for this analysis. In doing a GAM analysis they show that they are able to do complex statistical analyses, but an odds ratio is not a very helpful measure when the prevalence of the outcome is above about 10%. Here the outcome (stress) has a prevalence of about 36%. The authors correctly talk about elevated odds rather than elevated risk, but a prevalence rate ratio would be a much easier measure to interpret. The odds ratio is likely to be much further from one than a PRR, and gives a misleading impression of larger effect sizes than there actually are. For example, the unadjusted OR given in Table 3 is 0.61 but the actual PRR that can be derived from Table 1 is 0.72 – considerably closer to 1. I would suggest that a binary regression model would be much more appropriate than a logistic regression (or if the binary regression doesn't converge then a Poisson regression with robust variance can be used). Bicycling propensity needs explaining more clearly not just by giving a reference. The categories are given in table S2 but not in the text. Readers shouldn't have to refer to a reference to get the details. Lines 17-25 on page 13 don't make sense. What is the reference group? Maybe this sentence could be split into two? Nowadays, it is considered demeaning to describe research participants as 'subjects'. I suggest replacing the word 'subjects' with 'participants' throughout the paper. The English does need some improvement throughout the document. In particular, Table S1, needs considerable work. I

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	assume that the questions have been translated and that in the original they are correct – in English they make little sense now and need a better translation. All questions need revising but particularly the last two. I am very sympathetic to (and impressed by) people who write in a language that is not their mother tongue, but it appears from their names that at least two of the authors are likely to speak English as their first language so maybe they could sort this out.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name:
Klara Johansson
Institution and Country
Umeå University,
Department of Public Health and Clinical Medicine, Unit of Epidemiology and Global Health
Please state any competing interests or state 'None declared':
None declared.
Please leave your comments for the authors below

GENERAL COMMENTS:

This is a well performed and ambitious study on an interesting and important topic, and addresses a question where we still lack knowledge, and in a geographical setting where there is a lack of studies on active commuting.
The article needs some further work however, mainly in clarifying to the reader what has been done and how. Many parts of the article are unclear, or clarification comes in the wrong place. I suggest the authors try to approach the article as if they were a reader, and try to understand what information the reader needs, and in what order, to understand and evaluate the article. Some of the confusions might be due to language mistakes.
Some of the analyses are also unclear or not explained.
I am very impressed by the wealth of environmental variables included in the study. They are very interesting, complex and relevant for the overall topic. However, it is not clear how they fit into the specific research question and the theorized association between bicycle commuting and stress. I can see that they might, and the authors give some hints, but it is not explained or discussed in full. Are they considered to be confounders? They might: for example people living for instance in greener areas might be more likely to cycle and also less stressed because there is green outside their home. But they are not discussed as potential confounders. Are they considered to be moderating variables? They might: people who live in greener areas might benefit more from cycling compared to cyclists in non-green areas - but interaction effects are not tested in these analyses, so authors probably don't consider them as moderating variables. Are they mediating variables? Well, no, because cycling does not make the area green. Could they be a way to increase cycling? Yes absolutely, but that is not a part of the research question here, and could be better addressed in another paper. I suggest the authors either clarify the role of the environmental variables or else save them for a future paper.
The sampling procedure needs to be more described (see detailed comment below).

Authors' answer:
We thank the reviewer very much for these comments. We will address all of them below, in the 'Step-by-step comments'section.

Step-by-step comments:

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ABSTRACT:
Comment 1:
My first question on reading the abstract was whether pedestrians were included in the non-bicycling category. In the manuscript itself, it is explained that they are excluded from analyses. This could be clarified in the abstract by inserting "(pedestrians excluded)".

Authors' answer:
As the reviewer pointed out, the pedestrians were excluded in the recruitment. We added a sentence in the Methods section of the Abstract (page 3; line 11):
"A cross-sectional study was performed with 788 adults who regularly travelled to work or study locations in Barcelona, Spain, excluding those who only commuted on foot."

Introduction:

Comment 2:
The introduction is well written, but it could be better organised in order to lead up to the research question. The intro starts with physical activity, but PA is not the main focus of the article, only a co-variable. The main point of the article is bicycle commuting in relation to stress, so the intro should lead with either the potential positive effects of bicycle commuting or with the negative aspects of stress, and how active commuting could alleviate this.
The importance of environmental aspects is mentioned, but not clarified how they fit into the theorised association here; this should be clarified in order to assess if the statistical models fit the question.

Authors' answer:
We have made some modifications in the Introduction section such as adding more information on health benefits associated with bicycle commuting and the relevance of environmental determinants. Find the different additions below:
Page 6, lines 4-12:
"Walking and bicycling for transport is increasingly being promoted due to its potential for increasing physical activity (PA) levels in the general population(1–3). Active commuting – walking and bicycling for travel to and/or from work or educational addresses – has been associated with multiple health benefits from reductions to cardiovascular risk (4,5), lowering of body weight(2,5), improvement of fitness, reduced risk of diabetes (3), to higher levels of physical and mental well-being(6,7)."
Page 6, lines 18-20:
"Active commuting has been shown to have other societal benefits such as helping reduce air pollution, greenhouse gas emissions, and noise, and improving social interaction(10)."
Page 7, lines 3-6:
"Furthermore, others have suggested gender differences in stress-related variables. Women seem to be more physiologically reactive to social rejection challenges(13), are more likely to have daily stress, and be more impacted by life events(14)."
Page 7, lines 8-10:
"Some literature recognises commuting as a potential source of stress(15); however, active commuters have been shown to have higher levels of satisfaction, lower stress, higher relaxation and a heightened sense of freedom compared to car drivers.(16–18)."
Page 7, lines 20-25:
"Emerging literature has highlighted the relevance of positive natural and built environment to increase bicycle commuting and to improve mental health outcomes. Bicycle lane connectivity, bikeability, separation of bicycling from other traffic, high population density, short trip distance, proximity of a cycle path, green space and also walkability have been suggested as determinants of bicycling(20–24). Green space has also been associated with better self-perceived general health and better mental health(25,26)."

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Research question:

Comment 3:
In my opinion, the research question should be general, not specific. Thus, the geographic location should not be mentioned. That the study is done in Barcelona is an operationalisation, not a research question. Instead, it would be better to write something like "...in a dense urban setting" or something else that describes the Barcelona setting in a way that is relevant and generalizable to other similar settings.

Authors' answer:
Taking into account this suggestion, some modifications have been made in different sections of the manuscript:
Abstract (page 3: line 8):
"The current study evaluated the relationship between bicycle use for commuting among working or studying adults in a dense urban setting and perceived stress."
Introduction (page 8: lines 12-13, 15):
"Moreover, most studies of active commuting benefits on mental health have been conducted in North America or Northwest Europe, where the urban design tends to be less dense than many parts of the world(6,7,17,28–30). Consequently, a need exists to understand the relationship between bicycle commuting and perceived stress, particularly in dense urban environments."
Introduction (page 8: lines 19-20):
"The current study aimed to evaluate the relationship between bicycle commuting among the working or studying adult population and perceived stress in a dense urban setting."

Methods:

Comment 4:
It is unclear how the participants were selected. The "locations" were randomly selected, but how were participants within locations selected and approached? And what represents a location? This is important to know in order for the reader to understand if the participants can be assumed to be independent of each other, or if they are clustered within locations, which in such case should be adjusted for, for instance by using multilevel models or robust confidence intervals.
There is also no mention of the non-reponse rate. This information has to be included.
NB: they authors refer to a previous publication for details. That is good, but the information I ask for above (selection of participants, non-response) needs to be included here, it is not enough to refer to previous publication.

Authors' answer:
The word "locations" has been exchanged for the word "points" to make the explanation clearer. The random sampling points were used to recruit participants in the street, but the participants were not clustered within the random sampling points. More information has been added in the Materials and Methods section, Study population sub-section (page 9: lines 5-10, 14-18, 21-25):
"Participant recruitment was conducted by trained interviewers on the streets of Barcelona city between June 2011 and May 2012. To ensure adequate geographic coverage, a total of 40 random points (four random points within each of the ten city districts across Barcelona) were sampled. Adult bicycle commuters and non-bicycle commuters were asked in the street to answer a few screening questions, and those who fulfilled the inclusion criteria (being older than 18 years of age; living in Barcelona city since 2006 or earlier; working or going to school in Barcelona city; being healthy enough to ride a bicycle for 20 minutes; having a commute distance greater than a 10-minute walk; and using at least one mode of transport other than walking to commute) were invited to respond to a telephone survey. Bicycle commuters were oversampled to ensure enough bicycle commuters in the study. Those solely commuting on foot were excluded as the main interest was in the contrast

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between motorized modes (private and public transportation) and the bicycle. Of the 18469 participants approached across the forty sampling random points, 6701 agreed to answer screening questions. Of these, 1508 met the inclusion criteria, and 871 participants completed the survey. After survey responses were checked by the research team, 815 still fulfilled the inclusion criteria and 789 had geocodable home address."

Comment 5:
The inclusion of environmental variables is very impressive and represent a considerable work effort by the authors. They are very briefly described however, and it is difficult for a reader to assess what they represent. I understand this is a case of staying within word count, but if these variables could be explained in a more clear way, it would help the reader.

Authors' answer:
Due to space restrictions, we prefer to refer to the paper by Cole-Hunter et al 2015 where there is a more detailed description of the environmental variables. We have changed some parts of the text to make it more understandable and for some missing variables, we have added additional information in Materials and Methods section, 'Other explanatory measures' sub-section.
Page 13: lines 11-15, 25:
"The number of public bicycle stations within a 400m buffer surrounding home and work/study addresses was calculated based on information from the Ajuntament de Barcelona - Informació de Base i Cartografia (IBC) (Barcelona City Council – Basic information and mapping)."
Page 14:line 1:
"Commute distance did not use buffers and it was calculated in km following the street network of the shortest route from home address to work address."

Comment 6:
For "bicycle commuting propensity", readers are referred to a previous publication. But the variable has to be explained here. It can be explained more briefly than in the previous publication, with reference to the previous publication, but it is not ok to completely skip the explanation of a central variable that is not by itself intuitively understandable.

Authors' answer:
An explanation of bicycle commuting propensity has been included and some text modifications have been done in order to make 'Bicycle commuting' sub-section, in Materials and Methods section, more understandable:
Page 10, lines 21-25:
"Bicycle commuting levels classification was based on the days of bicycle commuting in the week prior to survey administration: "low" being three days or fewer, "medium" for four days, and "high" for five or more days."
Page 11, lines 7-19:
"Bicycle commuting propensity classification took into account both frequency and willingness to commute by bicycle:the "bicycle commuters" were further classified as "frequent" (four or more days) or "infrequent" (three or less days), and the "non-bicycle commuters" were classified as "willing" or "unwilling". The "willing" group were those "non-bicycle commuters" who indicated bicycling as "never or nearly never" their general transport mode, but who also indicated that they would consider bicycle commuting in Barcelona (they answered positively to "considering costs, travelling time, comfort and safety, how ready would you be to use the bicycle/Bicing (public bicycle-sharing system) for your trip to work or education centre?"). The "unwilling" group were those "non-bicycle commuters" who indicated "never or nearly never" bicycling for travel and indicated that they would not consider bicycle commuting in Barcelona by answering negatively to the above question. More details of the bicycle commuting propensity classification are given elsewhere(24)."

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Comment 7:
The variable ">2 employed persons employed in household" is confusing. MORE than 2 employed people? In a nuclear family, there is often MAX two adults, so who else would be employed? Or should it be "at least 2 employed" instead of "more than 2 employed"? Or less than (which would be "<2 employed")?

Authors' answer:
We appreciate this observation. As the reviewer correctly identified, the variable was not properly defined. It should be "at least 2 employed" as it is specified in the text. The tables 1, 2, S3, S4 have been modified with the following specification: "Employed people in household (2-5)".

ANALYSES

Comment 8:
Due to lacking information in the methods, it's not possible to assess if the sample is clustered. If it is, this should be addressed either by using robust confidence intervals or mixed model (multilevel) analyses.

Authors' answer:
As answered in Comment 4 (above) and now addressed with revised text in the manuscript, the sample was not clustered so the multilevel analysis was not needed.

Comment 9:
It's great that the authors have tested assumptions of linearity! Many other papers miss this important step (including myself on occasion...) However, it seems to be misdirected here. Linearity should be tested for continuous variables, but the authors test linearity of "bicycle commuting" on "stress", even though the main analyses only use these variables as binary or categorical variables, in which case linearity is not assumed and does not need to be tested. The test of linearity reported here would make sense if the intention was to test the assumptions before performing a linear regression. But the GAM performed here, while interesting, seems to be a confusing side-track, but I could be mistaken and would be happy to be enlightened. (A linear regression would be doubtful anyway, since the stress index is a count variable and not a true continuous variable; poisson or ordinal regression could be other options; but since linear regression is pretty robust to violations of assumptions, and the GAM reported by the authors does indicate that the relationship could be linear, a linear regression could have been possible.) However, there are some continuous independent variables in the analyses, for instance minutes of PA, MVPA and VPA. The authors show that there is no association between these variables and stress, and thus correctly conclude that they cannot be mediators. But they enter them in the model as linear variables, but have not tested if the association with stress is linear. Here, a logistic GAM would make sense, to test if the relationship with minutes of PA, MVPA and PA on the log odds of being stressed increases equally for each minute, or if there is a non-linear relationship. Also age could be tested for linearity, and if the association is not linear, could be entered in analyses as age categories instead.

Authors' answer:
Originally, the aim of performing the GAM graph was to show graphically the protective effect of bicycling commuting frequency (days/week) in perceived stress levels (score from 0 to 16). But from the moment we decided to categorize the variables, we agree with the reviewer that this can be very confusing. For that reason, we have decided to delete the GAM graph from the supplementary material and all its references in the text.
Regarding to the other continuous independent variables (Total PA, MVPA, VPA, and age), linearity was already tested, but not reported in the previous manuscript version. These tests are now reported in the new version, see modifications in Materials and Methods section, 'Statistical analyses' sub-

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section (page 14; lines 6-11):
“A Generalized Additive Model (GAM) was used to test linearity between perceived stress and total physical activity (Total PA), moderate-to-vigorous physical activity (MVPA), vigorous physical activity (VPA), and age(40). As there was no statistical evidence to reject linearity between perceived stress and Total PA (p-value = 0.3816), MVPA (p-value = 0.5025), VPA (p-value = 0.1630), and age (p-value = 0.2282), these variables were included as continuous variables in the model assuming a linear relationship.”

Comment 10:
GAM seems to be appropriate for testing for linearity from what I understand, though it should be written out in full as generalized additive models, not only the abbreviation. It could also be good to add a reference for the use of GAM to test linearity.

Authors’ answer:
The full name of GAM and a literature reference have been included in Materials and Methods section, ‘Statistical analyses’ sub-section (page 14, lines 6, 8):
“A Generalized Additive Model (GAM) was used to test linearity between perceived stress and total physical activity (Total PA), moderate-to-vigorous physical activity (MVPA), vigorous physical activity (VPA), and age(40).”

Comment 11:
If tests of linearity are performed in order to test if it is possible to enter variables as continuous variables in the analyses, the results of the test of linearity should be reported in the method section and not the results section, because it is the argument for why the main analyses were performed this way and not another way.

Authors’ answer:
The results of GAM used to test linearity between the different PA variables and age, and perceived stress have been included in Material and Methods section, ‘Statistical analyses’ sub-section (page 14; lines 8-11):
“As there was no statistical evidence to reject linearity between perceived stress and Total PA (p-value = 0.3816), MVPA (p-value = 0.5025), VPA (p-value = 0.1630), and age (p-value = 0.2282), these variables were included as continuous variables in the model assuming a linear relationship.”

Comment 12:
Authors have performed a lot of sensitivity analyses, which is good in that it strengthens the results. But it would be good to see some more theoretical reasoning of why they have chosen the cut-offs and categorisations they have.

Authors’ answer:
Regarding exposure variables, we created the variable bicycle commuting level as a proxy of bicycle commuting frequency. Some text has been added in Materials and methods section, ‘Bicycle commuting’ sub-section (page 10: lines 24-25):
“This measure could be interpreted as a proxy of bicycle commuting frequency.”

The bicycle commuting propensity was a variable already created in a previous paper that used the same database. We thought it would be interesting to include this variable because of its inclusion of willingness to cycle between the non-bicycle commuters, as it showed interesting results in relation of environmental determinants to bicycle commuting. Some text has been added in Materials and methods section, ‘Bicycle commuting’ sub-section (page 11: lines 19-20):
“This measure was included in the analysis to assess the effect of being willing to commute by bicycle in perceived stress.”

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Regarding outcome variables, perceived stress is a variable that doesn't have a defined cut-off. Our sample didn't have high levels of perceived stress and it was mainly distributed in the first five values (0-4) of the fifteen values present in our sample (0-14). We thought it would be easier to interpret with a cut-off using the median and then do sensitivity analyses with p75 and p90 to strength our results. Some text has been added in Materials and methods section, 'Perceived stress' sub-section (Page 12, lines 8-10, 12-15):
"The sample did not have high levels of perceived stress (Table S2); therefore, for an easier interpretation participants with a PSS-4 score higher than 3 (median of the total sample) were classified as "stressed", and those equal or lower than 3 were classified as "non-stressed". The sensitivity of our results to this choice was examined further in sensitivity analyses by classifying the respondents with PSS-4 scores in the 75th percentile (P75) and above (a score higher than 4) and in the 90th percentile (P90) and above (a score of 6 and above) as stressed and all others as non-stressed."

RESULTS

Comment 13:
Good that the authors have excluded an extreme outlier, and mentioned it. It should be mentioned in Methods however, not in Results.

Authors' answer:
The sentence has been moved to Materials and Methods section, 'Study population' sub-section (page 9: line 25; page 10: line 1).

Comment 14:
Only the binary categorisation of "Stress" is presented in Table 1. It would be relevant to know more about the distribution of the stress scale. Since only 35% were categorised as stressed, even though the cut-off was above the median, indicates that a large number of respondents were actually ON the median. More information about the full scale makes it easier for the reader to assess the relevance of the cut-off chosen.

Authors' answer:
We have given the distribution of stress in a new table (Table S2) in the Supplementary material (page 2).

Comment 15:
The authors state that physical activity is not significantly related to the stress variable. This is interesting and unexpected. Since previous research indicates that physical activity does affect positively on stress, it could be worth doing some further analyses on this. Such as: testing the linearity of hypothesized relation, as indicated above; testing this relationship in some multivariate analyses to see if there is some negative confounding happening.
Authors also state that because this association is not significant, mediation is not further explored. That makes sense, but, mediation IS in fact further explored in Table 3, at least a little bit, since MVPA is included as a control variable in some models. Since the OR:s change little with the inclusion of MVPA, it confirms their conclusion that MVPA is not a mediator, which is interesting (and unexpected) and could be discussed in the discussion. (It is currently mentioned, but not discussed.)

Authors' answer:
As explained in Comment 9, the linearity between the different PA variables and perceived stress was tested and reported in Materials and Methods section, 'Statistical analyses' sub-section (page 14; lines 6-11):

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“A Generalized Additive Model (GAM) was used to test linearity between perceived stress and total physical activity (Total PA), moderate-to-vigorous physical activity (MVPA), vigorous physical activity (VPA), and age(40). As there was no statistical evidence to reject linearity between perceived stress and Total PA (p-value = 0.3816), MVPA (p-value = 0.5025), VPA (p-value = 0.1630), and age (p-value = 0.2282), these variables were included as continuous variables in the model assuming a linear relationship.”

As the study of mediation was not the main aim of the paper and taking into account the non-statistically significant relationship between the two variables, we considered that further analyses were not needed and could be addressed in another paper. Some text has been added in the Discussion section, ‘Comparison with previous studies’ sub-section:
Page 23, lines 23-24; page 24, lines 1-2:
“These findings suggest that the physical activity gained during bicycle commuting(31) may act as a mediator in the relationship between bicycle commuting and perceived stress.”
Page 24, lines 5-7:
“Our sample was composed of young, healthy, and active participants with low levels of perceived stress, which might have led to an underestimation of the relationship between PA and perceived stress.“

Comment 16:
Authors say they test interaction between bicycle commuting and sex on stress and that this is non-significant; but no such test is presented.

Authors’ answer:
We would like to notify that we decided to change the concept of sex with gender, as we feel it is more proper taking into account the variables assessed. Regarding the interaction test results, this information has been added in the Results section (page 21: lines 6-10):
“In the fully adjusted models, we found no statistically significant interactions between gender and bicycle commuters (p-value= 0.165) between gender and bicycle commuting levels (p-value=0.226, p-value=0.266, p-value=0.431), or between gender and bicycle commuting propensity (p-value=0.982, p-value=0.197, p-value=0.277) (results not shown).”

DISCUSSION

Comment 17:
At some point, it could be a good idea to mention in a short sentence that cyclists are vulnerable traffic users, at higher risk of traffic injury, and that public policy should endeavor to make cycling as safe as possible.

Authors’ answer:
We also think injury risk is a very important topic to think about when talking about cycling policies. However, we preferred not to include any mention of it as we felt it was beyond the scope of our intentions.

Comment 18:
The authors discuss the importance of environmental variables, but it is still unclear how the environmental variables fit into the research question here. I really like the environmental variables and I think they could be valuable contribution to the research, but the whole article is very unclear about how they fit into this specific research question.

Authors’ answer:
We have added more information about the relevance of environmental determinants in the

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Introduction section (pages 7: lines 20-25):
“Emerging literature has highlighted the relevance of positive natural and built environment to increase bicycle commuting and to improve mental health outcomes. Bicycle lane connectivity, bikeability, separation of bicycling from other traffic, high population density, short trip distance, proximity of a cycle path, green space and also walkability have been suggested as determinants of bicycling(20–24). Green space has also been associated with better self-perceived general health and better mental health(25,26).”
And in the Discussion section, ‘Comparison with previous studies’ sub-section (page 24: lines 17-18):
“Therefore, it seems that perceptual and environmental factors related to bicycle commuting could affect perceived stress, in the way that more pleasant an environment to bicycle commute is, better perceived stress results we will get.”

Comment 19:
The authors mention the possibility of reverse causation but they don't make a note of what this would actually mean for their results. Some amount of reverse causation is highly plausible in this case, in that stressed persons might be less likely to bicycle. This is, in my view, strengthened by the result in Table 3, that non-bicycle commuters who want to bicycle are less stressed. This does not detract from the findings here - the relationship between cycling and stress increases with more cycling for instance, indicating that at least part of the association found is in the direction theorized. I just want to see this discussed in the discussion in specific terms, not general.

Authors’ answer:
A reasoning related to the possible reverse causality has been added in the Discussion section, ‘Limitations and strengths’ sub-section (page 21: line 25; page 25, lines: 8-11):
“It has been suggested that stressed people can engage in unhealthy behaviours, such as poor dietary practices or a lack of physical activity(46). This reasoning could be applied to a behaviour like bicycle commuting, where those individuals who are more stressed would bicycle less.”

Comment 20:
The authors also mention the possibility of self-report bias, but they don't discuss if such a bias might be systematic rather than just white noise.

Authors’ answer:
We think the misclassification error caused by self-reported data is random (white noise). This has been clarified in the Discussion section, ‘Limitations and strengths’ sub-section (page 25: lines 12-14):
“With the questionnaire data we could have random misclassification error of bicycle commuting and PA because of the data being self-reported. Therefore, the risk estimate and also the potential mediation by PA could be under-estimated(47).”

Table 1

Comment 21:
The footnote is not understandable. Did the authors mean "except for" instead of "instead of"?

Authors’ answer:
We appreciate this observation. The suggested modification has been made in the footnote of Table 1 (page 18)

Comment 22:
* Reporting the IQR as one number instead of a range is confusing to me. Does it mean I as a reader should compute the plus/minus of half the number given, to get the 25th and 75th percentile? It would be more helpful to the reader to just write out the IQR as a range, for example age as median 36, IQR

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(29-43)

Authors' answer:
Taking into account the suggestion, we have exchanged IQR for a range P25-P75 to make it clearer for the reader. Changes made in Tables 1 and S3, and in Results section (page 15, lines 4-5):
"The included sample had an equal distribution of genders and the median age (P25-P75) was 36 (29-43) years (Table 1)."

Table 2

Comment 23:
The effective sample size in this table is not mentioned. Each table needs to mention the n=

Authors' answer:
This table followed the complete case analysis approach followed in the models (Table 3); which means that it has the same sample size as the models (n=771). This information has been added in the footnote. The variables that have missing data and are not included in the models are also detailed in the footnote (page 19):
"Complete case analysis excluding missing data of the variables of final models (Table 3; n=771). The variables that still present missing data and are not included in the final models are: Total PA (5; 0.63%), People living with in household (1; 0.13%), Children in household (2; 0.25%), Children <3years old in household (3; 0.38), BMI (2; 0.25%); Stress releasing (15; 1.90%), Bicycle trip enjoyment (12; 1.52%), Commute distance (20; 2.54%), Greenness (20; 2.54%), NO2 (20; 2.54%)."

Comment 24:
It is unclear if this table is bivariate or if all variables are controlled for each other. Probably bivariate, but it is unclear.

Authors' answer:
Clarification added in the title of Tables 1, 2, and 3:
Title Table 1 (page 17):
"Descriptive analyses of perceived stress and determinants of participants and according to bicycle commuting status."
Title of Table 2 (page 19):
"Bivariate analyses showing the relationships between perceived stress (median) and determinants of participants."
Title of Table 3 (page 22):
"Multivariate models showing the relationships between bicycle commuting and perceived stress (median) of participants."

Comment 25:
The environmental variables, it is unclear how they are modelled. Are they treated as continuous variables? Why does it say "median;IQR" in several places, but the table obviously reports OR:s and confidence intervals, is this leftover from copy+paste from Table 1 ?

Authors' answer:
All the environmental determinants were treated as continuous variables. The "median;IQR" and other misleading details have been removed from Table 2 (page 19).

Table 3

Comment 26:

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This table is overall clear and well explained.
It could be clarified that the variables in the table are not controlled for each other (though that is pretty clear, since they are all variations of the bicycle commuting variable, and the footnotes clarify which variables are controlled for, so this is just a suggestion and not essential).

Authors' answer:
We thank the reviewer for this suggestion, however have considered that it was not necessary to clarify that the variables in the table are not controlled for each other.

Minor:
Comment 27:
Language: Spelling is good, but there are several minor grammatical errors, in a few instances to the point of making sentences difficult to understand.

Authors' answer:
The co-authors of this paper who are native English speakers have made further revisions throughout the manuscript to make the text more understandable.

Comment 28:
What is "Bicing", is that the name of the bike-sharing system in Barcelona?

Authors' answer:
"Bicing" is the bike-sharing system in Barcelona city. Clarification is added in Materials and Methods section, 'Bicycle commuting' sub-section (page 11, line 14).
"The "willing" group were those "non-bicycle commuters" who indicated bicycling as "never or nearly never" their general transport mode, but who also indicated that they would consider bicycle commuting in Barcelona (they answered positively to "considering costs, travelling time, comfort and safety, how ready would you be to use the bicycle/Bicing (public bicycle-sharing system) for your trip to work or education centre?")."

Comment 29:
There should be no hyphen in "statistically significant", it is not written *"statistically-signifiant".

Authors' answer:
The hyphen in "statistically-significant" has been removed throughout the manuscript.

Reviewer: 2
Reviewer Name
Masanori Ohta
Institution and Country
Department of Food and Health Sciences
International College of Arts and Sciences,
Fukuoka Women's University
JAPAN
Please state any competing interests or state 'None declared':
None declared.

Please leave your comments for the authors below

Comments for the authors:

The aim of the present study was to evaluate the relationship between commuting by bicycle in the

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working or studying adult population and perceived stress, using a cross-sectional study design. The authors demonstrate that stress reduction may be an important consequence of routine bicycle use. The authors' findings provide an important contribution to the field of public health and inform policy makers to consider the potential benefits of bicycle use. However, some major and minor issues need to be addressed.

Authors' answer:
We thank the reviewer for this comment.

Major comments

Comment 1:
In the Results section (page 9), there is no information regarding participant recruitment and response rate. Although the authors refer to the paper by Donaire-Gonzalez for the details of the study population, there is a discrepancy in the number of participants studied (present paper: N=788, Donaire-Gonzalez's paper: N=752). Please include a brief comment about recruitment in the Methods section and response rate in the Results section.

Authors' answer:
Clarifications about the recruitment and the response rate have been included in the Materials and Methods section, Study population sub-section (page 9: lines 5-10, 14-18, 21-25):
"Participant recruitment was conducted by trained interviewers on the streets of Barcelona city between June 2011 and May 2012. To ensure adequate geographic coverage, a total of 40 random points (four random points within each of the ten city districts across Barcelona) were sampled. Adult bicycle commuters and non-bicycle commuters were asked in the street to answer a few screening questions, and those who fulfilled the inclusion criteria (being older than 18 years of age; living in Barcelona city since 2006 or earlier; working or going to school in Barcelona city; being healthy enough to ride a bicycle for 20 minutes; having a commute distance greater than a 10-minute walk; and using at least one mode of transport other than walking to commute) were invited to respond to a telephone survey. Bicycle commuters were oversampled to ensure enough bicycle commuters in the study. Those solely commuting on foot were excluded as the main interest was in the contrast between motorized modes (private and public transportation) and the bicycle. Of the 18469 participants approached across the forty sampling random points, 6701 agreed to answer screening questions. Of these, 1508 met the inclusion criteria, and 871 participants completed the survey. After survey responses were checked by the research team, 815 still fulfilled the inclusion criteria and 789 had geocodable home address."

Comment 2:
In Table 1, the data about "public bicycle stations around the home" are 4 (3) for non-bicycle commuters and 4 (3) for bicycle commuters. There is a significant difference between these two groups, although the median and IRQ are same. Please recheck these data.

Authors' answer:
Originally the data of Environmental determinants was presented using median and IQR as they were not of a normal distribution. In the new version the data are presented in mean and standard deviation to better show the differences and to be also consistent with Cole-Hunter et al, 2015 (Table 1, page 17).

Comment 3:
In the Results section (page 10, line 14), the authors describe "higher levels of bikeability at home and work/study address compared with non-bicycle commuters", but there is no significant difference in "bikeability at home" according to Table 1. Please recheck the results.

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Authors' answer:
The results were rechecked and an error in the data of Table 1 mentioned was detected and changed (Table 1, page 17).

Minor comments

Comment 4:
In the Methods section (pages 8, and 9), the abbreviations MEDEA and GAM should be defined at their first appearance.

Authors' answer:
The abbreviation MEDEA has been defined in Materials and Methods section, 'Other explanatory measures' sub-section (page 13: lines 1-3):

"In addition, the MEDEA Index (Mortalidad en áreas pequeñas Españolas y Desigualdades socioEconómicas y Ambientales, in Spanish; Environmental and socioEconomic Inequalities in Mortality in small Spanish areas, translated to English) was used as an area deprivation indicator assigned to each participants' address."

The abbreviation GAM has been defined in Materials and Methods section, 'Statistical analyses' sub-section (page 14: line 6):
"A Generalized Additive Model (GAM) was used to test linearity between perceived stress and total physical activity (Total PA), moderate-to-vigorous physical activity (MVPA), vigorous physical activity (VPA), and age(40)."

Comment 5:
In the Methods section (Other explanatory measures, page 8), NO2 should be expressed as NO2.

Authors' answer:
The suggested modification has been made in Materials and Methods section, 'Other explanatory measures' sub-section (page 13: line 17).

Comment 6:

Table S2 has not been cited in the text; please cite it at the appropriate location in the manuscript.

Authors' answer:
With the addition of a new table (Table S2) in the Supplementary material, the former Table S2 is called Table S3 in the new version of the manuscript. Some text has been added in order to cite the current Table S3 in Results section.
Page 15: lines 19-23:
"Bicycle commuters had shorter commutes compared to non-bicycle commuters, and we observed a gradient between commute distance and bicycle commuting levels with shorter distances for those who cycled more frequently. This tendency was also followed by bicycle commuting propensity, with decreasing commute distance from unwilling to bicycle to frequent bicyclists (Table S3)."
Page 16: lines 1-3:
"These environmental determinants stayed statistically significant for bicycle commuting propensity, but not between bicycle commuting levels (Table S3)."

Reviewer: 3
Reviewer Name

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Steven Lane
Institution and Country
Department of Biostatistics
University of Liverpool
UK
Please state any competing interests or state ‘None declared’:
None declared

Please leave your comments for the authors below

Introduction
Comment 1:
DALY - could be included in full for non-health economist reader

Authors’ answer:
We thank the reviewer for this comment. We deleted the text in the Introduction section that included DALY, so no modifications related to that comment have been made in the manuscript.

Reviewer: 4
Reviewer Name
Hazel Inskip
Institution and Country
University of Southampton, UK
Please state any competing interests or state ‘None declared’:
I am an active bicycle commuter

Please leave your comments for the authors below

General Comment:
This is an interesting paper, though the cross-sectional nature of it does make it difficult to interpret. The possibility of reverse causation is very strong but unmeasurable in a cross-sectional study. The authors note the reverse causation possibility briefly, but don’t seem to consider it in their conclusions when they are making recommendations to decision makers. I think this needs more caution – it is too early on the basis of this cross-sectional study to make such recommendations.

I have been asked to review the statistics and my comments are below, followed by some more general comments on the paper.

Authors’ answer:
We thank the reviewer for this general comment. Regarding making recommendations, we agree that this needs more caution taking into account the nature of the study design. Therefore, we have rewritten the text related to recommendations to decision makers in the Abstract section (page 4: line 2-3):
“Stress reduction may be an important consequence of routine bicycle use and should be considered by decision makers as another potential benefit of its promotion.”

And we have added and rewritten some text in the Conclusions section (page 26: lines 17-20):
“Further research is needed in order to disentangle the relationship between bicycle commuting and perceived stress, and its determinants (individual and environmental) and potential mediators. Our findings suggest that decision-makers may promote bicycle commuting as a daily routine, to reduce stress levels and improve public health and well-being.”

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All other comments are addressed below.

Comment 1:
The authors mention using a GAM. I assume this is a Generalised Additive Model, but this is never spelled out and it does seem rather a black box. I am not at all clear why they used this method nor what they were aiming to do in using it. This either needs more detail or needs to be dropped. The graph in the supplement needs explaining to a general reader. Without more explanation it is of little help to any reader, let alone someone who doesn't know what a GAM is.

Authors' answer:
The abbreviation GAM has been defined in Materials and Methods section, 'Statistical analyses' sub-section (page 14: line 6):
"A Generalized Additive Model (GAM) was used to test linearity between perceived stress and total physical activity (Total PA), moderate-to-vigorous physical activity (MVPA), vigorous physical activity (VPA), and age(40)."

Regarding the GAM graph, originally, the aim of performing the GAM graph was to show graphically the protective effect of bicycling commuting frequency (days/week) in perceived stress levels (score from 0 to 16). But from the moment we decided to categorize the variables, we agree with the reviewer that this can be very confusing. For that reason, we have decided to delete the GAM graph from the supplementary material and all its references in the text.

Comment 2:
I was surprised that the authors used logistic regression for this analysis. In doing a GAM analysis they show that they are able to do complex statistical analyses, but an odds ratio is not a very helpful measure when the prevalence of the outcome is above about 10%. Here the outcome (stress) has a prevalence of about 36%. The authors correctly talk about elevated odds rather than elevated risk, but a prevalence rate ratio would be a much easier measure to interpret. The odds ratio is likely to be much further from one than a PRR, and gives a misleading impression of larger effect sizes than there actually are. For example, the unadjusted OR given in Table 3 is 0.61 but the actual PRR that can be derived from Table 1 is 0.72 – considerably closer to 1. I would suggest that a binary regression model would be much more appropriate than a logistic regression (or if the binary regression doesn't converge then a Poisson regression with robust variance can be used).

Authors' answer:
We appreciate this comment very much. The bivariate analyses looking at the relationships between perceived stress and determinants, and the multivariate models have been re-performed using Poisson regression with robust variance models. Thus, text modifications have been made in:
Methods and Results section of the Abstract (page 3: lines: 15, 18-21):
"Multivariate Poisson regression with robust variance models of stress status based on bicycling exposure, adjusting for potential confounders, were estimated.
Results: Bicycle commuters had significantly lower risk of being stressed [RR (95%CI) = 0.73 (0.60, 0.89)]. Bicycle commuters who bicycled four or more days per week had lower risk of being stressed than those who bicycled less than four days."

Materials and Methods section, Statistical analyses sub-section:
Page 14, lines 12, 15-16:
"Multivariate Poisson regression with robust variance models were used to assess the relationship between bicycle commuting and perceived stress. Possible mediation by different levels of PA between bicycle commuting and perceived stress, and any interaction between gender and bicycle commuting were also tested with Poisson regression with robust variance models."

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Page 14, lines 19-22:
“The first descriptive statistical analyses were conducted in Stata version SE 12 (StataCorp LP, Texas USA), while Poisson regression with robust variance models were conducted in Stata version SE 14 (StataCorp LP, Texas USA).”

Results section:
Page 16, line 13:
“The possible mediation of PA was not further explored as there was no statistically significant relationship between levels of PA (Total PA, MVPA and VPA) and perceived stress [RR: 1.00; 95% CI: (0.99, 1.00)] for the three different classifications of perceived stress (P50, P75, P90) (Table 2, Table S4).”
Page 20, lines 3, 5-6:
“Multivariate Poisson regression with robust variance analyses showed a statistically significant inverse relationship between bicycle commuting and perceived stress. Bicycle commuters had a lower risk of being stressed compared to non-bicycle commuters [RR (95%CI) = 0.73 (0.60, 0.89)].”
Page 20, lines 11-13:
“There was a statistically significant inverse relationship between medium and high levels of bicycle commuting and perceived stress using non-bicycle commuters as a reference group [RR (95%CI) = 0.46 (0.28, 0.78); RR (95%CI) = 0.63 (0.49, 0.81)] and also when using low levels of bicycle commuting [RR (95%CI) = 0.42 (0.24, 0.73); RR (95%CI) = 0.57 (0.42, 0.77)] as a reference group.”
Page 20, lines 19-20:
“Regarding bicycle commuting propensity, there was a statistically significant inverse relationship between frequent bicycle commuters and perceived stress, using unwilling non-bicycle commuters [RR (95%CI) = 0.53 (0.41, 0.67)] and infrequent bicycle commuters [RR (95%CI) = 0.54 (0.40, 0.72)] as respective reference groups.”
Page 20, line 25; page 21, lines 1-2:
“Also, there was a statistically significant inverse relationship between willing non-bicycle commuters and perceived stress, using unwilling non-bicycle commuters [RR (95%CI) = 0.72 (0.56, 0.94)] as a reference group in the bicycle commuting propensity variable and also looking only in the non-bicycle commuting group.”

Tables 2, 3, S4, and S5 have also been modified according to this methodological change.

Comment 3:
Bicycling propensity needs explaining more clearly not just by giving a reference. The categories are given in table S2 but not in the text. Readers shouldn’t have to refer to a reference to get the details.

Authors‘ answer:
An explanation of bicycle commuting propensity has been included and some text modifications have been done in order to make ‘Bicycle commuting’ sub-section, in Materials and Methods section, more understandable:
Page 10, lines 21-25:
“Bicycle commuting levels classification was based on the days of bicycle commuting in the week prior to survey administration: “low” being three days or fewer, “medium” for four days, and “high” for five or more days.”
Page 11, lines 7-19:
“Bicycle commuting propensity classification took into account both frequency and willingness to commute by bicycle:the “bicycle commuters” were further classified as “frequent” (four or more days) or “infrequent” (three or less days), and the “non-bicycle commuters” were classified as “willing” or “unwilling”. The “willing” group were those “non-bicycle commuters” who indicated bicycling as “never or nearly never” their general transport mode, but who also indicated that they would consider bicycle commuting in Barcelona (they answered positively to “considering costs, travelling time, comfort and

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safety, how ready would you be to use the bicycle/Bicing (public bicycle-sharing system) for your trip to work or education centre?"). The "unwilling" group were those "non-bicycle commuters" who indicated "never or nearly never" bicycling for travel and indicated that they would not consider bicycle commuting in Barcelona by answering negatively to the above question. More details of the bicycle commuting propensity classification are given elsewhere(24)."

Comment 4:
Lines 17-25 on page 13 don't make sense. What is the reference group? Maybe this sentence could be split into two?

Authors' answer:
Text modifications have been made to make this clearer in Results section (page 20: lines 11-14).
"There was a statistically significant inverse relationship between medium and high levels of bicycle commuting and perceived stress using non-bicycle commuters as a reference group [RR (95%CI) = 0.46 (0.28, 0.78); RR (95%CI) = 0.63 (0.49, 0.81)] and also when using low levels of bicycle commuting [RR (95%CI) = 0.42 (0.24, 0.73); RR (95%CI) = 0.57 (0.42, 0.77)] as a reference group.."

Comment 5:
Nowadays, it is considered demeaning to describe research participants as 'subjects'. I suggest replacing the word 'subjects' with 'participants' throughout the paper.

Authors' answer:
The term "subjects" has been replaced for "participants" throughout the manuscript.

Comment 6:
The English does need some improvement throughout the document. In particular, Table S1, needs considerable work. I assume that the questions have been translated and that in the original they are correct – in English they make little sense now and need a better translation. All questions need revising but particularly the last two. I am very sympathetic to (and impressed by) people who write in a language that is not their mother tongue, but it appears from their names that at least two of the authors are likely to speak English as their first language so maybe they could sort this out.

Authors' answer:
The Table S1 has been reviewed checking the original source of the questions (PSS4) and TAPAS Travel Survey (administered in Spanish). The proper changes have been done in the questions in Supplementary materials (Table S1, page 1):
"In the last month, how often have you felt that you were unable to control important things in your life?
In the last month, how often have you felt confident about your ability to handle your personal problems?
In the last month, how often have you felt that things were going your way?
In the last month, how often have you felt that difficulties were piling up so high that you could not overcome them?"

and in some of the answer options in Supplementary materials (Table S1, page 1):
"Almost never"
"Nearly always"
"Always"

The co-authors of this paper who are English speakers have further reviewed it and now think that the text is more understandable.

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VERSION 2 – REVIEW

REVIEWER	Klara Johansson Umå University, Department of Public Health and Clinical Medicine, Sweden I am an active bicycle commuter.
REVIEW RETURNED	26-Jan-2017

GENERAL COMMENTS	I am happy with how my comments were addressed, and congratulate the authors on good work.

REVIEWER	Masanori Ohta Department of Food and Health Sciences International College of Arts and Sciences Fukuoka Women's University
REVIEW RETURNED	19-Jan-2017

GENERAL COMMENTS	The revised manuscript is much improved and acceptable. The reviewer has a minor comment. (Page 15 of 71, line 11) The end of sentence "~ the majority of sensitivity analyses" needs a period.

REVIEWER	Hazel Inskip University of Southampton, UK None declared (though as a 5-day a week bicycling commuter who came off the bike this morning in icy weather, and am rather bruised, I'm feeling that cycling has led to rather high stress today).
REVIEW RETURNED	25-Jan-2017

GENERAL COMMENTS	The authors have addressed the issues I raised in relation to the previous version and I have no further comments.

VERSION 2 – AUTHOR RESPONSE

Reviewer: 2
Reviewer Name
Masanori Ohta
Institution and Country
Department of Food and Health Sciences
International College of Arts and Sciences
Fukuoka Women's University

Please state any competing interests or state 'None declared':
None declared

Please leave your comments for the authors below
The revised manuscript is much improved and acceptable.
The reviewer has a minor comment.

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(Page 15 of 71, line 11)
The end of sentence "~ the majority of sensitivity analyses" needs a period.

Authors' answer: We thank the reviewer for this comment. As suggested, we wrote a period at the end of the highlighted sentence. We also reviewed all text again and corrected some other punctuation marks.

Reviewer: 4
Reviewer Name
Hazel Inskip
Institution and Country
University of Southampton, UK

Please state any competing interests or state 'None declared':
None declared (though as a 5-day a week bicycling commuter who came off the bike this morning in icy weather, and am rather bruised, I'm feeling that cycling has led to rather high stress today).

Please leave your comments for the authors below
The authors have addressed the issues I raised in relation to the previous version and I have no further comments.

Authors' answer: We thank the reviewer for this comment and we are glad the issues were addressed properly.

Reviewer: 1

Reviewer Name
Klara Johansson

Institution and Country
Umå University, Department of Public Health and Clinical Medicine, Sweden

Please state any competing interests or state 'None declared':
I am an active bicycle commuter.

Please leave your comments for the authors below
I am happy with how my comments were addressed, and congratulate the authors on good work.

Authors' answer: We thank the reviewer for this comment. We are also happy that her comments were addressed properly.

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