

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Dog ownership, the natural outdoor environment and health: a cross-sectional study
<b>AUTHORS</b>	Zijlema, Wilma; Christian, Hayley; Triguero-Mas, Margarita; Cirach, Marta; van den Berg, Magdalena; Maas, Jolanda; Gidlow, Christopher; Kruize, Hanneke; Wendel-Vos, Wanda; Andrusaitytė, Sandra; Grazuleviciene, Regina; Litt, Jill; Nieuwenhuijsen, Mark

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Elizabeth A. Richards Purdue University, School of Nursing, USA
<b>REVIEW RETURNED</b>	10-May-2018

<b>GENERAL COMMENTS</b>	<p>This paper is well-written and has the potential to significantly contribute to the literature.</p> <p>Introduction: This section has logical flow and good use of pertinent references.</p> <p>1) However, I feel you are missing some more current or timely references to dog walking and physical activity impact. Please see:</p> <ul style="list-style-type: none"><li>• Christian, H., Bauman, A., Epping, J., Levine, G., McCormack, G., Rhodes, R., Richards, E., Rock, M., &amp; Westgarth, C. (2016). State of the art review: Encouraging dog walking for health promotion and disease prevention. <i>American Journal of Lifestyle Medicine</i>. Advanced online publication.</li><li>• Richards, E., Ogata, N., &amp; Cheng, C. (2016). Randomized, controlled e-mail based walking intervention: Differences between dog owners and non-dog owners. <i>Clinical Nursing Research</i>. 65(3), 191-201. doi: 10.1097/NNR.000000000000155</li><li>• Richards, E., Ogata, N., &amp; Cheng, C. (2016). Evaluation of the Dogs, Physical Activity, and Walking (Dogs PAW) intervention: A randomized controlled trial. <i>Nursing Research</i>. 65(3), 191-201. doi: 10.1097/NNR.000000000000155</li><li>• Richards, E. (2016). Does dog walking predict physical activity participation: Results from a national survey. <i>American Journal of Health Promotion</i>, 30(5), 323-330.</li><li>• Richards, E., McDonough, M., Edwards, N., Lyle, R., Troped, P. (2013). Psychosocial and environmental factors associated with dog walking. <i>International Journal of Health Promotion and Education</i>, 51(4), 198-211.</li></ul> <p>2) In addition, the description or use of theory is lacking. Is there a health behavior theory that could support your hypothesis?</p> <p>Design and Methods</p> <ol style="list-style-type: none"><li>1) Recruitment of participants should include more detail</li><li>2) How was green and blue space defined?</li></ol>
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	<p>3) Adding a description of data screening is also important- for example normality? Missing data? Results- this section is clearly written- well done. 1) How did your sample rates of dog ownership compare to population level estimates of dog ownership? Discussion section is well written but I feel the concluding statement could be stronger. In addition, more could be stated about next steps/areas for future research/ and implications of findings.</p>
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<b>REVIEWER</b>	Sophie Hall University of Lincoln, UK
<b>REVIEW RETURNED</b>	25-Jun-2018

<b>GENERAL COMMENTS</b>	<p>Overall a well written article which should add to this growing field. Specific comments include:</p> <p>Abstract – P3 Line 8: Unclear what is meant by ‘additional’ health benefits Line 29: Should be including not included</p> <p>Introduction – P6 Line 18 – this is not necessarily true, only some research has shown this. Line 33-34 – doesn’t read well – also many dog walkers do not have access to parks In general there could be a greater consideration of the dog walking literatures. It is also important to consider the literatures which do not show positive influences of dog ownership on walking and subsequent health outcomes.</p> <p>Method – P7 Line 15: How can it be 30-35 adults were invited, what is the exact number? Is this a typo? Line 15: 18-75 years?? P8 Line 41: Spell out abbreviations</p> <p>Results – P11 Line 18: Was should be were It should be emphasised that these were ‘self-reported’ measures e.g. line 47: There no significant differences in self-reported general health status.....</p> <p>Discussion – P13 Line 31: This needs expanding upon in the results, did you check the demographics of these samples to see if the Stoke participants were under/over represented on a specific factor? Same with regards to the Barcelona sample. P14 Line 15: State whether these were also self-report measures. For instance, participants in your sample may have had lower blood pressure if they were dog owners and walked a lot, but they would not necessarily be aware of this. Line 30: Limitations also include lack of detail about the dog, breed, age, health size, temperament. If the dog is anxious or aggressive when out walking this may inhibit potential mental health benefits associated with stress reduction etc.</p>
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<b>REVIEWER</b>	Dr. Timo-Kolja Pfortner IMVR, University of Cologne, Germany
<b>REVIEW RETURNED</b>	29-Jul-2018

<b>GENERAL COMMENTS</b>	<p>I would like to thank the authors for this very interesting manuscript that was quite challenging for me as I'm not an expert in this field of research, in particular with regard to the usage of GEO-data. However, I have some expertise in the application of interaction terms. I think what your manuscript needs is a bit more explanations on your statistical strategy and the presentation of results. I really had some difficulties to understand the results tables and the statistical strategy of modeling. Below you will find my comments that might improve your manuscript.</p> <p>Page 6, line 50/51: rather high access to NOE than high exposure to NOE. You mixed these terms in the manuscript</p> <p>Page 7, line 14/15: Please elaborate how you receive 1000 respondents per city when a random sample of 30-35 adults was drawn?</p> <p>Page 10, line 6-12: Using different indicators of neighborhood-SES is difficult as you cannot say, whether differences and associations of this indicator is based on differences in terms of the SES or in terms of the indicator in use.</p> <p>Page 10: When reading your statistical analysis section, I was a little bit confused about the relevant independent and dependent variables. Please differentiate in the section before between dependent and independent variables. Moreover, the application of interaction terms are for non-methodologist to some extent difficult. Therefore, I would recommend including a figure about the assumed pathways and interactions.</p> <p>Page 10: What is meant by varying exposure to NOE, and what is the rationale of point three. Please elaborate your idea behind this statistical strategy of stratifying the analysis.</p> <p>Page 11, line 42: Why is it <math>\geq 121</math> minutes per week walking, when you equalized this measure to the city-specific mean as mentioned in the method section.</p> <p>Page 12, line 12: To help readers understanding interaction terms, I would try to write the sentence like: ... the association between dog ownership and leisure time walking by number of NOE...</p> <p>Page 12: Table 3 and the explanation of interaction terms are difficult to understand. For interactions terms it is relevant to know the main effect of dog ownership and the outcome variable, and the interaction term in one table. Therefore, I would recommend using more tables for the presentation of interaction terms of the i.e. 300m variable and the lowest and highest variables. As said, it is not clear whether you have estimated all these interaction terms in one model simultaneously or not. Therefore, it would be better to differentiate the presentation of model results by tables or to explain your strategy more carefully in the method section. Moreover, for example, on page 12, line 33, you present the OR of 4.31. Is this the main effect of being a dog owner and having NOE in a 1000m buffer, or is this the interaction term between dog ownership and having a NOE in a 1000m buffer. If the latter is true, your presentation of results (dog ownership + having a NOE in a 1000m buffer vs. non dog owners) is false as the interaction term specified the difference in the association of dog ownership with time spent in NOE by levels of having a NOE. However, it is possible to estimate the main effect, but, therefore, you need to</p>
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	<p>sum the effect of the main effect (dog ownership) and the interaction effect (having a NOW in a 1000m buffer).</p> <p>I would also recommend presenting all full results in a web appendix.</p> <p>If you like to compare models with different outcome variables, it is necessary that the number of observations are the same between different models.</p>
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<b>REVIEWER</b>	Lene Theil Skovgaard Dept. of Public Health, Biostatistics Section, University of Copenhagen, Denmark
<b>REVIEW RETURNED</b>	03-Aug-2018

<b>GENERAL COMMENTS</b>	<p>It should be more clearly stated which variables are considered as outcome variables and which as explanatory variables. Furthermore, conclusions from analyses including intermediate variables could be elaborated upon, e.g. including "perceived safety..." when the outcome is "Time spent...".</p> <p>In table 1, dog owners are compared to non-dog-owners for each variable separately, but it seems that the correlation between subjects were not taken into account in these analyses. This may mean too small P-values.</p> <p>In the analyses of Table 2 and 3, the outcomes "Time spent..." and "General health" were collapsed to binary variables. Why? Time spent in NOE was originally in units of minutes, which would yield much more information.</p> <p>In Table 3, interactions were included, leading to a huge table, with many NA due to nonsignificant interactions. Could it be an idea to produce a graph instead? And even include the nonsignificant interactions (since non-significant results do not prove a non-existing effect).</p> <p>What kind of software was used to perform the analyses?</p>
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<b>REVIEWER</b>	Diego Montano Ulm University, Germany
<b>REVIEW RETURNED</b>	16-Aug-2018

<b>GENERAL COMMENTS</b>	<p>Statistical review of the manuscript: "Interactions between dog ownership, the natural outdoor environment and health: a cross-sectional study"</p> <p>Major issues</p> <p>1. Dichotomization of continuous variables. It is unfortunate that the authors dichotomized the leisure time walking (LTW) and time spent in NOE, given the fact that these variables are truly continuous ones. Dichotomization has been found to be a really bad practice (see e.g. Royston et al 2006: "Dichotomization of continuous data is unnecessary for statistical analysis"). Today, there are lots of statistical models which are appropriate to model</p>
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continuous variables, depending on the characteristics of the probability distribution of the variables of interest. The loss of statistical power and the risk of biased estimates is substantial when truly continuous variables are dichotomized, and, from my point of view, there is no statistical argument which justifies this unfortunately common practice.

2. Interaction effects. There are two major issues. First, the authors make use of an unsuitable methodology to investigate and report interaction effects. From the point of view of statistical estimation, there is no need to dichotomize the dependent variables in order to estimate interaction effects and the direction of associations (i.e. synergistic or antagonistic). For instance, there are excellent visualisation and analysis tools available in R for the presentation and analysis of interaction effects (see e.g. <https://cran.r-project.org/web/packages/interplot/vignettes/interplot-vignette.html>). The model parameters obtained in the analyses contain all information necessary to assess the direction of interaction effects. Moreover, the category "NA" in Table 3 is somewhat confusing, since the estimates are actually "available", but the authors decided not to report them.

The second issue which is more difficult to address is related to the the number of expected interactions, and confounding. In the present manuscript, the authors are estimating 5 interaction effects between dog ownership and several characteristics of the NOE (area, distance, etc). However, as showed by Yzerbyt et al. (2004), doi:10.1016/j.jesp.2003.10.001, it is not enough to control for the main effects of potential confounding variables, e.g. age, sex, education, etc. In order to obtain unbiased estimates of interaction effects, it is necessary to include all interactions that may affect the relationship between dog ownership and age, sex, education and so on (i.e. interaction between age and dog ownership, sex and dog ownership, etc). From this perspective, the authors need a sound theoretical causal model which can be used to select all necessary interactions, even if they have only cross-sectional data. In the present form, the analyses are driven actually by data availability, and not by a sound theoretical model of human behaviour.

3. Multiple testing. The authors perform unadjusted multiple testing in Table 1.

4. Random effects. I'm assuming that the authors nested neighbourhoods within cities, i.e. a two-level random effects structure. It is not clear, however, the type of sampling design used in the study. The authors state "Neighborhoods were selected to maximize variability in access to NOE and socioeconomic status" (p. 7). Please check that the random effects and the levels of nesting are congruent with the sample design (cluster or stratified sample), and include that information in the main manuscript.

#### Minor issues

1. What does the authors mean with "exposure to NOE"? = Time spent in NOE? Or Residential surrounding greenness?

2. On page 9 the authors state: "We furthermore used varying buffer sizes to obtain a better understanding of what distance to NOE is most beneficial to health" (p. 9). What kind of analysis did the authors perform? Depending on the kind of analyses this may lead to statistical artefacts related to overfitting, i.e. it seems as if

	<p>buffer sizes were optimized to highly correlate with health scores of individual participants.</p> <p>3. Sensitivity analysis. I do not understand the rationale for the sensitivity analyses. The authors considered a wide array of variables without providing sound arguments. It seems as if the authors were just including all variables available in the dataset in order to see what “happens” with the estimates. This procedure is a sort of “multiple modelling” (see Young 2011, doi: 10.1111/j.1740-9713.2011.00506.x).</p> <p>4. Buffer. Please briefly explain what buffer means in spatial data analysis.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewers' Comments to Author:

Reviewer: 1

Reviewer Name: Elizabeth A. Richards

Institution and Country: Purdue University, School of Nursing, USA

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

This paper is well-written and has the potential to significantly contribute to the literature.

Introduction: This section has logical flow and good use of pertinent references.

1) However, I feel you are missing some more current or timely references to dog walking and physical activity impact. Please see:

- Christian, H., Bauman, A., Epping, J., Levine, G., McCormack, G., Rhodes, R., Richards, E., Rock, M., & Westgarth, C. (2016). State of the art review: Encouraging dog walking for health promotion and disease prevention. *American Journal of Lifestyle Medicine*. Advanced online publication.
- Richards, E., Ogata, N., & Cheng, C. (2016). Randomized, controlled e-mail based walking intervention: Differences between dog owners and non-dog owners. *Clinical Nursing Research*. 65(3), 191-201. doi: 10.1097/NNR.0000000000000155
- Richards, E., Ogata, N., & Cheng, C. (2016). Evaluation of the Dogs, Physical Activity, and Walking (Dogs PAW) intervention: A randomized controlled trial. *Nursing Research*. 65(3), 191-201. doi: 10.1097/NNR.0000000000000155
- Richards, E. (2016). Does dog walking predict physical activity participation: Results from a national survey. *American Journal of Health Promotion*, 30(5), 323-330.
- Richards, E., McDonough, M., Edwards, N., Lyle, R., Troped, P. (2013). Psychosocial and environmental factors associated with dog walking. *International Journal of Health Promotion and Education*, 51(4), 198-211.

Thank you for these suggestions – we have now added these references to the introduction, please see page 6.

2) In addition, the description or use of theory is lacking. Is there a health behavior theory that could support your hypothesis?

We did not work from a specific health behavior theory while conducting this study. We have constructed our hypotheses based on previous studies and based on the idea that health and health-related behaviors are influenced by individual factors, but also by the physical environment. This is acknowledged by many theories, for example the social cognitive theory and the social ecologic perspective. We added the following to the Introduction, page 6-7:

According to the theories of health behavior [37] and the social ecological framework [38], identifying the environmental factors that influence health outcomes (e.g. access to NOE) could lead to potential intervention strategies that could eventually improve health.

In line with above-mentioned studies and health behavior theories, we hypothesized that dog owners walk more, spend more time in NOE and are healthier than non-dog owners, and that the health benefits are more apparent in dog owners within green neighborhoods and with access to NOE compared to those in less green areas and with poor NOE access. We therefore investigated the associations between dog ownership, leisure time walking, time spent in NOE, and general and mental health status, and whether these associations differed among those with good and poor access to NOE and those living in green and less green areas.

#### Design and Methods

1) Recruitment of participants should include more detail

We have provided additional details about the recruitment process, please see page 7:

Neighborhoods were selected to maximize variability in access to NOE and socioeconomic status. In order to arrive at a final sample of approximately 1000 respondents per city, a random sample of 30 to 35 addresses per neighborhood were mailed with a letter explaining the purpose of the project after which they were visited by interviewers. In Doetinchem, persons were asked to send back an answer card to indicate their willingness to participate before they were visited by the interviewers; and in Kaunas, persons were approached by mail to fill out postal questionnaires. Respondents needed to have an age between 18 and 75 years and to be able to speak the local language. Data were collected using interview-administered questionnaires (except in Kaunas, where self-administered questionnaires were used) at respondents' residences during May-November 2013.

2) How was green and blue space defined?

A definition of NOE (or green and blue spaces) is now provided on page 9:

NOE were defined as all public and private outdoor spaces that contain 'green' and/or 'blue' natural elements such as street trees, forests, city parks and natural parks/reserves, and also included all types of waterbodies such as canals, ponds, creeks, rivers, beaches.

3) Adding a description of data screening is also important- for example normality? Missing data?

We have added this information on page 12:

Associations with leisure time walking and time spent in NOE were estimated with Poisson regression which is used to model count variables; associations with general health status were estimated with logistic regression because of the dichotomous nature of the data; and since mental health scores were normally distributed, associations were estimated with linear regression.

Analyses were based on complete cases (missing data differed by outcome and ranged between n=360 and 416).

Results- this section is clearly written- well done.

1) How did your sample rates of dog ownership compare to population level estimates of dog ownership?

We were not able to find reliable dog ownership statistics for all four countries, but the Doetinchem prevalence (16.5%) coincided with the household prevalence of dog ownership in the Netherlands (18%); and the Stoke-on-Trent prevalence (25.3%) coincided with the UK household prevalence (24%) too.

Discussion section is well written but I feel the concluding statement could be stronger. In addition, more could be stated about next steps/areas for future research/ and implications of findings.

Thank you for pointing out that the concluding statement could be stronger. We edited to the following, please see page 18:

Dog owners performed more leisure time walking and spent more time in NOE compared with non-dog owners, especially when they had access to NOE and when they lived in green areas. There was no consistent relationship between dog ownership and better perceived general or mental health status. In a largely physically inactive society where many people remain indoors, dog walking in parks or other NOE may be an opportunity to engage people in walking behavior as a path towards better health. Cities should therefore ensure that there is access to NOE for dog owners and provide green infrastructure in order to promote dog walking. Future research should focus on natural experiments and evaluation of intervention strategies to increase dog owners' access to NOE.

Reviewer: 2

Reviewer Name: Sophie Hall

Institution and Country: University of Lincoln, UK

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

Overall a well written article which should add to this growing field. Specific comments include:

Abstract – P3

Line 8: Unclear what is meant by 'additional' health benefits

Thank you for pointing out that this is unclear. We now added more explanation, please see page 3:

Dog owners walking their dog in natural outdoor environments (NOE) may benefit from the physical activity facilitated by dog walking and from time spent in nature. However, it is unclear whether dog owners receive additional health benefits associated with having access to NOE above the physical activity benefit of walking with their dog.

Line 29: Should be including not included

We changed this sentence, please see page 3:

n=3586 adults from Barcelona (Spain), Doetinchem (the Netherlands), Kaunas (Lithuania), and Stoke-on-Trent (United Kingdom).

Introduction – P6



Line 18 – this is not necessarily true, only some research has shown this.

There is strong evidence that suggests that dog owners walk more often and are more physically active than non-dog owners. We have added additional review papers that back up this statement, please see page 6:

There is strong evidence to suggest that dog owners walk more often and are more physically active than non-dog owners [10-12].

Line 33-34 – doesn't read well – also many dog walkers do not have access to parks

In general there could be a greater consideration of the dog walking literatures. It is also important to consider the literatures which do not show positive influences of dog ownership on walking and subsequent health outcomes.

We have changed this sentence for better readability, please see page 6:

Not all dog owners have access to parks thus improving access to parks in residential areas could be important for facilitating dog walking, especially since local parks are a common place for dog walking [29].

We also consider studies that did not report benefits of dog ownership, please see page 6:

However, not all studies show health benefits of dog ownership [24–26] and a large proportion of dog owners do not walk their dog [27,28].

Method – P7

Line 15: How can it be 30-35 adults were invited, what is the exact number? Is this a typo?

We regret that this was not clearly described. In each city and in each included neighborhood, 30 to 35 adults were invited to participate, resulting in 4 cities \* 30 neighborhoods \* 30-35 adults ≈ 4000. Please see the Methods section (p. 7):

In order to arrive at a final sample of approximately 1000 respondents per city, a random sample of 30 to 35 adults per neighborhood were invited to participate.

Line 15: 18-75 years??

Yes, respondents aged 18-75 years were included. We adapted this sentence for more clarity (see page 7):

Respondents needed to be aged between 18 and 75 years and to be able to speak the local language.

P8

Line 41: Spell out abbreviations

We now spell out the abbreviation, please see page 10:

Mental health was assessed using the Short Form Health Survey (SF-36) mental health subscale, including five items (e.g. nervousness, depression), and is a valid and reliable measure of mental health [40].

Results – P11

Line 18: Was should be were

Thank you spotting that error. This is now corrected.

It should be emphasised that these were 'self-reported' measures e.g. line 47: There no significant differences in self-reported general health status.....

We have put more emphasis on the fact that general health status was self-reported in this study. We have done this throughout the manuscript, see for example page 12:

There were no differences in perceived general health status and mental health between dog owners and non-dog owners.

Discussion – P13

Line 31: This needs expanding upon in the results, did you check the demographics of these samples to see if the Stoke participants were under/over represented on a specific factor? Same with regards to the Barcelona sample.

The samples from Barcelona and Stoke-on-Trent were on average younger than those from the other cities (average age of 46 vs. 56 and 60 years). Other factors that were overrepresented in these samples were a comfortable perceived income situation, and a low prevalence of disabilities restricting mobility, perhaps indicating that the Barcelona and Stoke-on-Trent samples are healthier than the samples from the other two cities. We however adjusted for these factors and can therefore not conclude that the differences between the cities are due to these factors. We do explain in the manuscript that walking and NOE time was lowest in these cities and that health benefits of dog ownership may arise when walking and NOE time is low to begin with. Please see page 16:

Dog ownership was only related to better perceived general or mental health in Barcelona and Stoke-on-Trent, the two cities where respondents were the least active and spent the least time in NOE. This suggests that the health benefits of dog ownership exist when walking and time spent in NOE is low to begin with.

P14

Line 15: State whether these were also self-report measures. For instance, participants in your sample may have had lower blood pressure if they were dog owners and walked a lot, but they would not necessarily be aware of this.

The BMI was based on self-reported data, but blood pressure was measured by research staff. We have added this information in the text, page 16:

Furthermore, an Australian study found that although pet ownership was associated with higher levels of physical activity, it was also associated with higher self-reported BMI, higher diastolic blood pressure, and smoking [54].

Line 30: Limitations also include lack of detail about the dog, breed, age, health size, temperament. If the dog is anxious or aggressive when out walking this may inhibit potential mental health benefits associated with stress reduction etc.

Thank you for pointing out this limitation, we have included the following statement in the Discussion (page 17):

Limitations of this study include the lack of information about the dog (e.g. breed, age, temperament), the dog owner's level of attachment to their dog, the duration of dog ownership, and if the respondent was the primary carer of the dog. Such factors may have influenced the potential health benefits of dog ownership, but we were unable to take these factors into account.

Reviewer: 3

Reviewer Name: Dr. Timo-Kolja Pfortner

Institution and Country: IMVR, University of Cologne, Germany

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

I would like to thank the authors for this very interesting manuscript that was quite challenging for me as I'm not an expert in this field of research, in particular with regard to the usage of GEO-data. However, I have some expertise in the application of interaction terms. I think what your manuscript needs is a bit more explanations on your statistical strategy and the presentation of results. I really had some difficulties to understand the results tables and the statistical strategy of modeling. Below you will find my comments that might improve your manuscript.

Page 6, line 50/51: rather high access to NOE than high exposure to NOE. You mixed these terms in the manuscript

Thank you for pointing out the inconsistent use of terminology. We have corrected this on page 7 and throughout the manuscript:

In line with above-mentioned studies and health behavior theories, we hypothesized that dog owners walk more, spend more time in NOE and are healthier than non-dog owners, and that the health benefits are more apparent in dog owners within green neighborhoods and with access to NOE compared to those in less green areas and with poor NOE access. We therefore investigated the associations between dog ownership, leisure time walking, time spent in NOE, and general and mental health status, and whether these associations differed among those with good and poor access to NOE and those living in green and less green areas.

Page 7, line 14/15: Please elaborate how you receive 1000 respondents per city when a random sample of 30-35 adults was drawn?

Please see our response to Reviewer 2, page 6 (of this document).

Page 10, line 6-12: Using different indicators of neighborhood-SES is difficult as you cannot say, whether differences and associations of this indicator is based on differences in terms of the SES or in terms of the indicator in use.

Thank you for pointing this out. We agree that country-specific data for neighborhood SES might have complicated comparisons between the cities. We added this as a limitation in the Discussion. Please see page 17:

Although data collection was similar in each city, data on neighborhood SES was based on country-specific data and this might have complicated comparisons between cities.

Page 10: When reading your statistical analysis section, I was a little bit confused about the relevant independent and dependent variables. Please differentiate in the section before between dependent and independent variables. Moreover, the application of interaction terms are for non-methodologist to some extent difficult. Therefore, I would recommend including a figure about the assumed pathways and interactions.

Thank you for this suggestion. We have now structured these variables into explanatory variables and outcomes variables, please see pages 7-9. Also, we have removed the results of the interaction terms in the manuscript. However, we do report the associations between dog ownership and health

outcomes in subgroups with low/high access to NOE and with low/high residential surrounding greenness. This is because based on literature and health behavior theory, we have a clear hypothesis: health benefits of dog ownership are more apparent in dog owners within green neighborhoods and with access to NOE. This hypothesis is tested by stratifying the analyses by NOE access and residential greenness. We explain this in the introduction, and hope that this clarifies the assumed pathways. Please see page 6-7.

According to the theories of health behavior [37] and the social ecological framework [38], identifying the environmental factors that influence health outcomes (e.g. access to NOE) could lead to potential intervention strategies that could eventually improve health.

The aim of this study was to investigate the relationships between dog ownership, walking, the NOE and health. In line with above-mentioned studies and health behavior theories, we hypothesized that dog owners walk more, spend more time in NOE and are healthier than non-dog owners, and that the health benefits are more apparent in dog owners within green neighborhoods and with access to NOE compared to those in less green areas and with poor NOE access. We therefore investigated the associations between dog ownership, leisure time walking, time spent in NOE, and general and mental health status, and whether these associations differed among those with good and poor access to NOE and those living in green and less green areas.

Page 10: What is meant by varying exposure to NOE, and what is the rationale of point three. Please elaborate your idea behind this statistical strategy of stratifying the analysis.

As explained above, we have made a small change to our analytical strategy based on the feedback from reviewers. We report on the associations between dog ownership and health outcomes in subgroups with low/high access to NOE and with low/high residential surrounding greenness. We do this because based on literature and health behavior theory, we have a clear hypothesis: health benefits of dog ownership are more apparent in dog owners within green neighborhoods and with access to NOE. This hypothesis is tested by stratifying the analyses by NOE access and residential greenness.

The analytical strategy is described on page 11-12:

1. The associations between dog ownership, leisure time walking, time spent in NOE, general health status, and mental health.
2. The associations specified at 1, stratified by NOE access (good and poor) and by residential surrounding greenness (high and low) to investigate whether the associations between dog ownership and the outcomes differ in these subgroups.

Furthermore, we refrain from using the term 'interaction' in the manuscript (including in the title).

Page 11, line 42: Why is it  $\geq 121$  minutes per week walking, when you equalized this measure to the city-specific mean as mentioned in the method section.

We no longer dichotomize this variable and instead report on the number of minutes leisure time walking per week. Please see methods section, results section and tables 2-3 for updated results and data description.

Page 12, line 12: To help readers understanding interaction terms, I would try to write the sentence like: ... the association between dog ownership and leisure time walking by number of NOE...

Thank you for this suggestion. We have adopted this way of writing in the manuscript. Please see e.g. the abstract:

Dog ownership was associated with higher rates of leisure time walking and time spending in NOE (incidence rate ratio 1.65, 95% CI 1.65, 1.66 and 1.48, 95% CI 1.45, 1.51 respectively). These associations were stronger in those with better access to NOE and in greener areas.

And e.g. page 14:

Stratified analyses showed that the association between dog ownership and leisure time walking for those with the highest number of NOE around their home (IRR= 1.72, 95% CI 1.71, 1.73) was stronger than for those with little NOE (IRR= 1.61, 95% CI 1.60, 1.62).

Page 12: Table 3 and the explanation of interaction terms are difficult to understand. For interactions terms it is relevant to know the main effect of dog ownership and the outcome variable, and the interaction term in one table. Therefore, I would recommend using more tables for the presentation of interaction terms of the i.e. 300m variable and the lowest and highest variables. As said, it is not clear whether you have estimated all these interaction terms in one model simultaneously or not. Therefore, it would be better to differentiate the presentation of model results by tables or to explain your strategy more carefully in the method section. Moreover, for example, on page 12, line 33, you present the OR of 4.31. Is this the main effect of being a dog owner and having NOE in a 1000m buffer, or is this the interaction term between dog ownership and having a NOE in a 1000m buffer. If the latter is true, your presentation of results (dog ownership + having a NOE in a 1000m buffer vs. non dog owners) is false as the interaction term specified the difference in the association of dog ownership with time spent in NOE by levels of having a NOE. However, it is possible to estimate the main effect, but, therefore, you need to sum the effect of the main effect (dog ownership) and the interaction effect (having a NOW in a 1000m buffer).

Thank you for these suggestions. As we no longer test for interaction, we no longer report the results for the interaction terms. Instead, we report the associations between dog ownership and health outcomes in subgroups with low/high access to NOE and with low/high residential surrounding greenness (please see above).

I would also recommend presenting all full results in a web appendix.

We have added supplemental table S1 showing the full model results for the associations between dog ownership and the outcomes.

If you like to compare models with different outcome variables, it is necessary that the number of observations are the same between different models.

Models with different outcome variables are not really compared since we are not comparing estimates of associations for e.g. leisure time walking and time spent in NOE. The number of observations differed by outcome due to missing data, and we did not want to restrict the sample size by using the number of observations that was available for all outcomes. We have added a comment about the different sample sizes per outcome on page 12:

Analyses were based on complete cases (missing data differed by outcome and ranged between n=360 and 416).

Reviewer: 4

Reviewer Name: Lene Theil Skovgaard

Institution and Country: Dept. of Public Health, Biostatistics Section, University of Copenhagen, Denmark

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

It should be more clearly stated which variables are considered as outcome variables and which as explanatory variables. Furthermore, conclusions from analyses including intermediate variables could be elaborated upon, e.g. including "perceived safety..." when the outcome is "Time spent...".

Thank you for this suggestion. We have now structured these variables into explanatory variables and outcomes variables, please see pages 7-9.

In table 1, dog owners are compared to non-dog-owners for each variable separately, but it seems that the correlation between subjects were not taken into account in these analyses. This may mean too small P-values.

In the analyses of Table 2 and 3, the outcomes "Time spent..." and "General health" were collapsed to binary variables. Why? Time spent in NOE was originally in units of minutes, which would yield much more information.

The variables leisure time walking and time spent in NOE are no longer used as dichotomous variables, but in their original continuous form (minutes/week and hours/week). General health status was originally answered on a 5 point scale, but categories were collapsed in order to increase the number of subjects in each group.

In Table 3, interactions were included, leading to a huge table, with many NA due to nonsignificant interactions. Could it be an idea to produce a graph instead? And even include the nonsignificant interactions (since non-significant results do not prove a non-existing effect).

Please see our response to Reviewer 3, page 6-7 (of this document). Also, to improve the readability of table 3, we report only the stratified analyses for the 500m buffer. The results for the other buffers are reported in a supplemental table.

What kind of software was used to perform the analyses?

All analyses were performed in STATA 14.2. This statement is now added to page 12.

Reviewer: 5

Reviewer Name: Diego Montano

Institution and Country: Ulm University, Germany

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

Major issues

1. Dichotomization of continuous variables. It is unfortunate that the authors dichotomized the leisure time walking (LTW) and time spent in NOE, given the fact that these variables are truly continuous ones. Dichotomization has been found to be a really bad practice (see e.g. Royston et al 2006: "Dichotomization of continuous data is unnecessary for statistical analysis"). Today, there are lots of statistical models which are appropriate to model continuous variables, depending on the characteristics of the probability distribution of the variables of interest. The loss of statistical power and the risk of biased estimates is substantial when truly continuous variables are dichotomized, and, from my point of view, there is no statistical argument which justifies this unfortunately common practice.

Thank you for this suggestion. Variables leisure time walking and time spent in NOE are no longer used as dichotomous variables, but in their original form (minutes/week and hours/week). We used Poisson regression to analyze these outcomes because of the nature of the data and the resemblance of the distribution. Please see page 12 (and tables 2-3):

Associations with leisure time walking and time spent in NOE were estimated with Poisson regression which is used to model count variables; associations with general health status were estimated with logistic regression because of the dichotomous nature of the data; and since mental health scores were normally distributed, associations were estimated with linear regression.

2. Interaction effects. There are two major issues. First, the authors make use of an unsuitable methodology to investigate and report interaction effects. From the point of view of statistical estimation, there is no need to dichotomize the dependent variables in order to estimate interaction effects and the direction of associations (i.e. synergistic or antagonistic). For instance, there are excellent visualisation and analysis tools available in R for the presentation and analysis of interaction effects (see e.g. <https://cran.r-project.org/web/packages/interplot/vignettes/interplot-vignette.html>). The model parameters obtained in the analyses contain all information necessary to assess the direction of interaction effects. Moreover, the category "NA" in Table 3 is somewhat confusing, since the estimates are actually "available", but the authors decided not to report them.

The second issue which is more difficult to address is related to the the number of expected interactions, and confounding. In the present manuscript, the authors are estimating 5 interaction effects between dog ownership and several characteristics of the NOE (area, distance, etc). However, as showed by Yzerbyt et al. (2004), doi:10.1016/j.jesp.2003.10.001, it is not enough to control for the main effects of potential confounding variables, e.g. age, sex, education, etc. In order to obtain unbiased estimates of interaction effects, it is necessary to include all interactions that may affect the relationship between dog ownership and age, sex, education and so on (i.e. interaction between age and dog ownership, sex and dog ownership, etc). From this perspective, the authors need a sound theoretical causal model which can be used to select all necessary interactions, even if they have only cross-sectional data. In the present form, the analyses are driven actually by data availability, and not by a sound theoretical model of human behaviour.

We thank the reviewer for suggesting a different analytical strategy for interaction effects. We have used the suggested R package to explore the interactions without using dichotomized variables and also including multiple interaction terms (e.g. sex and dog ownership). After this, we decided to change our analytical strategy. We no longer test for interaction and thus we no longer report the results of the interaction terms in the manuscript. We do report the associations between dog ownership and health outcomes in subgroups with low/high access to NOE and with low/high residential surrounding greenness. We do this because based on literature and health behavior theory, we have a clear hypothesis: health benefits of dog ownership are more apparent in dog owners within green neighborhoods and with access to NOE. This hypothesis is tested by stratifying the analyses by NOE access and residential greenness. We explain this in the introduction, please see page 6-7.

According to theories of health behavior [36], identifying driving environmental factors of health and health-related behaviors that we can intervene upon, such as access to NOE, could eventually lead to improved health.

The aim of this study was to investigate the relationships between dog ownership, walking, the NOE and health. In line with above-mentioned studies and health behavior theories, we hypothesized that dog owners walk more, spend more time in NOE and are healthier than non-dog owners, and that the health benefits are more apparent in dog owners within green neighborhoods and with access to NOE compared to those in less green areas and with poor NOE access. We therefore investigated the associations between dog ownership, leisure time walking, time spent in NOE, and general and

mental health status, and whether these associations differed among those with good and poor access to NOE and those living in green and less green areas.

The analytical strategy is described on page 11-12:

1. The associations between dog ownership, leisure time walking, time spent in NOE, general health status, and mental health.
2. The associations specified at 1, stratified by NOE access (good and poor) and by residential surrounding greenness (high and low) to investigate whether the associations between dog ownership and the outcomes differ in these subgroups.

Furthermore, we refrain from using the term 'interaction' in the manuscript (including in the title).

3. Multiple testing. The authors perform unadjusted multiple testing in Table 1.

Thank you for this comment. We have used the Benjamini-Hochberg Procedure to adjust the p-values for the false discovery rate. Please see page 11 (and Table 1):

Equivalence tests with Benjamini-Hochberg adjustments for false discovery rates (5%) [55] were used to test for differences between dog owner and non-dog owner characteristics.

4. Random effects. I'm assuming that the authors nested neighbourhoods within cities, i.e. a two-level random effects structure. It is not clear, however, the type of sampling design used in the study. The authors state "Neighborhoods were selected to maximize variability in access to NOE and socioeconomic status" (p. 7). Please check that the random effects and the levels of nesting are congruent with the sample design (cluster or stratified sample), and include that information in the main manuscript.

Yes, this is correct, neighborhoods were nested within cities and a random effects model was applied to the data in order to take into account the clustering within cities and neighborhoods. However, when using Poisson models in Stata (for leisure time walking and time spent in NOE variables), it was not possible to specify random intercepts at the city and neighborhood level, because only one level was allowed. We therefore chose to specify random intercepts at the neighborhood level in all models. We account for city differences by running models by city (table 2) and by using city-specific cut points for the NOE access and residential surrounding greenness variables (table 3). Please see page 11:

Associations were estimated using multilevel analysis with a random intercept defined at the neighborhood level.

The study participants were selected from 30 different neighborhoods in Barcelona (Spain), Doetinchem (the Netherlands), Kaunas (Lithuania), and Stoke-on-Trent (United Kingdom). The neighborhoods themselves were selected based on based on access to NOE and SES in order to maximize variability. Please see page 7:

Respondents were recruited from 30 different neighborhoods in Barcelona (Spain), Doetinchem (the Netherlands), Kaunas (Lithuania), and Stoke-on-Trent (United Kingdom) [34]. Neighborhoods were selected to maximize variability in access to NOE and socioeconomic status.

Minor issues

1. What does the authors mean with "exposure to NOE"? = Time spent in NOE? Or Residential surrounding greenness?



Thank you for pointing out the inconsistent use of terminology. We have corrected this throughout the manuscript.

2. On page 9 the authors state: “We furthermore used varying buffer sizes to obtain a better understanding of what distance to NOE is most beneficial to health” (p. 9). What kind of analysis did the authors perform? Depending on the kind of analyses this may lead to statistical artefacts related to overfitting, i.e. it seems as if buffer sizes were optimized to highly correlate with health scores of individual participants.

The buffer sizes (300m, 500m, and 1000m) mentioned on page 9 were predetermined and were based on previous research (please see reference list 42 and 43). We performed the interaction analyses with ‘the varying buffer sizes’, i.e. 300m, 500m, and 1000m to see whether results are consistent across the various buffer sizes. To clarify this, see edited text on page 10:

We furthermore used three predetermined buffer sizes to obtain a better understanding of what distance to NOE is most beneficial to health [46]. Using GIS we constructed three indicators: (1) the number of, and (2) the total surface area NOE within road network buffers of 300m, 500m, and 1000m, and (3) the road network distance to the nearest NOE, all dichotomized using the median values.

3. Sensitivity analysis. I do not understand the rationale for the sensitivity analyses. The authors considered a wide array of variables without providing sound arguments. It seems as if the authors were just including all variables available in the dataset in order to see what “happens” with the estimates. This procedure is a sort of “multiple modelling” (see Young 2011, doi: 10.1111/j.1740-9713.2011.00506.x).

The first sensitivity analysis was undertaken to investigate whether adjustment for characteristics that were found to differ between dog owners and non-dog owners, and that may relate to health, changed the associations. For example, there was a higher prevalence of current smokers among dog owners compared to non-dog owners. Because of the detrimental effect of smoking on health, we wanted to see whether smoking was confounding the relationship between dog ownership and health. Furthermore, social interaction may mediate the relationship between dog ownership and health as reported previously. It was not the aim of the research to investigate this pathway, but by adjusting for frequency of contact with family and/or friends, and whether respondents felt part of a group of friends we were able to see whether the observed relationship between dog ownership and health changed.

The second sensitivity analysis was undertaken to evaluate differences between time spent in NOE near home, and in or near the city. Perhaps dog owners receive health benefits from spending time in NOE in or near the city instead of the NOE near their home. However stratification by time spent in NOE has been removed from the manuscript as it is also an outcome in this study and we do not wish to confuse the reader.

We added additional explanation to justify these analyses on page 11:

Sensitivity analyses were undertaken to investigate whether additional adjustment for covariates changed the associations. Additional adjustments were carried out for characteristics that were found to differ between dog owners and non-dog owners and that may relate to health (smoking and BMI), or that have been found to be mediators of the dog ownership-health relationship (frequency of contact with family and/or friends, and whether respondents feel part of a group of friends) [59,60].

4. Buffer. Please briefly explain what buffer means in spatial data analysis.

We have added a figure to the manuscript to explain about the different buffer types used in this study:

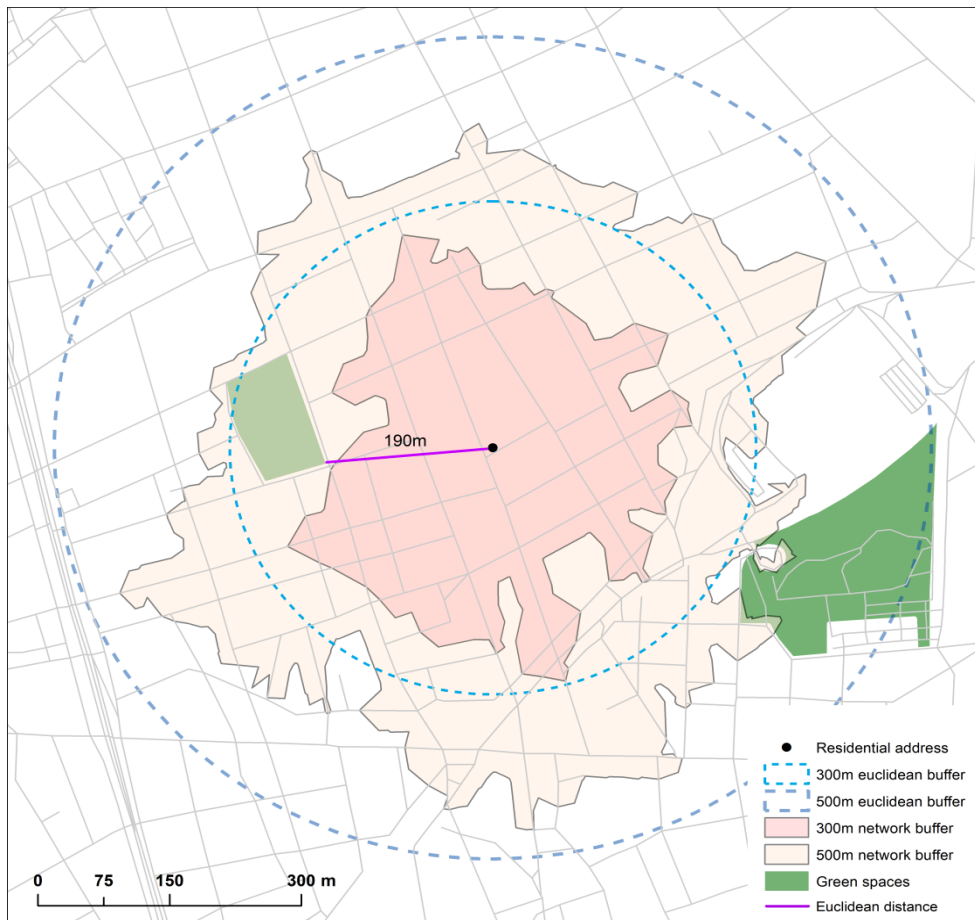


Figure 1. Example of different buffer types and the distance to natural outdoor environments from a residential address.

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Elizabeth A. Richards Purdue University USA
<b>REVIEW RETURNED</b>	06-Nov-2018

<b>GENERAL COMMENTS</b>	<p>This paper is well-written and will significantly contribute to the literature.</p> <p>Introduction: This section has logical flow and good use of pertinent references.</p> <p>1) The authors jump to the built environment quickly- I feel the argument for focusing on this could be built up a bit more</p> <p>2) A link could be made about how walking is a population level strategy to address inactivity and then move onto dog walking and address the prevalence rates of dog ownership.</p> <p>Design and Methods</p> <p>1) Are there supporting references that could be provided describing the methods of the PHENOTYPE study</p> <p>2) Did each study location have similar availability of publicly available NOE and NVDI data?</p> <p>Results- this section is clearly written- well done.</p> <p>Discussion section is well written.</p>
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<b>REVIEWER</b>	Sophie Hall University of Lincoln, UK
<b>REVIEW RETURNED</b>	02-Nov-2018

<b>GENERAL COMMENTS</b>	I am happy to approve this paper and I think it will make a useful contribution to the existing literatures
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<b>REVIEWER</b>	Lene Theil Skovgaard Dept. of Biostatistics, Institute of Public Health, University of Copenhagen, Denmark
<b>REVIEW RETURNED</b>	19-Nov-2018

<b>GENERAL COMMENTS</b>	<p>. Unfortunately, the quality of this paper is much deteriorated from the previous version for reasons described below. As a consequence of this, I have not performed a thorough review.</p> <p>The suggestion to use the outcomes in units of minutes instead of collapsing them to binary outcomes has lead the authors to perform Poisson regression and state the results as incidence rate ratios, which is absolute nonsense. There are no rates here, since we are not counting anything. We are dealing with a quantitative variable, measured in units of minutes, and even though the distribution may resemble a Poisson distribution, it is surely not. Probably, it may be transformed to a normal distribution by taking logarithms (if there are no zeros) and results would then be expressed in ratios, or percentage differences. In case of zeroes, maybe a Gamma distribution could be used. A Poisson distribution (apart from being totally inadequate in this situation) has the property that mean and variance coincide, i.e. it has no free variance. This means, that results in this paper is suffering from seriously biased standard errors (confidence intervals become much too small) and therefore also highly exaggerated P-values and conclusions.</p> <p>In the "Statistical analysis" section, it is said that they want to focus on "the association between .....&lt;a list of 5 variables&gt;". This should be more focused: Which variables are considered as outcomes and which as covariates? Surely, you don't look at all associations pairwise, so state more clearly what you do. Is dog ownership the only covariate and all the rest are outcomes?</p>
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## VERSION 2 – AUTHOR RESPONSE

Reviewers' Comments to Author:

Reviewer: 1

Reviewer Name: Elizabeth A. Richards

Institution and Country: Purdue University, USA

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

This paper is well-written and will significantly contribute to the literature.

Thank you reviewing our paper. Please see below our responses to your comments.

Introduction: This section has logical flow and good use of pertinent references.

1) The authors jump to the built environment quickly- I feel the argument for focusing on this could be built up a bit more

We added the following to page 6 to make a better link between physical activity and the built environment:

“Physical activity behavior is influenced by many factors. Apart from individual-level factors including age, sex, and health status, the built environment is an important determinant of physical activity [4,5].”

2) A link could be made about how walking is a population level strategy to address inactivity and then move onto dog walking and address the prevalence rates of dog ownership.

We added the following to page 6:

“Promotion of walking could be a population-level strategy to address physical inactivity. As such, and considering the prevalence of dog ownership (e.g. 18% in the Netherlands [8]), dog walking has been identified as a simple way of promoting physical activity [9–12].”

Design and Methods

1) Are there supporting references that could be provided describing the methods of the PHENOTYPE study

On page 7, we referred to the study protocol (ref 41) and another important PHENOTYPE paper describing the characterization of the natural environment (ref 42):

41 Nieuwenhuijsen MJ, Kruize H, Gidlow C, et al. Positive health effects of the natural outdoor environment in typical populations in different regions in Europe (PHENOTYPE): a study programme protocol. *BMJ Open* 2014;4:e004951. doi:10.1136/bmjopen-2014-004951

42 Smith G, Cirach M, Swart W, et al. Characterisation of the natural environment: quantitative indicators across Europe. *Int J Health Geogr* 2017;16:16. doi:10.1186/s12942-017-0090-z

2) Did each study location have similar availability of publicly available NOE and NVDI data?

Please see page 8 for:

“The NDVI was derived from Landsat 5 and 8 satellite images at a resolution of 30 m × 30 m on cloud-free images within the greenest season (April to September) in the relevant period for this study (2011-2013).”

Access to NOE was estimated with land use maps from local sources in each city.

Data sources were:

- Stoke-on-Trent City Council Green Space Audit 2012 OS MasterMap Topographic layer
- Mapa Ecològic de Barcelona (3a edició) Topogràfic updated 2011
- Inventory of green areas of Kaunas city (Kaunas city administration), applied to Urban Atlas boundaries, as at 2012
- Top10Vector (state mapping agency) 2006 (for Doetinchem)

These details are described in reference [42]. We describe this in the paper on page 8:

“Access to NOE was estimated with land use maps from local sources in each city (details in [42]).”

Results- this section is clearly written- well done.

Discussion section is well written.

Thank you.

Reviewer: 2

Reviewer Name: Sophie Hall

Institution and Country: University of Lincoln, UK

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

I am happy to approve this paper and I think it will make a useful contribution to the existing literatures

Thank you for reviewing our paper.

Reviewer: 4

Reviewer Name: Lene Theil Skovgaard

Institution and Country: Dept. of Biostatistics, Institute of Public Health, University of Copenhagen, Denmark

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

Unfortunately, the quality of this paper is much deteriorated from the previous version for reasons described below. As a consequence of this, I have not performed a thorough review.

The suggestion to use the outcomes in units of minutes instead of collapsing them to binary outcomes has lead the authors to perform Poisson regression and state the results as incidence rate ratios, which is absolute nonsense. There are no rates here, since we are not counting anything. We are dealing with a quantitative variable, measured in units of minutes, and even though the distribution may resemble a Poisson distribution, it is surely not. Probably, it may be transformed to a normal distribution by taking logarithms (if there are no zeros) and results would then be expressed in ratios, or percentage differences. In case of zeroes, maybe a Gamma distribution could be used. A Poisson distribution (apart from being totally inadequate in this situation) has the property that mean and variance coincide, i.e. it has no free variance. This means, that results in this paper is suffering from seriously biased standard errors (confidence intervals become much too small) and therefore also highly exaggerated P-values and conclusions.

Thank you for suggesting a different analytical strategy. Transformation of these outcome variables was not possible because of zeros. The outcome variables did not seem to fit a Gamma distribution or negative binomial distribution. We therefore decided to report the results of the binary outcomes as we did in a previous version of the manuscript. We understand that not all reviewers were in favor of this approach. Considering that the distribution of these outcome variables is not making it easy to model the associations, we preferred to dichotomize the variables. The resulting loss of variation of these data is described in the discussion, please see page 16:

“Minutes of walking and time in NOE were dichotomized because of non-normal distributions and although this resulted in easier interpretation of data, it also resulted in information loss.”

In the “Statistical analysis” section, it is said that they want to focus on “the association between .....<a list of 5 variables>”. This should be more focused: Which variables are considered as outcomes and which as covariates? Surely, you don’t look at all associations pairwise, so state more clearly what you do. Is dog ownership the only covariate and all the rest are outcomes?

Thank you for this suggestion. We specified the analyses as follows (page 11):

“To investigate the association between dog ownership, and outcomes leisure time walking, time spent in NOE, and general and mental health status, and whether these associations differed for respondents with good/poor access to NOE and high/low residential surrounding greenness, we investigated:

1. The associations between dog ownership, and outcomes leisure time walking, time spent in NOE, general health status, and mental health.
2. The associations specified at 1, stratified by NOE access (good and poor) and by residential surrounding greenness (high and low) to investigate whether the associations between dog ownership and the outcomes differ in these subgroups.”

### VERSION 3 – REVIEW

<b>REVIEWER</b>	Elizabeth A. Richards Purdue University USA
<b>REVIEW RETURNED</b>	16-Jan-2019

<b>GENERAL COMMENTS</b>	The authors have address my previous comments and concerns. No further requests.
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<b>REVIEWER</b>	Lene Theil Skovgaard Dept. of Public Health, Section of Biostatistics, University of Copenhagen, Denmark
<b>REVIEW RETURNED</b>	18-Jan-2019

<b>GENERAL COMMENTS</b>	<p>The outcomes and covariates are now clearly separated and adequately described.</p> <p>The outcome variables "leisure time walking", "time spent in NOE" and "general health status" are now being dichotomized, and the analyses seems OK. I still think it is a pity though, that they could not be treated as quantitative variables, and I wonder whether the assumption of normality was taken too seriously. Note that we only demand residuals to be normally distributed, and with such a large data set, even this may not be very crucial.</p> <p>When quoting results, please make sure that "no difference" or "no association" means "nothing of importance in the confidence interval", otherwise state "no significant difference".</p>
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	<p>Table 1: Why not write the actual P-values? It is more informative.</p> <p>Table 2: Do you find evidence of a significant interaction between "dog ownership" and "city".</p> <p>In Table 3 (and S2), the title could be clarified by writing "stratified by access to NOE....". Also, in the results (the main text), when you state "stronger associations between....for those living 300m....", did you actually test whether the associations differ significantly?</p> <p>Table S1: The readers might be interested in evidence of effect of these covariates, e.g. an overall test for difference between the 5 different household types etc. Moreover, the effect of age should be given as the effect of e.g. 10 years to provide something a bit more informative. Did you investigate the assumption of linearity for age?</p>
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### VERSION 3 – AUTHOR RESPONSE

Reviewers' Comments to Author:

Reviewer: 1

Reviewer Name: Elizabeth A. Richards

Institution and Country: Purdue University, USA

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

The authors have address my previous comments and concerns. No further requests.

Thank you for reviewing our paper.

Reviewer: 4

Reviewer Name: Lene Theil Skovgaard

Institution and Country: Dept. of Public Health, Section of Biostatistics, University of Copenhagen, Denmark

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

The outcomes and covariates are now clearly separated and adequately described.

Thank you.

The outcome variables "leisure time walking", "time spent in NOE" and "general health status" are now being dichotomized, and the analyses seems OK. I still think it is a pity though, that they could not be treated as quantitative variables, and I wonder whether the assumption of normality was taken too seriously. Note that we only demand residuals to be normally distributed, and with such a large data set, even this may not be very crucial.

When quoting results, please make sure that "no difference" or "no association" means "nothing of importance in the confidence interval", otherwise state "no significant difference".

Thanks for this suggestion, we checked the wording of the results carefully.

Table 1: Why not write the actual P-values? It is more informative.

These are now added to table 1.

Table 2: Do you find evidence of a significant interaction between "dog ownership" and "city".

We did not calculate the interaction effect between dog ownership and city since we planned in advance to analyse both pooled and city-specific associations. This choice was made because of our sampling method (sampling in four cities) and the expected differences between the four cities.

In Table 3 (and S2), the title could be clarified by writing "stratified by access to NOE....". Also, in the results (the main text), when you state "stronger associations between....for those living 300m....", did you actually test whether the associations differ significantly?

Titles are now adapted to:

Table 3. Associations between dog-ownership, walking, time in NOE, perceived general and mental health status stratified by access to NOE and residential surrounding greenness.

Supplemental Table S2. Associations between dog-ownership, walking, time in NOE, perceived general and mental health status stratified by access to NOE and residential surrounding greenness

In a previous version of the manuscript we tested these interactions, but we no longer report these results. We have adapted the wording of the results to avoid suggestion of significance testing for these associations, e.g.:

We observed a larger odds ratio for the association between dog ownership and time spent in NOE for those living within 300m of a NOE (OR= 2.64, 95% CI 2.18, 3.20) compared to those living within >300m of a NOE (OR= 1.82, 95% CI 1.31, 2.55; Table 3).

Table S1: The readers might be interested in evidence of effect of these covariates, e.g. an overall test for difference between the 5 different household types etc. Moreover, the effect of age should be given as the effect of e.g. 10 years to provide something a bit more informative. Did you investigate the assumption of linearity for age?

We added p for trend to show the effect of the covariates on the associations between dog ownership and the outcomes (please see Table S1). The associations for age are now provided per 10 years (please see Table S1). We did not evaluate linearity of the association for age since we were not so much interested in interpreting the associations with age per se, but merely in the effect on the associations between dog ownership and the outcomes while adjusting for age.