

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Neighbourhood greenspace and physical activity and sedentary behaviour among older adults with a recent diagnosis of type 2 diabetes: A Prospective analysis
<b>AUTHORS</b>	Chong, Shanley; Mazumdar, Soumya; Ding, Ding; Morgan, Geoff; Comino, Elizabeth; Bauman, Adrian; Jalaludin, Bin

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Usama Bilal Drexel University, USA
<b>REVIEW RETURNED</b>	03-Apr-2019

<b>GENERAL COMMENTS</b>	<p>bmjopen-2019-028947</p> <p>Neighbourhood greenspace and physical activity and sedentary behaviour among older adults with a recent diagnosis of type 2 diabetes: A Prospective analysis.</p> <p>This manuscript aims to study the association between greenspace availability and physical activity behaviors in people with recently diagnosed diabetes and in people without diabetes. The authors find a weak association and no apparent interaction between the two subgroups. This manuscript needs some work in improving the reporting, guaranteeing consistency and improving the statistical methods. I have some comments that I hope can help improve the manuscript.</p> <p>Major comments</p> <ol style="list-style-type: none"><li>1. Abstract: objective (newly diagnosed T2DM) doesn't match methods (both newly diagnosed and people without T2DM). Results are unclear. For example, "the changes in amount of MVPA and walking remained fairly stable with increasing percentage of greenspace". Does this mean there's no association, or that there's association? Or that there's a threshold? The conclusion is also unclear.</li><li>2. Objective: the objectives of the paper in the introduction (P7, L150) does not match the objective in the abstract (or what's inferred from the title). In general, the authors need to justify why the association between greenspaces and physical activity may differ by T2DM status, and what's their hypothesis regarding this effect modification. I could think that newly diagnosed people may be more likely to engage in physical activity (higher motivation?), but I am not sure if there's evidence behind this or whether this is what the authors are thinking based on their expertise. Moreover, the study question itself may be problematic, since physical activity itself is a risk factor for diabetes. Therefore the authors are studying an effect modifier that is downstream from both the exposure and the outcome. I highly recommend the authors draw a DAG and try to problematize the research question</li></ol>
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	<p>3. Study sample: The response rate is really low, and I wonder if given that this is a randomly selected sample there are any sampling weights that the authors can use. The authors mention a “full study cohort” of 267,153 people, then mention a response rate of 18% (so are the 267k people before or after that response rate?).</p> <p>4. Exposure: if I am understanding this part right, the authors create a set of buffers (of varying sizes) around the residence of the participant, and then calculate what’s the proportion of green space within that buffer. First, how is greenspace calculated? Is it land use? Second, Is this standard procedure in the greenspace literature? My main concern is that if a 500m buffer includes the entrance to a (large) national park, then the proportion green space will be low, but accessibility to the park will be high. A potential sensitivity analysis to address robustness to this measurement strategy could potentially include counts of green spaces</p> <p>5. Moderator: while the assessment of diabetes is clear, it is unclear to me what the purpose of age at diagnosis is, since the authors seem to be only considering two groups: those with NO diabetes at baseline and NO diabetes at follow up, and those with NO diabetes at baseline, and a diagnosis at follow-up. So, excluding measurement error, subjects can only get diagnosed with diabetes in between.</p> <p>6. Statistical analysis: using statistical significance as a criteria for confounding has been shown over and over as a biased way of making this decision. My suggestion to draw a DAG (see comment 2 above) is even more important for the issue of confounding. For example, given that the authors seem to be running stratified models by T2DM diagnosis status, the newly diagnosed group has very few people, giving this sample very little power to detect differences in the target population. However, a confounder is a confounder regardless of whether the study is powered to detect it or not. Regardless of whether the authors are running these stratified models (inferred by the inclusion of the covariate of duration of T2DM diagnosis only for new diagnoses), assessing confounding by significance is never a good idea. The modeling strategy is a little bit obscure and would benefit from a restructuring of this paragraph. The authors seem to have one model per outcome, then an interaction term of diabetes X greenspace, then do a stratified model by diabetes status, but it is unclear why this last model (stratified by diabetes) is needed given the model with an interaction term. Last, given that greenspace is a contextual variable and that physical activity is clustered by area (because of both compositional and contextual effects), individuals living in the same area are not independent of each other. The authors do not describe any way of checking this, or any way of addressing this (clustered standard errors, GEE, multilevel models, etc.).</p> <p>7. The sensitivity analysis in the statistical analysis section is (a) poorly justified, (b) very unspecified (how is this modelled?), and (c) I don’t think this is a sensitivity analysis (in fact the authors call it sub-analysis in the results section). What assumptions are the authors checking here? If they believe this is an important aspect, they should include it in their main model. If they believe this is an exploratory analysis, they should specify so. I am not sure if this is a mediator or a confounder, but this highlights the need for the DAG I’ve requested in comments 2 and 6 above.</p> <p>8. The authors have 3 outcomes and 3 exposures, all with the same hypothesis (at least that’s what it seems from the</p>
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	<p>introduction and methods section). They are, therefore, making 9 comparisons without independent hypothesis, which warrants a correction for multiple testing.</p> <p>9. Results, Figure 2: this figure is hard to read, since lines are crossing. I suggest the authors make the connecting lines thinner, or otherwise confidence intervals are really hard to see.</p> <p>10. Results, figures 2 and 3: Another more important concern about the modeling approach here is that the authors do not present comparisons but rather show marginal results, making it very hard for the reader to see if more green space is associated with more physical activity. The authors should be more transparent with their findings and present coefficients, or figures that do actual comparisons.</p> <p>Minor comments</p> <p>11. Strengths and limitations box: first bullet makes a claim about no other study having looked at this question in people with newly diagnosed T2DM, but most of the results section of the abstract is devoted to people without T2DM. Wording of second bullet is odd. Third bullet is unclear with regards to whether this is a limitation or a strength (or both?).</p> <p>12. P6, L132: “lack of local facilities” is unclear. Facilities for physical activity?</p> <p>13. Methods, Outcomes: the wording of “sitting” as an outcome seems odd. It’s not sitting that is problematic, is the amount of time spent sitting. The authors use “duration of sitting” in other parts of the manuscript, which I believe is a more accurate term.</p> <p>14. Methods, covariates: Australia should be included as a country in country of birth, since even if people come from other English speaking countries similar to Australia, they would be migrants and therefore different from the native population (see migrant paradoxes).</p> <p>15. Statistical analysis: The authors state they are using a “one-way interaction between the status of type 2 diabetes and percentage of green space”, but this is technically a two-way interaction.</p> <p>16. The first paragraph of the results section (P13) belongs to a methods section, since it deals with the inclusion/exclusion into the sample.</p> <p>17. P12, L273: the authors state they present “Actual change values (i.e., marginal means)”. If they present means of their results, they are not presenting actual change values, but rather predicted values. Moreover, “marginal” and “mean” are redundant.</p> <p>18. Table 1: how is body mass index categorized? “acceptable weight” seems quite stigmatizing.</p> <p>19. This may be an edition/PDF issue but I cant see the footnotes of the tables.</p>
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<b>REVIEWER</b>	Carron Gordon Lecturer and Head of Section Faculty of Medical Sciences The University of the West Indies, Mona, Kingston 7 Jamaica
<b>REVIEW RETURNED</b>	21-Apr-2019

<b>GENERAL COMMENTS</b>	The paper was interesting and is an important addition to the literature. Page 9- under exposure: The definition of green space was very broad. It may not be reasonable to expect MVPA to occur to any
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	<p>great degree in all these areas included. This could have influenced the results.</p> <p>Page 8: The follow -up period varied for the participants. Though the change scores were used for the outcome measures, more or less change may have occurred depending on the time of follow-up measures. This could have affected the results.</p>
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<b>REVIEWER</b>	Geir Aamodt Norwegian University of Life Sciences Norway
<b>REVIEW RETURNED</b>	25-Apr-2019

<b>GENERAL COMMENTS</b>	<p>This is a well-written paper about the relationship between green Space, T2D, and physical activity (PA).</p> <p>The aim of your study is a bit complex. Could you please include a causal graph showing the relationships between the three variables (or three groups of variables)?</p> <p><b>Introduction</b> You discuss the association between green space and T2D very briefly. This should be extended (please see the review in the attached paper).</p> <p><b>Methods</b> You identify T2D as a potential moderator. Could T2D work as a mediator / indirect effect as well?</p> <p>Green space -&gt; T2D -&gt; PH?</p> <p><b>Results</b> Could you please include a p-value for the interaction term between green space and T2D?</p> <p><b>Discussion</b> Lines 384-394. It is not clear if this statement is a speculation or if this based on ref 36. Could you please clarify a bit more. You should include more limitations to the study such as selection bias and possible consequences, and residual confounding and possible consequences.</p> <p>Could you please include figures about the study area and also of the types of "buffer" for the neighborhoods you have used? (This is by the way a strength you have not included).</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Usama Bilal

Institution and Country: Drexel University, USA Please state any competing interests or state 'None declared':None declared

Please leave your comments for the authors below  
bmjopen-2019-028947

Neighbourhood greenspace and physical activity and sedentary behaviour among older adults with a recent diagnosis of type 2 diabetes: A Prospective analysis.

This manuscript aims to study the association between greenspace availability and physical activity behaviors in people with recently diagnosed diabetes and in people without diabetes. The authors find a weak association and no apparent interaction between the two subgroups. This manuscript needs some work in improving the reporting, guaranteeing consistency and improving the statistical methods. I have some comments that I hope can help improve the manuscript.

#### Major comments

1. *Abstract: objective (newly diagnosed T2DM) doesn't match methods (both newly diagnosed and people without T2DM). Results are unclear. For example, "the changes in amount of MVPA and walking remained fairly stable with increasing percentage of greenspace". Does this mean there's no association, or that there's association? Or that there's a threshold? The conclusion is also unclear.*

#### Objective:

We agree with the reviewer and have restated the objective. See page ....

#### Result:

"the changes in amount of MVPA and walking remained fairly stable with increasing percentage of greenspace" means there is no significant association between the changes in amount of MVPA and walking and percentage of greenspace. We agree with the reviewer and have rewritten the result.

#### Conclusion:

We agree and have re-written the conclusion.

2. *Objective: the objectives of the paper in the introduction (P7, L150) does not match the objective in the abstract (or what's inferred from the title). In general, the authors need to justify why the association between greenspaces and physical activity may differ by T2DM status, and what's their hypothesis regarding this effect modification. I could think that newly diagnosed people may be more likely to engage in physical activity (higher motivation?), but I am not sure if there's evidence behind this or whether this is what the authors are thinking based on their expertise. Moreover, the study question itself may be problematic, since physical activity itself is a risk factor for diabetes. Therefore the authors are studying an effect modifier that is downstream from both the exposure and the outcome. I highly recommend the authors draw a DAG and try to problematize the research question*

Justify – newly diagnosed diabetics are provided with multidisciplinary where that are given advice about among other things diet and physical activity. Therefore we hypothesised that people with a new diagnosis of diabetes would be more motivated to increase their PA levels and that greenspace would facilitate this.

3. *Study sample: The response rate is really low, and I wonder if given that this is a randomly selected sample there are any sampling weights that the authors can use. The authors mention a "full study cohort" of 267,153 people, then mention a response rate of 18% (so are the 267k people before or after that response rate?).*

The 45 and up study is based in the population of the state of New South Wales, Australia. Prospective participants were randomly sampled from the Medicare Australia database. About 18% of those invited participated and participants included and participants included about 11% of the New South Wales population aged 45 years and over. The total number of participants joined the 45 and up study and completed a baseline questionnaire is 267,153.

Sampling weights are not provided in the dataset. It is a standard practice to apply weights to survey data to calculate prevalence estimates in order to account for the differences in probability of selection (Mealing 2010). However, use of sampling weights is less common when comparing variable of interest within cohort study data. In this study, the estimates generated in the regression model were adjusted for age, sex and area-level deprivation. Hence weighting is not necessary when the variables used in calculating the weights were used as covariates in the analysis (Mealing 2010).

Mealing et al (2010). Investigation of relative risk estimates from studies of the same population with contrasting response rates and designs. BMC Medical Research Methodology 2010;10:26.

4. *Exposure: if I am understanding this part right, the authors create a set of buffers (of varying sizes) around the residence of the participant, and then calculate what's the proportion of green space within that buffer. First, how is greenspace calculated? Is it land use? Second, Is this standard procedure in the greenspace literature? My main concern is that if a 500m buffer includes the entrance to a (large) national park, then the proportion green space will be low, but accessibility to the park will be high. A potential sensitivity analysis to address robustness to this measurement strategy could potentially include counts of green spaces*

We agree that if a small buffer includes the entrance to a large national park then the proportion of greenspace will be low. However, larger buffers would be less likely to suffer from this potential problem. We have included three buffer sizes in our analyses to account for this.

Counting the number of discrete greenspace does not account for the actual amount of greenspace available to an individual and may not be an accurate metric. The use of different buffer sizes and proportion of greenspace within the buffers are widely used in numerous studies (Oliver et al (2007), Klompmaker et al (2018), Mytton et al (2012), Kolt et al (2013) and Astell-burt (2014)).

#### References:

Astell-Burt T, Feng X, Kolt GS: Is Neighborhood Green Space Associated With a Lower Risk of Type 2 Diabetes? Evidence From 267,072 Australians. Diabetes Care 2014, 37:197-201.  
Klompmaker JO, Hoek G, Bloemsmas LD, Gehring U, Strak M, Wijga AH, van den Brink C, Brunekreef B, Lebret E and Janssen NAH: Green space definition affects associations of green space with overweight and physical activity. Environmental Research, 2018, 160: 531-541.  
Kolt G, Astell-Burt, Feng X: Proximity to greenspace increases the propensity for regular walking and physical activity in adults. Medicine and science in sports and exercise 2013, 45(5):495.  
Mytton OT, Townsend N, Rutter H, Foster C: Green space and physical activity: An observational study using Health Survey of England data. Health & Place 2012, 1034-1041.  
Oliver LN, Schuurman N, Hall AW: Comparing circular and network buffers to examine the influence of land use on walking for leisure and errands. International Journal of Health Geographics 2007, 6:41.

5. *Moderator: while the assessment of diabetes is clear, it is unclear to me what the purpose of age at diagnosis is, since the authors seem to be only considering two groups: those with NO diabetes at baseline and NO diabetes at follow up, and those with NO diabetes at baseline, and a diagnosis at follow-up. So, excluding measurement error, subjects can only get diagnosed with diabetes in between.*

Age at diagnosis was used to calculate the duration between diagnosis and follow-up. Newly diagnosed T2D with a longer time interval between diagnosis and follow-up would have had more time to change their behaviour compared to participants diagnosed with T2D and a shorter time interval between diagnosis and follow-up. We adjusted for this in the regression models.

6. *Statistical analysis: using statistical significance as a criteria for confounding has been shown over and over as a biased way of making this decision. My suggestion to draw a DAG (see comment 2 above) is even more important for the issue of confounding. For example, given that the authors seem to be running stratified models by T2DM diagnosis status, the newly diagnosed group has very few people, giving this sample very little power to detect differences in the target population. However, a confounder is a confounder regardless of whether the study is powered to detect it or not. Regardless of whether the authors are running these stratified models (inferred by the inclusion of the covariate of duration of T2DM diagnosis only for new diagnoses), assessing confounding by significance is never a good idea. The modeling strategy is a little bit obscure and would benefit from a restructuring of this paragraph. The authors seem to have one model per outcome, then an interaction term of diabetes X greenspace, then do a stratified model by diabetes status, but it is unclear why this last model (stratified by*

*diabetes) is needed given the model with an interaction term. Last, given that greenspace is a contextual variable and that physical activity is clustered by area (because of both compositional and contextual effects), individuals living in the same area are not independent of each other. The authors do not describe any way of checking this, or any way of addressing this (clustered standard errors, GEE, multilevel models, etc.).*

- *Dag: We agree and have presented a DAG (Figure 2) to explain the issue of confounding.*
  - *Model: Combining the two diabetes status, all the potential confounders listed in DAG (Figure 2) were tested in univariate regression model. Variables which were found significant were included in the multivariate linear models. Two-way interaction between T2D status and greenspace were then explored for each outcome variable. There were no statistically significant interactions for each outcome variable except for the changes in MVPA with percentage of greenspace within 2km (p=0.039). Though, the interactions were not significant, in order to observe any differences in the pattern of relationship between greenspace and outcome variable by T2D status, we fitted regression models, stratified by the T2D status.*

We have added a sentence related to the used of DAG into the “Covariates” (page 11, line 242) and “Statistical analysis” section (page 12, line 266-267).
  - *Clustered, GEE, multilevel: Using Akaike Information criterion (AIC) for model comparison, the simpler linear models (after adjusting for area-level deprivation at postcode level) performed better than the GEE models (accounting for correlation between participants within postcodes). Therefore, we presented the results from the linear models.*
7. *The sensitivity analysis in the statistical analysis section is (a) poorly justified, (b) very unspecified (how is this modelled?), and (c) I don't think this is a sensitivity analysis (in fact the authors call it sub-analysis in the results section). What assumptions are the authors checking here? If they believe this is an important aspect, they should include it in their main model. If they believe this is an exploratory analysis, they should specify so. I am not sure if this is a mediator or a confounder, but this highlights the need for the DAG I've requested in comments 2 and 6 above.*

The results of the sub-analysis is for the whole study population and as it is not relevant to our objectives, we have deleted this section from the manuscript.

8. *The authors have 3 outcomes and 3 exposures, all with the same hypothesis (at least that's what it seems from the introduction and methods section). They are, therefore, making 9 comparisons without independent hypothesis, which warrants a correction for multiple testing.*

We agree and have adjusted the multiple comparison using Bonferroni method in our final models. See Figure 4.

9. *Results, Figure 2: this figure is hard to read, since lines are crossing. I suggest the authors make the connecting lines thinner, or otherwise confidence intervals are really hard to see.*

We have made the connecting lines thinner and rescaled all the y-axis in order to make the confidence intervals easier to read.

10. *Results, figures 2 and 3: Another more important concern about the modeling approach here is that the authors do not present comparisons but rather show marginal results, making it very hard for the reader to see if more green space is associated with more physical activity. The authors should be more transparent with their findings and present coefficients, or figures that do actual comparisons.*

Marginal means were used to show the predicted values of change at the follow-up compared to baseline. This estimate is more informative than just presenting a coefficient which only compares with a reference group. For example, using 0-5% of greenspace as a reference group, a positive coefficient at >5-10% of greenspace can have multiple meanings. The first possible meaning is the change at the follow-up for both greenspace categories are positive with higher positive change for

>5-10% greenspace. The second possible meaning is the change at the follow-up for both greenspace categories are negative with smaller negative change for >5-10% greenspace. The third possible meaning is the change for the greenspace >5-10% is positive and negative for the reference group. In addition, the corresponding 95% C.I. were presented which can be used to compare between proportions of greenspace. To enhance the presentation of the results, type 3 errors are presented to show the overall relationship between proportion of greenspace and outcome variable.

Moreover, marginal means are widely used in many studies to present the predicted value of health-related outcome variables change over time (Swaminathan et al 2011, Ghani et al 2014, Green et al 2015, Dempsey et al 2016, van Elten et al 2018)

#### References:

- Dempsey PC, Sacre JW, Larsen Rn, Straznicky NE, Sethi P, Cohen ND, Cerin E, Lambert GW, Owen N, Kingwell BA and Dunstan DW: Interrupting prolonged sitting with brief bouts of light walking or simple resistance activities reduces resting blood pressure and plasma noradrenaline in type 2 diabetes. *Journal of Hypertension* 2016, 34:12.
- Ghani Ra, Shyam S, Arshad F, Wahab NA, Chinna K, Safii S, Nisak MYB and Kamaruddin NA: The influence of fasting insulin level in post-gestational diabetes mellitus women receiving low-glycaemic-index diets. *Nutrition & Diabetes* 2014.
- Green CA, Yarborough BJH, Leo MC, Stumbo SP, Perrin NA and Nichols GA and Stevens VJ: Weight maintenance following the STRIDE lifestyle intervention for individuals taking antipsychotic medications. *Obesity* 2015, 23:10.
- Swaminathan S, Selvam S et al: Longitudinal trends in physical activity patterns in selected urban south Indian school children. *The Indian Journal of Medical Research* 2011, 134:174-180.
- Van Elten TM, Karsten MDA, Geelen A, van Oeers AM, van Poppel MNM, Groen H, Gemke RJB, Mol BW, Mutsaerts MAQ, Roseboom TJ and Hoek A: Effects of a preconception lifestyle intervention in obese infertile women on diet and physical activity; A secondary analysis of randomized controlled trial. *PLOS* 2018.

#### Minor comments

11. *Strengths and limitations box: first bullet makes a claim about no other study having looked at this question in people with newly diagnosed T2DM, but most of the results section of the abstract is devoted to people without T2DM. Wording of second bullet is odd. Third bullet is unclear with regards to whether this is a limitation or a strength (or both?).*

We agree with the reviewer and have re-written our strengths and limitations box.

- First bullet point: This is the first study to explore environmental influences on the behaviours of people who transition into living with type 2 diabetes, and compare its association with those without T2DM.
- Second bullet point: This is a large population-based cohort with data available at two time points.
- Third bullet point: A limitation is that the change in duration of physical activity and sitting were calculated from self-reported surveys.

12. *P6, L132: "lack of local facilities" is unclear. Facilities for physical activity?*

In the study of Thomas et al (2004), one of the questions were addressed was what are the perceived factors that prevent them from doing more physical activity. One of the contributing factors was "lack of local facilities". No further information was given what it actually means.

13. *Methods, Outcomes: the wording of "sitting" as an outcome seems odd. It's not sitting that is problematic, is the amount of time spent sitting. The authors use "duration of sitting" in other parts of the manuscript, which I believe is a more accurate term.*



We agree. We have changed “sitting and physical activity” into “duration of sitting and physical activity”. See page 9, line 205.

14. *Methods, covariates: Australia should be included as a country in country of birth, since even if people come from other English speaking countries similar to Australia, they would be migrants and therefore different from the native population (see migrant paradoxes).*

In Australia, it is recommended that all people with T2D should be referred for structured diabetes patient education as there is good evidence that it has a positive effect on increasing physical activity. These education programs are mainly offered in English and so people who were not able to communicate in English were not able to attend.

Moreover, in Australia, people mainly speak a language other than English have been considered a disadvantaged group with reduced access to government and community programs and services in Australia (Australian Bureau of Statistics: Standards for Statistics on Cultural and Language Diversity, 1999, Canberra: ABS). Studies in Australia have investigated the disparities in health services utilisation between English speaking countries and non-English speaking countries. People from non-English speaking countries were less likely to use health services (Ou et al 2010) and English literacy was one of the barrier in using health services (Plunkett and Quine 1996). Hence, in this study, we grouped all English speaking countries together with Australia-born.

15. *Statistical analysis: The authors state they are using a “one-way interaction between the status of type 2 diabetes and percentage of green space”, but this is technically a two-way interaction.*

We agree and have corrected it to two-way interaction. See page 12, line 274

16. *The first paragraph of the results section (P13) belongs to a methods section, since it deals with the inclusion/exclusion into the sample.*

We agree and have moved it into the method section under “Study population”. See page 8, lines 175-181

17. *P12, L273: the authors state they present “Actual change values (i.e., marginal means)”. If they present means of their results, they are not presenting actual change values, but rather predicted values. Moreover, “marginal” and “mean” are redundant.*

We agree and have changed “actual change values” to “predicted values of change”. See page 12, line 277.

18. *Table 1: how is body mass index categorized? “acceptable” weight” seems quite stigmatizing.*

We have changed “acceptable weight” to “healthy weight”.

19. *This may be an edition/PDF issue but I cant see the footnotes of the tables.*

Reviewer: 2

Reviewer Name: Carron Gordon

Institution and Country:

Lecturer and Head of Section

Faculty of Medical Sciences

The University of the West Indies, Mona, Kingston 7 Jamaica Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

The paper was interesting and is an important addition to the literature.

*Page 9- under exposure: The definition of green space was very broad. It may not be reasonable to expect MVPA to occur to any great degree in all these areas included. This could have influenced the results.*

We agree that the definition of greenspace is broad and could have influenced the results. The commercial dataset did not allow us to further categorise greenspace into the various categories and therefore we were unable to analyse by specific types of greenspace. We have discussed this limitation on page 19, lines 425-429.

*Page 8: The follow -up period varied for the participants. Though the change scores were used for the outcome measures, more or less change may have occurred depending on the time of follow-up measures. This could have affected the results.*

To account for the fact that the outcome measures varied between participants with different follow-up periods, we adjusted for this in all regression models (see page 12, line 271).

Reviewer: 3

Reviewer Name: Geir Aamodt

Institution and Country: Norwegian University of Life Sciences, Norway Please state any competing interests or state 'None declared':None declared

Please leave your comments for the authors below

This is a well-written paper about the relationship between green Space, T2D, and physical activity (PA).

*The aim of your study is a bit complex. Could you please include a causal graph showing the relationships between the three variables (or three groups of variables)?*

Please see reviewer 1 point 6.

*Introduction*

*You discuss the association between green space and T2D very briefly. This should be extended (please see the review in the attached paper).*

Most studies have investigated the relationship between greenspace as exposure and T2D as outcome variable (Astell-Burt et al 2014, Bodicoat et al 2014, Dalton et al 2016, Dendup et al 2017, Twohig-Bennett and Jones 2018, ). In our study T2D was treated as effect modifier and we investigated whether the association between greenspace and change of physical activity and sitting time varied between new T2D and no T2D. As this idea is complex, in the introduction we focussed on the change of behaviour after diagnosed with T2D with neighbourhood greenspace as a motivating factor rather than on the association between greenspace and T2D which may lead to confusion about the aim of the study.

References:

Astell-Burt T, Feng X, Kolt GS: Is Neighborhood Green Space Associated With a Lower Risk of Type 2 Diabetes? Evidence From 267,072 Australians. *Diabetes Care* 2014, 37:197-201.

Bodicoat DH, Donovan GO, Dalton AM, Gray LJ, Yates T, Edwardson C, Hill S, Webb DR, Khunti K, Davies MJ and Jones AP: The association between neighbourhood greenspace and type 2 diabetes in a large corss-sectional study. *BMJ Open* 2014,

Twohig-Bennet C and Jones A: The health benefits of the great outdoors: A systematic review and meta-analysis of greenspace exposure and health outcomes. *Ennvironmental Research* 2018, 628-637.

*Methods*

*You identify T2D as a potential moderator. Could T2D work as a mediator / indirect effect as well?*

*Green space -> T2D -> PH?*

Please see Figure 1.

**Results**

*Could you please include a p-value for the interaction term between green space and T2D?*

There were no statistically significant Interactions between access to greenspace and T2D for each outcome variable except for the changes in moderate to vigorous physical activity with percentage of greenspace within 2km (p=0.039). We have included this p-value in page 15 line 3212.

**Discussion**

*Lines 384-394. It is not clear if this statement is a speculation or if this based on ref 36. Could you please clarify a bit more.*

We agree. We have added the relevance reference to this statement. See page 18, lines 377-380.

*You should include more limitations to the study such as selection bias and possible consequences, and residual confounding and possible consequences.*

We have mentioned in the “Study population” section, 2<sup>nd</sup> paragraph that all potential participants in the 45 and Up Study were randomly selected and which has almost complete coverage of the whole NSW population. A study by Mealing et al comparing participants from the 45 and Up Study with a representative sample from the New South Wales Population Health Survey, reported that exposure-outcome associations had similar estimates although risk factor prevalence differed between the samples (Mealing et al 2016). Therefore, we believe that the risk of selection bias is minimal.

Although we adjusted for a number of important potential confounders, there may yet be some residual confounding. However, we share this limitation with most other published studies on neighbourhoods and health.

**Reference:**

Mealing et al. Investigation of relative risk estimates from studies of the same population with contrasting response rates and designs. BMC Medical Research Methodology 2010;10:26.

*Could you please include figures about the study area and also of the types of “buffer” for the neighborhoods you have used? (This is by the way a strength you have not included).*

We agree and have included a figure for the study area and the type of buffer we have used in the study.

**VERSION 2 – REVIEW**

<b>REVIEWER</b>	Usama Bilal Drexel University
<b>REVIEW RETURNED</b>	30-Aug-2019

<b>GENERAL COMMENTS</b>	<p>bmjopen-2019-028947.R1 Neighbourhood greenspace and physical activity and sedentary behaviour among older adults with a recent diagnosis of type 2 diabetes: A Prospective analysis The authors have addressed some of my comments, but I believe there are still a few things that need to be addressed, especially in terms of statistical and analytical decisions. I focus on three aspects that I believe are key and have not been addressed by the authors.</p> <p>1. While the authors have proceeded with drawing a DAG, I believe there are some issues here. First, I can't see how some of the variables shown as confounders (BMI, stress) can be causally</p>
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	<p>affecting the exposure (green spaces), especially as they have been shown in many previous studies (many of them linked by the authors) to be caused by the exposure. They are therefore mediators and should not be considered as confounders in any case. The DAG needs to be rethought with a review of the literature and expert knowledge.</p> <p>2. Again, as with a comment in my initial review: using significance in univariate models as criteria to determine what to include in models is not advisable. The previous comment about drawing the DAG was intended to help the authors decide what to adjust for, instead of relying on criteria that has been shown repeatedly to be biased. An example of this is “Causal Knowledge as a Prerequisite for Confounding Evaluation: An Application to Birth Defects Epidemiology” (AJE 2002).</p> <p>3. The data structure the authors are using seems to be clustered, where individuals are nested within areas, which given residential segregation and other neighborhood selection phenomenon leads to people being more similar to each other than to people outside the area. Therefore, one of the assumptions of regular linear models the authors are violating is that of independence between observations, since people living in the same areas are not independent of each other. The authors claim that the AIC of a linear model is lower than that of a GEE model, which is not a test that can be used to answer this question. A model that, in some way corrects for this (accounting for correlation in errors through GEE, accounting for correlation within areas with the addition of a random effect, even correcting for clustering in the standard errors using robust standard errors) is needed to provide unbiased standard errors.</p>
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<b>REVIEWER</b>	Carron Gordon University of the West Indies
<b>REVIEW RETURNED</b>	15-Aug-2019

<b>GENERAL COMMENTS</b>	<p>Lines 171-177. If recruitment was done for the 45 and up study between 2006 and 2009 and the SEEF questionnaire was distributed in 2010, how is it that maximum follow up period was 5.1 years? This needs to be clarified/corrected.</p> <p>Line 315 -320. It should be noted that the p-value for interaction between access to greenspace within 2 km (.039) and type 2 diabetes status is not shown in the figure 4.</p> <p>Lines 468-471. Ethics approval for the SEEF study should be included here as done in lines 179-181.</p> <p>Some references are incomplete eg ref 25 in line 545.</p>
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### VERSION 2 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1 (Usama Bilal)

Neighbourhood greenspace and physical activity and sedentary behaviour among older adults with a recent diagnosis of type 2 diabetes: A Prospective analysis The authors have addressed some of my comments, but I believe there are still a few things that need to be addressed, especially in terms of

statistical and analytical decisions. I focus on three aspects that I believe are key and have not been addressed by the authors.

1. While the authors have proceeded with drawing a DAG, I believe there are some issues here. First, I can't see how some of the variables shown as confounders (BMI, stress) can be causally affecting the exposure (green spaces), especially as they have been shown in many previous studies (many of them linked by the authors) to be caused by the exposure. They are therefore mediators and should not be considered as confounders in any case. The DAG needs to be rethought with a review of the literature and expert knowledge.

Response: Thank you for your valuable comments. We made an unintentional error when drawing the directions of the relationships in the original DAG. We agree with the comments and have re-drawn the DAG, treated BMI and level of psychological distress at baseline as potential mediators, and added two paragraphs for explanation (see page 11-12, lines 252-262). GEE models were applied to examine the effects of neighbourhood greenspace on psychological distress and BMI, and the associations were non-significant. The effects of psychological distress and BMI on change in outcome variables were also examined. BMI was a significant risk factor for changes in outcome variables. Psychological distress was not associated with changes in outcome variables. As psychological distress was not associated with outcome variables, it was not included in the final regression models.

2. Again, as with a comment in my initial review: using significance in univariate models as criteria to determine what to include in models is not advisable. The previous comment about drawing the DAG was intended to help the authors decide what to adjust for, instead of relying on criteria that has been shown repeatedly to be biased. An example of this is "Causal Knowledge as a Prerequisite for Confounding Evaluation: An Application to Birth Defects Epidemiology" (AJE 2002).

Response: Thank you for your valuable comments. We have re-arranged the structure of our analysis and re-written the "Statistical analysis" section by removing the use of univariate regression models to determine what to include in our models (see page 12, lines 268-291).

3. The data structure the authors are using seems to be clustered, where individuals are nested within areas, which given residential segregation and other neighborhood selection phenomenon leads to people being more similar to each other than to people outside the area. Therefore, one of the assumptions of regular linear models the authors are violating is that of independence between observations, since people living in the same areas are not independent of each other. The authors claim that the AIC of a linear model is lower than that of a GEE model, which is not a test that can be used to answer this question. A model that, in some way corrects for this (accounting for correlation in errors through GEE, accounting for correlation within areas with the addition of a random effect, even correcting for clustering in the standard errors using robust standard errors) is needed to provide unbiased standard errors.

Response: Thank you for your comments. We have re-run all the models using GEE to adjust for the correlation among participants within local government areas (see page 12, lines 271-274).

Reviewer: 2 (Carron Gordon)

1. If recruitment was done for the 45 and up study between 2006 and 2009 and the SEEF questionnaire was distributed in 2010, how is it that maximum follow up period was 5.1 years? This needs to be clarified/corrected.

Response: Thank you for pointing this out. There were small number of participants (n=16) where the SEEF follow-up questionnaires were completed in 2011. Therefore, it is not meaningful to present the range and we have presented the median and interquartile range only (see page 8, line 177).

2. Line 315 -320. It should be noted that the p-value for interaction between access to greenspace within 2 km (.039) and type 2 diabetes status is not shown in the figure 4.

Response: We did not, on purpose, present the p-value for interaction in Figure 4 as that made for a very busy figure. However, we have mentioned it on page 16 line 334.

3. Lines 468-471. Ethics approval for the SEEF study should be included here as done in lines 179-181.

Response: Thank you for pointing that out. We have included the ethics approval for the SEEF study on page 21, lines 456-459.

4. Some references are incomplete eg ref 25 in line 545.

Response: Thank you for pointing that out. We have checked the references and have corrected the incomplete references.