Author’s response to reviews

Title: Built environmental characteristics and diabetes: a systematic review and meta-analysis

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Author’s response to reviews:

Reviewer #1: The purpose of this article is to summarize the literature on the built environment and diabetes using a systematic review and meta-analysis. Overall the review methodology was sound and well done. The cautions around interpretation cross-sectional evidence were well described. The comments below mainly relate to the interpretation of the findings, which if addressed will significantly strengthen the paper.

Response:

We thank the reviewer for his/her time and effort in reviewing our manuscript, for the kind words and proposals to strengthen our manuscript. Please find a point-by-point response below.

1. One of the challenging aspects of studying the built environment across jurisdictions is the great heterogeneity that exists in definitions - above and beyond income level of the country. For example, the urban/rural classifications differs greatly across countries. In European countries, where they are geographically smaller, the urban rural differences are quite different than a geographically vast country such as Canada where rural populations are very far from urban centres, often north and with a higher number of Indigenous people and lower number of immigrant populations. The authors should comment on the definition of urban and how this definition actually differs depending on the country/region.

Response:
We thank the reviewer for this comment and we agree that heterogeneity is an issue in defining urban and rural areas. Nevertheless, the studies that focus on urban-rural differences in the present review were in majority (38 out of 60) conducted in middle income countries as defined by the world bank, and thus not in countries like Canada or European countries. This reduces the heterogeneity in the included studies. However, also across studies in middle income countries the definition of urban and rural areas differs. There is indeed no homogeneous definition for urban and rural areas used, and it may well be that a universal definition - if it would exist- would not be feasible. In addition, the definitions that are used are often not specified in the articles. To put these findings into perspective we added the following part to our discussion section, page 11, to capture this complexity:

“It must be recognized that great heterogeneity in definitions of urban vs. rural exists beyond stratification on country-income level. Across countries with the same country-income level, there is large variety what urban or rural areas may look like and the populations that reside in these areas. At present there is no homogeneous and generally accepted definition of urban or rural areas and the majority of studies did not include a definition that was used to make this classification.”

2. Selection bias is also a major concern with these studies. When looking at these comparisons one must always be cognizant of who is selecting to live in certain areas and why. For example, immigrants tend to settle in urban environments and may also be at increased risk for diabetes depending on their country of origin. Also related to the above point - selection pressures of living in an urban and more densely environment may differ according to setting. Finally, there are life stage and socioeconomic factors that determine where one lives, for example younger age, marital status, having dependents and income. It's unclear how the included studies attempted to address selection bias (or if they did). Regardless, the authors should consider how selection bias may be an explanation for the findings from the meta-analysis.

Response

We thank the reviewer for raising this issue. We recognize that selection is indeed a problem in environmental studies, that has not been taken or could not be taken into account in many studies. Not taking residential self-selection into account is a limitation in many studies included in our review and is a very important issue to take into account in future studies, as the place where people reside is seldom random. The impact of such residential self-selection can be substantial, but to date this is insufficiently investigated. Although we cannot solve this issue completely, we did attempt to take such selection bias into account by including only adjusted associations in our meta-analyses.
With regard to immigration status, we are dependent on the information of the original studies, and on them taking this issue into account when necessary and where possible, as was done by e.g. Booth et al., 2013 and Cubbin et al., 2006 [1, 2]. If not, the impact on the association most probably differs per country and per city and the proportions of immigrants. This lack of information makes it hard to include immigration status in our rationale. We hope that the reviewer appreciates that we added confounding variables to Table 1 and 3, as a response on point 3. This addition includes immigration status, which will allow to see which studies accounted for immigrant status in the studied association.

To elaborate on self-selection, we added a paragraph to the discussion section, page 13

“Finally, residential self-selection is an important issue that should be included in studies investigating the associations between built environment and disease. Self-selection is the phenomenon where residents choose a residence based on socioeconomic or other circumstances, or lifestyle preferences. Evidently, such selections may influence our results, as for instance higher SES neighbourhoods may contain more green space, as well as more highly educated and health conscious residents. However, the true effect of residential self-selection on these associations has often not been accounted for in the included studies and is difficult to investigate. One narrative review observed that studies using various approaches to identify self-selection (i.e. questionnaire, statistical methods) explained only a minor part of the associations between built environment and travel behaviours [3]. Two studies included in the present review observed that residential relocation, as an indicator of self-selection, resulted in inconsistent effects on associations with health outcomes [4, 5] It is therefore hard to conclude on the effect of self-selection bias on our results, based on the current evidence.”

3. Confounding is a major challenge with respect to built-environment studies and associations, particularly in cross-sectional studies. The authors should provide more detail on the level of confounder adjustment in the various studies. The confounder/risk adjustment should be detailed in the study characteristics table and wherever possible when summarizing the analytic results the nature of the adjustment should be summarized.

Response

We thank the reviewer for this valuable suggestion and recognize the challenge of various levels of confounder adjustment. We did take into account the level of adjustment in the quality assessment and set a minimum adjustment level for the meta-analyses (i.e. age and sex). However, we do agree that it is important to include more detailed information on adjustment levels. Therefore, this information was now added to Table 1 for urban and rural studies and in Table 3 for studies investigating physical activity-, food environment and residential noise.
Reviewer #2: This is an important publication for the field documenting the intersection between diabetes and the built environment. The methods were appropriate and well documented for this review and meta-analysis.

Response:

We thank the reviewer for the time and effort invested in reviewing the manuscript and for the positive remarks.

As many off the journals addressing the built environment (e.g., cartography, ESRI, etc.) may not be a part of the traditional peer-review literature this an important caveat to add to this publication. the caveat would facilitate further inquiry by researchers seeking to explore this area of inquiry.

Response

We thank the reviewer for the suggestion of adding non-traditional/non-scientific sources to our literature search. However, we think peer-reviewing is a quality assurance system, which results in publications without peer-review often scoring weak in quality assessment. This led to exclusion of non-peer-reviewed publications. We decided therefore prior to the study not to include grey literature in the primary search, as we stated in the protocol. Our search strategy was developed with great care and with the help of an information specialist and conform the PRISMA guidelines.

In addition, we performed a reference search in the included papers, which increased our chances of finding other relevant publications. Also, we now updated our search until July 2017, which resulted in including 26 additional papers, adding up to 109 studies included in our systematic review. This shows that the present review covers a vast range of publications in the field.

We hope that the reviewer can agree with these arguments of not including non-peer reviewed sources in our literature search.

Lastly, given the small numbers of publications meeting the criteria, the conclusion section should reflect a statement on the need for further research.

We thank the reviewer for this comment. As we stated in the discussion, there is a need for more homogeneous research, not necessarily more research. As is clear, a large body of evidence is
available, and we gained novel insights from this review. However we argue that future research could be of added value if the identified limitations are addressed; especially with regard to more homogeneity in definitions and taking potential confounding variables into account. In addition, more in-depth explorations of the pathways through which environments affect diabetes risk can help in gaining a better understanding of underlying mechanisms.

We added the following passage to our future conclusion paragraph on page 14

“Future research should focus on developing a more homogeneous definition of environmental characteristics, particularly in relation to the food environment. Also more in-depth explorations are necessary of the pathways through which environments affect diabetes risk, while taking the potential confounding variables into account.”

References


