Author's response to reviews

Title: The association between sedentary behaviour and risk of anxiety: A Systematic Review

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Author's response to reviews: see over
Editor, *BMC Public Health*

**Date**, April 15th, 2015

Dear Editor,

We have revised our manuscript “The association between sedentary behaviour and risk of anxiety: A Systematic Review” in accordance with reviewer’s suggestions, and are pleased to re-submit this manuscript for your further consideration. We would like to thank the reviewer’s for their thoughtful comments, which we feel have helped to significantly strengthen the manuscript. We have attached our response to reviewers comments below.

The paper is original, has not been previously published either in whole or in part, and is not under consideration for publication elsewhere. The manuscript involves no conflicts of interest, has been read and approved by all the authors, and the requirements for authorship have been met.

We look forward to hearing from you about the outcome of this submission.

Yours sincerely

Dr Megan Teychenne
Response to reviewers

Reviewer 1

1. In general, the one thing I would recommend is to pay a little more attention to the diversity of the studies in terms of their population in the discussion section. Besides, two studies (Dolenc & Pisot and Kroeders) are very different from the rest as they are conducted in a clinical/hospital setting. Some specific mentioning might do.

We have considered this further and agree that the Reviewer makes a good point. We have since omitted these studies. Please see response to Reviewer 3 (Point 4) and Reviewer 2 (Point 2) for further clarification and rationale.

Minor Essential Revisions

Abstract

1. Line 44: ‘)’ after ‘databases’ should be deleted

This has been amended.

2. Lines 44-45: I think there is one paper missing here. Nine observational + one intervention makes 10 studies while the authors included 11

This has now been amended, as follows;

“A total of ten observational (eight cross-sectional and two longitudinal) and one intervention study were identified.”

3. Line 47: could the authors add a sentence on the meaning of the positive association? (as done in the main findings, Line 205)

We have now included this, as follows;

“However, our findings suggest a positive association (i.e. anxiety risk increases as SB time increases) may exist”

4. Line 51: should ‘dependant’ be ‘dependent’?

This has been amended.

5. Line 51: should ‘sedentary behaviour’ be ‘SB’?

This has been amended.

6. Line 58: I am not sure whether the key word ‘lifespan’ is really appropriate. In total, the word has been used twice in the entire manuscript and there is no elaboration on youth versus adults or children versus adolescents.

We agree – We have now removed this key word.

Background

7. Line 73: should ‘;’ after ‘[8]’ be deleted?

This now been amended

8. Lines 73-74: I think it should be ‘AND poor psychological well-being […]’

This now been amended

9. Lines 81-82: and what about the findings related to self-esteem? The authors only include a summary on association with depression while in the same sentence, self-esteem has been mentioned too
We have now included a summary which captures the balance of evidence on depression as well as self-esteem, as follows;

“on the balance it has been suggested that a positive relationship exists between most sedentary behaviours and depression [11] and self-esteem [8].”

10. Line 111: it should be ‘anxiety IS unclear’
   This has now been amended

11. Line 112: is the word ‘for’ redundant?
   This has now been amended

Methods
12. Line 117: please include a rational for searching from 1990 onwards

We have now included a rationale, as follows;

“since the 1990’s saw an increase in sedentary behaviour levels of the population with the widespread use of online technology [26]”.

13. Line 118-119: should it be ‘PsychINFO, AND SPORTDiscus’?
   This has now been amended

14. Line 124-125: there should be a hard return between the two sub-sections
   This has now been amended

15. Line 134: could the authors please explain in the text what ‘domain’ is referring to? (I know it refers to SB, but for any reader it would be easier to specify here).
   This has now been included, as follows;

   “.domain (e.g. leisure time sitting, occupational sitting, total sitting)”

16. Lines 138-154: could the authors please explain in which way they have modified the EPHPP? The original format as I know it has 8 components (A to H).
   Thank you for this – We have now amended this typo. It now reads as an eight-component rating scale throughout. Further, we have now included information on how the tool was modified, as follows;

   “In regards to the observational studies that were assessed, the tool was modified so that those studies were not scored on 1) the blinding component or 2) other intervention-specific criteria within any other components”

17. Lines 138-154: it would be interesting to see how each study scores on each component. Have the authors considered to add an overview of the complete quality/risk of bias scoring?

We have tabulated these scores and have attached them as ‘Table 2’ in the document. We have also referred to this Table in the results section, as follows;

“All of the studies were missing essential information regarding the methodological quality. For example, only three studies included reliable and valid measures for both sedentary behaviour and anxiety [35, 38, 39], and only two studies demonstrated a high retention rate (i.e. 80-100%) [33, 36]. Further detail of the scoring of methodological quality of each study is provided in Table 2 (see Additional file 1).”
18. Line 148-149: please make sure to replace the squares with appropriate symbols

We have now replaced the square brackets with round brackets as suggested

**Results**

19. Lines 159-160: I don’t quite understand why the authors choose to report % and I am not sure whether they are accurate (i.e., 2 of 11 = 18%, 1 of 11 = 9%)

**Percentages have now been removed from this section**

20. Line 163: I think the word ‘and’ after ‘adolescents’ should be deleted

**This has now been amended.**

21. Line 194: The authors report that ‘all of the studies were missing essential information regarding the methodological quality’. Have they considered contacting the respective authors for additional information? And if not, why? I understand that this is time consuming, but as the review includes 11 papers only and ALL of them seem to miss essential information, it seems desirable and workable to try to obtain this.

We have now contacted authors in regards to this additional information and amended the paper where relevant. Further, we have included the following description of the procedure in the methods section (methodological quality);

“If a component was not described, authors of those studies were attempted to be contacted to provide this information. In the instance that authors of those studies did not respond, the (undescribed) component was given a weak rating.”

22. Line 210: should ‘dependant’ be ‘dependent’?

**This has now been amended.**

23. Line 216: the author name should be spelled with a G instead of a Q: UIJTDEWILLIGEN

**This has now been amended.**

**Discussion**

24. Line 298: consider to place ‘(aged over 10 years)’ right after ‘children’ on the previous line. It now reads odd; adults are always aged over 10 years…

**This has now been amended.**

25. Line 327-328: it is somewhat confusing that the authors state ‘in particular sitting time and television viewing’ as sitting time can include television viewing as well. Could this be rephrased?

We have now re-phrased this, as follows;

“…on the balance this evidence suggests a positive association may exist between sedentary behaviour (e.g. general sitting time and television viewing specifically) and anxiety risk.”

**Abbreviations**

26. Line 332: ‘behavior’ should be ‘behaviour’

**This has now been amended**

27. Should abbreviations like EM/SDI (see results section), be included here too?

**This has now been amended**
In text, it is not explained that additional records were identified through other sources. Please include

This has now been included in the methods section (under ‘Search strategy’)

1. Table 1 - could the authors include the respective reference numbers in the ‘Paper’ column?

These have now been included

2. last column: the ‘l’ from ‘methodological’ is placed on the second line, please
Change

This has now been amended

3. row Granner et al: it should be ‘Caucasian’

This has now been amended

4. last row: the author name should be spelled with a G instead of a Q: UIJTDEWILLIGEN

This has now been amended

Reviewer 2

Major concerns
1. The authors have evaluated the methodological quality of each study using a tool assessing six methodological components. However, it remains unclear why did assessed the quality. In the description of the results, or the conclusions about the evidence on the association between sedentary behavior and anxiety, the methodological quality has not been taken into account. In this context, I also wonder how the authors came to a conclusion. The use of a best evidence synthesis could have been applied, or would be strongly recommended.

We thank the reviewer for this insightful suggestion, and as such have now applied a best-evidence synthesis for the review. We based this upon previous reviews (Proper et al, 2011; Chinapaw et al, 2011) and have amended relevant sections (methods, results, discussion, abstract), as follows;

Methods - Page 7-8:
“Best-evidence synthesis
In order to draw conclusions on the association between sedentary behaviour and anxiety risk with regards to the methodological quality of studies, a best-evidence synthesis was conducted, which was based on previous systematic reviews in the area of sedentary behaviour and health outcomes [8, 12]. As such, three levels of evidence considered:
1. Strong evidence: defined as consistent (i.e. at least 75% of studies show results in same direction) results in ≥2 high quality studies
2. Moderate evidence: defined as consistent results in one high quality study and at least one weak quality study; or consistent results in ≥2 weak quality studies
3. Insufficient evidence: defined as having only one available study; or inconsistent results in ≥2 studies.”

Results –
Page 10: “Since one strong [33], three moderate [35, 37, 38] and three weak quality studies [31, 34, 39] demonstrated at least one positive relationship between sedentary behaviour and anxiety risk, based on the best-evidence synthesis, there was moderate evidence for the overall relationship between sedentary behaviour and anxiety risk.”

Page 10: “Based on the consistent findings of the three moderate- [35, 37, 38] and one weak-quality [39] studies, the best-evidence synthesis demonstrated there was moderate evidence for the positive relationship between sitting time and anxiety risk.”
Page 11: “Based on the inconsistent findings of the one strong- [33], two moderate- [32, 36] and one weak-quality [34] studies, there was insufficient evidence for the relationship between screen time and anxiety risk.”

Page 11-12:” However, based on the inconsistent findings of the one moderate- [38] and two weak-quality [31, 34] studies, there was insufficient evidence for the relationships between television viewing and anxiety risk.”

Page 12: “Based on these inconsistent findings there was insufficient evidence for the relationship between computer use and anxiety risk.”

Discussion –

Page 12: “In other words, there is moderate evidence suggesting that engaging in overall sedentary behaviour (namely sitting) was linked to an increased risk of anxiety.”

Page 13: “However, in our review, based on the best-evidence synthesis and when considering the different types of sedentary behaviour separately, moderate evidence was found for the positive relationship between sitting time and anxiety risk, whilst inconsistent evidence was found for the relationship between screen time, television viewing time and computer use and anxiety risk.”

Page 15: “on the balance this evidence suggests a positive association may exist between overall sedentary behaviour (e.g. sitting time specifically) and anxiety risk, whilst inconsistent evidence remains for other types of sedentary behaviours (e.g. computer use, television viewing, screen time) and their link with anxiety risk.”

Abstract –

Methods: “Methodological quality of studies was assessed and a best-evidence synthesis was conducted.”

Results: One cross-sectional study demonstrated a strong methodological quality, five cross-sectional studies demonstrated a moderate methodological quality and three studies (two cross-sectional one longitudinal) received a weak methodological quality rating. Overall, there was moderate evidence for a positive relationship between total SB and anxiety risk as well as for a positive relationship between sitting time (only) and anxiety risk. There was inconsistent evidence for the relationship between screen time, television viewing time, computer use and anxiety risk.

Conclusions: “Limited evidence is available on the association between SB and risk of anxiety. However, our findings suggest a positive association (i.e. anxiety risk increases as SB time increases) may exist (particularly between sitting time and risk of anxiety).”

2. Another concern relates to the inclusion of an experimental study. As the aim is to study the association between sedentary behavior and anxiety, I would not include an intervention study, except for when analyses had been done in the control group, or when baseline data of the total group had been used. Otherwise, the effect of the intervention on anxiety would have been studied, and not primarily the relation between sitting and anxiety.

We have taken this on board and have now omitted the one experimental study from the review, given the reasons outlined by the reviewer. We have outlined this briefly in the reasons for exclusion in Figure 1.

3. Finally, in the introduction, but also in the discussion, the authors have referred to literature about the relation between sedentary behaviors and various health outcomes. However, the authors describe a too ‘positive’ association, by only referring to studies that showed an association between sitting and adverse health outcomes. I miss a couple of systematic reviews in this field that have concluded either inconclusive, insufficient evidence or limited evidence for sedentary behavior being a risk factor for certain health outcomes.

We have now included discussion of a number of systematic reviews (Proper et al, 2011; Thorp et al, 2011; Chinapaw et al, 2011) to provide a more balanced conclusion of the evidence between sedentary behaviour and health outcomes, as follows;

Introduction:
“Among the general adult population some evidence suggests sedentary behaviour is associated with increased risk of developing various chronic diseases (e.g. overweight and obesity [4], cardiovascular disease [5], osteoporosis [6], type two diabetes [4], and various cancers [7]). However, reviews of prospective studies suggest that there is yet insufficient or no evidence to conclude relationships between sedentary behaviour and certain health outcomes (e.g. adulthood weight gain, cardiovascular disease risk and some cancers) [8, 9]. A range of health consequences of sedentary behaviour in children and adolescents have also been identified (e.g. unfavourable body composition, decreased fitness, lowered scores for self-esteem and prosocial behaviour and decreased academic achievement [10], sleep problems, musculoskeletal pain, depression, and poor psychological well-being [11]). However, like that of the adult literature, reviews of prospective studies in young people suggest that there still remains insufficient evidence to conclude associations between sedentary behaviour and some health indicators (e.g. body weight, blood pressure, bone mass) [12].”

Minor comments

4. The authors apply the term risk of bias and methodological quality. These terms imply the same, where the quality items refer to items that indicate the risk of bias: a high methodological quality implies a low risk of bias.

We have now amended this and included only the term “Methodological quality” in the headings and related sections

5. In case the component was not described, the authors decided to give a weak rating. Instead, authors of the papers could have been contacted.

Thank you for this suggestion - we have now addressed this. Please see Response to Reviewer 1 (point 21)

6. Lines 148-149. The symbols are not readable.

We have now amended this

7. Line 216. The author name (ref 37) has not been written correctly.

We have now amended this

8. The results were described separately for screen-time (including TV, computer and electronic games), TV viewing, and computer use. I got confused in the reporting of the studies on TV viewing and computer use. Where were they described? Moreover, in the paragraph about TV viewing and computer use, there were two references (31 and 36) that were not described in the paragraph on screen-time, i.e. TV viewing and computer use. Please, explain this.

We have now clarified the discussion of screen time in the results and included further description of all relevant studies in that section, as follows;

Screen-time (i.e. combined TV, computer and/or electronic games):

“Of those, two cross-sectional studies (one in a sample of adolescence and one in adults) showed a positive association between screen-time (i.e. combined TV and computer use) and anxiety risk [30, 31].”

“In contrast, one cross-sectional study [29] showed an inverse association between screen-time (i.e. combined TV, computer and electronic games use) and anxiety risk amongst 5-year old girls”

“However, Sanchez-Villegas et al. [33] found no association between self-reported TV/computer use time and anxiety (defined as previous or current diagnoses of anxiety from a health professional).”

9. Lines 290-299 are about the link with depression, i.e. another mental disorder, and in my opinion not relevant in this section.

We have now removed this, as suggested.
Reviewer 3

Major Compulsory Revisions

1 – It is nice that the authors introduced potential mechanisms for the anxiolytic effects of sedentary behavior; however I think this should be extended in an important way. Specifically, it is important to consider how these acute effects of sedentary behavior on mood (e.g., distraction, CNS arousal, sleep) may result in more chronic/long-term impacts on anxiety symptoms or risk of anxiety.

We have now included discussion of this in the introduction (page 4-5), as follows;

“Given the plausibility of these short-term effects of sedentary behaviours on mood (i.e. distraction, CNS arousal, sleep), it is likely that the cumulative effects of these behaviours may further result in longer-term impacts on anxiety risk.”

2 – Selection Criteria: Please clarify the definition of “risk of anxiety” used for the purposes of this review. Does this encompass likelihood of developing a disorder and non-clinical anxiety symptoms?

We have now included a definition of “risk of anxiety” in the selection criteria section, as follows;

“For the purpose of this review, risk of anxiety was defined as either the likelihood of developing or experiencing an anxiety disorder or non-clinical anxiety symptoms”

3 – Please clarify what is meant by ‘domain’ in the first sentence on page 6: “Key study characteristics of the identified studies were extracted….”

This has now been clarified as follows;

“(e.g. leisure time sitting, occupational sitting, total sitting)…”

4 – Was any consideration taken to separate patient vs non-patient samples or were both included in the review? Can these effects be combined meaningfully?

This is a very valid point. After considering this, we have decided to omit the one study (Kroeders et al.) that included patient samples as we agree with the reviewer that the associations between sedentary behaviour and anxiety may be confounded by other prominent factors. We have now outlined that only “healthy” populations were included (in the Study Selection Criteria section), as follows;

“…examined ‘healthy’ children, adolescents or adults (i.e. those who were not patients suffering from underlying chronic physical conditions that may confound results)”

Minor Essential Revisions

1 – There is an extra “)” ending the first sentence of the methods section of the abstract.

This has now been amended

2 – the word ‘inverse’ should be included in the first sentence on page 5: “Although research has indicated an association between physical activity and anxiety may exist.”

This has now been included
3 – Two of the symbols appeared as empty boxes in the ‘risk of bias and methodological quality’ section. Please insert the appropriate symbol.

This has now been amended

Discretionary Revisions

1 – I’m not sure if this would add any relevant literature, but it may be worth including “anxiolytic” into the search terms?

We have now run a separate search using the term “anxiolytic” which retrieved no further relevant studies. Thus, we have chosen not to include this term in the final review.