

MJ - Decision on
Manuscript ID
BMJ.2016.035517

Body:

Dear Dr. Celis-Morales

Manuscript ID BMJ.2016.035517 entitled "Active commuting is associated with lower risk of cardiovascular events: Evidence from a prospective cohort study of 264,337 UK Biobank participants"

Thank you for giving us the opportunity to consider this paper, which we discussed at our editorial meeting ***. We are pleased to make a provisional offer of publication if you are able to revise it to address the points made by the referees and the editors. The referees' comments are available at the end of this letter, and the points raised by the editors are set out below.

We hope that you will be able to revise the paper and send it back to us within one month. When you resubmit, could you kindly ensure that you provide:

- (a) A covering letter outlining how you have responded, or not responded and why, to both the referees comments and those of the editors.
- (b) A word count (excluding the references and words in boxes and tables). You should aim to keep this count below or very close to 2000 words.
- (c) Please check that all the information required in the manuscript (see note below) is included in the revised manuscript.

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I hope you will find the comments useful.

Best wishes

Yours sincerely

Rubin Minhas
Dr Rubin Minhas
BMJ Associate Editor
rm1000@live.com

Manuscript Meeting
Chair: Wim Weber
Doug Altman, John Fletcher, Joe Freer, Georg Roeggla, Rubin Minhas

Some people may have retired - were these people included or how were they dealt with?
It would be desirable to see data for walking and cycling separately, and also to see journey time analysed if this is not overly onerous.

Adjustment for leisure time activity preferable to sedentary behaviour.

The term "cancer event" is undefined and unfamiliar to cancer methodologists.

Need to be wary of saying no association when they mean no significant association.

What are the policy implications of this?

Decision:

Put points

INFORMATION TO INCLUDE IN REVISION

Please would you also check that you have provided the following information

* Competing interest statement (in the style explained at <http://www.bmj.com/about-bmj/resources-authors/forms-policies-and-checklists/declaration-competing-interests>)

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Reviewer(s)' Comments to Author:

Reviewer: 1

Recommendation:

Comments:

Reviewer: Lars Bo Andersen

This study analyze a unique database from the UK Biobank with more than 250,000 participants. As such the study is unique. The study analyses all cause mortality and CVD and cancer specific mortality beside incidents of cancer and CVD. It is also a strength that they can analyze data with a short follow-up time making misclassification caused by changes in exposure during follow-up minimal. I consider this study very strong. However, there are also a number of limitations in the analysis. Many studies have confirmed that the benefit of cycling is substantial bigger compared to walking and with this huge database authors have sufficient statistical power to present the two active commuting modes separately. Also, I don't quite understand why they claim that mixed mode transport such as walking and public transport cannot be analyzed, because it was done in the article of Flint and Cummings based on UK Biobank published earlier this year in Lancet Diab Endocrinol.

Minor comments

I think the models should be adjusted for leisure-time physical activity rather than sedentary behavior or both (there is usually not co-linearity).

It would be interesting to see how distance in people commuting to work relates to outcomes and correlates. You must have distance because you have residence and work addresses.

How many had accelerometer measurements? Can a sensitivity analysis be done using only these and with adjustment for PA? How close was the association between objectively assessed PA and the different self-reported PA domains?

CRF was assessed in >75,000 participants. This is a large sample and could be analysed according to active transport.

You adjusted models for BMI. Did you adjust for BMI as a categorical variable? BMI and all cause mortality is U-shaped.

You wrote: The models for CVD events and CVD mortality were run excluding participants with a history of myocardial infarction, angina or stroke at baseline; similarly models for cancer were run excluding participants with a cancer diagnosed at baseline.

I believe models of all cause mortality should be with exclusion of all with chronic disease at baseline, because disease could be related to exposure and it is definitely related to mortality. It could be questioned if participants with diseases where an association with transport mode should not be expected, could be included and then adjust for the condition (such as hypertension and T2D without serious complications).

It is surprising that more women use active transport. My guess is that this is only the case for walking and not cycling. If there is interaction here, you need to make the analysis stratified on transport mode. I would also like to see if difference in parameters such as fitness differ between cycling and walking.

Page 11, line 39. The 30% decrease is supported by Wathews et al 2007. The review by Oja et al 2011 also supported both the relationship with all cause-, CVD, and cancer mortality. Again, it's a guess because you haven't presented data, but if you analyze cycling and walking separately, you may find support for an association for cycling where you found nothing for total active transport. Your results are not in disagreement with Matthews et al, where no association was found for walking but a clear benefit for cycling.

Page 12, top. The meta-analysis had heterogeneous outcomes, but this was in my view not the main problem. It was rather the exposures where environments differed from study to study, which means that the proportion of cyclists compared with walkers differed.

Page 12, line 51. The association between transport mode and SES in the referenced study (by me) was relevant in the early 1980s where baseline measurements were done. Today it is reversed, so higher SES is more likely to cycle (and walk). The higher prevalence of active transport in low SES groups in UK is more likely caused by lack of infra structure and promotion of especially cycling. It is not comparable to Denmark. It is not wrong to cite the Danish study, but it has clearly been shown in Denmark that the key determinant of cycling is not SES. Cycling has increased 50% in Copenhagen since 1998 and it is mainly

high SES individuals who have changed habits.

Additional Questions:

Please enter your name: Lars Bo Andersen

Job Title: professor

Institution: Sogn and Fjordane University College

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

Fees for consulting?: No

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If you have any competing interests (please see BMJ policy) please declare them here: none

Reviewer: 2

Recommendation:

Comments:

Thank you for the opportunity to review this manuscript which explored the temporal associations between active commuting and CVD events, CVD mortality, cancer events, cancer mortality and all-cause mortality. The findings revealed that active commuters were less likely to have a CVD event or die from CVD. The manuscript further explored socio demographic correlates of active commuting.

As the authors note, this study is strengthened by its very large, representative sample, the fact that it controlled for a wide range of potential confounders and that it explored cancer and CVD (both events and mortality).

There are a number of limitations of this research however that I feel the authors don't adequately discuss. The first is the crude measure of active commuting. Previous work in the field is strengthened by the fact that the measure of exposure tends to be minutes spent in active commuting in a typical or past week. Not only do such measures provide a more accurate measure of exposure they are important for establishing whether there is a dose-response relationship (consistent with Hill's Criteria). Further, the measure did not allow for multi-modal journeys to be detected. As the authors allude to briefly in the discussion, because someone selects public transport or car as their 'main' mode of transport does not mean it is their only mode of transport. In fact, they may have accrued substantial and meaningful walking or cycling as part of their journey but because the total distance travelled is great, it was not the main mode of 'transport' used. I realise this limitation cannot be addressed easily but it should be clearly outlined as an important limitation of the research.

To my mind the second major limitation of this research is that analyses did not control for total or leisure time physical activity nor fitness. It is therefore highly possible that the associations that are seen reflect the fact that active commuters participate in greater overall physical activity (as the authors found). Controlling for other non-commuting PA is crucial for determining the specific impact that active commuting has on health. This should be addressed by the authors.

Because of these substantial limitations I think the authors need to temper their statement in which they write 'provides the most robust assessment of the association between active commuting and CVD outcomes'.

A couple of other suggestions (observations):

The secondary aim to identify correlates of active commuting seems like an afterthought. No information is provided in the introduction and the method section seems focused on the first research question (no mention is made in the statistical analyses section and therefore the methods used to derive at the Figure are not clear). It is not clear why the papers focuses on two distinct research questions. Could the authors please revise the manuscript so that the exploration of socio-demographic and health correlates becomes a more legitimate focus, or alternatively remove it from the manuscript?

I don't think the title accurately reflects the study. A mentioned aim was to explore the associations with cancer and this should be alluded to (whether the findings were significant or not).

A possible explanation for the null findings that the authors did not allude to has been discussed in previous studies. That is, that in contrast to countries like China and Denmark with high rates of active travel (and where positive associations are often reported), the population levels of active commuting in England are not done at a level sufficient to provide health benefit. Given the crude measure of active commuting used, this could not be explored but it is a legitimate explanation.

Minor comments

In the Discussion the authors write that 'because daily active commuting could be an important contributor to physical activity'. Much research exists to suggest that this more an 'is' than a 'could' and this evidence

should be referenced.

The authors also write that, For people with longer commutes.....'. This assertion does not seem appropriate in the context of these findings given that these people would be categorized as traveling by car and therefore, based on the data, not at a reduced risk of CVD.

There appears to be a typo on line 52 in the Introduction

Reviewer Name: Dr Shannon Sahlqvist

Additional Questions:

Please enter your name: Shannon Sahlqvist

Job Title: Lecturer

Institution: Institute for Physical Activity and Nutrition (IPAN), Deakin University

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

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If you have any competing interests (please see BMJ policy) please declare them here: None declared

Reviewer: 3

Recommendation:

Comments:

MS ID: BMJ.2016.035517

Title: Active commuting is associated with lower risk of cardiovascular events: Evidence from a prospective cohort study of 264,337 UK Biobank participants.

Thank you for the opportunity to review this interesting manuscript. In this paper, the authors have explored prospective data to examine the relationship between active commuting and incident cardiovascular disease, cancer and all-cause mortality in adulthood. The authors have used data from the UK Biobank.

The use of prospective data to explore the possible relationship between active commuting and health outcomes is to be welcomed, and on the whole I believe this paper adds to the current evidence to explain why active commuting is associated with lower risk of cardiovascular outcomes. I do however have a few suggestions for the authors to improve this paper, and these are presented below.

Introduction

1) A strong case is made for considering the association of active commuting on the risk of health outcomes, independent of major potential confounders. I would however expect to see further explanation as to whether the association is modified by other domains of physical activity (leisure time, occupational and household) or cardiorespiratory fitness. I think the authors want to say something like Active commuting contributes to overall physical activity levels thus might make an important contribution to the health benefit attributed to physical activity.

Methods

2) How exactly was the active commuting questionnaire validated – please provide more detail. Was it validated with objective measurement?

3) Please clarify if the measure of self-report physical activity is confined to leisure activities only and not work. One limitation of using a self-reported questionnaire is that reliability and validity may change as the population become better educated about the benefits of physical activity, a wide range of ages, and social desirability influences their responses.

4) Please describe how many participants are asked to wear an accelerometer for the assessment of physical activity. What is the relationship between subjective and objective measures of physical activity?

5) It is unclear if the mode of travel is the main mode or only mode.

6) The authors should provide information on whether walking and cycling distance assessed. If this was not possible from the questionnaire, then the authors may like to state this limitation.

7) In the analysis section, the authors indicated that four incremental models were conducted, but only explained three (models 0, 1 and 2). The last one is missing (model 3).

8) In the results section, how does controlling for additional variables in the model 3 in Figure 2 and Table

S1? Does the model 3 is adjusted for model 2 plus physical activity and cardiorespiratory fitness? If yes, please clarify in details. It is important to know whether active commuting affects the health outcomes exclusively via some major confounder factors or also via physical activity and cardiorespiratory fitness. 9) In Figures 1 and 2, please do not present the same data in both a figure and a table.

Discussion

10) I would expect the authors to strength the discussion concerning the effects of physical activity and cardiorespiratory fitness on active commuting. Current study did not deal with the two variables in the incremental model, which could contribute importantly to adult's active commuting. If the physical activity and cardiorespiratory fitness have also been adjusted, will the same patterns be observed in Figure 2 and Table S1? The question will be how important the physical activity and fitness are to do with active commuting?

Additional Questions:

Please enter your name: Xiaolin Yang

Job Title: Adjunct professor

Institution: LIKES Research Centre for Physical Activity and Health

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

Fees for consulting?: No

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Reviewer: 4

Recommendation:

Comments:

Overall this was a well written, strong study showing solid evidence for the relationship between active travel and CVD. I have a few recommendations and suggestions for clarification.

Abstract: line 26 Theses? These? This?

Introduction: a mention of the relationship of mental health/psych well-being and active travel is worthwhile since stress is associated with CVD risk

Methods

Why is public transportation classified as as a non-active travel mode? There is literature supporting it in both directions- provide a rationale why you opted to classify it as non active travel.
p5 line 8: how was mode of commute assessed? More details are needed. Was the number of trips/week by mode assessed? was it a "most of the time" categorical question?
The inclusion of dietary outcomes was curious. What is the hypothesis behind this? Is there other literature to support investigating this relationship?
Sedentary behavior is mentioned in the results- was this captured in the IPAQ or the accelerometer data?
What outcomes came from the accelerometer data?
how many people wore the acceleroemters?
p6 line 28 finesse? fitness?

Discussion

The finding that women were more likely to be active commuters was interesting and contrary to many of the other studies - the authors should comment on this.
Please expand on the implications of the study- how could the information be used? Community, environment or policy approaches to address the findings?

Additional Questions:

Please enter your name: Melissa Bopp

Job Title: Associate professor

Institution: Pennsylvania State University

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

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