PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Active Travel Behavior in the Family Environment: Protocol for the Mixed-Methods Cross-Sectional ARRIVE Study
AUTHORS	Reimers, Anne; Marzi, Isabel; Beck, Franziska; Engels, Eliane; Renninger, Denise; Buttazzoni, Adrian; Krieger, Claus; Demetriou, Yolanda

VERSION 1 – REVIEW

REVIEWER	Stewart, Tom Auckland University of Technology, Faculty of Health and Environmental Sciences
REVIEW RETURNED	19-Sep-2021

GENERAL COMMENTS	This is a protocol paper for a study titled Active Travel Behavior in the Family Environment (ARRIVE). The paper is very well written and was a pleasure to read. The authors do a great job to build the rationale for performing the study. I particularly like the focus on non-school destinations, and the interaction between parent and adolescent perspectives. I have several specific comments below, which you may find useful (in order of appearance): P3L82: Consider using en dashes for number ranges. P4L98: Providing some examples of 'Family environment predictors' would be helpful for the reader. Does this encompass parental decision making, or are these predictors broader? Is this the same as 'family context predictors' mentioned later in the introduction? P5L120–124: This is a very long sentence. P5L136: Suggest deleting the word 'exist'. P6L145: I was thinking the study protocol would benefit from explicit aims, but I see these are listed lower down in the methods section. P6L157: What is the rationale for selecting these four destination types? Are these exhaustive and mutually exclusive? P8L192: Does this panel only contain people living in Germany? P8L192: Although the panel (as a whole) may be representative of the German population for key sociodemographic factors, I wonder whether it is representative of travel behaviours in Germany (as travel behaviours are obviously a result of many factors other than sociodemographic ones). This also comes down to who you are planning to generalise these results to. Another limitation (which is not identified as a limitation of the study design) is that this sampling strategy is not designed to maximise the variability of your exposure variables (e.g., individual factors and physical environmental factors). One of the key attributes of the stronger studies in this area is their multistage sampling strategies, which are used to maximise heterogeneity in

the environmental variables (e.g., purposely selecting participants from a range of different environments). If your sample share very similar individual and environmental characteristics, it will limit the strength and clarity of associations you will be able to observe.
P8L204: Your sample size calculation is currently not replicable. If using G*Power, you should state the name of the model chosen, and the reason for selecting it. An unconditional/random multiple regression model is much more common (compared to a fixed X model) as you generally don't know the values of your predictors while planning the study. Keep in mind that if you are planning to adjust for any covariates in your analysis (i.e., in addition to the predictors of interest), the required sample size will likely increase.
P8L213: Please ensure consistency between 'child' and 'adolescent' as these terms generally refer to distinct age ranges.

In general, the data analysis section is overly simplistic. One of the main reasons to publish a study protocol is to be upfront with your data analysis plan, and the hypothesis tests you wish to conduct.
P9L231: Differences between groups will be examined. What variables are you comparing between the groups? What are the independent variables?
P9L235: What does the 'overall score' represent? Is this the total number of active trips performed within a given period? Or the proportion of total trips travelled by active modes?
P9L234: Are all analyses going to be stratified by destination type? Would it make more sense to examine how the relationship between the predictors and active travel vary by destination type? This seems to match the intention of the first aim. This would obviously mean you have repeated measures per subject and would require a different method of analysis.
Are all analyses stratified by gender, and by parent/adolescent (as this was inferred in the participants section, but is not mentioned in the analysis section)? Gender was also identified as a moderator in the aims section (so there are several discrepancies throughout).
P9L237: SEMs generally require significantly more subjects to estimate (compared to a linear model), particularly for structures as complex as illustrated in Figure 1 (which I'm sure is an oversimplification of the actual variables measured). You could perhaps position this as an exploratory analysis or justify your sample size based on this analysis.
P9L240: What are the variables of interest in this correlation analysis?
P10L256: I understand you will identify the sample for the qualitative component later. It might be useful to say whether they will be selected from the same panel as the quantitative component, or will they be recruited via a different channel? Will you still recruit one adolescent from each household?
P12L311: The qualitative analysis plan seems brief (i.e., you refer to your research questions, but I don't recall reading explicit research questions). I'm wouldn't say I'm an expert in qualitative methodology, so maybe this is all that is required.
P13L345: The sentence '…intended to provide the necessary empirical evidence to illustrate the influence of family context influences…' comes across as a bit strong, particularly for a single cross-sectional study. You will (hopefully) be able to contribute evidence around factors that are associated with travel behaviours.
You have two reference lists (and each of them is different). I'm not sure if this is a problem.

REVIEWER	Niederseer, David
	University Hospital Zurich, Department of Cardiology
REVIEW RETURNED	13-Oct-2021

GENERAL COMMENTS	With great interest I read the protocol for the Mixed-Methods Cross-Sectional ARRIVE Study. In general, I really like this study protocol. Congratulations! Looking forward to the results of this study. I have several minor aspects that need further attention.
	I would suggest shortening the introduction section of your manuscript substantially. Put this material into the discussion section. I would suggest the discussion section to be as long as your current introduction and the introduction as long as your current discussion.
	As in many protocol papers, please provide a table with a summary of previous studies in this field. You submitted this protocol to a medical journal. I fully agree that the topic you address with your study is of utmost medical importance. However, the content of your study uses more psychological and behavioral science methods than medical ones. Why did you not include some medical aspects like childhood obesity, cardiovascular risk factors, etc. Or measure physical fitness objectively? I feel this is a missed opportunity. You mention this in the limitations, however, please expand on this major limitation. Gamification is used often to increase physical activity. I would at least include this aspect into the interviews. E.g. Is a smartwatch that counts your steps motivating you for active travel behavior? You did not reference the recently published guidelines of the ESC on prevention that also address this issue. https://academic.oup.com/eurheartj/article/42/34/3227/6358713 (especially in the supplemental section). Please also reference and discuss the GISMO study. https://onlinelibrary.wiley.com/toc/16000838/2020/30/S1 Please also cite and discuss this document. Mozaffarian D, et al. Population approaches to improve diet, physical activity, and smoking habits: a scientific statement from the American Heart Association. Circulation 2012;126:1514_1563.

REVIEWER	De Angelis, Marco
	University of Bologna, Department of Psychology
REVIEW RETURNED	18-Oct-2021

GENERAL COMMENTS	It is unclear whether participation implies a reward or not is a child selected at random or whose name appears first in the alphabet to fill out the adolescent portion of the survey? not clear. The possibility of people adopting a mutlimodal travel chain (e.g., bike + train; walk + bus + walk) does not appear to be considered. Is there a rationale for this that remains unclear?
	In the event that they do not remember (or have not actually made) a trip to the same destination by another mode of transportation, what is the alternative strategy? please specify.
	In the qualitative analysis, how many researchers will be involved? it is unclear the coding process and whether you have considered including an additional figure if consensus is not reached.

| | Perhaps it makes more sense to talk about perceived urbanization.

are there any broad assumptions about this? for example, you have described the degree of parental influence on adolescents but what kind of impact should we expect? do you think it is more of a parental issue to incentivize the use of active modes? in case it is related to the perception of the environment, do you think the measures taken are sufficient? it does not seem clear from the text, please be as clear as possible since this is a cross-sectional mixed-method study design.

Do you think it might be useful to include a dimension that asks parents the primary reason for choosing current residence? Some work by Zhou,J. (2012) may help clarify. |
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Mr. Tom Stewart, Auckland University of Technology
Comments to the Author:
This is a protocol paper for a study titled Active Travel Behavior in the Family Environment (ARRIVE). The paper is very well written and was a pleasure to read. The authors do a great job to build the rationale for performing the study. I particularly like the focus on non-school destinations, and the interaction between parent and adolescent perspectives.
Thank you.

I have several specific comments below, which you may find useful (in order of appearance):
P3L82: Consider using en dashes for number ranges.
Thanks for pointing this out. We changed this throughout the manuscript. With regard to referencing we still use hyphen to report on sequences of consecutive numbers as requested in the BMJ Author Hub ("For sequences of consecutive numbers, give the first and last number of the sequence separated by a hyphen...."; https://authors.bmj.com/writing-and-formatting/formatting-your-paper/).

P4L98: Providing some examples of 'Family environment predictors' would be helpful for the reader. Does this encompass parental decision making, or are these predictors broader? Is this the same as 'family context predictors' mentioned later in the introduction?
We have added some examples in brackets (e.g., parental support, role modelling, availability of a bicycle). As this phraseology (i.e., "family environment predictors") is the same as the "family context predictors" mentioned later, we have changed the term to be consistent through the manuscript.

P5L120–124: This is a very long sentence.
Oh, yes, you are right. We have now shortened the sentence.

P5L136: Suggest deleting the word 'exist'.
Thanks, we deleted this word.

P6L145: I was thinking the study protocol would benefit from explicit aims, but I see these are listed lower down in the methods section.
We added the word "overall" here to clarify that this is just the overarching objective.

P6L157: What is the rationale for selecting these four destination types? Are these exhaustive and

mutually exclusive?

Thank you for this comment. As mentioned in the introduction, these destinations were identified as the most visited and popular settings among German adolescents (Nobis & Kuhnimhof, 2018). Further international studies have confirmed these findings (Christian et al., 2015; Kyttä et al., 2018; Villanueva et al., 2014). We have also added a sentence in the Measures section of the manuscript to justify the selection of the four destinations.

P8L192: Does this panel only contain people living in Germany?

Yes, we have added the following information: "The panel contains people living in Germany and is representative of the German population regarding age, gender, education and place of residence."

P8L192: Although the panel (as a whole) may be representative of the German population for key sociodemographic factors, I wonder whether it is representative of travel behaviours in Germany (as travel behaviours are obviously a result of many factors other than sociodemographic ones). This also comes down to who you are planning to generalise these results to.

We fully agree. Consequently, we reported that "The panel contains people living in Germany and is representative of the German population regarding age, gender, education and place of residence.". Additionally, based on your feedback we have added a quick note on this point in the limitations section.

Another limitation (which is not identified as a limitation of the study design) is that this sampling strategy is not designed to maximise the variability of your exposure variables (e.g., individual factors and physical environmental factors). One of the key attributes of the stronger studies in this area is their multistage sampling strategies, which are used to maximise heterogeneity in the environmental variables (e.g., purposely selecting participants from a range of different environments). If your sample share very similar individual and environmental characteristics, it will limit the strength and clarity of associations you will be able to observe.

Thanks for pointing this out. We fully agree with you that multistage sampling strategies have specific benefits. For the ARRIVE quantitative study we had the chance to make use of a nationwide online panel maintained by forsa.omninet (a private research firm in Germany) which is based on a diverse sample of people from all over Germany and which is representative of the German population regarding age, gender, education and place of residence. Unfortunately, we did not have the chance to apply a multistage sampling strategy as we have now finished the data collection of the quantitative study (within the last three months during which the protocol paper was in the review process). We have since then analysed our final sample of the quantitative study and found that the sample is diverse and similar to the population in Germany with regard to urbanisation.

P8L204: Your sample size calculation is currently not replicable. If using G*Power, you should state the name of the model chosen, and the reason for selecting it. An unconditional/random multiple regression model is much more common (compared to a fixed X model) as you generally don't know the values of your predictors while planning the study. Keep in mind that if you are planning to adjust for any covariates in your analysis (i.e., in addition to the predictors of interest), the required sample size will likely increase.

There seems to be a mistake in our calculation using G*Power. Nevertheless, beside this calculation we decided to include a study sample of 500 based on current recommendations: "Based on previous studies and study sample recommendations for observational studies with large population size that involve logistic regression in the analysis, taking a minimum sample size of 500 is necessary to derive the statistics that represent the parameters." (Bujang, Sa'at, Sidik, & Joo, 2018). We changed the sample size paragraph accordingly.

P8L213: Please ensure consistency between 'child' and 'adolescent' as these terms generally refer to distinct age ranges.

Thanks for pointing this out. We checked the manuscript to ensure the consistency.

In general, the data analysis section is overly simplistic. One of the main reasons to publish a study protocol is to be upfront with your data analysis plan, and the hypothesis tests you wish to conduct. We fully agree that the current version of our data analysis section is restricted to an overview of statistical analysis we plan to apply in our project. Thus, we revised the whole section and structured the paragraphs based on our stated aims to make it more comprehensive and detailed. Due to the extensive aims and numerous research questions in our project, we decided to refrain from providing hypotheses. We hope that by revising our data analysis section we addressed all your concerns regarding the analysis we will apply in our research project.

P9L231: Differences between groups will be examined. What variables are you comparing between the groups? What are the independent variables?
In line with the previous comment pertaining to the data analysis section, we revised the corresponding paragraph and provided more details on this topic. Specifically, we will examine for differences in transport mode choice across gender (boys vs. girls) and degree of urbanization (rural vs. urban).

P9L235: What does the 'overall score' represent? Is this the total number of active trips performed within a given period? Or the proportion of total trips travelled by active modes?
Thank you for this comment. We added a sentence to explain the overall score in the analysis section.

P9L234: Are all analyses going to be stratified by destination type? Would it make more sense to examine how the relationship between the predictors and active travel vary by destination type? This seems to match the intention of the first aim. This would obviously mean you have repeated measures per subject and would require a different method of analysis.
Thank you for this valuable feedback. As stated above, we revised the corresponding paragraph to provide more details on our planned analysis. To evaluate predictors of active travel, we will run separate logistic regressions for each destination. As we assumed that transport mode choice differs between destinations (Herrador-Colmenero, Escabias, Ortega, McDonald, & Chillon, 2019), rather than conducting repeated measures per subject we instead treated the four destinations as independent outcomes and ran separate logistic regressions, as has been done in previous research (Scheiner, Huber, & Lohmüller, 2019). However, unlike these studies, in some analysis we also intend to use the overall score (i.e., active trips/total trips) to investigate travel behaviour in adolescents.

Are all analyses stratified by gender, and by parent/adolescent (as this was inferred in the participants section, but is not mentioned in the analysis section)? Gender was also identified as a moderator in the aims section (so there are several discrepancies throughout).
Thank you for bringing this inconsistency to our attention. Similar to our phraseology point discussed earlier, we checked our manuscript for consistency with respect to how we are examining gender. Based on theoretical assumptions, in our analysis the gender variable will be either stratified (Aim 1) or included as a moderator (Aim 3). In other analyses, parental and adolescents' gender will both be considered as a covariate (e.g., association between parental and adolescents' travel behaviour, see data analysis section.

P9L237: SEMs generally require significantly more subjects to estimate (compared to a linear model), particularly for structures as complex as illustrated in Figure 1 (which I'm sure is an oversimplification of the actual variables measured). You could perhaps position this as an exploratory analysis or justify your sample size based on this analysis.
Thank you for this feedback. We discussed the SEM in our research group and decided to forego the originally planned SEM analysis as we fully agree with you that more subjects will be required to

estimate our theoretical model. Due to limited funding, we are not able to recruit the required number of participants in our research project. The new analysis plans feature logistic regressions and multiple regressions; more details regarding these revised analytical plans can be found in the manuscript in the data analysis section.

P9L240: What are the variables of interest in this correlation analysis?
After discussion in our research group, we decided to do our analysis without any correlation analysis.

P10L256: I understand you will identify the sample for the qualitative component later. It might be useful to say whether they will be selected from the same panel as the quantitative component, or will they be recruited via a different channel? Will you still recruit one adolescent from each household? Thank you for this feedback to further explain our qualitative approach. We have included an additional sentence to clarify this matter in our revised manuscript.

P12L311: The qualitative analysis plan seems brief (i.e., you refer to your research questions, but I don't recall reading explicit research questions). I'm wouldn't say I'm an expert in qualitative methodology, so maybe this is all that is required.
You are right. We added the four main research questions that will be guiding our qualitative investigation. Additionally, we have further elaborated on the analysis part by providing more details which outline our initial deductive coding process that is based off the categories presented in Figure 1, the subsequent inductive coding process, and how these processes will be combined in a third step to develop the final themes.

P13L345: The sentence '…intended to provide the necessary empirical evidence to illustrate the influence of family context influences…' comes across as a bit strong, particularly for a single cross-sectional study. You will (hopefully) be able to contribute evidence around factors that are associated with travel behaviours.
We fully agree with you and changed this sentence.

You have two reference lists (and each of them is different). I'm not sure if this is a problem. Originally, one reference list was provided for the manuscript and the other one was an additional list that we thought was needed for Table 1. However, we have changed this and now just present one reference list for both the manuscript and Table 1.

Reviewer: 2
Dr. David Niederseer, University Hospital Zurich

Comments to the Author:
With great interest I read the protocol for the Mixed-Methods Cross-Sectional ARRIVE Study. In general, I really like this study protocol. Congratulations! Looking forward to the results of this study. Thank you.

I have several minor aspects that need further attention.
I would suggest shortening the introduction section of your manuscript substantially. Put this material into the discussion section. I would suggest the discussion section to be as long as your current introduction and the introduction as long as your current discussion.
As in many protocol papers, please provide a table with a summary of previous studies in this field.
Thank you for these suggestions, they are very much appreciated. To get a sense of the desired/standard composition of these types of manuscripts, in our consideration of restructuring the introduction and discussion sections of the manuscript we decided to look at a few other recent study protocols that were published in BMJ open (Goldfeld et al., 2017; Mandic et al., 2016; Quested et al., 2018). From these evaluations we found that our manuscript appears to have similar length

introduction and discussion sections as the referenced articles above. One point of clarity may be helpful here: as we believe it is important to provide a detailed outline of the state of current research on this topic in the introduction section to set up other parts of the manuscript, we have used our introduction to contextualize many aspects of the ensuing methods section (e.g., selection of various analytical methods). Having said that, we agree that it is important to be as concise as possible in our overview of the topic, so we have done our best to shorten the introduction section to what we deem the most essential information while moving some of the more complementary information to the discussion section.

Regarding the final point on incorporating a summary table, unfortunately we did not find examples of protocol papers providing tables with a summary of previous studies. While we are interested in this form of presentation of the state of research, we feel that it could be difficult to provide a table like this because no identical or very similar studies as the ARRIVE study are available, thus it could be difficult to define inclusion and exclusion criteria for the presentation of studies in such a table.

You submitted this protocol to a medical journal. I fully agree that the topic you address with your study is of utmost medical importance. However, the content of your study uses more psychological and behavioral science methods than medical ones. Why did you not include some medical aspects like childhood obesity, cardiovascular risk factors, etc. Or measure physical fitness objectively? I feel this is a missed opportunity. You mention this in the limitations, however, please expand on this major limitation.

Thank you for pointing out this potential inconsistency and giving us an opportunity to clarify our submission decision-making process, as well as our view of the paper's aims and suitability. To clear about the selection of the journal, the manuscript was submitted to BMJ Open for a few different reasons: 1) we feel that this is a suitable journal for active travel topics, broadly speaking; 2) more specifically, BMJ Open publishes research from a range of disciplines that address "research questions in clinical medicine, public health and epidemiology", which we believe this research does primarily as an evaluation of a public health issue; and 3) building on the second point, active travel in adolescents has consistently been argued to be a public health issue that contributes to multiple other health outcomes (Brooks, Tingay, & Varney, 2021; Giles-Corti, Foster, Shilton, & Falconer, 2010). Additionally, as many other manuscripts that addressed active travel from a public health perspective were published in BMJ Open (Kriit, Williams, Lindholm, Forsberg, & Sommar, 2019; Macdonald, McCrorie, Nicholls, & Olsen, 2019; Mandic et al., 2016; Oliver et al., 2016; Smith, Boehnke, Graham, White, & Prady, 2019), we feel that this journal serves as a great opportunity to publish this type of research. With regard to the suggestion to include some medical aspects, we have revised our introduction and discussion to consider these aspects. And while we certainly agree that more can be done to examine some of the medical factors that you described above, unfortunately we cannot include any further ideas or constructs in our quantitative study because the data collection has since finished from the time of the original submission. However, we will address these outcomes in future projects, so thank you for suggesting a few new avenues of study.

Gamification is used often to increase physical activity. I would at least include this aspect into the interviews. E.g. Is a smartwatch that counts your steps motivating you for active travel behavior? Thank you for this suggestion. We agree that gamification can be an important and effective mechanism by which to promote or increase active travel among adolescents. However, where our quantitative data collection is complete, our qualitative data collection is currently nearing completion at the time of this response (early December 2021) so we will be unable to incorporate this feedback into our qualitative data collection design. We do think this is an idea worth making mention of, so we have noted it in our discussion, and we will use this idea to inform our inductive coding processes (i.e., our two independent coders will look for elements of and references to gamification ideas and concepts) that will be carried out with the interview transcripts.

You did not reference the recently published guidelines of the ESC on prevention that also address

this issue. https://academic.oup.com/eurheartj/article/42/34/3227/6358713 (especially in the supplemental section).

This is a good recommended reference. We have now made reference to the relevant portions of ESC guidelines in the discussion section of our revised manuscript.

Please also reference and discuss the GISMO study.
https://onlinelibrary.wiley.com/toc/16000838/2020/30/S1

Thank you for calling our attention to the GISMO study. We have now added a reference to this project in our discussion section to bring attention to its contribution. However, we feel that this study's fit with our project is relatively limited as the GISMO study addresses adults who present a variety of different factors and contextual influences that can influence their health behaviours. In the interest of further illuminating findings similar to those of the GISMO study, however, we have added some additional literature addressing cardiovascular health outcomes associated with active travel specifically in relation to adolescents.

Please also cite and discuss this document. Mozaffarian D, et al. Population approaches to improve diet, physical activity, and smoking habits: a scientific statement from the American Heart Association. Circulation 2012;126:1514_1563.

Thank you for the reference suggestion here. Upon reading this article, we found that its primary aim and purpose is rather crude relative to the aim and purpose of our paper, and thus difficult to find a meaningful connection. Principally, the suggested paper covers a wide range of population-level approaches (e.g., taxation to promote active travel, walking school bus programs) that aren't overly relevant to our paper which is primarily concerned with examining theoretical relationships related to adolescent active travel predictors, mode choices, and decision-making processes. If the reviewer has a specific idea as to how this paper should be incorporated, we would be happy to listen and attempt to integrate in the way they suggest.

Reviewer: 3
Dr. Marco De Angelis, University of Bologna

Comments to the Author:
It is unclear whether participation implies a reward or not
For taking part in our quantitative or qualitative study, participants do not receive any reimbursement or compensation. We added this information in the Ethics and dissemination section.

is a child selected at random or whose name appears first in the alphabet to fill out the adolescent portion of the survey? not clear.
Yes, this is correct. When selecting an adolescent from a family that has multiple potential participants, our protocol stipulated that the participant selected to be included in our study will be done through through having the parent identify the adolescent whose first name is alphabetically first. We have rephrased this a little bit in the manuscript to hopefully clarify this point.

The possibility of people adopting a mutlimodal travel chain (e.g., bike + train; walk + bus + walk) does not appear to be considered. Is there a rationale for this that remains unclear?
This is a really good point that we would like to take up but think is a bit beyond the scope of our paper. To focus our manuscript's approach to investigating adolescent active travel transport predictors, mode choice, and relevant socio-demographic moderators (quantitative study aims), and decision-making processes (qualitative study aims), especially in reference to understudied phenomena such as non-school commutes, we felt it was necessary to conduct more granular assessments of individual trips. To this end, we only asked about adolescents' "main" mode of travel and not all potential travel modes as expanding our focus to multi-modal and/or multi-stop trip chains, we thought, ran the risk of potentially confusing some of our aims (e.g., having parents and

adolescents consider and explain decision-making processes in reference to hypothetical multi-stop trips or multi-modal trips that they may not have undertaken before or be familiar with).

In the event that they do not remember (or have not actually made) a trip to the same destination by another mode of transportation, what is the alternative strategy? please specify.
Thank you for this valuable feedback. We totally agree with you that there might be some adolescents not remembering or not taking a trip to various destinations with different transport modes. In the event that this is the case for some participants, the interviewer will try their best to recall a trip with another transport mode. Alternatively, if adolescents only use one of either active or passive transport modes to the destinations they discuss in their interview, the interviewer will probe participants about any trips they made with the other transport mode to identify how their habits or perceptions may have changed or could change in response to a shift in transport mode. To clarify this, we added a corresponding sentence in the methods section.

In the qualitative analysis, how many researchers will be involved? it is unclear the coding process and whether you have considered including an additional figure if consensus is not reached.
Thank you. To clarify the analysis process and state the number of researchers that are involved (2-3), we have included some additional information in our revised manuscript. The method of theoretical sampling implies including additional participants until consensus is reached.

Perhaps it makes more sense to talk about perceived urbanization. are there any broad assumptions about this? for example, you have described the degree of parental influence on adolescents but what kind of impact should we expect? do you think it is more of a parental issue to incentivize the use of active modes? in case it is related to the perception of the environment, do you think the measures taken are sufficient? it does not seem clear from the text, please be as clear as possible since this is a cross-sectional mixed-method study design.
We aren't entirely sure what is meant with the term "perceived urbanization". Our read of this term is that it may imply some type of relational dynamic (e.g., how do adolescents perceive, understand, or make sense of the level of urbanization/urbanicity related to their trip environments), which, to be sure, is a relevant and important question. However, this question is one that we believe is also beyond on the scope of this paper, as this manuscript is foremost concerned with family context predictors (quantitative study) and decision-making process (qualitative study). Alternatively, if this comment is inquiring about subjective self-report measures/methods regarding the local environments that adolescents interact with in relation to their active travel behaviours and perceptions, this is a good point that we will include in our inductive coding processes (i.e., our two independent coders will also look for elements of or references to subjective perceptions of urbanicity/density/city size as they relate to adolescent active travel) that will be carried out soon. One last note on urbanization, our quantitative study does account for urbanicity/urbanization through collecting a sample that includes participants from a range of different places of residence based on recognized German city categorizations (see Table 1: Urbanization).

Do you think it might be useful to include a dimension that asks parents the primary reason for choosing current residence? Some work by Zhou,J. (2012) may help clarify.
Thanks for pointing this out. As we already finished the data collection of the quantitative study (within the last three months that the protocol paper was in the review process) we cannot incorporate this suggestion into the quantitative study. However, we do think this is an important issue and we will also use this idea to inform our inductive coding processes (i.e., our two independent coders will look for elements of and references to place of residence decision/rationale) that will be carried out with the interview transcripts.

Please include the ethical form for parents to sign as an additional element.
As both the quantitative and qualitative studies were only conducted in German, the corresponding

informed consent documents were also exclusively provided in German. It is our best guess that these documents will likely not be very informative for the international readers of BMJ open. Furthermore, we do not have the impression that it is common to provide this material as an additional file. However, if these documents are deemed to be priority information that the journal believes its audience would be interested in, we can translate and attach these forms.

Further revisions:
As the Cochrane Sex/Gender Methods Group, a subgroup of the Campbell and Cochrane Equity Methods Group, emphasizes that sex-based biological factors and gendered social factors influence each other and interactively shape health behavior, opportunities and outcomes. In recognition of this theoretical and empirical entanglement, the group recommends using the term sex/gender (see also https://methods.cochrane.org/equity/sex-and-gender-analysis) (Springer, Mager Stellman, & Jordan-Young, 2012). We adopted this terminology in this protocol and replaced the term gender with sex/gender (Demetriou et al., 2019).
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VERSION 2 – REVIEW

REVIEWER	Niederseer, David University Hospital Zurich, Department of Cardiology
REVIEW RETURNED	17-Dec-2021

GENERAL COMMENTS	The authors have addressed all my remarks adequately.

REVIEWER	De Angelis, Marco University of Bologna, Department of Psychology
REVIEW RETURNED	23-Dec-2021

GENERAL COMMENTS	I would like to thank the authors for their constructive response to the review. The article is well structured and clear, easy to read and promising. All the relevant points from the review have been dealt with and argued effectively. As things stand, I consider the article accepted for publication.

REVIEWER	Stewart, Tom Auckland University of Technology, Faculty of Health and Environmental Sciences
REVIEW RETURNED	24-Dec-2021

GENERAL COMMENTS	The authors have done well responding to the three reviewers. The paper has improved. I don't have any major concerns that

<table>
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<td>should preclude publication. I do have some general comments (which the authors may or may not like to consider).

I like how you have organised your analysis plan to align with your study aims. However, I would argue that your 'descriptive analysis' section (L295–302, track changes copy, specifically) is actually an inferential analysis, rather than simply reporting descriptive statistics. If you are interested in comparing travel behaviours across age groups or between sex/gender groups (without adjustment), then this should be an explicit aim. By using ANOVAs, chi-square, and t-tests you will be inherently generalising any group differences to the German population. I feel you need to describe the purpose of this analysis or provide some rationale, particularly as you are comparing these groups without adjusting for other confounders (which you are doing in Aim 1/2/3). Perhaps a solution is to simply report unadjusted and adjusted ORs for Aim 1 or remove this paragraph altogether.

L316 (track changes copy), I can't quite visualise how the parent-adolescent-dyad analysis will work from the description provided. Is travel behaviour still the outcome variable? and is the independent variable the monad (i.e., parent or adolescent)? Are confounders still included here?</td>
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VERSION 2 – AUTHOR RESPONSE

Mr. Tom Stewart, Auckland University of Technology Comments to the Author: The authors have done well responding to the three reviewers. The paper has improved. Thank you. I don't have any major concerns that should preclude publication. I do have some general comments (which the authors may or may not like to consider). I like how you have organised your analysis plan to align with your study aims. Thank you. However, I would argue that your 'descriptive analysis' section (L295–302, track changes copy, specifically) is actually an inferential analysis, rather than simply reporting descriptive statistics. If you are interested in comparing travel behaviours across age groups or between sex/gender groups (without adjustment), then this should be an explicit aim. By using ANOVAs, chi-square, and t-tests you will be inherently generalising any group differences to the German population. I feel you need to describe the purpose of this analysis or provide some rationale, particularly as you are comparing these groups without adjusting for other confounders (which you are doing in Aim 1/2/3). Perhaps a solution is to simply report unadjusted and adjusted ORs for Aim 1 or remove this paragraph altogether. Thank you for this valuable feedback. As this manuscript conceptualizes travel behavior among adolescents from Germany as downstream from (i.e., predicted by) socio-demographic characteristics (e.g., sex/gender; degrees of urbanization), and to provide a comprehensive overview of travel behavior related to the German population, we have added a further explicit aim (now Aim 1) in our manuscript with details in the corresponding Aims section and Data analysis section. L316 (track changes copy), I can't quite visualise how the parent-adolescent-dyad analysis will work from the description provided. Is travel behaviour still the outcome variable? and is the independent variable the monad (i.e., parent or adolescent)? Are confounders still included here? Thank you for highlighting this unclear point. For better comprehension, we have now clarified the dependent and independent variables in the binary logistic regressions. Furthermore, we now state that the confounders mentioned are included in all analyses of aim 2. Reviewer: 2 Dr. David Niederseer, University Hospital Zurich Comments to the Author: The authors have addressed all my remarks adequately. Thank you. Reviewer: 3 Dr. Marco De Angelis, University of Bologna Comments

to the Author: I would like to thank the authors for their constructive response to the review. The article is well structured and clear, easy to read and promising. All the relevant points from the review have been dealt with and argued effectively. As things stand, I consider the article accepted for publication. Thank you.

VERSION 3 – REVIEW

REVIEWER	Stewart, Tom Auckland University of Technology, Faculty of Health and Environmental Sciences
REVIEW RETURNED	12-Jan-2022

GENERAL COMMENTS	I am happy with the authors' responses. Well done. Just a small note for the final proof: L172: I would change "in dependence of" to "in relation to" as the new sentence doesn't quite make sense.