**RESPONSE TO REVIEWER COMMENTS**

We would like to thank the editor and three reviewers for their constructive comments.

In broad terms, the reviewers were in consensus that the paper has a clear scope which aligns with the journal. We tried to balance the feedback that was received and made the following revisions: i) sharpened the framing of the paper; ii) clarified the methodology and analysis; and iii) emphasized the methodological and practical contributions of the findings. In what follows, we respond to the reviewers in turn and describe how the paper was revised (our responses are in blue).

The suggestions from the reviewers helped to improve our manuscript, and we believe it is a stronger paper as a result. For ease of rereading, we have highlighted all substantive changes in the resubmission in yellow. Revisions have been made to the manuscript, highlights, and cover letter.

**Reviewer #1**

This research investigate the impact of various factors on promotion of bicycle trips. The topic is attracting many researchers, so it is inside of this journal scope. Although it is written well, but the methodology is not enough and the contribution is very limited. The some big concerns of the reviewer are as follows.

Thank you for reading our paper and providing constructive comments. We have addressed your concerns to the best of our ability and hope that you will be persuaded that the analysis is not only technically correct, but also provides useful information about the use of an open and free routing algorithm for modelling bicycle flows; about testing models for network autocorrelation to ensure that they are complete; and about bicycle flows in a medium-sized city.

1. Poisson regression model

The second equation in page 7 is not Poisson regression. This equation just log-transfomed function of gravity model. Actually, Poisson regression can be transformed like this formulation. But this equation cannot treat zero count. However, the authors mention that one of advantage of Poisson regression is that it can treat zero dependent variable.

Thank you for this comment. We assumed that readers familiar with Generalized Linear Models (GLM) would understand that the equation in question was for the mean function. In the GL modelling framework, the “log” link function gives the Poisson model. The notation may have proved somewhat confusing too. We revised the Methods section to clarify the use of the Poisson regression and the appropriate equations. Please see pages 7-8.

2. Spatial interaction Poisson regression model.

In page 7, the authors are explaining this model very briefly. But there is no explanation about how to calculate eigenvectors. I wonder whether the authors could apply this model correctly.

The senior author of the paper has applied eigenvector filtering in numerous papers in the past, apparently without making mistakes. We could describe how eigenvectors are calculated, but this would be a distraction, because spatial filtering is not really used in the research. To avoid confusion, we have removed any references to spatial filtering.

3. Model comparison

The authors apply base model and three other models to bicycle trip data and compare them. But this comparison is not meaningful. It is because the only full model is reasonable for analysis and the other models are insufficient models.

It is true that only the full model is complete. The objective of presenting and comparing the full suite of models is to illustrate the pitfalls of incomplete models, in effect to show how each of the zones of the behavioral model of the environment (see Moudon & Lee, 2003) are necessary for spatial interaction modelling. By estimating model 1 with zonal attributes only (which represents only the first and second spatial zones – characteristics of the origin and the destination), we find that there is network autocorrelation. This means that the model is not yet a *sufficient* explanation of the pattern observed. By estimating model 2 with cost function only (which represents the friction of distance/time between the origin and spatial zones – i.e., the route), some characteristics of the route are included through the inferred routing algorithm, but again there is network autocorrelation. It is only model 3 that considered all spatial zones of the behavioral model of the environment, which leads to no residual network autocorrelation. Thus, all systematic variation has been accounted for with the variables included to estimate trip flows. The model can be considered a *sufficient* explanation of the pattern observed.

We have clarified the purpose and value of model comparison on page 14.

4. index of AIC difference

I could not follow the reason why equation in page 14 gives the probability.

The equation for calculating Akaike’s Information Criterion is now included on page 15.

**Reviewer #2**

Thank you for this paper. It was lovely to read, being both well written and based on a well done analysis. The main issue that I have with the paper is that I think, generally, modeling route choice limits the inference we can make about the analysis. I think the focus on flows is the right focus. Any link to route choice is limited, because we don't know what routes were used. We are just inferring patterns of flow from patterns in an O/D survey. I would ask that the authors take a careful look through the paper to ensure this framing is used.

The manuscript has been revised to emphasize that the use of inferred routes helps to explain patterns of trip flows from the O/D survey. The finding that *quietest* distance routes best explain the pattern of flows leads to the hypothesis that bicyclists are using streets that minimize interactions with traffic (either because of infrastructure or “quiet streets”). This hypothesis is supported by other studies using bike share data in Hamilton (see - Scott, D. M., Lu, W., & Brown, M. J. (2021). Route choice of bike share users: Leveraging GPS data to derive choice sets. Journal of Transport Geography, 90, 102903. <https://doi.org/10.1016/j.jtrangeo.2020.102903>; and Lu, W., Scott, D. M., & Dalumpines, R. (2018). Understanding bike share cyclist route choice using GPS data: Comparing dominant routes and shortest paths. Journal of Transport Geography, 71, 172–181. <https://doi.org/10.1016/j.jtrangeo.2018.07.012>).

Another issue I have with modeling routes, generally, is that the analysis tends to overemphasize a generalized bicyclists, that is confident and experienced. It misses the more nuanced observations about who we are building cities for: women, children, new bicyclists. I would recommend that you add something about this in the limitations.

Thank you for this suggestion. Indeed, the trips recorded in the O/D survey are likely from confident and experienced bicyclists. Although the *quietest* distance route best explained the pattern of travel, we do not know the quality of these routes which means that they may not appeal to women, children, or new bicyclists. We discuss this further in the *Limitations* (see page 25).

Just a comment… Ideally, we would be able to attribute these models with more detailed trip routing algorithms that are based on observed preferences. I see this is a step in that direction as the authors are moving beyond the use of shortest path algorithms. It will be interesting to think about how to build models flexible enough to keep updating with new knowledge on route selection.

That would certainly be more informative and useful for planners. Open source geographic tools, such as OpenStreetMap, hold promise for incorporating local data that can inform trip routing algorithms. We discuss this further on page 21.

Finally, I think the paper could better emphasize the real "nugget" of knowledge gained from this work. Yes, we can look at flows and that might be handy. But, perhaps highlighting what we know now that we didn't know before would help strengthen the impact of the work.

We have revised the *Discussion* section tobetter explain the methodological and practical contributions of the findings. Please see pages 19 and 21.

All that said, well done on an interesting paper.

Thank you for your helpful and constructive feedback.

**Reviewer #3**

This paper is about modeling and understanding cycling flows in Hamilton, Ontario. The paper has a clearly defined scope and the analysis is very thorough and complete. It is an applied paper that describes practical findings from applying a spatial interaction model to draw conclusions on both route factors and built environment factors that attract bicycle trips. Such findings are relevant to planners, especially in similar small North American cities.

Thank you for your generous assessment of our paper and suggestions for improvements. These are much appreciated.

The paper can be improved in several minor ways:

- Equations are not numbered

The equations are now numbered in the revised manuscript.

- P. 7: beta in the equation is not explained

We have revised the notation for clarity and explained it in the text.

- P. 9 It is not clear what is a zone and what is a ward in Figure 1, and are wards relevant at all in the paper? Note that when printed in black and white, the figure becomes meaningless

We have revised Figure 1 to only include traffic analysis zones, which are outlined in black.

- There are some spelling mistakes in the document, e.g. « assosication » (p. 5), « a significant factors » (p. 5), « conducive » (p. 10) or weird wording, e.g. « my cases » (p. 24)

Thank you for bringing this to our attention. We have reviewed the manuscript in its entirety and fixed all spelling and grammatical errors.

- p. 11 OD pairs are labelled depending on whether they are in the same geographical class: why are there not two different labels for 1) origin in the city and destination in the rural area and 2) origin in the rural area and destination in the city ?

There are not two different labels because both trip flows would require a bicyclist to navigate changes in elevation or natural features due to changing topography between the city’s rural and urban areas. The dummy variable is assigned to trip flows to only account for a change in topography along the route. In most cases, trips that occurred between regions of the city (i.e., urban-rural or rural-urban) were reciprocal flows which likely represents a roundtrip commute from home to work or another frequently visited destination.

- The paper should better explain how is the quietness score computed

We have added additional publicly available information about the calculation of the *quietness* score on pages 12-13. It is important to note, as we did in the original submission, that there is somewhat of a lack of transparency with respect to the specific attributes considered by the algorithm when minimizing *busyness*, but that the scores are adjusted based on user feedback.

- In the final model the distance on the quietest route is used as a measure of friction in the model. This only gives information on the best possible route between the OD, but not on whether there are multiple attractive routes between the OD, which is also an important factor to consider. A measure of biking accessibility computed from a route choice model would be more suitable. I wonder if failing to capture this is what leads to underestimating cycling flows.

Thank you for this comment. You are right that the routing algorithm provides a proxy for cost, only, not on the actual routes used. In reality, there are multiple routes that could be used and that presumably are correlated (see the work of Scott and colleagues in this respect). That said, the value of checking the model for network autocorrelation is to investigate whether we are missing any important information. Since the residuals of the full model are not autocorrelated (i.e., they are random) we are highly confident that we are not missing any relevant variables. Replacing variables might induce autocorrelation (if they are not sufficient), or might change the magnitude of the coefficients (if they are). It is important to note as well that the model is not Gaussian. The Poisson distribution, on the other hand, is not symmetric, so it is natural that flows would be underestimated; as long as this happens at random this is not a reason for concern. We expand on this in the section *Limitations* (see pages 26-27).

- Instead of using the distance on the quietest route as a measure of friction in the model, couldn't the authors also incorporate the score of quietness? For some OD pairs, the quietest route could sometimes still not be very quiet.

The *quietness* score could not be incorporated as a measure of friction since it is not expressed as a distance or time; instead, it is used by the routing algorithm for link selection. It is possible that the *quietest* route would sometimes not be very quiet or the most quiet, however, the algorithm takes *quietness* into account when finding a route that either i) minimizes time or ii) minimizes distance. Therefore, the *quietest* distance route between an origin and destination is one with the least amount of *busyness* and that minimizes distance.

Ideally, we would be able to use a routing algorithm that considers revealed preferences (for instance, using GPS data) but such data are often not publicly available and are often difficult to collect. Using crowdsourced data, such as Strava, can reveal routes actually travelled by bicyclists but it is generally a biased sample. By using a routing algorithm like *CycleStreets*, we attempt to move beyond the use of the shortest path to better consider characteristics at the route level that might influence bicycling. Open source tools like OpenStreetMap include a variety of geographic points and features that were considered by the algorithm. While the *quietest* inferred route could sometimes still not be very quiet, we found that using this measure as a cost function led to a model that performed better than using the Euclidean distance.