

I appreciate the efforts the authors put in to revise the paper and agree with both reviewers that the paper has improved substantially. However, both reviewers still have a fair number of comments which I think are important for the authors to address. I also have a couple of comments myself. I believe all comments are addressable, but given the amount of work required, my recommendation is major revision. Since it will be the second revision, it is crucial that the authors address all comments to the review team's satisfaction. Given the history of the paper and some of the comments below, I would also like to ask the authors to read through the manuscript carefully before re-submitting it, and make sure it reads like a coherent and polished paper instead of a draft put together in a rush.

### Model

I agree with both R1 and R2 that the model in the current version which allows for both supply and demand side economies of density (EOD) has improved the paper substantially. But given the high similarity of the mechanism and the results of supply and demand EOD, I agree with R1 that it is important to include a discussion about why this is the case. See R1's comment #1.

### Calibration

The model calibration has also changed substantially to accommodate both supply and demand side EOD. The identification results make the calibration more convincing than the previous version. However, both R1 and R2 are not yet fully convinced by the robustness of the results. I agree with their assessment. Specifically, R1 questions the validity of Proposition 6 and asks for extensions of the current analysis when regional heterogeneity in price and pick-up time sensitivities is allowed. R2 has similar suggestions about providing robustness checks on the functional forms of customer response, price and pick-up time elasticities, and allowing for heterogeneity of trip characteristics. I think they are all great suggestions to strengthen the empirical results. Given there has been quite a few empirical papers using NYC ride-share data, many of the key parameters have been estimated in the literature. The authors could use these results to both benchmark their findings and conduct the additional analyses suggested by the reviewers.

The identification result is interesting, but as the authors state, it is based on the model, or put differently, within the model calibration exercise instead of an actual structural estimation exercise. While I agree with the authors that, if the key assumptions apply, the intuition could be potentially extended to structural estimation problems and other empirical transportation settings, it takes a whole new paper to show that. Therefore, I suggest the authors tone down the contribution on the identification result.

### Counterfactuals

The current counterfactual section is a long list of counterfactual scenarios calculated using the estimated model. It's not clear to me how they are connected to each other, why they are chosen to be included, or what the main managerial/policy messages are. The entire section (6.2) seems rushed without much thoughtful discussions about the findings. I suggest the authors select 1-2 main messages/insights first, present the analyses around them, choose carefully the most relevant and interesting analyses, and discuss the insights clearly. Relatedly, the figures in this section are also presented without much thinking. Given the similar patterns in most subfigures and across different supply-demand EOD ratios, it is clearly not necessary to present all of them in the main text.

There are many typos and spelling mistakes in the paper. Please edit it thoroughly before submitting the revision.