August 17, 2023

Co-Editor, *Management Science*

Dear Professor Besbes,

We thank you and the review team for the constructive feedback, which we believe has improved the paper significantly in clarity and quality of contribution. We are also grateful to you for granting our request to extend the re-submission deadline due to personal circumstances.

We include a separate response to the issues raised by each member of the review team: AE, R1, and R2. We agree with you that this team has gone out of its way to be constructive. We have worked hard to take advantage of these constructive comments and improve the paper on multiple fronts: focus of the message, consistency between theory and empirics, clarity of the writing, and relevance and robustness of the results. We describe our revisions both on a high-level and through point-by-point responses to the members of the review team. We hope that you and the rest of the team find our revisions addresses the comments raised and is acceptable.

Best,
The Authors

Summary of Major Changes

Before getting into the details of how we address specific points by each member of the review team, we provide a high-level summary of the main ways in which the paper has changed in response to the recommendations:

- **Focus:** We center the paper around the following three main insights:
  - (A) There is geographical access skew in favor of denser regions (i.e., regions with higher potential demand per unit of size.)
  - (B)The access is more skewed for smaller platforms.
  - (C) The platform, using optimal wage and price levers in each region, would mitigate but not eliminate access skew.

  We have retained in the paper only propositions that are central to how we deliver these main messages, how we show its robustness, and how we implement it empirically. Any other result not directly related to this has either been removed from our submission or moved to an appendix.

- **Consistency:** Instead of supplementing our theoretical model with a "reduced-form" supporting analysis (which is what we did in the previous submission), we now fully take the model to data and calibrate its parameters. This creates a direct connection between our theory and empirical analysis.

- **Clarity:** We have tried to be fully clear about the limitations of our analysis, including but not limited to the fact that our empirical approach cannot separate supply- v.s. demand-side economies of density.

- **Relevance:** We use our calibrated model to conduct counterfactual simulations of scenarios which we believe are relevant to public policy and business strategy decisions.

- **Robustness:** We show throughout the paper that both our theoretical and empirical insights are robust to whether the source of economies of density is the supply side or the demand side.

Table 1 provides a point-by-point comparison against the previous submission to show how this revision implements the above major changes. After that, we turn to specific comments by members of the review team.

Table 1: Major Changes in Paper

<table>
<tr><th>Previous Version</th><th>Current Revision</th></tr>
<tr><td colspan="2">Theory</td></tr>
<tr><td>

We had 10 theory propositions in the previous version. We started with the setting without inter-region rides in Proposition 1-7.
Proposition 1-4 corresponds to a model with a fixed number of drivers without modeling driver entry, and characterizes the equilibrium market allocation.
Proposition 5-7 characterizes the equilibrium with drivers' free entry and a monopoly platform setting optimal prices and wages.
Proposition 8 extends Proposition 5-7 to the setting with inter-region rides.
The 8 propositions consider only supply-side economies of density.
Proposition 9 and 10 develop testable implications and connect the theory model to the real-world data.

</td><td>

We have 5 theory propositions in the current version, and later propositions build upon the prior ones in a logical manner. Still, we start with the setting without inter-region rides. We model drivers' free entry and a monopoly platform setting optimal prices and wages.
Proposition 1-3 characterizes the equilibrium with supply-side economies of density.
Proposition 4 characterizes the equilibrium with demand-side economies of density.
Proposition 5 extends the results in Propositions 1-4 to the setting with inter-region rides.
We extend the Proposition 9 in the previous version to the multi-region setting and move it to the empirical section.

</td></tr>
<tr><td colspan="2">Empirical including Data</td></tr>
</table>

Table 1: Major Changes in Paper

Previous Version	**Current Revision**
• For the main analysis, we use ride-level data on all rides within the New York City from July 2017 to December 2019. Clarify how it's different from data used in current revision. • To show evidence of supply-side economies of density, we use data from "Ride Austin", a non-profit ride-share platform, from June 2016 until April 2017.	• We focus on ride-level data on all rides within Uber from March 2019 to June 2019. Durig this period, TLC provides prices and wages data which we require for our new empirical model. • We do not use "Ride Austin" data in our analysis. Explain Why. • We exclude rides either picked up or dropped off in Staten Island because the rides between Staten Island and other boroughs are scarce on some of the days within our data period. This leads to extreme outliers in the measures relative outflows and biases the estimates.

Table 1: Major Changes in Paper

Previous Version	**Current Revision**
Reduced Form Analysis	
We conduct two sets of reduced form analysis: • We test our theory result that access to rides is higher in regions with densities of potential demand. • We tried to provide evidence for the mechanism of the model with supply-side economies of density: drivers' location choice in response to pickup times.	We remove both sets of reduced form analysis in the current version for the following reasons: • We want to keep the empirical model closely connected to the theory model. The analysis provided in the previous version is qualitatively different. • The assumption for the first set of analysis that incoming rides serves as a good proxy for potential demand may not be convincing. Therefore, we directly estimate the potential demand using a model, and do not rely on any proxies. • We no longer claim that the economies of density is driven only by supply-side. In the current version, we show that there can be either supply or demand- side economies of density, and both lead to similar equilibrium characteristics and policy recommendations. Thus, we no longer require the Ride-Austin analysis which tried to motivate a supply-based only model.
Empirical Model	

Table 1: Major Changes in Paper

Previous Version	**Current Revision**
• We did not have an theory-based empirical model of supply and demand. Rather, we tested some predictions from the theoretical model using Relative Outflows in a regression framework.	• We construct an empirical model that is directly connected to our theory model, in that we "take the theory model to data." • We estimate the model, and obtain parameter estimates for demand and supply parameters, and then run the counterfactuals. • We show that our results are robust with either the empirical model with supply-side economies of density or demand-side economies of density.
Exposition/Writing	
We have two versions of theory model: (i) a model with fixed number of drivers; (ii) a set of three models that allows for free drivers' entry and platform optimal strategy: (a) endogenous wage and fixed price; (b) endogenous price and fixed wage; (c) endogenous price and endogenous wage	We keep only the second version with both price and wage endogenous in the main paper and move the other three models to the appendix. we should discuss whether we really need the other models even in the appendix. what purpose do they serve?
We include only models with supply-side economies of density in the theory section.	We present both the theory model with supply-side economies of density and the theory model with demand-side economies of density and show that the results are robust to the source of economies of density.

Response to the Associate Editor

Thank you very much for your constructive suggestions, your aggregation of referee comments, the flexibility about some of the limitations of the analysis, and the opportunity to submit a revision of the paper. We provide our point-by-point responses to your comments below. For ease of reference, we copy your original comments here.

AE 1: *R1 is still not convinced that incoming rides can be used to estimate potential demand. The same concern was raised in the previous round. I also do not think the authors provided convincing answer. See R1's comments at the bottom of page 2 and top of page 3 of the report.*

Reply: We agree that using incoming rides to estimate potential demand is not ideal. In the previous version, we were not calibrating our model in the empirical section; and were just attempting to provide reduced form evidence for the implications of our theory model. In doing so, we used incoming rides as a form of proxy for potential demand, which we agree has clear drawbacks.

In the current version, we calibrate the theory model based on direct recommendations by you and R2, and based on the above concern raised by R1 which indirectly motivates moving from a reduced form analysis to calibration. In this calibration, we do not use incoming rides to identify potential demands $\bar{\lambda}_i$. The identification comes from relative outflows, number of rides, prices, etc. Please see our identification argument in ?? of main paper.

AE 2: *R2 is still not convinced that the magnitude of the supply side effect is first order, or as important as the demand side. This was also a comment from the previous round. I agree with R2. One important issue here is that the demand and supply effects are not separately identified.*

To address both comments, I suggest the authors look into exogenous variations such as those used in Rosaia (2020) (e.g. changes in pricing policy) which provides identification of demand and supply effects separately. This can also be used to better calibrate the model (see my last comment below about model calibration).

Reply: We agree that there is not enough evidence to show that the magnitude of the supply side effect is of first order. Our revision no longer makes this claim, implicitly or explicitly. We strengthen our analysis by showing that our results are robust with either supply-side economies of density or demand-side economies of density:

On the theory side we formally show that if the source of economies of density came from the demand side instead of the supply side, our main results would still hold. We achieve this by extending the simulation results provided in our previous response letter and incorporating them as part of the formal theoretical analysis of the revision.

For the empirical analysis, we agree that the demand and supply effects are not separately identified in our current setting. We attempted to find the right kind of exogenous variation (such as those used by Rosaia (2020)) to help us separate between the two sources of economies of density but this proved challenging.[1] See Appendix A for more details on these challenges. As a result of this, we turned to the next best solution: we show that our empirical results are not qualitatively sensitive to whether the source of economies of density is supply side or demand side. To this end, we did two exercises:

1. We calibrated both versions of our theoretical model (i.e., supply-side-only and demand-side-only economies of density) using our data. We used either version to conduct the counterfactual analyses of interest and find that the results are robust to which model we used.

2. We took a step further and calibrated a more general model that allows for both supply side and demand side economies of density at the same time. In that model, there is a parameter roughly capturing the "relative weights between supply and demand sides economies of density". The two extreme values of ∞ and 0 for this parameter reduce the model to demand-side-only and supply-side-only EOD respectively. This parameter, as you and R2 point out, cannot be estimated off of relative outflows only. But we can still show that our results are robust to this parameter. We set the parameter to a number of intermediate values between ∞ and 0 (specifically, we take the values of 10, 0 and 0.1), calibrated the rest of the model based on it, and used the calibration for counterfactual analysis. We find that our result are robust to where this parameter is set.

Again, we agree that the ideal solution to this problem would be to separate between the

[1] We cannot use exactly the changes used in Rosaia (2020). Rosaia has collected the price and wage data before and after the policy change leveraging the client apps and the responds from the platform servers, while our data source (the public TLC data) provides the price and wage data only *after* the policy change. We explored other exogenous changes as well but our main variables of interest were not too responsive to those policies.

two types of economies of density. That said, we hope our solution of demonstrating robustness helps alleviate concerns by you and R2 on both the theory and the empirical implications of this matter.

AE 3: *On the modeling side, like R2, I'm also puzzled by the objective of the analysis. I do not find it helpful to have many versions of the model and a long list of results. The authors need to think carefully about the objective of the analysis and whether and how each set of result contribute to delivering the main message, and then reorganize the analysis to tell a coherent story. See R2's comments for more details.*

Reply: We agree that having many versions of the model and a long list of results is not ideal. We have carefully thought through and streamlined the theory model. In the main paper, **we only present the model with free entry of drivers where platforms set both wages and prices across regions**. We show that the models with either supply-side or demand-side economies of density have the same qualitative outcomes. Though the model with *fixed number of drivers* was more technically complex as pointed out by R2 last round, we moved it alongside its related propositions to the appendix.

AE 4: *Both R1 and R2 also find proposition 9 and 10 problematic. I agree with them, and again, I encourage the authors to think carefully about the purpose each proposition serves in reaching the overall objective of the theoretical analysis.*

Reply:

These propositions were meant to help connect between our theoretical and empirical analysis in the previous version. Given that we have changed our empirical approach in response to comments by the review team, those propositions were no longer of value and have been removed.

Overall, our theory section now consists only of five propositions, summarized in Table 2.

AE 5: *Overall, I agree with both referees that the connection between the theoretical and the empirical analysis is still weak, and as a result, the overall message of the paper is not clear. I also feel it may not be possible for the authors to significantly strengthen the empirical analysis given the data limitations. Therefore, the option left is to strengthen the model and the simulation sections.*

Table 2: List of Propositions

Proposition	Model Setting	Model Statement
Proposition 1	model with supply-side EOD and without inter-region rides	Access is skewed towards denser regions.
Proposition 2	model with supply-side EOD and without inter-region rides	The skew is more pronounced for smaller platforms.
Proposition 3	model with supply-side EOD and without inter-region rides	The platform uses prices and wages to mitigate but not eliminate access skew.
Proposition 4	model with demand-side EOD and without inter-region rides	Propositions 1-3 hold in a model with demand-side EOD and without inter-region rides.
Proposition 5	model with supply-side/demand-side EOD and with inter-region rides	Propositions 1-4 hold with inter-region rides.

R2 has some great suggestions (see the last paragraph of their report for details). I agree with those and think it would be interesting to allow for both demand and supply side economies of scale instead of focusing on the supply side only in the model, calibrate the model using the data, and provide more concrete policy recommendations to platforms or city governments using counterfactual simulations. This way the analysis would not rely so heavily on being able to provide strong evidence about the supply side effect. It also strengthens the managerial insights the analysis can generate. I recognize that this change is substantial, and there is a lot of uncertainty. However, if it works out, I think the paper can make a nice contribution to the literature, and am willing to give the authors another chance to revise the paper.

Reply:

Thanks for this summary. We did our best to follow it closely in putting together this revision. More specifically:

1. To strengthen the connection between the theoretical and the empirical analysis, we have

focused on a unified set of claims (A)-(C) (provided in the **Summary of Major Changes**) which are examined for both theory model and empirical model. Also, unlike the previous version, we are taking the actual theoretical model to data and calibrating it as opposed to attempting to provide tests for some of the implications.

2. We took your suggestion and considered both supply-side and demand-side economies of density. We did so both theoretically and empirically. We showed that our theoretical predictions are robust to which side the source of economies of density is. We also calibrated both supply-side and demand-side EOD model using our data and established that our counterfactual simulation results are directionally robust to this.
3. We also followed your instructions and performed a series of counterfactual simulations using our model that we hope will be of value to policymakers and platforms.

To sum up, we agree with you that some of the possible improvements to the empirical setting are hindered by our data limitations (such as separating between supply and demand side economies of density). We appreciate your flexibility around this limitation. We did our best, however, to not compromise on the improvements that were feasible and that you had recommended us to carry out. There are two reasons that give us some confidence that the conclusions we draw are nevertheless valid. First is that the policy implications are qualitatively similar whether the supply-side or demand-side EOD is responsible for the access skew. Second, when we move beyond the two extreme cases and test across a wide range of magnitudes for the relative supply-side and demand-side effects, we find that the policy recommendations remain robust.

Response to Reviewer 1

Thank you very much for your constructive comments and for recommending that we have an opportunity to resubmit. Broadly speaking, your comments helped us in two major ways. First, we have strengthened the connection between the theoretical and empirical parts of the model by directly taking the model to the data and calibrating it, as opposed to using "proxies" for potential demand that are difficult to justify. Second, we have tried to remove propositions that do not directly speak to the main message of the paper (e.g., Props 9 or 10 in the old version) or replace them with more clearly relevant results. We now provide our point-by-point responses to your comments below. For ease of reference, we copy your original comments here.

R 1.1: *Overall, I can see a tangible improvement in the paper. However, I still think the connection between the theoretical and empirical parts is not very strong. In the following, I first summarize the paper and then provide my comments.*

The paper aims to convey three main messages: (i) the supply of drivers is skewed towards regions with denser demands, (ii) such a skew is more pronounced in small markets, and (iii) a revenue- maximizing platform tends to mitigate such skew but does not eliminate it.

Reply: We appreciate your assessment that the our second submission was tangibly improved upon the first. We also agree with you that connecting the theory to the empirical portion needs more work. Following your summary above, we centered the paper around the three main messages (i)-(iii), which are consistent with our claims (A)-(C) provided in the **Summary of Major Changes**.

R 1.2: *(Overview/Empirical Part) The central part of the empirical section is to use NYC ridesharing data to validate (i) and (ii) of the main results. Specifically, the goal is to test whether access to rides (defined as $A_i = \frac{r_i}{\bar{\lambda}_i}$, the ratio of fulfilled requests over the potential demand) is higher in regions with high density (density is defined as $D_i = \frac{\bar{\lambda}_i}{t_i}$, the ratio of the potential demand over region size). However, the potential demand $\bar{\lambda}_i$ is not observable. The authors instead compare the relative outflow (defined as $RO_i = \frac{r_i^{\rightarrow}}{r_i^{\leftarrow}}$, the ratio of outgoing rides over incoming rides) and the density of dropoff (defined as $D_i^{\leftarrow} = \frac{r_i^{\leftarrow}}{t_i}$, the ratio of incoming rides over region size) and provide strong evidence that these two are positively related. The reasoning of such proxy is that the incoming rides $r_i^{\rightarrow}$ might be proportional to the potential demand $\bar{\lambda}_i$ in the region, because a rider that takes a trip into the region may need to take a trip back.*

Reply: Yes, we agree completely with your point. In response to your concern here as well as concerns by other members of the review team, our empirical analysis is substantially revised. In this revision, we calibrate the theory model and use it for counterfactual analyses of interest. That calibration exercise backs out potential demand values as parameters to be estimated and does *not* use incoming rides to proxy them.

R 1.3: *Viewing separately, both the theoretical and empirical parts look good to me. The model is pretty simplistic, yet it provides some interesting results, e.g., the optimal platform pricing and wage policy over regions with different densities. For the empirical part, the authors provide strong evidence that the relative outflow and dropoff density are strongly related based on regressions and visualizations.*

Reply: We appreciate the value you see in the individual components of our paper. Our focus, in this present revision, has been on strengthening the connections between the theoretical and empirical parts and making sure they speak to the same consistent message.

R 1.4: *But I am still concerned with on how the two parts are connected, i.e., how well the empirical part supports the theoretical part. As said, a main goal is to compare the access to rides A_i with the demand density D_i. But since both A_i and D_i depend on the unobservable potential demand $\bar{\lambda}_i$, the authors replace $\bar{\lambda}_i$ with the incoming rides $r_i^{\leftarrow}$ and compare the relative outflow RO_i with the dropoff density $D_i^{\leftarrow}$ instead. To me, $\bar{\lambda}_i$ being proportional to $r_i^{\leftarrow}$ is just a heuristic, and I do not find convincing and rigorous justifications in the paper. I think the authors should explicitly state this assumption in the paper, and whether the empirical method is admired or not depends on whether people buy the assumption. This is my main concern of the paper. Regarding the current justification, I have two questions.*

First, in the second paragraph of Section 4.1, the authors argue that the issue of using the number of people who open the app to estimate the potential demand is that " those who need rides in region i may respond to persistently high prices and/or wait times in the region by not going on the app in the first place;" hence, such approach can underestimate the potential demand. Supposing it is the case, then using incoming rides to estimate the potential demand should suffer from the same issue? Thus, it is unclear to me if the asserted advantage of using incoming rides is sound and reliable.

Reply:

We agree that the assumption of potential demand being proportional to incoming rides is a heuristic. The incoming rides are endogenous and affected by price and wait time. Following your suggestion, we have completely changed our empirical approach, given this concern about the validity of using the incoming rides as proxy for potential demand.

In this revision, we do not *proxy* the potential demand by the incoming rides. Instead, we estimate the potential demand from data. Specifically, we observe prices, wages, wage corresponding to outside option, region areas and the number of rides at region-day level, which is our unit of analysis. We identify three sets of model parameters: (1) price sensitivity α which is uniform across regions and days; (2) the region size modeled as $t_i = as_i$, where s_i is the observable region area and a is the parameter to be estimated; (3) potential demand $\bar{\lambda}_i$ at region-day level. All of these parameters, together, are identified using the variation in the data and assumptions of our model. See **??** in the revised manuscript for more detail.

We would like to end the answer to this question with one note: we believe the use of heuristics may indeed be useful when providing summary stats to motivate the modeling decisions. We are doing so in the present version as well, when we visualize summary stats that indicate a relationship between relative outflows and population densities of different boroughs (see **??** in the revised manuscript). If we understand your concern correctly, you were not worried about such use of heuristics per se; you were, rather, more concerned about the heuristic being the main and final outcome of the analysis without formal backing. If our interpretation is incorrect, please let us know and we will remove summary stats that implicitly use population density to proxy potential demand in our next submission (should one be invited).

R 1.5: *Second, the authors try to make the empirical method plausible using Propositions 9 and 10, but this part quite confuses me. Specifically, the two propositions crucially relies on using* $r_1 = r_{12}$, $r_2 = r_{12}$, *and* $\bar{\lambda}_1 = \bar{\lambda}_{12}$, $\bar{\lambda}_2 = \bar{\lambda}_{21}$. *Do we assume all the rides are cross-region here? How can the results extend to a general multi-region case and say anything here? Why we are showing* $\frac{A_1(n)}{A_2(n)} = RO_1$ *rather than each* $A_i(n)$ *being proportional to* RO_i*? If Propositions 9 and 10 are only for a very special two-region case under stringent assumptions, I do not feel the results very valuable.*

Reply: Thank you for raising this point. We have removed Proposition 10 and do not require it given our completely revised empirical approach.

Nevertheless it is useful to give some clarifications regarding a few different points of concern:

1. We do not impose the restriction of two regions in the revision. Rather, the model is generally applicable to an arbitrary number of regions. See ?? in the revision regarding inter-region rides.

2. As ?? also shows, we now define relative outflow across a pair of regions i and j as $RO_{ij} = \frac{r_{ij}}{r_{ji}}$. This pair-wise approach stands in contrast to just comparing inflow and outflow for each region. This new definition of relative flows is connected to access as follows: $\frac{r_{ij}}{r_{ji}} = RO_{ij} = \frac{A_i}{A_j}$. Since r_{ij} and r_{ji} are directly observable from the data, the access ratios can be inferred in directly, assuming the structure of our model.

3. We do not assume that all rides are cross-regional. Rather, we assume a mix of within-region and cross-regional rides. The proportion of rides in region i that are within-region rides is given by $\frac{r_{ii}}{\sum_{j \in I} r_{ij}}$. Following Bimpikis et al. (2019), our assumption is that, for each region, **the ratio of realized rides to the potential demand** is the same within and across regions. That is, the proportion of unfulfilled demand does not depend on the destination.

R 1.6: *Minor Comment 1: I think the number of drivers n_i in region i in the model is assumed to be a real number rather than an integer. Then, it is unclear what "no driver" means in Definition 1 (i.e., item (ii) there). The authors should be more precise in the definition. Specifically, to be an equilibrium, I feel Definition 1 only checks that any small deviation of n_i is unfavorable, rather than considering all the finite deviations, right?*

Reply: Yes, we agree that, more precisely, only small deviations are checked as being unfavorable. Thanks you for bringing this to our attention. We clarify this in ?? in the paper. For similar equilibrium definitions used in the literature, see e.g., Bimpikis et al. (2019).

Aside from the formal definition above, it is also worth intuiting why we concern ourselves only with small deviations as opposed to all finite deviations. A small deviation is meant to model decisions by a single driver. Given that we are modeling the mass of all drivers as continuous, the notion of a "single driver" is not well defined and we need to turn instead to infinitely small masses. Incorporating larger finite deviations into the definition would be equivalent to requiring that no set of drivers can *coordinate with each other* and change their strategy. Two notes about such coordinated actions by drivers: 1) they, in our view, do not reflect the reality of the market; and 2) they do not impact our analysis anyway: in what has now become the main specification of the model in the most current revision, the equilibrium is unique. Given that allowing for more deviations could only shrink the set of equilibria, we do not expect adding those deviations to the definition to change our analysis.

R 1.7: *Minor Comment 2: Where is n^* in the description of Definition 3?*

Reply: We have clarified this in the **??** of the current manuscript.

R 1.8: *Minor Comment 3: Propositions 9 and 10: it should be $I = 2$ rather than $N = 2$.*

Reply: Thanks for pointing this out. We have changed the notation in the statement of the revised propositions. See **??** of the paper.

R 1.9: *Minor Comment 4: What does "App Turnoff" mean in Table 5?*

Reply: Thanks for pointing this out. We are not including the analysis on RideAustin any more in the main text of the revision. We still have it in **??** and there we have clarified the table.

Response to Reviewer 2

We thank you for your big picture as well as detailed guidance. Broadly, your feedback helped us with two major improvements. First, we now have a closer connection between our theoretical and empirical analyses. Both parts are centered around the main claims (A)-(C) (as listed in the **Summary of Major Changes)**; and unlike the previous version, this version directly takes the empirical model to data, doing calibration and counterfactual analysis. Second, we are no longer claiming (implicitly or explicitly) that the source of economies of density is primarily supply. Following your suggestion, we now (i) are clear in the paper that the two sources (supply and demand side EOD) are not separately identifiable from one another using our data, and (ii) demonstrate in both our theoretical and empirical analyses that the results are robust to which side is the major EOD source. In addition to these general directions, we have tried to address your specific comments one by one. Please see below for details.

R 2.1: *I think, as in my previous review, that "the empirical section plausibly shows that low density places are less desirable ride-sharing areas, as measured by relative outflows," and that this may be worse for smaller platforms.*

In the previous review, I continued that the paper "does not establish that access to supply due to driver choice in a relocation game as the authors have set up is an important factor." Here, my view has changed somewhat, but not substantially. The authors in the letter acknowledge that relative outflows do not distinguish between supply-side and demand-side decisions; they now do substantial work using the RideAustin data, showing that there exist driver side decision mechanisms (ending shifts in low-value areas and relocating to the city center between trips). The results align with previous work (Lu, Frazier, Kislev 2018)1, and are intuitive, and the authors should be commended for the work. However, I am still left unconvinced that the authors have shown that these mechanisms are important/first-order. There are two related issues here, both related AE's comment regarding the magnitude of effect sizes:

1. *Is supply-side skew (where drivers are) more important or as important as demand-side density economics? As far as I can tell, no analysis speaks to this question.*
2. *Is supply-side skew a function of driver decisions (supply density economics) or something else?*

Reply: These are important questions, and we agree with your points. We agree that the claim we were making in the previous version (re the role of supply-side economies of density) was not fully backed up by the Ride Austin analysis. In this revision, we follow your suggestion to extend and build upon our robustness analysis, showing that our main insights (A)-(C) do not depend on which side of the market the economies of density is arising from. As a result, we no longer implicitly or explictly claim to have separately identified between supply- and demand-side EOD; and we have moved the Ride Austin analysis to the appendix.

Regarding your more specific sub-questions here:

1. As you pointed out, neither of our analyses (NYC or Austin) can tell separate the role of demand-side EOD from that of supply side. In this revision, we try to be clear about this limitation in multiple places in the paper. We have also shown a strong degree of robustness of *all* of the results that we present to the source of economies of density.
2. Given that our analysis no longer hinges on the entire access skew coming from supply, it is no longer necessary that we provide direct evidence on the magnitude of supply-side EOD by examining driver behavior, which was the sole purpose the Austin analysis was serving. Due to our robustness analysis, it suffices that we show that there is some access skew systematically in favor of denser regions, which is what you point out at the end of your referee letter and what we do provide evidence for. As a consequence of this robustness addition to the revision, and due to the criticisms you brought up on what can be inferred from the Austin analysis, we omit it from the main manuscript. We still provide it in the appendix in case it might be informative. In there, we describe the analysis as "suggestive" evidence for the role of supply and mention the caveats you pointed out. Alternatively, we would be happy to remove it from the appendix as well if that is what the review team advises us to do.

R 2.2: *I would need to see a theoretical results section aligned with an introduction/empirical section that makes it clear to the reader what real-world setting are the paper is modeling, and why the model and results say something interesting about that setting:*

1. Is the intention to model driver decisions to relocate between Staten Island and Manhattan during the day? Or is it to model a driver's decision to drive for Uber in Phoenix, compared to the decision a similar driver is making in New York? Or is it to model the same driver deciding to drive in either Sacramento or SF?

2. Why are each of the models in the proposition informative toward the central setting considered? If there are multiple settings, it should be clear which results map to which one, and how it connects to an overall story. It's ok for some results to be in slightly different/more specific models than other results, but in an applied modeling paper the reader should be able to understand how the pieces fit together for a management recommendation.

3. With the new inter-region model, I am confused about what setting it is trying to capture, both on a timescale and a distance-scale. Does this mean if a ride takes a driver to a suburb, they will just turn the app off and drive home, even if it was just their first trip of the day (as opposed to trying to relocate)?

Reply:

1. Our purpose is closest to the third option you described. We are interested in modeling the decision made by each driver on which borough (Manhattan, Brooklyn, Queens, or The Bronx) if any to operate in. We assume that to arrive at their decisions, drivers consider steady state wages, prices, pickup times, and idle times in those boroughs. Given that drivers' decisions indeed feeds back into idle and pickup times, we use an equilibrium model to analyze what spatial distribution emerges in the market. Then, taking a step back, we ask what prices and wages would the platform optimally set across regions when it expects drivers to behave according to our equilibrium model.

2. Thanks for your suggestion. The choice and arrangement of propositions in the revision is much more focused, tied to the major objectives of the study, and connected to our empirical analysis. Also, in the revised manuscript, we focus on the setting where drivers are allowed free entry and the platform sets prices and wages across regions, and move propositions related to other settings to the appendix. We have also created a table that conveys the central setting of the theory model and the corresponding results. Please see Table 3.

3. You are correct. Once we bring in inter-region rides, it is no longer as clear how to think about the decision making by the same driver on whether to operate in Manhattan or Queens. In particular (and as you pointed out) what if a driver who "chose Manhattan" finds herself in Queens after dropping off a passenger? How much agency does such a driver have in "choosing where to operate"? We address your point here on two fronts: modeling, and connection to the real world.

On the modeling front: The ideal model that has inter-region rides and at the same time allows drivers to "choose where to operate" would have to explicitly capture the drivers' decision among the three options of relocating vacant, looking for rides where they dropped a passenger, and leaving the market.

Our model of inter-region rides deviates from this ideal. More specifically, we make a variant of the "demand-balancedness" assumption of Bimpikis et al. (2019) –who do not study economies of density–whereby we allow the net flow of drivers joining the market in each region from "outside" to be unrestricted. Under such flexibility in the net flow, the market forces that could lead a driver to relocate from i to i' will instead lead to a driver exiting i and another driver joining the market in i'. More specifically, and as discussed in Bimpikis et al. (2019) and in our previous response letter, in such balanced-demand environments one need not worry about vacant relocation. This is because in equilibrium, the most desirable regions deliver as much expected revenue to drivers as does the outside option. Thus, if a driver finds herself in a region that is "worse than another region", this worse region has to also be worse than the outside option. As a result, the driver can leave the market and we need not model her vacant relocation decision.[2] In this balanced-demand environment, it is meaningful to think about the stock-variable notion of "the **number** of drivers operating in region i" n_i in steady state, even though there are non-zero inflows and outflows of drivers into and out of each region which can constantly change the **identities** of those n_i drivers. Nonetheless, we agree that allowing for unbalanced demand (hence vacant relocation) would have been ideal. For a more detailed discussion of why we skipped a model that has both economies of density and "unbalanced demand", please see our previous response letter.

On the front of connection to real world decisions, we note that as anecdotal evidence shows, there is still significance to drivers "choosing where to opearate" even though such choices might be partially disturbed by destinations of different rides. In rideshare forums, one can see use of phrases such as "To those who drive Lyft in the suburbs" implying thre is a choice on where to drive. See appendix A to the main text of our revision for details.

We finish this discussion by making two further points re our model with inter-region rides:

- Note that inter-region rides are *not* necessary for our main three results regarding economies

[2] Another interpretation of this, which is more in line with Bimpikis et al. (2019) comes from high exogenous exit rate of drivers. If a high portion of drivers exogenously exit the market in each region, then the lower steady state number of drivers in less dense regions can be governed only by lower entry rate of drivers into those regions, rendering not only vacant relocation, but also endogenous exit unnecessary.

of density. Essentially, our theoretical model and its main three results could exist without them (and that would be closer to a model of drivers choosing between, say, Sacramento and SF, as opposed to Manhattan and Queens). Nevertheless, inter-region rides, as we argue, contribute to an empirical approach to *measure* economies of density. This is the main and only reason why we incorporate them into the model: to be consistent with our empirical analysis.

- Our restrictions on vacant relocation are stronger than those in Bimpikis et al. (2019). In their paper, the structure of the model in principle allows for vacant relocation of drivers but such relocation does not arise in equilibrium under demand balancedness. In fact they indeed show that once you relax demand balancedness, vacant relocation can happen. We, however, use a variant of the demand-balancedness assumption to justify not modeling vacant relocation.

Table 3: List of Propositions

Proposition	**Model Setting**	**Model Statement**
Proposition 1	model with supply-side EOD and without inter-region rides	Access is skewed towards denser regions.
Proposition 2	model with supply-side EOD and without inter-region rides	The skew is more pronounced for smaller platforms.
Proposition 3	model with supply-side EOD and without inter-region rides	The platform uses prices and wages to mitigate but not eliminate access skew.
Proposition 4	model with demand-side EOD and without inter-region rides	Propositions 1-3 hold in a model with demand-side EOD and without inter-region rides.
Proposition 5	model with supply-side/demand-side EOD and with inter-region rides	Propositions 1-4 hold with inter-region rides.

R 2.3: *Finally, especially given the discussion in the letter about relative outflows not distinguishing between supply and demand side density economics (as well as all the other confounding factors), I'm troubled by the new results (Props 9 and 10) where the authors claim that relative outflows is a good measure for just supply-side access skew.*

Reply:

We still use relative outflows (albeit a slightly modified version of them) as a measure of access skew but we no longer claim explicitly or implicitly that this access skew comes from the supply side.

R 2.4: *I'm satisfied with the functional form simulations, and personally find it acceptable that the paper does not consider competition in the theory – with potentially a brief discussion on how it would or would not affect the analysis.*

Reply: While it would be difficult to speculate on the impact of competition without formal modeling, our conjecture is that it would exacerbate the access skew by breaking up the market into multiple pieces, each thinner than what would emerge under monopoly. We now mention this point in the limitations section of the paper.

R 2.5: *Overall, I'm still a bit confused about the overall message of the paper, and how what we have learned connects to what is claimed. The authors have done a tremendous amount of work (multiple times!) in response to reviews, and there are many pieces that I think would make up a nice paper.*

However, the abstract, introduction, and new results (Prop 9 and 10) continue to claim that the main empirical technique (relative outflows) shows a skew of supply, when it cannot possibly do so (as the authors acknowledge in the letter). The additional supporting analyses with RideAustin help, but as discussed above do not provide convincing evidence that (1) where supply is located is more important than demand density economics, and (2) supply density economics is a strong causal reason for the supply skew. If establishing the effect of spatial supply density economics is the paper's claimed contribution, the challenges discussed in the empirical analysis section above must be overcome; I'd need some sort of evidence comparing the magnitude of the effects of various causes of the access differences. I'm not sure what this evidence would look like.

Reply: We are no longer making the claim (explicitly or implicitly) that access skew arises primarily from supply. Rather, we claim and analyze certain implications of economies of density, demonstrating theoretically and empirically that those implications are robust to whether the main source of economies of density is demand side or supply side. This change was made in response to the concerns you raised.

R 2.6: *At the risk of over-stepping as a reviewer (I trust the authors to make the most appropriate decisions for their paper), I wonder whether this would be a stronger paper if the authors didn't make the empirical claim that driver decisions are a major factor of access differences. The authors would still have the (more convincing to me) empirical claims that there are access differences, and they are worse for smaller platforms.*

Reply:

We are grateful to you for taking this risk of over-stepping! Your suggestion here had a critical role in the revision strategy we devised and implemented.

Per your suggestion, and as you can see throughout this letter and the revised manuscript, we no longer make the empirical claim that the primary source of economies of density is the supply side. We rather focus on the access difference, the effect of platform size on it, and the optimal strategy by the platform in its presence; and we show that our insights are robust to the source of economies of density.

R 2.7: *The new simulations regarding demand-side would suggest that the platform prescription is the same even if demand side density economics are causing the issue.*

Reply: Per the AE's elaboration on your suggestion, we went beyond just using those simple simulations. We now incorporate a demand-side economies of density proposition in the theory model and show that the main results are the same as a model of supply side EOD. We did the same in the empirical analysis, where we show that the emprical model with supply-side EOD and the one with demand-side EOD both lead to similar implications.

Moreover, we took the further step of allowing for a parameter that captures how much of the economies of density comes from demand side v.s. supply side. This is exactly the parameter that we cannot identify using the relative outflows data only. We clearly state this limitation in the paper. Additionally, to argue that this limitation is not fatal to our analysis, we vary this parameter and re-calibrate the rest of the parameters of the model to show that, again, the main results are not sensitive to how we tune this particular parameter.

R 2.8: *And then counter-factual simulations could be run showing what level of driver response to increased wages in low-density areas would be needed to counter-act the access differences.*

Reply: We have run counterfactual simulations showing the wage levels in low-density areas that are needed to counter-act the access differences as you have suggested. These are detailed in ?? of the revised manuscript.

R 2.9: *The current theory could then play a supporting role in establishing (a) that these differences are against the platforms interests, (b) if the wage lever is effective, it is one the platform should use to mitigate but not eliminate the differences, (c) it might be more important but harder for a small platform to do.*

Reply: The current theory supports the claims by showing that profit margins are lower in less dense regions, which is consistent between the supply-side and demand-side theory models. This is suggestive of the fact that the platform mitigates access differences by allowing for lower margins in less dense regions.

In the empirical analysis, we show that the access differences are smaller when platforms optimally customize prices and wages compared to the case when the platform adopts uniform price and wage. Thus, the differences are against the platforms interest, and the platforms uses both wage and price lever mitigate but not eliminate the differences. We also show that as the platform becomes larger, the profit difference between customized and uniform pricing decreases.

R 2.10: *The current empirical analysis could then be used as suggestive evidence on causes, and/or to help calibrate counter-factual simulations on driver responses to counter-act access differences.*

Reply: The counterfactuals for our model are used to derive the following prescriptive recommendations for the regulator:

1. Allowing for platform customized pricing and wages mitigates access skewness across regions.
2. Higher minimum wage for drivers helps mitigate access skewness.
3. As the platform gets larger, less price and wage differences are required to equalize access across regions.

References

Bimpikis, K., Candogan, O., and Saban, D. (2019). Spatial pricing in ride-sharing networks. *Operations Research*, 67(3):744–769.

Rosaia, N. (2020). Competing platforms and transport equilibrium: Evidence from new york city.

Appendix

A Exogenous Variations

In this appendix, we explore several exogenous variations (including those used in Rosaia (2020)) that could potentially help separately identify supply-side EOD and demand-side EOD. Nevertheless, the analysis shows that our main variables of interest (especially the relative outflows across regions) are not too responsive to the exogenous variations. As a result, we conclude that it is challenging to separately identify supply and demand-side EOD using our data. Nevertheless, and as we mentioned above, we do not think this lack of identification undermines our main conclusions given that in the paper we show abundant evidence that our results are robust to the source of EOD.

Below, we describe some of our attempts at finding exogenous variation that could potentially separate between demand- and supply-side EOD.

A.1 Exogenous Variations in Rosaia 2020

In this section, we explore whether the two policies exploited in Rosaia's paper as the sources of exogenous variations affect the outcome variables in the scope of our data as well. Specifically, the two policies, a congestion tax of $2.75 for rides crossing the CBD and a minimum pay standard for the ride-sharing platform, went into effect on Feb. 1st, 2019. However, prior to Feb. 1st, 2019, the TLC dataset does not provide driver pay or passenger fare at ride-level. Instead, only the pickup time and taxi zone location is available for each trip. Therefore, we focus on the measure of the number of rides and the relative outflow, and test if these outcome variables change in a consistent pattern before and after the policy implementation. Fig. 1 shows the trend of the ride volume and the relative outflow before and after the the policy. The overall ride volume increases in February. The relative outflow between Brooklyn and Manhattan increases, and that between Queens and Manhattan decreases, while it is not clear if there are changes in the relative outflows between other pairs of regions.

To control for the effect of seasonality, we run a set of regressions. Specifically, we compare the change in ride volume and RO from January to February between the year of 2018 and 2019. We also control for the day of week, month, year, and holiday effects.

Figure 1: Change in r and RO before and after the Policy Change

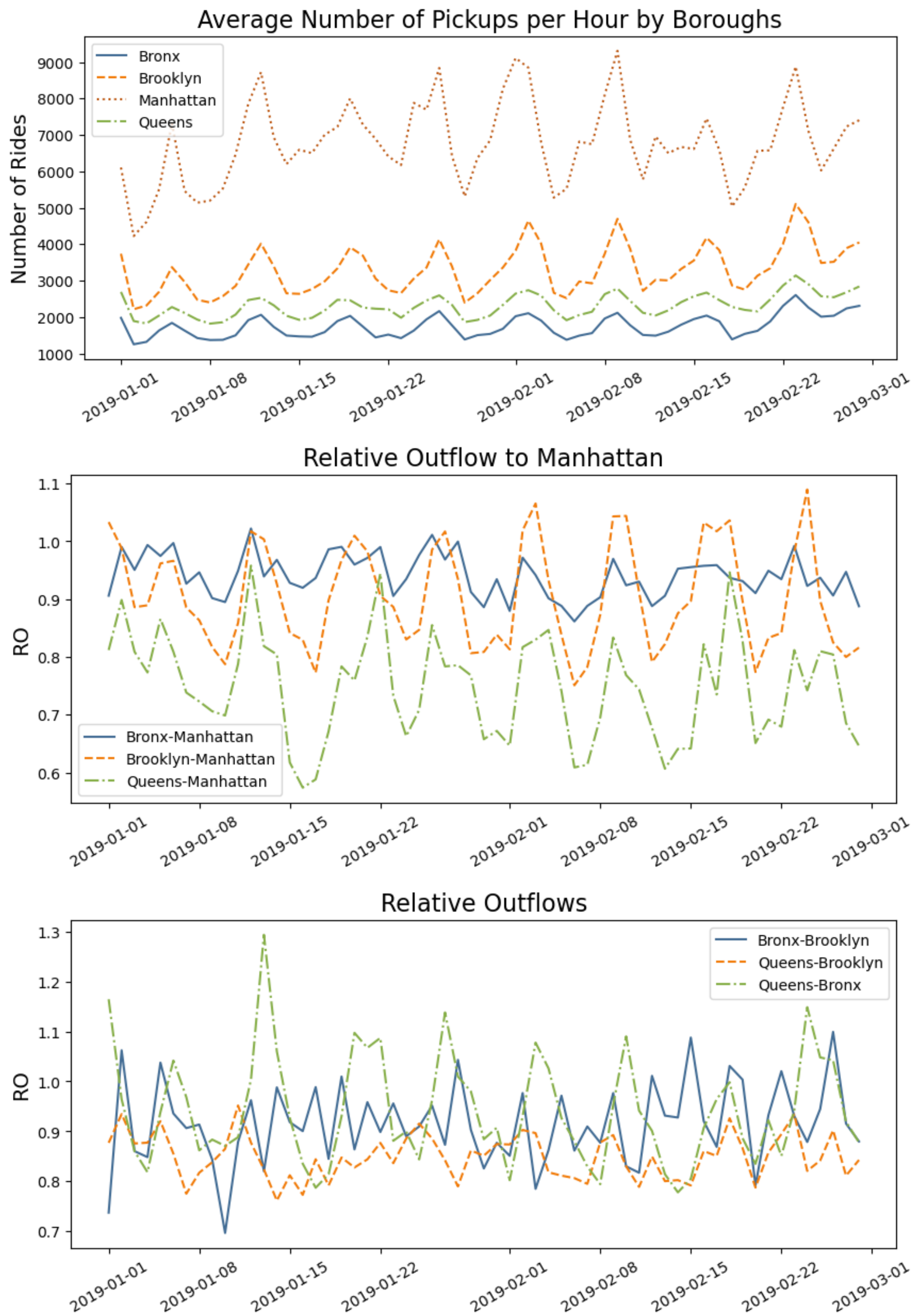


Table 4 shows how r_i in each region responds to the policy change, where the difference is only significant for the rides in Manhattan. Table 5 shows how the relative outflows between each pair of regions changes in response the policy. The impact only shows up in the relative outflow between Bronx and Manhattan. Therefore, it is not clear that the policy change helps identify the model in our context. Note that even if this policy change were indeed helpful, we would still be unable to directly use it in the calibration process carried out in the revision, given that this exogenous change takes place before the time window of the data available to us that has information on prices and wages.

A.2 Strike on May 8th

Besides the exogenous policy changes in Rosaia (2020), we explore two other exogenous shocks that may help identify the our model. On May 8th, 2019, New York Uber drivers went on strike, denouncing the employer payment practices of the platform,[3] while the impact of the strike is controversial.[4] Figure 2 shows the ride volume, average driver pay and average passenger fare across days in May. There is a drop in ride volume on May 8th for Manhattan, while there is no significant change for other boroughs. On the other hand, the driver pay and passenger fare went down in Manhattan on the strike date, while the curves are smooth for other boroughs. If the impact of the strike dominates other shocks to the platform, one would expect that both driver pay and passenger fare increase due to the lack of drivers. Besides, the strike only lasted for one day, which is a relatively short period of time. Therefore, it is not clear to us how it would help with the estimation.

A.3 Subway Construction

Another exogenous shock we consider is the partial hibernation of a subway line between Manhattan and Brooklyn. Starting Apr. 26th, 2019, the L train was partly shut down on nights and weekends for construction.[5] The service was resumed on April 27th, 2020.[6] Given our data covers March through June in 2019, we test if the subway slowdown has an impact on the observables before and after April 26th.

Fig. 3 shows the daily average hourly ride volume, the average driver pay per ride, and the average passenger fare per ride by date. The patterns of change before and after of the subway construction are not obvious. Further, to distinguish between the impact of the policy and the seasonality across months, Table 6 regression of ride volume, price per ride

[3] https://www.nytimes.com/2019/05/08/technology/uber-strike.html

[4] https://nypost.com/2019/05/08/rush-hour-uber-and-lyft-driver-strike-was-a-flop-in-nyc/

[5] https://www.nytimes.com/2019/04/26/nyregion/l-train-repairs-shutdown.html

[6] https://www.6sqft.com/l-train-tunnel-project-is-complete-regular-subway-service-resumes/

Table 4: Regressions on Number of Rides per Hour (r) before and after the Policy Change. *Year2019* and *February* are indicators of whether the observation is from year 2019 and whether the observation is from February. *holiday* is an indicator of whether the observation is from a national holiday. Week trend is the number of weeks passed since the first day of 2018 (if the observation is from year 2018) and the number of weeks passed since the first day of 2019 (if the observation is from year 2019). The variable captures the effect of platform growing larger across time.

	(1)	(2)	(3)	(4)
	Bronx	Brooklyn	Manhattan	Queens
PostPolicy	-104.5	-318.0	-1250.7**	-159.7
	(-0.98)	(-1.69)	(-2.65)	(-1.48)
Year2019	527.1***	24.54	217.0	146.9**
	(9.48)	(0.25)	(0.89)	(2.62)
February	30.53	267.3*	373.5	132.3
	(0.40)	(2.00)	(1.11)	(1.72)
holiday	122.9	597.8***	512.5	325.6***
	(1.74)	(4.82)	(1.64)	(4.57)
WeekTrend2018	13.75	-22.74	31.86	-18.90
	(0.88)	(-0.83)	(0.46)	(-1.20)
WeekTrend2019	71.48***	136.8***	282.5***	76.52***
	(4.57)	(4.99)	(4.10)	(4.87)
day of week FE	Yes	Yes	Yes	Yes
NumObs	118	118	118	118
Dep. Var. Mean	1394.8	3048.5	6468.9	2094.6
$adjR^2$	0.877	0.836	0.683	0.821

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

and wage per ride. Only Queens shows significant increase in driver pay and passenger fare, while there is no significant change in the observables for Manhattan and Brooklyn after the construction. Given that Manhttan and Brooklyn are the regions directly affected by the subway construction, the results do not suggest that the exogenous shock drives major

Table 5: Regressions on RO before and after the Policy Change. *Year2019* and *February* are indicators of whether the observation is from year 2019 and whether the observation is from February. *holiday* is an indicator of whether the observation is from a national holiday. Week trend is the number of weeks passed since the first day of 2018 (if the observation is from year 2018) and the number of weeks passed since the first day of 2019 (if the observation is from year 2019). The variable captures the effect of platform growing larger across time.

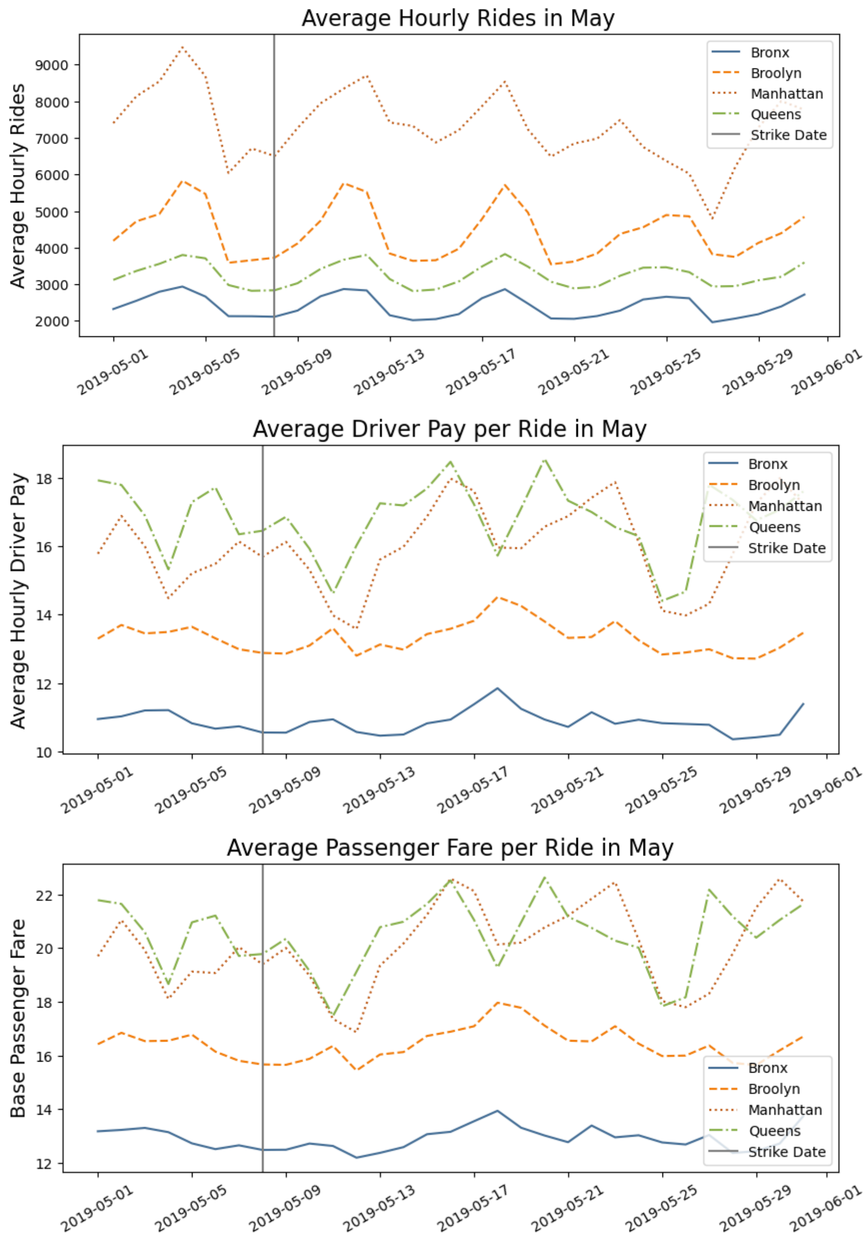
	(1)	(2)	(3)	(4)	(5)	(6)
	RO(Brx,M)	RO(Brk,M)	RO(Q,M)	RO(Brx,Brk)	RO(Q,Brk)	RO(Q,Brx)
PostPolicy	-0.0899***	-0.0193	-0.0422	-0.108	-0.00339	0.0100
	(-4.15)	(-0.66)	(-0.96)	(-1.86)	(-0.12)	(0.20)
Year2019	0.0716***	0.0375*	0.0377	-0.0202	0.0196	0.0382
	(6.36)	(2.47)	(1.65)	(-0.67)	(1.38)	(1.50)
February	0.0390*	0.0382	0.0384	0.0497	-0.00831	-0.0653
	(2.52)	(1.83)	(1.22)	(1.21)	(-0.42)	(-1.87)
holiday	0.00615	0.106***	0.0720*	-0.0331	0.0662***	0.109**
	(0.43)	(5.49)	(2.48)	(-0.87)	(3.65)	(3.36)
WeekTrend2018	-0.00494	-0.00911*	-0.0201**	-0.00679	-0.00351	0.00660
	(-1.56)	(-2.13)	(-3.13)	(-0.80)	(-0.87)	(0.92)
WeekTrend2019	0.00497	-0.00538	-0.00594	0.0175*	0.00192	0.00534
	(1.57)	(-1.26)	(-0.93)	(2.07)	(0.48)	(0.75)
day of week FE	Yes	Yes	Yes	Yes	Yes	Yes
NumObs	118	118	118	118	118	118
Dep. Var. Mean	0.907	0.883	0.713	0.904	0.830	0.924
$adj R^2$	0.672	0.766	0.598	0.132	0.459	0.655

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

change in data patterns.

Figure 2: Ride Volume, Driver Pay, Passenger Fare in May



Moreover, we run regressions using the calibrated potential demand from the model with supply-side EOD and the model with demand-side EOD separately. **??** presents the coefficient estimates. The table suggests that the construction does not drive any significant change in potential demands in Manhattan and Brooklyn in the post-period based on our model estimates.

Figure 3: Ride Volume, Driver Pay, Passenger Fare before and after Subway Construction

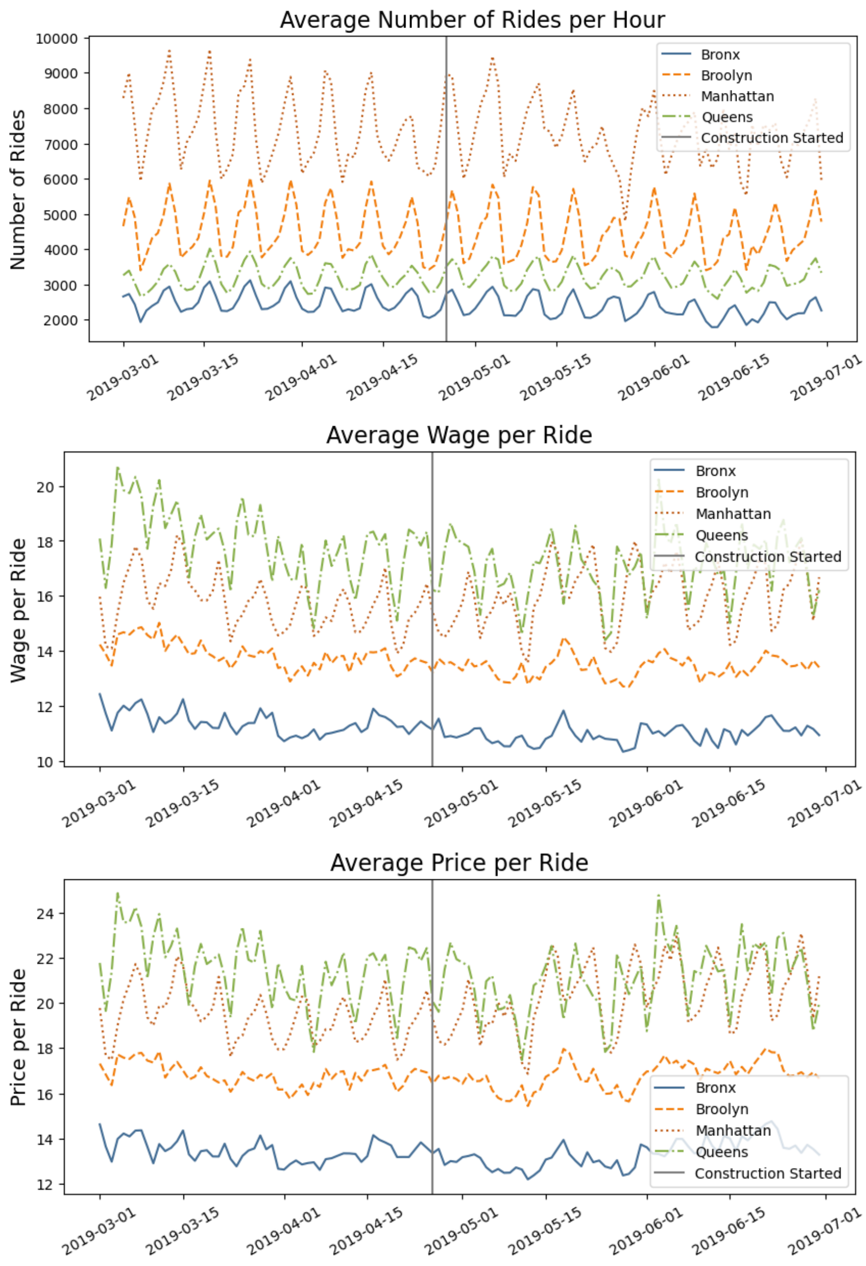


Table 6: Regressions on observables by boroughs. *PostConstruction* is an indicator of whether the date of the observation is after the subway construction. *WeekTrend* is the number of weeks passed since the first day of the data period, which captures the growth of the platform. *holiday* is an indicator of national holidays.

(a) Regression on ride volume (r)

	Bronx	Brooklyn	Manhattan	Queens
PostConstruction	-64.18	12.14	493.8	25.86
	(-0.97)	(0.09)	(1.53)	(0.33)
WeekTrend	-4.474	-19.56	-147.7***	21.86*
	(-0.50)	(-1.14)	(-3.39)	(2.10)
holiday	-30.96	175.3	-399.3	-74.41
	(-0.34)	(1.01)	(-0.91)	(-0.71)
month FE	Yes	Yes	Yes	Yes
day of week FE	Yes	Yes	Yes	Yes
NumObs	122	122	122	122
ymean	2415.3	4483.6	7308.7	3214.4
$adj R^2$	0.860	0.902	0.666	0.832

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

(b) Regressions on Price per Ride (p)

	Bronx	Brooklyn	Manhattan	Queens
PostConstruction	-0.0616	0.197	0.0746	1.019*
	(-0.27)	(0.72)	(0.17)	(2.23)
WeekTrend	0.0106	-0.0394	0.0590	-0.0392
	(0.35)	(-1.07)	(1.02)	(-0.64)
holiday	0.122	-0.169	-0.599	0.404
	(0.40)	(-0.45)	(-1.02)	(0.65)
month FE	Yes	Yes	Yes	Yes
day of week FE	Yes	Yes	Yes	Yes
NumObs	122	122	122	122
ymean	13.38	16.79	19.91	21.29
$adj R^2$	0.476	0.240	0.713	0.707

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

(c) Regressions on Wage per Ride (c)

	Bronx	Brooklyn	Manhattan	Queens
PostConstruction	-0.0311	0.165	0.0130	0.872*
	(-0.19)	(0.77)	(0.04)	(2.34)
WeekTrend	-0.0132	-0.0595*	0.0339	-0.0643
	(-0.60)	(-2.07)	(0.70)	(-1.28)
holiday	0.141	-0.146	-0.566	0.237
	(0.63)	(-0.50)	(-1.16)	(0.47)
month FE	Yes	Yes	Yes	Yes
day of week FE	Yes	Yes	Yes	Yes
NumObs	122	122	122	122
ymean	11.17	13.63	15.89	17.46
$adj R^2$	0.511	0.401	0.653	0.735

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Regressions on calibrated potential demand from the model with supply-side EOD and the model with demand-side EOD by boroughs. *PostConstruction* is an indicator of whether the date of the observation is after the subway construction. *WeekTrend* is the number of weeks passed since the first day of the data period, which captures the growth of the platform. *holiday* is an indicator of national holidays.

(a) Regressions on $\bar{\lambda}$ Calibrated from the Model with Supply-Side EOD

	Bronx	Brooklyn	Manhattan	Queens
PostConstruction	-95.73	69.89	951.1	79.56
	(-1.18)	(0.37)	(1.59)	(0.79)
WeekTrend	0.642	-36.50	-266.2**	35.13*
	(0.06)	(-1.44)	(-3.31)	(2.60)
holiday	-45.17	328.5	-518.1	-95.41
	(-0.41)	(1.28)	(-0.64)	(-0.70)
month FE	Yes	Yes	Yes	Yes
day of week FE	Yes	Yes	Yes	Yes
NumObs	122	122	122	122
ymean	4847.6	8420.1	12940.4	6943.3
$adj R^2$	0.874	0.911	0.598	0.867

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

(b) Regressions on $\bar{\lambda}$ Calibrated from the Model with Demand-Side EOD

	Bronx	Brooklyn	Manhattan	Queens
PostConstruction	-71.62	53.11	784.0	72.69
	(-0.93)	(0.31)	(1.58)	(0.77)
WeekTrend	-5.029	-32.48	-223.6**	27.52*
	(-0.48)	(-1.43)	(-3.34)	(2.17)
holiday	-22.11	280.6	-488.4	-43.73
	(-0.21)	(1.22)	(-0.72)	(-0.34)
month FE	Yes	Yes	Yes	Yes
day of week FE	Yes	Yes	Yes	Yes
NumObs	122	122	122	122
ymean	4182.2	7333.4	11155.6	6008.2
$adj R^2$	0.871	0.908	0.622	0.842

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$