Review Report for Manuscript MS-RMA-21-02699.R1: "Spatial Distribution of Access to Service: Theory and Evidence from Ridesharing"

This is the third time I have reviewed the paper. The main changes are twofold:

1. The revision no longer asserts that the supply-side economies of density is the driving force of the main results. Instead, it shows that both supply-side and demand-side economies of density leads to similar equilibrium characteristics of the market, via both theoretical and empirical analyses.
2. The revision employs a new empirical approach. Specifically, instead of using incoming rides as a proxy for potential demand, they now calibrate the theoretical model using Uber data, based on relative flows of rides among regions (Proposition 6). They then conduct counterfactual simulations to validate the theoretical findings and provide operational insights.

The revision has improved substantially. Both two changes make the theory and empirics parts more consistent. However, I still have several major comments hoping to get addressed.

1 Comments on the theoretical results

Since the empirical part now is a calibration of the theoretical model using real data and then do counterfactual simulation, the theoretical part becomes more important.

Looking into the model, it is not surprising to me that both the supply-side and demand-side EOD models and their combinations obtain similar results (i.e., (A) - (C) in the Summary of Major Changes in the response letter). Furthermore, I feel there is something more central that haven't been brought to light to readers much.

Essentially what makes the main results work is the formulation of idle time and pick-up time and how they are inserted into the model as a building block. Drivers distribute across regions to balance the idle time (which increases linearly with number of servers in a region) and pick-up time (which diminishes to zero as number of drivers grows) in different regions. The supply-side and demand-side EOD models differ in how the pick-up time affects drivers' payoffs. Pick-up time affects drivers' payoffs directly in the supply-side model (because pick-up time is counted toward drivers' total waiting time) and indirectly in the demand-side model (because pick-up time impacts customer demand, which then impacts drivers' payoffs). But no matter in what manner, as long as drivers prefer shorter pick-up and idle times, and the pick-up time diminishes to zero and idle time increases asymptotically linearly when there are more drivers, the main results should (intuitively) hold regardless of a specific model. From Equation (16) in the revision, it is the case for both supply-side and demand-side models and their combinations. Therefore, it is not surprising to me that the main results are robust to whichever model.

As a result, I feel the formulation of idle and pick-up times and how they work as a building block, are more fundamental than the classifications of supply-side and demand-side EOD. It is ideal to generalize the results fully so that the main results hold as long as the pick-up time and idle time affect the drivers in the a particular way. By doing this, we demonstrate to the readers the central thing. Nevertheless, given the amount of work in anticipation, I am ok for the authors discuss the shared features of both the supply-side and demand-side EOD models (and their combinations) that renders them to have the similar market equilibrium results.

2 Other major comments

1. Page 22, Proposition 6: the authors make a critical assumption that $r_{ij}/\bar{\lambda}_{ij} = r_i/\bar{\lambda}_i$, i.e., the ratio of fulfilled rides over potential demands does not depend on the destination. What makes much more sense is to instead assume that the ratio of fulfilled rides over the real requests does not depend on the destination, i.e., $r_{ij}/\lambda_{ij} = r_i/\lambda_i$. Footnote 18 is false: Bimpikis et al. (2019) actually uses the latter one, because it is $\frac{\theta_i(1-F(p_i))}{x_i}$ in the first term of (2) of their paper, rather than $\frac{\theta_i}{x_i}$.

   The authors assume that $r_{ij}/\bar{\lambda}_{ij} = r_i/\bar{\lambda}_i$ only to make their empirical approach work. Beyond that, is there any convincing justification? Could the empirical approach (or other approaches) work when we assume $r_{ij}/\lambda_{ij} = r_i/\lambda_i$, which is more reasonable?

2. Many critical parameters are assumed homogeneous across regions, for example, the f function in $\lambda_{ij} = \bar{\lambda}_{ij} f(p_i, n_i)$, the price sensitivity α, and the sensitivity parameter β to pick-up time. Can the authors extend the theoretical and empirical results to incorporate more realistic region-depend parameters?

3 Minor comments

1. Page 10, Definition 1: should we assume $n_i^* > 0$ for the inequality to hold?

Although the revision has improved substantially, I unfortunately have to recommend a major revision again for the authors to respond to the above comments in their best efforts.