The Congestion Costs of Uber and Lyft

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Abstract

I study the impact of transportation network companies (TNC) on traffic delays using a natural experiment created by the abrupt departure of Uber and Lyft from Austin, Texas. Applying difference in differences and regression discontinuity specifications to high-frequency traffic data, I estimate that Uber and Lyft together decreased traffic speeds in Austin by roughly 2%. Setting-specific estimates of the value of travel time imply that Austinites would be willing to pay $61 million annually to avoid these slowdowns. Back of the envelope calculations suggest this cost is similar in magnitude to the consumer surplus provided by TNCs in Austin.

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1 Introduction

Transportation network companies (TNC) like Uber and Lyft have grown rapidly over the past decade to become integral parts of urban transit systems. A small but growing literature has attributed to these companies benefits that include billions in annual consumer surplus (Cohen et al., 2016), reductions in drunk driving (Greenwood and Wattal, 2016), and flexible work (Angrist et al., 2017; Cramer and Krueger, 2016).

The costs of TNC expansion, however, have yet to receive commensurate treatment in the economics literature. Most notably, TNCs have been accused of contributing to traffic congestion (San Francisco Transit Authority, 2018; Schaller Consulting, 2018), but existing studies of the impact of TNCs on congestion are few, arrive at varied conclusions, and do not quantify the implied congestion costs (Erhardt et al., 2019; Li et al., 2019). Back of the envelope calculations suggests these costs could be substantial. A 2017 Inrix report, for example, placed the annual cost of congestion to US drivers at $305 billion (Inrix, 2018)—roughly two orders of magnitude larger than estimates of national consumer surplus provided by Uber (Cohen et al., 2016). This suggests that if TNCs have even a modest impact on traffic congestion, the negative externalities associated with lengthening travel times could offset consumer surplus benefits. Understanding how and whether TNCs impact traffic congestion therefore plays a crucial role in determining appropriate policy response to the continued growth of these companies.

Two identification problems, however, make causal inference difficult when studying the relationship between TNC activity and traffic congestion. First, Uber and Lyft likely select entry locations based on trends in city-level characteristics unobservable to the econometrician (e.g., car ownership rates). Comparisons that leverage differences in TNC entry dates across locations may therefore suffer from reverse causality. Second, within-city time series regressions may be biased by omitted variables (e.g., gentrification) which are serially correlated with TNC activity and also impact congestion.

In this paper I leverage a natural experiment in Austin, Texas to circumvent these identification challenges: On May 9$^{\text{th}}$, 2016, both Uber and Lyft unexpectedly exited Austin following a vote that upheld a city ordinance requiring driver background checks. I combine this variation in TNC activity with novel and granular Bluetooth traffic speed data, and setting-specific estimates of the willingness to pay for travel time reductions to answer two research questions. First, do transportation network companies impact traffic congestion? And if so, what are the travel-time related costs or benefits of TNC operation?

This setting informs two empirical strategies: a difference in differences comparing pre versus post

May 9th traffic speeds in 2015 (where both companies operated year round) to 2016 (where both companies exit on May 9th), and a regression discontinuity in time. Across specifications, I find that Austin area traffic speeds increased by roughly 2% following the exit of Uber and Lyft. Surprisingly, the largest TNC-related slowdowns occur during the middle of the day (11 a.m. - 2 p.m.), and show no clear spatial pattern. Back of the envelope calculations suggest that Austinites would be willing to pay roughly $61 million annually to avoid these slowdowns. This figure is 2.2% of total Austin-area travel time costs and is roughly the size of estimates of the consumer surplus associated with TNC operation in Austin.

These findings improve on the existing literature in three ways. First, this is to my knowledge the only paper to use the exit of Uber and Lyft to study the impacts of TNCs on congestion. This translates to weaker identifying assumptions than those imposed in analyses leveraging the staggered expansion of these companies. Second, I extend existing analyses by mapping changes in travel speeds to changes in travel time costs, providing the first estimates of the congestion costs associated with TNC activity. And third, the spatial and temporal granularity in the Bluetooth data allows me to perform analyses that contribute to a more complete picture of the heterogeneous impacts of TNC activity on traffic congestion.

These findings also provide several important takeaways for policymakers. First, TNC activity can be viewed roughly as a transfer, as the consumer surplus enjoyed by TNC passengers is of similar size to the time loss incident on incumbent drivers. Second, it is difficult to rationalize TNC restrictions purely on welfare grounds, as the lost consumer surplus may outweigh travel time gains. In other words, even if TNC regulation is more politically achievable than are price-based congestion controls, TNC regulation appears (at least in the Austin case) to be a poor tool to address congestion-related externalities. Lastly, the fact that speeds slow in response to TNC activity suggests TNCs add vehicles miles traveled (VMT) to the transit system. In other words, the VMT avoided by sharing rides are outweighed by additional trips induced by the availability of TNCs.

The rest of the paper is organized as follows. Section 2 describes related literature and background. Section 3 details the events that precipitated the departure of Uber and Lyft from Austin. Section 4 outlines the data sources. I describe my empirical strategy and threats to identification in section 5, and present results in section 6. Section 7 concludes.

2 Background and Related Literature

Traffic congestion is a significant urban disamenity. It is costly (Inrix, 2018), it is associated with lower self-reported happiness (Anderson et al., 2016), and it comes with considerable co-costs in terms of noise and pollution (Currie and Walker, 2011). Although a tax is the canonical policy prescription for congestion (Vickrey, 1969), both theory and empirics suggest that because targeting individual contributions to congestion is difficult, realistic congestion pricing instruments (e.g., cordon charges) may fall well short of the welfare gains achievable by a hypothetical first best policy (Knittel and Sandler, 2018; Prud'homme and Bocarejo, 2005). This, coupled with the potential political advantage of TNC regulation over congestion taxation suggests that understanding the sign and magnitude of TNC related time costs or savings will be important for informing city-level policy. Indeed, several cities have already moved to regulate TNCs in the name of congestion. New York City, for example, cited congestion as a motivation for its 2018 ridesharing cap (New York Times, 2018). Other cities like London and Vancouver have weighed congestion impacts as they deliberate over TNC policy (Reuters, 2019; Vancouver Sun, 2019).

As a number of other observers have noted, however, the impact of TNCs on traffic congestion is theoretically ambiguous. While survey data from Rayle et al. (2014) and Clewlow and Mishra (2017) suggest TNCs induce trips, and Mangrum and Molnar (2018) demonstrate that taxis—the closest analog to TNCs—increase congestion on the margin, Cramer and Krueger (2016) show that Uber drivers spend a significantly higher fraction of their time with a passenger in their vehicle than do taxi drivers. This ride-sharing effect could attenuate or outweigh the effect of induced trips. There may also be complementarities between TNCs and public transit: Hall et al. (2018) use a difference in differences design on measures from the National Transit Database to conclude that Uber is indeed a complement to public transportation. It is unclear, though, whether complementarity between TNCs and public transit will result in more or fewer vehicle trips.

To date there exists little econometric work on whether TNCs cause traffic congestion, and existing results arrive at varied conclusions. Li et al. (2019), for example, use city-level congestion measures and differences in Uber's entry date to estimate the company's impact on congestion, concluding that Uber improves city-level congestion measures. Erhardt et al. (2019), on the other hand, use 2010 and 2016 Inrix traffic data and scraped measures of Uber activity to calibrate a traffic engineering model of San Francisco. They conclude that ridesharing companies were responsible for significant (30%) increases in vehicle hours traveled. In addition to the fact that these studies reach contradicting conclusions, the identification concerns outlined in the introduction suggest value in reassessing this question using a natural experiment.

3 Natural Experiment

Austin is the 11th largest city in the United States, and suffers from considerable congestion: According to Inrix, Austin ranked 14th nationally and 72nd globally in measures of overall congestion in 2018. Similarly ranked cities include San Diego, Berlin, and Manchester. Both Uber and Lyft began operating in Austin in 2014.

In December 2015, the Austin City Council passed ordinance No. 20151217-075, which imposed a series of regulations on TNCs, including data requirements, restrictions on idling locations, and most controversially, fingerprinting requirements to facilitate driver background checks (The City Council of Austin, 2015). Proposition 1, sponsored by Uber and Lyft, attempted to overturn this ordinance. On May 7th, 2016, the Proposition was defeated in a citywide vote, with 56% of voters casting against (The Texas Tribune, 2016). In protest, Uber and Lyft exited the Austin market on May 9th (New York Times, 2016). 13 Months later, Uber and Lyft re-entered Austin as Governor Greg Abbott signed into law HB 100, which overturned Austin's local ordinance (The 85th Texas Legislature, 2017). This variation in TNC activity provides the basis for my empirical identification.

During the yearlong absence of Uber and Lyft, Austin was not without ridesharing. A number of smaller TNCs entered the market or expanded their Austin presence following the defeat of Proposition 1. In date of their arrival in Austin, these companies are: GetMe (December 2015), Fare (Mid-May 2016), Fasten (June 1st 2016), Tride Technologies (June 15th 2015), and RideAustin (June 16th 2016). Wingz, which provides rides to and from the airport, also started operating in Austin in May of 2016. A survey of Austin commuters conducted in November 2016 by Hampshire et al. (2018) offers a view of take up of these alternative rideshare companies. RideAustin held the largest market share (47.4%), followed by Fasten (34.5%), Fare (12.9%), GetMe (2.8%), Wingz (1.6%), and Tride (0.4%). Informed by the Hampshire et al. survey and the universe of RideAustin's 2016 trip-level data, I am able to infer the level of total TNC activity in Austin following the exit of Uber and Lyft. I can therefore identify a window following the proposition 1 vote where alternative TNC activity is negligable (see section 5.1).

4 Data

I use data collected from an array of Bluetooth sensors along major roadways (both freeway and surface-level) operated by the Austin Department of Transportation. Located inside traffic signal cabinets, these sensors detect unpaired Bluetooth devices (e.g., smartphones, car systems), and estimate traffic speeds based on the movement of single devices (which are given unique anonymous identifiers) through the network of sensors.

I use an aggregated version of this dataset prepared by Post Oak Traffic Systems, which isolates device movements through specific road segments (henceforth *segments*), which are short sections along just one road. This company pre-processes the data in several ways. Data are aggregated at 15 minute bins, and represent the average speed across the segment for devices that appear at the origin reader first, and then the destination reader, and do not appear at any other sensors in the interim. These data are also filtered for outliers: only observations that fall within 75% of the IQR of the previous 15 observations are used in calculating speeds. This type of filtering is applied to combat bias from the movement of non-vehicle Bluetooth devices (like those carried by pedestrians) through the sensor network.

In addition to the data cleaning performed by Post Oak Traffic Systems, I further restrict my sample to consitently-reporting sensors. Of the 430 total segments, I drop segments that report in fewer than 70% of days during each year (2015, 2016) of the study period, leaving me with panel of 79 segments. For robustness I also report results using a) all segments that report in more than 30% of study period days and b) only segments that report during 100% of study period days.

The 79 segments I use in my preferred specification are plotted in Figure 1 and summarized in Table 1. The mean segment length is 0.73 miles, with minimum and maximum lengths of 0.06 and 3.8 miles, respectively. I observe 966,301 15-minute speed reports during my study period. On average, a segment sees 4.77 devices move from origin to destination during each 15 minute period, meaning that my data summarize roughly 4.6 million segment traverses. The average travel speed is 2.94 minutes per mile, which corresponds to 20.41 miles per hour. This figure is consistent with periods of significant congestion, as the lowest posted speed limits in the region are 30 mph.

My variable of interest is minutes per mile, which has two advantages over miles per hour. First, a change of one mile per hour does not represent a constant damage over the domain of this variable: In terms of time lost, changing from 5 to 4 miles per hour is roughly 20 times as costly as changing from 20 to 19 miles per hour. Second, multiplying outcomes in minutes per mile by estimates of the value of time is a straightforward way to arrive at cost calcuations from changes in traffic delays.

While novel and granular, the Bluetooth data bring challenges for estimation. Namely, if filtering does not eliminate all measurement error originating from Bluetooth devices used by Austinites walking or biking, and the use of these modes of transit is correlated with the period where Uber and Lyft exited Austin, the empirical strategies I describe below will arrive at biased estimates. I further investigate this and other threats to identification in section 5.4.

I compile several other datasets to augment my analysis. To ameliorate weather-related noise, I use precipitation and temperature data accessed through the National Oceanographic and Atmospheric Administration's National Centers for Environmental Information. To isolate a period of time where the impact of other TNCs is minimal, I use RideAustin's trip-level data. These data range from June 2$^{\text{nd}}$, 2016 to April 13$^{\text{th}}$, 2017, and are publicly available through the company's DataWorld account. Lastly, to map changes in travel times to costs I use toll data from the MoPac variable price freeway in Austin. These were provided courtesy of the Central Texas Regional Mobility Authority, and are further detailed in Appendix A.

5 Empirical Strategy

5.1 Timeframe

My traffic speed data range from February 2015 to March 2017. I truncate this window to isolate periods where the variation in traffic speeds can be credibly attributed to the failure of Proposition 1. As described in section 3, a number of TNCs entered the market following the exit of Uber and Lyft. Estimations using the entire yearlong suspension period as a comparison would therefore underestimate any changes relative to a TNC-free counterfactual. Informed by the universe of trips from RideAustin—the TNC that enjoyed the largest market share during Uber and Lyft's absence—I truncate my estimation period on August 1$^{\text{st}}$, 2016. Similarly, Austin hosts the South by Southwest Music Festival (SXSW) each March. I restrict my analysis to exclude the 2015 and 2016 festivals. This leaves me with data from March 20$^{\text{th}}$ to August 1$^{\text{st}}$ for both 2015 and 2016. The 2016 study period is plotted with TNC data in Figure 2.

5.2 Difference in Differences

To study the effect of the exit of Uber and Lyft on travel times, I compare traffic speeds pre and post May 9$^{\text{th}}$ in 2016 (where Uber and Lyft exited) to 2015 (where both companies operated year-round). To capture heterogeneity in the congestion impacts across time of day, I perform this comparison within each hour of day, h (or equivalently, interacting each right-hand side term below with an hour-of-day dummy):

$$s_{i,y,t} = \alpha + \beta_h \delta_y \eta_t + \gamma_1 \delta_y + \gamma_2 \eta_t + \boldsymbol{\gamma}_3 \delta_y \boldsymbol{\theta}_i \cdot t + \boldsymbol{\gamma}_4 \boldsymbol{\theta}_i + \boldsymbol{\Gamma} \boldsymbol{X}_{y,t} + \epsilon_{i,y,t} \tag{1}$$

Where $s_{i,y,t}$ is the speed (in minutes per mile) measured over segment i on day t of year y. δ_t is a dummy that equals one for the year 2016, and η_t is a dummy that equals one for days (in any year) after May 9^{th}. $\boldsymbol{\theta_i}$ is a set of dummies for each road segment, and t is the signed number of days between a given date and May 9^{th} of that year. $\boldsymbol{X_t}$ is a vector of controls that includes day of week fixed effects, holiday fixed effects, 10 10-degree daily temperature bins, and 10 daily precipitation level bins. The interacton betwen $\delta_y \eta_t$ is the treatment indicator, as it takes a value of 1 for observations after May 9^{th}, 2016, and zero otherwise. $\delta_y \boldsymbol{\theta}_i \cdot t$ are segment-year specific linear time trends.

The identifying assumption in the estimation of β_h—the effect of Uber and Lyft operation on travel speeds during a given hour of day h—is that conditional on seasonality and weather, the difference in travel speeds between 2016 and 2015 at hour h does not change after May 9^{th} for reasons other than the operation of Uber and Lyft.

I calculate hour-specific congestion impacts with the goal of producing more accurate cost estimates. As I show in Appendix A, variable-toll data suggest that the value of travel time in Austin varies significantly from hour to hour. Similarly, the number of vehicles on the road peaks during rush hours. Together, this information suggests that the same change in traffic speeds could produce different aggregate congestion costs at different times of day. By matching hour-specific estimates of the impact of TNCs to hour-specific vehicle miles traveled (VMT) and hour-specific estimates of the value of travel time, my cost calculations account for temporal heterogeneity that pooled estimates may not reflect. To demonstrate this idea, I also estimate a model pooling across hours of day. This estimator is equation 1, but run without interacting hour of day fixed effects with the right hand side variables. The rationale for this regression is to simulate what estimation and inference might look like using aggregated data.

In order to investigate spatial heterogeneity, I estimate a model pooling over hours of day and allowing an idosyncratic treatment effect for each road segment. This model is equivalent to equation 1, but interacts the set of segment dummies with the treatment indicator, $\delta_y \eta_t$. $\boldsymbol{\beta}$ is now a 1x79 vector of segment-specific treatment effect estimates. Note that in this pooled equation hour of day fixed effects are included in X_t.

$$s_{i,y,t} = \alpha + \boldsymbol{\beta}\delta_y \eta_t \boldsymbol{\theta}_i + \gamma_1 \delta_y + \gamma_2 \eta_t + \boldsymbol{\gamma}_3 \delta_y \boldsymbol{\theta}_i \cdot t + \boldsymbol{\gamma}_4 \boldsymbol{\theta}_i + \boldsymbol{\Gamma} \boldsymbol{X}_{y,t} + \epsilon_{i,y,t} \tag{2}$$

5.3 Regression Discontinuity

Lastly, I estimate a regression discontinuity model, again estimating hour-specific treatment effects (β_h) by interacting each term in the regression equation with a set of hour-of-day fixed effects.

$$s_{i,t} = \alpha + \beta_h \eta_t + \boldsymbol{\gamma}_1 \boldsymbol{\theta}_i + \boldsymbol{\gamma_2} \boldsymbol{\theta}_i \cdot t + \gamma_2 \boldsymbol{\theta_i} \cdot t^2 + \boldsymbol{\gamma_4} \boldsymbol{\theta_i} + \boldsymbol{\Gamma X_t} + \epsilon_{i,t} \tag{3}$$

The identifying assumption for β_h is that conditional on weather, potential outcomes (traffic speeds) in hour of day h are continuous about May 9$^{\text{th}}$, 2016. While the identifying assumption for the RD is aruably weaker than that of the difference in differences estimator, the RD will produce estimates of the short-term response to the exit of Uber and Lyft. As such, I rely on the difference in difference estimator to produce my preferred annual congestion cost figures.

5.4 Threats to Identification

Outside of unobserved events impacting traffic within a specific season of a specific year, I note two main identification threats.

Threat 1: Other Modes of Transportation. If the exit of Uber and Lyft lead Austinites to substitute toward walking or biking, and these trips were not correctly dropped as outliers during data processing, β will not be identified. In other words, for other modes of transportation to bias my estimates, traffic speed must be mismeasured, and that mismeasurment must be correlated with the treatment.

Data on mode shares and mode speeds suggest that this type of bias cannot alone account for my results. Hampshire et al. (2018) suggest 1.8% of TNC users switch to bikes following Uber's exit. If TNCs made up 10% of Austin trips, and bikes constituted 1.53% (United States Census Bureau, 2015), this mode shifting represents an 11.8% increase in total bike trip volume. The average car in my sample took 2.94 minutes to traverse a mile—3.06 minutes per mile fewer than the 6 minutes per mile (10 mph) assumed by Google biking directions. These figures imply that for changes in bike shares to alone account for a change of 0.1 minutes per mile (roughly the average treatmenet effect across daytime hours), bikes would need to constitute roughly 28% of observed Bluetooth samples *after* dropping extreme travel time outliers. This figure is inconsistent with the travel speeds implied by the movement of Bluetooth devices, which greatly exceed 10 miles per hour on average.

Nonetheless, I draw on a second traffic speed dataset to empirically examine this concern. In addtion to Bluetooth sensors, the city of Austin also maintains pneumatic sensors which take periodic measurements of traffic speeds. While these measurements are not frequent enough to act as a replacement dependent variable, they do allow me to study the relationship between Bluetooth speed measurements and true traffic speeds by matching segments to pneumatic sensors.

While we should not expect pneumatic sensors to match segment speeds exactly (segments often include intersections), if there is significant switching to non-vehicle modes of transport that biases the Bluetooth speed measurments, this would be reflected in a change in the *relationship* between the two measurments. For example, say we have a segment-sensor pair, and prior to May 9$^{\text{th}}$, 2016, when the pneumatic sensor reports a speed of 25 mph, the Bluetooth segment on average reports a speed of 20 mph. If there is bias from mode-switching, we would expect this relationship to change in the post period. Now, when the pneumatic sensor again registers 25 mph, the incresed number of non-filtered pedestrian datapoints biases the segment measurement downward, to, say, 18 mph.

To operationalize this anecdote, I match segments to pneumatic sensors, and run a regression of segment speeds on sensor speeds, allowing for a differential slope term interacted with a post May 9$^{\text{th}}$ 2016 dummy. If I find a statistically (and economically) significant difference in slopes, I treat this as evidence of mode-choice related bias.

This exercise is detailed in Appendix B. I match 39 Bluetooth segments to pneumatic road sensors. In a simple regression with month of year and road segment fixed effects, I find little evidence to support pedestrian-induced bias in my estimates. As shown in B1, the coefficient on the interaction between the post dummy and the pneumatic segment speed is not statistically different from zero, nor is it of meaningful magnitude.

Threat 2: TNC Driving Speeds. If TNC vehicles drive significantly slower or faster than the average non-TNC vehicle in a way that remains after filtering, the above estimates of β_h will be biased. During congested conditions it is unlikely that this should occur: if congestion slows all drivers, then travel time measurements from any subset of vehicles should be representative of average speeds. At free-flow traffic speeds, however, it is possible that TNCs drive faster (due to profit motive) or slower (idling to find riders) than do non-TNC vehicles.

To test these concerns, I use public trip-level data from the startup RideAustin, which entered the market following the departure of Uber and Lyft. Following Mangrum and Molnar (2018), who construct "taxi races" to test whether different types of taxi travel at different speeds, I match RideAustin trips to Bluetooth segments, allowing me to test the null hypothesis that TNC vehicles drive at the same speeds as the average mix of vehicles.

This exercise is detailed in Appendix C. Over 221 trip-segment matches, I find that on average RideAustin vehicles traveled 0.03 minutes per mile slower while traversing a given segment than did the average device during the same time period. This difference is not statistically significant, nor should it meaningfully bias my results. Assuming TNCs account for 10% of vehicle trips, for example, this difference in speeds implies a bias on the order of 0.003 minutes per mile—one to two orders of magnitude smaller than my estimates of the impact of TNCs on traffic speeds. To the extent that speeds differences do generate bias, they will lead me to overstate improvements in traffic speeds resulting from a TNC ban. As such, I interpret my results as an upper bound for the travel time costs of TNC activity.

6 Results

6.1 Traffic Speeds

Across multiple specifications, I find evidence of modest increases in traffic speeds following the exit of Uber and Lyft. Results from my preferred specification (equation 1) are displayed in Table 2 and Figure 3. Point estimates of changes in minutes per mile are largely negative, suggesting reduced congestion after the exit of Uber and Lyft. While the 95% confidence intervals for hour-specific estimates of congestion generally include zero, an F-test rejects the null hypothesis of $\beta_h = 0 \ \forall h$ ($p < 0.0001$). Although TNCs appear to negatively impact morning rush hour conditions, I estimate little change in evening rush hour speeds. The largest improvements in travel times following TNC exit come, surprisingly, between 11 a.m. and 2 p.m. This pattern could be a result of TNCs comprising a higher share of vehicles during the middle of the day than during peak hours. Alternatively, evening rush hour effects could be muted if TNC users are more likely to share cars during the evening than they are during the morning and early afternoon. Point estimates for off-peak hours (8 p.m. - 6 a.m.) are small and straddle zero.

Figure 5 and Table 3 display results from equation 3, a regression discontinuity by hour of day. These figures are qualitatively similar to, but larger in magnitude than the difference in differences results, suggesting that the short-term impacts of TNC exit may be more pronounced than the medium-term impacts. Figure 6 plots residuals from a pooled regression discontinuity performed on daytime traffic speeds in 2016 (when Uber and Lyft exited) and 2015 (where both companies operated year-round). That the 2015 regresion discontinuity estimates a null effect offers evidence that the 2016 regression discontinuity results are not driven by seasonal changes in traffic patterns. I investigate the robustness of both the difference in differences and regression discontinuity results over a range of alternative specifications and bandwidth choices in Appendix D.

Table 4 shows the results from running versions of equations 1 and 3, pooling across hours. The pooled difference in differences results suggest that on average, speeds increase by -0.056 ($p = 0.03$) minutes per mile (1.9%) following TNC exit. Consistent with the hour-specific estimates, restricting the pooled DD analysis to daytime hours (7 a.m. to 7 p.m.) generates larger estimates of speed increases following TNC exit ($\beta = -0.089$, $p = 0.007$). Figure 4 displays an event study of raw speed data for daytime traffic by week of year. This Figure is consistent with the results from equation 1, and serves as evidence corroborating the parallel trends assumption. As in the hour-specific estimates, the pooled RD estimates are larger in magnitude than are the DD results (-0.102, $p = 0$), and imply a 3.4% increase in travel speeds immediately following the exit of Uber and Lyft.

6.2 Heterogeneity and External Validity

Results from equation 2, which allows for segment-specific congestion responses, are plotted in Figures 7 and 8. This analysis is ad hoc, and should be viewed as motivation for further research into the heterogeneous impacts of TNC congestion, rather than as a set of well-identified results. With this qualifier in mind, two themes emerge. First, to the extent that there are large positive or negative treatment effect outliers, these occur near the city center. Second, Figure 7 shows that segments that experienced exceptionally large changes in traffic speed were characterized by exceptionally high levels pre-period traffic congestion, suggesting construction or other segment-specific shocks may explain these estimates. Absent these outliers, the segment-specific effects exhibit relatively low variance. An important avenue for future work would be to investigate whether these outliers indeed represent extreme congestion reductions from TNC operation in select locations, or whether they can be explained by data absent from this setting.

The external validity of the results presented in this paper hinges on whether Austin is representative of other metropolitan areas in terms of commuter preferences and the substitutability of transit options. To determine which cities have transit ecosystems that resemble Austin's, Figure 9 depicts how public transit use and personal vehicle travel vary across the 20 largest metro areas in the US. Commuters in the majority of large American cities (especially those located in the "Sun Belt") exhibit mode choices similar to those in Austin, where commuters heavily favor solo commutes in personal vehicles. In cities with extensive public transit systems (e.g. New York, Washington, San Francisco), however, commuter choices are quite different than they are in Austin. This suggests caution when applying the results described in this paper to address policy questions in these metro areas.

6.3 Congestion Costs

Armed with estimates of hour-specific changes in travel times, I calculate the external congestion cost associated with TNC operation as follows:

$$\Delta\textbf{congestion cost} = \sum_h \Delta \textbf{ minutes per mile}_h * \textbf{miles driven}_h * \textbf{value of time}_h \tag{4}$$

Δ*minutes per mile* are the coefficients, by hour of day, h, estimated above. To estimate *miles driven*$_h$, I use periodic traffic counts to estimate the share of VMT by hour of day in Austin, and multiply these shares by estimates of daily VMT provided by the Texas Department of Transportation. Note that this operation assumes that my estimates represent an average effect for all VMT within the Austin City limits. Table 5 provides evidence in support of this assumption: According to data maintaned by the Texas Department of Transportation, roads included in my preferred specification resemble those not included in terms of congestion, VMT, and speed. Finally, I calculate an Austin-specific willingness to pay (WTP) for reductions in travel time (*value of time*$_h$, above) using data from Austin's variable-toll freeway (see Appendix A). This sum can then be written in vector notation:

$$\Delta\textbf{congestion cost} = \boldsymbol{\beta}\mathbf{R} \tag{5}$$

Where element h of vector R equals *miles driven*$_h$ * *value of time*$_h$. The standard error of this object is then $\sigma_w = \mathbf{R'\Sigma R}$, where Σ is the covariance matrix associated with $\boldsymbol{\beta}$. Note that this standard error reflects only uncertainty in the travel speed parameters; neither uncertainty the WTP estimates, nor model uncertainty are reflected in this figure.

I report the results of this exercise in Table 6. Using my preferred specification (equation 1), I recover estimates of daily congestion costs associated with Uber and Lyft activity of $169,231, with a standard error of $43,069, suggesting that TNCs result in an economically meaningful and statistically significant increase in costs associated with traffic congestion. These estimates correspond to annual costs of $61,769,392.

Several outside studies provide valuable context when interpreting these numbers. First, according the Inrix Global Scorecard, the aggregate 2017 travel time cost in Austin, TX was $2.8 billion. My estimates therefore suggest that Uber and Lyft together accounted for roughly 2.2% of travel time costs in Austin. Second, estimates of consumer surplus assocated with TNCs offer a useful benchmark for policymakers. Because TNC operation offers benefits not captured by consumer surplus, a necessary but not sufficient condition for limits on TNC activity to be a welfare-improving policy is that congestion costs must be strictly larger than estimates of consumer surplus. Cohen et al. conclude that in the four cities they examine, $1.57 of consumer surplus are generated for every dollar spent on TNCs. Although Austin is not one of these 4 cities, Uber reported that in 2015 it's drivers grossed $27 million in the Austin area (Uber, 2015), suggesting consumer surplus on the order of $42 million for Austinites using Uber in 2015. Applying the 70% Uber market share estimates reported by Hampshire et al. (2018), and assuming an equal expenditure-consumer surplus ratio between Lyft and Uber implies a total TNC-related consumer surplus for the city of austin of roughly $60 million.

If we impose the assumption that Uber activity in Austin was not meaningfully different than that of an average urban area in the US, we can estimate consumer surplus in a second way. Cohen et al. estimate the 2015 national consumer surplus from Uber at $6.8 billion. Multiplying this figure by Austin's share of the US urban population (0.49%) yields an estimate of 2015 consumer surplus of $33 million annually. Again inflating this using Uber's market share suggests a TNC-related consumer surplus for the city of austin of roughly $47 million.

As a final note, a comparison of the cost estimates produced by my preferred specification to those generated by the pooled estimates suggests that the convolution between congestion impacts and value of time fluctuations is consequential. As reported in Table 4, applying an inflation-adjusted $11.91 figure suggested by Small and Verhoef (2007) to my pooled estimator yields an annual congestion cost of $54,936,372—roughly 12 percent lower than the estimate using temporal disaggregation.

7 Conclusion

Using a natural experiment in Austin, TX, I study whether transportation network companies impact traffic speeds. I estimate that on average, TNCs are responsible for a 1.9% increase in Austin-area congestion. This figure masks important heterogeneity, with the largest TNC-related slowdowns coming between 11 a.m. and 2 p.m. By matching hour-specific changes in traffic speeds to hour-specific estimates of the value of travel time, I find that Austinites would be willing to pay $61 million annually to avoid the slowdowns induced by TNC activity. Back of the envelope calculations using estimates of TNC consumer surplus from Cohen et al. (2016) suggest that the cost of TNC-related congestion in Austin is of similar magnitude to the consumer surplus generated by these companies.

These findings are policy-relevant, especially in cities with low rates of public transit use. If the Austin case holds for cities with similar transit ecosystems, TNCs should not be viewed as a solution to urban traffic congestion, which runs counter to claims made by both Uber and Lyft. At the same time, my findings also caution against using TNC regulation as a means for addressing congestion. In theory, if TNCs were to operate at places and times with high external marginal costs of congestion, regulating TNCs directly could be part of an optimal second-best congestion policy. I do not find this to be the case in Austin: If consumer surplus and congestion costs are roughly equal in size, then other benefits provided by TNCS (producer surplus, flexible work, and reductions in drunk driving) suggest that policies restricting TNC operations are unlikely to be welfare-improving. Instead, we can view the commuter-side costs and benefits of TNC operation as a rough transfer from incumbent drivers (who pay via time losses) to ridesharing users (who gain consumer surplus).

These results also pose several questions that may inform future research. Replicating this type of analysis in other settings with similar identification opportunities would provide a valuable test of external validity. Identifying the drivers of the spatial and temporal heterogeneity in congestion responses could also be useful to decisionmakers in both the policy and industry spheres. Lastly, that speeds slow in response to TNC activity suggests TNCs add vehicles to the road. In other words, my results suggest the ride-induction effect dominates the ride-sharing effect. This conclusion will be important to test in other cities, as the impact of TNCs on VMT is an important uncertainty in the prediction of transportation sector emissions.

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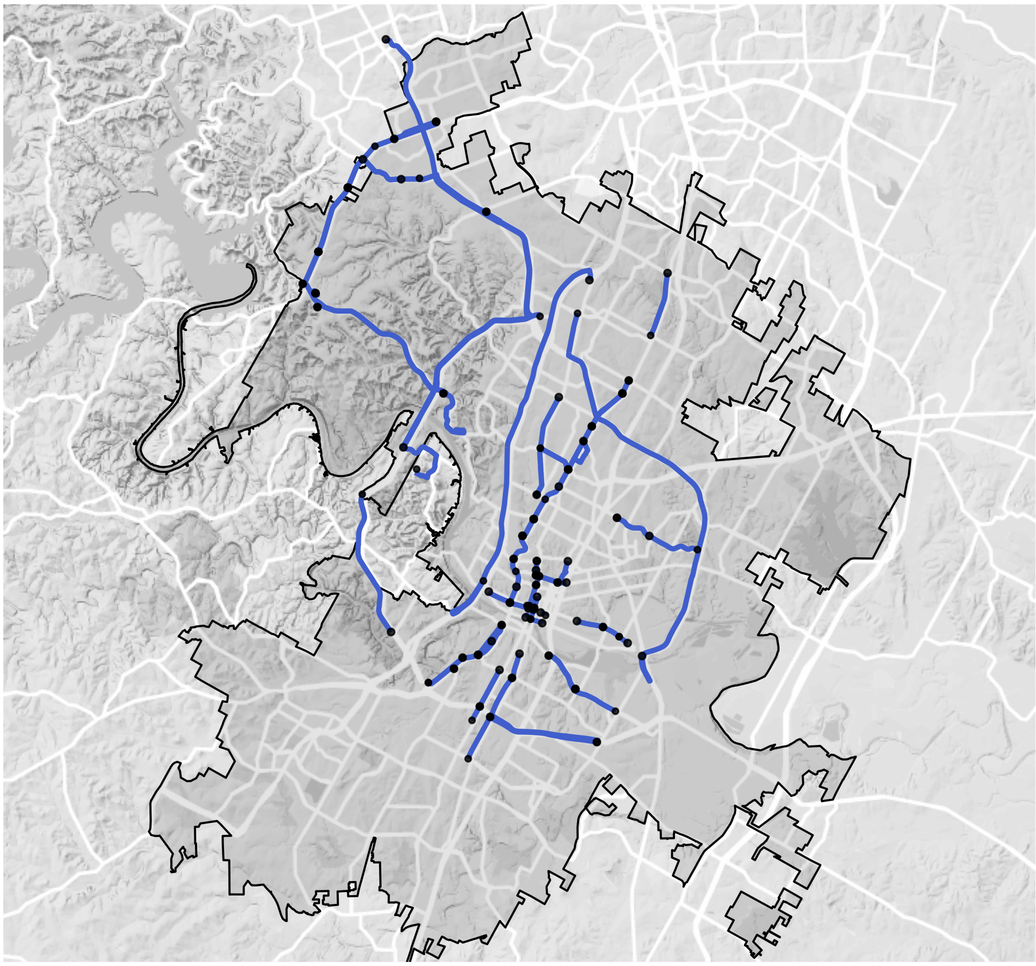
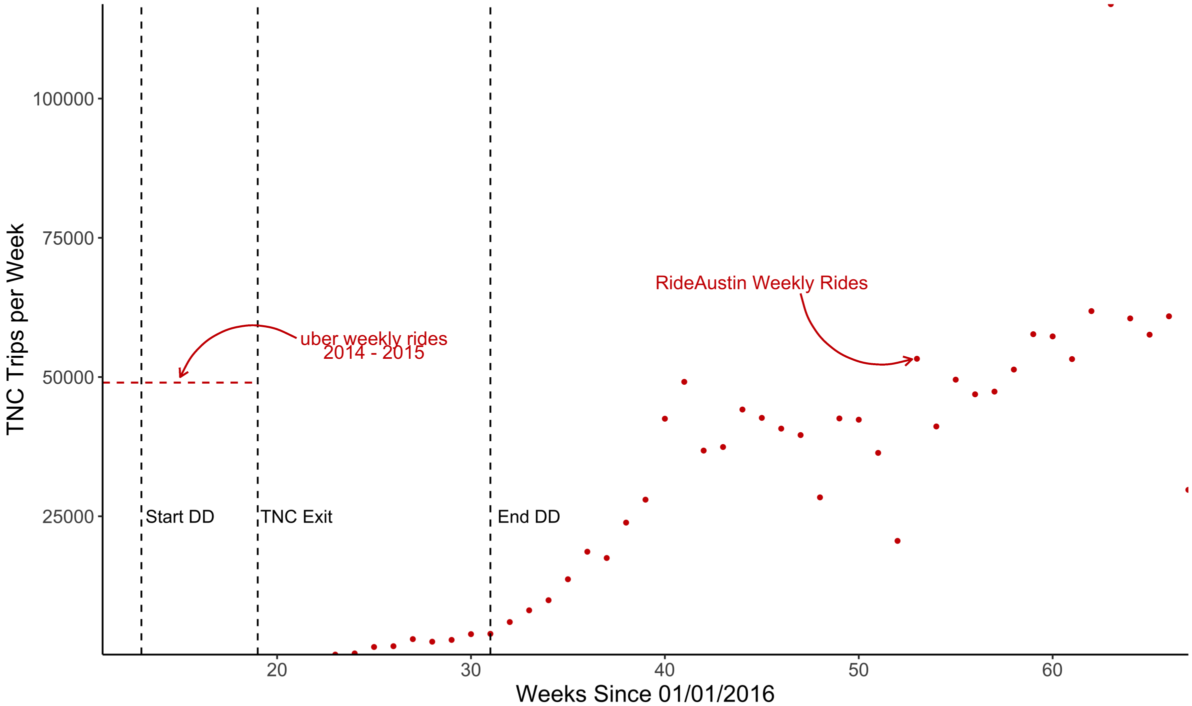
8 Figures

Figure 1—Bluetooth Segment Locations in Austin, TX



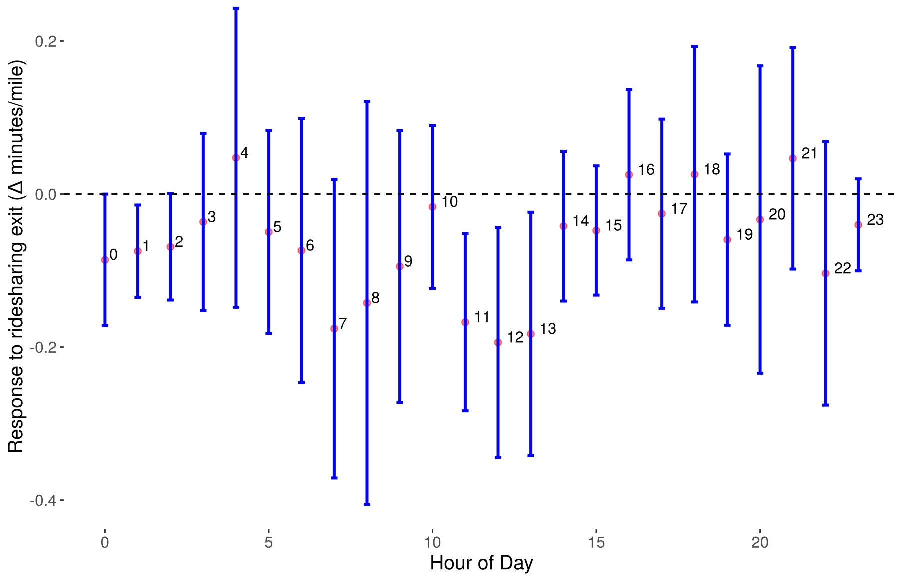
Notes: Nodes represent terminal Bluetooth sensor locations for each of the 79 segments used in analysis. Note that some sensors act as both origin and destination readers for different segments. Paths represent Google Maps recommended driving directions between endpoints of a given segment. The black line is the Austin city limit.

Figure 2—Variation in TNC Activity



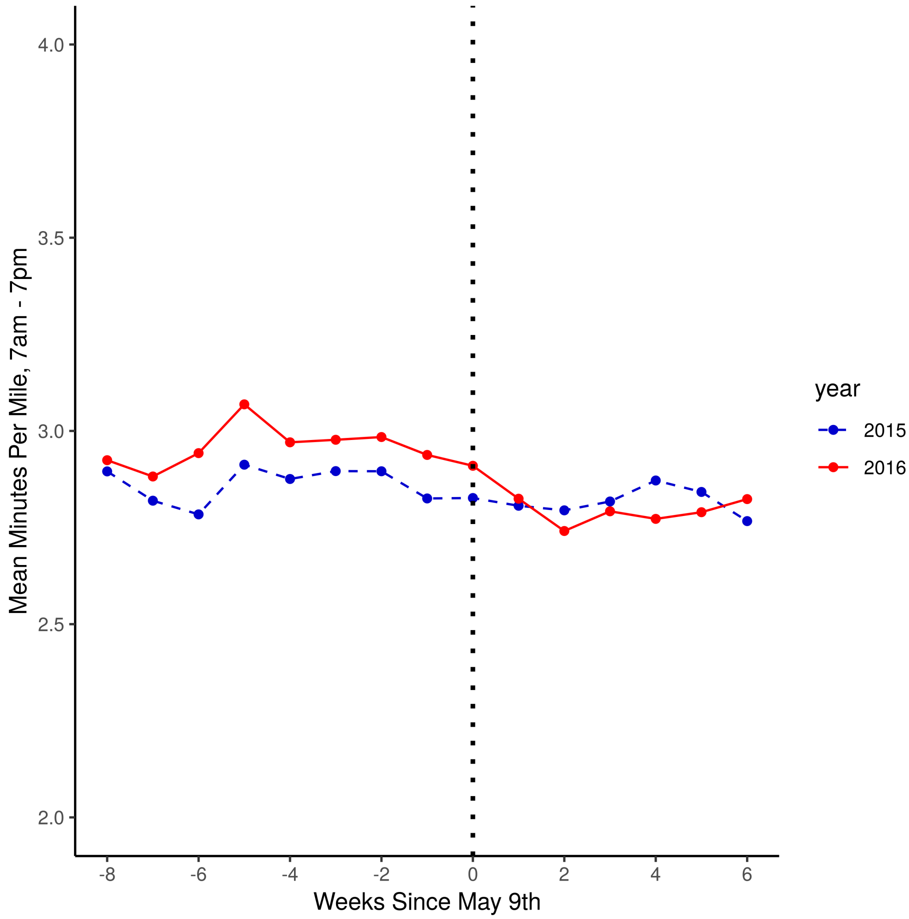
Notes: This graph plots weekly RideAustin activity for their first 9 months of the company's operation. From left to right, the vertical lines represent the start of the 2016 difference in differences period (March 20th), the failure of proposition 1 (May 9th), and the end of the 2016 difference in difference period period (August 1st). The average number of Uber trips per week (as per an Uber Report on 2014-2015 operations) is provided as a baseline for comparison. Note that because Uber's market share in Austin was roughly 70%, and both Uber and Lyft entered Austin in 2014, the actual number of TNC trips in early May 2015 was likely much larger than Uber's 2014-2015 average of 50,000 per week.

FIGURE 3—DIFFERENCE IN DIFFERENCES RESULTS



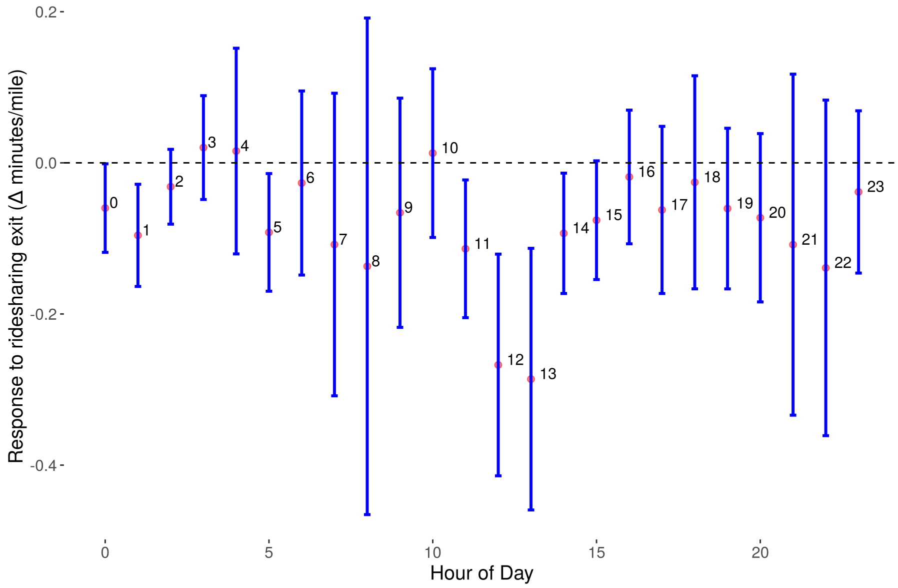
Notes: Results from equation 1, a difference in differences comparing pre vs. post May 9$^{\text{th}}$ traffic speeds in 2015 (where both Uber and Lyft operated in Austin) to pre vs. post May 9$^{\text{th}}$ traffic speeds in 2016 (where both TNCs exited Austin). Points represent the estimated effect of TNC departure on traffic speeds (in minutes per mile) by hour of day. Controls include day of week, holiday, and segment fixed effects, segment-specific linear trends in days since May 9$^{\text{th}}$, and flexible controls for temperature and precipitation. Bars reflect 95% confidence intervals from twoway standard errors clustered by segment-week. Traffic speed data were accessed through the City of Austin's Open Data Portal.

Figure 4—Event Study



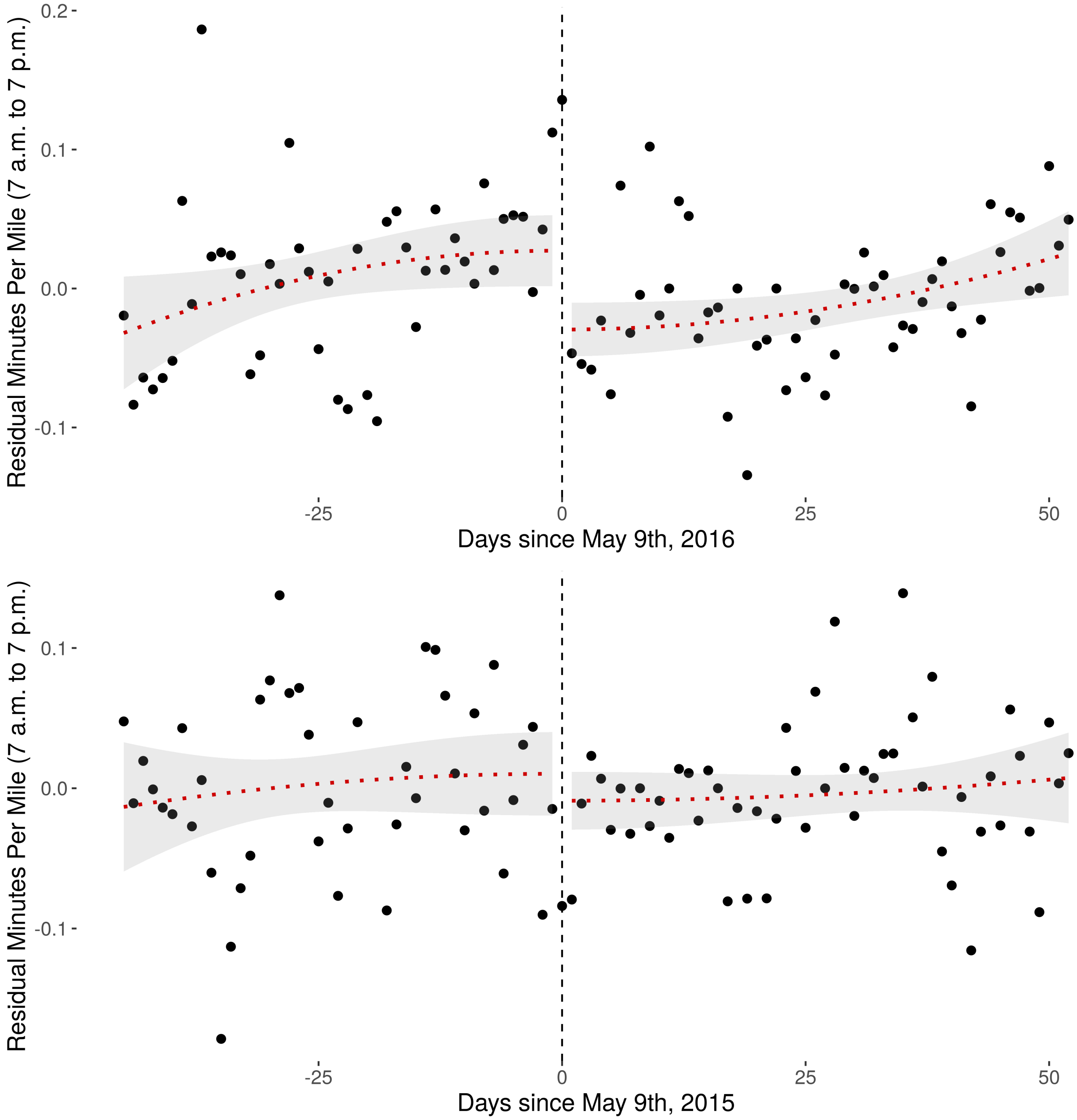
Notes: This Figure shows raw average speed (minutes per mile) between 7 a.m. and 7 p.m. over 79 road segments in Austin, TX, plotted by week of year for 2015 and 2016. Data were accessed through the City of Austin's Open Data Portal. The dotted line represents the week of May 9th, where Uber and Lyft ceased operation in Austin in 2016. Note that week zero is partially treated, as May 9th, 2016 was a Monday.

Figure 5—Regression Discontinuity Results



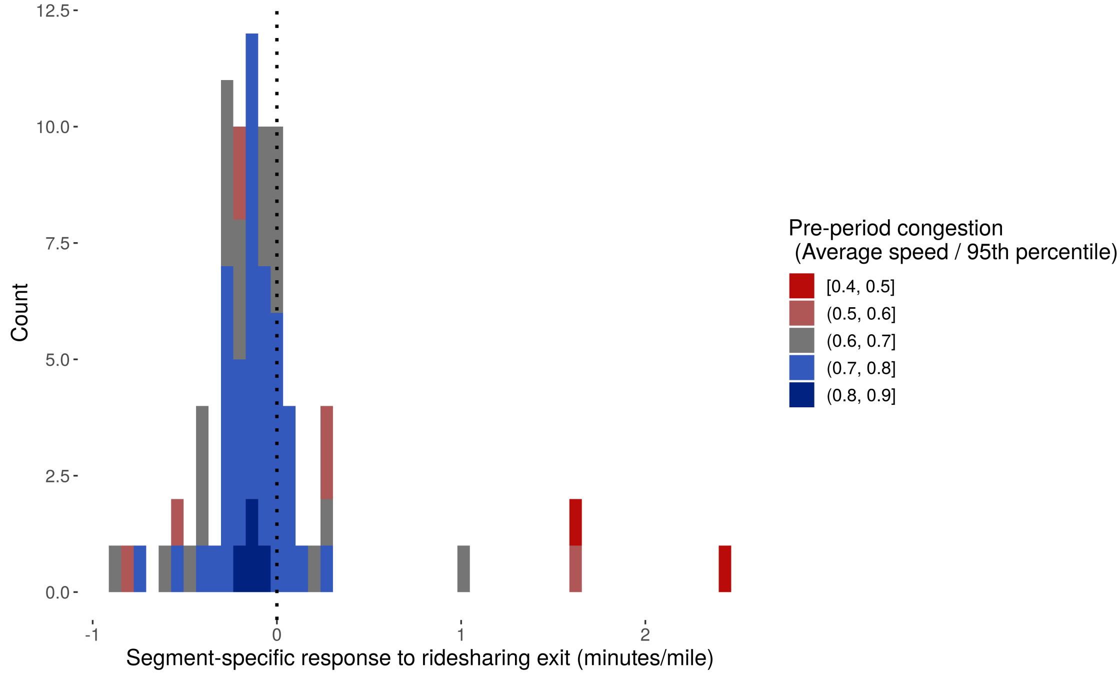
Notes: Results from equation 3, a regression discontinuity performed on traffic speeds across 79 road segments in Austin, TX. The bandwidth is March 20th - August 1st of 2016, which (asymetrically) spans the May 9th departure of Uber and Lyft. Points represent the estimated effect of TNC exit on traffic speeds by hour of day. A negative point indicates an estimated increase in traffic speed. Controls include day of week, holiday, and segment fixed effects, segment-specific second degree polynomials in days since May 9th, and flexible controls for temperature and precipitation. Bars reflect 95% confidence intervals from twoway standard errors clustered by segment-week. Traffic speed data were accessed through the City of Austin's Open Data Portal.

Figure 6—Regression Discontinuity Residual Plots



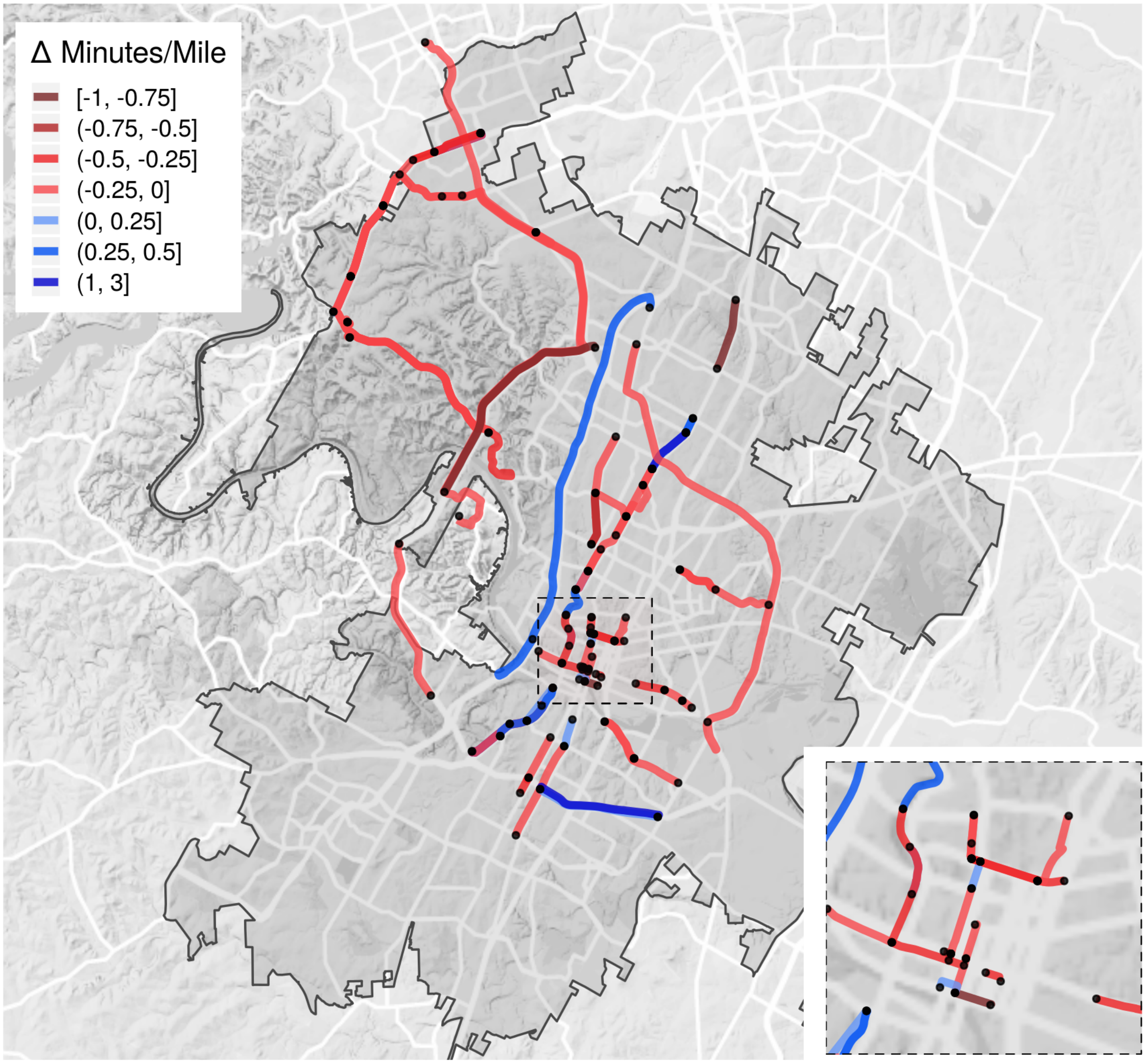
Notes: This Figure plots the daily mean residuals from a pooled version of equation 3, omitting the $\delta_y\eta_t$ (year*post) indicator. The dependent variable is minutes per mile, measured over 79 road segments in Austin, TX. The bandwidth is March 20th - August 1st of 2016 (or 2015), which (asymetrically) spans the May 9th departure of Uber and Lyft. Controls include hour of day, day of week, holiday, and segment fixed effects, segment-specific second degree polynomials in days since May 9th, and flexible controls for precipitation and temperature. The dotted line represents a second degree polynomial in days since May 9th, the shaded region is the 95% confidence interval. Traffic speed data were accessed through the City of Austin's Open Data Portal.

Figure 7—Distribution of Segment-Sepcific Responses



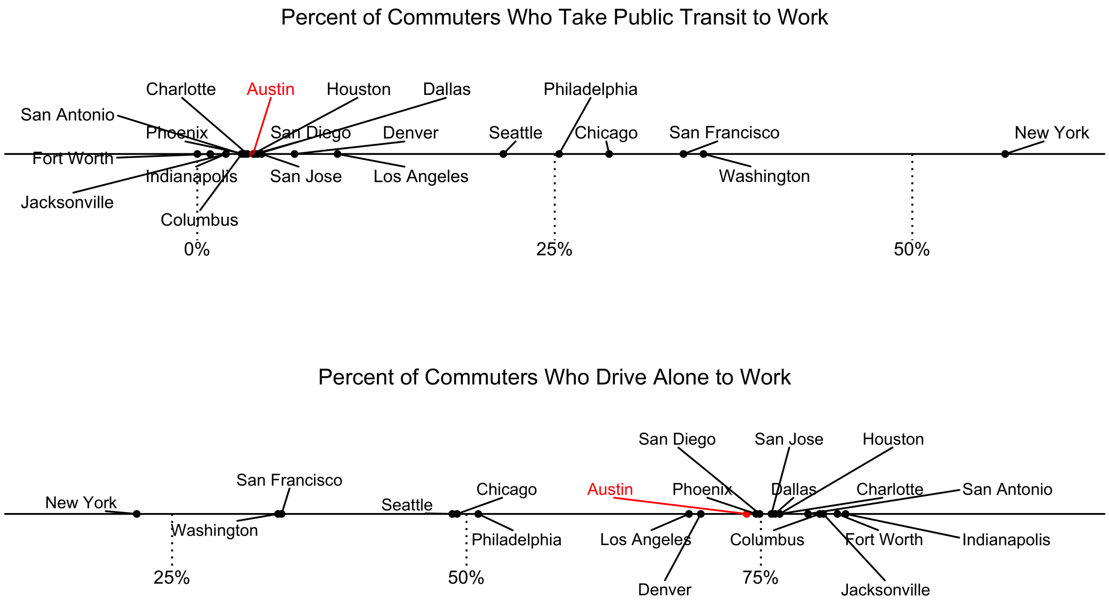
Notes: Results from equation 2, a difference in differences comparing pre vs. post May 9th traffic speeds in 2015 (where both Uber and Lyft operated in Austin) to pre vs. post May 9th traffic speeds in 2016 (where both Uber and Lyft exited Austin), allowing for segment-specific congestion responses. Bars represent the number of segments with idiosyncratic changes in traffic speeds falling withing a given bin. Cells are colored by the pre May 9th 2016 congestion level, as measured by the ratio of average speed to the 95th percentile of speed. Traffic speed data were accessed through the City of Austin's Open Data Portal.

Figure 8—Segment-Sepcific Responses



Notes: Results from equation 2, a difference in differences comparing pre vs. post May 9th traffic speeds in 2015 (where both Uber and Lyft operated in Austin) to pre vs. post May 9th traffic speeds in 2016 (where both Uber and Lyft exited Austin), allowing for segment-specific congestion responses. Paths represent Google Maps recommended driving directions between endpoints of a given segment, colored by the sign and magnitude of the estimated segment-specific change in traffic speed. The black line is the Austin city limit. Traffic speed data were accessed through the City of Austin's OpenData Portal.

Figure 9—External Validity



Notes: This figure depicts vehicle and public transit use across the 20 largest US metros for the year 2017. Data for both subfigures come from the U.S. Census Bureau's 2017 American Community Survey. Similarities in commuting behavior between Austin and other "Sun Belt" cities suggests that the findings in this paper may be most applicable to this group of metros.

9 Tables

Table 1—Summary Statistics

	mean	sd	min	max
Average Speed (mph)	24.91	9.51	3.50	95.04
Minutes Per Mile	2.94	1.71	0.63	17.13
Segment Length	0.73	0.57	0.06	3.80
Number samples	4.77	3.77	1.00	45.00

Summary statistics for traffic data along 79 road segments in Austin, TX. Speed data reflect the average travel time for Bluetooth devices that move from origin sensor to destination sensor during a given 15-minute interval. As described in section 4, data are also filtered for outliers. Traffic speed data were accessed through the City of Austin's OpenData Portal.

Table 2—Difference in Differences Results

Hour of Day	β_h	se	p
0	**-0.0860**	0.0439	0.0498
1	**-0.0747**	0.0308	0.0153
2	**-0.0690**	0.0355	0.0518
3	-0.0364	0.0591	0.5375
4	0.0474	0.0997	0.6345
5	-0.0496	0.0676	0.4634
6	-0.0738	0.0881	0.4024
7	**-0.1759**	0.0996	0.0773
8	-0.1424	0.1343	0.2891
9	-0.0946	0.0906	0.2965
10	-0.0168	0.0543	0.7573
11	**-0.1676**	0.0590	0.0045
12	**-0.1940**	0.0766	0.0113
13	**-0.1828**	0.0812	0.0243
14	-0.0420	0.0499	0.3999
15	-0.0476	0.0431	0.2689
16	0.0252	0.0568	0.6573
17	-0.0257	0.0631	0.6838
18	0.0257	0.0851	0.7624
19	-0.0596	0.0571	0.2968
20	-0.0333	0.1025	0.7451
21	0.0466	0.0738	0.5278
22	-0.1037	0.0878	0.2376
23	-0.0403	0.0307	0.1889
F-test			0.0000
N			966,301

Notes: Results from equation 1, a difference in differences comparing pre vs. post May 9$^{\text{th}}$ traffic speeds in 2015 to pre vs. post May 9$^{\text{th}}$ traffic speeds in 2016 (where both Uber and Lyft exited Austin). Controls include segment-specific linear in day trends, a precipitation dummy, day of week fixed effects, and year and post May 9$^{\text{th}}$ dummies. Standard errors are clustered by segment-week. β_h represent the estimated effect of TNC departure on traffic speeds (in minutes per mile) by hour of day. Bold coefficients are significant at the 10% level. The final row reports the p-value from a joint hypothesis test of $\beta_h = 0\ \forall h$.

Table 3—Regression Discontinuity Results

Hour of Day	β_h	se	p
0	**-0.0599**	0.0299	0.0452
1	**-0.0959**	0.0345	0.0054
2	-0.0316	0.0253	0.2107
3	0.0202	0.0351	0.5646
4	0.0156	0.0694	0.8222
5	**-0.0920**	0.0397	0.0205
6	-0.0267	0.0621	0.6677
7	-0.1081	0.1022	0.2900
8	-0.1369	0.1676	0.4141
9	-0.0660	0.0774	0.3934
10	0.0129	0.0569	0.8213
11	**-0.1138**	0.0465	0.0144
12	**-0.2675**	0.0748	0.0004
13	**-0.2862**	0.0883	0.0012
14	**-0.0933**	0.0406	0.0217
15	**-0.0759**	0.0401	0.0583
16	-0.0187	0.0451	0.6790
17	-0.0623	0.0564	0.2696
18	-0.0258	0.0719	0.7202
19	-0.0605	0.0542	0.2646
20	-0.0727	0.0568	0.2010
21	-0.1083	0.1151	0.3470
22	-0.1390	0.1133	0.2199
23	-0.0385	0.0548	0.4823
F-test			0.0000
N			501,010

Notes: Results from equation 3, a regression discontinuity performed on traffic speeds across 79 road segments in Austin, TX. The bandwidth is March 20th - August 1st of 2016, which (asymetrically) spans the May 9th departure of Uber and Lyft. Controls include hour of day, day of week, holiday, and segment fixed effects, segment-specific second degree polynomials in days since May 9th, and flexible controls for temperature and precipitation. Standard errors are clustered by segment-week. β_h represent the estimated effect of TNC departure on traffic speeds (in minutes per mile) by hour of day. Bold coefficients are significant at the 10% level. The final row reports the p-value from a joint hypothesis test of $\beta_h = 0 \ \forall h$.

TABLE 4—POOLED ESTIMATES

	β (Δ minutes/mile)	*se*	*p*	implied annual cost ($)
Difference in Differences (All hours)	-0.0559	0.0252	0.0267	-54,936,372
Difference in Differences (7 a.m. - 7 p.m.)	-0.0887	0.0327	0.0067	-64,213,508
Regression Discontinuity (All Hours)	-0.1015	0.0354	0.0041	-99,760,456
Regression Discontinuity (7 a.m. - 7 p.m.)	-0.1335	0.0433	0.0021	-96,579,551

Notes: The first two rows display results from a variation of equation 1, a difference in differences specification that estimates the pooled impact of TNC exit on traffic speeds across hours of day. Controls include segment-specific linear in day trends, controls for precipitation, day of week fixed effects, hour of day fixed effects, and year and post-May 9$^{\text{th}}$ dummies. Standard errors are clustered by segment-week. Row 1 shows the results of this regression using speed data on all hours, and column 2 shows results restricted to 7 a.m. to 7 p.m. β represent the estimated effect of TNC departure on traffic speeds, measured in minutes per mile. The final column displays annual costs implied by multiplying β by annual Ausin-area VMT, and then by \$11.91, which is an inflation-adjusted figure from Small and Verhoef (2007). Traffic data were accessed through the City of Austin's OpenData Portal. Rows 3 and 4 repeat this exercise for equation 3.

TABLE 5—AUSTIN METRO VALIDITY

	Not Sampled	Sampled	p
Annual Delay per Mile (person-hours)	118,372.98	101,368.11	0.58
Texas Congestion Index	1.36	1.39	0.61
Peak Period Average Speed	33.22	29.52	0.16
Freeflow Speed	41.20	37.97	0.28
Average Daily VMT	204,488.84	166,805.50	0.46
Peak Period Annual Hours of Delay (person-hours)	377,230.86	241,909.86	0.34

Notes: This Table uses data maintained by the Texas Department of Transportation (TXDoT) to compare observable characteristics of Austin-area roads that do not appear in my sample (column 1) to those that do (column 2). Column (3) reports p-value of the corresponding t-test for a difference in means. The data cover 86 road segments in Austin, 35 of which overlap with the 79 Bluetooth segments used in the above analysis. Note that roads sections are coded as sampled even if the Bluetooth segment does not cover the entire corresponding TXDoT road segment.

Table 6—Congestion Cost Estimates

	daily cost ($)	*se*	*p*	annual cost ($)
MoPac WTP	-169,231	43,069	0.0000	-61,769,392
Small and Verhoef (2007) WTP	-191,576	31,319	0.0000	-69,925,327

Notes: Estimates of the travel-time congestion costs of TNC operation in Austin, TX. The first row displays the result of the exercise described in equation 4, which matches hour-specific changes in travel time to hour-specific willingess to pay estimates (detailed in Appendix A) and hour-specific traffic volume measurements. The second row applies a uniform value of travel time from Small and Verhoef (2007) across all hours. This figure was $9.69 in 2005 dollars, which corresponds to $11.91 in 2016 dollars. Standard errors for cost figures are calculated as described in section 6.

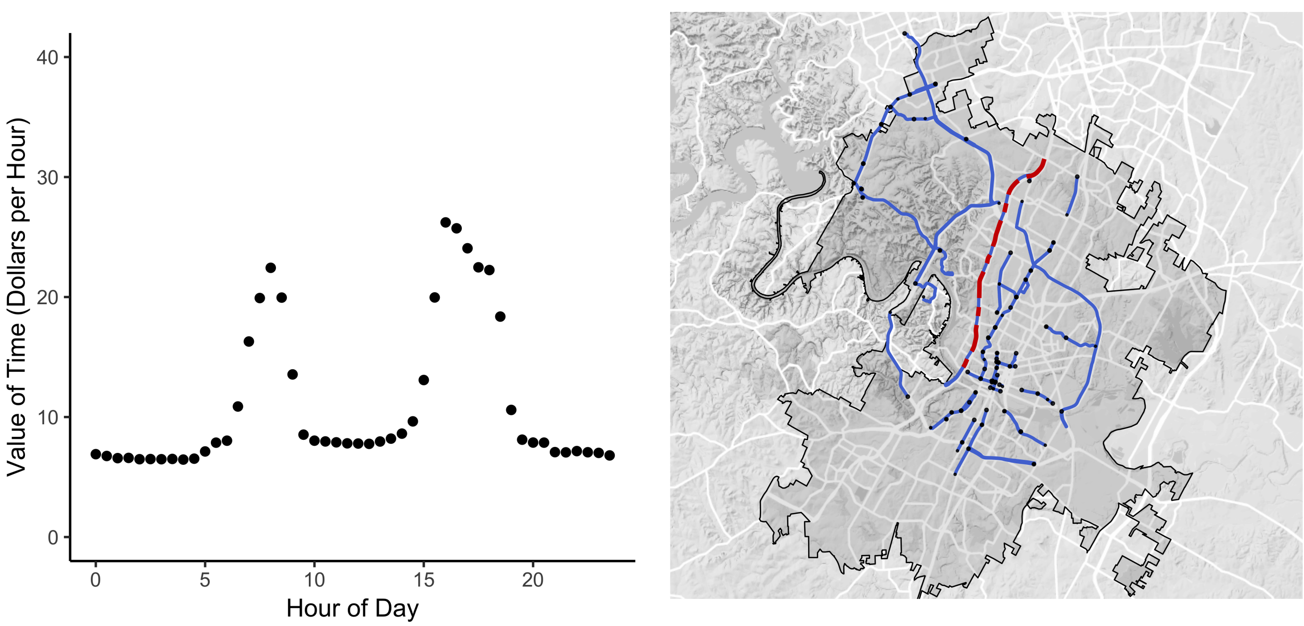
A Revealed Preference WTP Estimates

The MoPac (Texas State Highway 1) is a north-south route in Austin. Starting in November 2017, the Central Texas Regional Mobility Authority opened an 11-mile variable-price express lane on the MoPac (see Figure A1). The price of using this lane adjusts in order to keep the express lane moving at free-flow speeds: tolls increase when the express lane is busy and decrease when it is underused. Toll rates are posted at the northbound and sounthbound entrances.

Using 30-minute resolution data provided by the Central Texas Regional Mobility Authority on MoPac prices and average travel times on the tolled and non-tolled lanes, I recover time-varying estimates of the willingness to pay for travel time reductions. As commuters see posted prices but not traffic coditions, I produce implied willingness to pay estimates by dividing the observed toll price on a given date and time by the *expected* travel time savings for that time of day. To calculate the expected time savings, I take the average time difference between tolled and non-tolled lanes by half hour of day. For example, if the average difference between tolled and non-tolled lanes is 4 minutes between 9:00 a.m. and 9:30 p.m., and I observe an average price between 9:00 and 9:30 on a given day of 1 dollar, then the implied value of travel time for that half-hour block on that day is $15 per hour. I then aggregate these observations across days to recover an average WTP for travel time reductions for each half hour of day.

These estimates are summarized in Figure A1, and are broadly consistent with estimates of value of travel time estimated in related settings (Small and Verhoef, 2007). Importantly, however, WTP peaks during morning and evening rush hour periods, possibly reflecting different commuters, or heterogeneity in value of time based on trip purpose. Note that the motivation for using MoPac data is to study the *convolution* between willingness to pay for travel time reductions and estimated congestion impacts, not to produce novel estimates of the value of travel time.

Figure A1—Willingness to Pay for Travel Time Reductions



Left Pane: Willingness to pay for travel time reductions in Austin, TX. Constructed using 2017 data from the Mopac variable-toll lane (Highway Loop 1), provided by the Central Texas Regional Mobility Authority. Estimates reflect means of observed equilibrium prices divided by expected time savings by hour of day.

Right Pane: Mopac expressway (red dashed) plotted with the 79 road segments used in estimation of the changes in travel speeds (blue). Nodes represent Bluetooth sensors, and the black line is the Austin City limit.

B Threats to identification from other modes of transportation

In addition to Bluetooth sensors, the city of Austin maintains pneumatic sensors which take periodic measurements of traffic speeds. Pneumatic sensors are stretched across traffic lanes, and therefore will not be influenced by pedestrian activities. Additionally, pneumatic sensors classify observations by axel length, meaning activity from bicycles will not be reflected in vehicle speed measurments. I identify 39 instances where Bluetooth road segments overlie pneumatic sensors (see Figure B1), and test whether the relationship between the two speed measures changes significantly after the Proposition 1 vote.

For other modes of transportation to bias my estimates, speed must be mismeasured on Bluetooth segments, and that mismeasurment must be correlated with the treatment. To test for this bias, I perform the following regression:

$$y_{i,t} = \alpha + \beta_1 s_{i,t} + \beta_2 D_t \cdot s_{i,t} + \gamma_1 \phi_t + \gamma_2 \boldsymbol{\theta}_i + \epsilon_{i,t} \tag{6}$$

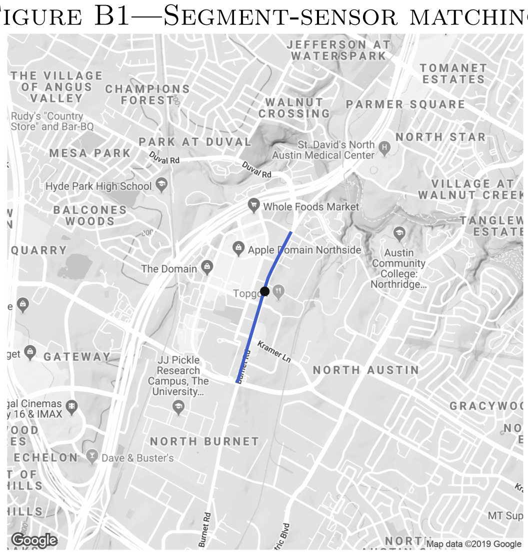
Where $y_{i,t}$ is the Bluetooth speed measurment on segment i and time t, and $s_{i,t}$ is the pneumatic road segment speed measurment on segment i and time t. D_t is a treatment dummy, which equals one for days after May 9$^{\text{th}}$, 2016. ϕ_t is a set of month of year fixed effects, and θ_i is a set of segment fixed effects. I test the null hypothesis $\beta_2 = 0$, and interpret a significant result as evidence of bias in the Bluetooth speed masurements. Results from equation 6 are displayed in Table B1.

Table B1—Tests for Bias

	point estimate	*se*	*p*
β_1	0.0366	0.0221	0.0979
β_2	-0.0094	0.0222	0.6727

Notes: Results from equation 6, which tests whether the relationship between pneumatic speed measurements ($s_{i,t}$) and Bluetooth speed measurements ($y_{i,t}$) changes after the exit of TNCs in Austin. In addition to the variables listed, this regression includes segment and month of year fixed effects.

Figure B1—Segment-sensor matching



Notes: An example of a Bluetooth segment (blue path) matched to a pneumatic sensor (black dot). Segments were matched to sensors by both location and direction of travel

C Threats to identification from TNC driving speeds

If TNC vehicles drive significantly slower or faster than the average non-TNC vehicle in a way that remains after filtering, the above empirical strategy will be biased. To test for this potential bias, I match RideAustin trips to segments, allowing me to test the null hypothesis that TNC vehicles drive at the same speeds as the average mix of vehicles.

I match TNC trips to segments based on the following criteria: for a given segment, the sum of the distance between segment and TNC trip termini must be less than 500 meters, and the distance traveled by the TNC vehicle must be within 10% of the segment length. I identify 1901 such matches, with several segments recording multiple TNC trips the fit this criteria. I then replicate the type of data filtering applied by Post Oak Traffic Systems. Recall that in the data I use for my analysis, only observations that fall within 75% of the interquartile range (IQR) of the 15 most recent observations are used to calculate average speeds. While I do not have access to the IQR data, I do have the standard deviation of speed measurements for any given 15-minute interval. I use this to estimate the IQR, and drop RideAustin trips that fall outside of IQR estimate for the corresponding segment, time, and date, as these trips likely be dropped from the Bluetooth speed measurements.

After applying these filters, I am left with 221 trip matches, which are summarized in Table D1. The regression coefficient reflects a difference in means between TNC trip speeds and the segment speeds recorded at corresponding times. On average, TNC vehicles traversed segments 0.03 minutes per mile slower than did the average recorded vehicle. This difference is not statistically significant, and under reasonable assuptions about TNCs as a share of total vehicles (5-15%), should not generate meaningful bias in the results reported above.

Table C1—TNC vehicle speeds

Δ Minutes per Mile	*se*	*p*
-0.0324	0.1643	0.8438

Notes: A comparison of means between Bluetooth-recorded travel speeds and TNC vehicle speeds. The coefficient *Δ Minutes per Mile* represents the difference in means between travel speeds (in minuts per mile) for these TNC trips and the average travel times recorded by the corresponding segment over the 15-minute period where the TNC trip occurred.

D Robustness

In this section I investigate the robustness of my results to alternative specifications and alternate data slices. Table D1 shows the results of difference in difference regressions using alternate linear trend specifications. Table D2 shows the results of running equation 1 on different sub and supersets of the Bluetooth data used in the main analysis. My results are stable over these specifications: the F-test rejects the null that the hour-specific effects are jointly zero.

Figure D1 plots estimated regression discontinuity and difference in differences coefficients pooling over hours of day for symetric bandwidths ranging from 20 to 70 days about May 9$^{\text{th}}$. The regression discontinuity results are stable over this range. The difference in differences results show positive, but not statistically significant point estimates for a small minority of bandwidths. Across all bandwidths, for both RD and DD specifications, point estimates of congestion impacts between 7 a.m. and 7 p.m. are negative. Taken together, the results presented in Figure D1 are consistent with the conclusions in the body of this paper: daytime travel speeds in Austin likely increased following the exit of Uber and Lyft.

Table D1—Alternate specifications

	Model 1		Model 2		Model 3	
Hour of Day	β_h	p	β_h	p	β_h	p
0	**-0.0860**	0.05	**-0.0839**	0.06	-0.0838	0.12
1	**-0.0747**	0.02	**-0.0723**	0.03	**-0.0821**	0.07
2	**-0.0690**	0.05	**-0.0633**	0.09	-0.0962	0.10
3	-0.0364	0.54	-0.0350	0.57	-0.1013	0.11
4	0.0474	0.63	0.0468	0.64	-0.0892	0.27
5	-0.0496	0.46	-0.0488	0.51	**-0.1923**	0.04
6	-0.0738	0.40	-0.0879	0.29	**-0.2677**	0.01
7	**-0.1759**	0.08	**-0.1895**	0.05	**-0.3600**	0.04
8	-0.1424	0.29	-0.1823	0.26	-0.1892	0.20
9	-0.0946	0.30	-0.1239	0.22	-0.1629	0.19
10	-0.0168	0.76	-0.0296	0.63	-0.1303	0.16
11	**-0.1676**	0.00	**-0.1663**	0.01	-0.0678	0.22
12	**-0.1940**	0.01	**-0.1918**	0.01	**-0.1845**	0.02
13	**-0.1828**	0.02	**-0.1829**	0.02	**-0.1149**	0.05
14	-0.0420	0.40	-0.0418	0.38	-0.0411	0.33
15	-0.0476	0.27	-0.0481	0.26	-0.0299	0.49
16	0.0252	0.66	0.0224	0.68	0.0419	0.37
17	-0.0257	0.68	-0.0254	0.67	0.0154	0.75
18	0.0257	0.76	0.0265	0.75	-0.0175	0.73
19	-0.0596	0.30	-0.0538	0.34	-0.0310	0.56
20	-0.0333	0.75	-0.0352	0.73	-0.0277	0.65
21	0.0466	0.53	0.0468	0.52	-0.0097	0.89
22	-0.1037	0.24	-0.1055	0.22	-0.0654	0.39
23	-0.0403	0.19	-0.0429	0.19	**-0.1539**	0.03
F-test		0.00		0.00		0.00

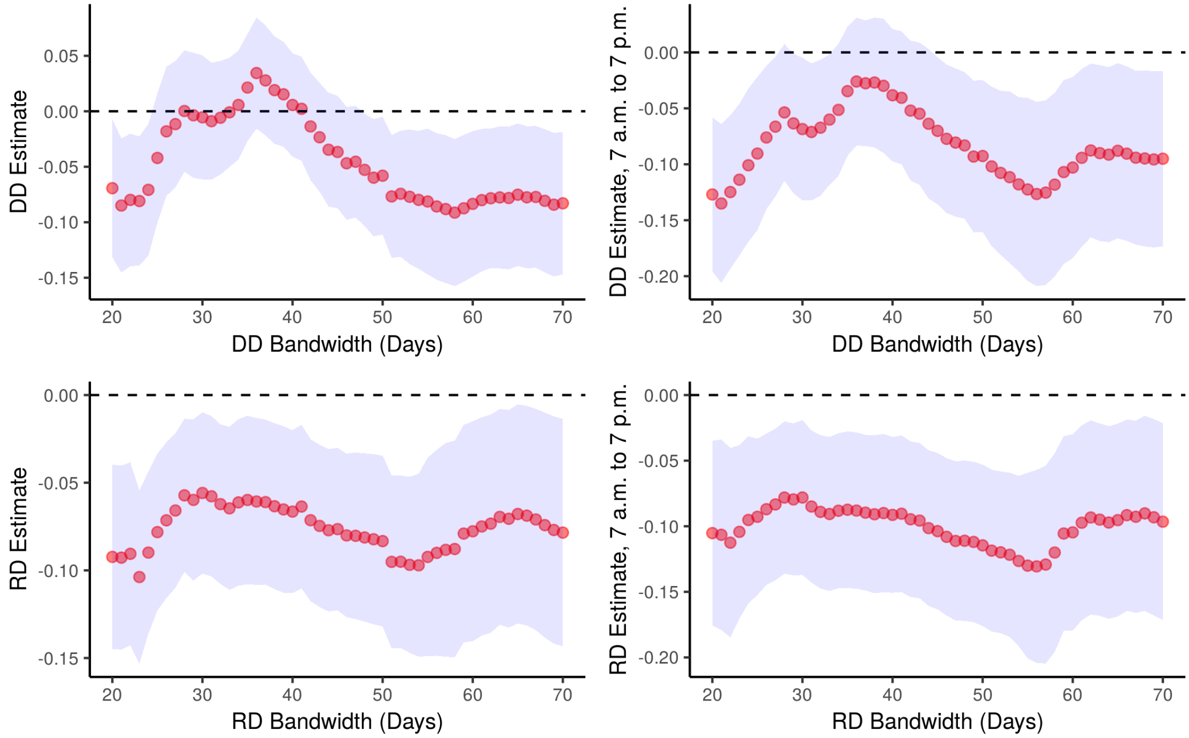
Notes: Results from equation 1. Model 1 reproduces the results from my preferred specification, with year-segment specific linear trends. Model 2 includes only year-specific linear trends (i.e, pools across segments), and model 3 includes only segment-specific linear trends (i.e., pools across years). Bold coefficients are significant at the 10% level. The final row reports the p-value from a joint hypothesis test of $\beta_h = 0 \; \forall h$.

Table D2—Alternate segment groups

	Group 1		Group 2		Group 3	
Hour of Day	β_h	p	β_h	p	β_h	p
0	**-0.0860**	0.05	-0.0561	0.17	-0.1731	0.36
1	**-0.0747**	0.02	**-0.0644**	0.01	**-0.3219**	0.01
2	**-0.0690**	0.05	**-0.0551**	0.08	0.0428	0.83
3	-0.0364	0.54	-0.0306	0.39	-0.1277	0.21
4	0.0474	0.63	0.0228	0.71	-0.1343	0.23
5	-0.0496	0.46	-0.0264	0.67	-0.1479	0.33
6	-0.0738	0.40	-0.0268	0.69	0.1327	0.47
7	**-0.1759**	0.08	-0.1121	0.17	-0.4757	0.15
8	-0.1424	0.29	-0.1533	0.17	-0.1348	0.59
9	-0.0946	0.30	-0.0894	0.17	-0.0542	0.84
10	-0.0168	0.76	-0.0479	0.26	0.0973	0.23
11	**-0.1676**	0.00	**-0.1600**	0.01	**-0.3853**	0.06
12	**-0.1940**	0.01	-0.1386	0.12	**-0.4112**	0.06
13	**-0.1828**	0.02	-0.1145	0.14	-0.2931	0.18
14	-0.0420	0.40	-0.0138	0.76	0.1086	0.50
15	-0.0476	0.27	-0.0377	0.29	0.1079	0.54
16	0.0252	0.66	0.0155	0.77	0.3188	0.18
17	-0.0257	0.68	-0.0496	0.42	0.2582	0.18
18	0.0257	0.76	-0.0393	0.58	0.2257	0.21
19	-0.0596	0.30	-0.0733	0.11	0.1238	0.58
20	-0.0333	0.75	-0.0206	0.83	0.3183	0.18
21	0.0466	0.53	0.0804	0.21	0.3653	0.29
22	-0.1037	0.24	-0.0702	0.44	0.1690	0.28
23	-0.0403	0.19	-0.0406	0.19	**-0.2256**	0.01
F-test		0.00		0.00		0.00

Notes: Results from equation 1, applied different groups of road segments. Group 1 is my preserred specification, which uses all traffic segments which report in more than 70 percent of days in each year. Group 2 relaxes this level to segments that report in 30 percent of days. Group 3 uses only segments that report in every day of the study period. Bold coefficients are significant at the 10% level. The final row reports the p-value from a joint hypothesis test of $\beta_h = 0 \ \forall h$.

FIGURE D1—BANDWIDTH SENSITIVITY



Notes: This Figure plots estimated difference in differences and regression discontinuity coefficients pooling over hours of day (a variation of equation 3) for symetric bandwidths ranging from 20 to 70 days about May 9$^{\text{th}}$, 2016. Bars represent the 95% confidence interval using standard errors clustered by segment-week. Specifications with a bandwidth of over 45 days include a dummy for the SXSW festival. For reference, my preferred specification uses an asymmetric bandwidth of 45 pre-period days and 52 post-period days.