

Do Transportation Network Companies Cause Congestion? Evidence from Austin, TX

Matthew Tarduno



Department of Agricultural and Resource Economics
University of California, Berkeley
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Abstract

I study the impact of transportation network companies on traffic delays using a natural experiment created by the abrupt departure of Uber and Lyft from Austin, Texas. Using high-frequency traffic data and a difference in differences specification, I estimate that Uber and Lyft are together responsible for traffic slowdowns that Austinites would be willing to pay \$49 million annually to avoid. This figure is 1.8% of total 2017 Austin-area travel time costs, and is of the same order of magnitude as back of the envelope estimates of annual consumer surplus associated with the operation of transportation network companies in Austin.

1 Introduction

Transportation network companies (TNC) like Uber and Lyft have revolutionized urban transportation in the last decade. These services offer substantial benefits to residents—increased mobility, reduced drunk driving, flexible work—but have expanded during a time when cities are grappling with traffic congestion. Due to the relative infancy of these companies, uncertainty remains with respect to the realtive costs and benefits associated with their operation. This uncertainty is reflected in heterogeneous legislation: New York City imposed a cap on ridesharing vehicles in August of 2018, citing congestion concerns ([New York Times, 2018](#)). Florida and Texas, on the other hand, both passed measures in 2017 that prohibited local regulation of TNCs, each listing job creation and consumer benefits as justification ([The Texas Tribune, 2017](#)). Outside of the US, legislators in Vancouver are considering allowing TNCs to operate within their city ([BCBusiness, 2018](#)), while London is considering revoking Uber's license ([BBC, 2018](#)).

Back of the envelope calculations suggest congestion externalities resulting from TNC activity could be substantial. A 2017 Inrix report pegged the total cost of congestion to US drivers at \$305 billion dollars ([Inrix, 2018](#))—roughly 2 orders of magnitude larger than estimates of national consumer surplus from Uber ([Cohen et al., 2016](#)). This comparison suggests that if TNCs are responsible for even a modest fraction of traffic congestion, the negative externalities associated with lengthening travel times could outweigh consumer surplus benefits. As of 2018, however, there are few credible causal estimates of the welfare implications of TNC operation that city governments can use to inform policy decisions.

In this paper I exploit plausibly exogenous variation in TNC activity in Austin, TX to answer the following questions: Do transportation network companies impact traffic congestion? And if so, what are the magnitudes of the implied time-related welfare losses or gains? Using a difference in differences design, I find that Uber and Lyft are together responsible for traffic slowdowns that Austinites would be willing to pay \$49 million annually to avoid. This figure is 1.8% of total 2017 Austin-area travel time costs, is roughly the size of estimates of the consumer surplus associated with TNC operation in Austin.

These results have two implications for policymakers. First, TNC activity can be viewed roughly as a transfer: 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 a poor tool to address congestion-related externalities.

The rest of the paper is organized as follows. Section 2 describes related literature. Section 3 provides background on the events that precipitated the departure of Uber and Lyft from Austin. Section 4 outlines the 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 Literature Review

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](#)). While the traditional policy recommendation for traffic is a congestion charge ([Vickrey, 1969](#)), for several reasons cities may consider TNC regulation as an alternative.

First, congestion pricing may be politically infeasible, especially in the United States, where there are examples of toll roads and variable pricing, but currently no cordon charge zones. Second, even if there existed the political will to implement congestion charges, a recent and growing literature suggests that feasible congestion prices may perform poorly in terms of reducing deadweight loss. [Kreindler \(2018\)](#), for example, demonstrates that for drivers in Bangladesh, the time externality saved by encouraging commuters to avoid peak hours is not much larger than the cost of temporal relocation. Relatedly, [Prud'homme and Bocarejo \(2005\)](#) conclude that the time savings from London's congestion charge are outweighed by the administrative costs of operating the system. [Knittel and Sandler \(2018\)](#) provide potential rationale for these observations: because individual contributions to congestion are right-skewed, second best optimal uniform taxes (e.g., cordon charges) still leave significant portions of the initial deadweight loss remaining. This poor targeting means that providing pareto improvements can prove difficult in practice.

Even if we remove pricing from the set of congestion policy instruments, however, it is not clear that TNCs should be considered as objects for regulation. As a number of other observers have pointed out, the impact of TNCs on traffic congestion is theoretically ambiguous. While survey data suggests

TNCs may induce trips ([Rayle et al., 2014](#)) and therefore worsen congestion, [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, which could attenuate or outweigh the impact 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 transportation will result in more or fewer total vehicle trips.

In the absence of peer-reviewed studies of the local impacts of TNC operation, some cities have commissioned their own TNC studies. Most notably, a report by the San Francisco County Transportation Authority concludes that 50% of the 3.1 mph decrease in average San Francisco speeds since 2010 can be attributed to TNC activity ([San Francisco Transit Authority, 2018](#)). While the data used in this report has a high level of spatial granularity, they contain only observations from 2010 and 2016, meaning this estimate may be biased by other non-TNC changes that occurred in the city between these years.

To date, the most complete econometric evaluation the impact of TNCs on congestion is [Li et al. \(2018\)](#). The trio use 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. For several reasons, however, Uber entry may not provide clean variation for econometric exercises. As with the San Francisco Study, there may be other city-specific unobservables that both correlate with Uber entry and change traffic patterns (e.g., demographic changes or delivery of online purchases). Relatedly, Uber may have selected cities based on pre-period trends in characteristics that influence traffic congestion, like car ownership rates.

In this paper, I contribute to the existing literature in three ways: First, I can plausibly interpret my estimates of congestion responses to TNC operation as causal, given the circumstances that precipitated the exit of Uber and Lyft. Second, previous analyses of TNC impacts on congestion often use city-level data, and the bulk of economic studies on congestion rely on highway sensors. The unique street-level dataset I exploit allows for a more nuanced analysis of the spatial and temporal heterogeneity in congestion impacts. Lastly, using value of time estimates from a variable-toll freeway in Austin, I am able to translate changes in traffic speeds to policy-relevant welfare costs.

3 Background and Natural Experiment

Austin is the 11th largest city in the United States, and suffers from considerable congestion: According to Inrix, Austin ranks 14th nationally and 72nd globally in measures of overall congestion. 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. Proposition 1, sponsored by Uber and Lyft, attempted to overturn this ordinance. The Proposition was defeated on May 7th, 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 1 2016), Tride Technologies (June 15 2015), and RideAustin (June 16 2016). Wingz, which provides rides to and from the airport, also started operating in Austin in May 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%), 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 negligible (see section [5.1](#)).

4 Data Description

To study traffic speeds, 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.

To avoid the problem of route choice through the city, 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. 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. To further address data quality concerns, I restrict my sample to consistently-reporting sensors. Of the 430 total segments, I drop segments that fail to report for more 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. On average, a segment observes 4.77 devices move from origin to destination during each 15 minute period. 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 welfare calculations 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 5.3.

The structure of these data also present several challenges to inference. In addition to segment-specific characteristics suggesting a departure from homoskedasticity, sensor-specific speed innovations

(due to accidents, for example) are likely highly correlated over space and time. As such, I calculate standard errors robust to general forms of spatial and temporal heteroskedasticity, as per [Conley \(1999\)](#).

Outcome variables (minutes per mile), may be mismeasured if road segments and/or travel times are rounded. This error is likely classical, and motivates the temporal aggregation I employ during estimation. To ameliorate weather-related noise, I use precipitation data accessed through the National Oceanographic and Atmospheric Administration's National Centers for Environmental Information. Lastly, RideAustin's trip-level data—which I use in Robustness checks and to determine the study window—are publicly available through the company's DataWorld account.

5 Empirical Strategy

5.1 Timeframe

My data range from February 2015 to March 2017. I truncate this window to isolate periods where the variation in traffic congestion can be credibly attributed to the failure of Proposition 1. As described in section 3, a number of ridesharing firms entered the market following Uber and Lyft's exit. 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 ridesharing company that enjoyed the largest market share during Uber and Lyft's absence—I truncate my estimation period on August 1st, 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 April 1st to August 1st for both 2015 and 2016. The 2016 study period is plotted with TNC data in Figure 7.

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 9th in 2016 (where Uber and Lyft exit) 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 (or equivalently, interacting each term below in the regression below with an hour-of-day dummy):

$$mpm_{i,t} = \alpha + \beta D_t * year_t + \gamma_1 D_t * seg_i + \gamma_2 year_t * seg_i + \gamma_4 year_t * linear_day_t * seg_i + \Gamma X_t + \epsilon_{i,t} \quad (1)$$

Where D_t is a dummy that equals one for days (in any year) after May 9th. $year_t$ is a dummy that equals one for the year 2016, and seg_i is a factor variable for each road segment. $linear_day_t$ is the signed number of days between a given date and May 9th of the year, and X is a vector that includes a precipitation dummy variable and a set of day of week fixed effects. The identifying assumption in the estimation of β_h —the effect of Uber and Lyft operation on travel speeds in 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 9th for reasons other than Uber and Lyft's operation.

I calculate hour-specific congestion impacts with the goal of producing more accurate welfare 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 welfare impacts at different times of day. By matching hour-specific estimates of the impact of TNCs to hour-specific traffic VMT and hour-specific estimates of value of time, my welfare calculations account for temporal heterogeneity that pooled estimates may not reflect. To demonstrate this idea, I also estimate a ‘naive’ approach, pooling over hour of day. This estimator is equation 1, but run without interacting hour of day fixed effects with the treatment indicator, $D_t * year_t$. The rationale for the naive regression is to simulate what estimation and inference might look like using disaggregate data.

In order to investigate spatial heterogeneity, I estimate a model pooling over hours of day and allowing an idiosyncratic treatment effect for each road segment. This model is equivalent to equation 1, but does not use hour of day fixed effects, and interacts the set of segment dummies with the treatment indicator, $D_t * year_t$.

$$mpm_{i,t} = \alpha + \beta * D_t * year_t * seg_i + \gamma_1 D_t * seg_i + \gamma_2 year_t * seg_i + \gamma_4 * year_t * linear_day_t * seg_i + \Gamma X_t + \epsilon_{i,t} \quad (2)$$

Lastly, I estimate a regression discontinuity model, again interacting each term with a set of hour of day dummies:

$$mpm_{i,t} = \alpha + \beta D_t + \gamma_1 * seg_i + \gamma_2 linear_day_t * seg_i + \gamma_3 * year_t * (linear_day_t)^2 * seg_i + \Gamma X_i + \epsilon_{i,t} \quad (3)$$

This is not my preferred specification for two reasons. First, I am interested in ‘equilibrium’ changes in congestion, i.e., the difference in a city with and without TNCs. If computers take time to transition to their new optimal mode choice, a regression discontinuity may not reflect the coefficient of interest. Second,

regression discontinuity designs may be biased by seasonal patterns in traffic speeds that a difference in differences design would more effectively capture.

5.3 Threats to Identification

Outside of unobserved year-specific events impacting traffic, I identify 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 dropped as outliers, β will not be identified. In other words, for other modes of transportation to bias my estimates, speed must be mismeasured, and that mismeasurement must be correlated with the treatment. Two pieces of evidence suggest that this may not be the case. First, the data filtering applied by Post Oak Traffic Systems produces sensible estimates of traffic speeds—there is no evidence of systematic mismeasurement. Second, [Hampshire et al. \(2018\)](#) show that the substitution channel is relatively small: the shares of Austinites that switched to walking or biking following the failure of proposition 1 were 0.7 and 1.8%, respectively. That is, there isn't strong reason to believe any mismeasurement would be correlated with the treatment.

I nonetheless draw on a second traffic speed dataset to empirically examine this concern. In addition 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 measurements, this would be reflected in a change in the *relationship* between the two measurements. For example, say we have a segment-sensor pair, and prior to May 9th, 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 increased 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 9th 2016 dummy. If I find a statistically (and economically) significant difference in slopes, I treat this as evidence of mode-choice related bias.

The details and results of this exercise are 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 6, the coefficient on the interaction between the post dummy and the pneumatic segment speed is not statistically different from zero ($post * dummy = -0.009, p = 0.67$), 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.

The details of this exercise are reported 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 time same 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 the 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 welfare costs of TNC activity.

6 Results

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 2. 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 $\beta_h = 0 \forall h$ ($p < 0.01$). The largest improvements in travel times following TNC exit come, surprisingly, between 11 a.m. to 2 p.m. Point estimates for off-peak hours (8pm - 6am) are small and straddle zero. Table 4, shows the results from running a ‘naive’ version of 1, pooling across hours. On average, speeds increase by 0.042 minutes per mile (1.3%) following TNC exit, but this result is not statistically significant ($p = 0.24$). Consistent with the hour-specific estimates, restricting the naive analysis to peak hours (7 a.m. to 7 p.m.) generates a larger estimates of speed increases following TNC exit ($post * treated = -0.07$, $p = 0.11$). Figure 4 displays an event study, plotting 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 trend assumption.

Figure 3 plots results from equation 3, a regression discontinuity. These figures are larger in magnitude, and suggest that the short-run impacts of TNC exit may be more pronounced than long-term impacts. Results from equation 2, which allows for segment-specific congestion responses, are plotted in figures 5 and 6. Figures 5 shows that while the mean traffic segment sees an improvement in travel speeds following TNC exit, there is significant heterogeneity in segment-specific responses.

6.1 Welfare Calculations

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

$$\Delta \text{ welfare} = \sum_h \Delta \text{ minutes per mile}_h * \text{miles driven}_h * \text{value of time}_h \quad (4)$$

$\Delta \text{minutes per mile}$ 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. Finally, I calculate an Austin-specific willingness to pay for reductions in travel time (value of time_h , above) using data from Austin’s variable-toll freeway (see Appendix A). This sum can be written in vector notation:

$$\Delta \text{ welfare} = \beta \mathbf{R} \quad (5)$$

Where element h of vector \mathbf{R} equals $\text{miles driven}_h * \text{value of time}_h$. The standard error of this object is then $\sigma_w = \mathbf{R}' \Sigma \mathbf{R}$, where Σ is the covariance matrix associated with β .

I report the results of this exercise in Table 3. Using my preferred specification (equation 1),

I recover estimates of daily congestion costs associated with Uber and Lyft activity of \$134,568, with a standard error of \$53,561, 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 \$49,117,395.

Several outside studies provide valuable context when interpreting these numbers. First, according to the Inrix Global Scorecard, the aggregate 2017 travel time cost in Austin, TX was \$2.8 billion. My estimates therefore suggest Uber and Lyft together accounted for roughly 1.8% of travel time costs in Austin. Second, estimates of consumer surplus associated 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 congestions cost must be strictly larger than estimates of consumer surplus. Cohen et al. conclude that in the four cities they examine, \$1.57 dollars 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 its drivers grossed \$27 million in the Austin area, 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 US 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.

A final note is that a comparison of the welfare estimates produced by my preferred specification to those generated by a naive approach (a pooled effect over hours) suggests that the convolution between congestion impacts and value of time fluctuations is consequential. As reported in table 4, applying the \$22.40 figure suggested by the Texas Department of Transportation to my pooled estimator yields an annual welfare cost of \$77,633,119—roughly 50% larger than those using temporal disaggregation.

7 Conclusion

Using a natural experiment in Austin, TX, I study whether transportation network companies—Uber and Lyft—impact traffic congestion. I find that on average TNCs increase congestion roughly 1.8%. This figure, however, masks important heterogeneity. Namely, the largest slowdowns occur between 11 a.m. and 2 p.m., when the value of travel time is low. 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 \$49 million annually to avoid the slowdowns induced by TNC activity. Back of the envelope calculations using estimates of Uber consumer surplus from [Cohen et al. \(2016\)](#) suggest that in Austin, the cost of TNC-related congestion is of similar magnitude to the consumer surplus generated by these companies.

While external validity of my results may be limited by the geographical focus of this paper, these results nonetheless have important policy implications. The impact of TNC activity on congestion in Austin, together with the potential for other positive spillovers due to TNC activity, tell a cautionary tale for policymakers aiming to reduce traffic congestion: TNC regulation appears a poor instrument to address congestion, as the associated costs may outweigh travel time benefits. Instead, TNC operation can be viewed as a rough transfer from incumbent drivers (who pay via time losses) to ridesharing users (who gain consumer surplus). These findings also have implications for emissions policies. Under the assumptions of the fundamental diagram of traffic congestion, that speeds slow in response to TNC activity suggests TNCs add vehicles to the road. In other words, the ride-induction effect dominates the ride-sharing effect. This conclusion will be important to test in other settings, as the impact of TNCs on VMT is an important uncertainty in the prediction of transportation sector emissions. Lastly, this paper contributes to a growing literature studying the difficulty of achieving pareto improvements in practice. While the finding that TNC regulation appears (in Austin) to be a blunt policy instrument is not particularly surprising, it is an important result given the relative political viability of TNC regulation compared to road pricing. The findings I present therefore provide further motivation for continued research into politically palatable instruments that can effectively target congestion-related externalities.

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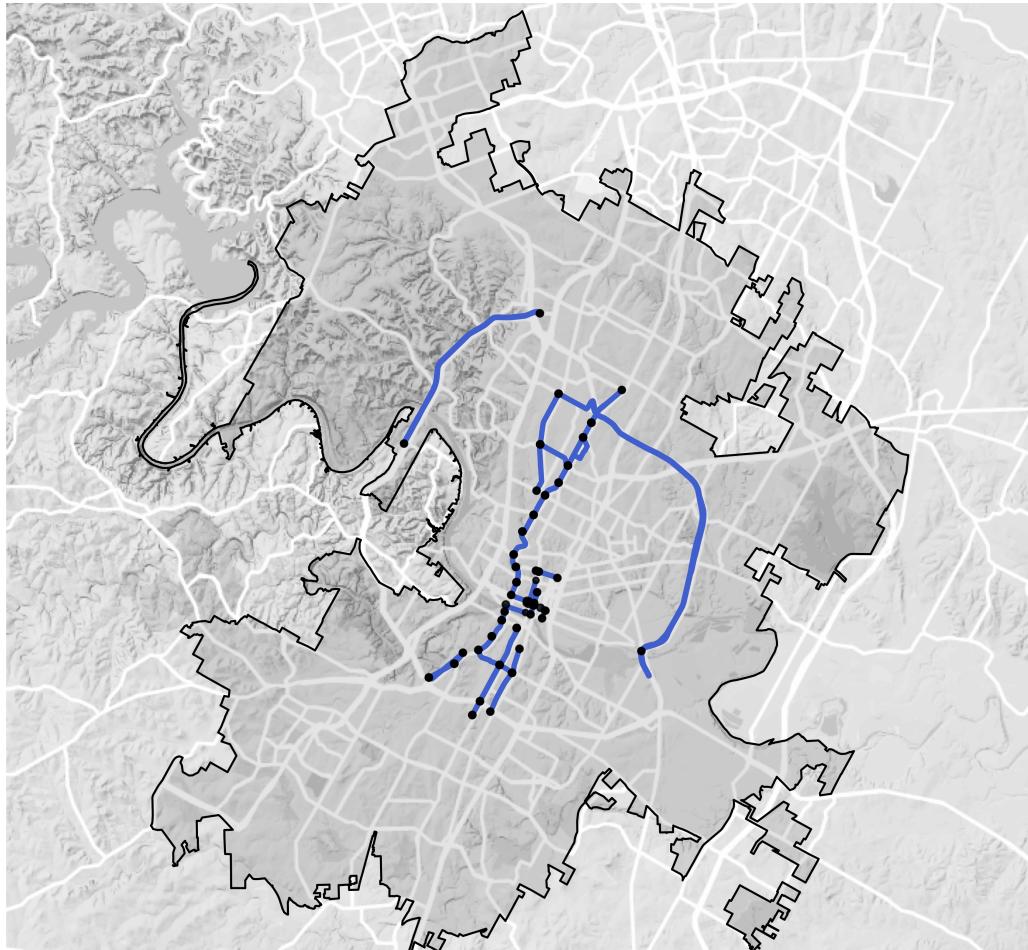
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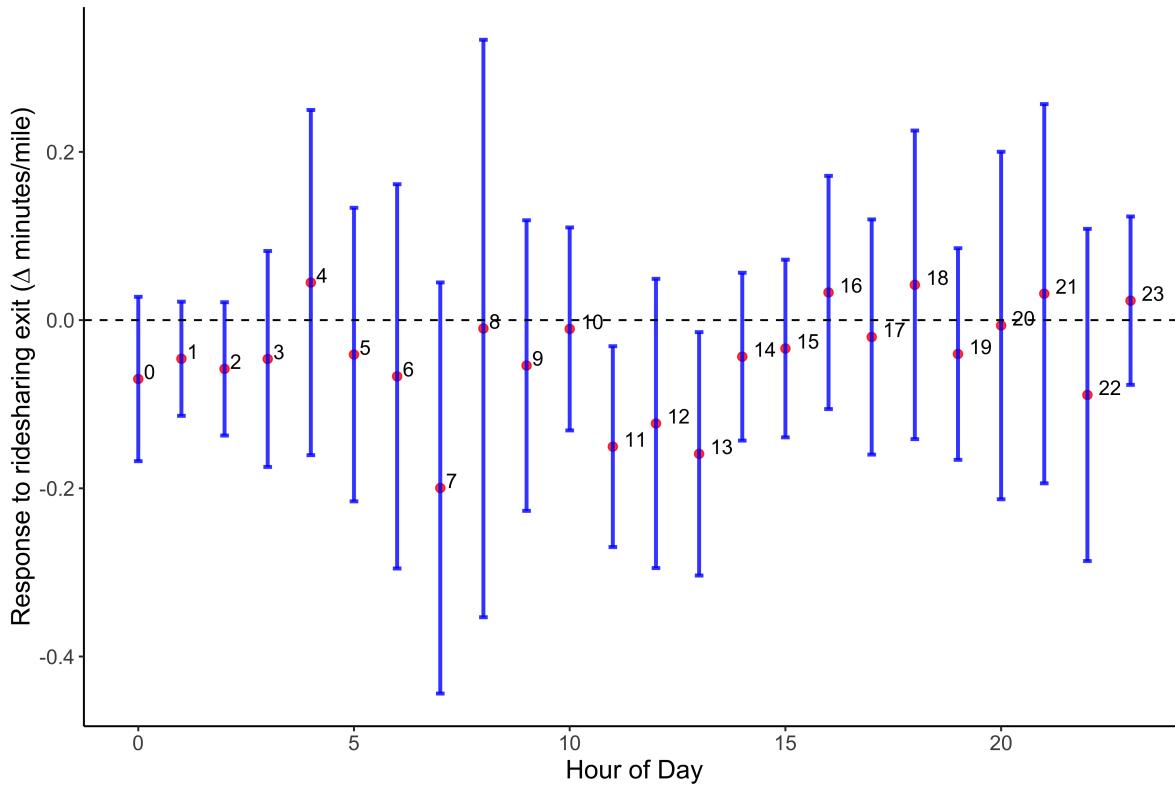
8 Figures

Figure 1: Bluetooth Segment Locations in Austin, TX



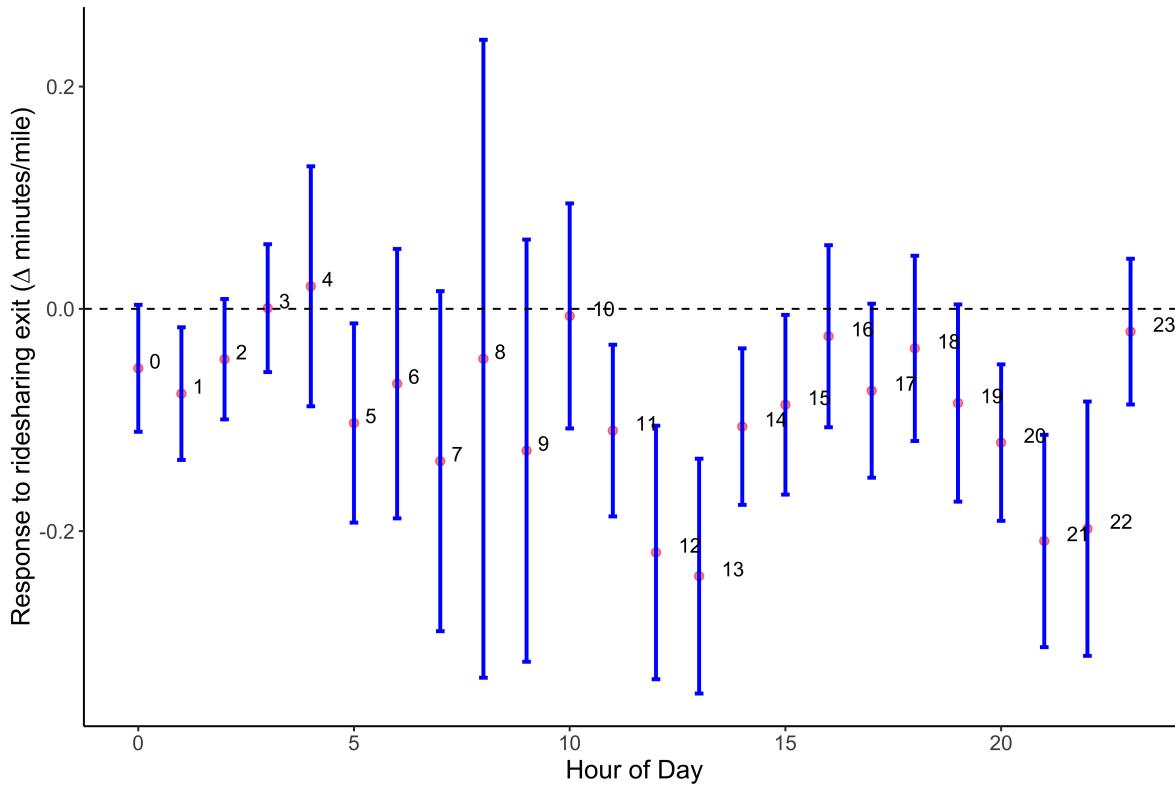
Note: Nodes represent terminal bluetooth sensor locations for each of the 79 segments used in analysis. Note that some sensors act as both origin and destination readers for different segments. Paths represent google maps recommended driving directions between endpoints of a given segment. The black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

Figure 2: Difference in Differences



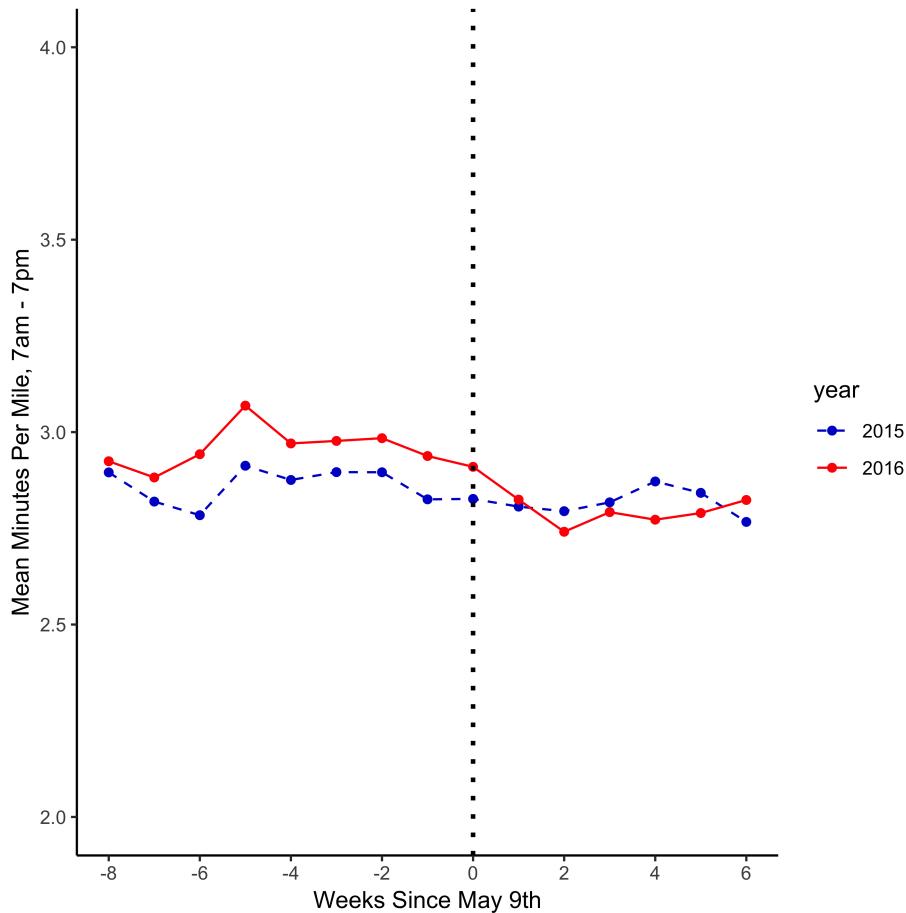
Note: Results from equation 1, a difference in differences comparing pre vs. post May 9th traffic speeds in 2015 (where both Uber and Lyft operated in Austin) to pre vs. post May 9th traffic speeds in 2016 (where both Uber and Lyft exited Austin). Points represent the estimated effect of TNC departure on traffic speeds (measured in minutes per mile) by hour of day. Bars reflect 95% confidence intervals from two-way standard errors clustered by sensor-week. Controls include segment-specific linear in day trends, a dummy for precipitation, day of week fixed effects, and year and post-May 9th dummies. Traffic speed data, accessed through the City of Austin's OpenData Portal, represent average speeds of bluetooth devices traversing 79 road segments in Austin, TX.

Figure 3: Regression Discontinuity



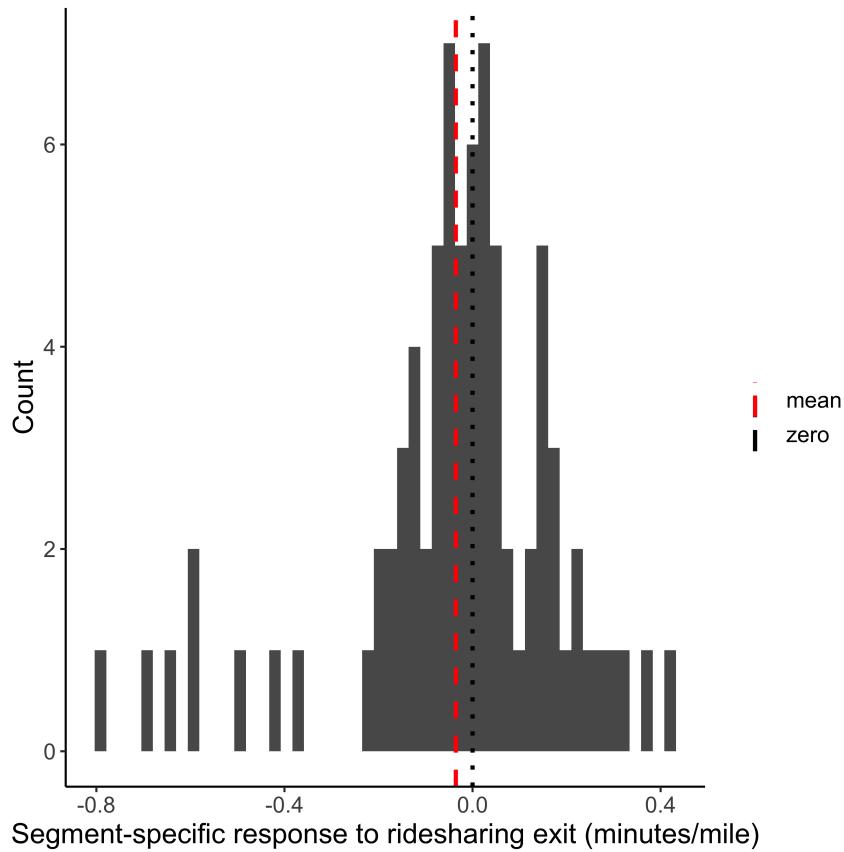
Note: Results from equation 3, a regression discontinuity performed on traffic speeds across 79 road segments in Austin, TX. The bandwidth is April 1st - August 1st of 2016, which (asymmetrically) spans the May 9th departure of Uber and Lyft. Points represent the estimated effect of TNC departure on traffic speeds by hour of day. A negative point indicates an estimated increase in traffic speed. Bars reflect 95% confidence intervals from two-way standard errors clustered by sensor-week. Traffic speed data are maintained by the City of Austin's OpenData project.

Figure 4: Event Study



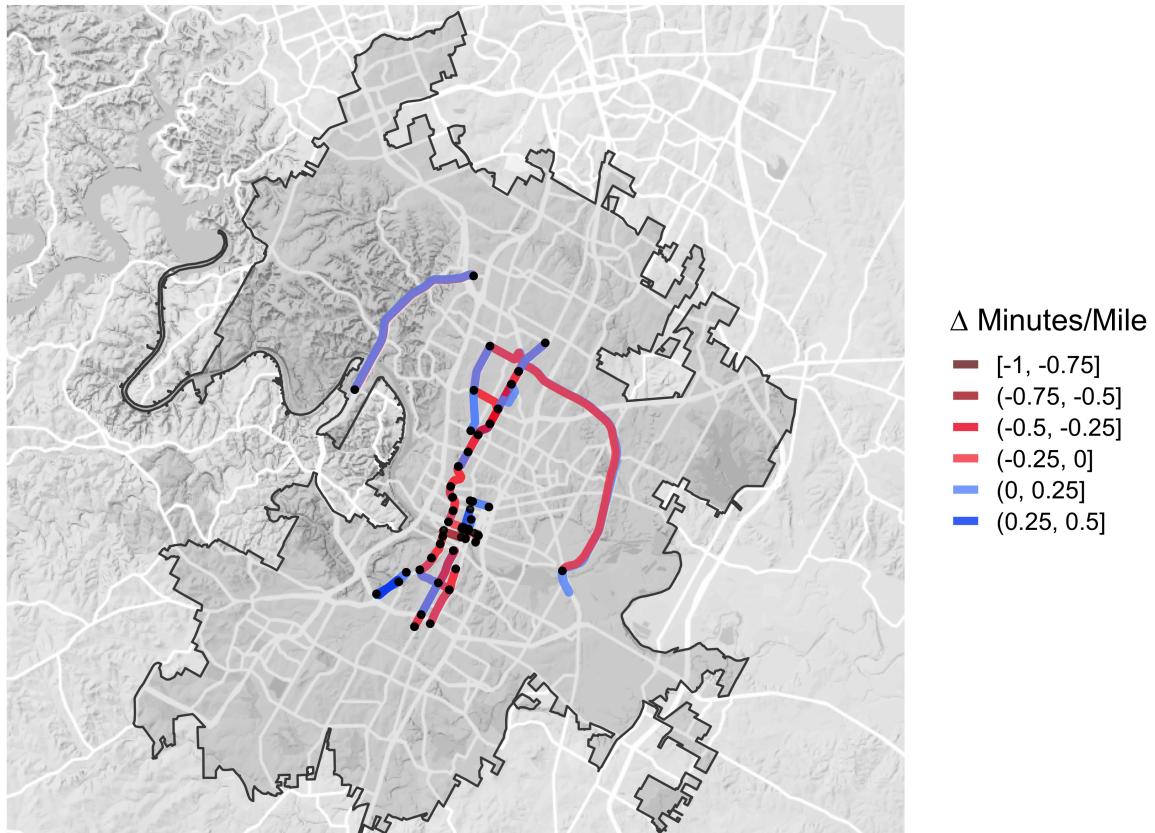
Note: This figure shows raw average speed (minutes per mile) between 7 a.m. and 7 p.m. over 79 road segments in Austin, TX, plotted by week of year for 2015 and 2016. Data were accessed through the City of Austin's Open Data Portal. The dotted line represents the week of May 9th, where Uber and Lyft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

Figure 5: Distribution of Segment-Specific Responses



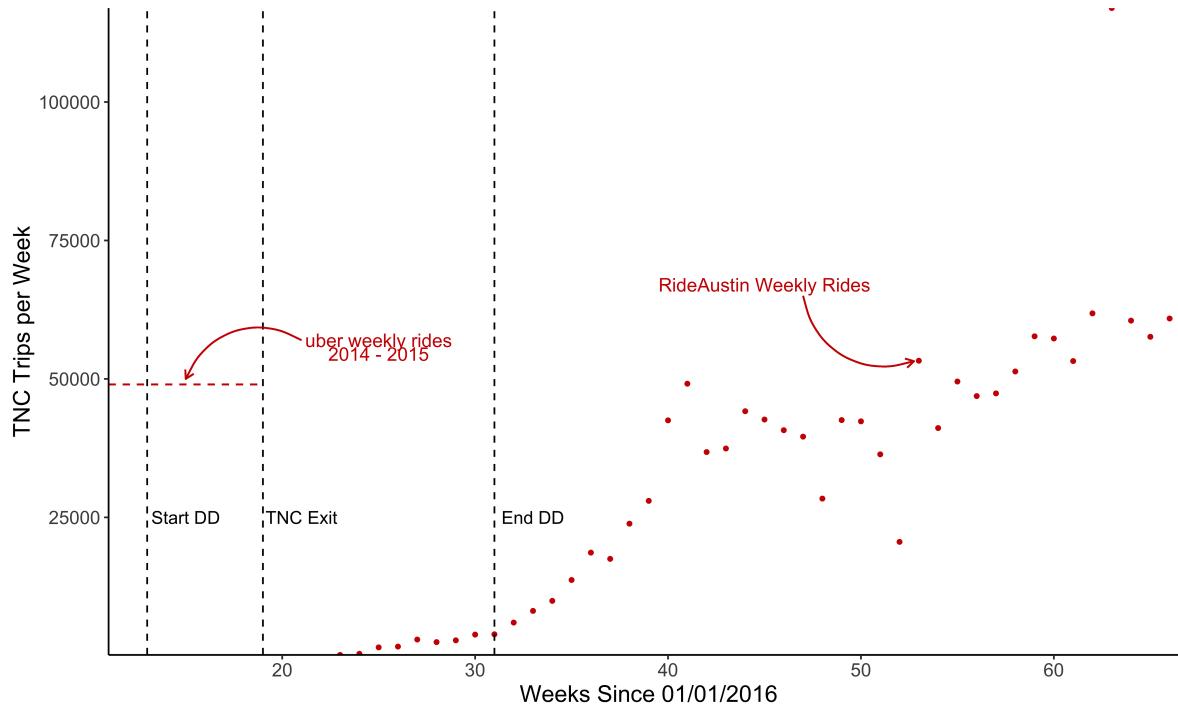
Note: Results from equation 2, a difference in differences comparing pre vs. post May 9th traffic speeds in 2015 (where both Uber and Lyft operated in Austin) to pre vs. post May 9th traffic speeds in 2016 (where both Uber and Lyft exited Austin), allowing for segment-specific congestion responses. Bars represent the number of segments with idiosyncratic changes in traffic speeds falling within a given bin. The black dotted line is zero minutes/mile, and the red dashed line is the mean of segment-specific responses. Traffic speed data were accessed through the City of Austin's Open Data Portal.

Figure 6: Segment-Specific Responses



Note: 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 7: Variation in TNC Activity



Note: This graph plots weekly Riseaustin 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 (April 1st), 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 (2014-2015) 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 average of 50,000 per week.

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: Regression Results

Hour of Day	β_h	se	p
0	-0.0700	0.04986	0.16028
1	-0.0460	0.03463	0.18444
2	-0.0580	0.04044	0.15150
3	-0.0462	0.06550	0.48055
4	0.0446	0.10471	0.66991
5	-0.0410	0.08905	0.64543
6	-0.0669	0.11656	0.56613
7	-0.1996	0.12469	0.10939
8	-0.0100	0.17516	0.95447
9	-0.0540	0.08815	0.53987
10	-0.0105	0.06156	0.86452
11	-0.1504	0.06091	0.01352
12	-0.1229	0.08773	0.16131
13	-0.1590	0.07382	0.03124
14	-0.0435	0.05091	0.39319
15	-0.0338	0.05390	0.53083
16	0.0329	0.07074	0.64235
17	-0.0201	0.07133	0.77816
18	0.0419	0.09360	0.65404
19	-0.0403	0.06419	0.52990
20	-0.0064	0.10541	0.95142
21	0.0314	0.11503	0.78487
22	-0.0890	0.10076	0.37708
23	0.0231	0.05108	0.65110
F-test			0.00000

Note: Results from equation 1, a difference in differences comparing pre vs. post May 9th traffic speeds in 2015 to pre vs. post May 9th 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 9th dummies. Standard errors are clustered by sensor-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: Welfare Estimates

daily cost (\$)	<i>se</i>	<i>p</i>	annual cost (\$)
-134,568	53,561	0.0060	-49,117,395

Note: Estimates of the travel-time welfare costs of TNC operation in Austin, TX. Figures are the result of the exercise described in equation 4, which matches hour-specific changes in travel time to hour-specific willingness to pay estimates and hour-specific traffic volume measurements. Standard errors for welfare figures are calculated as described in section 6.

Table 4: Naive Regression

	β (Δ minutes/mile)	<i>se</i>	<i>p</i>	implied annual cost (\$)
post*treatment (All hours)	-0.0424	0.0364	0.2448	-77,633,119
post*treatment (7 a.m. - 7 p.m.)	-0.0675	0.0427	0.1136	-65,475,857

Note: 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 9th dummies. Standard errors are clustered by sensor-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 Texas Department of Transportation's \$22.40 per hour value of travel time. Traffic data were accessed through the City of Austin's OpenData Portal.

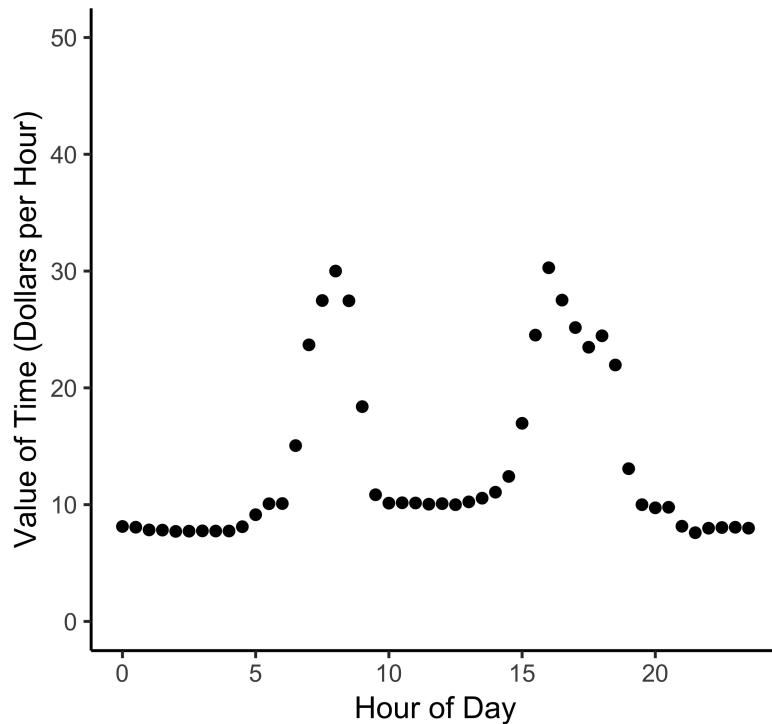
A Revealed Preference WTP Estimates

The MoPac (Texas State Highway 1) is a north-south route in Austin. Starting in November 2017, as part of the MoPac improvement project, the Central Texas Regional Mobility Authority opened an 11-mile variable-price express lane (plotted in figure 9) on the MoPac. The price of using this lane adjusts 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 southbound entrances.

Using 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 conditions, 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. For example, if the toll averages \$3.00 between 9:00 am and 9:30 am on December 1st, and the express lane is on average 6 minutes faster than the free lane for that time of day, then the implied WTP is \$0.5 per minute. I then aggregate these estimates by 30-minute block (that is, I take the mean of all estimates of WTP from 9:00 to 9:30 am, 9:30 to 10:00 am, etc.) to produce estimates of WTP by time of day.

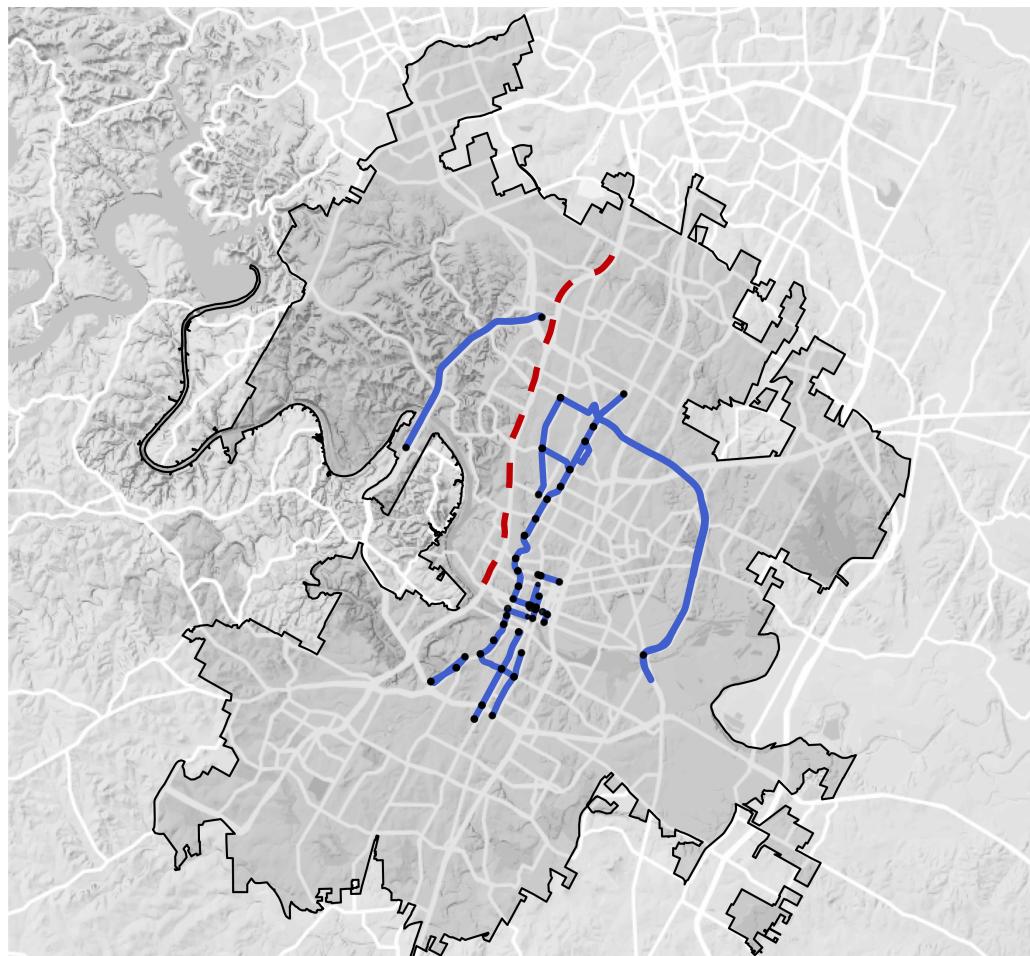
These estimates are summarized in figure 8, and are broadly consistent with estimates of value of travel time estimated in related settings ([Small and Verhoef, 2007](#)). Importantly, however, WTP peaks during morning and evening rush hour periods, possibly reflecting different commuters, or heterogeneity in value of time based on trip purpose.

Figure 8: Willingness to Pay for Travel Time Reductions



Note: 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.

Figure 9: Mopac Expressway



Note: 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. Variable pricing on the Mopac expressway allows for the estimation of location-relevant willingness to pay for travel time reductions.

B Threats to estimation 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 measurements. I identify 39 instances where bluetooth road segments overlie pneumatic sensors (see figure 10), 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 mismeasurement must be correlated with the treatment. To test for this bias, I perform the following regression:

$$mpm_{i,t} = \alpha + \beta_1 sensor_mpm_{i,t} + \beta_2 D_t * sensor_mpm_{i,t} + \gamma_1 month_t + \gamma_2 seg_i + \epsilon_{i,t} \quad (6)$$

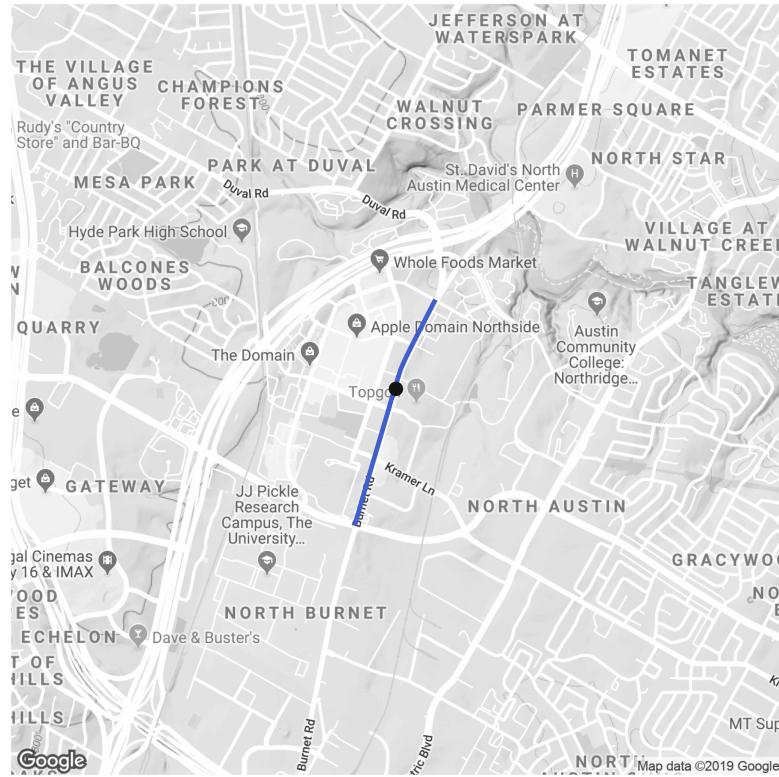
Where $mpm_{i,t}$ is the bluetooth speed measurement on segment i and time t , and $sensor_mpm_{i,t}$ is the pneumatic road segment speed measurement on segment i and time t . D_t is a treatment dummy, which equals one for days after May 9th, 2016. $month_t$ is a set of month of year fixed effects, and seg_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 measurements. Results from equation 6 are displayed in table 6.

Table 6: Tests for Bias

	coefficient	se	p
$sensor_mpm$	0.0366	0.0221	0.0979
$D * sensor_mpm$	-0.0094	0.0222	0.6727

Note: Results from equation 6, which tests whether the relationship between pneumatic speed measurements and bluetooth speed measurements 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 10: Example of Segment-Sensor Matching



Note: 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. Pneumatic road sensors measure road traffic only, allowing me to test for pedestrian-induced bias in the bluetooth traffic data.

C Threats to estimation 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 that 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 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 7. 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 assumptions about TNCs as a share of total vehicles (usually 5-15%), should not generate meaningful bias in the results reported above.

Table 7: TNC vehicle speeds

Δ Minutes per Mile	<i>se</i>	<i>p</i>
-0.0324	0.1643	0.8438

Note: 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 minutes 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

Table 7 shows the results of difference in difference regressions using alternate linear trend specifications. Table 8 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.

Table 7: Alternate specifications

Hour of Day	Model 1		Model 2		Model 3	
	β_h	p	β_h	p	β_h	p
0	-0.0700	0.16	-0.0736	0.12	-0.0861	0.14
1	-0.0460	0.18	-0.0561	0.06	-0.0707	0.16
2	-0.0580	0.15	-0.0654	0.02	-0.0932	0.13
3	-0.0462	0.48	-0.0541	0.35	-0.0831	0.18
4	0.0446	0.67	0.0215	0.83	-0.0909	0.31
5	-0.0410	0.65	-0.0640	0.49	-0.1794	0.08
6	-0.0669	0.57	-0.1103	0.29	-0.2566	0.04
7	-0.1996	0.11	-0.2184	0.04	-0.3088	0.11
8	-0.0100	0.95	-0.0403	0.83	-0.1298	0.44
9	-0.0540	0.54	-0.0841	0.33	-0.0772	0.52
10	-0.0105	0.86	-0.0218	0.70	-0.1108	0.24
11	-0.1504	0.01	-0.1537	0.01	-0.0608	0.31
12	-0.1229	0.16	-0.1282	0.13	-0.1317	0.10
13	-0.1590	0.03	-0.1635	0.02	-0.0901	0.12
14	-0.0435	0.39	-0.0454	0.37	-0.0482	0.29
15	-0.0338	0.53	-0.0356	0.50	-0.0316	0.52
16	0.0329	0.64	0.0330	0.64	0.0388	0.50
17	-0.0201	0.78	-0.0184	0.79	0.0089	0.87
18	0.0419	0.65	0.0379	0.69	-0.0298	0.60
19	-0.0403	0.53	-0.0396	0.54	-0.0318	0.56
20	-0.0064	0.95	-0.0114	0.91	-0.0252	0.72
21	0.0314	0.78	0.0314	0.78	-0.0126	0.89
22	-0.0890	0.38	-0.0873	0.37	-0.0578	0.49
23	0.0231	0.65	0.0263	0.58	-0.1203	0.12
F-test	0.00		0.00		0.00	

Note: 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 8: Alternate segment groups

Hour of Day	Group 1		Group 2		Group 3	
	β_h	p	β_h	p	β_h	p
0	-0.0700	0.16	-0.0495	0.30	-0.2672	0.17
1	-0.0460	0.18	-0.0465	0.05	-0.2406	0.16
2	-0.0580	0.15	-0.0460	0.07	-0.2667	0.20
3	-0.0462	0.48	-0.0229	0.56	-0.2157	0.29
4	0.0446	0.67	0.0054	0.93	-0.2228	0.38
5	-0.0410	0.65	-0.0533	0.46	-0.4003	0.15
6	-0.0669	0.57	-0.0716	0.36	-0.4217	0.19
7	-0.1996	0.11	-0.1486	0.15	-0.7895	0.12
8	-0.0100	0.95	-0.0605	0.72	-0.7129	0.10
9	-0.0540	0.54	-0.0801	0.29	-0.3155	0.44
10	-0.0105	0.86	-0.0535	0.28	-0.5148	0.15
11	-0.1504	0.01	-0.1310	0.02	-0.2157	0.21
12	-0.1229	0.16	-0.0759	0.39	-0.3375	0.13
13	-0.1590	0.03	-0.1099	0.11	-0.2540	0.19
14	-0.0435	0.39	-0.0209	0.68	-0.2349	0.16
15	-0.0338	0.53	-0.0334	0.44	-0.1379	0.40
16	0.0329	0.64	0.0386	0.57	-0.0132	0.93
17	-0.0201	0.78	-0.0243	0.74	-0.0487	0.75
18	0.0419	0.65	-0.0062	0.94	-0.1834	0.32
19	-0.0403	0.53	-0.0441	0.47	-0.0673	0.69
20	-0.0064	0.95	0.0113	0.91	0.0215	0.88
21	0.0314	0.78	0.0860	0.34	-0.0440	0.78
22	-0.0890	0.38	-0.0596	0.54	-0.0517	0.73
23	0.0231	0.65	0.0249	0.59	-0.4236	0.10
F-test		0.00		0.00		0.06

Note: 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 $\beta_h = 0 \forall h$.