Getting from Ridehailing to Ride*sharing*: Effects of a Congestion Tax in Chicago

Samuel Stolper and Morteza Taiebat*

February 28, 2024

Abstract

We study the impacts of a price incentive to share rides, introduced as part of a City of Chicago congestion tax targeting ride-hailing use. We estimate via difference-in-differences that a \$1.00 rise in the relative price of a private ride causes a 2.2 percentage-point (or 24 percent) rise in the short-run rate of shared ridership. This effect is driven overwhelmingly by substitution from private to shared trips rather than reduced travel. The effect size, however, is modest. Observed price/time-cost tradeoffs suggest that the in-vehicle disutility of sharing a ride is a significant barrier to ridesharing takeup.

Keywords: ridesharing; congestion; taxation; transportation policy
JEL Codes: R41, R48, H23, Q48

*Both authors' affiliation: University of Michigan, School for Environment and Sustainability. We are grateful to seminar participants at Midwest Energy Fest, the AERE Summer Conference, University of Michigan, Michigan State University, and Cornell University, as well as Michael Moore, for helpful comments.

1 Introduction

Transportation network companies (TNCs) like Uber, Lyft, and Didi are now fixtures of urban transportation systems around the world, providing on-demand ridehailing services to thousands of cities and hundreds of millions of customers (Uber, Inc., 2023; Liu, 2023), and with potential for vast expansion if vehicle automation becomes widespread (Boffa et al., 2023). These services can yield significant consumer surplus through increased travel (Christensen and Osman, 2023; Camilo Castillo, 2023; Cohen et al., 2016). However, increased car travel also imposes external costs through impacts on climate, environment, traffic safety (Barrios et al., 2022), congestion (Tarduno, 2021; Christensen and Osman, 2023), and road quality.

Vehicle electrification – in conjunction with a decarbonized electricity grid – is a key strategy for neutralizing climate and environmental externalities. To that end, Lyft has committed to a 100-percent electric vehicle (EV) fleet in its United States (US) and Canada service territory by 2030 (Lyft, Inc., 2023), while Uber has the same goal for its global ridehailing service territory by 2040 (Hawkins, 2020). But this strategy does nothing to address congestion, accident risk, and road damage. Ride*sharing*, in contrast – sharing a trip with an independent rider – can reduce all of these, by reducing VMT. The external impacts of TNCs may thus be mitigated by a heavier reliance on shared trips. Yet ridesharing rates have never been high, ranging from 13-36 percent in select US cities before the COVID-19 pandemic (Schaller, 2021; Brown, 2020), and Lyft recently discontinued its ridesharing platform (Davalos, 2023). The social value proposition of TNCs hinges in part on whether riders can and do share their rides.

In this paper, we study the short-run effects of price incentives to share TNC rides. We focus on a congestion policy introduced by the city of Chicago, called the Ground Transportation Tax, that imposed, beginning in January 2020, a set of fees exclusively targeting TNC rides and differentiated by location, timing, and whether a ride is shared or private. Chicago has recently been ranked among the most traffic-congested cities in the world (Inrix, 2022); this policy is a direct response to perceived impacts of TNCs on congestion and public transit ridership (City of Chicago, 2019) and one of the earliest examples of an effective tax on private ride-hailing.

The policy changed fees everywhere in Chicago, so there is no unaffected part of the city to serve as a counterfactual. We compare downtown-area outcomes in the "peak" time-period (6AM - 10PM on weekdays) with those in the *off*-peak period (10PM-6AM), before and after the policy introduction, in a difference-in-differences (DD) setup. Essentially, the policy introduced a wedge between the cost of a shared trip and a private trip, and the wedge was larger (by $1.15 per ride) in the peak time period than in the off-peak period. The policy "treatment" we are studying is thus a (relative) cost premium on private TNC ridership. Since the policy took effect on January 6th, 2020 and the pandemic became widely visible in March 2020, we focus on a two-month post-period in comparison to a six-month pre-period (beginning July 2019).

We examine ride-hailing outcomes both in an all-hours sample of rides and in a subsample of rides within a half-hour of a peak/off-peak temporal boundary. In the "boundary" subset, riders are likely to have similar characteristics (such as elasticities of demand for ride-hailing) across the temporal boundary. In both samples, we show graphical evidence of parallel (peak vs. off-peak) trends in rates of ridesharing and the price premium for a private ride. In the all-hours sample, person-trips exhibit a positive linear trend in the peak period relative to the off-peak period (which we control for in subsequent analysis).

We then estimate average effects via DD regression. In the all-hours sample, we find that the "DD policy" of an extra \$1.15 cost premium in the peak period causes a 2.28 percentage-point rise in ridesharing authorization, relative to a baseline rate of 9.1 percent. 91 percent of the private-ride cost premium is passed through to the corresponding price premium. Consistent with these numbers, we estimate an impact of +2.16 percentage points on the rate of ridesharing authorization per dollar private price premium. At the same time, we find no evidence of a reduction in overall person-trips or VMT, despite higher fees for all rides in the peak time period. These results suggest that our estimate of the impact on ridesharing rate per dollar private-ride price premium (2.16) can be interpreted as an elasticity of substitution.

We further find that effects on price differences and ridesharing rates vary significantly with income levels, trip length, and airport presence, but not with transit stop counts. The substitution elasticity is larger when both origin and destination neighborhoods are in the top quartile of median income, relative to all other rides. Shorter trips show significantly larger pass-through of the cost premium and increases in ridesharing rate. Trips to and from airports show relatively smaller effects, consistent with our trip-length estimates and the probability of higher values of time for these rides.

We then investigate why ridesharing rates are persistently low in Chicago – 12 percent even after the policy change – using observed price/time-cost tradeoffs and ridesharing rates. In our post-policy sample, a private ride during peak hours saves just above three minutes of time relative to a shared ride and costs four dollars more on average, conditional on origin and destination area. Were there to be no disutility of the in-vehicle ridesharing experience, a value of time (VOT) of greater than \$63.35 per hour would be required to prefer the private ride given this price/time-cost tradeoff. This is significantly higher than recent values estimated (Goldszmidt et al., 2020) and used in government (US Department of Transportation, 2016), which suggests that the disutility of sharing a ride is a key barrier to ridesharing takeup in Chicago. At a VOT of \$20.33 per hour (in 2019 dollars, adapted from Goldszmidt et al., 2020), we find that a rider would prefer a private ride if the dollar-value disutility of sharing is greater than \$2.96, or 17 cents per minute.

In sum, price incentives to share ridehailing trips do shift behavior, but meaningful reductions in vehicle miles traveled and traffic congestion through this policy lever would likely require much larger-magnitude price premiums on private rides. Short of this, actions that shift social norms around ridesharing, improve perceptions of the ridesharing experience, and improve the actual experience itself may make ridesharing more widespread. Ultimately, however, congestion policy should cover all vehicle miles traveled, rather than just VMT from transportation network companies, since such companies currently only account for a small percentage of citywide travel.

2 Background

2.1 Transportation network companies, externalities, and ridesharing

Transportation network companies (TNCs) have been of great interest since first entering the ridehailing industry in 2011. The fast matching of drivers and riders through use of smartphones can raise consumer welfare through increased travel and reduced wait times, and real-time pricing that reflects demand for rides and supply of drivers can further enhance efficiency in the ridehailing market. Cohen et al. (2016), for instance, estimate that the UberX service generated nearly $3 billion per year in consumer surplus in four US cities considered. Christensen and Osman (2023) show that randomly assigned price discounts on Uber in Egypt drive large increases in Uber usage and total miles traveled. Camilo Castillo (2023) finds in the context of Houston, Texas that real-time (that is, "surge") pricing raises total welfare by 1.9 percent of gross revenue and rider surplus by 3.55 percent, relative to a uniform pricing counterfactual.

Driving carries with it a number of negative externalities, however, including pollution (Holland et al., 2016), traffic congestion (Parry and Small, 2005), and accident risk (Edlin and Karaca-Mandic, 2006; Parry et al., 2007). Ridehailing today necessarily creates these social costs, but it moreover could exacerbate these costs relative to a no-TNC counterfactual – depending on how travel demand, vehicle speeds, and vehicle choices change. Hall et al. (2018) find that TNCs in the US have been a complement for public transit, raising the latter's use, while Diao et al. (2021) find that they have been a substitute. Barreto et al. (2021) find that the introduction of Uber in Brazil reduced traffic fatalities through decreased drunk driving, while Barrios et al. (2022) find that it *in*creased them in the US, through induced additional vehicle travel. The evidence on congestion effects of TNCs is less mixed: Tarduno (2021) estimates that Uber and Lyft together decreased daytime traffic speeds in Austin, Texas by roughly 2.3 percent, and Diao et al. (2021) find that they increased the frequency of congested periods in US cities by 4.5 percent. Tarduno (2023), meanwhile, studies per-ride TNC taxes in several US cities and finds little evidence of impacts on congestion or air pollution.

Vehicle electrification could lead to decarbonization but, if anything, will *in*crease VMT, due to a reduced price per mile of driving. Ridesharing, on the other hand, can reduce VMT and thus the external costs that come with it. Uber and Lyft both introduced a ridesharing option (Uber Pool and Lyft Line, respectively) in 2014 – that is, the option to pay a lower price in exchange for willingness to share a trip with another rider going in the same direction. However, ridesharing has never been the norm; surveys between 2017 and 2019 find rates in New York, Chicago, Boston, Denver, and San Francisco ranging from 13 to 36 percent (Schaller, 2021). Both companies temporarily suspended their ridesharing option during the pandemic, and both companies later rebranded their ridesharing programs to attract more users. Lyft ended its ridesharing program in May 2023, while Uber continues to offer its ridesharing program, under the name UberX Share.

There is a sizeable literature outside of economics focused on predictors of willingness to share a TNC ride (Hou et al., 2020; Alonso-Gonzalez et al., 2021; Taiebat et al., 2022). Three existing studies (Abkarian et al., 2022; Zheng et al., 2023; Liang et al., 2023) estimate policy impacts on ridesharing, all in the same Chicago Ground Transportation Tax (GTT) context as our study.[1] These studies consistently estimate the GTT to have increased shared trips and decreased total (shared plus private) trips. Our own study yields some differing results as well as some new ones. Beyond the trip data that other studies have used, we take advantage of price and travel time data to investigate (a) the effect of the policy on prices, (b) the effect of price changes on willingness to share, and (c) the relative magnitude of the disutility of ridesharing.

2.2 Policy context

The canonical economics solution to congestion is pricing to make all drivers internalize the social cost of their actions (Vickrey, 1969), but such a policy is still somewhat rare around the world. Cities like Singapore, London, Stockholm, and Milan use it, imposing a fee or system of fees for private travel within a certain zone (e.g., "downtown") during a certain time period (e.g., rush hour). New York City has approved a plan for congestion pricing with a possible 2024 start date (Ley, 2023); it would be the first US city to employ this policy. In contrast, fees exclusively for TNCs in the US have been notably more common. More than 20 state and city governments charge TNCs fees (Fuller and Brown, 2021) per trip or per mile within a specified area. At least three cities – New York City, Chicago, and San Francisco – and one state (New Jersey) levy fees that are differentiated by whether the trip was shared or private.

Chicago – the city we examine here – is one of the largest ride-hailing markets in the US and one of its most congested cities. Uber, Lyft, and Via together dispatched 102.5 million rides starting or ending in Chicago in 2018, which represents 271% growth relative to 2015 (City of Chicago, 2020). Yet ride-hailing made up only about three percent of total regional VMT measured in September of that year (Fehr & Peers, 2019). Inrix (2022) estimates that Chicagoans lost 155 hours per person to traffic in 2022, which ranked it second most-congested globally. A 2019 report (City of Chicago, 2019) emphasized TNCs' negative effect on congestion, and later that year the city approved the "Ground Transportation Tax", a new set of congestion-motivated fees for TNC trips. The new

[1] Another relevant study is by Chang et al. (2021), who estimate the effect of increased comfort of Microsoft commuter buses – a different form of ridesharing – on employees' choice of commuting mode.

policy went into effect starting January 6, 2020 and is currently the highest ride-hailing tax in the nation, estimated to have raised $128 million for the city in 2022 (City of Chicago, 2023).

The policy changed the TNC fee per ride everywhere in the city. First, it newly distinguished between a relatively higher-traffic "downtown zone" of Chicago and the lower-traffic remainder of the city (which we call "neighborhoods"): per-ride fees became higher in (that is, to or from) the downtown zone from 6AM-10PM on weekdays. The map in Figure 1 (Panel A) identifies this zone's geographic span. Second, the policy introduced a wedge between the fees for private rides versus shared ones. In the neighborhoods as well as during the off-peak time in the downtown zone, this wedge was 60 cents; at peak time in the downtown zone, it rose to $1.75. The table in Figure 1 (Panel B) presents the precise fee structure before and after the policy took effect.

3 Empirics

3.1 Data

The City of Chicago has had a data sharing mandate in place for TNCs since November 2018 (City of Chicago, 2020). We use the city's person-trip level database, which covers all Lyft, Uber, and Via passenger rides starting or ending in the city. The data include the day, time rounded to the nearest quarter of an hour, and origin and destination census tracts and community areas[2] of each person-trip (to which we will refer interchangeably as "trips" or "rides"). We observe whether the rider (a) authorized ridesharing and (b) successfully shared at least a part of the ride; authorizing a shared ride does not always result in successfully sharing, but the rider secures the shared-trip cost regardless. We also observe the price (excluding tip) of each ride rounded to the nearest $2.50 increment, the ride distance (person-level VMT), and the ride duration in seconds. We collect data from July 1, 2019 to March 8, 2020.

There are 75.7 million raw ride records. After removing rides missing required information or with illogical values, we are left with 64.5 million rides in the city-wide cross-sectional sample. Our analysis focuses on the downtown zone, which covers 33.5 million rides out of the remaining 63.4 million (52.8 percent). We furthermore restrict the analysis to weekdays – since there is no variation in peak/off-peak status on weekends – and non-holiday weeks, which leaves us with 20.7 million rides. For computational tractability, we collapse this sample to observations at the level of community area (CA) pair, week, and hour range (peak or off-peak).[3] Our final, "all-hours" analysis sample contains 28,086 observations.[4] We also conduct our analysis with a parallel

[2]Community areas are spatial units defined and used by the City for urban planning purposes. They are larger than census tracts; there are 77 community areas and 866 census tracts in Chicago.

[3]Using census tract level observations would require us to drop the 23 percent of rides missing a census tract due to the City's deidentification policy.

[4]In the Appendix, we provide further details on our data cleaning steps and assignment of rides to downtown or neighborhoods.

Figure 1: Details of Chicago's TNC congestion policy

Panel A. Area of Chicago designated "downtown" by the policy

Panel B. Ride-hailing fees, before after the policy change

	Pre-policy (1/1/19-1/5/20)	Post-policy (1/6/20-present)	
	All	Private	Shared
Downtown peak	\$0.72	\$3.00	\$1.25
Downtown off-peak	\$0.72	\$1.25	\$0.65
Neighborhoods	\$0.72	\$1.25	\$0.65
Airports – downtown peak	\$5.72	\$8.00	\$6.25
Airports – all others	\$5.72	\$6.25	\$5.65

Notes: "Downtown" is the area defined in the Panel A map. Airports include O'Hare and Midway. "Neighborhoods" span the remaining areas of the city. "Peak" refers to 6am-10pm on weekdays, as defined by the policy; "off-peak" spans all other times. Source: City of Chicago.

"boundary" sample whose only difference is a restriction to rides that start within a half-hour of a peak/off-peak boundary – that is, between 5:30 and 6:30AM or between 9:30 and 10:30PM.

To the full, all-hours sample, we merge in CA-level median income estimates from the Chicago Metropolitan Agency for Planning (2020) and transit stop counts from the National Neighborhood Data Archive (2020). Table 1 provides summary statistics on all relevant variables, for both the all-hours sample and the boundary sample. Appendix Figures A1 and A2 present overall time series of person-trip counts, ridesharing rates, and prices. For context, we highlight a few key averages in the all-hours sample from Table 1 (Panel B): 738.4 person-trips per CA-pair week hour-range observation; 10.4 percent of trips authorized to share; and a \$4.11 difference between average shared price and average private price (where both types of trips are observed). The boundary sample (Panel A) has less than 10 percent of the total trips contained in the all-hours sample but has a similar ridesharing rate (10.0 percent) and private price premium (\$3.51).

3.2 Methods

The policy changed the fee structure for TNC rides everywhere and for all times of day and night. Thus there is no purely untreated group in Chicago to act as a control. We do not attempt to estimate full-policy impacts; rather, we estimate "marginal" impacts of high fees relative to low fees. Our strategy is a DD comparison of a higher-fee group and a lower-fee group, before and after the fees change.[5] We compare the downtown area during the policy's peak period (weekdays, 6:00AM-10:00PM) to the downtown area during the off-peak period (weekdays, 10:00PM-6:00AM), before and after the policy change. The change caused the cost premium for a private ride to rise by \$1.15 more in the peak period than in the off-peak period.

Willingness to share a ride, however, is in part a function of one's value of time, which has been shown to vary not just across people but also across time of day (Goldszmidt et al., 2020). Treatment *intensity* already varies with time of day through the policy's peak/off-peak designation; if treatment effect and treatment intensity are correlated, then we risk conflating the two (Sun and Shapiro, 2022). It is for this reason that we conduct our analysis in both the all-hours sample and the "boundary" sample described above. Within a half-hour of a peak/off-peak boundary, riders on either side of the boundary are more likely to have similar elasticities of demand for ridehailing and elasticities of substitution from private to shared ridership, among other characteristics – which strengthens the theoretical argument for a causal interpretation of the DD comparison.

With both this boundary sample and the full, all-hours sample, we make graphical DD comparisons in two ways: plotting raw weekly averages in the peak and off-peak periods, and plotting week-specific peak/off-peak differences estimated via event study regression. The specification is

$$Y_{ith} = \alpha + \Sigma_w \tau^w D^w_{ith} + \theta_t + \phi_{ih} + \epsilon_{ith}. \tag{1}$$

An observation is a CA-pair i, in week t, in peak or off-peak period h. Y_{ith} is the outcome variable of choice. The D^w_{ith} are binary variables for an observation falling in week w and in the peak period; the τ^w are week-specific event study coefficients. θ_t and ϕ_{ih} are week and CA-pair hour-range fixed effects, respectively. We cluster standard errors at the CA-pair level (rather than pair-by-hour-

[5] This is analogous to an exposure-model strategy, in which a uniform policy change has differential exposure across groups, and which is used across many fields of applied microeconomics (Finkelstein, 2007; Nunn and Qian, 2011).

Table 1: Summary statistics in the main analysis samples

	N	Mean	St. Dev.	Min.	Max.
Panel A. Boundary sample					
Post (0/1)	28,086	0.29	0.45	0	1
Peak (0/1)	28,086	0.5	0.5	0	1
Person-trips	28,086	51.84	164.3	0	2,756
Person-trips authorized to be shared	28,086	5.17	11.54	0	205
Person-trips successfully shared	28,086	3.81	7.61	0	101
Private person-trips	28,086	46.68	153.9	0	2,579
Authorized to share (%)	23,708	9.97	9.13	0	100
Successfully shared (%)	23,708	7.35	8.29	0	100
Average price, shared ride (requested) ($)	17,912	10.57	4.42	3.22	45.00
Average price, private ride ($)	22,644	14.04	7.47	7.27	137.32
Average-price differential, shared vs. private ($)	16,848	3.51	2.26	-19.42	115.96
Panel B. All-hours sample					
Post (0/1)	28,086	0.29	0.45	0	1
Peak (0/1)	28,086	0.5	0.5	0	1
Person-trips	28,086	738.4	3,084	0	56,853
Person-trips authorized to be shared	28,086	76.58	250.8	0	5,049
Person-trips successfully shared	28,086	57.24	174.8	0	2,807
Private person-trips	28,086	661.8	2,847	0	53,844
Authorized to share (%)	26,978	10.37	6.37	0	100
Successfully shared (%)	26,978	7.75	5.66	0	100
Average price, shared ride (requested) ($)	24,940	10.14	4.30	4.98	55.28
Average price, private ride ($)	26,588	14.39	7.83	7.34	108.30
Average-price differential, shared vs. private ($)	24,550	4.11	2.30	-23.22	97.43
Average median income at dropoff CA ($)	26,978	54,118	16,451	13,518	115,756
Average # of transit stops at dropoff CA	26,978	260	80	19	521

Notes: An observation is a unique origin-destination community-area (CA) pair i in week t during hour-range r (peak or off-peak). "Boundary" sample refers to data from 5:30-6:30AM and 9:30-10:30PM only. In both panels, the sample is limited to observations from the downtown area. The first six variables are non-missing for every itr combination, even if no rides take place. "Authorized to share" and "successfully shared" are only non-missing if at least one ride takes place. The number of observations for price variables is lower because these variables are only non-missing if at least one ride of the appropriate type takes place. In calculating means and standard deviations for authorized to share, successfully shared, and the price variables, we weight by a count of the appropriate trip type, in order to present sample means that account for differences in ride volumes across locations. Data source: City of Chicago.

range) following de Chaisemartin and Ramirez-Cuellar (2022), who argue that paired experiments should use pair-level clustering rather than individual-level.

We focus on three outcomes: the average private-ride price premium, the percent of rides authorized to share, or the natural logarithm of total person-trips. For the first two outcomes, we estimate Equation 1 via ordinary least squares, weighting observations by total person-trips to account for different trip volumes underlying observations of price and ridesharing rates. For the third, we estimate Equation 1 via unconditional negative binomial (NB) regression, because log person trips is a count variable with significant overdispersion (higher variance than mean), which violates the assumption that variance equals mean in the more typical Poisson model (Hausman, 2014).[6]

Figures 2 and 3 present the graphical DD comparisons, for the boundary sample and all-hours sample, respectively.[7] In both figures, private price premium (Panel A) and percent authorized to share (Panel B) exhibit parallel trends and a significant mean shift upwards in the peak/off-peak difference. Log total trips (Panel C) shows evidence of parallel trends in the boundary sample (Figure 2 but a clear pre-trend in the all-hours sample. In both cases, no policy effect is observable. In light of the pre-trend here, we include a peak-specific linear time trend in our full-sample regressions with trip-count outcomes.[8]

Regardless of parallel trends, differing substition elasticities (from private to shared ridership) across the peak and off-peak periods can theoretically still induce bias in our DD estimates. We are comforted, however, by the degree of pre-period variation observable in the left-hand graphs of Figures 2 and 3, Panels A and B. Pre-trends are parallel here through a period of significant variation in prices. If elasticities differed, we might expect to see evidence of that in the pre-policy period.

It is possible that the higher premium required to take a private ride in the peak period causes some temporal shifting of rides from the peak period to the off-peak one, and that this contamination of the counterfactual could bias our regression estimates. Appendix Figures A6 and A7 show a relatively larger policy effect on shared rides and a relatively smaller negative impact on private rides. Given the signs of these effects, shifting a private ride from peak to off-peak would bias our estimated magnitudes of the private ridership effect away from zero, while shifting a *shared* ride from peak to off-peak would bias estimated magnitudes of the shared ridership effect *towards* zero. In this light, we can think of our estimates as lower and upper bounds on the true effect magnitude of the rise in ridesharing and the drop in private ridership, respectively.

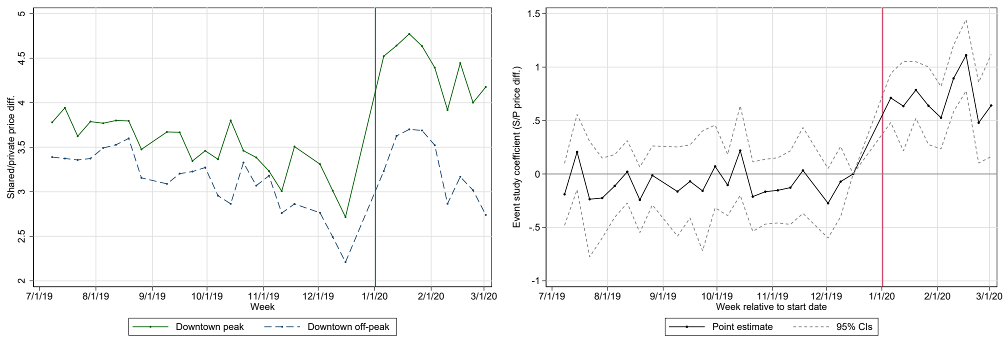
We view the graphs in Figures 2 and 3 collectively as justification of the peak/off-peak DD

[6]See the Appendix for further detail on this issue.

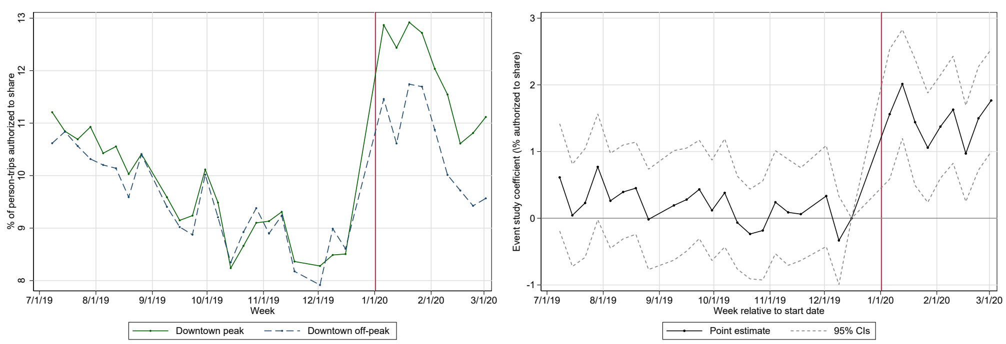
[7]We show versions of these graphs with narrower (30-minute instead of 60-minute) time-of-day windows in Appendix Figure A3; results look qualitatively similar to those of Figure 2.

[8]Appendix Figure A4 shows qualitatively indistinguishable patterns with holidays included in the sample. Appendix Figure A5, meanwhile presents peak and off-peak trends in percent authorized to share in the *neighborhoods* rather than downtown, as a falsification check (fees in the neighborhoods are uniform across across hours); no policy effect is visible.

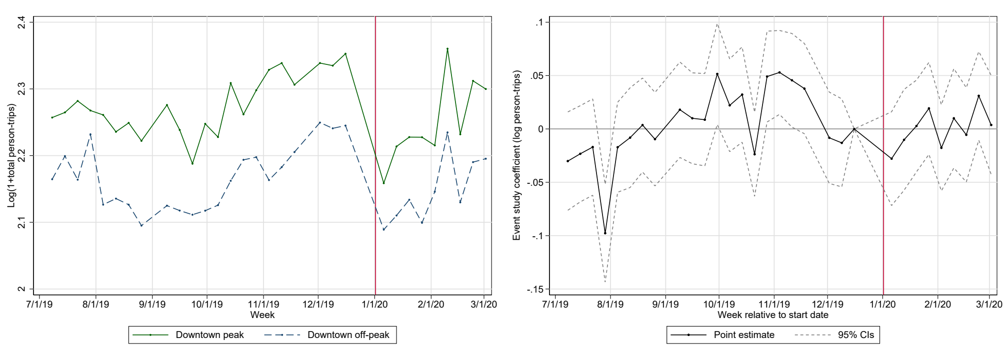
Figure 2: Peak vs. off-peak comparisons, boundary sample



(a) Private ride price premium



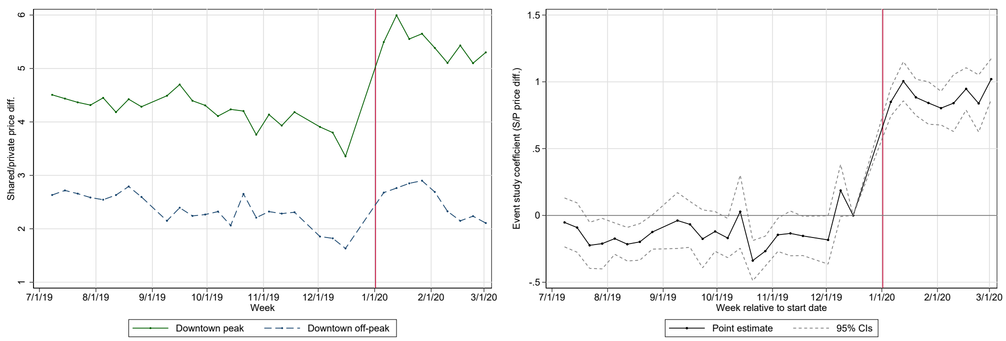
(b) Percentage authorized to share



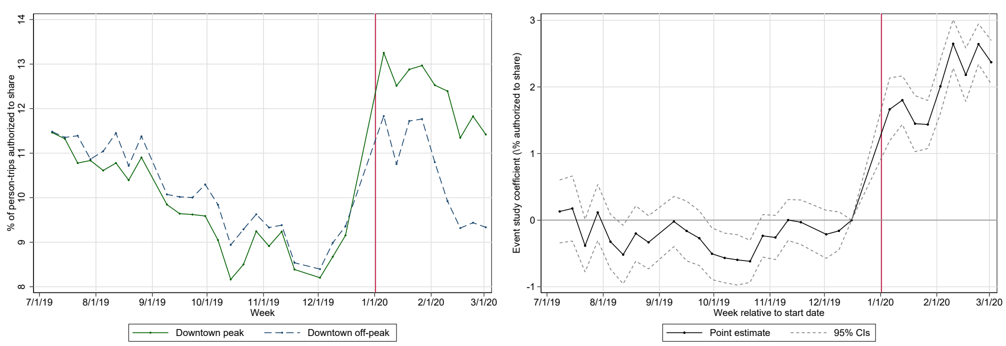
(c) Log(total person-trips)

Notes: "Boundary sample" refers to rides starting between 5:30-6:30AM or 9:30-10:30PM. In the left-hand side graphs, data points are week- and hour-range specific means of the outcome given in the panel title, taken across community area pairs. Means are weighted by trip count in Panels A and B. In the right-hand side graphs, data points are estimates from the event study model of Equation 1; week -3 relative to the policy is the omitted week (week -2 and -1 are holiday weeks dropped from the analysis sample).

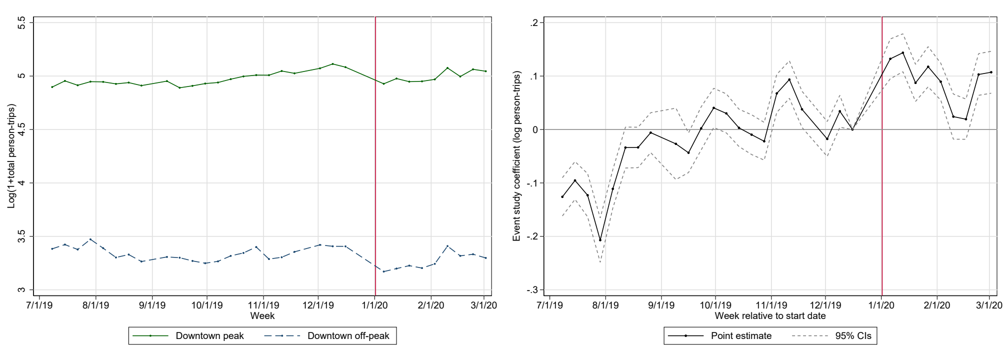
Figure 3: Peak vs. off-peak comparisons, all-hours sample



(a) Private ride price premium



(b) Percentage authorized to share



(c) Log(total person-trips)

Notes: In the left-hand side graphs, data points are week- and hour-range specific means of the outcome given in the panel title, taken across community area pairs. Means are weighted by trip count in Panels A and B. In the right-hand side graphs, data points are estimates from the event study model of Equation 1; week -3 relative to the policy is the omitted week (week -2 and -1 are holiday weeks dropped from the analysis sample).

strategy for impact evaluation, acknowledging non-parallel trends in total trip counts in the all-hours sample and the need for a pre-trend term in associated regressions. To obtain point estimates of DD policy impacts on price premiums and percent authorized to share (or successfully shared), we estimate the following DD model:

$$Y_{ith} = \alpha + \beta \text{Post}_t \times \text{Peak}_h + \theta_t + \phi_{ih} + \epsilon_{ith}. \tag{2}$$

$\text{Post}_t \times \text{Peak}_h$ is a binary variable equaling one if an observation is from a week after the policy change during peak hours. All else procedural is identical to what we describe above for Equation 1.

Expressing ridesharing impacts in price elasticity terms (rather than as an aggregate effect of policy change) facilitates consideration of impacts for other policies with different-magnitude price changes. We thus additionally estimate impact as an (absolute) elasticity of willingness (that is, percent authorized) to share with respect to the private-ride price premium, in percentage points per dollar. To do so, we use the DD policy change as an instrument for the private price premium in a Two-Stage Least Squares instrumental variables (IV) model:

$$P_{ith} = \gamma + \delta \text{Post}_t \times \text{Peak}_h + \theta_t + \phi_{ih} + \epsilon_{ith} \tag{3a}$$

$$\text{Shared}_{ith} = \alpha + \beta \hat{P}_{ith} + \theta_t + \phi_{ih} + \eta_{ith}. \tag{3b}$$

To investigate heterogeneity of policy impacts, we alter Equations 2-3b to additionally include interactions between the DD variable and binary (dummy) variables capturing four attributes: area income, trip length, transit stops, or airport presence. We create four income quartile dummies, where each dummy equals one if the lower of the two given CAs' median income falls in the corresponding quartile of the median income distribution. We use single dummies for above-median average trip length (in VMT) and for above-median transit stop counts at both CAs in a given pair. Lastly, we create a dummy for rides to or from a community area with a major airport in it. In all specifications with these interactions, we adjust our fixed effects to be estimated at the week by X level, where X is the vector of interacted attribute dummies.

We obtain DD policy impacts on person-trips and person-VMT with the following NB regression model:

$$\log Y_{ith} = \alpha + \beta \text{Post}_t \times \text{Peak}_h + \gamma \text{Week}_t \times \text{Peak}_h + \delta \text{Peak}_h + \theta_t + \phi_i + \epsilon_{ith}. \tag{4}$$

$\text{Week}_t \times \text{Peak}_h$ is a linear count of weeks since the start of the sample for peak-period observations, and zero otherwise. Y_{ith} is total, shared, or private person-trips or person-VMT. We separately employ CA-pair fixed effects and the binary variable Peak_h, because the maximum likelihood estimator does not converge when we use CA-pair hour-range fixed effects. In the Appendix, we show results of estimating Equation 4 with an analogous Poisson regression.

4 Results

4.1 Regression estimates

Table 2 displays estimated impacts on percent of trips shared and the price premium for taking a private ride, in both the temporal boundary sample and the full, all-hours sample. In the first three columns, β denotes the coefficient on the $\text{Post}_t \times \text{Peak}_{ih}$ binary variable, which in turn captures the \$1.15 additional cost premium introduced in the peak period relative to the off-peak period. We estimate that the policy change increased percent authorized to share by 1.3 percentage points in the boundary sample (Panel A) and 2.3 percentage points in the full sample (Panel B). Corresponding estimated impacts on percent successfully shared (column 2) are, logically, slightly smaller. These estimates are statistically significant at the one-percent level. We also find that the ridesharing effect was significantly larger among rides to and from the highest-income community areas (Panel D) – about 3.3 percentage points versus 2.2-2.5 when lower-income areas are involved.

Meanwhile, the price premium for a private ride (column 3) rose by 81 cents in the peak period relative to off-peak in the boundary sample (Panel A), with a corresponding estimate of \$1.05 in the full sample (Panel B). These estimates translate to pass-through rates (of the private-ride *cost* premium) of 70 percent and 91 percent, respectively. Income-interaction terms (Panel C) show no significant predictive effect of income quartile on the price premium. Column 4 puts together the effects in columns 1 and 3 by displaying results from IV estimation of the elasticity of ridesharing rate with respect to private-ride price premium. We find that this rate rises by 1.5 percentage points per dollar price premium in the boundary sample and 2.2 in the full sample.

The specification with income interactions suggests that the ridesharing elasticity is higher – on the order of 2.9 percentage points – among rides to and from the wealthiest areas of Chicago. Wealthier riders may be expected to have a higher value of time in general and thus a greater reticence to share a ride; but they may also use ridehailing services for more discretionary (for example, non work related) trips, such that their value of time savings and certainty while ridehailing is *lower*. Our finding of a higher elasticity in the wealthiest areas is consistent with the latter story.

Table 3 provides further evidence on heterogeneity in policy impacts along three dimensions: trip length; transit alternatives; and airport presence. We find that the DD effect on ridesharing rates is significantly larger among CA pairs with shorter than median (average) trip length – 2.3 percentage points versus 1.5 for longer trips (column 1) – and that the shared-private price difference changed very little for longer riders (column 3). There is no statistically distinguishable predictive effect of above-versus-below median public transit stops on any outcome (Panel B). Rides

Table 2: Impacts on ridesharing rates and price differences

	% authorized to share (OLS)	% successfully shared (OLS)	S/P price difference (OLS)	% authorized to share (IV)
	(1)	(2)	(3)	(4)
Panel A. Boundary sample				
β	1.293***	1.174***	0.813***	1.539***
	(0.159)	(0.131)	(0.066)	(0.222)
N	23,698	23,698	16,802	16,802
Panel B. All-hours sample				
β	2.283***	2.197***	1.050***	2.162***
	(0.102)	(0.098)	(0.054)	(0.126)
N	26,978	26,978	24,539	24,539
Panel C. All hours, across income				
β	3.324***	2.450***	1.156***	2.908***
	(0.562)	(0.309)	(0.102)	(0.352)
β_3	-1.019*	-0.363	-0.173	-0.573
	(0.586)	(0.382)	(0.158)	(0.447)
β_2	-1.161**	-0.307	-0.097	-0.880**
	(0.577)	(0.325)	(0.120)	(0.384)
β_1	-0.934	0.091	-0.047	-0.773*
	(0.606)	(0.367)	(0.134)	(0.447)
N	26,978	26,978	24,539	24,539

Notes: The dependent variable is listed above each column. In the first three columns, β is the coefficient on the DD binary variable "1[Post] X 1[Peak]" in Equation 2, estimated via OLS regression. In the fourth column, β is the coefficient on the "shared-private price difference" variable from estimation of Equations 3a and 3b via IV regression, where we use the DD variable as an instrument for the price difference. An observation is a unique origin-destination community-area (CA) pair i in week t during hour-range h (peak or off-peak). Panel A only includes weekday observations starting between 5:30-6:30AM and 9:30-10:30PM; Panel B extends to cover all weekday hours. Panel C uses all weekday hours and includes interactions with dummies for the bottom three quartiles of median income, corresponding to β_1-β_3. See Section 3 for further details. * indicates $p<0.1$; ** indicates $p<0.05$; *** indicates $p<0.01$.

to and from airports show muted DD effects (Panel C), consistent with the relatively smaller effect we find among longer rides as well as high values of time in airport travel.

Table 4 depicts results for person-trip counts and person-VMT, overall and broken out by shared and private ridership. Panel A provides estimates using the boundary sample, while Panel B relies on the full sample. Our column-1 estimates of impact on total person-trips have negative signs but are statistically indistinguishable from zero. Columns 2 and 3 provide estimates specific to shared rides and private ones, respectively. The coefficients from the shared-ride regressions are 0.103 log-points in the boundary sample and 0.139 in the full sample, both statistically significant at the one-percent level; these are interpretable as 10.8 and 14.9 percent increases, respectively. The coefficients from private-ride regressions are -0.024 and -0.034, indicating statistically significant decreases in private ridership of 2.4 and 3.5 percent, respectively. Corresponding estimates of impacts on total, shared, and private person-VMT (columns 4-6) are consistent with trip-count estimates except for insignificant estimated effects on private VMT. Appendix Table A1, which employs Poisson regression but is otherwise identical in method to Table 4, presents qualitatively similar results.

4.2 Interpretation

The insignificant effect of the DD cost premium on total person-trips and person-VMT is important because it suggests that estimates of the ridesharing response to rising price-premiums (column 4 of Table 2) are interpretable as elasticities of substitution. In other words, a $1.00 private price premium caused 2.16 percent of total person-trips to switch from private to shared in the full sample. This is a large proportional change in ridesharing rate – 22.3 percent – but is small in the broader context of urban auto travel. Multiplying our estimate of the DD impact on percent successfully shared (2.197; Panel B, column 2 of Table 2) by mean weekly number of rides in the peak period, post-policy (548,798) yields an estimated 12,057 extra rides shared per week due to the DD policy change. If we assume that half of each ride is physically shared with another rider, and using the peak period, post-policy average VMT per ride of 4.82 miles, we calculate an estimated 14,541 VMT avoided per week. This is 0.44 percent of average weekly, downtown ridehailing VMT in our data.[9]

Such a small percentage of ridehailing, which is in turn only three percent overall VMT in Chicago (Fehr & Peers, 2019), is unlikely to have had a measurable impact on congestion. We do not test this formally, because while we observe the speed of rides, it is likely that the congestion (that is, speed) elasticity with respect to ridesharing is larger in the peak period, which contains 6.5 times as many rides as the off-peak period. This would create an upward bias in our estimate of speed improvements. Nonetheless, we can infer from a comparison with existing research that

[9]If we assume that our estimated ridesharing effects apply uniformly to *all* time-series variation in downtown fees, this only raises our back-of-the-envelope estimated effect on ridehailing VMT to 0.77 percent.

Table 3: Impacts by trip length, transit stops, and airport presence

	% authorized to share (OLS)	% successfully shared (OLS)	S/P price difference (OLS)	% authorized to share (IV)
	(1)	(2)	(3)	(4)
Panel A. Average trip length				
β	2.325***	2.223***	1.112***	2.090***
	(0.107)	(0.103)	(0.036)	(0.103)
β_{long}	-0.812***	-0.588**	-0.901***	4.749
	(0.294)	(0.251)	(0.216)	(7.211)
N	26,978	26,978	24,539	24,539
Panel B. Number of public transit stops				
β	2.350***	2.177***	1.024***	2.291***
	(0.097)	(0.111)	(0.065)	(0.164)
β_{high}	0.018	0.212	0.158	-0.314
	(0.218)	(0.189)	(0.111)	(0.271)
N	26,978	26,978	24,539	24,539
Panel C. Presence of an airport				
β	2.364***	2.269***	1.104***	2.129***
	(0.106)	(0.105)	(0.035)	(0.104)
β_{air}	-1.241***	-1.070***	-0.958***	5.500
	(0.281)	(0.204)	(0.272)	(14.922)
N	26,978	26,978	24,539	24,539

Notes: The dependent variable is listed above each column. In the first three columns, β is the coefficient on the DD binary variable "1[Post] X 1[Peak]" in Equation 2, estimated via OLS regression. In the fourth column, β is the coefficient on the "shared-private price difference" variable from estimation of Equations 3a and 3b via IV regression, where we use the DD variable as an instrument for the price difference. An observation is a unique origin-destination community-area (CA) pair i in week t during hour-range h (peak or off-peak), in the all-hours sample. Regressions include interaction terms between the DD dummy and dummies for: above-median average trip length (β_{long}); above-median number of public transit stops (β_{high}); or the presence of an airport in the origin or destination CA (β_{air}). See Section 3 for further details. * indicates p<0.1; ** indicates p<0.05; *** indicates p<0.01.

Table 4: Impacts on person-trips and person-VMT

	All rides	Shared rides	Private rides	All VMT	Shared VMT	Private VMT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Just 5:30-6:30am and 9:30-10:30pm						
β	-0.003	0.103***	-0.024**	0.009	0.101**	-0.019
	(0.009)	(0.018)	(0.010)	(0.023)	(0.043)	(0.024)
N	28,086	28,086	28,086	28,086	28,086	28,086
Panel B. All hours						
β	-0.011	0.139***	-0.034***	0.005	0.130***	-0.019
	(0.012)	(0.018)	(0.013)	(0.019)	(0.031)	(0.022)
1[Peak] x linear trend	0.007***	0.005***	0.007***	0.007***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	28,086	28,086	28,086	28,086	28,086	28,086

Notes: All columns show estimates from an unconditional negative binomial regression, as described in Section 3.2. In columns 1-3, the dependent variable is a count of all person-trips, shared person-trips, and private person-trips, respectively. In columns 4-6, it is all VMT, shared VMT, and private VMT, respectively. β is the coefficient on the difference-in-differences binary variable $1[Post]x1[Peak]$. Panel A only includes observations within a half-hour of the weekday peak/off-peak boundaries (5:30-6:30AM and 9:30-10:30PM), while Panel B extends to include all weekday hours; an observation is a unique origin-destination community-area (CA) pair i in week t during hour-range h (peak or off-peak). See Section 3.2 for further details. * indicates p<0.1; ** indicates p<0.05; *** indicates p<0.01.

any effect of the Chicago policy on speeds is probably small. Tarduno (2021) estimates that the (temporary) *complete* exit of Lyft and Uber from Austin, Texas reduced minutes per mile there by approximately 0.1 (from a mean of 2.77), which he translates to a monetized benefit of $33-52 million annually. The magnitude of the effect on ridehailing VMT is much smaller in the Chicago policy context – on the order of one percent – which suggests a much smaller speed (and welfare) impact.

If the policy goal is to raise ridesharing rates, it is important to understand what keeps rates persistently low in our context. The data shed some light on the cost tradeoff inherent to the ridesharing decision. We can model the rider as choosing a ride to minimize cost, with the cost of a private ride C_p and a shared ride C_s given by

$$\begin{aligned} C_p &= t_p * \text{VOT} + P_p \\ C_s &= t_s * \text{VOT} + P_s + D, \end{aligned} \tag{5}$$

where t is the time duration of the ride, VOT is one's value of time (in \$/minute), P is the ride price, and D is the monetized disutility of sharing a ride (for example, due to sitting next to a stranger in a confined space). Then the condition for choosing a shared ride is

$$\text{VOT} < \frac{P_p - P_s + D}{t_s - t_p}. \tag{6}$$

In our data, the average private price premium (taken across CA pairs, weighted by trip counts) is \$4.00 in the peak period after the policy change, while the corresponding average private-ride time savings is 184 seconds. Plugging these numbers into Equation 6, we find that if there were no disutility of sharing a ride, riders with a VOT of less than \$63.35 would find it preferable to do so in this model.

The US Department of Transportation (1997) recommends using half the median or mean wage rate for person travel (and the full median or mean for business travel). Goldszmidt et al. (2020) estimate an average value of time of approximately \$19 per hour, or roughly 75 percent of the mean wage rate, in 13 US cities; in 2019 dollars, this translates to \$20.33. Thus it seems that most TNC riders would have VOT below \$63.35 at the average price/time-cost tradeoff and find it preferable to take the shared ride in the absence of ridesharing disutility. Yet ridesharing rates are only 10.4 percent in our downtown Chicago sample (Table 1). These numbers imply that the disutility of sharing a ride may be significant for many people. Inverting Equation 6, we find that a rider with a VOT of \$20.33 per hour would prefer a private ride if the dollar-value disutility of sharing is greater than \$2.96. If we instead model the disutility as a function of time in Equations 5 and 6, we find a threshold disutility value of 17 cents per minute, which is more than half the value of time itself.

External validity of these findings outside of the Chicago context is likely a function of several aspects of urban living. We highlight four relevant city attributes that may be relevant when considering whether our results are applicable in other US cities. The first is the magnitude of congestion; Chicago is ranked highest in this regard among all US cities (Inrix, 2022). The second is public transit ridership, an important alternative to ridehailing; Chicago is one of seven US cities with greater than 25 percent of commuters using public transit (according to the 2019 American Community Survey [ACS]; the others are New York City, San Francisco, Philadelphia, Seattle, Boston, and Washington, D.C.). The third is per capita income, a fundamental input to the value of time; we observe from the 2019 5-year ACS that Chicago has the tenth highest per capita income (\$37,103 per year) among the twenty most populous US cities. The fourth and final attribute is weather, which may affect the decision of whether or not to take a trip; Chicago experiences among the coldest winters in the country. Thus, Northern US cities with public transit systems and high congestion may be where our Chicago-based results are most applicable.

5 Conclusion

Increasing ridesharing rates would contribute, through reducing VMT and the external social costs that come with them, to climate change mitigation, improved air quality, and decreased urban congestion. We study the impacts of an early attempt by a major US city to raise ridesharing rates through price incentives. Chicago's Ground Transportation Tax is a congestion pricing policy that exclusively targets Transportation Network Companies (Uber, Lyft, and Via) and imposes larger fees for private rides than for shared ones. We find evidence that this policy raised ridesharing rates in the short run, primarily through substitution from private to shared ridership (rather than reductions in total ridehailing trips). The effect is large proportional to baseline rates, but it is small relative to what is needed to drive noticeable impacts on urban congestion. The data suggest that the disutility of sharing a ride with another stranger is a significant barrier to ridesharing takeup.

We believe these findings can inform policymaking for urban transportation and sustainability. Price incentives can indeed work to increase ridesharing, but perhaps modestly so at observed policy levels. Our estimate of the elasticity of substitution in Chicago may be useful in forecasting impacts of incentive schemes in other contexts, to the extent that the Chicago setting is externally valid. Our evidence of modest impacts may inform future policy design by suggesting the need for stronger incentives and targeted efforts to reduce the (actual as well as perceived) disutility of ridesharing. Finally, our findings highlight the need for transportation policies that span the whole of car travel in cities, since TNC-specific policies only directly affect small fractions of drivers and riders.

References

Abkarian, H., S. Hegde, and H. Mahmassani (2022): “Does Taxing TNC Trips Discourage Solo Riders and Increase the Demand for Ride Pooling? A Case Study of Chicago Using Interrupted Time Series and Bayesian Hierarchical Modeling.” *Transportation Research Record*.

Alonso-Gonzalez, M., O. Cats, N. vanOort, S. Hoogendoorn-Lanser, and S. Hoogendoorn (2021): “What are the determinants of the willingness to share rides in pooled on-demand services?” *Transportation*, 48, 1733–1765.

Barreto, Y., R. da Mota Silveira Neto, and L. Carazza (2021): “Uber and Traffic Safety: Evidence from Brazilian Cities.” *Journal of Urban Economics*, 123, 103347.

Barrios, J. M., Y. V. Hochberg, and H. Yi (2022): “The Cost of Convenience: Ridehailing and Traffic Fatalities.” *Journal of Operations Research*, 69, 823–855.

Boffa, F., A. Fedele, and A. Iozzi (2023): “Congestion and incentives in the age of driverless fleets.” *Journal of Urban Economics*, 137, 103591.

Brown, A. (2020): “Who and where rideshares? Rideshare travel and use in Los Angeles.” *Transportation Research Part A: Policy and Practice*, 136, 120–134.

Camilo Castillo, J. (2023): “Who Benefits from Surge Pricing?” Working Paper.

Chang, P., J. LaRiviere, and J. Thorpe (2021): “Time, Comfort, and Preferences for Shared Transit.” *Journal of the Association of Environmental and Resource Economists*, 8, 411–442.

Chicago Metropolitan Agency for Planning (2020): “Community Data Snapshots Raw Data 2020.” <https://datahub.cmap.illinois.gov/maps/2a0b0316dc2c4ecfa40a171c635503f8/about>.

Christensen, P. and A. Osman (2023): “The Demand for Mobility: Evidence from an Experiment with Uber Riders.” National Bureau of Economic Research Working Paper 31330.

City of Chicago (2019): “Transportation Network Providers and Congestion in the City of Chicago.” Tech. rep., Department of Business Affairs and Consumer Protection, <https://www.chicago.gov/content/dam/city/depts/bacp/Outreach and Education/MLL_10-18-19_PR-TNP_Congestion_Report.pdf>.

——— (2020): “Transportation Network Providers – Trips.” Tech. rep., Department of Business Affairs and Consumer Protection, <https://data.cityofchicago.org/Transportation/Transportation-Network-Providers-Trips/m6dm-c72p>.

——— (2023): "2023 Budget Forecast." <https://www.chicago.gov/content/dam/city/depts/obm/supp_info/2023Budget/2023-Chicago-Budget-Forecast.pdf>.

Cohen, P., R. Hahn, J. Hall, S. Levitt, and R. Metcalfe (2016): "Using Big Data to Estimate Consumer Surplus: The Case of Uber." National Bureau of Economic Research Working Paper 22627.

Davalos, J. (2023): "Lyft Will Discontinue Pooled Rides, Launch New Airport Feature," <https://www.bloomberg.com/news/articles/2023-05-11/lyft-will-discontinue-pooled-rides-roll-out-new-features?embedded-checkout=true>.

de Chaisemartin, C. and J. Ramirez-Cuellar (2022): "At What Level Should One Cluster Standard Errors in Paired and Small-Strata Experiments?" National Bureau of Economic Research Working Paper 27609.

Diao, M., H. Kong, and J. Zhao (2021): "Impacts of transportation network companies on urban mobility." *Nature Sustainability*, 4, 494–500.

Edlin, A. and P. Karaca-Mandic (2006): "The Accident Externality from Driving." *Journal of Political Economy*, 114, 931–955.

Fehr & Peers (2019): "Estimated TNC Share of VMT in Six US Metropolitan Regions (Revision 1)." <https://www.fehrandpeers.com/what-are-tncs-share-of-vmt/>.

Finkelstein, A. (2007): "The Aggregate Effects of Health Insurance: Evidence from the Introduction of Medicare," *Quarterly Journal of Economics*, 122, 1–37.

Fuller, S. and A. Brown (2021): "Transportation Network Company (TNC) taxes/fees by state/city in the United States." .

Goldszmidt, A., J. List, R. Metcalfe, I. Muir, V. Smith, and J. Wang (2020): "The Value of Time in the United States: Estimates from Nationwide Natural Field Experiments," National Bureau of Economic Research Working Paper 28208.

Hall, J., C. Palsson, , and J. Price (2018): "Is Uber a substitute or complement for public transit?" *Journal of Urban Economics*, 108, 36–50.

Hausman, C. (2014): "Corporate Incentives and Nuclear Safety," *American Economic Journal: Economic Policy*, 6, 178–206.

Hawkins, A. (2020): "Uber pledges to shift to '100 percent' electric vehicles by 2030," *The Verge*, <https://www.theverge.com/2020/9/8/21427196/uber-promise-100-percent-electric-vehicle-ev-2030>.

HOLLAND, S., E. MANSUR, N. MULLER, AND A. YATES (2016): "Are There Environmental Benefits from Driving Electric Vehicles? The Importance of Local Factors." *American Economic Review*, 106, 3700–3729.

HOU, Y., V. GARIKAPATI, D. WEIGL, A. HENAO, M. MONIOT, , AND J. SPERLING (2020): "Factors Influencing Willingness to Pool in Ride-Hailing Trips." *Journal of the Transportation Research Board*, 2674, 1–11.

INRIX (2022): "2022 Global Traffic Scorecard." Tech. rep., Inrix.

LEY, A. (2023): "How Might Congestion Pricing Actually Work in New York?" *The New York Times*, <https://www.nytimes.com/2023/06/28/nyregion/congestion-pricing-nyc.html>.

LIANG, Y., B. YU, X. ZHANG, Y. LU, AND L. YANG (2023): "The short-term impact of congestion taxes on ridesourcing demand and traffic congestion: Evidence from Chicago," *Transportation Research Part A*, 172, 103661.

LIU, T. (2023): "Didi's Revival Shows China Can't Live Without Big Tech," *WIRED*, <https://www.wired.com/story/didis-revival-shows-china-cant-live-without-big-tech/>.

LYFT, INC. (2023): "Our commitment to achieve 100% electric vehicles across the Lyft platform by 2030," `https://www.lyft.com/impact/electric`, accessed: 2023-10-16.

NATIONAL NEIGHBORHOOD DATA ARCHIVE (2020): "Public Transit Stops (2016-2018)." <https://nanda.isr.umich.edu/project/public-transit-stops/>.

NUNN, N. AND N. QIAN (2011): "The Potato's Contribution to Population and Urbanization: Evidence from a Historical Experiment," *Quarterly Journal of Economics*, 126, 593–650.

PARRY, I. AND K. SMALL (2005): "Does Britain or the U.S. Have the Right Gasoline Tax?" *American Economic Review*, 95, 1276–1289.

PARRY, I., M. WALLS, AND W. HARRINGTON (2007): "Automobile Externalities and Policies." *Journal of Economic Literature*, 45, 373–399.

SCHALLER, B. (2021): "Can sharing a ride make for less traffic? Evidence from Uber and Lyft and implications for cities." *Transport Policy*, 102, 1–10.

SUN, L. AND J. SHAPIRO (2022): "A Linear Panel Model with Heterogeneous Coefficients and Variation in Exposure," *Journal of Economic Perspectives*, 36, 193–204.

TAIEBAT, M., E. AMINI, AND M. XU (2022): "Sharing behavior in ride-hailing trips: A machine learning inference approach." *Transportation Research Part D*, 103, 103166.

Tarduno, M. (2021): "The congestion costs of Uber and Lyft." *Journal of Urban Economics*, 122, 103318.

——— (2023): "Should Cities Tax Uber and Lyft?" Mimeo.

Uber, Inc. (2023): "Use Uber in cities around the world," <https://www.uber.com/global/en/cities/>.

US Department of Transportation (2016): "The Value of Travel Time Savings: Departmental Guidance for Conducting Economic Evaluations Revision 2 (2016 Update)," <https://www.transportation.gov/sites/dot.gov/files/docs/2016%20Revised%20Value%20of%20Travel%20Time%20Guidance.pdf/>.

Vickrey, W. S. (1969): "Congestion Theory and Transport Invesment." *American Economic Association Papers and Proceedings*, 59, 251–260.

Zheng, Y., P. Meredith-Karam, A. Stewart, and H. Kong (2023): "Impacts of Congestion Pricing on Ride-hailing Ridership: Evidence from Chicago." *Transportation Research Part A*, 103639.

Online Appendix to "Getting from Ridehailing to Ride*sharing*: Effects of a Congestion Tax in Chicago" (Stolper and Taiebat)

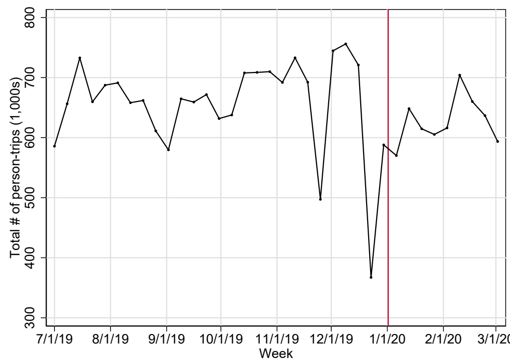
Data and methods details

Starting with 75.7 million raw ride records, we first remove approximately 370,000 rides with either a zero or unrecorded trip fare. 10.2 million rides are missing both census tract and community area for either the ride origin or destination. We drop these rides from the sample, as we cannot distinguish between a downtown ride and a neighborhood ride without such information. 277,473 rides start or end outside Chicago proper and thus are missing community area; we drop these as well, though our results are robust to their inclusion. We then remove illogical observations: 186,000 rides listed as successfully shared but not *requested* to share; 126,000 with speeds greater than 80 miles per hour or less than 0.5; and 1,700 with trip time greater than 5 hours or less than 30 seconds. This leaves us with 64.5 million rides. Limiting the sample further to weekdays in the downtown zone reduces this count to 23.4 million rides.

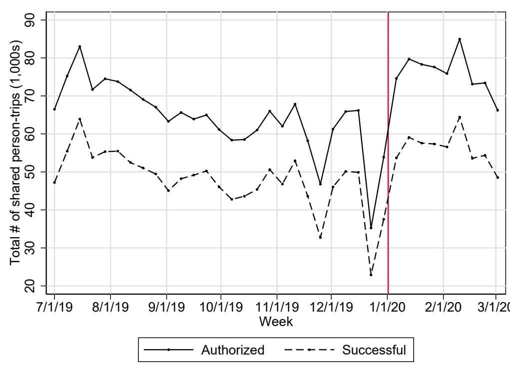
Assignment of these rides to 'downtown' and 'neighborhoods' as defined by the policy is complicated by the City of Chicago's data privacy procedures, which result in the masking of origin and destination census tracts for 14.8 million rides (23 percent). For any ride that is one of two or fewer unique trips involving a specific census tract and 15-minute time window, the City's privacy policy is to mask census tracts and only provide origin and destination community areas. All but one of Chicago's 77 community areas (CAs) are wholly contained by either the downtown zone or the neighborhoods zone, so we can assign zone to rides starting and ending in these 76 CAs regardless of missing census tracts. For rides starting or ending in the remaining CA, we assign zone based on census tracts and drop the (1.1 million) rides with a missing tract. Five census tracts are intersected by the downtown/neighborhoods boundary. We assign these census tracts to the neighborhoods zone, since that zone spans more than half the area of each of these tracts.

After collapsing rides to community-area pair, week, hour-range level observations, we run regressions to estimate policy impacts on several outcomes. For price and rate-of-ridesharing outcomes, we estimate impacts via Ordinary Least Squares Regression. For trip-count outcomes, we instead use unconditional negative binomial (NB) regression estimated via maximum likelihood (ML). Person-trips are a count variable with many zeros (15-36 percent, depending on the outcome). Our preferred specification for this type of outcome is the unconditional NB regression because, unlike Poisson regression (another common specification with count data), it does not assume that the conditional variance of the outcome is equal to the conditional mean. Due to the frequency of zeros, the variance in person-trips in our data is significantly higher than the mean – a hypothesis test estimating the "overdispersion" parameter α rejects a null of $\alpha = 0$ with high confidence in every one of our NB specifications – which threatens inference with Poisson through Type I error (Hausman, 2014). We thus use NB regressions for trip-count (and person VMT) outcomes in our main analysis, showing Poisson regression results in Appendix Table A1.

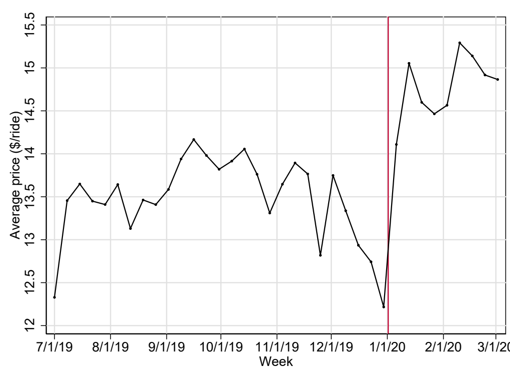
Figure A1: Overall time trends in rides, ridesharing, and prices



(a) Weekly total person-trips

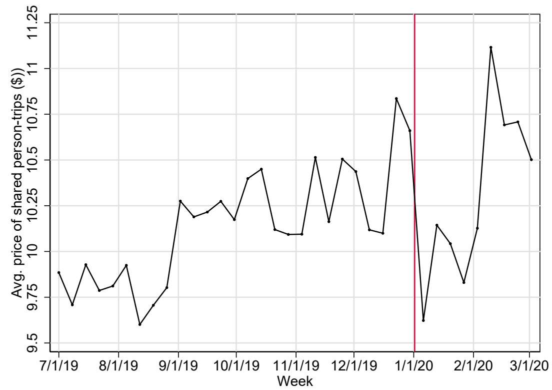


(b) Weekly total shared person-trips

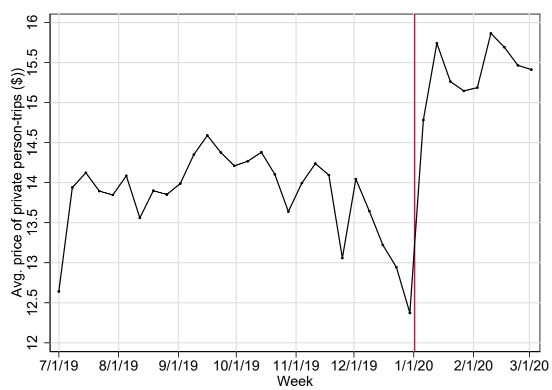


(c) Weekly average price

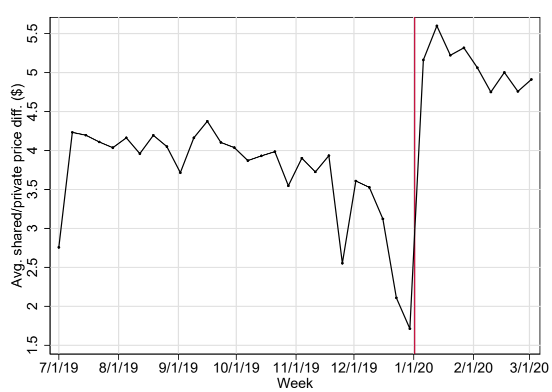
Figure A2: Shared and private price trends



(a) Weekly average shared price

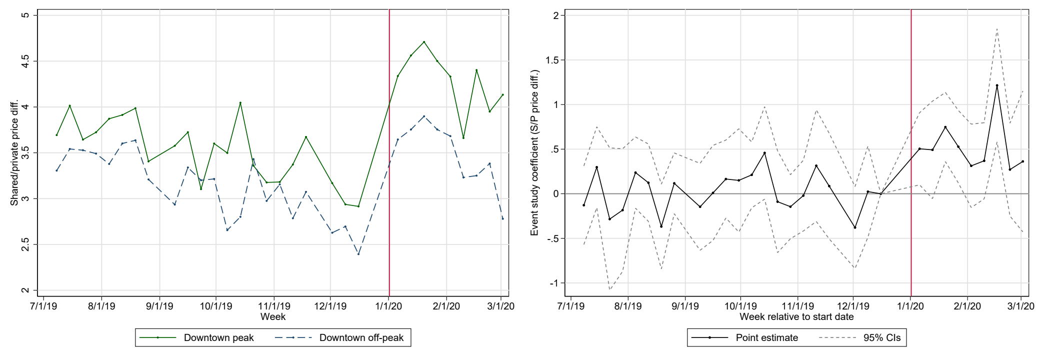


(b) Weekly average private price

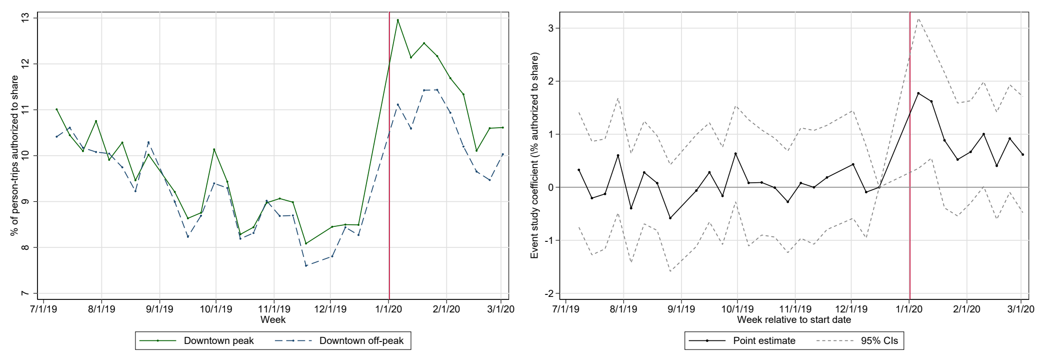


(c) Weekly average shared/private price differential

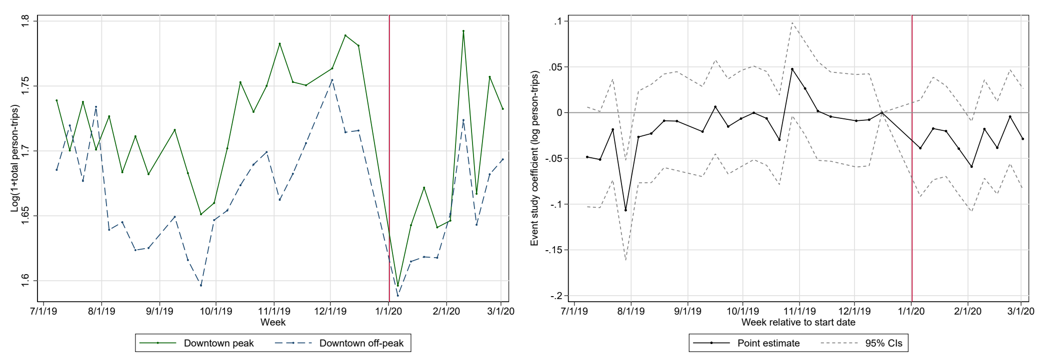
Figure A3: Peak vs. off-peak comparisons, 5:45-6:15AM and 9:45-10:15PM



(a) Private ride price premium



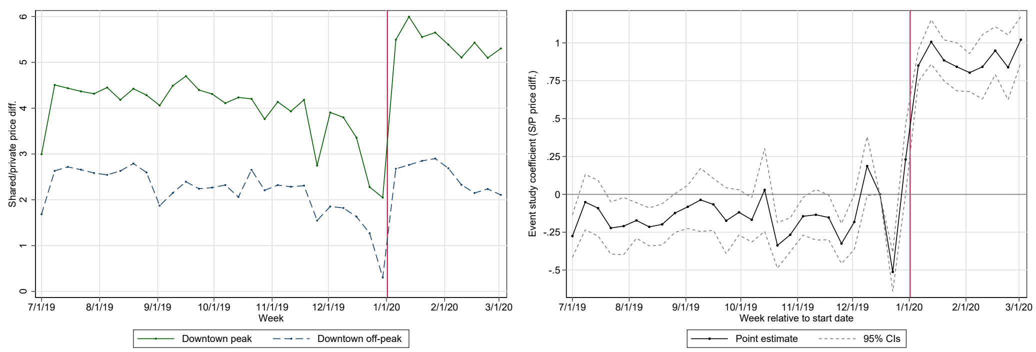
(b) Percentage authorized to share



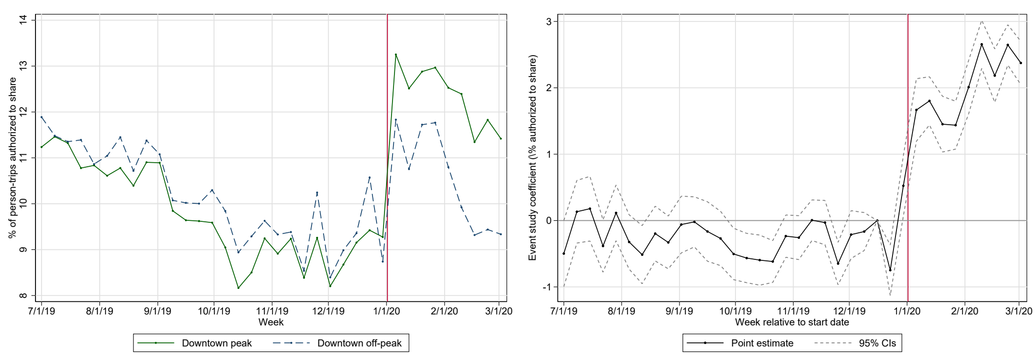
(c) Log(total person-trips)

Notes: In the left-hand side graphs, data points are week- and hour-range specific means or totals of the outcome given in the panel title. In the right-hand side graphs, data points are estimates from the event study model of Equation 1; week -3 relative to the policy is the omitted week (week -2 and -1 are holiday weeks dropped from the analysis sample).

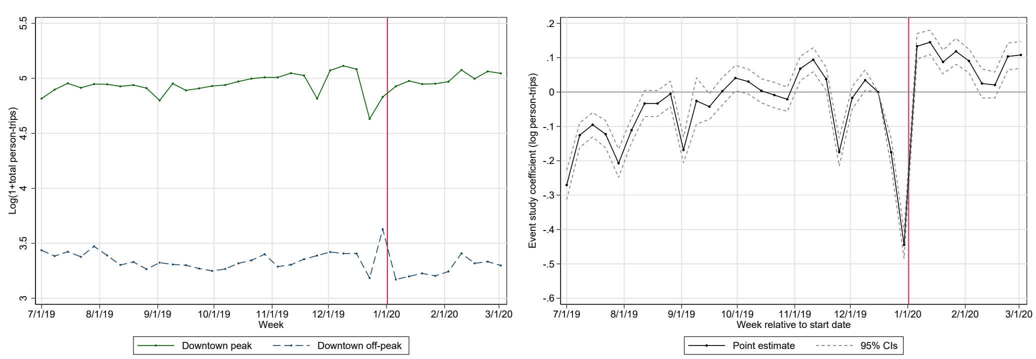
Figure A4: Peak vs. off-peak comparisons, all hours, including holidays



(a) Private ride price premium



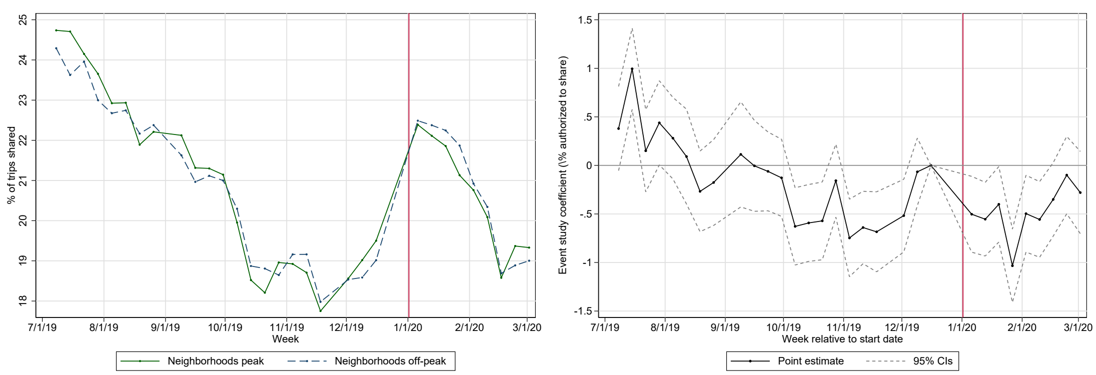
(b) Percentage authorized to share



(c) Log(total person-trips)

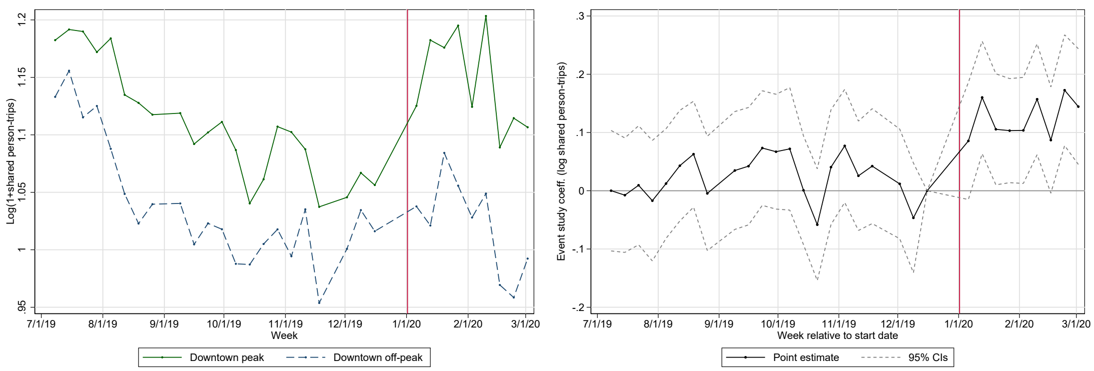
Notes: In the left-hand side graphs, data points are week- and hour-range specific means or totals of the outcome given in the panel title. In the right-hand side graphs, data points are estimates from the event study model of Equation 1; week -3 relative to the policy is the omitted week.

Figure A5: Peak vs. off-peak comparison of percent shared, neighborhoods only, all hours

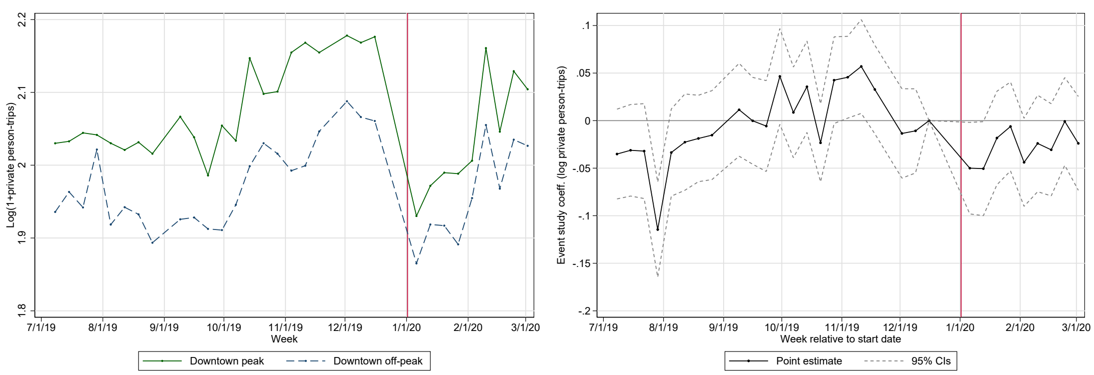


Notes: The sample includes neighborhoods rather than the downtown area. In the left graph, data points are week- and area-specific (peak or off-peak) percentages of person-trips authorized to be shared. In the right-hand side graphs, data points are estimates from the event study model of Equation 1; week -3 relative to the policy is the omitted week (week -2 and -1 are holiday weeks dropped from the analysis sample).

Figure A6: Peak vs. off-peak comparisons of shared and private trips, 5:30-6:30AM and 9:30-10:30PM



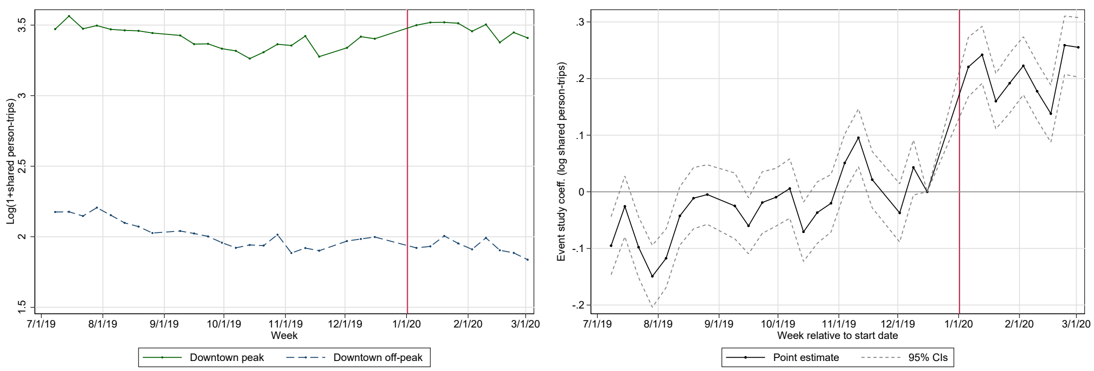
(a) Total shared person-trips



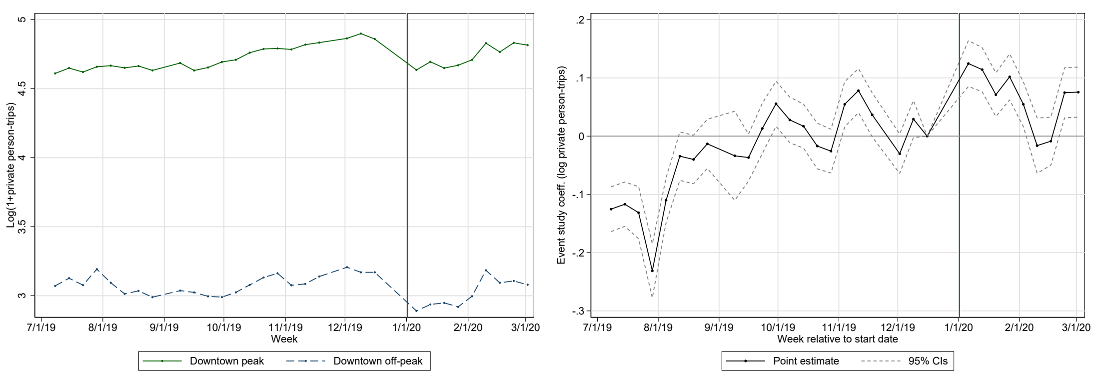
(b) Total private person-trips

Notes: In the left-hand side graphs, data points are week- and hour-range specific total shared (Panel A) or private (Panel B) person-trips. In the right-hand side graphs, data points are estimates from the event study model of Equation 1; week -3 relative to the policy is the omitted week (week -2 and -1 are holiday weeks dropped from the analysis sample).

Figure A7: Peak vs. off-peak comparisons of shared and private trips, all hours



(a) Total shared person-trips



(b) Total private person-trips

Notes: In the left-hand side graphs, data points are week- and hour-range specific total shared (Panel A) or private (Panel B) person-trips. In the right-hand side graphs, data points are estimates from the event study model of Equation 1; week -3 relative to the policy is the omitted week (week -2 and -1 are holiday weeks dropped from the analysis sample).

Table A1: Estimates of impacts on person-trip counts, Poisson regression

	All rides	Shared rides	Private rides	All VMT	Shared VMT	Private VMT
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Just 5:30-6:30am and 9:30-10:30pm						
β	0.009	0.114***	-0.004	0.010	0.126***	-0.011
	(0.006)	(0.016)	(0.007)	(0.009)	(0.021)	(0.011)
N	28,024	27,652	27,962	28,024	27,652	27,962
Panel B. All hours						
β	0.007	0.206***	-0.017*	0.025**	0.235***	-0.009
	(0.010)	(0.014)	(0.010)	(0.012)	(0.018)	(0.013)
1[Peak] x linear trend	0.005***	0.005***	0.005***	0.004***	0.005***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	28,086	28,086	28,086	28,086	28,086	28,086

Notes: All columns show estimates from a pseudo-Poisson regression, computed via maximum likelihood estimation, as described in Section 3.2. In columns 1-3, the dependent variable is a count of all person-trips, shared person-trips, and private person-trips, respectively. In columns 4-6, it is all VMT, shared VMT, and private VMT, respectively. β is the coefficient on the difference-in-differences binary variable $1[Post]x1[Peak]$. Panel A only includes observations within a half-hour of the weekday peak/off-peak boundaries (5:30-6:30AM and 9:30-10:30PM), while Panel B extends to include all weekday hours; an observation is a unique origin-destination community-area pair i in week t during hour-range h (peak or off-peak). All regressions include week and area-pair by hour-range fixed effects, and Panel B additionally includes a peak-period linear trend in week. See Section 3.2 for further details. * indicates p<0.1; ** indicates p<0.05; *** indicates p<0.01.