The Energy and Environmental Effects of New and Future Mobility: Econometric and Simulation Analysis of Ridesourcing Services Uber and Lyft

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Abstract

This thesis provides an initial understanding of the potentially fundamental changes to the way passenger vehicle transportation in the United States (U.S.) is changing given the introduction of ridesourcing via transportation network companies (TNCs), like Uber and Lyft, and the effects those changes have on energy and environmental outcomes. First, in a set of two complementary studies, I employ real-world data and econometric modeling to assess the impacts that TNCs have already had on U.S. states and urban areas. In the first study (Chapter 2), I focus on the state level, where relevant data are publicly available to estimate TNC market entry effects on vehicle registrations, gasoline use, vehicle miles traveled, and air pollutant emissions. I find an average decrease in vehicle registrations and no significant effect on other outcomes. In the second study (Chapter 3), I assess TNC effects on vehicle fleet composition (total registrations and fuel economy) and transit ridership at the urban area level and find evidence that TNC entry causes an average 0.7% increase in vehicle registrations and no average effect on overall fleet efficiency or transit ridership. The difference in state- and urban area-level effects on vehicle registrations is due, in part, to heterogeneity in the effects of TNC entry on different cities: I employ heterogeneous treatment effect, clustering, and regression interaction analysis and find significant heterogeneity across urban areas. TNC entry tends to increase vehicle ownership in urban areas with higher initial vehicle ownership and lower population growth rates, TNC entry tends to increase vehicle ownership, increase overall fleet efficiency more in urban areas with lower childless household rates, and increase transit ridership more in urban areas with lower average incomes and childless household rates. Where the first two studies look at aggregate past changes to the transportation system attributable to TNCs from the top down using observable indicators at the aggregate state and urban area levels, a third study in Chapter 4 considers a similar set of outcomes but focused at the vehicle level. I propose and apply a framework to quantify the external costs and benefits of TNC disruption to the transportation energy system by systematically characterizing the avoided cold start emissions and additional non-revenue miles and associated emissions and quantifying the relative size of external benefits and costs from TNC vehicles for several of the largest TNC markets in the U.S. and find that shifting travel from private vehicles to TNCs offers net external air pollutant benefits in some areas while incurring a net external cost in others; however, including externalities associated with additional vehicular travel yields net external costs everywhere. Taken together, these three studies confirm that TNCs have already affected the number and efficiency of vehicles owned and transit ridership rates in the U.S. and that they have done so heterogeneously as a function of pre-existing socioeconomic and passenger travel characteristics. And, at the individual TNC trip level, targeted sensitivity and policy analyses to illustrate how transportation and urban planning decisions can increase net external benefits and/or reduce negative external costs.

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Chapter 1. Introduction

In 2010, San Francisco-based start-up Uber launched a mobile device-based app that connected would-be passengers in need of a ride with would-be drivers and their respective vehicles, thereby introducing a new passenger transportation mode that enabled travelers to choose to source a ride in a private driver's vehicle real-time (i.e., on-demand ridesourcing). Uber was hailed as disruptive and introduced new flexibility and new uncertainty into a transportation sector previously marked by relatively consistent trends: the United States had 230 million light vehicles on the road (adding 2% more annually, on average, since 1970) that traveled 2.6 trillion miles (also increasing at 2% annually), consumed 125 billion gallons of gasoline (and increasing only 1% annually, with declines in growth rates over time attributable to national fuel economy standards), and emitted over 1 billion metric tons of carbon dioxide-equivalent (increasing at 0.5% annually since 1990 and also with declines in growth rates over time attributable to national fuel economy standards)[1]. Beyond Uber and other similar so-called transportation network companies (TNCs) disrupting the transportation market, these TNCs could fundamentally disrupt the relationship between personal travel and vehicle ownership, energy use, and emissions.

From 2009 to 2017, the number of passengers traveling in for-hire vehicles in the United States (U.S.) on any given day more than doubled[2]. Such growth is attributable not to a usage increase in conventional for-hire travel modes like taxis, but rather the advent and growth of new for-hire ridesourcing services like TNCs, of which the largest two examples in the U.S. are Uber and Lyft. Both are online platforms that use mobile-device applications to pair a passenger demanding a ride with a nearby driver to provide that ride at a transaction price determined in real time by the TNC. In absolute terms, the 0.5% of trips traveled via these travel modes is still relatively small, but the 10% of Americans—and as many as 30% in a survey of seven major metropolitan areas[3]—who now report having used a ridesourcing service (a new question in U.S. DOT's 2017 National Household Travel Survey) is notable for a travel mode that did not exist when the previous survey was issued in 2009.

Whether and how the emergence of ridesourcing services affects energy consumption and the environment is unclear *a priori*. Personal mobility is a complex system, with hundreds of millions of decision-makers in the United States alone, each assessing their own mobility needs and selecting how to travel (or not) as a function of various underlying factors, as Figure 1 depicts. Where an individual lives and works and the travel modes available to them are all observable considerations, but this system also includes difficult-to-measure subjective valuations, such as the value of travel time, an affinity or aversion to public transit, and/or a degree of dislike for traffic congestion. The introduction of ridesharing presents a novel opportunity to explore whether this new travel mode means those invisible travel preferences play out differently than in a world before Uber and Lyft in terms of outcomes that matter for energy and the environment, such as gasoline use, greenhouse gases (GHGs), and air pollutants.

Comparisons across component pieces of the personal transportation system offer clues as to why the introduction of ridesourcing could affect energy and environmental outcomes. For example, the energy use per passenger-mile of taxis (used here as a proxy for TNC vehicles, for which specific energy intensities have not been published) is estimated to be 40% higher than that of a traveler's own personal vehicle and 70% and 200% higher than transit rail or bus, respectively[4]. Accordingly, the novel opportunity to travel via TNC passenger vehicle that ridesourcing affords, then, could affect transportation energy use. This per-vehicle difference also plays out on a metropolitan scale: in New York, household vehicle ownership is 67% lower than in Los Angeles, and transportation energy consumption per capita is 25% lower, as Figure 2 shows.

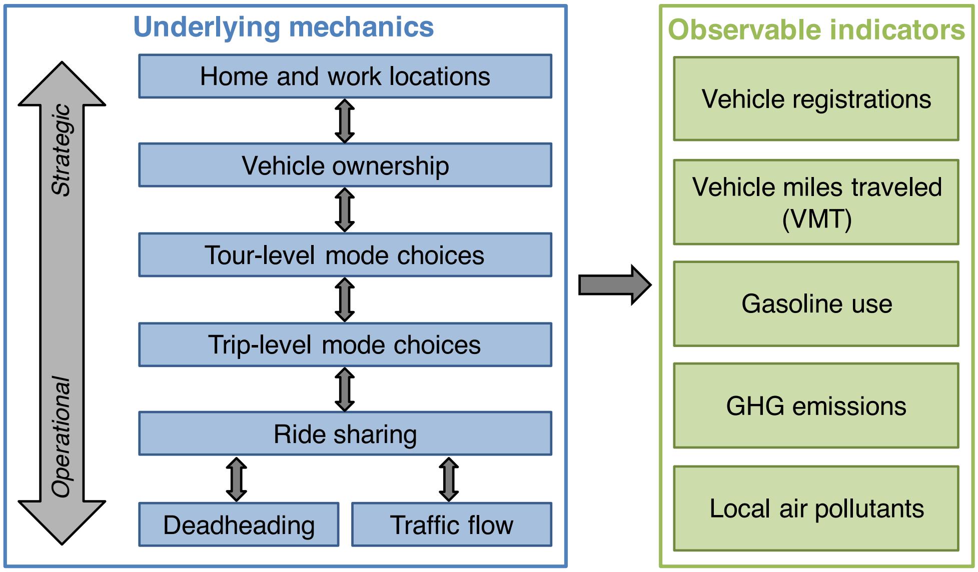

Figure 1 Notional schematic of the underlying mechanics and observable indicators of the personal mobility system.

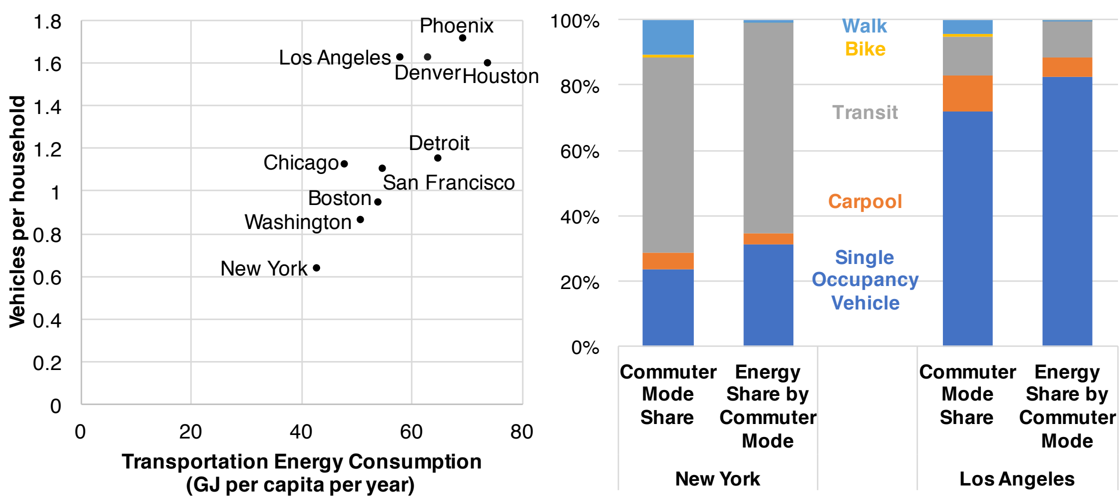

Figure 2 Transportation energy consumption as a function of vehicle per household for 10 major U.S. cities (left); data from U.S. Census and Newman and Kenworthy (2006)[5]. And a comparison of commuter transportation mode and energy share of those commuter transportation modes for New York and Los Angeles (right); data from U.S. Census and Banister (2009).

TNC effects on the underlying mechanics of the personal mobility system do not translate cleanly to observable indicators (as is depicted in Figure 1). What is known is that transportation is in transition: by 2017, 10% of Americans had used a travel mode that did not exist only years earlier. But, the impacts of ridesourcing are still unknown. Potential effects on petroleum consumption and GHG emissions are particularly important to understand, as transportation now contributes more to both than any other sector in the U.S.[1]

This thesis helps understand the potentially fundamental changes to the way Americans travel and the effects those changes have on energy and environmental outcomes. First, a set of two complementary studies employ real-world data and econometric modeling to study the impacts that Uber has already had on U.S. states and urban areas. In a first study, I focus on the state level, where relevant data are publicly available to estimate TNC market entry effects on vehicle registrations, gasoline use, vehicle miles traveled, and air pollutant emissions. The research questions addressed in Chapter 2 include:

- Does TNC entry increase or decrease vehicle ownership, personal travel, energy use, and emissions outcomes at the state level?
- Given that TNC use is primarily an urban phenomenon, does TNC entry have larger effects in more urbanized states?

In a second study, I aggregate ZIP code data to the urban area level to study TNC effects on vehicle fleet composition (vehicle registrations and average fuel economy) and transit ridership and heterogeneity in those effects across urban areas. The research questions addressed in Chapter 3 include:

- Does TNC entry increase or decrease vehicle ownership, fuel economy, and transit ridership outcomes at the urban area level?
- In what kinds of cities does Uber cause vehicle ownership and efficiency and transit ridership outcomes to increase vs. decrease?

Through these first two studies, I provide specific answers that address the current knowledge gap about TNC impacts as well as more general discussion of potential heterogeneity of those impacts across the U.S.

Where studies 1 and 2 looked at potential changes to the transportation system attributable to TNCs from the top down using observable indicators at the aggregate state and urban area levels that I can identify econometrically, a third study (Chapter 4) focuses at the vehicle level to consider a similar set of outcomes and the associated external costs of shifting passenger travel from a private to a TNC vehicle. While the relative strength of the first two studies was the ability to leverage real-world data, both in terms of dependent variables and model covariates, those studies could not look beyond net effects into the nuance of how TNC travel patterns potentially disrupt outcomes of interest. Cognizant of the potential external costs and benefits associated with TNC travel, this study assesses the tradeoffs between the monetized damages of additional deadhead miles (and associated gasoline usage and GHG emissions) and the monetized benefits of avoided damages that would otherwise result from passengers' vehicle cold starts. The research questions addressed in Chapter 4 include:

- How does shifting personal travel from private to TNC vehicles affect external costs (i.e., is there a net benefit or cost)?
    - Do the external benefits of avoided cold-start emissions from private vehicles outweigh the additional external costs of nonrevenue TNC vehicle miles between trips?
    - How does the additional consideration of travel externalities (i.e., congestion, crashes, and noise) affect net external costs?

- How can public policies (e.g. electric vehicle mandates) be most effective in minimizing external costs and/or maximizing external benefits?

Chapter 5 concludes with a summary of findings and overarching discussion across the three studies presented in the preceding chapters.

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Chapter 2. Effects of On-Demand Ridesourcing on Vehicle Ownership, Fuel Consumption, Vehicle Miles Traveled, and Emissions Per Capita in U.S. States

This study was co-authored with Jeremy Michalek, Inês Azevedo, Constantine Samaras, and Pedro Ferreira and published in Transportation Research Part C[1].

This chapter presents a first analysis of whether on-demand ridesourcing via TNCs increases or decreases vehicle ownership, personal travel, energy use, and emissions outcomes at the state level, as well as a consideration of whether those effects vary as a function of state urbanization. I estimate the effect of TNC market entry on per-capita vehicle ownership, energy use, travel distances, and emissions in U.S. states from 2005 to 2015 using a difference-in-difference propensity score-weighted regression model. I find evidence that TNC entry appears to cause a decline in state per-capita vehicle registrations by 3%, on average (95% confidence interval: 0.7% to 5.5%). My results regarding travel distances, gasoline consumption, and several air pollutants are not conclusive, but I find evidence of a relationship with some EPA-estimated vehicle air emissions. Such a change in air emissions would represent $300 million to $900 million in externalities during the analysis period; however, these air emissions are modeled, rather than measured, and uncertainty in the effects of TNC entry on travel distances and gasoline consumption could potentially swamp the air emissions effect.

2.1. Introduction

Transportation now contributes more carbon dioxide emissions than any other United States (U.S.) economic sector[2], and new personal transportation options are rapidly changing transportation. On-demand ridesourcing companies like Uber and Lyft, referred to as Transportation Network Companies (TNCs)[1], now provide on-demand mobility services that complement and compete with personal vehicle ownership and transit use, changing urban travel patterns and affecting energy and environmental implications of transportation. By 2015, Uber had entered 50% of U.S. combined statistical areas (CSAs, as shown in Figure 1). On an average weekday in late 2016, TNCs made more than 170,000 vehicle trips in San Francisco (15% of all intra-San Francisco vehicle trips)[3]. While prior literature has examined outcomes such as transit ridership, congestion, drunk driving, and local entrepreneurship (summarized in this chapter's Supplemental Information Section 1), the net impact of these services on vehicle ownership, energy consumption, travel distances, and emissions is either unexplored or still debated in the literature. Using scenario analysis, the U.S. Department of Energy's (DOE) national laboratories have suggested that energy use and emissions in a future combining shared mobility with connectivity and automation could be as much as 60% lower or 200% higher than a conventional mobility case[4]. In this wide range of possible future outcomes, a "lower-bound" scenario involves energy and emissions savings made possible by ridesharing, vehicle resizing and drivecycle smoothing; whereas, an "upper-bound" scenario involves higher energy consumption and emissions resulting from increased travel.

On-demand mobility is part of a larger ongoing transformation of shared mobility—a broader term used to describe a set of transportation modes where passengers travel using vehicles owned by another party on an as-needed basis. Transportation modes such as

[1] SAE J3163 defines ridesourcing as "prearranged and on-demand [are not allowed to street hail] transportation services for compensation in which drivers and passengers connect via digital applications". This category includes TNCs as well as some traditional services (e.g.: shuttle services).

carpooling, bike-sharing, and shuttle services have long fit into this category. Historically, vehicle travel and associated energy consumption have been relatively predictable: for example, since 2005, vehicle registrations and vehicle miles traveled (VMT) have increased by approximately 1% annually (except for declines during the recession from 2008–2011), non-public gasoline consumption has remained generally constant, and emissions of volatile organic compounds have declined 5% annually (EPA's Tier 2 emissions standards were phased-in from 2004–2009). More recently, car-sharing services have expanded customers' mobility options, introducing such options as renting a fleet-owned vehicle that is regularly available to other customers for either round-trip (e.g., Zipcar) or point-to-point (e.g., car2go) journeys. Furthermore, the growth and capabilities of smartphones enabled ridesourcing companies like Uber and Lyft to introduce on-demand mobility. Uber and Lyft launched in March 2010 and June 2012, respectively, in their first market: San Francisco, California. In 2017, Uber announced the completion of 10 billion total trips[5] and Lyft announced a billion total trips[6]. Finally, these services opened the door for dynamic ridesharing, where algorithms efficiently route on-demand mobility services to serve several customers with different destinations in the same physical vehicle.

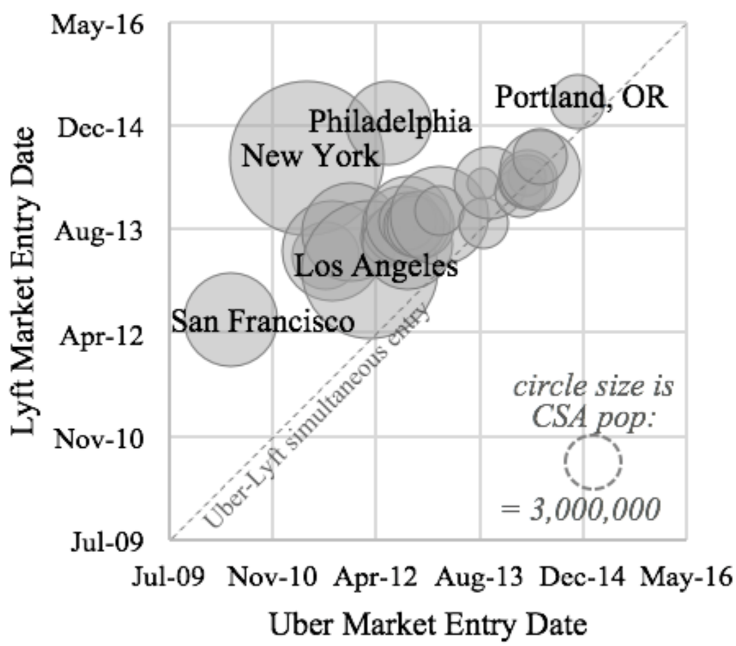


Figure 3. Comparison of Uber and Lyft market launch dates by combined statistical area (CSA). Some CSA labels are omitted for readability; data points include San Francisco, New York City, Seattle, Chicago, Washington (DC), Los Angeles, Philadelphia, San Diego, Atlanta, Boston, Dallas-Fort Worth, Denver, Minneapolis-St. Paul, Phoenix, Baltimore, Sacramento, Rhode Island (where Uber entered the entire state at once), Charlotte, Houston, Pittsburgh, Louisville, Cleveland, Tampa Bay, Miami, Orlando, St. Louis, and Portland (OR).

Despite rapid TNC growth in recent years, there is limited knowledge about on how they influence vehicle adoption, energy consumption, travel distances, and environmental outcomes. TNCs may reduce an individual's reliance on a personal vehicle, ultimately resulting in fewer vehicle registrations, or stimulate new vehicle purchases by TNC drivers, increasing registrations. TNCs may increase VMT by requiring vehicles to travel between passenger trips ("deadheading") and by increasing travel demand or shifting demand from mass transit to light-duty vehicles. But they may also reduce vehicle miles traveled (VMT) through ride pooling, by providing a "first/last-mile" solution that encourages partial use of public transportation, or by providing travelers with the option to pay per trip as an alternative to making a "lumpy" investment in a personal vehicle and observing lower marginal costs of travel. TNCs might increase or decrease energy consumption and emissions by changing VMT, by shifting VMT to vehicles with different efficiency and emissions rates, and by changing the portion of VMT traveled at hot operating temperature, when vehicles are more efficient and have lower emission rates.

2.2. Prior Literature

Peer-reviewed studies of the effects of TNCs on vehicle ownership, travel, energy, and environmental outcomes are limited: Rayle et al. (2016) found that while 33% of surveyed TNC users in San Francisco would have traveled via bus or rail if the TNC service were not available, "ridesourcing probably did not influence car ownership behavior"[7]. Hall et al. (2018) use a difference-in-difference econometric model in 147 U.S. metropolitan areas and conclude that, while transit ridership does not change immediately after Uber entry, transit ridership increases by five percent two years after Uber entry, on average, and that this heterogeneous effect is larger in big cities with small transit agencies[8]. They also find that Uber entry decreases commute times for transit users while increasing vehicular congestion. There are no peer-reviewed journal publications of TNC effects on energy or emissions, to my knowledge.

In the grey literature, some working studies and internal reports have suggested that TNCs have affected vehicle ownership, use, and emissions, but the estimated effects vary. Both Hampshire et al. (2017)[9] and Clewlow and Mishra (2018)[10] use survey methods to infer a reduction in overall vehicle ownership attributable to Uber and Lyft: Hampshire et al. surveyed former users of Uber after Uber left Austin, TX in 2016 and found a 9% increase in reported vehicle ownership among those former Uber users, and Clewlow and Mishra report that 9% of survey respondents who use ride-hailing across a group of 7 U.S. metropolitan areas disposed of one or more household vehicles. In contrast, Schaller (2018)[11] and Gong et al. (2017)[12] find that Uber is associated with an increase in vehicle ownership: Schaller observes that while TNCs were operating in the nine largest U.S. metropolitan areas from 2012–2016, growth in vehicle ownership outpaced that of population, and Gong et al. apply a difference-in-difference regression model in China and estimate an 8% increase in new vehicle registrations associated with Uber entry.

Vehicular travel effect estimates from working studies and internal reports have also varied (the two peer-reviewed studies mentioned earlier found different and even heterogeneous effects). Li et al. (2016)[13] find that TNCs are associated with reductions in some travel metrics: they use a difference-in-difference regression to estimate a 1.2% decline in overall congestion and associated travel times and fuel consumption. But other studies suggest an increase: Clewlow and Mishra (2018) suggest, based on survey responses from ride-hailing users across a group of 7 U.S. metropolitan areas, that 49% to 61% of ride-hailing trips are associated with an increase in VMT; Hampshire et al. (2017) find a 23% reduction in the likelihood to take a trip among former Uber users surveyed in Austin, TX that transitioned to a personal vehicle after Uber and Lyft left; and Schaller (2018) finds, based on a comparison of eight surveys from other working studies, that 60% of ride-hailing trips would have otherwise happened via transit, walking, or biking (or not have happened at all) in a group of nine U.S. metropolitan areas.

TNC services can have effects not only on the number of vehicles registered, but also on how those vehicles are used. Recent analysis suggests that less than 60% of miles traveled by a TNC vehicle are productive miles spent moving a passenger from an origin to a destination—the remaining 40% of TNC vehicle empty-mile travel is spent cruising in search of the next fare, driving to passenger pick-up, or driving after passenger drop-off[14]. Additionally, the travel demand that is shifted to vehicles from other modes (i.e., from walking, biking, and transit) due to the convenience of on-demand ridesharing services was estimated to be as high as 85% in Denver, CO[14], though Hall (2018) concludes that Uber is more of a complement to transit. Despite potential increases in the number of trips and the total number of miles travelled to complete each trip, evidence also suggests that chaining trips in the same set of vehicles can reduce criteria air pollutant emissions[15].

In summary, literature of the effects of TNCs on vehicle ownership, travel, energy, and environmental outcomes is inconclusive, and there are few peer-reviewed studies. I contribute to this literature by exploiting the staggered entry timing of Uber and Lyft across U.S. cities in seeking to identify causal relationships between TNC entry and my outcomes of interest.

2.3. Methods

I use difference-in-difference (DiD) models to estimate effects of the intervention (i.e. TNC entry) by comparing the trends of treated and untreated groups before and after the intervention occurs. DiD methods have been used previously to evaluate the effect of TNCs on other outcomes, including traffic congestion[13], vehicle-related homicides[16], entrepreneurial activity[17], and new vehicle ownership in China[12].

2.3.1. Difference-in-Difference Model

Our regression model is informed by models used in prior literature for my outcomes of interest. Regression analysis is conducted using inverse probability of treatment weighting (described below) and the following baseline specification:

$$y_{st} = \boldsymbol{\beta}^\top \mathbf{x}_{st} + \boldsymbol{\alpha}^\top \mathbf{z}_{st} + \gamma_s + \delta_t + \varepsilon_{st} \tag{1}$$

where y_{st} is one of the four dependent variables of interest for state s and year t: 1) vehicle registrations per capita; 2) VMT per capita; 3) gasoline use per capita, or 4) one of the per capita passenger vehicle emissions estimates (CO, NH_3, NO_x, PM_{10}, $PM_{2.5}$, SO_2, and VOCs); $\mathbf{x}_{st}$ is the vector of treatment effects (i.e., an Uber indicator)[2] for state s and year t with coefficients $\boldsymbol{\beta}$; $\mathbf{z}_{st}$ is a vector of controls for state s and year t, including population, urban population percentage, gas price, real personal income, unemployment, Section 177 status, and largest city population, density, and GDP, with corresponding coefficients $\boldsymbol{\alpha}$; γ_s is the dummy for state s; δ_t is the dummy for year t; and ε_{st} is unobserved error. Several additional statistical and diagnostic tests support the use of this linear model in my case (summarized in SI Section 4). The precision of my estimates is calculated using bootstrapped standard errors.

Two variations on this baseline incorporate different measures of shared mobility—(1) an average effect estimated using a dummy variable that indicates whether Uber has launched in a

[2] In my base model, this vector is of length one, but my model with interactions includes multiple terms.

state in a given year; and (2) an interaction model that estimates TNC entry effect for urban, middle, and rural states using an interaction between TNC treatment and a categorical variable indicating whether a state is urban (used as the baseline), middle, or rural.

The estimates of a difference-in-difference model are unbiased if the behavior of the untreated after the intervention mimics that of the treated had they not been treated. A common way to build confidence about this assumption is to check that, on average, trends over time were identical before the intervention took place (the "parallel trends" assumption), between the treated and the control groups. A potential concern regarding bias arises if the treated and non-treated groups (e.g. states) are systematically different in other ways that might explain differences in trends among those groups. This is the case here, where, by the end of the analysis period, TNCs have entered all but five states (Alaska, Montana, South Dakota, West Virginia, Wyoming), and these five states are not comparable to the other 45 where they did enter. To control for this potential bias, I apply inverse probability of treatment weights in a weighted least-squares model. This model compares post-treatment trends in treated states with weighted trends in non-treated states, probabilistically weighted to resemble the treated states along attribute dimensions that are correlated with treatment (state population, income, gasoline price, emissions standards, and largest city population, density, and GDP). I confirm that weighting yields control and treatment groups that are statistically comparable (i.e., no significant differences in covariates between groups, as shown in Figure 2), and I use an event study to confirm that the resulting weighted model does not violate the parallel trends assumption (see SI Section 4).

2.3.2. Propensity Score

I estimate propensity scores using gradient boosting[18], which combines machine learning, wherein the algorithm iterates over a series of decision trees, and optimization— where, via gradient descent, each subsequent decision tree is fitted to the residuals from the previously fitted tree—to estimate the probability of treatment for each observation. Previous studies have shown gradient boosting is superior to simple logistic regression models for propensity score estimation[19]. Here, I employ gradient boosting to approximate the logistic model:

$$\log\left(\frac{p_{st}(\mathbf{z}_{st})}{1-p_{st}(\mathbf{z}_{st})}\right) = \sum_m f_m(\mathbf{z}_{st}) + \epsilon_{st} \qquad (2)$$

where p_{st} is the probability of treatment for state s and year t; $\mathbf{z}_{st}$ is a vector of covariates for state s and year t, including population, urban population percentage, gas price, real personal income, unemployment, Section 177 status, and largest city population, density, and GDP; and ϵ_{st} is unobserved error. I estimate the additive function f_m using gradient boosting, given the treatment and covariate data, and compute estimated probability of treatment $\hat{p}_{st}$ for each state and year. The gradient boosting model predicts that a given state is more likely treated than not in a given year when a state is actually treated in that year with 92% accuracy (i.e., the model estimates a state is either less than 50% likely to be treated in years prior to Uber entry or greater than 50% likely after). The resulting estimates for probability of treatment are then used in a weighted regression for equation (1).

2.3.3. Robustness

I apply a battery of statistical tests to check model assumptions and test for robustness that are shown in the supplementary information (Sections 4 and 5). Model assumptions are informed by generalized additive models (GAMs) for dependent variable function form (SI Section 2), and final model fit is checked using visual inspection of residual errors (SI Section 4) to confirm no structural error. Additionally, for each model, I subject my results to three robustness checks:

(1) I conduct randomized treatment tests to ensure that the effects I estimate are unique to the particular observed pattern of treatments, rather than a result of the structure of the model. Model-estimated effects which fall in the tails (>95%) of the distribution of randomized treatment-estimated effects are considered robust;
(2) I conduct leave-one-out tests to ensure that my estimates do not hinge on the data of any one state. Model-estimated effects that are still estimated as significant when systematically leaving each state out are considered robust; and
(3) I conduct leave-multiple-out tests to ensure that my estimates do not hinge on outliers. I consider robust estimated effects that do not change in magnitude (i.e., 95% confidence intervals still overlap) or significance level.

I also test sensitivity of my model findings to a variety of sensitivity cases including alternative model specifications, alternative encoding, and unweighted models. I discuss these further in the Results section.

2.4. Data

I describe and identify data sources for dependent variables, treatment, and control variables in turn:

2.4.1. Dependent Variables

- *Vehicle registrations (measured):* I use vehicle registration data for each state and for each year for light-duty passenger vehicles from Ward's Automotive[20]. Ward's data are based on data published in U.S. DOT's State Statistical Abstracts and Highway Statistics Series[21,22], which is the set of official vehicle registration data published by state DOTs.
- *Gasoline consumption (measured):* DOT's State Statistical Abstracts and Highway Statistics Series also report Federal Highway Administration estimates of annual private and commercial vehicle state level on-highway motor fuel based on reports of aggregate motor fuel sales from state motor fuel tax agencies.
- *VMT (estimated):* VMT data comes from DOT's State Statistical Abstracts, which are tracked and reported annually as a function of figures reported by state agencies. State agencies estimate aggregate VMT based on vehicle count data measured on representative roadways and distributions of roadway type within the state (while DOT issues a Traffic Monitoring Guide, individual state methods may differ). VMT (table VM-2) has been published in DOT's State Statistical Abstract series since 2008; earlier data are available in DOT's Highway Statistics Series. Interpretation of statistical inference based on these VMT data is constrained by the representativeness of the underlying VMT estimation (rather than direct measurement) methods. Potential systematic differences in estimation methods by state or in differences in Uber and Lyft driving patterns compared to where states measure (i.e., primarily on the highway) could affect the signal in these data and, in turn, my model and results.
- *Emissions (estimated):* State-level emissions data are published annually in the EPA's State Average Emissions Trend report, which is informed by EPA's National Emission Inventory, which, in turn, relies on EPA's Motor Vehicle Emission Simulator (MOVES)

model. The MOVES model estimates vehicular emissions based on vehicle population and fleet characteristics, vehicle speed distributions, and relative hour- and day-type VMT distributions at the county level and aggregated. Emissions attributable to highway vehicles are estimated by the EPA annually[23]: 2008, 2011, and 2014 estimates were developed in conjunction with the National Emissions Inventory for those years; 2005, 2007, 2009 and 2010 estimates were updated using additional MOVES modeling; and 2006, 2012, and 2013 were interpolated. EIA estimates an annual series of State Carbon Dioxide Emissions based on energy consumption data contained in the State Energy Data System (SEDS). Transportation sector estimates are published without highway or light-duty vehicle detail after an approximately 2-year lag[24]. Interpretation of statistical inference based on these emissions data is limited to factors considered as part of emissions estimation modeling (rather than direct measurement). Potential changes to EPA models used could affect the emissions estimated just as changes to input assumptions to those models (such as vehicle registrations) could propagate through to model output. And, the three interpolated values could impose a linear structure unrepresentative of the actual data in those years.

I divide each of the four quantities above by state population each year to compute per-capita values. Annual state-level population estimates are from DOT's State Statistical Abstract and Highway Statistics series and, as such, they align with VMT data and are related to Ward's Automotive vehicle registration data (the ultimate source for which is also these DOT publications). DOT population reports match U.S. Census statistics in census years and are no more than 0.6% different than Census Bureau's annual estimates of the resident population in intercensal years[25], which the Census calculates assuming geometric interpolation with some exceptions[26].

2.4.2. Treatment Variables

- *Uber and Lyft entry dates:* I adopt data from previous sources that aggregated and published a time-series of Uber market entry dates. A 2014 Forbes article first aggregated Uber launch dates from 2010–2014[27] by service area, as originally announced on Uber's official blog (on a post no longer available) and/or in local media from each new service area. Forbes continued to update that dataset to reflect additional Uber markets launched through December 2015. Those dates are cross-referenced against Uber market launch date data that were independently gathered and published in two later studies[13,27,28]. Burtch et al. include a table of market launch dates for UberX—Uber's lower-cost, on-demand service provided in the driver's personal vehicle, which the authors compiled directly from the Uber Blog[17]. Lyft market launch dates were requested from and provided by Lyft[29]. A comparison of Uber and Lyft market launch date time-series is depicted by combined statistical area in Figure 1. Because Lyft market entry years are the same or later than Uber market entry years in all cases, I use Uber entry dates in my analysis to represent on-demand mobility availability in the state.

2.4.3. Control Variables

- *Population served by Uber in each state:* TNCs are introduced in specific areas or cities within a state, and the portion of a state's population with access can change over time as TNCs enter new cities. These time-series are derived for this analysis by summing within each state the population of each city where Uber is present (estimates of which are reported annually by the U.S. Census American Fact Finder) and dividing by the total state population, as published by the U.S. Department of Transportation's (DOT) annual State Statistical Abstracts[21,22]. Annual (intercensal) estimates for city populations are published in the U.S. Census' American FactFinder[25].

- *Control variables:* Our control variables include: (i) population, reported annually in DOT's State Statistical Abstract and Highway Statistics series, (ii) percentage of a state's population that is urbanized[30], (iii) state average real personal income, reported annually by the Bureau of Economic Analysis[31]; (iv) state unemployment, report by the Bureau of Labor Statistics[32]; (v) state average gasoline price data, reported annually by the U.S. Energy Information Administration[24], and (vi) an indicator for whether each state has adopted California's more stringent vehicle emissions control requirements, pursuant to Section 177 of the Clean Air Act[33]. Additionally, recognizing that TNC market entry and use is primarily a city phenomenon, additional control variables are included for the largest city within each state, including: (vii) population[34], (viii) population density, and (ix) GDP[35]. While these control variables are meant to yield unbiased estimates of Uber's effect on dependent variables of interest, the possibility of omitted variable bias cannot be overlooked. Sensitivities were conducted using several additional potentially relevant independent variables (number of licensed drivers, Lyft market entry, transit ridership, and Uber/Lyft leasing incentive programs), none of which greatly affected the magnitude or the significance of effects reported as significant and robust (see SI Section 3 for details).

While these control variables are intended to help reduce bias, the possibility of omitted variable bias cannot be overlooked. Sensitivity analyses were conducted using several additional potentially relevant independent variables (number of licensed drivers, Lyft market entry, transit ridership, and Uber/Lyft leasing incentive programs), as well as two variations on Uber treatment encoding; none greatly affected the magnitude or the significance of effects reported as significant and robust.

Variable encoding and summary statistics for each data source above are shown in Table 1. On average, population steadily increases, criteria pollutant emissions steadily decrease, and vehicle registrations and income generally increase, except for a dip in 2009–2010 corresponding to the Great Recession. Gasoline price is volatile and non-monotonic.

Table 1. Variable encoding descriptions and associated summary statistics (U.S. totals, except where averages are shown, as noted) for 2005, 2010, and 2015. Monetary values are reported in current dollars (as indicated).

Variable	Unit	Description	2005	2010	2015
(For variables below, values shown are U.S. totals)					
Population	million persons	Population	296	309	321
Light-Duty Vehicles	million vehicles	Light duty vehicles	234	232	241
Gasoline Use	billion gallons	Gasoline taxed by states as used by non-public, non-exempt vehicles	133	131	130
VMT	trillion miles	Vehicle miles traveled	2.99	2.97	3.10
CO	million tons	Highway carbon monoxide emissions	42.4	28.3	19.7
NH3	million tons	Highway ammonia emissions	0.14	0.12	0.10
NOx	million tons	Highway nitrous oxides emissions	8.30	5.70	4.12
PM10	million tons	Highway particulate matter emissions	0.38	0.28	0.30
PM2.5	million tons	Highway particulate matter emissions	0.31	0.20	0.15
SO2	million tons	Highway sulfur dioxide emissions	0.17	0.04	0.02
VOC	million tons	Highway carbon monoxide emissions	3.41	2.77	1.97
Income	trillion $ (current $)	Real personal income	10.6	12.5	15.5
s177	binary	A state's Section 177 status (whether it has adopted California's more stringer mobile-source emissions regulations)	5	11	13
Katrina	binary	Indicator for potential vehicle hurricane damage (2005 only)	1	0	0
Sandy	binary	Indicator for potential vehicle storm damage (2012 only)	0	0	0
Clunkers	Number of vehicles scrapped	Number of participants in "Cash for Clunkers" vehicle scrappage program (2009 only)	0	0	0
Treat	% states	Uber indicator, binary	0%	2%	90%
(For variables below, values shown are averages across states)					
Gas Price	$/gal (current $)	Average gasoline price	2.08	2.63	2.34
Pop_u	% pop, state avg.	% of state population that is considered Ubran by the Census (coded relativel to the average % urbanization for 2005-2015, which is 74%)	73%	74%	75%
Citypop	thousand persons	Population of center city in a state's largest metropolitan statistical area	652	703	714
Citydensity	persons per square mile	Population density of center city in a state's largest metropolitan statistical area	4120	4483	4539
CityGDP	billion $ (current $)	GDP of state's largest metropolitan statistical area	127	140	177
Treatpop	% pop, state avg.	Uber indicator, weighed by % of state population with Uber access	0%	0%	21%

2.5. Results

Table 2 summarizes results for the effect of TNC entry on state per capita vehicle registrations, gasoline consumption, and vehicle miles traveled. In all cases assessed, I show results for two different models with different representations of TNC service availability in a state: (1) an average effect based on whether Uber is present in a state in a given year; and (2) an interaction effect between Uber presence and state urbanization level (urban, middle, or rural). For each estimate reported, I provide the 95% confidence interval (using bootstrapped standard errors) in parentheses[3,36,37]. The model suggests that, on average, introducing shared mobility (in any portion of a state) decreases per-capita vehicle registrations by 3.1% (95% confidence interval: 0.7% to 5.5%) over the period examined (relative to per-capita registration had the TNC not been introduced). These findings pass my robustness checks (Table 4). The interaction model finds effects in urban, middle and rural states that span the -3.1% effect found in the base model, but differences across urban, medium, and rural states are not statistically significant. These findings are robust when subjected to my robustness tests, summarized in Table 4 (details reported in SI Sections 4 and 5). Estimated effects for gasoline consumption and VMT are not statistically significant (except the effect on VMT is estimated to be more negative in rural states than in urban states at the p=0.01 level that does not pass my robustness checks).

Table 2. Regression models for per-capita vehicle registrations, per-capita gasoline use, and per-capita VMT using three measures of shared mobility: (1) binary ("Average Effect") and (2) interaction ("Urban", "Middle", and "Rural") effects. "Middle Effect" and "Rural Effect" are computed post-hoc as the sum of the "Urban Effect" and either the "Middle-vs.-Urban" or the "Rural-vs.-Urban Effect", respectively, taking care to calculate standard errors with respect to appropriate model-estimated variances and covariances. Coefficients estimated for control variables are excluded from the table for brevity. See SI Section 3 for complete results.

	Dependent variable: log, per capita		
	Veh. Reg.	Gas. Use	VMT
Model (1)			
Average Effect	−0.031**	0.003	−0.003
	(0.010)	(0.004)	(0.003)
Observations	550	550	550
Deg. Freedom	474	474	474
Adjusted R-Sq.	0.865	0.985	0.988
Model (2)			
Urban Effect	−0.028	−0.001	0.003
	(0.018)	(0.002)	(0.004)
Middle-vs.-Urban	−0.001	0.009	−0.009*
	(0.019)	(0.006)	(0.005)
Rural-vs.-Urban	0.005	0.009	−0.019***
	(0.021)	(0.007)	(0.006)
Middle Effect†	−0.038***	0.008	−0.006
	(0.012)	(0.006)	(0.004)
Rural Effect†	−0.023**	0.008	−0.016***
	(0.010)	(0.007)	(0.005)
Observations	550	550	550
Deg. Freedom	472	472	472
Adjusted R-Sq.	0.864	0.985	0.988

Note: *p<0.1; **p<0.05; ***p<0.01
†computed post-hoc and not directly estimated

Table 3 summarizes my findings for the effect of TNC entry on estimated per-capita emissions of greenhouse gas emissions (GHGs), as reported by the Energy Information

[3] While cluster-robust standard errors are commonly employed (per Cameron and Miller, 2015), when the number of clusters is small (i.e., I only have 50 states here), bootstrapping can be preferred (per Camron et al., 2007).

Administration (EIA)[24], and of the seven air pollutants (CO, NH_3, NO_x, PM_{10}, $PM_{2.5}$, SO_2, and VOCs) reported in the U.S. Environmental Protection Agency's (EPA) State Emissions Inventory (EPA, 2016). I find that TNC entry is associated with an estimated decline of volatile organic compounds of 4.8% (1.5% to 8.2%), with a larger effect in rural states: 8.2% (4.0% to 12.3%). These effects pass my robustness checks. Some regression models yield significant results for CO, NH_3, PM_{10}, $PM_{2.5}$, and SO_2 emissions; however, these results fail my robustness checks (Table 4, see SI Sections 4 and 5 for details). Results for other emissions are not statistically significant. I view these estimated effects as exploratory, since these emissions are estimated using models, rather than measured, and there are several years where emissions estimates are interpolated. Additionally, one of the inputs used by the EPA to model estimated highway vehicle emissions is vehicle registrations, so it is possible that changes in vehicle registrations propagate through EPA's moves model and show up here for VOCs as an echo. Nevertheless, identification of a potential relationship with VOCs suggests value in further study of TNC implications for emissions and air quality, potentially by examining effects of pollutant concentrations at air quality monitor sites using higher resolution data.

Because I test for significance across multiple dependent variables, I employ a Benjamini-Hochberg correction for testing multiple hypotheses and find my per-capita vehicle registration and VOC emissions results are robust to false detection rates as low as 4.6% (see SI Section 3 for the detailed analysis). Considering only my primary dependent variables (vehicle registrations, gasoline consumption, and VMT average effects) and excluding my exploratory analyses of air emissions, my vehicle registration results are robust to false detection rates as low as 2.6%.

2.5.1. Robustness

I subject my results to a variety of checks including a set of robustness checks, sensitivity analysis, unweighted and alternatively weighted regressions, and an event study. I discuss each in turn.

The battery of robustness checks and sensitivity analyses that I apply support my findings. Both the estimated vehicle registration and VOC emission effects at the state level are robust (or "near-robust", as slightly crossing the threshold for the level of significance of the vehicle registration or VOC emissions effect estimates is sensitive to whether Ohio or Indiana, respectively, are included in the sample) to randomized treatment, leave-one-out, and leave-multiple-out checks (all described previously in the Methods section), as is summarized in the SI.

In addition to my primary robustness checks, summarized in Table 4, I also test several variations of my model, including (a) normalizing my dependent variable per licensed driver, (b) normalizing my dependent variable per urban population, (c) excluding entry data during the Great Recession of 2007-2010, (d) shortening the time frame of analysis to 2009-2015, (e) including control variable indicators for the presence of Uber leasing programs, (f) including Lyft entry years (which match or lag behind Uber entry years) as additional treatment variables, (g) including additional controls for transit ridership, (h) coding Uber treatment using lagged variables, (i) coding Uber entry using a June-July cutoff between years, rather than the default December-January cutoff, and (j) introducing linear time trends for each state. All of these model variations produce results consistent with my base model: statistically significant effects of comparable sign and magnitude with the exception of the linear time trends model, which produces similar point estimates at lower statistical significance.

Table 3. Summary of TNC indicator coefficients from the results of the set of regression model specifications discussed—(1) binary and (2) interaction—using the logarithm of per-capita emissions of each of seven criteria pollutants (CO, NH_3, NO_x, PM_{10}, $PM_{2.5}$, SO_2, and VOCs) as the dependent variable. "Middle Effect" and "Rural Effect" are the sum of the "Urban Effect" and either the "Middle-vs.-Urban" or the "Rural-vs.-Urban Effect", respectively, taking care to calculate standard errors with respect to appropriate model-estimated variances and covariances. Coefficients estimated for control variables (state population, urban population percentage, income, unemployment, gasoline price, emissions standards, and largest city population, density, and GDP, as well as indicators for Hurricane Katrina, Cash for Clunkers, and Superstorm Sandy and fixed effects for state and time) are excluded from the table for brevity. See SI Section 3 for complete results.

	Dependent variable: log emissions per capita							
	VOC	NO_x	CO	NH_3	PM_{10}	$PM_{2.5}$	SO_2	GHG
Model (1)								
Average Effect	−0.048***	−0.018	−0.032*	−0.027***	0.029	−0.002	0.031*	0.007
	(0.017)	(0.013)	(0.018)	(0.01)	(0.020)	(0.016)	(0.018)	(0.008)
Observations	550	550	550	550	550	550	550	550
Deg. Freedom	474	474	474	474	474	474	474	474
Adjusted R-Sq.	0.963	0.971	0.964	0.945	0.903	0.943	0.932	0.983
Model (2)								
Urban Effect	−0.037*	0.007	−0.012	−0.027**	0.091***	0.043*	0.022	0.005
	(0.021)	(0.018)	(0.022)	(0.012)	(0.034)	(0.023)	(0.026)	(0.009)
Middle-vs.-Urban	−0.011	−0.036	−0.025	0.008	−0.136***	−0.099***	0.010	0.002
	(0.021)	(0.024)	(0.021)	(0.011)	(0.039)	(0.031)	(0.028)	(0.010)
Rural-vs.-Urban	−0.044*	−0.079***	−0.070***	−0.018	−0.106***	−0.075**	0.034	0.005
	(0.023)	(0.025)	(0.023)	(0.014)	(0.040)	(0.031)	(0.035)	(0.012)
Middle Effect†	−0.048**	−0.029*	−0.037*	−0.019*	−0.045**	−0.056**	0.032	0.007
	(0.021)	(0.018)	(0.022)	(0.011)	(0.021)	(0.022)	(0.020)	(0.010)
Rural Effect†	−0.082***	−0.072***	−0.082***	−0.045***	−0.014	−0.032	0.056*	0.011
	(0.021)	(0.019)	(0.021)	(0.012)	(0.021)	(0.022)	(0.029)	(0.011)
Observations	550	550	550	550	550	550	550	550
Deg. Freedom	472	472	472	472	472	472	472	472
Adjusted R-Sq.	0.963	0.972	0.965	0.945	0.908	0.944	0.932	0.983

Note: *p<0.1; **p<0.05; ***p<0.01
†computed post-hoc and not directly estimated

In addition, I ran a standard OLS regression without weights. This regression resulted in a similar coefficient for registrations and VOCs as the base model with statistical significance reduced to p=0.08 (see SI Table S6 and S7). This result suggests that the effects I estimate are not artifacts of IPTW weighting, thought the level of significance is higher with the weighted OLS. Because OLS conflates treatment with other state attributes that may influence my dependent variables, I view the IPTW results as more trustworthy. Weighting is intended to avoid potential bias in a causal treatment effect estimate that could stem from systematic differences between the treated and non-treated states. E.g. by the end of the analysis period, TNCs have entered all but five states (Alaska, Montana, South Dakota, West Virginia, Wyoming), and these five states are not comparable to the other 45 where they did enter.

Table 4. Summary of subjecting treatment effect estimation to three robustness checks and ten sensitivities, as described in the table notes below. A half-filled circle denotes "near-robust" estimates, such as those for which the estimate is greater in magnitude than 90–95% of

randomized treatment estimates and/or where leave-one-out robustness hinges on the inclusion of just one particular state in the data set. An empty circle denotes estimates that are not robust. Estimates that are reported as statistically significant and robust are highlighted (the “Lag” model reduces the number of observations and thus statistical significance, so I do not consider a lack of statistically significant results in that model variant as a problematic robustness outcome).

Effect Estimate	Coefficient	Robustness			Sensitivities									
		(1) RT	(2) LOO	(3) LMO	(a) DL	(b) UP	(c) GR	(d) TF	(e) UL	(f) LE	(g) TR	(h) Lag	(i) Enc	(j) LTT
Vehicle Registrations														
Average	-3.1% **	●	●	●	●	●	●	◒	●	●	●	○	●	●
Urban	-2.8%	◒	◒	●	●	●	●	●	●	●	●	○	●	●
Middle	-3.8% ***	●	●	●	●	●	●	◒	●	●	●	○	●	○
Rural	-2.3% **	●	●	●	●	●	◒	●	●	●	●	●	●	○
Gasoline Usage														
Average	0.3%	○	○	○	○	○	○	○	○	○	○	●	○	○
Urban	-0.1%	○	○	○	○	○	○	○	○	○	○	●	○	○
Middle	0.9%	○	○	○	○	○	○	○	○	○	○	●	○	●
Rural	0.9%	○	○	○	○	○	○	○	○	○	○	○	○	○
VMT														
Average	-0.3%	○	○	○	○	○	○	○	○	○	○	○	○	◒
Urban	0.3%	○	○	○	○	○	○	○	○	○	○	○	○	○
Middle	-0.6%	○	○	○	○	○	○	○	○	○	○	○	○	○
Rural	-1.6% ***	○	○	○	●	●	●	●	●	●	●	○	○	○
VOC														
Average	-4.8% ***	●	●	●	●	●	●	●	●	●	●	○	●	●
Urban	-3.7% *	◒	◒	○	●	●	○	○	●	●	○	○	○	●
Middle	-4.8% **	●	○	○	●	●	●	●	●	●	●	○	○	●
Rural	-8.2% ***	●	●	●	●	●	●	●	●	●	●	●	●	○

Robustness checks: (1) RT: Randomized Treatment; **(2) LOO:** Leave-one-out; **(3) LMO:** Leave-multiple-out; **Sensitivities: (a) DL:** dependent variable per licensed driver; **(b) UP:** dependent variable per urban population; **(c) GR:** excluding Uber entry during Great Recession, 2007-2010; **(d) TF:** shortening analysis period to 2009-2015; **(e) UL:** indicators for locations and years with Uber-incentivized leasing; **(f) LE:** indicators for Lyft entry in a state and year; **(g) TR:** transit ridership control variable; **(h) Lag:** one- and two-year lags for Uber treatment **(i) Enc:** alternative annualization between June and July; **(j) LTT:** adding linear time trends by state**; Robustness indicators: ● robust, ◒ “near-robust”, ○ not robust**

Figure 4 compares the treatment and control states before and after weighting along a set of parameters used to calculate propensity scores. For variables other than unemployment, which is already not significantly different between the treatment and even the unweighted control group, weighting is shown to reduce mean differences between the treatment and unweighted control group parameters by 70% to 100%. The differences between treated and untreated states are statistically significant when unweighted, but, as desired, become not statistically significant in the weighted sample (even at the p=0.10 level).

I also posed a model using alternative IPTW weights that include an indicator for whether Uber had entered the state in a prior year. Results for this specification suggest that treatment is not significant for registrations in the base model but is significant in the model with interactions (see Table S10). Because this alternative weighting scheme did not generate balance satisfying my target threshold (i.e., statistically significant differences remain between covariate means of the treatment and weighted control groups; see Figure S11), I consider my base case results to be more trustworthy.

I also ran an alternative model specification where the treatment variable was specified as my estimate of the portion of the state's population with access to TNC services each year, rather than a binary variable (see SI Table S4 and S5). I do not find any statistically significant effects with the population-weighted model that passed my robustness checks (though, I note that lack of a statistically significant effect is not conclusive evidence that no effect exists). I use the binary treatment model as my base model because I are concerned that my inability to identify TNC service areas over time as they grow in each urban center and my need to approximate those service areas in the population-weighted model could lead to systematic biases across time and location that could influence results artificially as well as random errors that reduce statistical significance. Because of this potential issue, I do not use the population-weighted model as a hard robustness check but, rather, as an additional piece of information that suggests a need for more data and more research accounting for service areas.

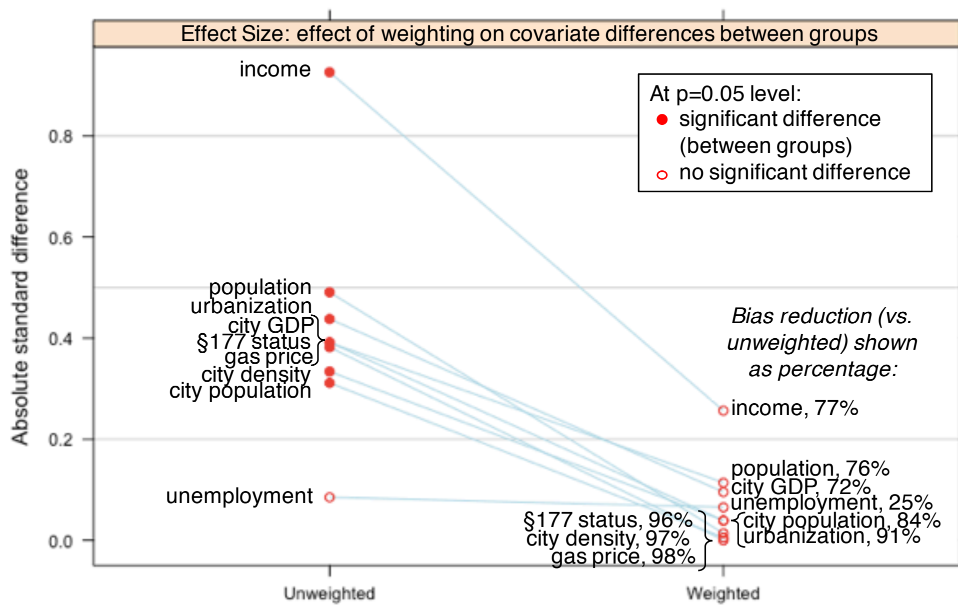


Figure 4. Effect size plot comparing the treatment states and control states before and after weighting. Closed red circles indicate a statistically significant difference before weighting; open circles reflect no significant difference after weighting.

Finally, I conducted an event study, summarized in Figure 5, and find no statistically significant prior to TNC entry and a statistically significant negative effect following TNC entry, consistent with my difference-in-difference results. These findings provide support for a causal interpretation of my difference-in-difference results and suggests that the effects I identify can also be observed without assuming parallel trends.

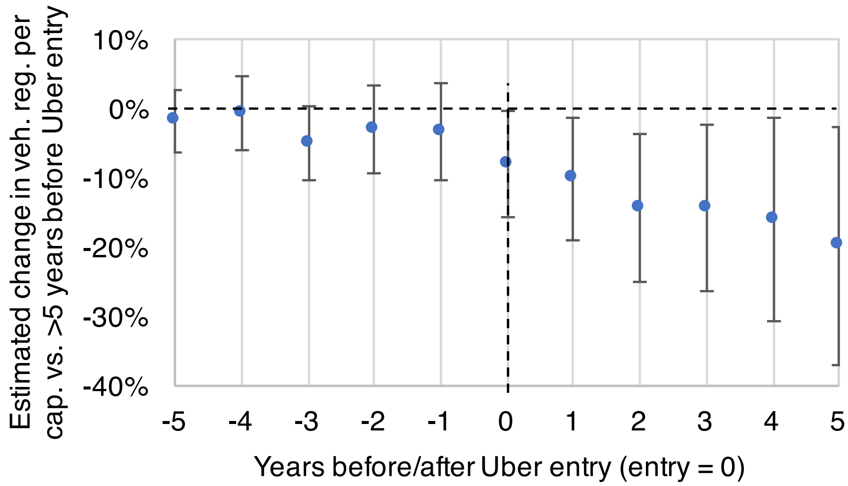


Figure 5. Event study showing the estimated change in vehicle registrations per capita from five years prior to Uber entry to five years after Uber entry. These estimates are generated using the same IPTW model as is described in the main text with the addition of a suite of indicator variables to capture the number of years before/after Uber market entry for each observation. The trend above suggests that no significant decline in per-capita vehicle registrations occurs until the year of Uber entry and is significantly lower (compared to the period more than five years prior to Uber entry) after entry.

In summary, I view my base case results as the most trustworthy, and their findings satisfy many robustness checks and alternative model specifications. Several model variations that did not produce comparable and statistically significant results have features that make them inferior to my base model. Nevertheless, they point to a need for additional research, potentially using higher-resolution data. Our event study results provide support for a causal interpretation, and suggest robustness to relaxing the parallel trends assumption.

2.6. Discussion

Our results, summarized in Figure 6, suggest that access to TNC services appears to cause a reduction in per-capita vehicle registrations in U.S. states. Interpreting this effect as causal relies on two key assumptions: 1) trends in outcomes of interest would have been parallel across treatment and control states in the absence of Uber entry, and 2) treatment (Uber entry) was exogeneous. For parallel trends, the patterns of vehicle registrations across states are noisy (SI Section 2), though trends appear somewhat more parallel when aggregated by treatment cohort, and event study results provide additional support without assuming parallel trends. For treatment exogeneity, Uber and Lyft described entry decisions as more opportunistic than strategic, in that decisions to enter were not made in anticipation of changes in vehicle registrations or other outcomes. Rather, entry decisions were informed by information such as Google searches for “Uber” and “Lyft” in cities that did not yet have TNCs. Further, my event study finds no statistically significant effects until after TNC entry, suggesting that changes in vehicle registrations did not precede entry. The balance in observed state attributes after applying IPTW suggests successful de-conflation of treatment with attributes of treated and untreated states, and the application of several diagnostic methods—visual inspection of regression residual errors as well as randomized treatment, leave-one-out, TNC market launch encoding, or excluding-outlier robustness checks—yields no evidence of systematic error or potential misspecifications (SI Section 4).

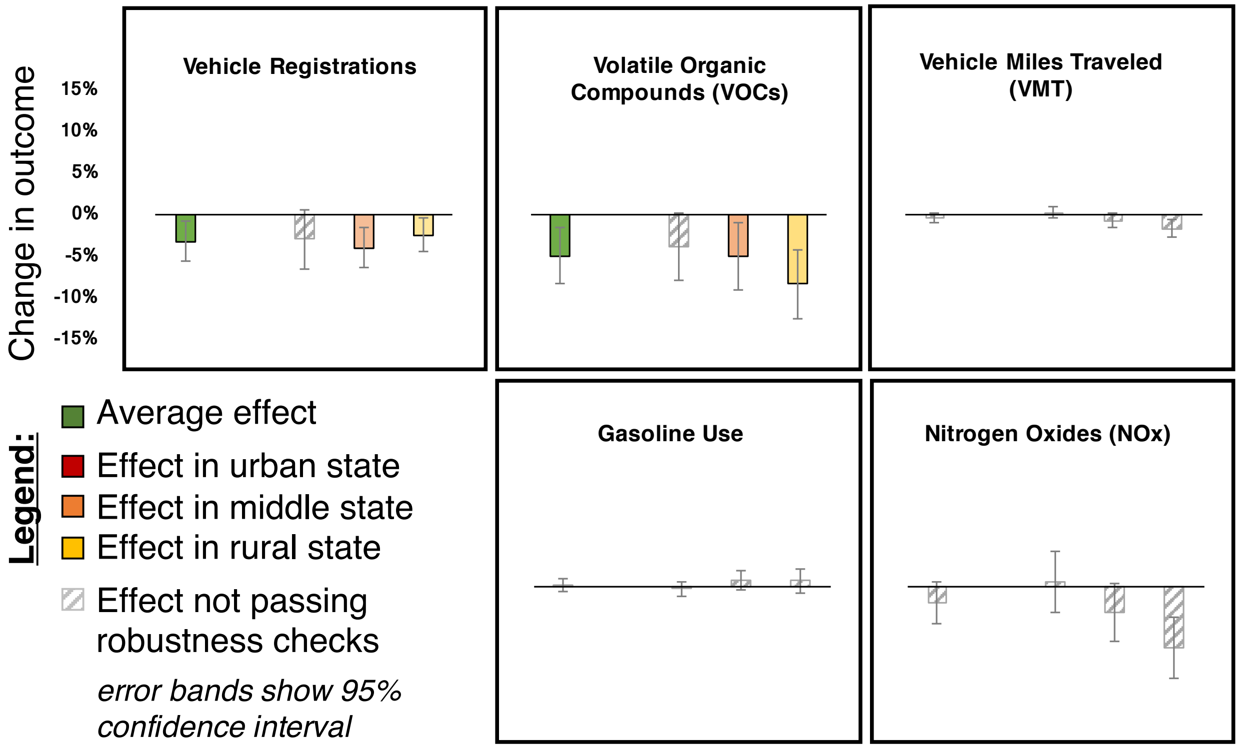


Figure 6. Summary depiction of the effect of TNC entry effects associated with vehicle registration, gasoline use, VMT, and emissions (NO_x and VOCs shown) estimated using a binary (average effect) and a TNC entry-state urbanization interaction regression model. The combined effects for urban, middle, and rural states are shown for effect estimated using the interaction model; all estimated effects, including interaction term details, are reported numerically in Tables 1 and 2.

Our results also suggest a relationship between TNC entry and VOC emissions of highway vehicles as estimated by the EPA, and these results pass my robustness checks, but I are cautious about interpretation because the dependent variable is modeled rather than measured and has several years of interpolated values. I view this finding as exploratory, indicating potential value in further study of TNC implications for emissions. I do not identify robust, statistically significant effects of TNC entry on gasoline consumption, vehicle miles traveled, or emissions other than VOCs, but this does not imply that TNCs have no effect on these outcomes. It is possible, for example, that TNCs have had substantial impact on these outcomes in particular U.S. cities without producing robust, statistically significant patterns across U.S. states that can be identified with my analysis.

Our analysis focuses on net effects to overall state-level outcomes after TNCs enter cities within those states. I cannot identify changes to vehicle fleet mix with the available data, and there are potentially multiple alternative—and sometimes competing—narratives that might explain these trends. For example, it is possible that TNCs reduce VOC emissions primarily by shifting VMT away from older, less efficient personal vehicles toward newer, more efficient

TNC vehicles that operate under hot steady-state conditions for a large portion of VMT, but it is also possible that the VOC emissions decline detected here results from the fewer vehicles (also detected here) used as an input to the models that EPA uses to produce published highway emissions data. Newer vehicles are associated with lower pollutant emissions: CO, NO_x, VOC, and PM emissions in light-duty transportation have declined 30-50% over the past ten years[38]. The EPA emissions estimates I use do not account for potential changes in cold start vs. hot operation ratios induced by TNCs, so any signal captured by my linear models and data is potentially attributable to a vehicle fleet transition but not likely to drive-cycle changes. As another example, it is possible that TNCs increase VMT on a per-trip basis due to "deadheading" (empty miles traveled between passenger trips) and trips induced from other travel modes and that TNCs simultaneously decrease the total number of trips traveled, since the marginal cost per trip is higher in a TNC than in a personal vehicle (where vehicle capital costs are "sunk"). Depending on their relative magnitudes, these dynamics could yield a near-zero net effect. Additional study on the effect of TNC market entry on vehicle fleet composition and distribution of VMT across the fleet is needed for deeper insight about the mechanisms that produce these outcomes.

2.7. Conclusion

I estimate that TNC service availability in U.S. cities has reduced state per-capita vehicle ownership by 3.1% on average during the period 2005 to 2015 (relative to expected trends if the TNCs had not entered). This effect is estimated at the state level, though TNC market entry and ridership is generally an urban phenomenon. An effect of this magnitude would correspond to a reduction in vehicle ownership of 4.1%, on average, across all urban areas (assuming no effect in rural areas). This reduction in vehicles is directionally consistent with the 9% increase in reported ownership found by Hampshire et al. (2017) among former Uber users after Uber left Austin, TX and with the 9% reduction in reported ownership found among users of ridesourcing in seven U.S. cities by Clewlow and Mishra (2018) but is opposite in direction to the effect that Gong et al. (2017) estimates in China. In total, I estimate that in a counterfactual scenario where TNC services had not been available over the analysis period (and all other variables are held constant), the U.S. vehicle fleet would have been larger by 8 million vehicles in 2015. Note that this does not necessarily imply a change in new vehicle sales.

Further, using EPA estimates of passenger transportation emissions, I estimate that TNC service availability is associated with a reduction in state per-capita VOC emissions by 4.8%. Using DOT's average per-mile estimates of the externality costs air pollutant emissions (NHTSA, 2012), I estimate that these emissions reductions represent external cost savings of \$500 million (\$20 million to \$800 million) over the analysis period (see SI for details). But I emphasize caution interpreting emissions results as causal and instead view them as exploratory.

While I do not identify a robust, statistically significant effect of TNC service availability on gasoline use or VMT, it is worth noting that the external costs associated with the range of the such outcomes estimated here could increase or more than counterbalance the external cost benefits from estimated VOC emissions reductions. Using DOT's estimates of the externality costs of driving from congestion, crashes, and noise travel distance effects, I calculate that the 95% confidence interval of potential effects of TNC entry on VMT corresponds to possible external benefits ranging from –\$1 billion to \$6 billion. Other factors, such as differences in the likelihood of TNC drivers to be involved in accidents or to drive at times and in locations with congestion, could further alter these estimates. Similarly, using estimates for the benefits of reducing price-shock risk to the U.S. economy associated with reducing gasoline imports, I estimate that potential external benefits of TNC-induced gasoline consumption reduction could range from –$600 million to $500 million.

These findings should be interpreted in context: my analysis does not identify the mix of trends that may lead to these net results, such as competing factors that act both to increase and to decrease VMT or changes in the fleet mix that result in fewer vehicle registrations overall but not necessarily fewer new vehicle purchases. Our effects are all estimated at the state level, and I lack the data to identify smaller-scale nuance at the city and regional levels. Future work using higher-resolution data at the zip code or urban area level may be able to further illuminate effects of TNC entry on U.S. cities and identify potentially heterogeneous effects.

2.8. Supplemental Information

The following text and figures offer additional detail in support of the main text, methods, and results reported in "Effects of on-demand ridesourcing on vehicle ownership, fuel consumption, vehicle miles traveled, and emissions per capital in U.S. states." It is organized as follows:

- **Section 1. Prior Literature:** this section summarizes in tabular form prior literature focusing on TNC outcomes and the set of previously published regression models used to study one of the dependent variables of interest considered here (per-capita vehicle registrations, gasoline use, VMT, and/or emissions);
- **Section 2. Data and Methods**: this section provides a series of plots of regression model dependent variables by state and time and by Uber entry cohort and time, a description of controls added and associated improvement of fit (quantified using cross-validation methods), a set of plots showing generalized additive model (GAM) fits for identifying regressor fit, multicollinearity plots for regression variables, a depiction of Uber market entry order as a function of urban area population, and two plots showing the categorization of states into "rural", "average", and "urban" and the characteristics (population, density, and GDP) of largest cities within each type of state;
- **Section 3. Results**: this section includes a table showing detailed results of regression models for all dependent variables of interest (which were only summarized in terms of treatment effect the main text) as well as analogous results using an ordinary least squares difference-in-difference model (i.e., without propensity score weighting); a set of regression results using an alternative approach to determining IPTW weights (along with a comparison of weights and balance across treatment and control groups); a set of regression results showing potential sensitivity to the alternative normalization (i.e., dependent variable per licensed driver or per urban population), timeframe (2009–2015) and additional control variables (indicators for Uber leasing/incentive programs, Lyft market entry, and transit); a set of results examining the effect of lagged treatment (by one and two years); and, finally, a discussion of Benjamini-Hochberg corrections for testing multiple hypotheses;
- **Section 4. Mechanical Robustness Checks**: this section tests for regression model robustness using visual inspection of regression model residual errors, event study results and a synthetic check of the parallel trends assumption, randomized treatment analysis, leave-one-out analysis, sensitivity to excluding discontinuities, and the addition of linear time trends;

- **Section 5. Encoding Robustness Checks**: this section tests for robustness against parameter encoding by comparing two DOT estimates of gasoline use, changing the annual assignment method of Uber entry dates, and using UberX instead of Uber entry dates; and
- **Section 6. Social Costs**: this last section offers details on the estimation method and calculation of the external costs avoided estimates reported in the main text.

2.8.1. Prior Literature

Summary of prior literature focusing on TNC outcomes: this table offers summary descriptions of the key findings on effects of interest, data, and methods for peer-reviewed studies, internal reports, or working papers that either 1) use a difference-in-difference model to identify a TNC market entry effects, or 2) use other methods to study TNC market entry effects on vehicle registrations, gasoline use, VMT, or air pollutant emissions.

Table 5. Prior literature focusing on TNC outcomes.

Study	Effect of interest	Data	Method	Key Findings
Greenwood (2015), journal article	Alcohol-related motor vehicle homicides in California	Webscraper tool for Uber initial service dates and California Highway Patrol's Statewide Integrated Traffic Report System	Difference-in-difference regression model	UberX entry causes fewer road fatalities
Li (2016), working paper	Traffic congestion in the U.S.	Individually collected Uber entry dates and the Urban Mobility Report	Difference-in-difference regression model	Uber causes lower traffic congestion time and fuel costs
Burtch et al. (2016), journal article	Local entrepreneurial activity in U.S. urban areas with	Kickstarter API, Uber Blog, Postmates website, and Area Health Resource Files	Difference-in-difference regression model	Fewer crowdfunding launches
Gong et al. (2017), working paper	New vehicle registrations in China	Manual aggregation of Uber launch data from Uber electronic sources, new domestic passenger vehicle data from China's National Bureau of Statistics, and demographic data from the China City Statistical Yearbook	Difference-in-difference regression model	8% increase in new vehicle ownership

Henao (2017), dissertation	Uber and Lyft vehicle travel patterns and driver earnings in Denver, CO	Self-collected	Descriptive statistical analysis	40% of TNC miles traveled without a passenger
Henao (2017), dissertation	Passenger travel behavior changes in Denver, CO	Self-collected	Survey and statistical analysis	Possible >80% increase in vehicle miles traveled
Hampshire et al (2017), working paper	Effect of Uber/Lyft suspension on travel behavior in Austin, TX	Texas A&M Transportation Institute (TTI) cross-sectional data set from 10-minute online questionnaire	Survey combined with regression	Uber exit associated with 9% increase in vehicle ownership and 23% increase in vehicle trips
Clewlow and Mishra (2017)	Collect data on TNC service adoption and impacts on travel choices	Comprehensive travel and residential survey deployed in seven major U.S. cities, in two phases from 2014 to 2016,	Survey	9% fewer vehicles and 49%-61% more vehicle trips
Hall et al. (2018)	Public transit ridership in U.S. metropolitan areas	National Transit Database, Google Trends	Difference-in-difference regression model	1.4% increase in transit ridership per standard deviation in Google searches for "Uber"
This study	Vehicle registrations, VMT, and emissions	DOT's State Statistical Abstract and Highway Statistics series (registrations, VMT, and population) and EPA's National Emissions Inventory (emissions)	Difference-in-difference regression model	Reductions in per-capita vehicle registrations and VOC emissions

Comparison of regression model structures in previous literature: In Table S2, I compare the use and form of dependent and independent variables as well as sample size and sample geographic location in other models estimating vehicle ownership, vehicle stock, or VMT. This comparison informed the inclusion of population, income, and gas price as dependent variables in my model.

Table 6. A comparison of the regression model posed in this analysis with models used in prior literature modeling one of the dependent variables of interest considered here (per-capita vehicle registrations, gasoline use, VMT, and/or emissions). Independent variables in each model are

denoted by an ‘X’ when included untransformed and by ‘log’ or ‘index’ when represented using those forms, respectively.

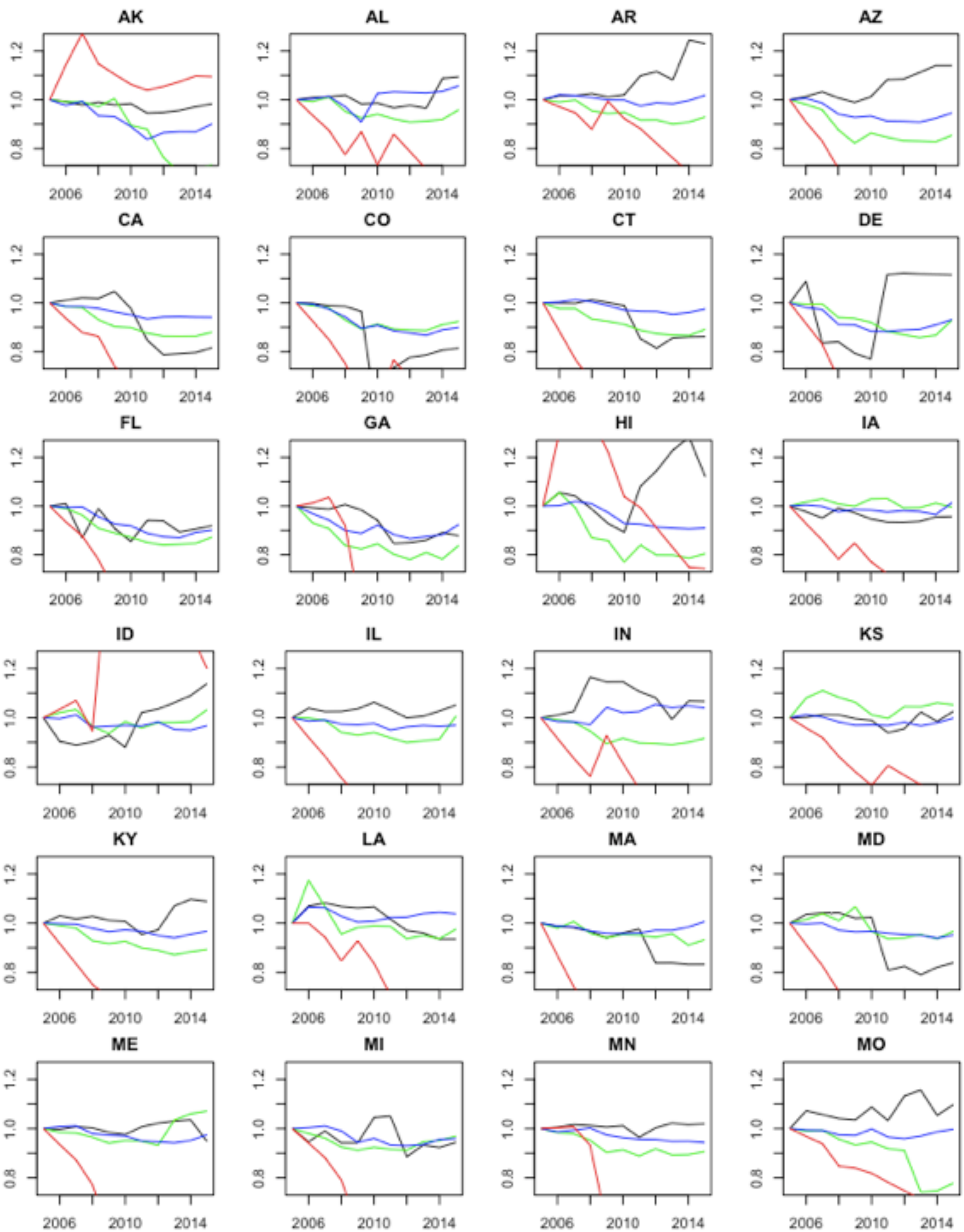
		Gillingham, Jenn, and Azevedo (2015)	Gillingham (2014)	Greene (2012)	Schimek (2014)	Small and Van Dender (2007)	this model
Dependent Variable		log(VMT)	log(VMT)	log(VMT)	number of household vehicles	Vehicle stock	per-capita vehicle regs., gasoline use, VMT, and emissions
Independent Variables	Population				Household size	Lagged (t-1) stock	log
	Gas Price	log	log	log		log	X
	Income	log(GDP)	index	X	log	log	X
	Vehicle Characteristics	X	X	X		X	
	Time FX	X	X	X		X	X
	Geographic FX	X					X
	Density				X		
	Demographics				X		
	Transit				X		
Sample Size		30 M	3 - 5 M	41	15,916	1,734	550
Geography		PA	CA	US	US	US	US

2.8.2. Data and Methods

This section provides a plot of regression model dependent variables by state and time, a comparison of regression models used in the previous literature, a set of plots showing generalized additive model (GAM) fits for identifying regressor fit, and multicollinearity plots for regression variables.

Figure S1 and S2 depict four dependent variables (per-capita vehicle registrations, gasoline use, VMT, and air pollutant emissions—NOx is shown here) by state and time. I note that individual states do not follow clean parallel trends, and noise in parallel trends has potential to bias my estimates. Figure S3 shows per-capita vehicle registration by annualized Uber entry cohort. Table S3 reports the results of five-fold cross validation root mean square error (rMSE) as a function of the stepwise introduction of covariates. Figure S4 illustrates partial residual errors from generalized additive model (GAM) fits showing the relationship between control variables and associated partial residuals. Figure S5 presents a scatterplot matrix showing correlations between treatment and control variables. And, Figure S6 plots Uber market population (measured for relevant combined statistical area, or CSA) as a function of Uber market launch date, showing that Uber generally entered larger cities (by population) first.

Plot of regression model dependent variable by state and time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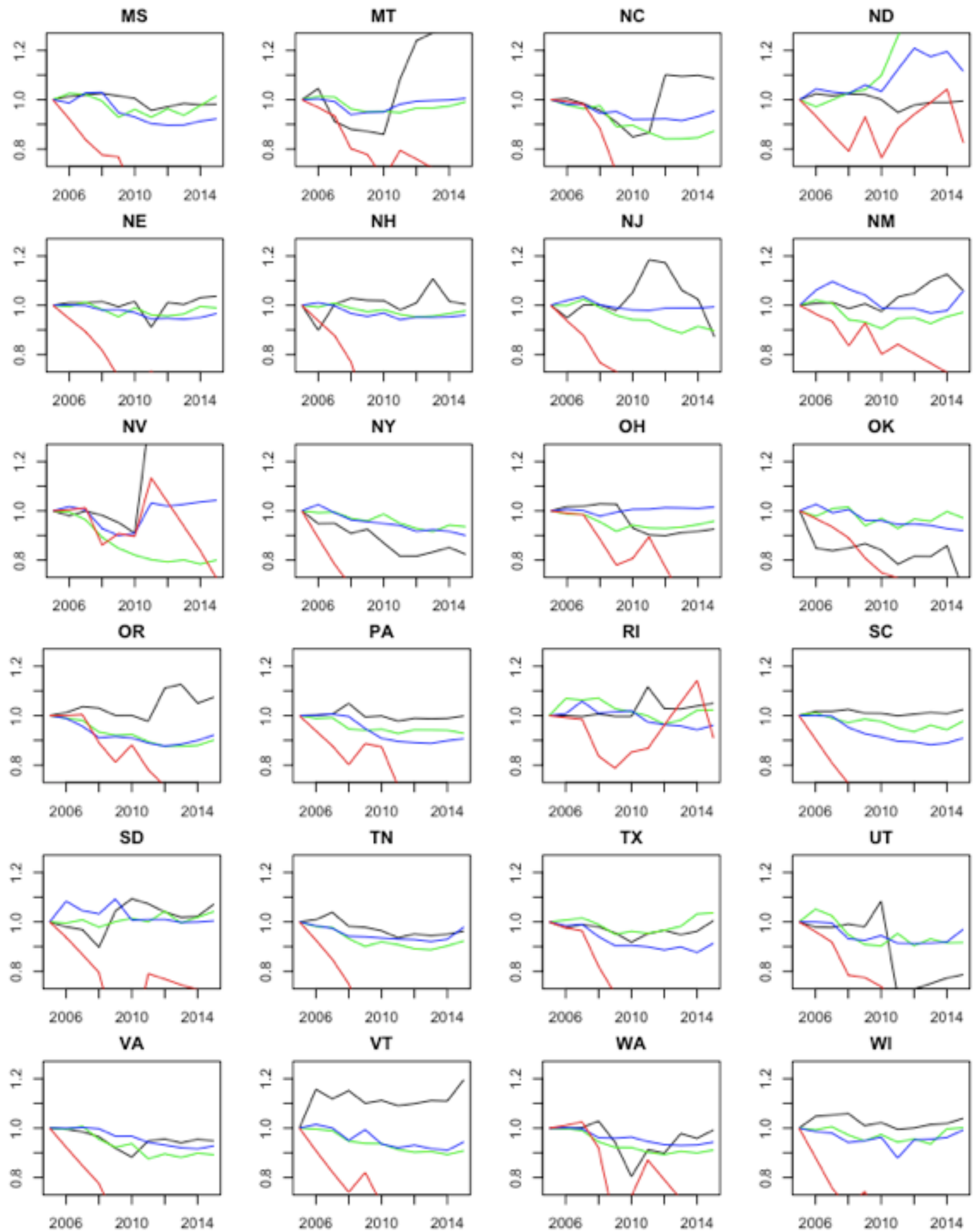
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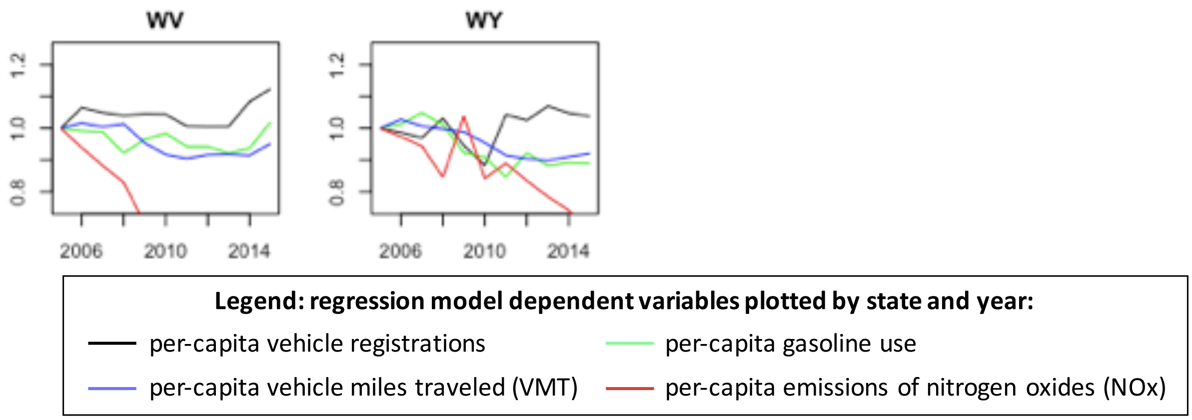
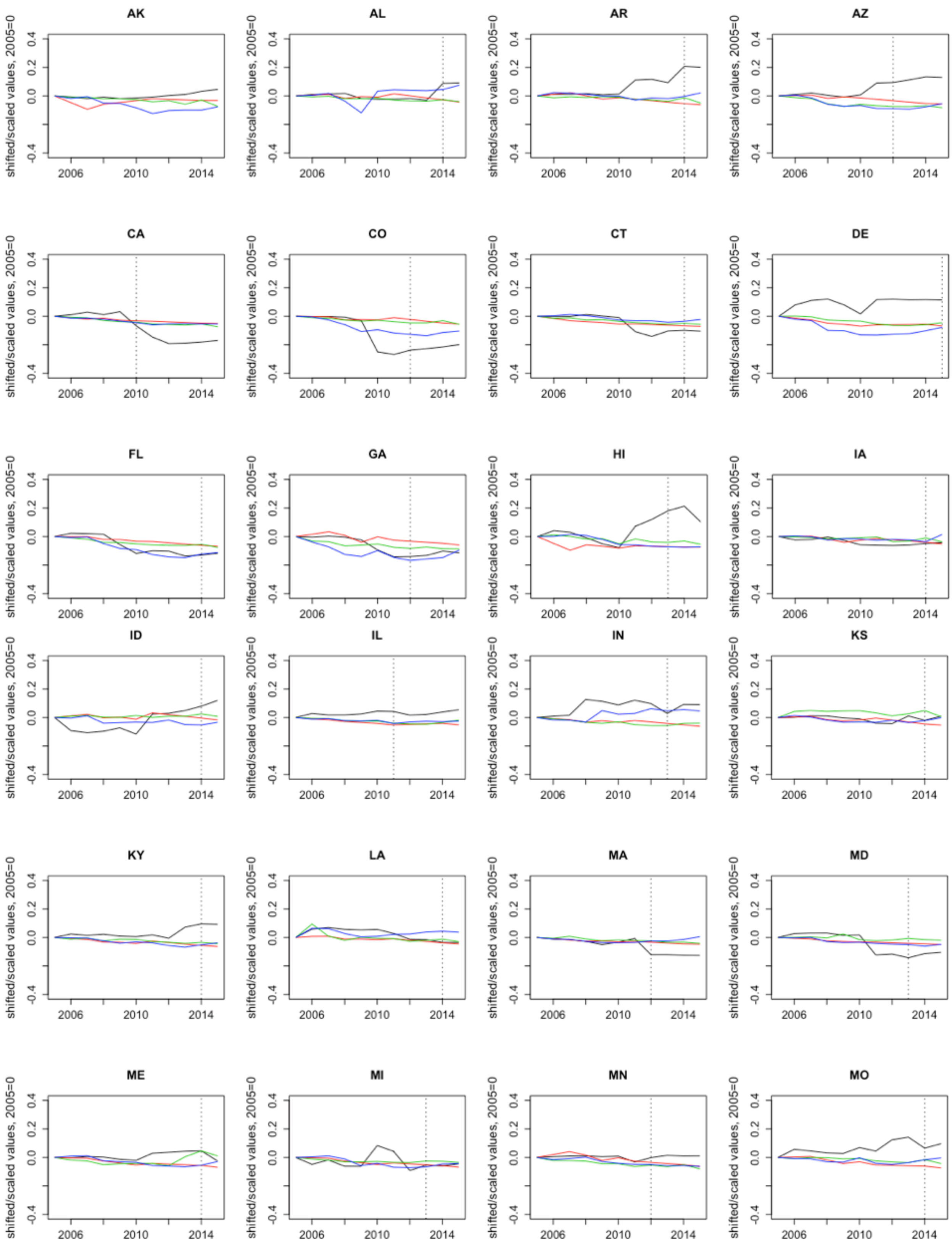


Figure 7. Dependent variables (per-capita vehicle registrations, per-capita gasoline use, per-capita vehicle miles traveled, and per-capita NOX emissions, which is shown as one indicator of the full suite of emissions data used) shown by state over time. Variables are indexed by their 2005 value (2005 = 1.0) to show this set of four heterogeneous measures on the same axes.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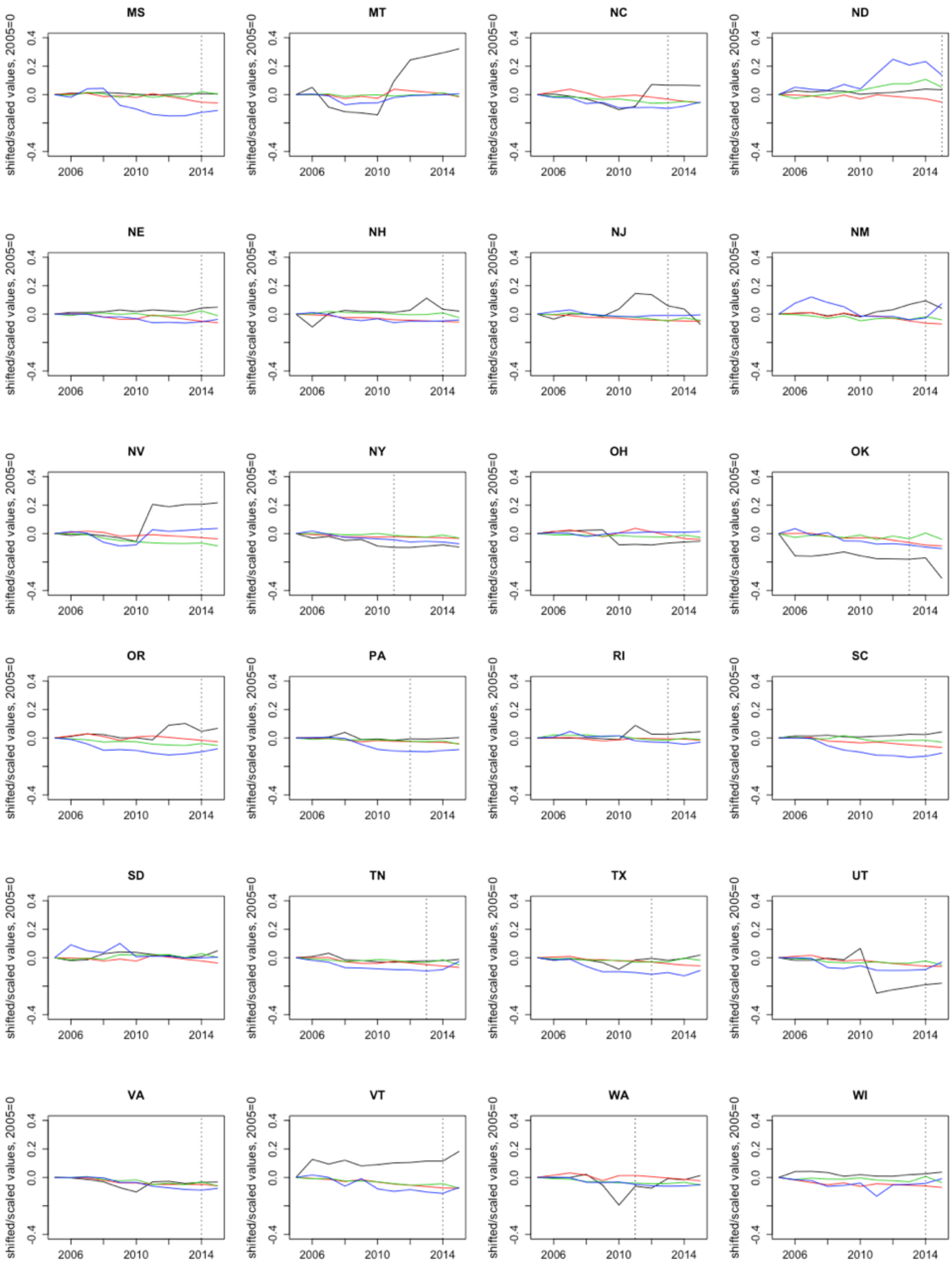
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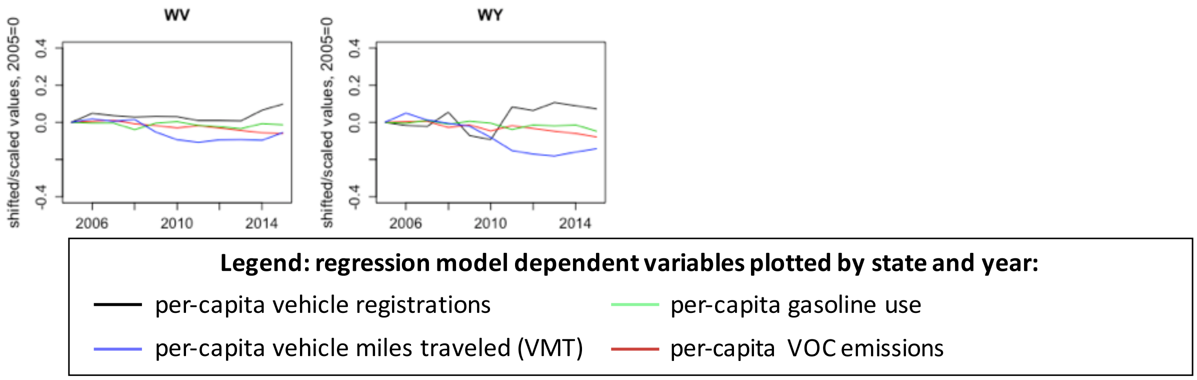


Figure 8. Dependent variables (per-capita vehicle registrations, per-capita gasoline use, per-capita vehicle miles traveled, and per-capita VOC emissions, which is shown as one indicator of the full suite of emissions data used) shown by state over time. Variables are shifted by subtracting the 2005 value from all years, and per-capita VMT and VOC emissions are scaled (by 100 and 10,000, respectively) to show this set of four heterogeneous measures on the same axes. The dotted line shows the year of Uber entry.

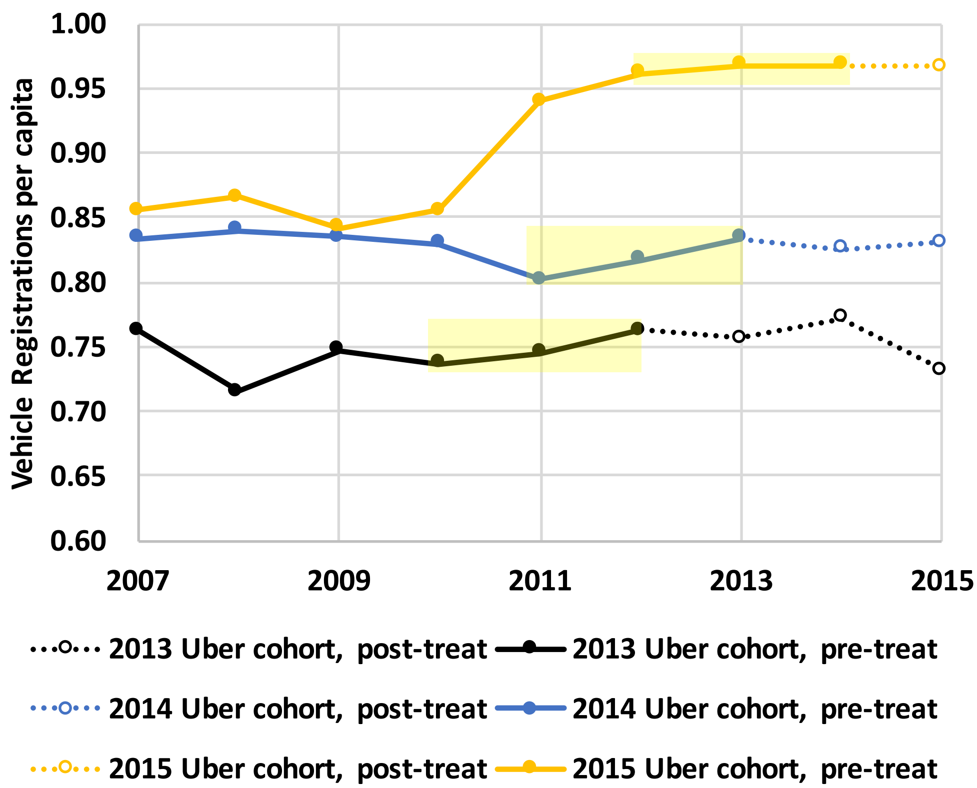


Figure 9. Comparison of per-capita vehicle registration data by Uber annual treatment cohort (i.e., the mean of states treated in 2012 compared to those treated in 2013 and 2014).

<u>Regression model form</u>

Figure 10. Improvement in model fit, as measured by reductions in five-fold cross validation root mean square error (rMSE), as a function of the stepwise introduction of covariates. Variables common to models in previous relevant literature (as shown in Table S2) motivate the set of covariates considered here. The GDP of a state's largest city (citygdp), the percentage of a state's population that is urban (pop_u2), real per-capita income (inc), gas price (gas), and the logarithm of a state's population (lpop) are all shown to reduce root mean square error, as calculated using 5-fold cross-validation on a model with and without each of those covariates (as is highlighted in the table below). While the population of a state's largest city (citypop), the density of that city (citydensity), and whether a state has adopted California's more stringent air pollutant standards (s177) are not shown to improve five-fold cross validation rMSE, all are included in this study's regression model to ensure that potential correlations between these variables and the dependent variables of interest are not misattributed to Uber entry.

	covariate to add:							
comparison model	**citygdp**	**pop_u2**	**inc**	**gas**	**lpop**	**citypop**	**citydensity**	**s177**
fixed effects only	-2.19%	-1.47%	-1.75%	0.33%	0.09%	-0.21%	-0.10%	0.22%
fixed effects + citygdp		-0.96%	-0.92%	0.20%	0.09%	0.10%	0.07%	0.25%
fixed effects + citygdp + pop_u2			-0.73%	0.06%	-0.02%	0.10%	0.03%	0.26%
fixed effects + citygdp + pop_u2 + inc				-0.17%	-0.02%	0.08%	0.05%	0.27%
fixed effects + citygdp + pop_u2 + inc + gas					-0.09%	0.05%	0.04%	0.26%
fixed effects + citygdp + pop_u2 + inc + gas + lpop						0.07%	0.03%	0.26%

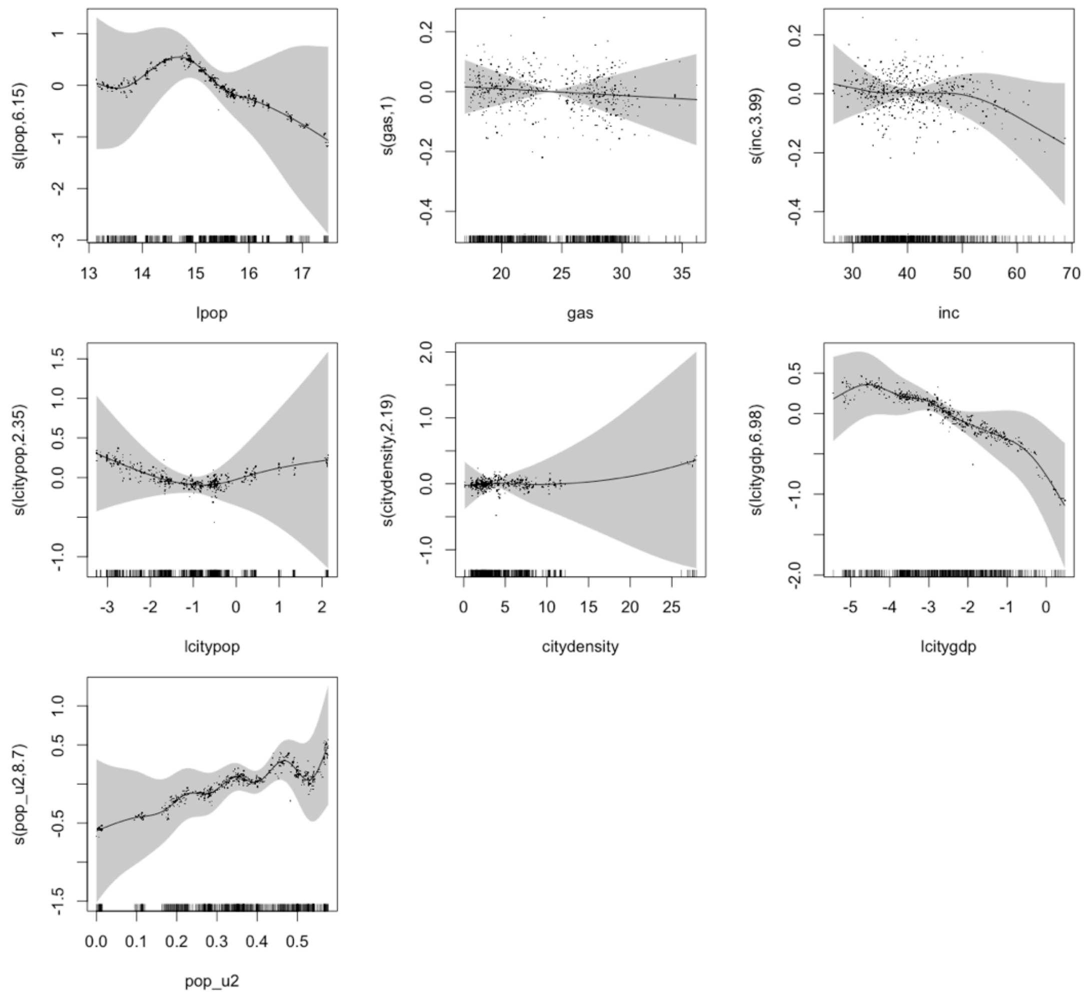


Figure 11. Partial residual plots with generalized additive model (GAM)[39] fits showing the relationship between control variables (x-axes) and associated partial residuals (actual minus fitted per-capita vehicle registrations; y-axes). Shaded areas show 95% confidence bands around the conditional mean. Fixed effects and indicators (Katrina, Harvey, and Cash for Clunkers) are included in the GAM but not examined for structure here because they are simple binary indicators. Gasoline price (gas), the population density of the largest city in a state (citydensity), and the percentage of a state's population that is urban (pop_u2) are estimated to have sufficiently linear relationships with partial residuals (i.e., a straight line could be fitted within the GAM's shaded confidence intervals) to be included without transformation. Structure in the residuals for the logarithm of state population (lpop), real personal income (inc), the logarithm of a state's largest city's population (lcitypop) and its GDP (lcitygdp) suggest including higher-order powers. The addition of a second-order term for each of these yields new GAM-estimated linear relationships. Independent variables are transformed until all GAM estimates are linear, such that the final model used is population (log, quadratic), urban population percentage (level), gas price (level), real personal income (quadratic), Section 177 status (level), and largest city population (log, quadratic), density (level), and GDP (log, quadratic).

Correlation plots

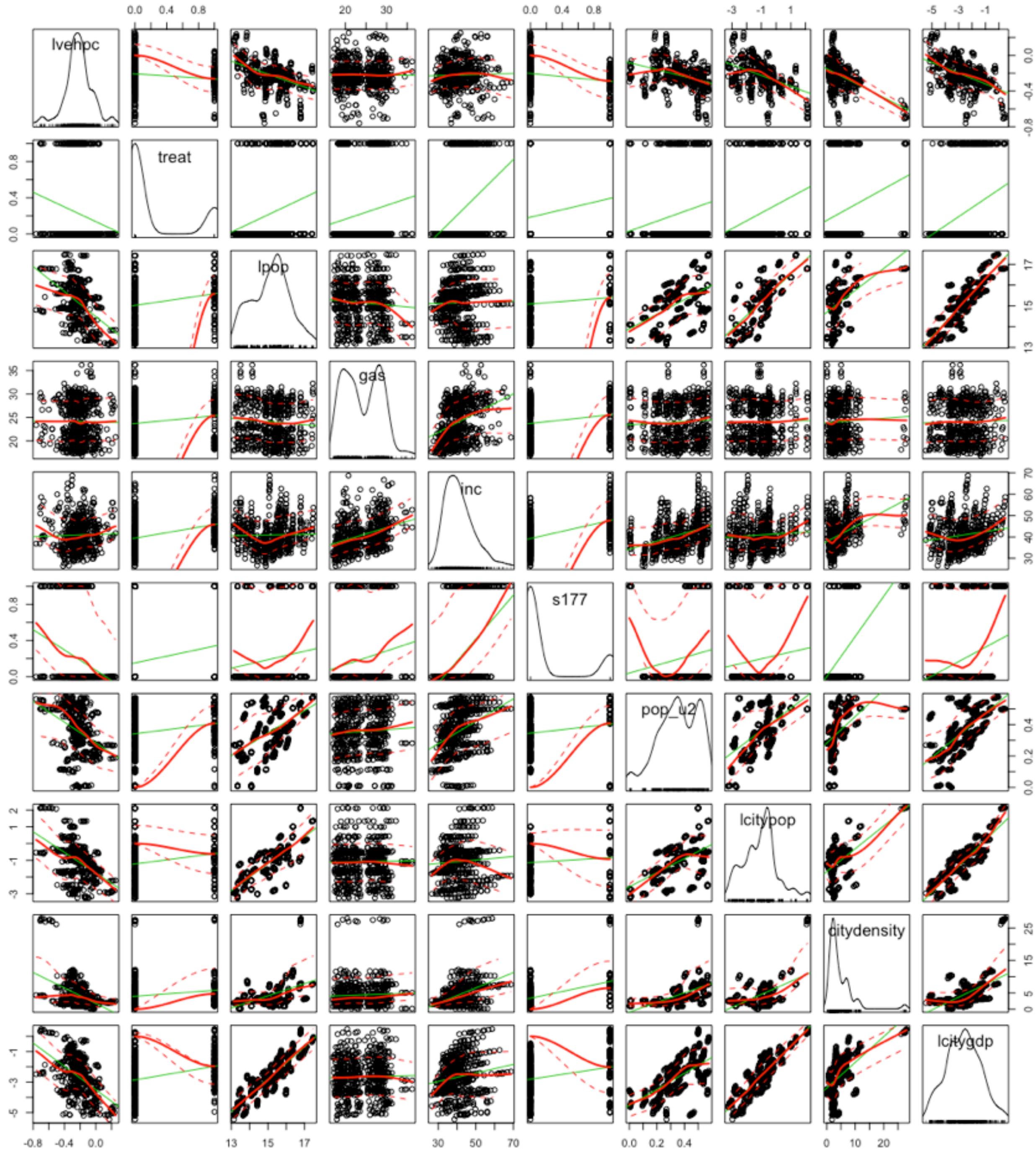


Figure 12. A scatterplot matrix of the dependent variable per-capita vehicle registrations, Uber treatment, and covariates (state population [lpop], gas price [gas], real per capita income [inc], Section 177 status [s177], and urban population percentage [pop_u2], as well as largest city population [lcitypop], density [lcitydensity], and GDP [lcityGDP]), showing evidence of correlation between Uber treatment and each of the other regressors as well as among some other regressors. Not shown is an alternative correlation matrix showing near-perfect correlation

between population and income but no such correlation between population and income per capita. Accordingly, to avoid multicollinearity, I express income in per capita terms (shown here), which also has the benefit of more direct interpretation compared to the dependent variable (which is also expressed per capita).

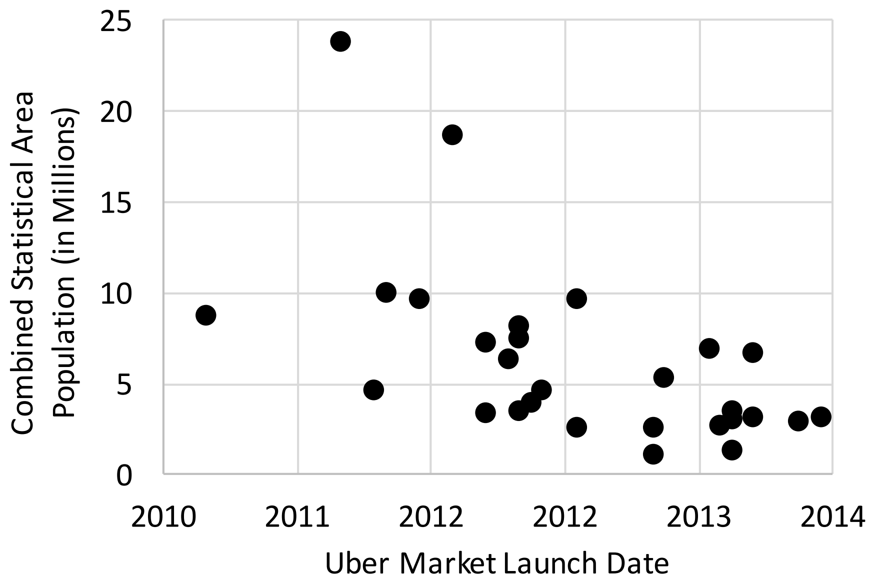


Figure 13. Uber market population (measured for relevant combined statistical area, or CSA) as a function of Uber market launch date, showing that Uber generally entered larger cities (by population) first, which tells us that including a measure of these city's population, which I do with each state's largest city's population, as a control variable and as a variable in the IPTW is likely important. Data points, from left to right, include San Francisco, New York City, Seattle, Chicago, Washington (DC), Los Angeles, Philadelphia, San Diego, Atlanta, Boston, Dallas-Fort Worth, Denver, Minneapolis-St. Paul, Phoenix, Baltimore, Sacramento, Rhode Island, Charlotte, Detroit, Houston, Pittsburgh, Louisville, Cleveland, Tampa Bay, Miami, Orlando, St Louis, and Portland (OR).

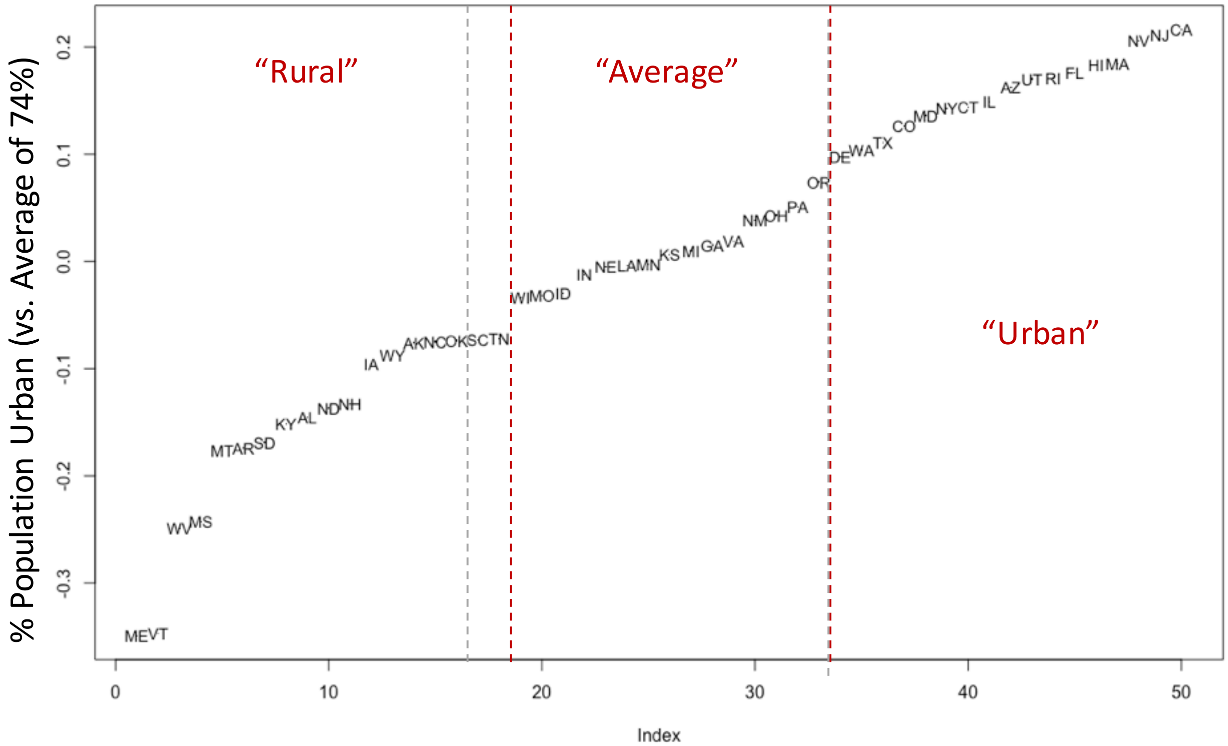


Figure 14. States indicated by their abbreviation ordered by percentage of population that is urban (y-axis). For the purposes of this analysis, states are classified as either "rural", "average", or "urban" as a function of their urban population percentage; groups are separated at discontinuities near indices 16 and 33 (i.e., roughly three equal groups without separating states that have similar urban population percentages). Rural states are Maine, Vermont, West Virginia, Mississippi, Montana, Arkansas, South Dakota, Kentucky, Alabama, North Dakota, New Hampshire, Iowa, Wyoming, Alaska, North Carolina, Oklahoma, South Caroline, Tennessee. Average states are Wisconsin, Missouri, Idaho, Indiana, Nebraska, Louisiana, Minnesota, Kansas, Mississippi, Georgia, Virginia, New Mexico, Ohio, Pennsylvania, Oregon. Urban states are Delaware, Washington, Texas, Colorado, Maryland, New York, Connecticut, Illinois, Arizona, Utah, Rhode Island, Florida, Hawaii, Massachusetts, Nevada, New Jersey, and California.

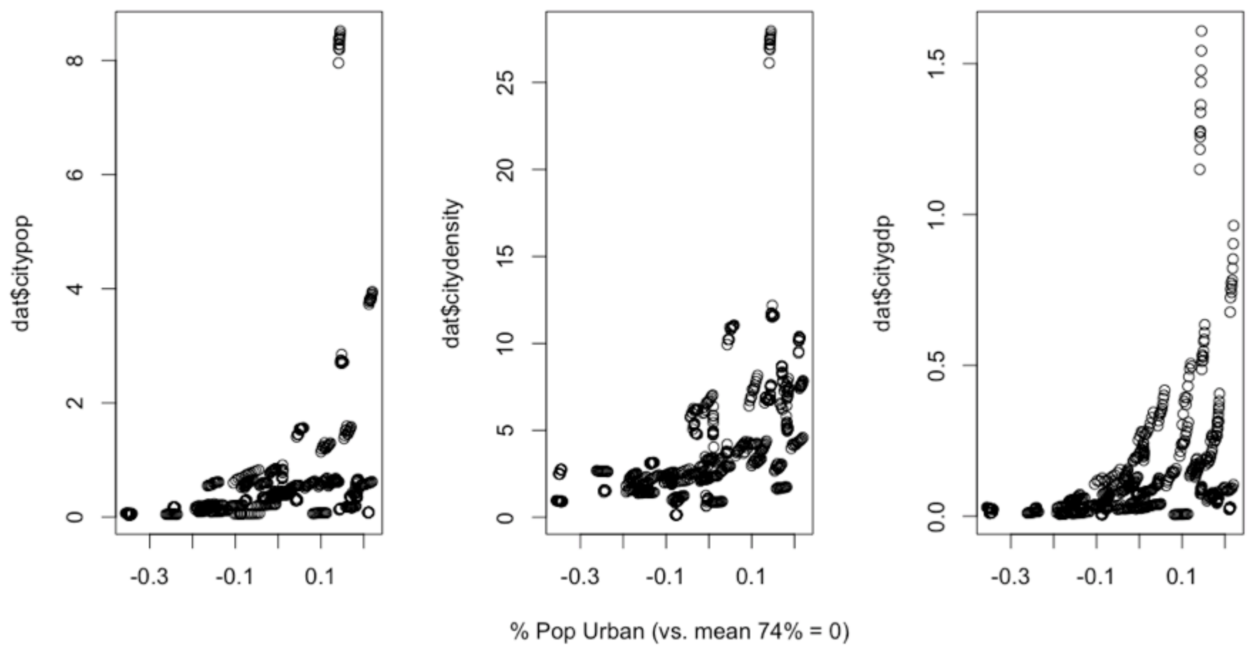


Figure 15. Characterization of the biggest city within a state in terms of its population (y-axis at left), density (y-axis in center), and GDP (y-axis at right) as a function of the percentage of a state's population that is urban (x-axis). The largest cities in states classified as rural in Figure S5 have low populations, densities, and GDPs; the largest cities in average states have similar or higher populations, densities, and GDPs; and the largest cities in urban states are the most heterogeneous. Because these city-level characteristics could influence Uber market entry and affect my outcomes of interest, I include these variables both as predictors in my probability of Uber treatment model and as controls in my regression model.

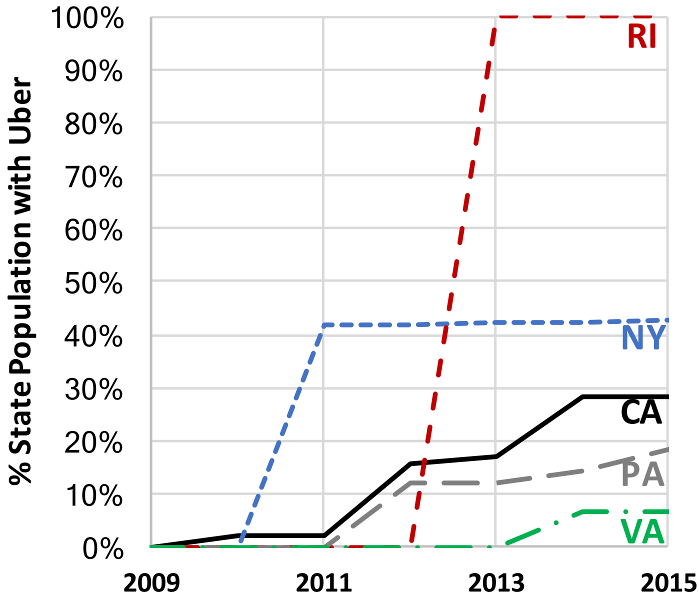


Figure 16. The population share of states with Uber access for five illustrative states from 2009–2015. Uber enters California (CA) first in 2010 and launches in other cities in the state over time. This "gradual expansion" trend is as is also shown by Pennsylvania (PA) and for most other states not shown (FL, IA, ID, IN, KS, LA, MA, MI, NC, NM, NV, OH, OK, OR, TX, WA, and WI). In New York (NY), Uber's entry into a single city (New York City) accounts for most of the state population share with Uber access, which is also the case for some other states where populations with Uber access are either entirely or mostly concentrated in a single metropolitan area (AZ, CO, HI, IL, KY, MO, ND, NE, OR, TN, and WI). In Rhode Island (RI), Uber declares entry for the whole state at once, which also occurs in CT and NJ. Virginia (VA) is shown to underscore that in some states Uber was available to a small part of the population (<10%) by the end of 2015 (AL, AR, GA, MD, ME, MN, SC, UT, VA, and VT). And, not shown are the five states Uber still had not entered by the end of this analysis period: AK, MT, SD, WV, and WY.

2.8.3. Results

This section includes a table showing detailed IPTW results for all regression models (which were only summarized in the main text) and regression results using an ordinary least squares difference-in-difference model (i.e., without propensity score weighting).

The results presented in this section also include effects calculated using the population-share model mentioned in the robustness discussion that concludes the Results section of the main text. In this model, I weight a binary Uber indicator with the percentage of a state's population that I estimate, using U.S. Census data for cities and states, has access to shared mobility at a given time.

Full regression model results

Table 7. Detailed regression model results using per-capita vehicle, gasoline use, and VMT as dependent variables and which were only summarized in the main text; fixed effects coefficients

for state and time are not shown. The estimated coefficients for the binary, population-weighted, and interaction models (numbered 1, 2, and 3, respectively) are shown. I emphasize caution when interpreting coefficients for control variables that also appear in the IPTW regression to determine probability of treatment weights. Note that the statistically significant results in my population-weighted model (treatpop) did not pass my robustness checks.

	Dependent variable:								
	log(Veh. Reg. per cap)			log(Gas. Use per cap)			log(VMT per cap)		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
treat	−0.031**		−0.028	0.003		−0.001	−0.003		0.003
	(0.012)		(0.018)	(0.004)		(0.004)	(0.003)		(0.004)
treatpop		0.018			−0.010			−0.018***	
		(0.027)			(0.008)			(0.006)	
treat:mid			−0.010			0.009			−0.009*
			(0.019)			(0.006)			(0.005)
treat:rural			0.005			0.009			−0.019***
			(0.021)			(0.007)			(0.006)
lpop	8.644***	9.204***	8.834***	0.330	0.220	0.331	0.493	0.428	0.365
	(2.874)	(2.889)	(2.889)	(0.882)	(0.891)	(0.877)	(0.864)	(0.859)	(0.839)
lpopsq	−0.287***	−0.303***	−0.294***	−0.013	−0.010	−0.011	−0.026	−0.024	−0.026
	(0.097)	(0.097)	(0.097)	(0.031)	(0.031)	(0.031)	(0.027)	(0.027)	(0.026)
gas	0.002	0.002	0.002	−0.009***	−0.009***	−0.009***	0.001	0.001	0.001
	(0.006)	(0.006)	(0.006)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
inc	0.0004	0.003	0.002	0.019***	0.018***	0.018***	−0.005	−0.007*	−0.004
	(0.011)	(0.011)	(0.011)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
incsq	−0.0001	−0.0001	−0.0001	−0.0001***	−0.0001***	−0.0001***	0.00005	0.0001*	0.00003
	(0.0001)	(0.0001)	(0.0001)	(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
unemp	−0.010	−0.013*	−0.010	−0.007***	−0.006***	−0.007***	−0.002	−0.003	−0.002
	(0.007)	(0.007)	(0.007)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
s177	−0.034	−0.032	−0.032	0.012	0.011	0.012*	0.005	0.005	0.003
	(0.022)	(0.022)	(0.022)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
lcitypop	−0.440	−0.451*	−0.450*	0.022	0.022	0.002	0.155*	0.151*	0.198**
	(0.267)	(0.269)	(0.269)	(0.065)	(0.066)	(0.066)	(0.084)	(0.082)	(0.084)
lcitypopsq	0.036	0.029	0.037	−0.018	−0.017	−0.020	0.011	0.011	0.015
	(0.076)	(0.077)	(0.077)	(0.021)	(0.022)	(0.021)	(0.023)	(0.024)	(0.023)
citydensity	0.095***	0.099***	0.097***	−0.021*	−0.022*	−0.020*	−0.034***	−0.034***	−0.037***
	(0.034)	(0.035)	(0.034)	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)
lcitygdp	−0.270	−0.298	−0.285	−0.034	−0.033	−0.021	−0.017	−0.023	−0.030
	(0.184)	(0.185)	(0.191)	(0.067)	(0.065)	(0.066)	(0.059)	(0.059)	(0.059)
lcitygdpsq	−0.030	−0.027	−0.028	−0.011	−0.011	−0.008	−0.002	−0.003	−0.007
	(0.021)	(0.022)	(0.022)	(0.009)	(0.009)	(0.009)	(0.007)	(0.007)	(0.007)
pop_u2	5.327***	5.599***	5.349***	−0.302	−0.378	−0.487	1.103***	1.018***	1.413***
	(1.002)	(1.000)	(1.049)	(0.417)	(0.421)	(0.454)	(0.341)	(0.338)	(0.359)
katrina	0.116**	0.117**	0.120**	0.017	0.017	0.018	−0.092***	−0.091***	−0.097***
	(0.050)	(0.051)	(0.051)	(0.017)	(0.017)	(0.017)	(0.019)	(0.018)	(0.019)
sandy	0.079	0.085	0.080	0.007	0.005	0.006	−0.002	−0.004	−0.001
	(0.050)	(0.063)	(0.051)	(0.006)	(0.006)	(0.006)	(0.008)	(0.006)	(0.009)
clunkers	−5.563	−7.109	−5.395	0.312	0.762	0.854	11.501*	12.019*	10.436
	(21.797)	(22.700)	(22.018)	(6.986)	(7.102)	(6.956)	(6.451)	(6.519)	(6.443)
mid			0.493***			0.050			0.297***
			(0.174)			(0.071)			(0.058)
rural			1.696**			−0.194			−0.945***
			(0.711)			(0.218)			(0.250)
Constant	−66.813***	−71.777***	−70.056***	−3.204	−2.223	−3.315	−6.886	−6.301	−4.375
	(21.173)	(21.267)	(21.575)	(6.361)	(6.424)	(6.314)	(6.735)	(6.678)	(6.738)
Observations	550	550	550	550	550	550	550	550	550
Adj. R-Sq	0.865	0.862	0.864	0.985	0.985	0.985	0.988	0.988	0.988
Note:							*p<0.1; **p<0.05; ***p<0.01		

Table 8. Detailed results for the per-capita emissions regression models, which were only summarized in the main text. The estimated coefficients for the binary, dynamic, and population-weighted models (numbered 1, 2, and 3, respectively, to match those shown in the main text) are shown for the emissions of volatile organic compounds (VOCs), nitrogen oxides (NOx), carbon monoxide (CO), ammonia (NH3), particulates (PM10 and, separately, PM2.5), sulfur dioxide (SO2), and greenhouse gases (GHG). Note that the statistically significant results do not pass my robustness checks with the exception of the VOC results, where are reported in the main text.

	Dependent variable:								
	log(VOC per cap)			log(NO_X per cap)			log(CO per cap)		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
treat	−0.048***		−0.037*	−0.018		0.007	−0.032*		−0.012
	(0.017)		(0.021)	(0.013)		(0.018)	(0.018)		(0.022)
treatpop		0.016			0.074**			0.001	
		(0.028)			(0.037)			(0.027)	
treat:mid			−0.011			−0.036			−0.025
			(0.021)			(0.024)			(0.021)
treat:rural			−0.044*			−0.079***			−0.070***
			(0.023)			(0.025)			(0.023)
lpop	30.692***	31.501***	30.267***	32.127***	32.838***	31.573***	21.890***	22.359***	21.318***
	(3.349)	(3.528)	(3.339)	(3.374)	(3.441)	(3.301)	(3.467)	(3.550)	(3.375)
lpopsq	−1.020***	−1.043***	−1.013***	−1.019***	−1.039***	−1.015***	−0.710***	−0.723***	−0.704***
	(0.112)	(0.117)	(0.111)	(0.107)	(0.109)	(0.105)	(0.114)	(0.117)	(0.112)
gas	−0.019**	−0.019**	−0.020**	−0.024***	−0.026***	−0.025***	−0.035***	−0.035***	−0.036***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
inc	0.051***	0.054***	0.052***	0.106***	0.114***	0.111***	0.018	0.019	0.021
	(0.013)	(0.013)	(0.014)	(0.015)	(0.016)	(0.016)	(0.014)	(0.014)	(0.014)
incsq	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.0003***	−0.0003***	−0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
unemp	0.006	0.0004	0.007	0.029***	0.027***	0.030***	0.015**	0.012	0.017**
	(0.007)	(0.007)	(0.006)	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)	(0.007)
s177	0.021	0.025	0.015	0.085***	0.088***	0.076***	0.019	0.021	0.010
	(0.022)	(0.022)	(0.022)	(0.027)	(0.027)	(0.027)	(0.024)	(0.024)	(0.024)
lcitypop	−0.171	−0.189	−0.075	−0.731**	−0.728**	−0.559*	−0.664**	−0.677**	−0.512*
	(0.273)	(0.275)	(0.267)	(0.303)	(0.304)	(0.299)	(0.286)	(0.287)	(0.278)
lcitypopsq	0.159**	0.148*	0.168**	0.134	0.129	0.152*	0.112	0.105	0.127
	(0.078)	(0.078)	(0.076)	(0.094)	(0.094)	(0.090)	(0.082)	(0.081)	(0.077)
citydensity	0.079*	0.085*	0.069	0.085*	0.089*	0.069	0.110**	0.114**	0.096**
	(0.044)	(0.044)	(0.043)	(0.044)	(0.045)	(0.044)	(0.048)	(0.048)	(0.047)
lcitygdp	0.628***	0.582**	0.614**	−0.153	−0.159	−0.202	0.486**	0.455**	0.453**
	(0.239)	(0.238)	(0.239)	(0.223)	(0.216)	(0.224)	(0.228)	(0.228)	(0.225)
lcitygdpsq	0.087***	0.091***	0.075**	0.059*	0.065**	0.038	0.061*	0.063*	0.043
	(0.033)	(0.033)	(0.032)	(0.032)	(0.031)	(0.031)	(0.035)	(0.036)	(0.034)
pop_u2	−1.187	−0.823	−0.564	0.201	0.716	1.444	−2.981**	−2.799**	−1.936
	(1.219)	(1.205)	(1.204)	(1.366)	(1.347)	(1.344)	(1.368)	(1.354)	(1.330)
katrina	−0.001	0.001	−0.016	0.176**	0.174**	0.153**	0.096	0.097	0.075
	(0.067)	(0.068)	(0.066)	(0.071)	(0.071)	(0.070)	(0.072)	(0.073)	(0.070)
sandy	−0.037	−0.029	−0.035	−0.055	−0.043*	−0.051	−0.057*	−0.052*	−0.053*
	(0.042)	(0.027)	(0.040)	(0.040)	(0.025)	(0.035)	(0.034)	(0.027)	(0.030)
clunkers	−11.430	−13.475	−13.779	−36.462	−39.513*	−40.789*	−6.142	−7.143	−9.910
	(20.133)	(19.647)	(19.754)	(23.348)	(22.943)	(22.593)	(23.084)	(23.149)	(22.358)
mid			0.083			−0.021			−0.096
			(0.196)			(0.213)			(0.204)
rural			2.381***			3.741***			2.449**
			(0.847)			(1.018)			(0.950)
Constant	−239.067***	−246.228***	−236.895***	−262.929***	−269.267***	−260.032***	−173.110***	−177.253***	−169.093***
	(24.759)	(26.082)	(25.049)	(26.050)	(26.669)	(26.146)	(26.052)	(26.636)	(25.697)
Observations	550	550	550	550	550	550	550	550	550
Adj. R-Sq.	0.963	0.962	0.963	0.971	0.972	0.972	0.964	0.964	0.965
Note:							*p<0.1; **p<0.05; ***p<0.01		

	Dependent variable:								
	$\log(NH_3$ per cap)			$\log(PM_{10}$ per cap)			$\log(PM_{2.5}$ per cap)		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
treat	−0.027***		−0.027**	0.029		0.091***	−0.002		0.043*
	(0.010)		(0.012)	(0.020)		(0.034)	(0.016)		(0.023)
treatpop		−0.046**			0.213***			0.122**	
		(0.019)			(0.069)			(0.051)	
treat:mid			0.008			−0.136***			−0.099***
			(0.011)			(0.039)			(0.031)
treat:rural			−0.018			−0.106***			−0.075**
			(0.014)			(0.040)			(0.031)
lpop	9.318***	9.444***	8.979***	20.732***	21.584***	21.095***	19.563***	20.324***	19.844***
	(2.793)	(2.898)	(2.762)	(5.101)	(5.392)	(4.932)	(5.061)	(5.261)	(4.972)
lpopsq	−0.305***	−0.308***	−0.295***	−0.662***	−0.685***	−0.701***	−0.619***	−0.640***	−0.648***
	(0.088)	(0.091)	(0.087)	(0.158)	(0.167)	(0.154)	(0.157)	(0.162)	(0.154)
gas	−0.017	−0.015	−0.017	−0.017	−0.023*	−0.018	−0.022*	−0.025**	−0.023*
	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	(0.013)
inc	0.024***	0.021**	0.022***	0.070***	0.089***	0.090***	0.117***	0.129***	0.132***
	(0.008)	(0.008)	(0.008)	(0.017)	(0.018)	(0.018)	(0.019)	(0.018)	(0.019)
incsq	−0.0003***	−0.0002***	−0.0003***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
unemp	0.011**	0.008*	0.011**	0.035***	0.037***	0.037***	0.041***	0.040***	0.042***
	(0.004)	(0.004)	(0.004)	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)
s177	0.026*	0.027*	0.022	−0.030	−0.028	−0.032	−0.006	−0.004	−0.007
	(0.015)	(0.015)	(0.015)	(0.037)	(0.037)	(0.036)	(0.041)	(0.041)	(0.039)
lcitypop	0.145	0.127	0.184	−0.888**	−0.843**	−0.648*	−1.171***	−1.153***	−0.999**
	(0.145)	(0.148)	(0.147)	(0.346)	(0.353)	(0.368)	(0.373)	(0.374)	(0.388)
lcitypopsq	0.056	0.051	0.058	0.125	0.129	0.162	0.051	0.049	0.077
	(0.054)	(0.055)	(0.053)	(0.106)	(0.110)	(0.104)	(0.112)	(0.113)	(0.110)
citydensity	−0.001	0.001	−0.006	0.143***	0.143***	0.128**	0.120**	0.123**	0.111**
	(0.028)	(0.028)	(0.027)	(0.053)	(0.052)	(0.053)	(0.052)	(0.052)	(0.054)
lcitygdp	0.001	−0.035	0.014	−0.332	−0.267	−0.528*	−0.554*	−0.535**	−0.697**
	(0.115)	(0.118)	(0.117)	(0.298)	(0.273)	(0.293)	(0.286)	(0.272)	(0.278)
lcitygdpsq	0.028	0.026	0.022	0.012	0.025	−0.012	0.012	0.021	−0.005
	(0.020)	(0.020)	(0.020)	(0.042)	(0.038)	(0.041)	(0.039)	(0.037)	(0.038)
pop_u2	1.774**	1.671**	1.923**	0.921	1.950	3.327**	2.363	3.057*	4.094**
	(0.879)	(0.827)	(0.925)	(1.680)	(1.554)	(1.637)	(1.692)	(1.649)	(1.736)
katrina	−0.020	−0.017	−0.029	−0.034	−0.042	−0.044	0.109	0.105	0.102
	(0.042)	(0.043)	(0.042)	(0.077)	(0.076)	(0.081)	(0.087)	(0.085)	(0.090)
sandy	−0.044***	−0.047***	−0.045***	0.035	0.060	0.048	−0.019	−0.002	−0.009
	(0.011)	(0.012)	(0.012)	(0.044)	(0.037)	(0.032)	(0.047)	(0.031)	(0.036)
clunkers	30.485***	31.190***	29.631***	−84.568***	−90.860***	−91.186***	−67.252***	−71.427***	−71.988***
	(11.211)	(11.095)	(11.299)	(29.176)	(28.852)	(28.069)	(25.328)	(25.460)	(23.648)
mid			0.272**			0.119			0.148
			(0.106)			(0.248)			(0.256)
rural			0.758			1.502			1.749
			(0.676)			(1.439)			(1.512)
Constant	−86.039***	−87.118***	−83.734***	−176.960***	−184.661***	−177.543***	−171.710***	−178.534***	−172.971***
	(21.553)	(22.343)	(21.867)	(39.893)	(42.377)	(39.616)	(39.546)	(41.312)	(40.178)
Observations	550	550	550	550	550	550	550	550	550
Adj. R-Sq.	0.945	0.945	0.945	0.903	0.903	0.903	0.943	0.944	0.944
Note:							*p<0.1; **p<0.05; ***p<0.01		

	Dependent variable:					
	log(SO_2 per cap)			log(GHG per cap)		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)
treat	0.031*		0.022	0.007		0.005
	(0.018)		(0.026)	(0.008)		(0.009)
treatpop		0.011			0.017	
		(0.038)			(0.014)	
treat:mid			0.010			0.002
			(0.028)			(0.010)
treat:rural			0.034			0.005
			(0.035)			(0.012)
lpop	−13.289**	−13.681**	−12.983**	0.458	0.366	0.517
	(6.101)	(6.188)	(6.116)	(1.998)	(1.947)	(2.002)
lpopsq	0.397**	0.408**	0.393**	−0.027	−0.024	−0.028
	(0.186)	(0.188)	(0.185)	(0.067)	(0.065)	(0.067)
gas	0.032**	0.031**	0.032***	−0.015**	−0.015**	−0.015**
	(0.012)	(0.012)	(0.012)	(0.007)	(0.007)	(0.007)
inc	−0.048*	−0.048*	−0.049*	0.035***	0.037***	0.035***
	(0.025)	(0.025)	(0.026)	(0.006)	(0.006)	(0.006)
incsq	0.0004*	0.0003*	0.0004*	−0.0002***	−0.0002***	−0.0002***
	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
unemp	0.038***	0.041***	0.037***	−0.005	−0.004	−0.005
	(0.014)	(0.014)	(0.014)	(0.004)	(0.003)	(0.004)
s177	−0.028	−0.029	−0.023	−0.024**	−0.025**	−0.024**
	(0.034)	(0.034)	(0.034)	(0.010)	(0.010)	(0.010)
lcitypop	−0.954**	−0.939**	−1.028**	−0.232*	−0.222*	−0.243*
	(0.464)	(0.464)	(0.463)	(0.133)	(0.132)	(0.138)
lcitypopsq	−0.350***	−0.344***	−0.357***	−0.007	−0.006	−0.008
	(0.119)	(0.119)	(0.117)	(0.040)	(0.040)	(0.040)
citydensity	0.070	0.067	0.078	0.032*	0.031	0.034*
	(0.062)	(0.063)	(0.061)	(0.020)	(0.020)	(0.020)
lcitygdp	0.713**	0.746**	0.726**	−0.037	−0.027	−0.034
	(0.295)	(0.301)	(0.295)	(0.125)	(0.123)	(0.124)
lcitygdpsq	0.085**	0.084**	0.094**	−0.022	−0.021	−0.020
	(0.037)	(0.037)	(0.038)	(0.015)	(0.015)	(0.015)
pop_u2	−0.593	−0.707	−1.084	0.818	0.902	0.727
	(2.081)	(2.076)	(2.176)	(0.682)	(0.682)	(0.742)
katrina	−0.182*	−0.184*	−0.171*	0.094***	0.091***	0.095***
	(0.104)	(0.105)	(0.103)	(0.034)	(0.033)	(0.034)
sandy	0.033	0.031	0.032	−0.013	−0.012	−0.014
	(0.032)	(0.036)	(0.033)	(0.017)	(0.016)	(0.017)
clunkers	−13.839	−13.244	−12.022	−9.132	−9.095	−8.912
	(37.538)	(37.722)	(37.440)	(9.841)	(9.623)	(9.869)
mid			0.330			−0.005
			(0.365)			(0.132)
rural			−2.250			0.798
			(1.922)			(0.492)
Constant	94.502*	97.956**	93.405*	−13.127	−12.431	−14.533
	(48.682)	(49.404)	(50.454)	(14.891)	(14.530)	(15.238)
Observations	550	550	550	550	550	550
Adj. R-Sq.	0.932	0.932	0.932	0.983	0.983	0.983
Note:				*p<0.1; **p<0.05; ***p<0.01		

<u>Ordinary least squares (OLS) regression models:</u> I provide OLS results here for reference and comparison with the IPTW results in Table S5. The OLS results produce similar estimates, though at a reduced level of statistical significance for vehicle registrations. For reasons explained in the main text, OLS without IPTW has the potential to misattribute effects, so I trust the IPTW results more.

Table 9. The regression results for the OLS counterparts to the IPTW regression results presented in Table S4. OLS coefficients are slightly smaller in magnitude than those in the IPTW case. In all cases, the OLS and IPTW estimates for 95% confidence intervals for Uber entry treatment coefficients overlap.

	Dependent variable:								
	log(Veh. Reg. per cap)			log(Gas. Use per cap)			log(VMT per cap)		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
treat	−0.024*		−0.010	−0.003		−0.003	0.001		0.010*
	(0.014)		(0.019)	(0.005)		(0.005)	(0.005)		(0.006)
treatpop		0.017			−0.011			−0.006	
		(0.027)			(0.009)			(0.008)	
treat:mid			−0.033*			−0.002			−0.011*
			(0.019)			(0.006)			(0.007)
treat:rural			−0.017			0.005			−0.019**
			(0.022)			(0.010)			(0.008)
lpop	5.522***	5.775***	5.942***	1.824**	1.813**	1.859**	1.047	1.017	1.159*
	(2.064)	(2.095)	(2.069)	(0.722)	(0.735)	(0.720)	(0.671)	(0.677)	(0.655)
lpopsq	−0.187***	−0.194***	−0.204***	−0.064***	−0.064***	−0.065***	−0.044**	−0.043*	−0.050**
	(0.071)	(0.072)	(0.071)	(0.024)	(0.025)	(0.024)	(0.022)	(0.022)	(0.022)
gas	0.007	0.007	0.006	−0.013***	−0.012***	−0.013***	−0.006***	−0.006***	−0.007***
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
inc	0.004	0.005	0.007	0.027***	0.026***	0.027***	0.006*	0.006	0.007*
	(0.010)	(0.011)	(0.011)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
incsq	−0.0002*	−0.0002*	−0.0002*	−0.0002***	−0.0002***	−0.0002***	−0.00001	−0.00001	−0.00002
	(0.0001)	(0.0001)	(0.0001)	(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
unemp	−0.005	−0.006	−0.005	−0.003*	−0.003*	−0.003*	0.001	0.001	0.001
	(0.005)	(0.005)	(0.005)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
s177	0.006	0.006	0.006	0.003	0.003	0.003	0.004	0.005	0.003
	(0.018)	(0.018)	(0.018)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
lcitypop	−0.024	−0.019	0.005	−0.154*	−0.153*	−0.160*	0.086*	0.085*	0.112**
	(0.144)	(0.144)	(0.143)	(0.093)	(0.093)	(0.096)	(0.049)	(0.049)	(0.048)
lcitypopsq	0.077	0.078	0.082*	−0.015	−0.014	−0.015	0.013	0.014	0.017
	(0.048)	(0.048)	(0.048)	(0.017)	(0.017)	(0.017)	(0.018)	(0.018)	(0.018)
citydensity	0.046**	0.045**	0.042**	0.015	0.015	0.016	−0.022***	−0.022***	−0.025***
	(0.020)	(0.021)	(0.020)	(0.014)	(0.014)	(0.015)	(0.007)	(0.007)	(0.006)
lcitygdp	−0.476***	−0.534***	−0.490***	−0.039	−0.042	−0.038	−0.066	−0.061	−0.073
	(0.146)	(0.143)	(0.147)	(0.054)	(0.052)	(0.053)	(0.055)	(0.053)	(0.055)
lcitygdpsq	−0.062***	−0.069***	−0.062***	−0.012	−0.012*	−0.011	−0.005	−0.005	−0.007
	(0.016)	(0.016)	(0.016)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)
pop_u2	1.985***	2.082***	2.224***	0.528	0.501	0.508	1.250***	1.228***	1.409***
	(0.665)	(0.662)	(0.685)	(0.398)	(0.407)	(0.425)	(0.282)	(0.285)	(0.297)
katrina	0.031	0.031	0.033	0.046***	0.046***	0.048***	−0.055***	−0.055***	−0.057***
	(0.039)	(0.039)	(0.039)	(0.016)	(0.016)	(0.016)	(0.014)	(0.014)	(0.014)
sandy	0.079	0.079	0.081	−0.003	−0.003	−0.003	0.0005	0.0001	0.002
	(0.061)	(0.070)	(0.066)	(0.006)	(0.006)	(0.006)	(0.015)	(0.014)	(0.017)
clunkers	−11.576	−10.978	−12.166	−1.171	−1.203	−1.157	6.429	6.353	6.121
	(13.115)	(13.258)	(13.143)	(4.332)	(4.358)	(4.298)	(5.656)	(5.663)	(5.651)
mid			0.0002			0.193***			0.283***
			(0.111)			(0.072)			(0.050)
rural			0.363			0.014			−0.788***
			(0.505)			(0.227)			(0.166)
Constant	−42.139***	−44.362***	−45.229***	−14.761***	−14.644***	−15.105***	−11.653**	−11.373**	−11.343**
	(15.007)	(15.175)	(15.277)	(5.419)	(5.517)	(5.495)	(5.027)	(5.069)	(4.989)
Observations	550	550	550	550	550	550	550	550	550
Adj. R-Sq	0.782	0.782	0.782	0.972	0.972	0.972	0.978	0.979	0.979

Note: *p<0.1; **p<0.05; ***p<0.01

Table 10. OLS regression results for per-capita emissions of volatile organic compounds (VOCs), nitrogen oxides (NOx), carbon monoxide (CO), ammonia (NH3), particulates (PM10 and, separately, PM2.5), sulfur dioxide (SO2), and greenhouse gases (GHG). OLS coefficients are generally larger in magnitude than those in the IPTW case (Table S5) and are the binary and dynamic effects are significant where IPTW effects were not. Still, in all cases, the OLS and IPTW estimates for 95% confidence intervals (at the p=0.05 level) for Uber entry treatment coefficients overlap.

	Dependent variable:								
	log(VOC per cap)			log(NO_X per cap)			log(CO per cap)		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
treat	−0.060***		−0.041	−0.028		0.002	−0.002		0.036
	(0.020)		(0.026)	(0.020)		(0.028)	(0.018)		(0.024)
treatpop		0.030			0.129*			0.094**	
		(0.050)			(0.070)			(0.039)	
treat:mid			−0.009			−0.031			−0.048*
			(0.028)			(0.033)			(0.025)
treat:rural			−0.066**			−0.086**			−0.096***
			(0.032)			(0.035)			(0.027)
lpop	23.536***	24.130***	23.488***	33.488***	34.097***	33.702***	14.625***	14.913***	15.043***
	(2.706)	(2.783)	(2.701)	(2.881)	(2.893)	(2.793)	(2.479)	(2.480)	(2.436)
lpopsq	−0.776***	−0.793***	−0.780***	−1.068***	−1.085***	−1.083***	−0.477***	−0.485***	−0.501***
	(0.089)	(0.092)	(0.090)	(0.093)	(0.094)	(0.092)	(0.082)	(0.082)	(0.081)
gas	0.025	0.026	0.024	−0.007	−0.008	−0.010	−0.011	−0.012	−0.014
	(0.018)	(0.018)	(0.019)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)	(0.009)
inc	0.057***	0.057***	0.056***	0.107***	0.114***	0.109***	0.029**	0.034***	0.031**
	(0.013)	(0.014)	(0.013)	(0.015)	(0.016)	(0.015)	(0.013)	(0.013)	(0.013)
incsq	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.0003***	−0.0004***	−0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
unemp	−0.002	−0.004	−0.002	0.030***	0.030***	0.030***	−0.006	−0.006	−0.006
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
s177	0.033	0.032	0.028	0.093***	0.091***	0.088***	0.064**	0.063**	0.059**
	(0.025)	(0.025)	(0.025)	(0.031)	(0.029)	(0.031)	(0.026)	(0.026)	(0.026)
lcitypop	0.598***	0.611***	0.681***	0.295	0.299	0.407*	0.155	0.154	0.284
	(0.220)	(0.224)	(0.211)	(0.227)	(0.230)	(0.223)	(0.202)	(0.202)	(0.195)
lcitypopsq	0.247***	0.250***	0.258***	0.200**	0.196**	0.215***	0.153**	0.149**	0.172***
	(0.072)	(0.073)	(0.071)	(0.078)	(0.078)	(0.078)	(0.067)	(0.067)	(0.066)
citydensity	0.010	0.009	−0.001	−0.009	−0.011	−0.023	0.049	0.048	0.033
	(0.035)	(0.035)	(0.033)	(0.034)	(0.034)	(0.032)	(0.031)	(0.031)	(0.030)
lcitygdp	0.180	0.037	0.164	0.038	−0.053	0.011	0.163	0.138	0.129
	(0.213)	(0.223)	(0.211)	(0.218)	(0.217)	(0.221)	(0.185)	(0.179)	(0.182)
lcitygdpsq	0.028	0.011	0.019	0.056**	0.047*	0.045*	0.017	0.016	0.007
	(0.024)	(0.025)	(0.023)	(0.025)	(0.024)	(0.025)	(0.023)	(0.022)	(0.022)
pop_u2	0.634	0.837	1.043	0.840	1.271	1.456	−0.163	0.116	0.583
	(0.998)	(0.988)	(0.977)	(1.098)	(1.068)	(1.081)	(0.981)	(0.958)	(0.931)
katrina	−0.084*	−0.084	−0.097*	−0.004	−0.003	−0.018	0.005	0.005	−0.009
	(0.051)	(0.051)	(0.050)	(0.061)	(0.061)	(0.059)	(0.053)	(0.053)	(0.052)
sandy	−0.067	−0.068	−0.064	−0.072*	−0.065**	−0.067**	−0.050*	−0.044*	−0.045*
	(0.060)	(0.043)	(0.057)	(0.037)	(0.027)	(0.032)	(0.028)	(0.025)	(0.024)
clunkers	−17.034	−15.635	−17.656	−42.962**	−41.453**	−44.046**	−12.737	−11.998	−14.129
	(18.572)	(18.413)	(18.246)	(19.529)	(19.402)	(19.385)	(20.819)	(20.509)	(20.424)
mid			0.193			0.159			0.105
			(0.179)			(0.203)			(0.164)
rural			2.224***			4.378***			1.807***
			(0.729)			(0.835)			(0.680)
Constant	−188.757***	−193.960***	−189.539***	−271.141***	−276.713***	−275.625***	−118.715***	−121.430***	−121.924***
	(20.216)	(20.708)	(20.436)	(22.019)	(22.107)	(21.711)	(18.566)	(18.535)	(18.478)
Observations	550	550	550	550	550	550	550	550	550
Adj. R-Sq.	0.916	0.915	0.916	0.939	0.940	0.940	0.944	0.944	0.944

Note: *p<0.1; **p<0.05; ***p<0.01

	Dependent variable:								
	log(NH_3 per cap)			log(PM_{10} per cap)			log($PM_{2.5}$ per cap)		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)	(1c)	(2c)	(3c)
treat	−0.016		−0.031	0.006		0.070**	−0.022		0.021
	(0.016)		(0.022)	(0.023)		(0.034)	(0.025)		(0.035)
treatpop		−0.061			0.221***			0.142	
		(0.055)			(0.069)			(0.090)	
treat:mid			0.042*			−0.135***			−0.085*
			(0.023)			(0.040)			(0.044)
treat:rural			0.012			−0.099**			−0.074
			(0.030)			(0.042)			(0.045)
lpop	6.278*	6.238*	5.707*	18.432***	19.017***	20.091***	28.221***	28.816***	29.229***
	(3.434)	(3.446)	(3.405)	(4.043)	(4.026)	(3.942)	(4.463)	(4.466)	(4.354)
lpopsq	−0.205*	−0.204*	−0.183*	−0.592***	−0.607***	−0.661***	−0.906***	−0.922***	−0.949***
	(0.107)	(0.107)	(0.106)	(0.131)	(0.131)	(0.127)	(0.144)	(0.144)	(0.140)
gas	0.031	0.032	0.032	−0.045*	−0.047*	−0.049*	−0.031	−0.032	−0.034
	(0.042)	(0.042)	(0.043)	(0.026)	(0.026)	(0.026)	(0.029)	(0.029)	(0.029)
inc	0.042***	0.038***	0.038***	0.078***	0.091***	0.089***	0.108***	0.116***	0.114***
	(0.010)	(0.011)	(0.010)	(0.018)	(0.018)	(0.018)	(0.019)	(0.020)	(0.019)
incsq	−0.0004***	−0.0004***	−0.0004***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***	−0.001***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
unemp	0.004	0.004	0.005	0.036***	0.037***	0.036***	0.040***	0.040***	0.040***
	(0.007)	(0.007)	(0.007)	(0.011)	(0.010)	(0.011)	(0.012)	(0.011)	(0.011)
s177	0.005	0.006	0.004	0.008	0.004	0.005	0.018	0.016	0.015
	(0.023)	(0.024)	(0.023)	(0.033)	(0.031)	(0.033)	(0.037)	(0.036)	(0.037)
lcitypop	0.143	0.147	0.116	0.225	0.220	0.382	0.272	0.275	0.385
	(0.292)	(0.294)	(0.272)	(0.404)	(0.408)	(0.401)	(0.447)	(0.450)	(0.439)
lcitypopsq	0.053	0.057	0.048	0.150	0.140	0.175	0.180	0.176	0.199
	(0.074)	(0.075)	(0.070)	(0.113)	(0.114)	(0.113)	(0.122)	(0.123)	(0.121)
citydensity	−0.007	−0.006	−0.004	0.031	0.029	0.012	−0.019	−0.021	−0.033
	(0.046)	(0.046)	(0.043)	(0.061)	(0.061)	(0.058)	(0.068)	(0.068)	(0.065)
lcitygdp	−0.032	−0.055	−0.016	−0.216	−0.250	−0.279	−0.135	−0.215	−0.176
	(0.197)	(0.202)	(0.192)	(0.267)	(0.258)	(0.273)	(0.286)	(0.281)	(0.289)
lcitygdpsq	0.020	0.016	0.018	−0.002	−0.002	−0.005	0.018	0.011	0.014
	(0.020)	(0.020)	(0.019)	(0.030)	(0.029)	(0.031)	(0.032)	(0.032)	(0.033)
pop_u2	2.687**	2.539**	2.432**	1.297	1.933	2.448*	1.820	2.277	2.607
	(1.075)	(1.004)	(1.127)	(1.409)	(1.401)	(1.477)	(1.641)	(1.631)	(1.693)
katrina	−0.061	−0.062	−0.066	−0.114	−0.112	−0.113	−0.040	−0.039	−0.042
	(0.050)	(0.050)	(0.048)	(0.074)	(0.074)	(0.074)	(0.086)	(0.086)	(0.086)
sandy	−0.071***	−0.076***	−0.074***	0.058	0.073**	0.068**	−0.019	−0.011	−0.012
	(0.020)	(0.018)	(0.019)	(0.044)	(0.034)	(0.032)	(0.051)	(0.034)	(0.041)
clunkers	15.957	15.817	16.652	−87.864***	−86.338***	−90.526***	−87.973***	−86.486***	−89.729***
	(14.205)	(14.106)	(13.842)	(25.582)	(25.621)	(25.185)	(25.298)	(25.351)	(25.084)
mid			0.425***			0.162			0.237
			(0.128)			(0.251)			(0.284)
rural			0.660			2.062**			3.513***
			(0.902)			(1.032)			(1.138)
Constant	−65.021**	−64.525**	−61.792**	−156.660***	−162.250***	−168.959***	−232.781***	−238.264***	−242.286***
	(26.720)	(26.885)	(27.198)	(30.806)	(30.551)	(30.730)	(34.152)	(34.169)	(34.116)
Observations	550	550	550	550	550	550	550	550	550
Adj. R-Sq.	0.769	0.769	0.768	0.810	0.815	0.813	0.864	0.865	0.864

Note: *p<0.1; **p<0.05; ***p<0.01

	Dependent variable:					
	log(SO_2 per cap)			log(GHG per cap)		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)
treat	0.052*		0.066*	−0.002		0.005
	(0.031)		(0.039)	(0.009)		(0.010)
treatpop		0.074			0.029	
		(0.063)			(0.022)	
treat:mid			−0.018			−0.019
			(0.039)			(0.012)
treat:rural			−0.037			−0.008
			(0.046)			(0.015)
lpop	−7.167	−7.392	−7.014	0.147	0.219	0.365
	(5.553)	(5.574)	(5.540)	(1.299)	(1.303)	(1.278)
lpopsq	0.174	0.181	0.165	−0.028	−0.030	−0.037
	(0.178)	(0.179)	(0.177)	(0.042)	(0.042)	(0.041)
gas	−0.021	−0.023	−0.022	−0.033***	−0.033***	−0.033***
	(0.031)	(0.031)	(0.031)	(0.007)	(0.007)	(0.007)
inc	−0.043*	−0.037	−0.042	0.042***	0.043***	0.043***
	(0.026)	(0.026)	(0.026)	(0.006)	(0.006)	(0.006)
incsq	0.0004*	0.0003	0.0004*	−0.0002***	−0.0003***	−0.0003***
	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
unemp	0.033*	0.035**	0.033*	−0.0004	−0.001	−0.001
	(0.017)	(0.017)	(0.017)	(0.004)	(0.004)	(0.004)
s177	−0.004	−0.005	−0.006	−0.012	−0.012	−0.012
	(0.040)	(0.040)	(0.040)	(0.010)	(0.010)	(0.010)
lcitypop	−1.257***	−1.270***	−1.207***	−0.171	−0.167	−0.160
	(0.447)	(0.450)	(0.436)	(0.108)	(0.107)	(0.109)
lcitypopsq	−0.462***	−0.469***	−0.455***	−0.037	−0.037	−0.035
	(0.124)	(0.125)	(0.123)	(0.029)	(0.029)	(0.029)
citydensity	0.136**	0.136**	0.130**	0.030*	0.029*	0.028*
	(0.063)	(0.063)	(0.061)	(0.016)	(0.016)	(0.016)
lcitygdp	1.255***	1.357***	1.242***	0.060	0.044	0.052
	(0.370)	(0.378)	(0.370)	(0.090)	(0.087)	(0.090)
lcitygdpsq	0.081**	0.094***	0.077**	−0.003	−0.005	−0.003
	(0.037)	(0.036)	(0.038)	(0.011)	(0.011)	(0.011)
pop_u2	2.874	2.992*	3.161*	2.118***	2.211***	2.238***
	(1.764)	(1.768)	(1.831)	(0.612)	(0.623)	(0.632)
katrina	−0.106	−0.106	−0.112	0.073***	0.073***	0.075***
	(0.118)	(0.118)	(0.118)	(0.027)	(0.027)	(0.027)
sandy	0.011	0.019	0.013	−0.035	−0.034	−0.035
	(0.047)	(0.046)	(0.045)	(0.026)	(0.023)	(0.025)
clunkers	−50.514	−50.978	−51.047	6.186	6.437	5.850
	(44.696)	(44.934)	(44.728)	(8.587)	(8.496)	(8.578)
mid			1.246***			0.126
			(0.364)			(0.107)
rural			−1.263			0.284
			(1.457)			(0.391)
Constant	54.298	56.062	55.084	−8.641	−9.336	−10.368
	(42.439)	(42.585)	(43.365)	(10.019)	(10.045)	(10.155)
Observations	550	550	550	550	550	550
Adj. R-Sq.	0.917	0.917	0.917	0.981	0.981	0.981
Note:				*p<0.1; **p<0.05; ***p<0.01		

IPTW Weighting: Inverse probability of treatment weights (IPTW) is a statistical method to compare post-treatment trends in treated and non-treated states controlling for potential bias that might otherwise be introduced by systematic differences between treated and non-treated groups. Non-treated states are probabilistically weighted to resemble the treated states along attribute dimensions that are correlated with treatment (state population, income, gasoline price, emissions standards, and largest city population, density, and GDP). Weights are estimated as described in Eq(2) in the main text.

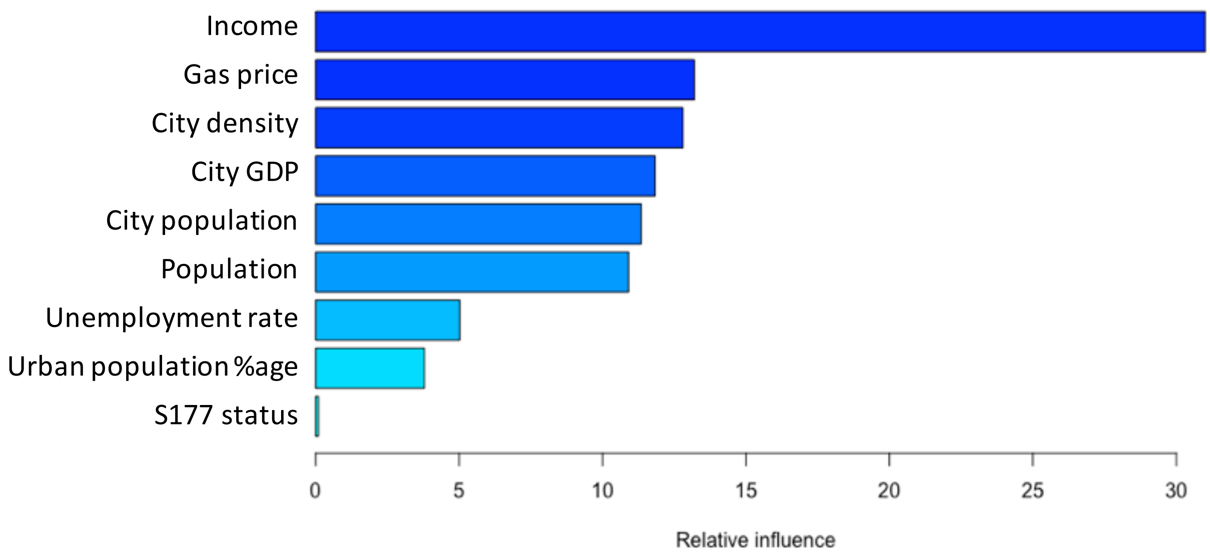


Figure 17. Relative influence diagram showing the respective influence of the selected control variables on determining probability of treatment. The relatively large influence shown for income at the top of the chart indicates that variable is most frequently used as the first branching variable in decision trees produced during gradient boosting; whereas, the relatively small influence shown for Section 177 status at the bottom of the chart indicates that variable is infrequently used as the first branching variable.

Alternative IPTW weights: Alternative weights for use in IPTW regression, calculated using an additional control in the probability of treatment regression that indicates whether Uber had already entered a state in the prior year, motivated by the observation that once Uber enters a state, it stays. This variation on Eq(2) from the main text is as follows:

$$\log\left(\frac{p_{st}(\boldsymbol{\zeta}_{st})}{1 - p_{st}(\boldsymbol{\zeta}_{st})}\right) = \sum_{m} g_m(\boldsymbol{\zeta}_{st}) + \epsilon_{st}$$

In this model, p_{st} is still the probability of treatment for state s and year t; $\boldsymbol{\zeta}_{st}$ is a vector of covariates matching $\mathbf{z}_{st}$ in Eq(2) in the main text plus an indicator for whether Uber was operating in a prior year for state s and year t. As in Eq(2), I estimate the additive function g_m using gradient boosting, given the treatment and covariate data, and compute estimated probability of treatment $\hat{p}_{st}$ for each state and year. The addition of the Uber in prior year indicator improved the fit of the model predicting treatment, resulting in more extreme weights (i.e., closer to zero) for control group observations.

Table 11. Alternative weights for use in IPTW regression, calculated using an additional control in the probability of treatment regression that indicates whether Uber had already entered a state in the prior year, motivated by the observation that once Uber enters a state, it stays.

BASE WEIGHTS

	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
AK	0.01	0.01	0.02	0.11	0.02	0.03	0.04	0.04	0.04	0.04	0.09
AL	0.07	0.07	0.06	0.09	0.03	0.03	0.06	0.06	0.07	1.00	1.00
AR	0.03	0.10	0.08	0.08	0.05	0.04	0.06	0.10	0.07	1.00	1.00
AZ	0.04	0.04	0.05	0.08	0.04	0.02	0.05	1.00	1.00	1.00	1.00
CA	0.13	0.10	0.16	0.56	0.38	1.00	1.00	1.00	1.00	1.00	1.00
CO	0.26	0.26	0.19	0.31	0.15	0.09	0.35	1.00	1.00	1.00	1.00
CT	0.44	0.23	0.23	0.32	0.20	0.10	0.23	0.24	0.28	1.00	1.00
DE	0.05	0.05	0.03	0.04	0.03	0.02	0.04	0.04	0.07	0.12	1.00
FL	0.02	0.09	0.05	0.07	0.05	0.03	0.13	0.15	0.17	1.00	1.00
GA	0.04	0.04	0.06	0.06	0.01	0.02	0.22	1.00	1.00	1.00	1.00
HI	0.07	0.04	0.04	0.20	0.03	0.10	0.25	0.32	1.00	1.00	1.00
IA	0.03	0.06	0.20	0.21	0.36	0.25	0.25	0.30	0.33	1.00	1.00
ID	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.02	1.00	1.00
IL	0.23	0.11	0.08	0.25	0.05	0.07	1.00	1.00	1.00	1.00	1.00
IN	0.04	0.04	0.03	0.06	0.03	0.02	0.22	0.23	1.00	1.00	1.00
KS	0.01	0.02	0.16	0.16	0.07	0.08	0.34	0.42	0.33	1.00	1.00
KY	0.08	0.08	0.05	0.08	0.03	0.03	0.05	0.06	0.06	1.00	1.00
LA	0.02	0.01	0.01	0.05	0.15	0.14	0.19	0.21	0.38	1.00	1.00
MA	0.21	0.46	0.30	0.38	0.51	0.13	0.78	1.00	1.00	1.00	1.00
MD	0.16	0.29	0.29	0.34	0.27	0.12	0.36	0.37	1.00	1.00	1.00
ME	0.01	0.02	0.01	0.10	0.07	0.06	0.07	0.08	0.16	1.00	1.00
MI	0.02	0.01	0.01	0.01	0.01	0.02	0.28	0.28	1.00	1.00	1.00
MN	0.09	0.08	0.06	0.09	0.03	0.04	0.34	1.00	1.00	1.00	1.00
MO	0.02	0.02	0.01	0.07	0.01	0.05	0.08	0.09	0.28	1.00	1.00
MS	0.01	0.02	0.02	0.04	0.01	0.02	0.02	0.02	0.02	1.00	1.00
MT	0.01	0.01	0.01	0.01	0.02	0.01	0.02	0.04	0.04	0.04	0.04
NC	0.03	0.07	0.08	0.53	0.06	0.03	0.11	0.37	1.00	1.00	1.00
ND	0.01	0.01	0.04	0.04	0.06	0.05	0.13	0.13	0.13	0.17	1.00
NE	0.05	0.05	0.19	0.19	0.26	0.19	0.38	0.44	0.47	1.00	1.00
NH	0.05	0.10	0.05	0.08	0.18	0.25	0.17	0.16	0.16	1.00	1.00
NJ	0.06	0.13	0.06	0.10	0.07	0.04	0.15	0.37	1.00	1.00	1.00
NM	0.03	0.03	0.02	0.03	0.02	0.01	0.03	0.03	0.03	1.00	1.00
NV	0.22	0.14	0.14	0.22	0.11	0.08	0.10	0.09	0.13	1.00	1.00
NY	0.14	0.26	0.50	0.61	0.32	0.26	1.00	1.00	1.00	1.00	1.00
OH	0.04	0.05	0.05	0.29	0.02	0.03	0.18	0.19	0.20	1.00	1.00
OK	0.01	0.01	0.01	0.04	0.03	0.05	0.31	0.33	1.00	1.00	1.00
OR	0.03	0.03	0.03	0.28	0.02	0.02	0.11	0.10	0.11	1.00	1.00
PA	0.03	0.13	0.10	0.29	0.07	0.08	0.54	1.00	1.00	1.00	1.00
RI	0.07	0.05	0.05	0.05	0.03	0.04	0.13	0.18	1.00	1.00	1.00
SC	0.02	0.02	0.06	0.06	0.02	0.02	0.03	0.04	0.05	1.00	1.00
SD	0.01	0.01	0.03	0.04	0.05	0.03	0.05	0.05	0.05	0.07	0.27
TN	0.06	0.05	0.05	0.09	0.04	0.03	0.16	0.32	1.00	1.00	1.00
TX	0.05	0.08	0.09	0.34	0.25	0.18	0.44	1.00	1.00	1.00	1.00
UT	0.05	0.05	0.04	0.05	0.03	0.02	0.07	0.07	0.12	1.00	1.00
VA	0.07	0.13	0.06	0.07	0.16	0.04	0.18	0.18	0.18	1.00	1.00
VT	0.01	0.05	0.03	0.04	0.07	0.04	0.07	0.05	0.09	1.00	1.00
WA	0.10	0.05	0.07	0.26	0.08	0.08	1.00	1.00	1.00	1.00	1.00
WI	0.02	0.01	0.06	0.07	0.03	0.02	0.07	0.10	0.24	1.00	1.00
WV	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.02	0.01	0.08
WY	0.04	0.07	0.04	0.10	0.09	0.06	0.13	0.10	0.10	0.10	0.22

ALTERNATIVE WEIGHTS

	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
AK	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.25	0.08
AL	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.08	1.00	1.00
AR	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.04	1.00	1.00
AZ	0.01	0.01	0.01	0.01	0.01	0.01	0.03	1.00	1.00	1.00	1.00
CA	0.02	0.02	0.06	0.27	0.05	1.00	1.00	1.00	1.00	1.00	1.00
CO	0.00	0.00	0.00	0.00	0.00	0.00	0.01	1.00	1.00	1.00	1.00
CT	0.01	0.00	0.00	0.00	0.00	0.00	0.01	0.04	0.31	1.00	1.00
DE	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.03	0.34	1.00
FL	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.08	0.50	1.00	1.00
GA	0.01	0.01	0.01	0.00	0.00	0.01	0.02	1.00	1.00	1.00	1.00
HI	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.09	1.00	1.00	1.00
IA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.15	1.00	1.00
ID	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.03	1.00	1.00
IL	0.01	0.01	0.01	0.03	0.01	0.01	1.00	1.00	1.00	1.00	1.00
IN	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.10	1.00	1.00	1.00
KS	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.06	1.00	1.00
KY	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.04	1.00	1.00
LA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.16	1.00	1.00
MA	0.01	0.02	0.01	0.01	0.02	0.01	0.51	1.00	1.00	1.00	1.00
MD	0.00	0.01	0.01	0.01	0.01	0.01	0.02	0.27	1.00	1.00	1.00
ME	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.02	0.09	1.00	1.00
MI	0.00	0.00	0.00	0.00	0.00	0.00	0.03	0.44	1.00	1.00	1.00
MN	0.00	0.00	0.00	0.01	0.00	0.00	0.07	1.00	1.00	1.00	1.00
MO	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.17	1.00	1.00
MS	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.04	1.00	1.00
MT	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.10	0.24
NC	0.00	0.00	0.00	0.00	0.00	0.00	0.02	0.13	1.00	1.00	1.00
ND	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.02	0.06	1.00
NE	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.03	0.21	1.00	1.00
NH	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.02	0.05	1.00	1.00
NJ	0.00	0.00	0.01	0.00	0.01	0.01	0.01	0.05	1.00	1.00	1.00
NM	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.06	1.00	1.00
NV	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.04	0.19	1.00	1.00
NY	0.00	0.01	0.02	0.05	0.01	0.04	1.00	1.00	1.00	1.00	1.00
OH	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.11	1.00	1.00
OK	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	1.00	1.00	1.00
OR	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.04	0.09	1.00	1.00
PA	0.00	0.00	0.00	0.00	0.00	0.00	0.45	1.00	1.00	1.00	1.00
RI	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.11	1.00	1.00	1.00
SC	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.10	1.00	1.00
SD	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.12	0.47
TN	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.05	1.00	1.00	1.00
TX	0.01	0.01	0.01	0.01	0.01	0.01	0.06	1.00	1.00	1.00	1.00
UT	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.06	1.00	1.00
VA	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.02	0.14	1.00	1.00
VT	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.12	0.02	1.00	1.00
WA	0.00	0.00	0.01	0.01	0.00	0.00	1.00	1.00	1.00	1.00	1.00
WI	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.26	1.00	1.00
WV	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.12	0.29
WY	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.07	0.07

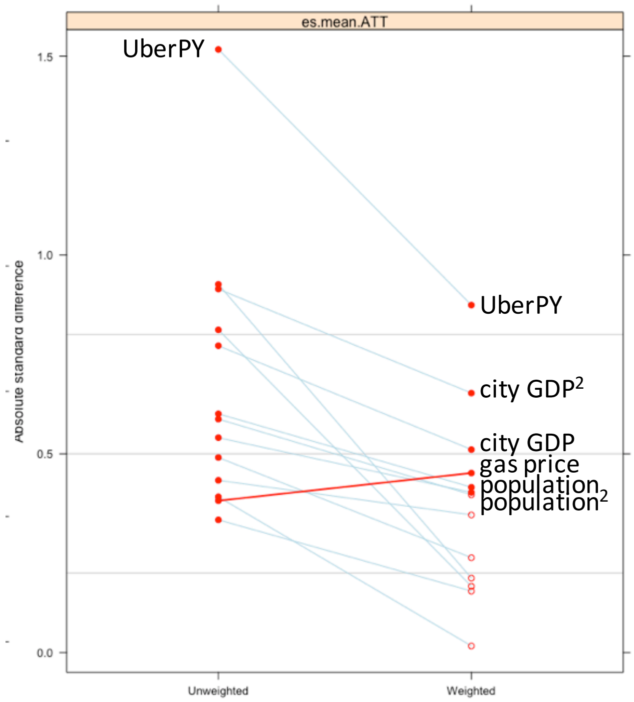


Figure 18. Balance for alternative weights calculated using an additional control in the probability of treatment regression for whether Uber had already entered a state in the prior year (UberPY). While the alternative-weight specification provides a better fit for estimating probability of treatment, the resulting weights do not yield acceptable balance between the treatment and control groups: significant differences remain between treatment and control biggest city GDP, state gasoline price, and state population, even after applying alternative weights. Note that this figure is directly comparable with Figure 2 (balance for base weights) in the main text.

Table 12. A comparison of IPTW regression results using base weights (i.e., those presented and discussed in the main text, *left*) and the alternative weights (*right*) calculated using an additional control in the probability of treatment regression for whether Uber had already entered a state in the prior year (UberPY, which is used to determine probability of treatment in Eq(2) of the main text but is not included in the main regression of Eq(1) itself). The change in weights yields differences in direction, magnitude, and significance for some treatment and control variable coefficients. Differences are the result of different weights (see Table S8), and base-weight results (between balanced control and treatment groups, per Figure S11) are preferred to those

using alternative weights (where improper balance suggests bias may be an issue, again per Figure S11).

	Dependent variable:					
	log(Veh. Reg. per cap)					
	—Base Weights—			*—Alternative Weights—*		
	Binary	Pop. Wt.	Intaxn.	Binary	Pop. Wt.	Intaxn.
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)
treat	−0.031**		−0.035*	−0.011		−0.026*
	(0.012)		(0.018)	(0.010)		(0.015)
treatpop		0.012			0.038	
		(0.027)			(0.027)	
treat:avg			−0.005			0.023
			(0.019)			(0.017)
treat:rural			0.017			0.041**
			(0.020)			(0.020)
lpop	8.286***	8.760***	8.587***	8.023*	9.444**	8.779**
	(2.780)	(2.815)	(2.711)	(4.174)	(4.232)	(4.199)
lpopsq	−0.272***	−0.285***	−0.271***	−0.241*	−0.287**	−0.252*
	(0.093)	(0.094)	(0.090)	(0.134)	(0.136)	(0.136)
gas	0.001	0.002	−0.004	−0.009	−0.010	−0.012**
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
inc	0.011	0.014	0.017	0.055***	0.062***	0.054***
	(0.011)	(0.011)	(0.013)	(0.018)	(0.019)	(0.018)
incsq	−0.0002**	−0.0002**	−0.0003***	−0.0005***	−0.001***	−0.001***
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
s177	−0.058**	−0.057**	−0.056**	−0.020	−0.016	−0.012
	(0.024)	(0.024)	(0.025)	(0.025)	(0.024)	(0.025)
lcitypop	−0.380	−0.394	−0.151	−1.402**	−1.343**	−1.145**
	(0.254)	(0.256)	(0.274)	(0.571)	(0.573)	(0.521)
lcitypopsq	0.063	0.053	0.112	−0.066	−0.056	−0.086
	(0.073)	(0.072)	(0.075)	(0.096)	(0.100)	(0.088)
citydensity	0.102***	0.107***	0.094***	0.206***	0.209***	0.190***
	(0.033)	(0.034)	(0.035)	(0.055)	(0.055)	(0.054)
lcitygdp	−0.278	−0.294	−0.298	0.056	0.093	0.217
	(0.179)	(0.180)	(0.189)	(0.254)	(0.244)	(0.248)
lcitygdpsq	−0.037*	−0.034*	−0.047**	0.011	0.028	0.018
	(0.020)	(0.020)	(0.021)	(0.038)	(0.037)	(0.037)
pop_u2	4.646***	4.838***		5.926**	6.092**	
	(0.985)	(0.987)		(2.507)	(2.480)	
katrina	0.103**	0.098**	0.110**	0.225***	0.226***	0.193**
	(0.048)	(0.049)	(0.053)	(0.082)	(0.081)	(0.077)
sandy	0.086	0.091	0.083*	0.030**	0.031**	0.028***
	(0.053)	(0.065)	(0.050)	(0.012)	(0.014)	(0.011)
clunkers	−6.057	−7.704	−6.998	−56.690	−60.265	−51.068
	(24.834)	(25.322)	(25.870)	(43.043)	(43.667)	(43.256)
avg			−0.097			0.536**
			(0.130)			(0.216)
rural			1.974***			4.296***
			(0.696)			(1.137)
Constant	−64.791***	−69.001***	−69.504***	−68.793**	−79.787**	−78.484**
	(20.590)	(20.812)	(20.491)	(31.657)	(32.199)	(32.651)
Observations	550	550	550	550	550	550
Adjusted R^2	0.838	0.835	0.831	0.954	0.954	0.952
Note:				*p<0.1; **p<0.05; ***p<0.01		

Table 13. Sensitivities of IPTW regression results using the logarithm of per-capita vehicle registrations as the dependent variable to a sequence of potentially relevant alternative normalizations or additional control variables, as follows: vehicle registrations are normalized per licensed driver instead of per capita ("Per license"); vehicle registrations are normalized per urban population instead of per capita ("Per urban pop."); states are dropped if Uber entered during the Great Recession (i.e., California, "Recession entry"); the regression is repeated with a shortened time frame (2009–2015) that more closely matches only years of Uber entry ("Time frame"); an indicator is added for states and years where and when Uber offered leasing incentives during the analysis period (Uber's Vehicle Solutions program in CA, GA, and MD in

2014 and 2015; the program was later expanded and became Xchange Leasing, "Uber leasing"); an indicator is added for Lyft market entry ("Lyft"), and a covariate is added for per-capita transit ridership ("Transit"). In all cases, vehicle registrations are consistently estimated to decline.

	Dependent variable: log(Vehicle Registrations), per capita†						
	†Per license	†Per urban pop.	Recession entry	Time frame	Uber leasing	Lyft	Transit
treat	−0.030**	−0.037**	−0.032***	−0.027**	−0.035***	−0.031**	−0.021*
	(0.013)	(0.014)	(0.011)	(0.012)	(0.012)	(0.013)	(0.013)
Observations	550	550	539	400	550	550	350
Adjusted R^2	0.715	0.845	0.845	0.855	0.839	0.838	0.844
treatpop	0.040	0.012	0.011	0.006	0.008		0.024
	(0.029)	(0.030)	(0.027)	(0.025)	(0.027)		(0.023)
Observations	550	550	539	400	550		350
Adjusted R^2	0.713	0.842	0.842	0.852	0.836		0.843
treat	−0.037*	−0.043**	−0.036**	−0.024	−0.038**	−0.034*	−0.017
	(0.019)	(0.021)	(0.017)	(0.021)	(0.018)	(0.019)	(0.019)
I(treat*middle)	0.018	−0.008	−0.006	0.016	−0.006	0.011	−0.021
	(0.021)	(0.022)	(0.019)	(0.019)	(0.019)	(0.017)	(0.020)
I(treat*rural)	0.045	0.037	0.021	−0.002	0.016	−0.022	0.015
	(0.022)	(0.026)	(0.020)	(0.021)	(0.020)	(0.033)	(0.022)
Observations	550	550	539	400	550	550	350
Adjusted R^2	0.709	0.835	0.840	0.851	0.832	0.831	0.844
Notes:	†per capita except as noted per licensed driver or per urban population						
	*p<0.1; **p<0.05; ***p<0.01						

Table 14. Comparison of main text model with a variant that does not normalize the dependent variables by population (i.e., the dependent variable is the logarithm of vehicles instead of the logarithm of vehicles per capita). I find the only change is a one-unit increase in the estimated coefficient on the population control variable; no other estimates are affected. Coefficients for treatment and population are shown; other estimates (covariates and fixed effects) are excluded.

	Dependent variable:	
	log(Veh. Reg)	
	per capita	not normalized
	(1)	(2)
treat	−0.028**	−0.028**
	(0.013)	(0.013)
lpop	9.029***	8.029***
	(2.906)	(2.906)
lpopsq	−0.292***	−0.292***
	(0.097)	(0.097)
Observations	550	550
Adjusted R^2	0.848	0.997
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 15. Regression results using a one- and two-year lagged indicator for Uber treatment to examine potential delayed-onset treatment effects. Per-capita vehicle registration and VOC emission effects described as significant in the main text are comparable in magnitude (confidence intervals overlap); though, level of significance generally declines, which can be

expected as the number of treated observations falls by ~40% and 70% (from n=124 to 79 and 36) when treatment is lagged by one and two years, respectively.

	Dependent variable:			
	log(veh reg per cap)		log(VOC per cap)	
	1-year lag	2-year lag	1-year lag	2-year lag
Model (1)				
Average Effect	−0.016	−0.021	−0.014	0.009
	(0.010)	(0.014)	(0.012)	(0.016)
Observations	550	550	550	550
Deg. Freedom	474	474	474	474
Adj. R-Sq.	0.863	0.913	0.972	0.972
Model (2)				
Urban Effect	−0.015	−0.012	0.007	0.027
	(0.015)	(0.016)	(0.019)	(0.022)
Middle-vs.-Urban	0.001	−0.002	−0.026	−0.019
	(0.014)	(0.014)	(0.021)	(0.025)
Rural-vs.-Urban	−0.002	−0.065	−0.058***	−0.094***
	(0.021)	(0.042)	(0.022)	(0.030)
Middle Effect†	−0.015	−0.014	−0.019	0.008
	(0.010)	(0.011)	(0.014)	(0.018)
Rural Effect†	−0.018	−0.078*	−0.052***	−0.067**
	(0.016)	(0.040)	(0.015)	(0.026)
Observations	550	550	550	550
Deg. Freedom	472	472	472	472
Adj. R-Sq.	0.863	0.914	0.972	0.972
Note:				*p<0.1; **p<0.05; ***p<0.01
				†computed post-hoc and not directly estimated

Benjamini-Hochberg Correction: Because I test five types of dependent variables (vehicle registrations, gasoline use, VMT, EPA-estimated emissions, and EIA-estimated emissions), I employ a Benjamini-Hochberg correction for testing multiple hypotheses. When correcting for the 5 families of dependent variables, I find my per-capita vehicle registration and VOC emissions results are robust to false detection rates as low as 2.6%. When correction for all 44 potential dependent variables (given the 7 types of EPA-estimated emissions series as well as interactions tested), I find my results are robust to false detection rates as low as 4.6%.

Table 16. Benjamini-Hochberg correction for testing multiple hypotheses showing the estimated effect of TNC entry on per-capita VOCs and Vehicle Registrations is robust to a false discovery rate as low as 2.6% for 5 unique types dependent variables (i.e., 5 average effects from unique sources), 3.8% for 11 unique dependent variables (i.e., 11 total average effects estimated), and 4.6% for consider the full set of 44 effects estimated (interactions included).

	Estimate	Std. Error	t value	Pr(>ltl)	robust. checks?	p-value rank	Critical values as f(FDR*)				min. FDR*
							1%	2%	5%	10%	
VOC - average	**-4.8%**	**1.7%**	**-2.83**	**0.005**	**Y**	**1**	**0.002**	**0.004**	**0.010**	**0.020**	**2.4%**
Registrations - average	**-3.1%**	**1.2%**	**-2.57**	**0.010**	**Y**	**2**	**0.004**	**0.008**	**0.020**	**0.040**	**2.6%**
VMT - average	-0.3%	0.3%	-1.00	0.316	*N*	3	0.006	0.012	0.030	0.060	52.7%
GHG - average	0.7%	0.8%	0.89	0.375	*N*	4	0.008	0.016	0.040	0.080	46.9%
Gasoline Use - average	0.3%	0.4%	0.79	0.428	*N*	5	0.010	0.020	0.050	0.100	42.8%

PM10 - treat:mid	-13.6%	3.9%	-3.490	0.001	*N*	1	0.000	0.000	0.001	0.002	2.3%
VMT - treat:rural	-1.9%	0.6%	-3.300	0.001	*N*	2	0.000	0.001	0.002	0.005	2.3%
NOx - treat:rural	-7.9%	2.5%	-3.217	0.001	*N*	3	0.001	0.001	0.003	0.007	2.0%
PM25 - treat:mid	-9.9%	3.1%	-3.216	0.001	*N*	4	0.001	0.002	0.005	0.009	1.5%
CO - treat:rural	-7.0%	2.3%	-3.067	0.002	*N*	5	0.001	0.002	0.006	0.011	2.0%
NH3 - average	-2.7%	1.0%	-2.863	0.004	*N*	6	0.001	0.003	0.007	0.014	3.2%
VOC - average	**-4.8%**	**1.7%**	**-2.828**	**0.005**	**Y**	**7**	**0.002**	**0.003**	**0.008**	**0.016**	**3.1%**
PM10 - urban	9.1%	3.4%	2.703	0.007	*N*	8	0.002	0.004	0.009	0.018	3.9%
PM10 - treat:rural	-10.6%	4.0%	-2.630	0.009	*N*	9	0.002	0.004	0.010	0.020	4.3%
Registrations - average	**-3.1%**	**1.2%**	**-2.569**	**0.010**	**Y**	**10**	**0.002**	**0.005**	**0.011**	**0.023**	**4.6%**
PM25 - treat:rural	-7.5%	3.1%	-2.392	0.017	*N*	11	0.003	0.005	0.013	0.025	6.9%
NH3 - urban	-2.7%	1.2%	-2.258	0.024	*N*	12	0.003	0.005	0.014	0.027	8.9%
VOC - treat:rural	-4.4%	2.3%	-1.921	0.055	*N*	13	0.003	0.006	0.015	0.030	18.7%
PM25 - urban	4.3%	2.3%	1.866	0.063	*N*	14	0.003	0.006	0.016	0.032	19.7%
VMT - treat:mid	-0.9%	0.5%	-1.854	0.064	*N*	15	0.003	0.007	0.017	0.034	18.9%
VOC - urban	-3.7%	2.1%	-1.806	0.072	*N*	16	0.004	0.007	0.018	0.036	19.7%
SO2 - average	3.1%	1.8%	1.740	0.082	*N*	17	0.004	0.008	0.019	0.039	21.3%
CO - average	-3.2%	1.8%	-1.736	0.083	*N*	18	0.004	0.008	0.020	0.041	20.4%
Gasoline Use - treat:mid	0.9%	0.6%	1.569	0.117	*N*	19	0.004	0.009	0.022	0.043	27.2%
Registrations - urban	-2.8%	1.8%	-1.563	0.119	*N*	20	0.005	0.009	0.023	0.045	26.1%
NOx - treat:mid	-3.6%	2.4%	-1.492	0.136	*N*	21	0.005	0.010	0.024	0.048	28.6%
NOx - average	-1.8%	1.3%	-1.437	0.152	*N*	22	0.005	0.010	0.025	0.050	30.3%
PM10 - average	2.9%	2.0%	1.422	0.156	*N*	23	0.005	0.010	0.026	0.052	29.8%
NH3 - treat:rural	-1.8%	1.4%	-1.279	0.202	*N*	24	0.005	0.011	0.027	0.055	37.0%
Gasoline Use - treat:rural	0.9%	0.7%	1.216	0.224	*N*	25	0.006	0.011	0.028	0.057	39.5%
CO - treat:mid	-2.5%	2.1%	-1.178	0.239	*N*	26	0.006	0.012	0.030	0.059	40.5%
VMT - average	-0.3%	0.3%	-1.003	0.316	*N*	27	0.006	0.012	0.031	0.061	51.5%
SO2 - treat:rural	3.4%	3.5%	0.966	0.335	*N*	28	0.006	0.013	0.032	0.064	52.6%
GHG - average	0.7%	0.8%	0.887	0.375	*N*	29	0.007	0.013	0.033	0.066	57.0%
VMT - urban	0.3%	0.4%	0.866	0.387	*N*	30	0.007	0.014	0.034	0.068	56.7%
SO2 - urban	2.2%	2.6%	0.862	0.389	*N*	31	0.007	0.014	0.035	0.070	55.3%
Gasoline Use - average	0.3%	0.4%	0.794	0.428	*N*	32	0.007	0.015	0.036	0.073	58.8%
NH3 - treat:mid	0.8%	1.1%	0.760	0.447	*N*	33	0.008	0.015	0.038	0.075	59.7%
GHG - urban	0.5%	0.9%	0.625	0.533	*N*	34	0.008	0.015	0.039	0.077	68.9%
CO - urban	-1.2%	2.2%	-0.544	0.587	*N*	35	0.008	0.016	0.040	0.080	73.8%
Registrations - treat:mid	-1.0%	1.9%	-0.525	0.600	*N*	36	0.008	0.016	0.041	0.082	73.3%
VOC - treat:mid	-1.1%	2.1%	-0.525	0.600	*N*	37	0.008	0.017	0.042	0.084	71.3%
GHG - treat:rural	0.5%	1.2%	0.432	0.666	*N*	38	0.009	0.017	0.043	0.086	77.1%
NOx - urban	0.7%	1.8%	0.358	0.720	*N*	39	0.009	0.018	0.044	0.089	81.3%
SO2 - treat:mid	1.0%	2.8%	0.358	0.721	*N*	40	0.009	0.018	0.045	0.091	79.3%
Gasoline Use - urban	-0.1%	0.4%	-0.283	0.777	*N*	41	0.009	0.019	0.047	0.093	83.4%
Registrations - treat:rural	0.5%	2.1%	0.249	0.803	*N*	42	0.010	0.019	0.048	0.095	84.1%
GHG - treat:mid	0.2%	1.0%	0.166	0.868	*N*	43	0.010	0.020	0.049	0.098	88.9%
PM25 - average	-0.2%	1.6%	-0.128	0.898	*N*	44	0.010	0.020	0.050	0.100	89.8%

2.8.4. Mechanical Robustness Checks

This section tests for regression model robustness using visual inspection of regression model residual errors, permutation test using iterative resampling, leave-one-out analysis, and comparative cross-validation.

<u>Regression residual errors</u>

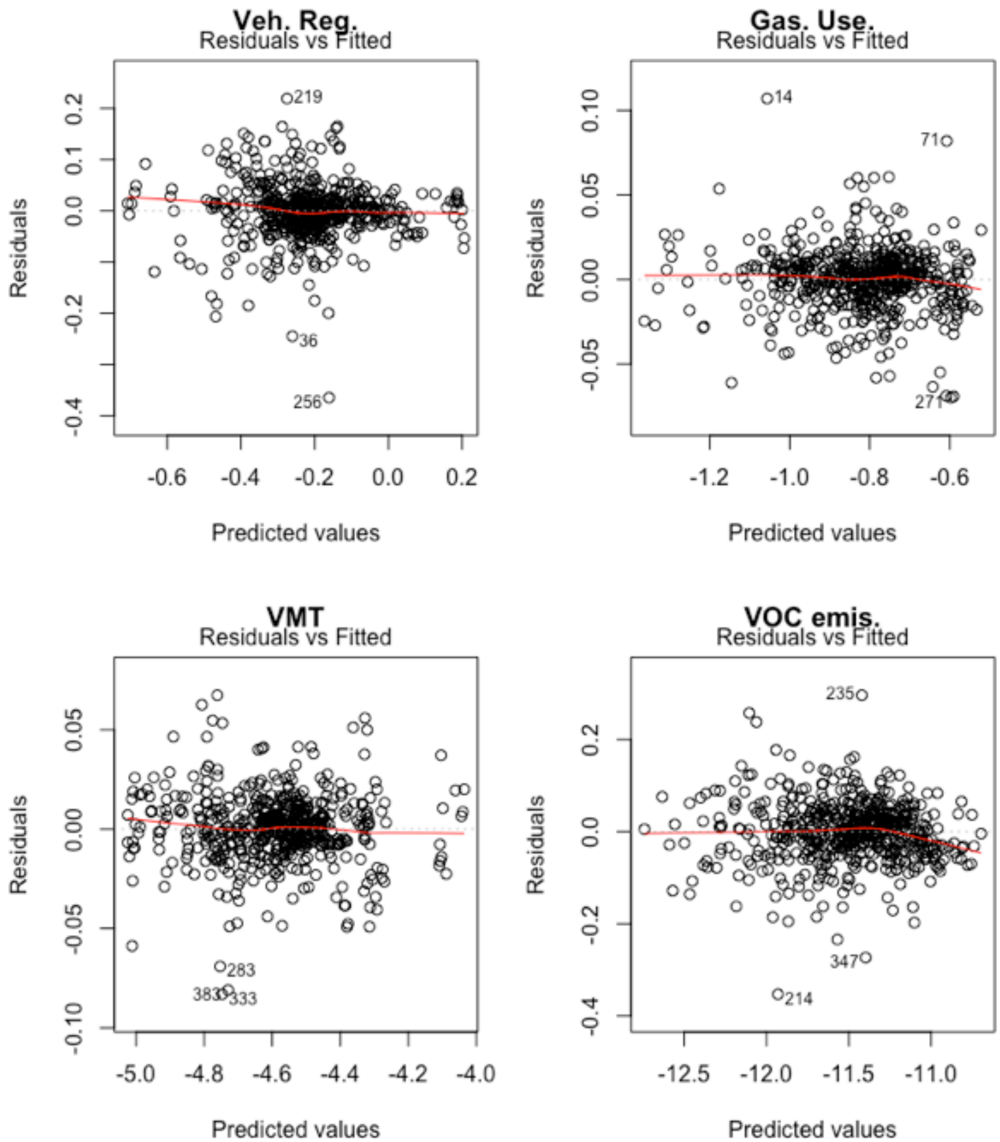
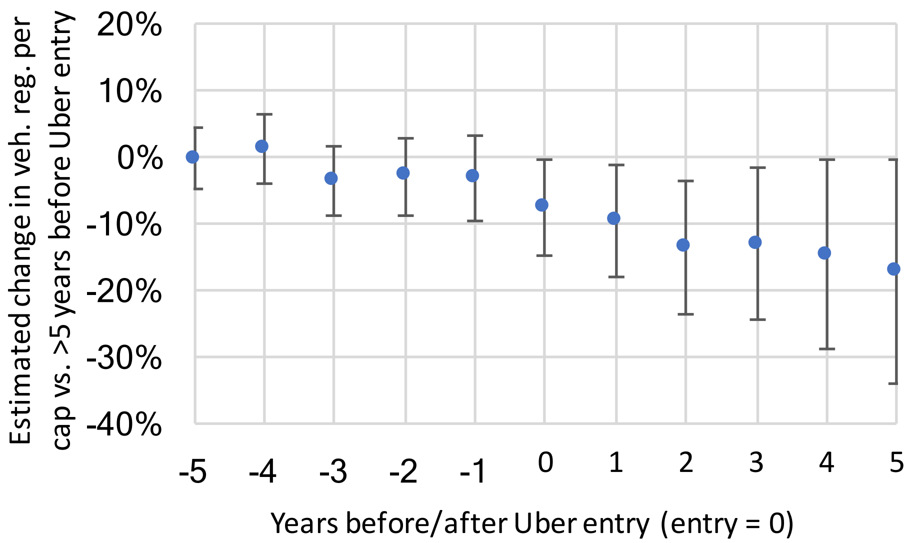


Figure 19. Plots showing residual errors versus fitted values for the binary specification of the IPTW regression model, using as the dependent variable per-capita vehicle registrations (*upper-left*), gasoline use (*upper-right*), VMT (*lower-left*), and VOC emissions (*lower-right*), respectively. Results show that there is no obvious structure to the distribution of residual errors, suggesting that there is not evidence here that the model is mis-specified.

<u>Event Study:</u> An Event Study is a statistical method for estimated the effect of a treatment (e.g., Uber market entry) in the context of related effects both before and after treatment occurs. By explicitly quantifying effects both before and after treatment, these studies are useful to ensure that any effect attributed to treatment does not precede the treatment, such that a pre-treatment effect could be misattributed to the treatment in a model without explicit before-and-after context. I find no evidence of an effect on vehicle registrations or VOC emissions in the years leading to Uber market entry, and I find a statistically significant effect for each at a point after Uber entry with the same sign as the effect identified in the difference-in-difference model.



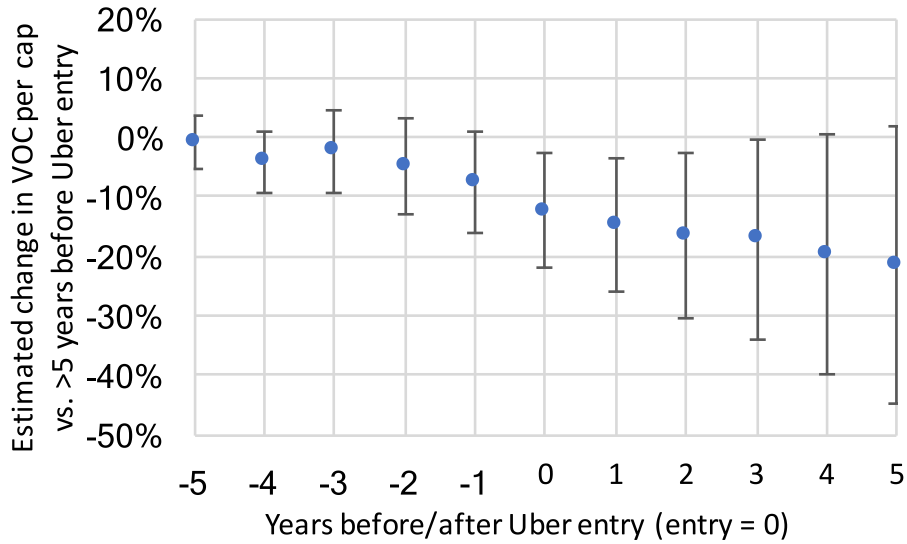


Figure 20. Event study showing the estimated change in vehicle registrations per capita (*top*) and the estimated change in VOC emissions per capita (*bottom*) from five years prior to Uber entry to five years after Uber entry. These estimates are generated using the same IPTW model as is described in the main text with the addition of a suite of indicator variables to capture the number of years before/after Uber market entry for each observation. The trend above suggests that no significant decline in per-capita vehicle registrations occurs until the year of Uber entry

and is significantly lower (compared to the period more than five years prior to Uber entry) after that.

Table 17. Annual pre- and post-treatment effect estimates for the event study, corresponding to the depiction in Figure S13 above.

	Dependent variable: log, per capita	
	Veh. Reg.	VOC
5 years prior	−0.005	−0.010
	(0.023)	(0.023)
4 years prior	0.01	−0.042
	(0.027)	(0.026)
3 years prior	−0.038	−0.024
	(0.026)	(0.036)
2 years prior	−0.032	−0.049
	(0.030)	(0.041)
1 year prior	−0.034	−0.077*
	(0.033)	(0.043)
Uber entry	−0.077**	−0.124**
	(0.036)	(0.049)
1 years after	−0.098**	−0.147**
	(0.042)	(0.058)
2 years after	−0.137***	−0.165**
	(0.051)	(0.070)
3 years after	−0.133**	−0.172**
	(0.058)	(0.086)
4 years after	−0.148**	−0.198*
	(0.072)	(0.103)
5 years after	−0.174**	−0.216*
	(0.086)	(0.119)
Observations	550	550
Adjusted R-Sq.	0.983	0.983
Note:	*p<0.1; **p<0.05; ***p<0.01	

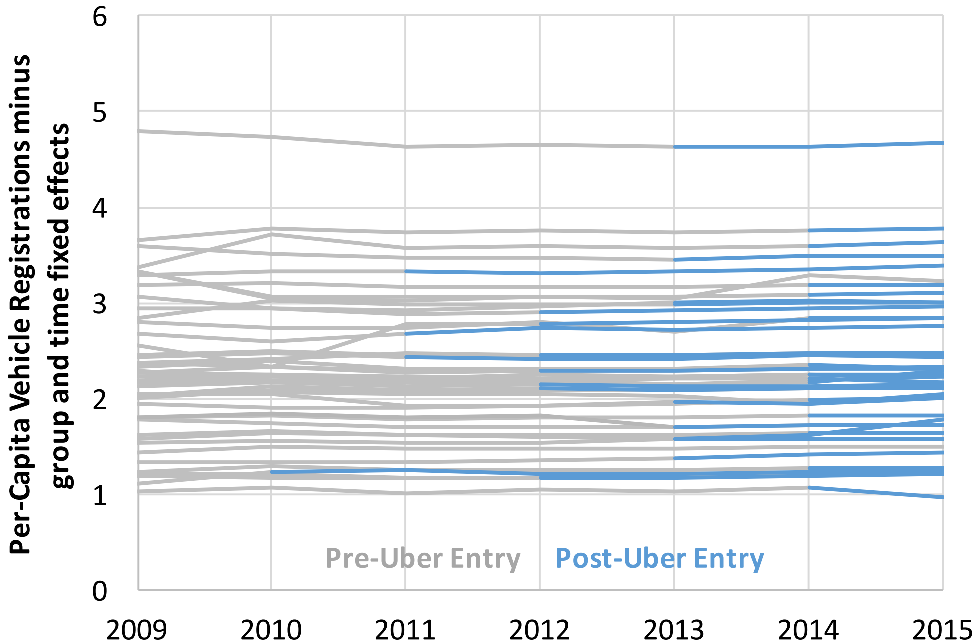


Figure 21. Plot over time of per-capita vehicle registration data with IPTW regression-estimated group and time fixed effects removed. A comparison of data from before Uber entry (in gray) with data after Uber entry (in blue) shows no clear evidence of a violation of the difference-in-difference model's parallel trends assumption.

Linear Time Trends

Table 18. I estimate an additional model that includes linear time trends for each state in order to control for distinct trends in each state, and I find that my estimated effect of TNC entry on vehicle registrations are relatively robust to this specification change: the magnitude of the vehicle registration effect declines slightly to -2.1%, and the p-value increases to 0.08. The VOC effect also declines slight to 4.3% but maintains significance. Coefficient estimates for treatment are shown; other estimates (i.e., covariates and fixed effects) are excluded.

	log(Veh. Reg. per cap)		*log(VOC per cap)*	
	base model	w/time trends	base model	w/time trends
	(1)	(2)	(3)	(4)
treat	-0.028^{**}	-0.021^{*}	-0.048^{***}	-0.043^{***}
	(0.013)	(0.012)	(0.014)	(0.017)
Observations	550	550	550	550
Adjusted R^2	0.848	0.894	0.965	0.974
Note:			*p<0.1; **p<0.05; ***p<0.01	

<u>Permutation test using iterative resampling</u>

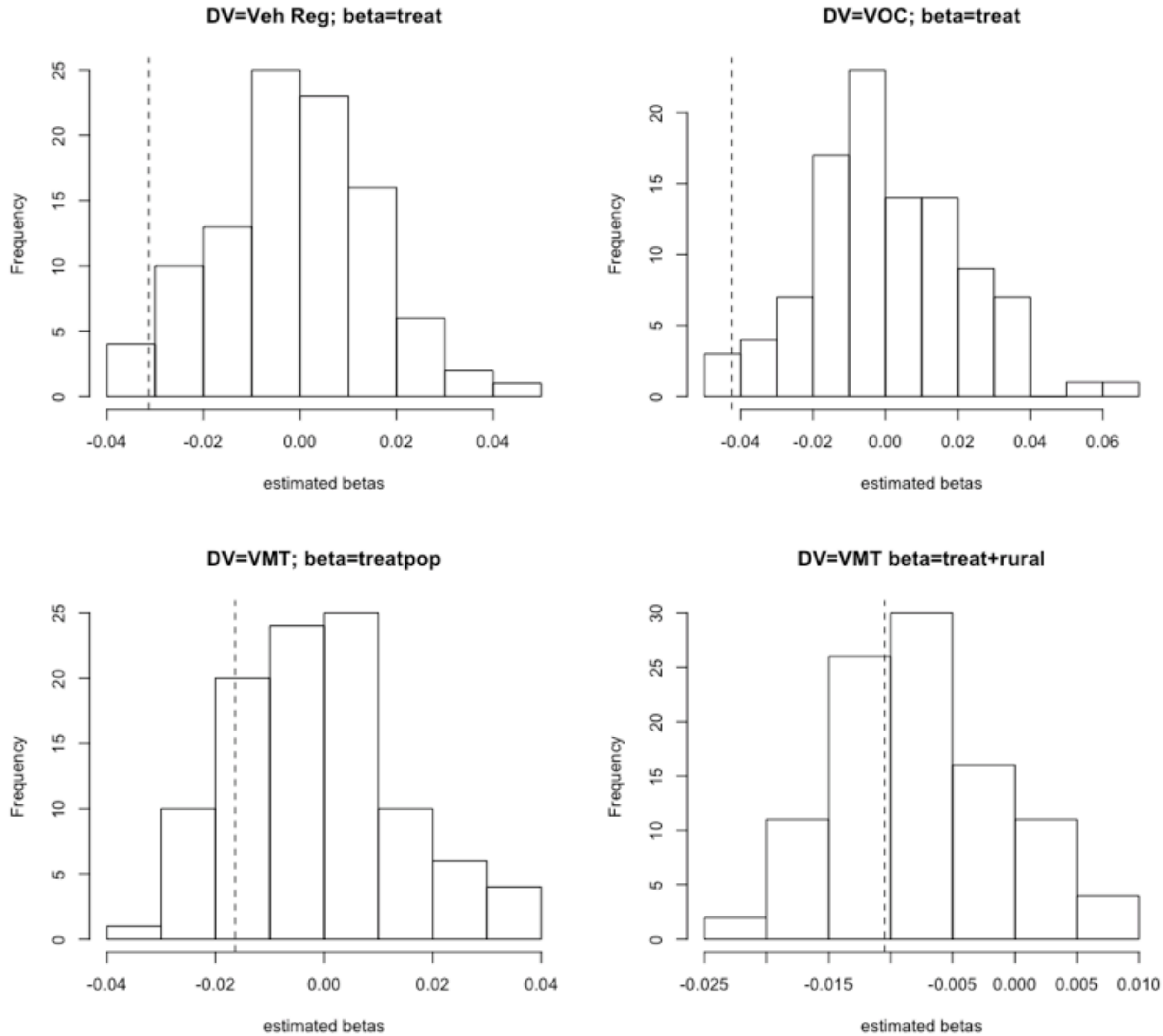


Figure 22. A set of histograms showing the results of the randomized treatment robustness check for the estimated effect ("beta") of treatment on the dependent variable ("DV") per-capita vehicle registrations (*upper-left*), where the true IPTW estimate, reflected by the vertical dotted line, is unlikely to have been generated by chance and the estimated effect of treatment on per-capita VOC emissions (*upper-right*), where the true IPTW estimate is again unlikely to have been generated by chance. In addition, two effects that do not pass this robustness check are included in contrast with those that do for illustrative purposes: the estimated effect of population-weighted treatment on per-capita VMT (*lower-left*), where the true IPTW estimate is not clearly significantly different from a randomly estimated effect, and the estimated combined effect of treatment in a rural area on per-capita VMT (*lower-right*), where the true IPTW estimate aligns with other estimates generated by chance. More specifically in the case of the estimated effect of treatment on per-capita vehicle registrations, 100 samples with randomized treatments subjected to the IPTW regression model yields the distribution shown, where the

mean estimated effect is zero, and only four estimates resulting from randomized treatment are beyond the effect estimated using actual treatment.

Leave-one-out analysis

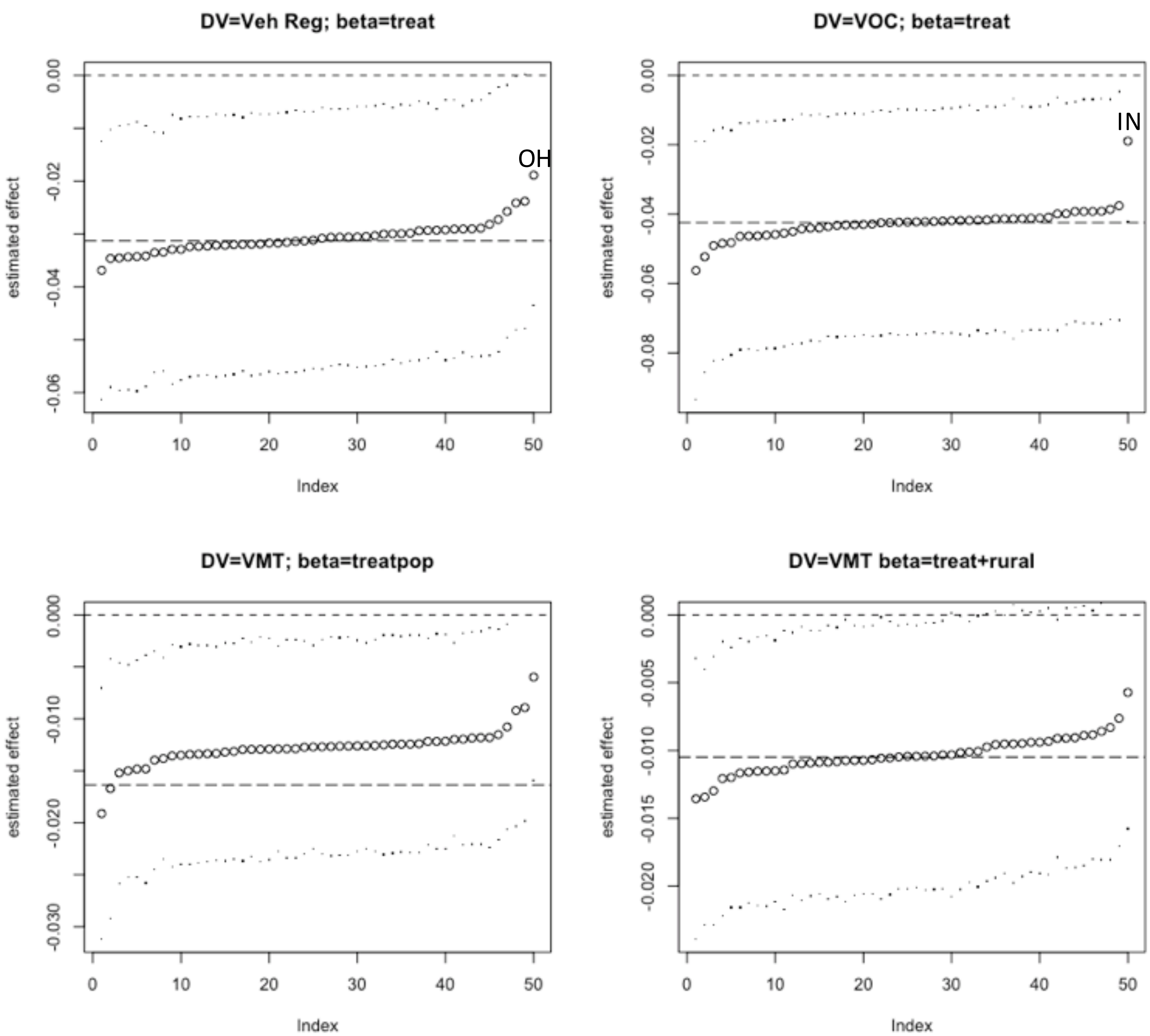


Figure 23. A set of histograms showing the results of the leave-one-out robustness check for the estimated effect ("beta") of treatment on the dependent variable ("DV") when the regression is performed separately without each state (results in order of the size of the estimated effect); the estimated effect of treatment on per-capita vehicle registrations (*upper-left*), where the true IPTW estimate, reflected by the horizontal dotted line near the median of the distribution is statistically significant when any one state is removed except Ohio, which produces estimates very close to the significance threshold and the estimated effect of treatment on per-capita VOC emissions (*upper-right*), where the true IPTW estimate is affected when Indiana is left out. In addition, two effects that do not pass this robustness check are included in contrast with those that do for illustrative purposes: the estimated effect of population-weighted treatment on per-

capita VMT (*lower-left*), where the true IPTW estimate is affected by a few states, and the estimated combined effect of treatment in a rural area on per-capita VMT (*lower-right*), where the true IPTW estimate is affected by several states. More specifically in the case of the estimated effect of treatment on per-capita vehicle registrations, 50 samples leaving one state out each is subjected to the IPTW regression model and yields the distribution shown, where the mean estimated effect is the median of the distribution, and only Ohio causes a sufficient shift to significantly affect the estimated effect.

Leave-multiple-out (discontinuity) analysis: For my four outcomes of interest (per-capita vehicle registrations, gasoline use, VMT, and VOC emissions), the dataset contains instances of unexpectedly large year-to-year changes (for example, Colorado's vehicle registrations jump from 1.6 million in 2010 to 4.2 million in 2011, reflecting a change in that state's reporting, of which U.S. DOT is aware), which I define an increase or decreases of more than 15% from one year to the next. In this section, I identify these discontinuities and repeat regression analysis without states exhibiting them to ensure they are not unduly influencing the result.

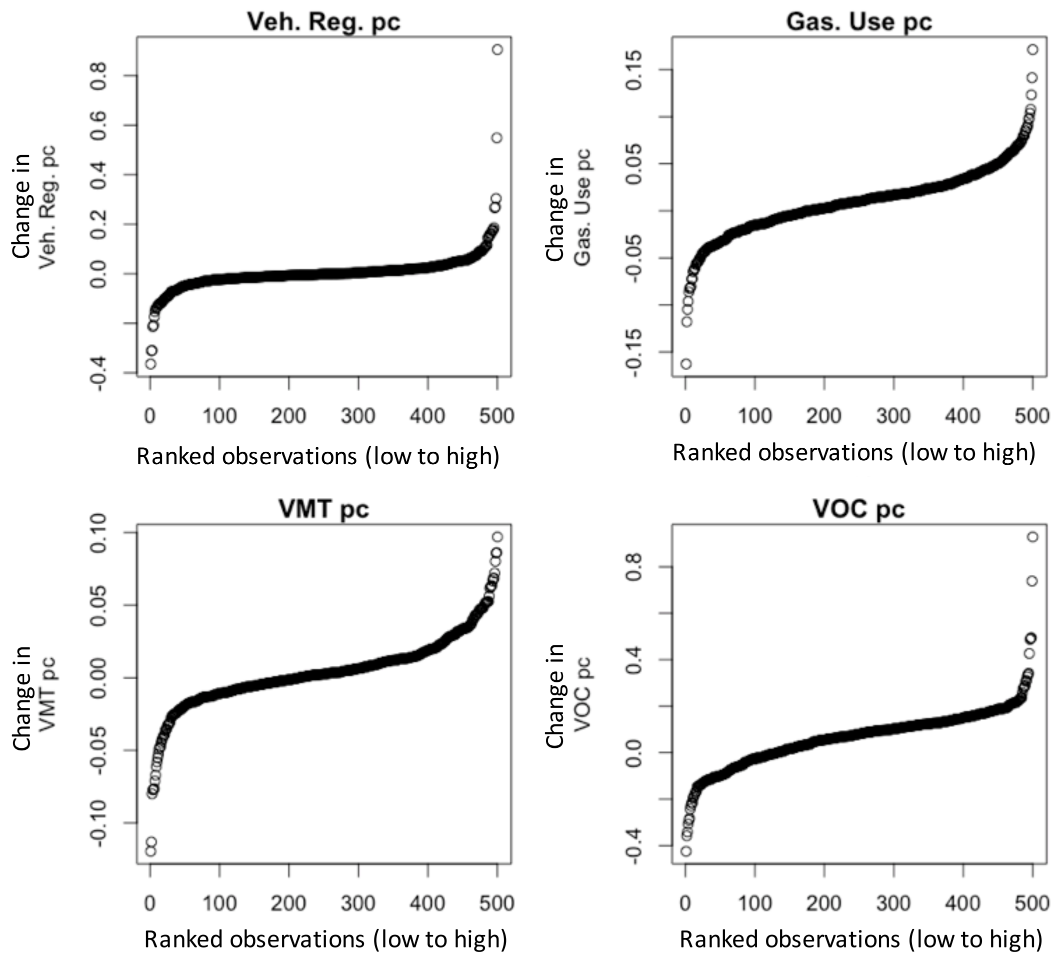


Figure 24. Sorted distribution plots comparing year-to-year changes in each of the dependent variables of interest (*clockwise, from upper-left*), vehicle registrations, gasoline use, VOC emissions, and VMT, all on a per-capita basis. Observations in the tails suggest relatively large

single-year changes (i.e., increase or decreases in per capita vehicle registrations more than 15% from one year to the next), indicating the presence of time-series discontinuities of potential concern. Regression analysis is repeated without states exhibiting these discontinuities to ensure they are not unduly influencing the result, and, as Table S16 below shows, per-capita vehicle registration and VOC emissions effects are robust to their exclusion.

Table 19. Repeating the regression leaving out all states that contain discontinuities in dependent variables of interest based on visual inspection of the distributions in Figure S13 above. For the specific case of per-capita vehicle registrations, this is defined as any single year-on-year change in absolute value greater than 0.15, which is the case for CO, DE, MT, NV, UT, and WY. For gasoline use, AK, HI, LA, and ME are excluded; for VMT, AL and NV are excluded, and for VOC emissions, California, Hawaii, Massachusetts, and New Jersey are excluded. Regression results excluding those results in the remaining states continue to yield statistically significant results (where results were previously significant and robust) with changes of no more than 10% in the magnitude of estimated TNC effects.

	Dependent variable: logarithm, per-capita			
	Veh. Reg.	Gas. Use	VMT	VOC
treat	−0.032***	−0.002	−0.001	−0.046**
	(0.011)	(0.004)	(0.003)	(0.020)
Observations	462	506	528	506
Adjusted R^2	0.911	0.985	0.991	0.948
treatpop	−0.003	−0.002	−0.014***	−0.015
	(0.024)	(0.007)	(0.006)	(0.020)
Observations	462	506	528	506
Adjusted R^2	0.908	0.985	0.991	0.947
treat	−0.032*	−0.000	0.000	−0.023**
	(0.018)	(0.004)	(0.003)	(0.026)
I(treat*middle)	−0.004	0.007	0.000	−0.022
	(0.019)	(0.006)	(0.004)	(0.022)
I(treat*rural)	0.009	−0.004	−0.013***	−0.068***
	(0.020)	(0.006)	(0.005)	(0.024)
Observations	462	506	528	506
Adjusted R^2	0.910	0.985	0.991	0.948
Notes:				*p<0.1; **p<0.05; ***p<0.01

2.8.5. Encoding Robustness Checks

This section tests for robustness against parameter encoding by replicating regression results after changing the method for assigning annual indicators representing Uber entry date and using UberX entry dates instead of Uber entry dates.

Changing Uber entry date

Table 20. Regression model coefficients using an adjusted measure of annual Uber treatment. In the main text, Uber market entry dates are binned directly into the year in which Uber enters (i.e., Uber entry in 2015 is categorized as a 2015 entry regardless of entry month); whereas, in this table, Uber entry is categorized using a break between June and July (e.g., a January 2014 Uber entry would be categorized as 2014, but a December 2014 entry would fall into 2015).

	log(Veh. Reg pc)		*log(Gas. Use pc)*		*log(VMT pc)*		*log(VOC pc)*	
	6 months	UberX	6 months	UberX	6 months	UberX	6 months	UberX
treat	−0.030***	−0.037***	−0.003	−0.009**	−0.002	−0.003	−0.027*	0.002
	(0.011)	(0.013)	(0.004)	(0.005)	(0.003)	(0.005)	(0.014)	(0.014)
Observations	550	550	550	550	550	550	528	550
Adjusted R^2	0.866	0.878	0.985	0.983	0.989	0.988	0.964	0.961
treat	−0.038**	−0.042*	−0.003	−0.011**	−0.001	0.004	−0.009	−0.016
	(0.016)	(0.017)	(0.004)	(0.006)	(0.004)	(0.006)	(0.018)	(0.023)
I(treat*middle)	0.010	0.001	0	0.004	−0.003	−0.008	−0.016	0.023
	(0.015)	(0.016)	(0.006)	(0.008)	(0.005)	(0.006)	(0.020)	(0.026)
I(treat*rural)	0.019	0.029	−0.001	−0.003	−0.003	−0.011*	−0.081**	0.035
	(0.020)	(0.020)	(0.007)	(0.009)	(0.006)	(0.006)	(0.022)	(0.034)
Observations	550	550	550	550	550	550	550	550
Adjusted R^2	0.860	0.868	0.985	0.983	0.988	0.988	0.965	0.961

Notes: *p<0.1; **p<0.05; ***p<0.01

2.8.6. Social Costs

External costs are calculated based on "Economic Values Used for Benefits Computations" estimates published in the Final Regulatory Impact Analysis for Corporate Average Fuel Economy for MY2017–MY2025 Passenger Cars and Light Truck by the National Highway Traffic and Safety Administration (NHTSA)[40]. That analysis estimates that 1) the emission damage costs of VOCs are $1,700/ton, 2) the economic benefit of avoiding price shocks by reducing oil imports is an additional $0.197 per gallon, and 3) the external costs from additional automobile and light truck use—including congestion, accidents, and noise—are $0.081 and $0.078 per vehicle-mile, respectively (an average of $0.080 is used here).

The 4.8% reduction in per-capita VOC emissions reported in the main text (95% confidence interval: 1.5% to 8.2%) is converted to total VOC emissions avoided by multiplying by state populations and state emissions, both as reported in DOT's State Statistical Abstracts[41], in all states and times after Uber market entry. Similarly, the [not significant] 0.3% (–0.5% to 1.1%) and –0.3% (–0.9% to 0.3%) estimated effects on per-capita gasoline use and VMT are converted to total gasoline not consumed and VMT not traveled by multiplying each effect by state populations and state gasoline consumption or VMT, respectively, in states and times after Uber market entry.

Using DOT's per-mile estimates of the externality costs of air pollutant emissions, I estimate that VOC emissions avoided represent external cost savings of $600 million ($300 million to $900 million) over the analysis period. Additionally, even though I do not find that TNC service availability causes a significant effect on either gasoline use or VMT, associated external costs could increase or more than counterbalance the external benefits from VOC emissions reductions. Again, using DOT's estimates of the externality costs of driving from congestion, accidents, and noise travel distance effects, I calculate that possible external benefits of TNC-induced changes to VMT could range from -$1 billion to +$7 billion. And, using estimates for the price shock benefits of reducing gasoline imports, I calculate that potential external benefits of TNC-induced gasoline consumption reduction could range from $800 million in costs to $400 million in benefits.

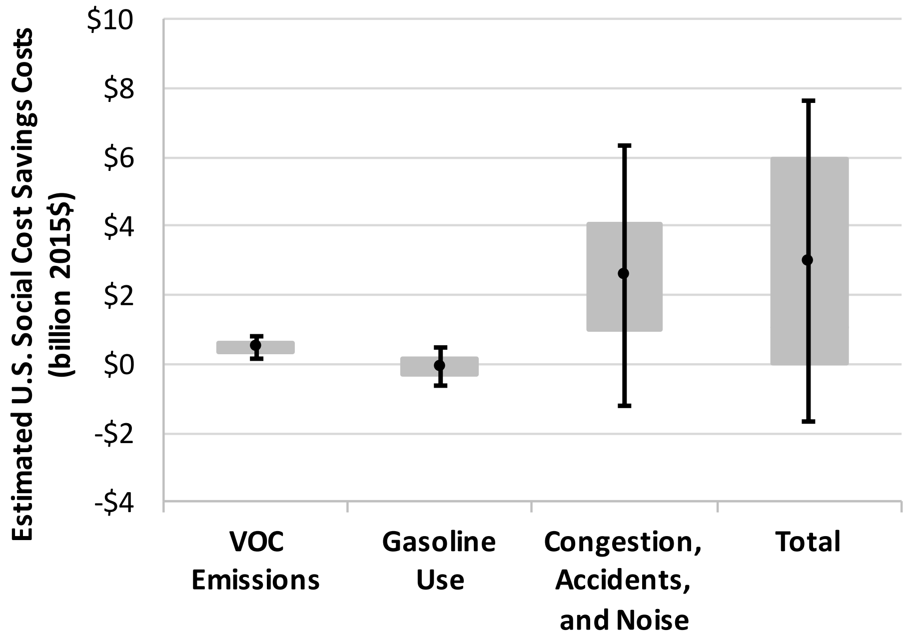


Figure 25. External costs calculated using DOT per-short ton (0.907 metric tons), per-gallon, and per-mile benefits for estimated TNC-induced effects to VOC emissions, gasoline use, and VMT in all states and years after Uber market entry. Points represent median estimates, shaded bars represent the inner-quartile range, and error bars represent the 95% confidence interval.

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Chapter 3. Ridesourcing Affects Different Cities Differently: Heterogeneity in Impacts on Vehicle Ownership, Fuel Economy, and Transit Ridership in Urban Areas in the United States

This study is based on a working paper co-authored with Jeremy Michalek, Constantine Samaras, Inês Azevedo, Alejandro Henao, Clement Rames, Tom Wenzel, and Ken Gillingham.

In this chapter, I complement the state-level analysis in the previous chapter with econometric modeling focused on urban areas as the observational unit to estimate whether and in what kinds of cities TNC entry increases or decreases vehicle ownership, fuel economy, and transit ridership outcomes. I estimate effects of on-demand ridesourcing services from transportation network companies (TNCs) Uber and Lyft on vehicle ownership, fleet average fuel economy, and transit use in U.S. urban areas using a set of difference-in-difference propensity score-weighted regression models that exploit staggered market entry across the U.S. from 2011 to 2017. I find evidence that TNC entry into urban areas causes an average 0.7% increase in vehicle registrations and no average effect on overall fleet efficiency; though, I also find significant heterogeneity in both effects. TNC entry tends to increase vehicle ownership more in urban areas with higher initial vehicle ownership and lower population growth rates, increase overall fleet efficiency more in urban areas with fewer childless households, and decrease transit ridership more in urban areas with higher median incomes and more childless households. These findings are robust to a range of robustness checks.

3.1. Introduction

The past decade saw the advent and growth of ridesourcing, a travel mode in which a passenger uses a mobile device to request a ride with a nearby driver at a transaction price determined in real time by a Transportation Network Company (TNC), such as Uber or Lyft. During this period, the number of Americans traveling in for-hire vehicles on any given day more than doubled[1,2]. Uber, which launched UberX, its basic level of service, in 2012, had entered 224 U.S. urban areas (approximately half of those identified by the U.S. Census) by the end of 2017. In absolute terms, the 0.5% of total passenger trips now served by TNCs is still relatively small, but in urban areas the effects can be substantial– by 2016 TNCs accounted for 15% of all intra-San Francisco vehicle trips on an average weekday[3].

While a growing body of research finds that TNCs lead to significant changes in transportation-related outcomes such as new vehicle purchases in China[4], traffic congestion[5], motor vehicle homicide[6,7,8], and passenger safety[9] as well as other outcomes as varied as AirBnB demand[10], entrepreneurial activity[11], and urban crime[12], few studies have yet reached conclusions about effects of TNCs on energy and climate change-relevant outcomes like petroleum consumption and greenhouse gas (GHG) emissions. Both metrics are particularly and increasingly important for the transportation sector, which has long consumed the bulk of petroleum products produced and recently became a greater GHG emitter than any other sector in the U.S.[13] These outcomes are a function of total vehicle travel and efficiency and are related to the size of the vehicle fleet and availability of transportation alternatives. Higher vehicle ownership rates are associated with both higher transportation energy consumption per capita[14], as Figure 26 shows, and greater emissions from vehicle production: there were nearly 17 million new vehicles sold in the U.S. in 2018, each generating nearly 8 metric tons of GHGs from the manufacturing process[13]. The introduction of TNC services could disrupt the relationship between vehicle ownership and transportation energy consumption and emissions not only in the U.S. but also globally, including in countries like China and India, where vehicle registration numbers are climbing more than 10% each year[13].

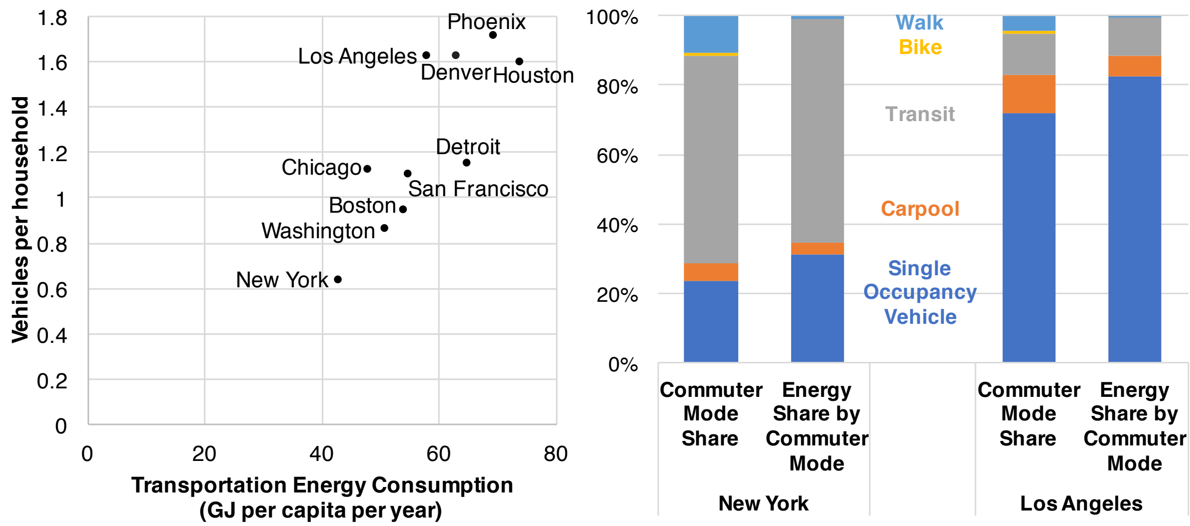


Figure 26. Transportation energy consumption is positively correlated with vehicle ownership, as is shown for 10 major U.S. cities (left; data from U.S. Census and Newman and Kenworthy[14]), and private vehicle travel consumes proportionally greater energy, as a comparison of commuter transportation modes and energy shares of those commuter transportation modes for New York and Los Angeles shows (right; data from U.S. Census and Banister[15]).

The few published articles that do examine TNC impacts on energy and the environment are constrained to survey methods or limited geographies, and, when taken together, reach apparently inconsistent inferences and conclusions. Previous analyses of TNC effects on vehicle ownership, for example, have found either a decrease in vehicle ownership[16] or that "ridesourcing probably did not influence car ownership behavior"[17]. Considered with appropriate context and constraints, these findings are not necessarily inconsistent (the decline in vehicle ownership was modeled at the U.S. state level, while the no-effect finding was based on a survey specific to San Francisco, CA), but their differences point to the need for a coherent framework within which to interpret such findings and their associated implications.

Plausible underlying narratives can explain either an increase or a decrease in vehicle ownership after TNC market entry (or a net-zero effect, as a result of the simultaneous presence of pressures in both directions): new economic opportunity afforded by TNC entry could motivate would-be TNC vehicle drivers to increase vehicle registrations; whereas, private vehicle-free mobility newly afforded to would-be passengers could prompt vehicle shedding and/or new vehicle purchase delays. Several working papers use surveys to bear out these vehicle ownership relationships: a positive correlation for TNC drivers[18] and a negative correlation for TNC passengers[19,20,21,22]. Other working papers use empirical panel data to observe or model a net effect, finding both increases[4,23] or decreases[24] in vehicle ownership.

Effects of TNCs on fleet fuel economy are also ambiguous *a priori*, as drivers motivated by lower operating costs could plausibly migrate to newer, more efficient vehicles (Uber and Lyft require newer vehicles in some cities[25]), just as drivers motivated by the potential for large-party-trip price premiums may shift to larger, less efficient vehicles. Only one peer-reviewed article to date reports a TNC-fuel economy relationship, finding that ridesourcing vehicles are more efficient compared to non-ridesourcing vehicles[26]. Two working papers agree[4,27]; while, another suggests that TNC vehicle fuel economies are lower than those of taxis[28].

The effects on TNCs on public transit ridership are similarly equivocal, with several survey-based studies finding ridesourcing can either replace transit[17] or complement it[29,30,31] as an effective extension of an otherwise fixed network (several working papers also suggest replacement[22,32,33] or complementarity[34,35]). Importantly, two of these previous studies report heterogeneous effects—a more positive transit correlation is found either in bigger cities as well as cities with smaller transit agencies[31] or as a function of high population density and households with fewer vehicles[30]—and a working paper finds rail usage increases while bus usage declines, on average, with variation in effects across cities[36].

I propose that heterogeneity in the effects of TNC entry in different types of cities may be responsible for some of the apparent discrepancies in reported effects, since average effects can look different depending on what subset of locations are included in the study scope and how they are aggregated. To quantify and systematically organize the potentially fundamental changes to personal travel across U.S. urban areas and associated energy and environmental outcomes, I estimate effects of Uber and Lyft entry on vehicle ownership, fleet average fuel economy, and transit ridership outcomes using a set of difference-in-difference propensity score-weighted regression models that exploit staggered TNC market entry into urban areas across the U.S. from 2010 to 2017. I combine annual individual vehicle registration data from Polk/IHS Markit with annual ZIP code-level sociodemographic data from the U.S. Census Bureau and aggregate to the urban area to estimate effects. I find that estimated vehicle ownership and fuel economy effects of TNC entry vary across urban areas. Accordingly, I characterize this heterogeneity across urban areas and determine that vehicle ownership levels and population growth rates are among the primary determinants of differences in TNC entry effects on vehicle ownership and efficiency. These results offer a systematic framework for informing future energy, transportation, and urban planning decision-making.

3.2. Results

Analyses are conducted using a series of difference-in-difference models with inverse probability of treatment weighting (IPTW). The difference-in-difference method is a quasi-experimental technique that compares trends before and after a treatment—i.e., TNC market entry in this study—against counterfactual trends in an untreated control group, and IPTW weights help ensure the control group is appropriately comparable to the treatment group. I specify an econometric model to estimate average TNC entry effects on vehicle ownership, fuel economy, and transit ridership and then use several variations on that primary specification to estimate heterogeneous effects across urban areas. I employ three complementary approaches to examining heterogeneity: 1) I conduct heterogeneous treatment effect (HTE) analysis to estimate urban area-specific TNC effects and then identify factors that differ between urban areas with positive versus negative estimated effects; 2) I cluster similar urban areas and calculate cluster-specific TNC effects; and, finally, 3) I add targeted treatment interaction terms identified by the HTE and cluster analysis to my regression model to confirm whether these the urban area characteristics explain the heterogeneity in TNC entry effects across urban areas. I present average effect results first (Table 21), followed by HTE (Figure 27, Table 22), cluster analysis (Figure 28, Table 23), and interaction regression results (Table 24).

3.2.1. Average Effect

In Table 21 I summarize results for the regressions specified in equation (1) (Section 4) for the effect of TNC entry vehicle registrations per capita, fleet average fuel economy, and transit trips per capita at the urban area level. On average, TNC entry in an urban area increases per-capita vehicle registrations by 0.7% (95% confidence interval: 0.1–1.3%) and has no significant average effect on fleet average fuel economy or per-capita transit trips. The estimated effect on registrations is also robust to a battery of robustness checks and sensitivity analyses (randomized treatment, leave-one-out analysis, and an event study, all of which are described in detail the Methods section and SI.)

Table 21. Treatment effects of TNC entry on urban areas in the U.S. from three regression models estimating vehicle registrations per capita, average fuel economy, and transit ridership (coefficients for control variables, fixed effects, and linear time trends are included in the SI).

	Dependent variable, log:		
	Vehicle Registration, Per Capita	Average Fuel Economy	Transit Trips, Per Capita
TNC entry	0.007**	3.00E-4	5.19E-4
	(0.004)	(8.76E-4)	(1.16E-2)
Observations	3395	3395	1848
Degrees of Freedom	2894	2894	1569
Adjusted R-Squared	0.948	0.979	0.998
Notes:	Covariate, time fixed effects, group fixed effects, and group time trend coefficient estimates not shown		
	*p<0.1; **p<0.05; ***p<0.01		

3.2.2. Heterogeneous Treatment Effects

The heterogeneous treatment effects regression specification is a variation on the primary regression that estimates urban area-specific TNC entry effects on vehicle ownership and fuel economy (transit ridership data are unavailable for nearly half of urban areas and so are not reported in this approach). Individual urban area effects are estimated to range from an 11.0% decrease in per-capita vehicle registrations in Redding, CA to a 15.7% increase in Gainesville, FL, and from a 1.9% decrease in fleet average fuel economy in Greely, CO to a 2.6% increase in Thousand Oaks, CA. Figure 27 shows the distribution of those estimated effects in urban areas that are statistically significant. TNC entry is associated with a significant decline in vehicle registrations per capita in 38 urban areas (17% of 224 treated urban areas) and an increase in 58 (26%) urban areas, as well as a significant decline in fleet average fuel economy in 30 (13%) urban areas and increase in 38 (17%) urban areas. Estimated effects on per-capita vehicle registrations and average fuel economy are not significant in 57% and 70% of urban areas, respectively (not shown).

The heterogeneity across cities raises the question of whether there are characteristics of cities that determine the sign of the effect. Table 22 presents the results of fitting a predictive linear model to whether an urban area will have a positive or negative TNC effect on vehicle registrations or fuel economy as a function of other covariates used in the primary regression. Urban areas with positive effects on vehicle registrations tend to be smaller, lower-income, and have more vehicle registrations per capita, transit commuters, and households without children than urban areas with negative effects. Urban areas with positive effects on fleet average fuel economy also tend to be smaller, lower-income (at the 90% confidence level), and have more vehicle registrations per capita and transit commuters but fewer households without children than urban areas with negative effects.

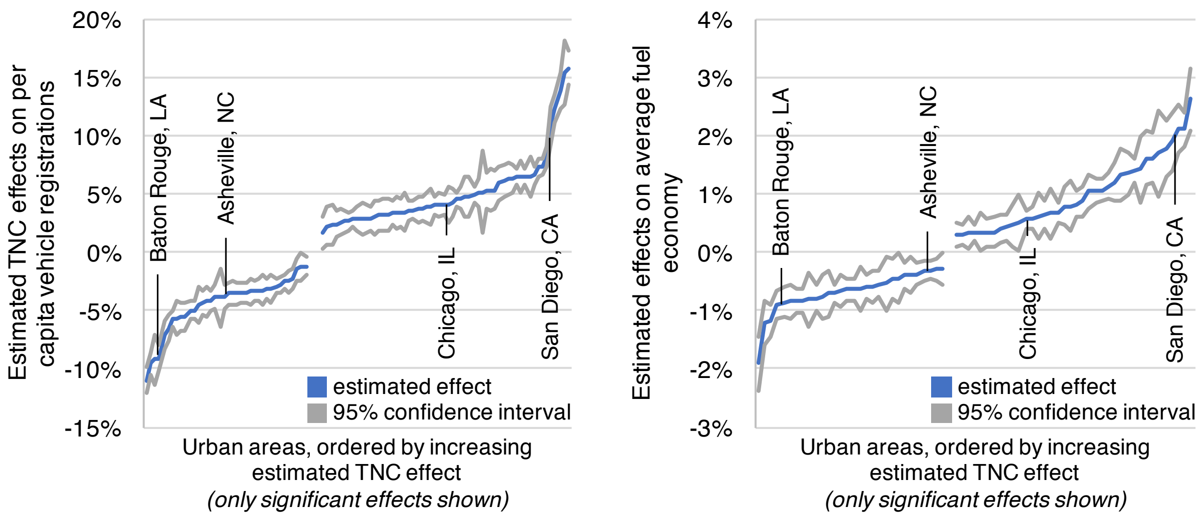


Figure 27. Heterogeneous treatment effect of TNC entry on per-capita vehicle registrations (left) and fleet average fuel economy (right), ranked by urban area from lowest to highest; only statistically significant effects are shown. The center blue line illustrates treatment effects, and the grey bands indicate 95% confidence intervals.

Table 22. Coefficients of a linear model estimating whether an urban area will have a positive (dependent variable indicator = 1) versus negative (dependent variable indicator = 0) estimated TNC effect on vehicle registrations or fuel economy, respectively, based on HTE-estimated

effects for each respective outcome as a function of other covariates used in the primary regression.

	Dependent variable:	
	1(TNC increases per-capita registrations)	*1(TNC increases fuel economy*
Vehicle registrations per capita	0.284**	0.628***
	(0.141)	(0.141)
Population, *log*	-0.040**	-0.033**
	(0.017)	(0.017)
Δ Population, *log*	-0.448	0.533
	(0.765)	(1.051)
Income, *log*	-0.282***	-0.161*
	(0.095)	(0.095)
Transit commuters, *log+1*	1.396**	2.528***
	(0.662)	(0.637)
Unemployment rate	-1.133	0.936
	(0.822)	(0.834)
Childless household rate	0.818***	-2.238***
	(0.294)	(0.305)
Gasoline price	-0.003	-0.002
	(0.003)	(0.003)
Observations	3395	3395
Degrees of Freedom	2895	2895
Adjusted R-Squared	0.042	0.057
Notes:		*p<0.1; **p<0.05; ***p<0.01

3.2.3. Cluster Effects

As an alternative approach to characterizing heterogeneity, I use hierarchical clustering to identify and group similar urban areas as a function of their observable features and then estimate TNC entry effects. In Figure 28 I show the estimated effect of TNC entry on per capita vehicle registrations (above) and average fuel economy (below) for each cluster, given an exogenously specified number of urban area clusters ranging from 2 to 10. Significant effects are highlighted for some clusters (New York City appears as a “cluster” of just one urban area in each figure and is not explicitly explored further in this analysis because the intent is to identify trends across cities). For the effect of TNC entry on transit ridership, none of my clusters had significant effects (see SI). As I show in Figure 28, estimated TNC effects vary by urban area clusters (regardless of how many clusters are specified). Across all urban area subsets shown, cluster-estimated effects range from a 1.8% decrease to a 3.0% increase in vehicle registrations per capita and from a 0.07% decrease to a 0.4% increase in average fuel economy; though, other than New York, only clusters with positive effects that are relatively large in magnitude are statistically significant, spanning a 2% to 3% increase in vehicle ownership and a 0.4% to 0.5% increase in fuel economy. These estimates are also robust to the battery of robustness checks already described (as described in detail the Methods section and SI.)

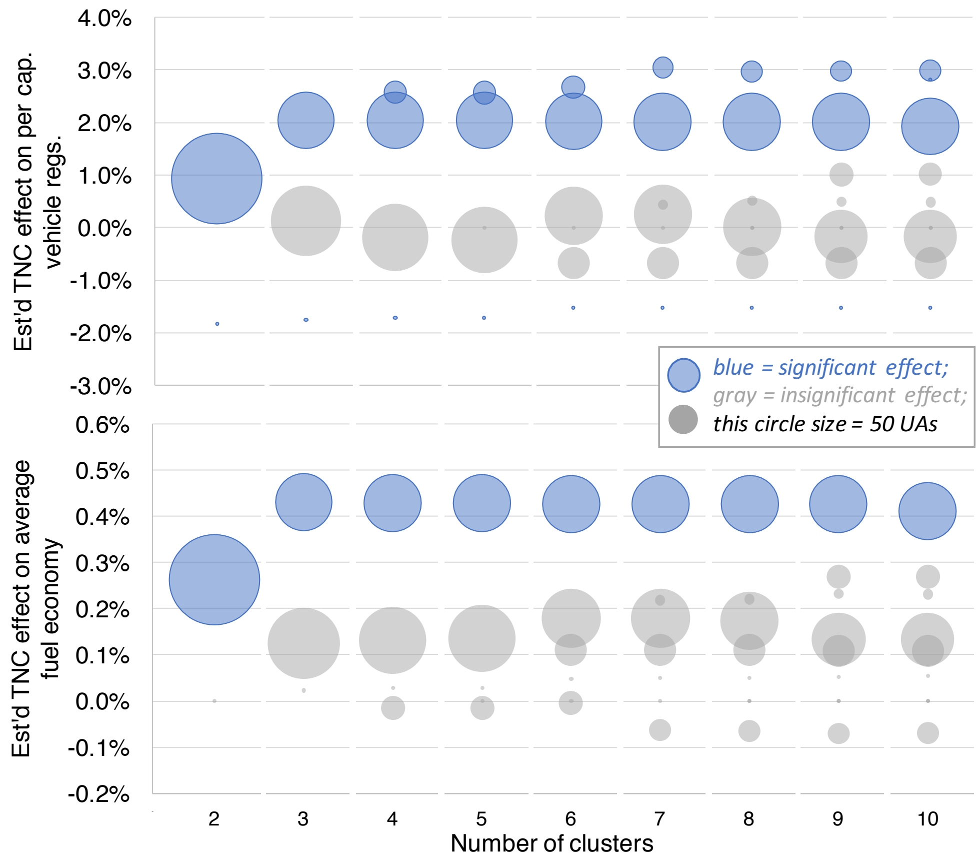


Figure 28. TNC treatment effect on the change in per-capita vehicle registrations (top) and average fuel economy (bottom) varies by urban area typology and is consistently significant and positive (indicated in blue) for one-to-two clusters of urban areas across a sweep of cluster numbers. Statistically significant effects are highlighted in blue, and estimates that are not significant are grey. The size of each circle reflects the number of urban areas in each cluster; note that the weighted average (by number of urban areas per cluster) of cluster effects is consistent across the number of clusters and with the average estimates in Table 21.

In Table 23 I fit a linear model to estimate differences in the characteristics of urban areas in clusters with and without significant estimated effects (shown in blue and shown in grey, respectively, at the $p<0.05$ level in Figure 28). Given the relatively steady pattern that emerges in estimated effects across number of clusters, I focus on the 3-cluster case: again, excluding the New York City "cluster" of just one urban area, I contrast one cluster with an estimated 2% increase in vehicle ownership and 0.5% increase in fuel economy after TNC entry with another cluster with no significant estimated effects. Compared to the insignificant-effects cluster, the significant-and-positive-effects cluster includes urban areas that are, on average, smaller with lower incomes and population growth rates and higher vehicle ownership rates and more households without children.

Table 23. Coefficients of a linear model estimating whether the cluster of urban areas will have a significant positive estimated TNC effect on vehicle registrations or fuel economy (dependent

variable indicator = 1) versus the cluster of other urban areas with no significant estimated effect (dependent variable indicator = 0) as a function of other covariates used in the primary regression. Note that because the cluster analysis groups similar (or identical) urban areas into the cluster for which significant effects are estimated for both vehicle ownership and fuel economy outcomes, the same predictive model relating outcomes to urban area characteristics is fit for TNC effects on both vehicle registrations and fuel economy.

	Dependent variable: 1(TNC increases per-capita registrations and fuel economy)
Vehicle registrations per capita	1.023***
	(0.083)
Population, *log*	-0.032***
	(0.009)
Δ Population, *log*	-5.663***
	(0.439)
Income, *log*	-0.243***
	(0.052)
Transit commuters, *log+1*	-0.695
	(0.516)
Unemployment rate	-0.080
	(0.386)
Childless household rate	1.155***
	(0.146)
Gasoline price	0.004*
	(0.002)
Observations	3395
Degrees of Freedom	2895
Adjusted R-Squared	0.189
Notes:	**p<0.1; **p<0.05; ***p<0.01*

In addition to the robustness checks already mentioned, I tested a variety of alternative clustering methods (as described in the Methods section with full detail in the SI), and, overall, the results were robust (i.e., no different sets of urban area characteristics were estimated as significantly different between clusters with significant TNC entry effects and other urban areas).

3.2.4. Interaction Regression and Heterogeneous Effects

The urban area characteristics identified as distinguishing urban areas with distinct responses to TNC entry in the cluster and HTE analysis have substantial overlap. Table 24 summarizes regression results that include treatment interactions with categorical measures (below or above the median; sensitivity to other quantiles is presented in the SI) of the six dimensions of heterogeneity suggested in common by the heterogeneous treatment effect (HTE) and clustering analyses: vehicle registrations per capita (in a pre-treatment reference year, 2010, to avoid endogeneity in modeling vehicle registrations in 2011–2017) and population, population growth, income, the percentage of commuters who travel by transit, and the percentage of households without children (all also measured in a pre-treatment reference year, 2011). I find that, on average, urban areas with higher-than-median initial vehicle ownership see a 0.9% (95% confidence interval: 0.1% to 1.5%) higher effect of TNC entry on ownership than urban areas with lower-than-median initial ownership, and, urban areas with lower-than-median growth rates also see a 0.9% (95% confidence interval: 0.1% to 1.5%) higher effect of TNC entry on ownership than urban areas with higher-than-median growth. These estimates are also robust to a battery of robustness checks, as well as alternative quantile approaches and continuous measures for estimating the interaction (as described in detail the Methods section and SI). Estimates of interactions with other variables are not statistically significant.

Additionally, I also find that urban areas with lower-than-median childless household rates see a 0.2% (0% to 0.4%) higher TNC effect on fuel economy than urban areas with higher-than- median childless household rates, an estimate which is also robust to a battery of robustness checks (again including alternative quantile and continuous approaches, as described in detail the Methods section and SI). The effect of TNC entry on average fuel economy is not found to vary as a function of other variables. Finally, I estimate that, on average, urban areas with higher-than-median incomes see a 5.1% (1.7% to 8.5%) lower effect of TNC entry on transit ridership than urban areas with lower-than-median incomes, and urban areas with higher-than-median childless household rates see a 2.6% (0% to 5.2%) lower effect of TNC entry on transit ridership than urban areas with lower-than-median childless household rates.

3.3. Discussion

Our results suggest that access to TNC services has significant and heterogeneous effects on vehicle ownership (per-capita vehicle registrations) and average fuel economy. I apply heterogeneous treatment effect analysis, cluster analysis, and a regression analysis with interaction effects as three different ways to characterize the heterogeneity of TNC entry effects across urban areas. All three methods agree that TNC entry tends to produce larger increases in vehicle ownership for urban areas with higher initial vehicle ownership and lower population growth rates compared to urban areas with lower initial vehicle ownership and higher growth rates, respectively, larger increases in average fuel economy for urban areas with fewer childless households incomes compared to urban areas with more childless households, and larger decreases in transit ridership for urban areas with higher incomes and more childless households than urban areas with lower incomes and fewer childless households, respectively.

Regarding TNC entry effects on fleet fuel efficiency, there is evidence that TNC entry increases fleet efficiency more in urban areas with fewer childless households less in urban areas with more childless households. An increase in fuel economy effect aligns with Wenzel et al.'s findings that the TNC fleet is more efficient (compared to the overall private vehicle fleet)[26], so it is plausible to consider TNC entry would have such an effect everywhere absent other dynamics. While TNC drivers may indeed buy the same efficient vehicles with larger and more comfortable backseats and four doors, regardless of urban area, in urban areas with more childless households, the pre-existing vehicle stock may have been smaller-sized and already disproportionately efficient, such that TNC entry in such areas results in a smaller increase in vehicle efficiency than in urban areas with fewer childless households and presumably a pre-existing stock of larger and less efficient vehicles.

Table 24. Treatment effects of TNC entry in the U.S. from a series of regression models separately estimating treatment interactions with categorical measures of vehicle ownership,

population, population growth, income, and transit commuters on vehicle registrations per capita, average fuel economy, and transit ridership per capita outcomes (coefficients for control variables, fixed effects, and linear time trends are included in the SI).

	Dependent variable, log:		
	Vehicle Registrations Per Capita	Average Fuel Economy	Transit Trips Per Capita
TNC entry in lower 50%ile	0.006	0.002**	0.003
	(0.008)	(0.001)	(0.027)
Pre-treatment vehicle registrations per capita			
TNC entry interaction (upper vs. lower 50%ile)	0.009**	-2.2E-5	-0.002
	(0.004)	(4.2E-4)	(0.013)
Effect in upper 50%ile†	0.016*	0.002*	4.9E-4
	(0.008)	(0.001)	(0.030)
Pre-treatment population growth rate			
TNC entry interaction (upper vs. lower 50%ile)	-0.009**	-5.2E-4	-0.002
	(0.004)	(3.8E-4)	(0.013)
Effect in upper 50%ile†	-0.002	0.002**	0.001
	(0.011)	(0.001)	(0.030)
Pre-treatment childless household rate			
TNC entry interaction (upper vs. lower 50%ile)	0.003	-0.001**	-0.026**
	(0.004)	(0.000)	(0.013)
Effect in upper 50%ile†	0.010	0.001	-0.023
	(0.009)	(0.001)	(0.029)
Pre-treatment income			
TNC entry interaction (upper vs. lower 50%ile)	0.000	-4.6E-4	-0.051***
	(0.005)	(4.4E-4)	(0.017)
Effect in upper 50%ile†	0.007	0.001	-0.048
	(0.012)	(0.001)	(0.036)
Other treatment interactions	Y	Y	Y
Covariate controls	Y	Y	Y
Time fixed effects	Y	Y	Y
Group fixed effects	Y	Y	Y
Group time trends	Y	Y	Y
Observations	3395	3395	1584
Deg. Freedom	2407	2407	1034
Adjusted R-Sq.	0.972	0.996	0.998

Notes: Covariate, time and group fixed effects, and group time trend coefficient estimates not shown; †computed post-hoc and not directly estimated; *p<0.1; **p<0.05; ***p<0.01;

Assuming continued TNC market growth and an extension of the effects I estimate here (which may, in fact, change over time), the 0.2% increase in average fuel economy estimated here would translate to 400 million fewer gallons of gasoline consumed each year when applied to roughly half of all passenger trips (based on 2018 consumption and assuming changes only in areas with lower-than-median childless-household rates[37]) and a shift in regional petroleum consumption away from urban areas with fewer childless households. Again, the net effect on total fleet energy consumption also depends on the distribution of vehicle miles traveled, which I do not observe and which may shift away from personal vehicles toward TNC vehicles.

Finally, regarding the larger declines in transit ridership associated with TNC entry in higher-income urban areas and those with more childless households, it is plausible first to consider that TNC entry appeals more as substitute to transit to potential passengers with a greater ability to pay for a relatively higher TNC fare in higher-income areas. And, in higher childless-household urban areas, travelers without families may have more flexibility across modal choices than travelers with families, as previous literature has suggested[38]. TNC entry, then, could divert a greater portion of transit trips for more flexible travelers; whereas, less flexible travelers' transit use unchanged (i.e., commuting via transit could continue while errands and other casual trips with family were already not happening via transit). Transit energy intensity is lower than that of personal vehicle travel, as Figure 26 showed, such that shifting trips from transit to TNC vehicles likely increases energy consumption.

The increases in per-capita vehicle registrations and average fuel economy estimated here are consistent with Gong et al.'s finding that Uber entry leads to an increase in new vehicle registrations[4] and Wenzel et al.'s conclusion that ridesourcing vehicles are more efficient[28] but seemingly contrary to Ward et al.'s results suggesting a decrease in post-TNC entry per-capita vehicle registrations at the state level (using total light-duty vehicle registrations from Ward's Automotive)[16]. It is not necessarily inconsistent that our results find a positive effect at the urban-area level when previous results found a negative TNC market entry effect on vehicle registrations at the state level, especially given the heterogeneity in urban area effects found in this analysis. We replicate the state level analysis by aggregating urban area vehicle registrations (and population-weighting average other explanatory variables) up to the state level and re-specifying the state-level regression model. Table 25 compares the average effect estimates from the earlier state-level analysis, the current urban area-level analyses, and a state-level analysis using urban area data aggregated to the state level. We find that the urban area data produces a significant negative estimate when aggregated to the state level, consistent with the state-level analysis. This suggests that, if TNC entry has different effects in different cities, averaging effects across urban areas within the same state can yield different results than analyzing effects across individual urban areas.

Table 25. Two sets of regression results exploring heterogeneity across urban areas (UAs): an Average-Effect Model that compares state-level analysis results, urban area-level analysis results, and a reproduction of state-level results using urban area data aggregated to the state level (i.e., arithmetic or population-weighted means?).

	Dependent variable:		
	log(Vehicle Registration, per capita)		
Treatment Effect	-0.031**	0.007**	-0.010**
	(0.012)	(0.003)	(0.005)
Observations	550	3395	287
Deg. Freedom	474	2894	229
Adjusted R-Sq.	0.844	0.948	0.963

Notes: Covariate controls, time fixed effects, group fixed effects, and group time trend coefficient estimates not shown.
*p<0.1; **p<0.05; ***p<0.01
†computed post-hoc and not directly estimated

While these mechanisms are plausible pathways by which the estimated effects manifest, I acknowledge my analysis is constrained to net overall outcomes after TNCs enter urban areas. I cannot identify more detailed changes to vehicle fleet mix with the available data, and there are potentially multiple alternative—and sometimes competing—narratives that might explain these trends. Depending on their relative magnitudes, these dynamics can yield a near-zero net effect (potentially the case for average fuel economy and transit ridership effects estimated here), even within analyses that explicitly targets heterogeneity. Additional study of the effect of TNC market entry on vehicle fleet composition and travel behavior across the fleet is needed for deeper insight about the mechanisms that produce these outcomes.

3.4. Methods

I use difference-in-difference (DID) models to estimate effects of the intervention (TNC entry into U.S. urban areas) by comparing the trends of treated and untreated groups before and after the treatment occurs. The DID technique is a quasi-experimental method that enables my models to isolate and estimate a TNC entry effect, even without a randomized controlled experiment. These specifications can control explicitly for relevant covariates as well as implicitly for potentially unobserved (or otherwise omitted) covariates that could otherwise bias my estimates by differencing away similarities between treated and untreated groups (both of which are ostensibly similarly affected by any potentially omitted covariates). DID methods have been used previously to evaluate the effect of TNCs on transportation and several other outcomes[4–12]. I employ inverse probability of treatment weighting (IPTW) to ensure that my control and treated groups are comparable, thereby avoiding potential selection bias (i.e., potential bias in my estimates actually attributable to TNCs systematically entering certain kinds of urban areas first). The details of my DID and IPTW implementations are described below.

3.4.1. Difference-in-Difference Model

Our regression model is informed by models used in prior literature for my outcomes of interest. Regression analysis is conducted using inverse probability of treatment weighting (described below) and the following baseline specification:

$$y_{ut} = \beta x_{ut} + \boldsymbol{\alpha}^\top \mathbf{z}_{ut} + \gamma_u + \delta_t + \gamma_u t + \varepsilon_{ut} \tag{1}$$

where y_{ut} is the dependent variable of interest for urban area u and year t and represents either 1) vehicle registrations per capita, 2) average fuel economy, or 3) transit ridership. x_{ut} is the treatment (TNC indicator) with coefficient β. $\mathbf{z}_{ut}$ is a vector of controls (population, unemployment rate, income, portion of households with no children, percent of population commuting by transit, and state average gas price), with corresponding coefficients $\boldsymbol{\alpha}$. γ_u and δ_g are fixed-effects dummies for urban area u and year t, respectively; $\gamma_u t$ allows for linear time trends by urban area; and ε_{ut} is unobserved error.

3.4.2. Propensity Score

I estimate propensity scores using gradient boosting[39], which previous studies have shown as superior to simple logistic regression models for propensity score estimation[40], to approximate the logistic model:

$$\log\left(\frac{p_{gt}(\mathbf{z}_{ut})}{1-p_{ut}(\mathbf{z}_{ut})}\right) = \sum_m f_m(\mathbf{z}_{ut}) + \epsilon_{ut}, \tag{2}$$

where p_{ut} is the probability of treatment for urban area u and year t; $\mathbf{z}_{ut}$ is the same vector of covariates for urban area u and year t as in equation (1), and ϵ_{ut} is unobserved error. I estimate the additive function f_m using gradient boosting, given the treatment and covariate data, and compute estimated probability of treatment $\hat{p}_{ut}$ for each urban area and year. The resulting estimates for probability of treatment are then used in a weighted regression for equation (1)[41].

3.4.3. Heterogeneous Treatment Effects Analysis

I estimate heterogeneous treatment effects by individual urban area in the following variation of my baseline specification:

$$y_{ut} = \beta_u x_{ut} + \boldsymbol{\alpha}^\top \mathbf{z}_{ut} + \gamma_u + \delta_t + \gamma_u t + \varepsilon_{ut}, \tag{3}$$

such that the coefficient β_u now takes on unique values for each urban area u. I subsequently fit an additional linear model to identify significant differences between urban areas where treatment effects are positive versus negative using the following specification:

$$\tau_u = \boldsymbol{\alpha}^\top \mathbf{z}_{ut} + \varepsilon_{ut}, \tag{4}$$

where τ_u is a binary indicator for whether the treatment effect is significant and positive or significant and negative (urban areas without significant estimated effects are excluded).

3.4.4. Cluster Analysis

I use hierarchical clustering to identify groups of urban areas that are similar in terms of their observable features, employing an agglomerative (rather than divisive) algorithm, in hopes of finding larger groups of similar urban areas, and computing [dis]similarity across urban areas using Euclidean distances and Ward's minimum variance method[42]. For a given number of clusters, n, I re-specify my regression as:

$$y_{ut} = \boldsymbol{\kappa}_u^\top \boldsymbol{\beta} x_{ut} + \boldsymbol{\alpha}^\top \mathbf{z}_{ut} + \gamma_u + \delta_t + \gamma_u t + \varepsilon_{ut}, \tag{5}$$

where $\boldsymbol{\kappa}_u^\top = [\kappa_{1u}\ \kappa_{2u} \dots \kappa_{nu}]$ is a vector indicating the cluster to which urban area u belongs ($\kappa_{iu} = 1$ if urban area u is in cluster i and $\kappa_{iu} = 0$ otherwise) and $\boldsymbol{\beta} = [\beta_1\ \beta_2 \dots \beta_n]^\top$ is the vector of treatment coefficients for each cluster. I run a series of models sweeping from $n \in \{2, 3, \dots, 10\}$ clusters and estimate cluster-specific TNC entry effects as described.

I test the sensitivity of my clustering analysis by varying the clustering algorithm (divisive rather than agglomerative), the distance measure (Manhattan rather than Euclidean), the linkage function (complete rather than Ward's method) and by using a subset (rather than all) of urban area features: vehicle registrations per capita, population, population density, and percent of population commuting by transit.

I again fit an additional linear model to identify significant differences between clusters of urban areas where treatment effects are positive and significant versus insignificant using the following specification:

$$\boldsymbol{\tau}_u = \boldsymbol{\alpha}^{\top}\mathbf{z}_{ut} + \varepsilon_{ut}, \tag{6}$$

where τ_u is a binary indicator for whether the treatment effect for the cluster to which an urban area belongs is significant and positive or not.

3.4.5. Interaction Analysis

In a final variation on the primary regression in equation (1), I interact specific variables with treatment. I specify these interaction regressions as:

$$y_{ut} = \beta_1 x_{ut} + \beta_2 x_{ut}\zeta_{ut} + \boldsymbol{\alpha}^{\top}\mathbf{z}_{ut} + \gamma_u + \delta_t + \gamma_u t + \varepsilon_{ut}, \tag{7}$$

where β_1 is the average treatment coefficient and β_2 is a treatment interaction coefficient estimating how the average treatment varies with ζ_u, an element of $\mathbf{z}_{ut}$. I interact a categorical measure of ζ_{ut}: 1 indicates a value greater than the mean for a given UA in a given year; while, 0 indicates not. Also, when the interaction term is vehicle registrations per capita, to avoid endogeneity problems (as a function of modeling a dependent measure of vehicle registrations per capita with an independent measure of the same), the interaction term refers to a pre-treatment categorization (i.e., vehicle ownership rates in an urban area in the year 2010).

3.4.6. Robustness

I subject my results to a variety of checks including a set of robustness checks, sensitivity analysis, and event studies. The battery of robustness checks and sensitivity analyses that I apply support my findings. All significant effects in my primary model are robust (i.e., still estimated as significant at similar magnitude) to five robustness checks, as follows:

(1) Randomized Treatment: I conduct placebo tests, in which I reassign the set of true TNC entry dates to other urban areas at random and re-specify my regressions, to ensure that the effects I estimate are unique to the particular observed pattern of treatments, rather than a result of the structure of the model. Estimated effects are considered robust if they fall in the tails (>95%) of the distribution of randomized treatment-estimated effects;

(2) Leave-One-Out: I conduct leave-one-out tests to ensure that my estimates do not hinge on accuracy of the timing of TNC entry in, or the data from, any one urban area. Estimated effects are considered robust if they remain significant when systematically leaving each urban area out;

(3) Leave-Multiple-Out: because some dependent variables exhibit what could be perceived as discontinuities resulting from data collection/input or other error for several urban areas, I incrementally remove urban areas that exhibit the largest year-on-year change in decreasing order. Estimated effects are considered robust if estimated magnitude and significant are similar after systematically excluding up to 25 urban areas;

(4) Alternative Quantiles: in my targeted interaction regressions, I increase the number of quantiles (the top and bottom 50%iles are compared in the main text) to confirm significant treatment interaction terms are robust and consistent across varying categorical interaction variable quantile sizes;

(5) Continuous Interaction: in my targeted interaction regressions, I replace the categorical with a continuous measure of the interaction term to test whether interaction effects are linear;

(6) Alternative Clustering: in my cluster analysis, I change the urban area features and algorithms used for clustering (including Manhattan instead of Euclidean distance measures, a complete instead of Ward's linkage function, a divisive instead of agglomerative algorithm, and an alternative feature selection, including only those features identified as significant in my HTE analysis) to confirm correlations between TNC effects on vehicle registration and fuel economy outcomes are consistent independent of clustering features and/or method; and
(7) Event Study: I model an event study by adding relative time indicators for the number of years before and after TNC entry to confirm the presence of post-treatment (i.e., post-TNC entry) effects without pre-treatment anticipation (i.e., estimated effects occur only after treatment).

Robustness test results are summarized in Table 26 below; details for all are provided in the SI.

Table 26. Summary of robustness tests and results.

	Estimated Effect	Randomized Treatment	Leave-One-Out	Leave-Multiple-Out	Alternative Quantile	Continuous Interaction	Alternative Clustering	Event Study
Vehicle Ownership								
Average Effect	0.007***	●	●	●	n/a	n/a	n/a	●
Interaction Effects								
Veh. Reg. per capita	0.009**	●	●	●	●	○	●	n/a
Population	0.003	○	○	○	○	○	○	n/a
Population Growth	-0.002	○	○	○	○	○	○	n/a
Household income	0.001	○	○	○	○	○	○	n/a
Transit Commute %	-0.002	○	○	○	○	○	○	n/a
Average Fuel Economy								
Average Effect	3.00E-4	○	○	○	n/a	n/a	n/a	○
Interaction Effects								
Veh. Reg. per capita	0.0002	○	○	○	○	○	○	n/a
Population	0.0002	○	○	○	○	○	○	n/a
Population Growth	5.33E-5	○	○	○	○	○	○	n/a
Household income	-0.0008**	●	●	●	●	○	●	n/a
Transit Commute %	6.00E-4	○	○	○	○	○	○	n/a
Transit Ridership								
Average Effect	5.19E-4	○	○	○	n/a	n/a	n/a	○
Interaction Effects								
Veh. Reg. per capita	-0.005	○	○	○	○	○	○	n/a
Population	0.031	○	○	○	○	○	○	n/a
Population Growth	0.013	○	○	○	○	○	○	n/a
Household income	0.004	○	○	○	○	○	○	n/a
Transit Commute %	-0.045*	○	○	○	○	○	○	n/a

3.5. Data

I describe and identify data sources for dependent variables, treatment, and control variables below:

3.5.1. Dependent Variables:

- *Vehicle registrations and fuel economy:* IHS Markit (formerly Polk) collects and sells vehicle registration information from U.S. state agencies responsible for vehicle registration data[43]. I rely on annual versions of the dataset from 2010–2017 that report individual vehicle make, model, and engine size for the approximately 240 million light-duty vehicles registered in the U.S. I aggregate vehicle counts to and average fuel economies at the urban area level.
- *Transit ridership:* U.S. DOT's Federal Transit Administration (FTA) reports annual summary statistics, including ridership, on more than 660 transit providers receiving federal funding in the National Transit Database (NTD)[44]. I focus on transit providers that consistently report data for all years of this analysis (2010–2017) and aggregate individual transit agencies by urban area, per classification in the database.

3.5.2. Treatment Variables:

- *Uber and Lyft entry dates:* I adopt data from previous sources that aggregated and published a time-series of Uber market entry dates. A 2014 Forbes article first aggregated Uber launch dates from 2010–2014[45] by service area, as originally announced on Uber's official blog (on a post no longer available) and/or in local media from each new service area. Forbes continued to update that dataset to reflect additional Uber markets launched through December 2015. Those dates are cross-referenced against Uber market launch date data that were independently gathered and published in two later studies[45,46] as well as the authors' own systematic comparison with local newspaper announcements. Burtch et al. include a table of market launch dates for UberX—Uber's lower-cost, on-demand service provided in the driver's personal vehicle, which the authors compiled directly from the Uber Blog for the rest of the analysis period (through 2017). Lyft market launch dates were requested from and provided by Lyft[47]. For each urban area, I use an annualized measure of the first entry date (Uber or Lyft) in my analysis to indicate when on-demand mobility became available (annualized Lyft market entry years are the same or later than annualized Uber market entry years in all but several cases in upstate New York).

3.5.3. Control Variables

- Control variables are 5-year American Community Survey (ACS) estimates reported by the U.S. Census and include: (i) population, (ii) unemployment rate, (iii) income, (iv) portion of households without children, and (v) percent of population commuting by transit. A state-level measure of gasoline price, published annually by the Energy Information Administration, is also included as a control variable[48]. Additional ACS variables used to determine IPTW weights include: population density, portion of the population over age 16 and 65, respectively, and percent of population that is female.

3.6. Supplemental Information

The following text and figures offer additional detail in support of the main text, methods, and results reported in "Ridesourcing Affects Different Cities Differently: Heterogeneity in Impacts on Vehicle Ownership, Fuel Economy, and Transit Ridership in Urban Areas in the United States." It is organized as follows:

- **Section 1. Descriptive Statistics:** this section summarizes in tabular form annual means and standard deviations of the variables included in this study over the analysis period 2011–2017;
- **Section 2. Results**: the tables and figures in this section are organized to accompany the results section in the main text, starting with a balance table offer context for the effectiveness of the inverse probability of treatment weights (IPTW) used in the various regressions in this section. The remaining tables and figures in this section report details on average-effect regression results (including diagnostic plots examining model fit), heterogeneous treatment effect (HTE) analysis (including urban area-specific treatment effect estimates and an accompanying discussion about level of significance), cluster analysis (including estimated effects, descriptive statistics by cluster, and a presentation of a k=4 clusters case as a complement to the k=3 cluster case in the main text), and the interaction regression analysis (including a series of three tables reporting regression result details and an additional accompanying table contrasting the mean and standard deviation for the lower and upper 50%ile for each respective interaction term). The section concludes with a data table detailing the values of each data point plotted in main text Figure 5 and summary table comparing the relationships between TNC market entry effects and the dimensions of heterogeneity identified as significant in each of the HTE, cluster, and interaction regression analyses; and
- **Section 3. Robustness Checks**: this final section offers detailed results and/or illustrative diagrams as evidence for the robustness checks described and summarized in the main text. For each of the randomized treatment, leave-one-out, and leave-multiple-out robustness checks, an illustrative set of histograms is presented to confirm results "pass" (i.e., remain consistent in terms of sign, magnitude, and level of significance) each robustness check. Next, an event study is presented in the form of a relative time model estimating in the years before and after TNC entry annual effects on vehicle registrations per capita (since no significant TNC entry effect was estimated on average fuel economy and transit ridership, they are not analyzed). The last sets of tables and figures focus on alternative approaches to the interaction regression (include variations on the number of quantiles included in the categorical interaction model described in the main text as well as an additional model using continuous measures of each interaction variable) and clustering analyses (including variations to distance measure, linkage function, hierarchy, and features included for clustering).

3.6.1. Descriptive Statistics

This section consists of one table offering annual means and standard deviations across all urban areas as a tabular summary of the variables included in this study, along with a quick reference at the relevant units and encoding names.

Table 27. Descriptive statistics (means and standard deviations) across n=485 urban areas annually for the analysis period 2011–2017.

Variable	Units	Coded as	2011	2012	2013	2014	2015	2016	2017
Vehicle registrataions	# vehicles	`polk_reg`	361898 (843768)	368543 (864999)	371413 (862048)	380005 (888522)	390421 (914096)	401301 (937200)	404148 (940863)
Average fuel economy	miles per gallon	`avg_mpg`	20.5 (0.5)	20.9 (0.5)	21.1 (0.6)	21.3 (0.6)	21.6 (0.7)	21.9 (0.7)	22.1 (0.8)
Transit trips	# trips	`NTDtrips`	36726260 (258009100)	37093390 (259439800)	37476220 (266073200)	37906230 (271915700)	37005930 (265042100)	36229950 (265285600)	35420920 (262407400)
Urban areas with TNC service	%	`treat`	0% (0%)	1% (10.1%)	6.4% (24.5%)	29.9% (45.8%)	41.2% (49.3%)	44.7% (49.8%)	49.9% (50.1%)
Population	# persons	`pop`	478538 (1291134)	482988 (1299632)	485327 (1304244)	489862 (1314047)	493613 (1322095)	498169 (1330433)	502986 (1344081)
Population density	# persons / sq. mi.	`pop_dens`	1363 (1595)	1371 (1606)	1378 (1619)	1390 (1638)	1398 (1652)	1407 (1661)	1415 (1678)
Unemployment rate	%	`p_unemp`	8.6% (2.4%)	9.3% (2.7%)	9.7% (2.8%)	9.2% (2.7%)	8.3% (2.4%)	7.5% (2.2%)	6.7% (2%)
Average household income	current $	`inc`	53228 (12340.94)	53456 (12463.93)	53480 (12578.51)	53980 (12787)	54375 (12927.58)	55735 (13316.58)	57969 (13999.23)
Childless households	%	`p_no_child`	66.1% (6.4%)	66.4% (6.5%)	66.8% (6.4%)	67.1% (6.3%)	67.4% (6.3%)	67.8% (6.2%)	68.1% (6.1%)
Transit commuting rate	%	`p_pt`	1.8% (2.5%)	1.8% (2.5%)	1.8% (2.6%)	1.9% (2.6%)	1.9% (2.6%)	1.9% (2.6%)	1.9% (2.7%)
Gasoline price	$/MMBtu	`Gas_Price`	28.7 (1.3)	29.6 (1.6)	28.7 (1.4)	27.6 (1.5)	20.4 (2.3)	18.2 (1.8)	20.4 (2)
Portion of population over age 16	%	`p_o_16`	78.9% (3.2%)	79% (3.2%)	79.2% (3.1%)	79.3% (3.1%)	79.5% (3.1%)	79.6% (3.1%)	79.8% (3.1%)
Portion of population over age 65	%	`p_o_65`	13.2% (4.1%)	13.4% (4.2%)	13.7% (4.3%)	14.1% (4.3%)	14.5% (4.4%)	14.9% (4.5%)	15.3% (4.6%)

3.6.2. Results

Before diving into regression results, this section opens with a balance table quantifying the differences in control variables across the treatment and control groups before the application of inverse probability of treatment weights (IPTW) and to what extent those weights succeed in "balancing" the control sample (i.e., such that the means are similar to those of the treatment sample).

The remaining tables and figures in this section are organized to accompany the results section in the main text, starting with detailed regression results (including covariate coefficient estimates not presented in the main text for brevity) and accompanying diagnostic plots demonstrating no evidence of regression misspecification (in terms of indicators examining potential structure in the distribution of residual errors, heteroskedasticity, and leverage). Then, as in the main text, the focus shifts to heterogeneous treatment effect (HTE) analysis, including a detailed list of urban area-specific treatment effect estimates for vehicle registrations, fuel economy, and transit ridership effects and a related couple of plots and accompanying discussion about the relationships between the level of significance (p-value) of HTE-estimated effects and TNC entry year by urban area. The next tables and figures present the detailed results for cluster analysis, including firstly the estimated effects on vehicle registrations per capita and average fuel economy and descriptive statistics by cluster for clusters of urban areas estimated in a 3-cluster and 4-cluster analysis and secondly a set of cluster analysis results for the 4-cluster case as a complement and/or alternative to the 3-cluster case presented in the main text.

Moving on to the interaction regression analysis (again, in alignment with the main text), a series of three tables then reports detailed interaction regression results, and an additional accompanying table contrasts the mean and standard deviation for the lower and upper 50%ile for each respective interaction term: vehicle registrations per capita, population, change in population, household income, and transit commuting rate. The section concludes with a data table detailing the values of each data point plotted in main text Figure 5 and summary table comparing the relationships between TNC market entry effects and the dimensions of heterogeneity identified as significant in each of the HTE, cluster, and interaction regression analyses.

Inverse Probability of Treatment Weights (IPTW): the balance table presented below quantifies the differences in control variables across the treatment and control groups both before and after the application of inverse probability of treatment weights (IPTW). It is clear from the table that nearly all significant differences before weighting are balanced after weighting. A significant difference remains between treatment and control mean unemployment rate, but the values (7.7% and 8.1%, respectively) are practically similar.

Table 28. Balance table for inverse probability of treatment weights (IPTW), showing significant differences across population, density, unemployment, income, female population percentage, and transit commuting rate that are balances (i.e., no significant differences) in the weighted sample (except for unemployment rate, which, though still statistically significantly different, is practically similar at 7.7% and 8.1% for the treated and control groups, respectively).

Unweighted							
	Treatmed Mean	**Treated Std. Dev.**	**Control Mean**	**Control Std. Dev.**	**Stand'ized Effect Size**	**t-Statistic**	**p-Value**
pop	86432.046	2176640.67	294194.305	763528.149	0.364	10.346	0.000
pop_dens	2284.468	2504.109	1094.597	1067.59	0.475	13.383	0.000
p_unemp	0.077	0.022	0.087	0.027	-0.451	-10.652	0.000
inc	59775.76	13117.84	52903.037	12521.718	0.524	13.325	0.000
p_o_16	0.795	0.028	0.793	0.033	0.067	1.609	0.108
p_o_65	0.141	0.039	0.142	0.046	-0.015	-0.349	0.727
p_no_child	0.673	0.054	0.67	0.066	0.048	1.132	0.258
p_fem	0.51	0.008	0.508	0.013	0.307	6.335	0.000
p_pt	0.027	0.039	0.016	0.019	0.306	8.538	0.000
Weighted							
	Treatmed Mean	**Treated Std. Dev.**	**Control Mean**	**Control Std. Dev.**	**Stand'ized Effect Size**	**t-Statistic**	**p-Value**
pop	86432.046	2176640.67	900627.294	1873260.74	0.085	1.263	0.207
pop_dens	2284.468	2504.109	2161.548	2319.981	0.049	0.642	0.521
p_unemp	0.077	0.022	0.081	0.022	-0.181	-3.717	0.000
inc	59775.76	13117.84	58856.287	13746.753	0.07	1.235	0.217
p_o_16	0.795	0.028	0.794	0.027	0.005	0.106	0.916
p_o_65	0.141	0.039	0.14	0.04	0.026	0.524	0.600
p_no_child	0.673	0.054	0.673	0.051	-0.008	-0.159	0.874
p_fem	0.51	0.008	0.511	0.008	-0.045	-0.889	0.374
p_pt	0.027	0.039	0.028	0.038	-0.01	-0.136	0.892

Regression results: the following table offers the same regression results as are presented in Table 1 of the main text with additional detail, including coefficient estimates for all additional covariates (year and urban-area fixed effect estimates are omitted for brevity and readability). An illustrative series of diagnostic plots (for the TNC entry effect on vehicle ownership case) is also depicted to explore model fit.

Table 29. Regression results estimating the effect of TNC market entry ("Treatment") on three dependent variables of interest—vehicle ownership, fleet fuel efficiency, and transit ridership—as a function of control variables (coefficients shown) both with inverse probability of treatment weighting (IPTW) and without (i.e., ordinary least squares, OLS).

	Dependent variable: log, per cap.					
	Vehicle Registrations		Fuel Economy		Transit Trips	
	IPTW	OLS	IPTW	OLS	IPTW	OLS
Treatment	0.007**	0.010***	0.0003	0.003***	-0.001	0.005
	(0.003)	(0.004)	(0.0009)	(0.001)	(0.012)	(0.015)
Population	-0.118***	-0.136**	0.029***	0.051***	0.041	0.046
	(0.038)	(0.055)	(0.009)	(0.011)	(0.096)	(0.099)
Unemp. Rate	0.382	0.529	0.057	-0.045	-3.771	-2.795
	(0.513)	(0.515)	(0.113)	(0.092)	(2.38)	(2.133)
Unemp. Rate^2	-5.68***	-5.799***	-0.322	0.260	16.431	8.053
	(1.886)	(2.011)	(0.462)	(0.376)	(9.986)	(8.900)
Income	3.275***	2.11**	1.618***	0.812***	7.357*	9.457**
	(1.155)	(0.895)	(0.448)	(0.282)	(4.02)	(4.143)
Childless Households	0.524	0.46*	0.148**	0.121***	2.587**	0.465
	(0.358)	(0.252)	(0.063)	(0.033)	(1.312)	(1.022)
Transit Commuting	-0.419	-0.028	0.324*	0.308*	1.683	0.695
	(0.547)	(0.461)	(0.175)	(0.121)	(1.804)	(1.724)
Gasoline Price	0.006***	0.008***	0.005***	0.004***	0.006	0.006
	(0.002)	(0.003)	(0.001)	(0.000)	(0.009)	(0.008)
Observations	3395	3395	3395	3395	1848	1848
Deg. Freedom	2895	2895	2895	2895	1569	1569
Adjusted R-Sq.	0.948	0.914	0.979	0.975	0.998	0.997
Notes:	*Time and group fixed effects and group time trend coefficient estimates not shown; *p<0.1; **p<0.05; ***p<0.01*					

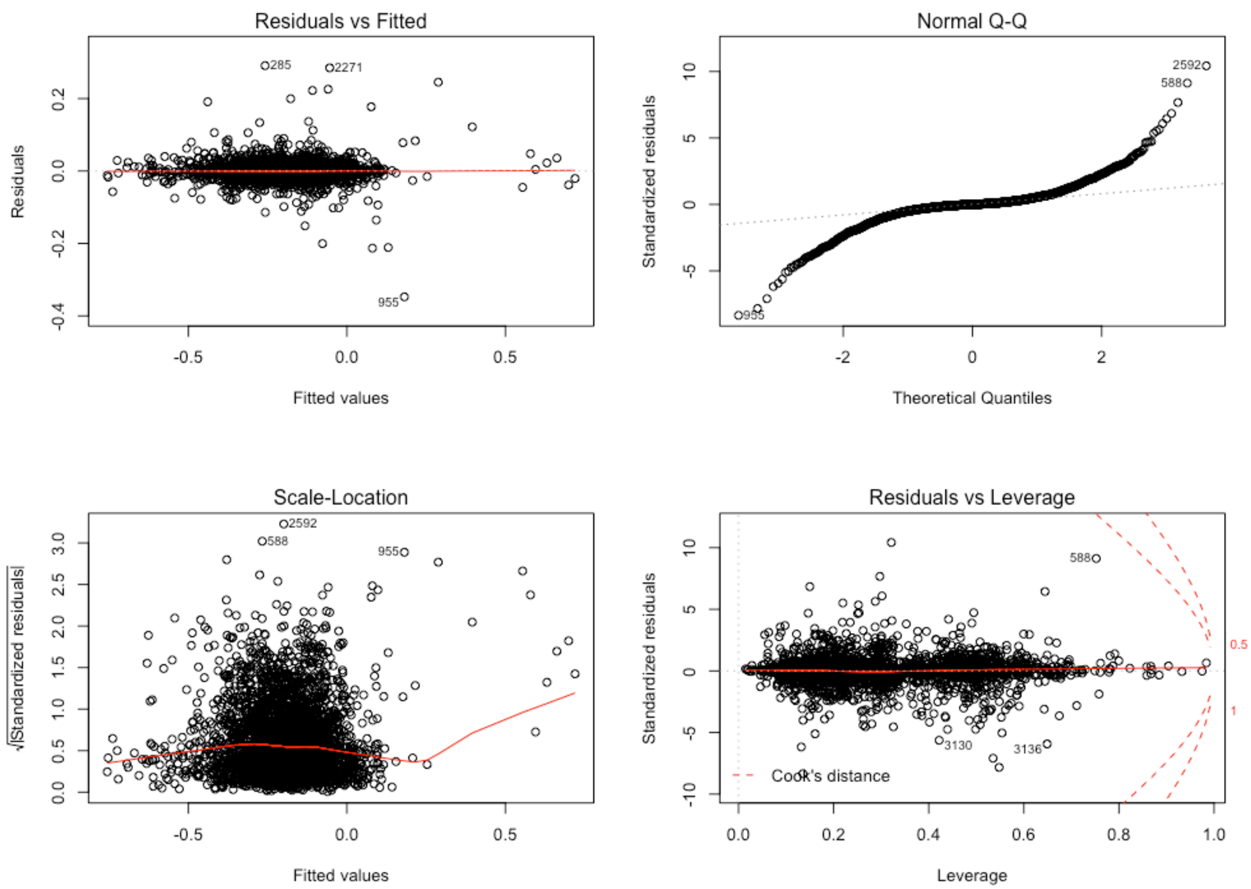


Figure 29. Diagnostic plots for an IPTW regression model estimating TNC market entry effect on vehicle ownership. The residuals-vs.-fitted values plot (top, left) confirms no structure in residual error; the normal quantile-quantile plot (top, right) illustrates a generally normal distribution of residual errors with heavy tails; the scale-location plot (bottom, left) suggests residuals are generally randomly spread over the range of fitted values (and my main text reports cluster-robust standard errors); and the residuals-vs.-leverage plot suggests no disproportionately influential observations.

<u>Heterogeneous treatment effect (HTE) detailed results</u>: the following table offers a detailed list of urban area-specific treatment effect estimates for vehicle registrations, fuel economy, and transit ridership effects, and the two plots and accompanying captions offer insight regarding the relationship between levels of significance (p-value) of HTE-estimated effects and TNC entry year by urban area.

Table 30. Urban area-specific treatment effect estimates from the heterogeneous treatment effect (HTE) regressions modeling vehicle registrations, fuel economy, and transit ridership.

Urban Area	Vehicle Reg's.	Fuel Economy	Transit Ridership
Aberdeen-Bel Air South-Bel Air North, MD	-0.039*** (0.007)	0.003*** (0.001)	-
Akron, OH	0.015*** (0.004)	0.004*** (0.001)	0.008 (0.017)
Albany-Schenectady, NY	0.003 (0.003)	0.002*** (0.001)	-0.015 (0.012)
Albuquerque, NM	-0.032*** (0.005)	0.001 (0.001)	-0.068*** (0.022)
Allentown, PA-NJ	0.050*** (0.003)	-0.005*** (0.001)	0.006 (0.014)
Amarillo, TX	0.006* (0.005)	-0.008*** (0.001)	-
Anchorage, AK	-0.084*** (0.004)	-0.004*** (0.001)	-0.050*** (0.013)
Ann Arbor, MI	-0.025*** (0.005)	0.005*** (0.002)	0.035* (0.021)
Appleton, WI	0.011*** (0.004)	-0.003*** (0.001)	-0.006 (0.021)
Asheville, NC	-0.033*** (0.005)	-0.001 (0.001)	0.267*** (0.024)
Athens-Clarke County, GA	0.013** (0.007)	0.006*** (0.001)	-0.196*** (0.023)
Atlanta, GA	0.011* (0.008)	0.011*** (0.002)	-0.052** (0.023)
Atlantic City, NJ	0.001 (0.006)	0.006*** (0.001)	-0.047** (0.021)
Augusta-Richmond County, GA-SC	-0.010** (0.006)	-0.004*** (0.001)	-
Austin, TX	0.049*** (0.005)	0.012*** (0.001)	0.347*** (0.021)
Bakersfield, CA	0.013** (0.006)	0.003*** (0.001)	-0.082*** (0.017)
Baltimore, MD	-0.023*** (0.004)	0.002** (0.001)	0.005 (0.015)
Barnstable Town, MA	0.004 (0.005)	-0.001 (0.001)	0.108*** (0.017)
Baton Rouge, LA	-0.096*** (0.005)	-0.008*** (0.001)	0.121*** (0.019)
Bellingham, WA	-0.016*** (0.005)	0.000 (0.001)	-0.082*** (0.016)
Beloit, WI-IL	-0.008*** (0.003)	-0.008*** (0.001)	-
Billings, MT	-0.012*** (0.004)	-0.023*** (0.001)	-0.114*** (0.017)
Binghamton, NY-PA	-0.011*** (0.004)	0.003*** (0.001)	-0.040*** (0.015)
Birmingham, AL	-0.005** (0.003)	-0.003*** (0.001)	0.066*** (0.013)
Bloomington, IN	-0.005 (0.005)	-0.002* (0.001)	0.078*** (0.025)
Boise City, ID	0.033*** (0.006)	-0.003*** (0.001)	-
Bonita Springs, FL	0.043*** (0.008)	0.012*** (0.001)	-0.147*** (0.026)
Boston, MA-NH-RI	0.002 (0.006)	-0.002 (0.003)	-0.011 (0.019)
Boulder, CO	0.162*** (0.005)	0.005*** (0.001)	-
Bridgeport-Stamford, CT-NY	0.050*** (0.006)	0.001 (0.002)	-0.010 (0.026)
Brunswick, GA	-0.024*** (0.008)	0.002 (0.002)	-
Buffalo, NY	-0.012*** (0.003)	0.004*** (0.001)	0.004 (0.013)
Canton, OH	0.017*** (0.005)	-0.001 (0.001)	0.059*** (0.017)
Cape Coral, FL	0.019*** (0.006)	0.011*** (0.001)	-0.003 (0.028)
Cedar Rapids, IA	-0.021*** (0.004)	-0.008*** (0.001)	0.103*** (0.018)
Champaign, IL	-0.013*** (0.004)	-0.002*** (0.001)	0.084*** (0.017)
Charleston-North Charleston, SC	0.003 (0.004)	-0.001 (0.001)	-0.107*** (0.015)
Charlotte, NC-SC	-0.034*** (0.005)	0.003*** (0.001)	-0.072*** (0.015)
Chattanooga, TN-GA	-0.026*** (0.003)	0.000 (0.001)	0.000 (0.016)
Chicago, IL-IN	0.042*** (0.004)	0.006*** (0.001)	-0.030** (0.015)
Chico, CA	0.009* (0.006)	-0.003*** (0.001)	0.005 (0.017)

Urban Area	Vehicle Reg's.	Fuel Economy	Transit Ridership
Cincinnati, OH-KY-IN	0.054*** (0.004)	0.005*** (0.001)	-0.026* (0.018)
Clarksville, TN-KY	0.050** (0.022)	-0.003* (0.002)	-
Cleveland, OH	0.010*** (0.004)	0.009*** (0.001)	-0.069*** (0.017)
Colorado Springs, CO	0.030*** (0.005)	-0.010*** (0.001)	0.177*** (0.016)
Columbia, SC	0.018*** (0.003)	-0.005*** (0.001)	0.411*** (0.013)
Columbus, GA-AL	-0.040*** (0.005)	-0.003*** (0.001)	-
Columbus, OH	0.000 (0.004)	0.003*** (0.001)	0.048*** (0.014)
Concord, NC	-0.060*** (0.006)	-0.002** (0.001)	-
Corpus Christi, TX	-0.013*** (0.004)	0.004*** (0.001)	-0.015 (0.018)
Dallas-Fort Worth-Arlington, TX	0.082*** (0.011)	-0.002 (0.003)	-0.021 (0.030)
Danbury, CT-NY	0.020*** (0.003)	0.004*** (0.001)	-0.064*** (0.017)
Davenport, IA-IL	-0.035*** (0.005)	-0.005*** (0.001)	0.025 (0.020)
Dayton, OH	0.025*** (0.005)	-0.001* (0.001)	0.054*** (0.017)
Deltona, FL	0.036*** (0.006)	0.018*** (0.002)	-
Denver-Aurora, CO	0.063*** (0.007)	-0.011*** (0.002)	0.028 (0.022)
Detroit, MI	0.027*** (0.006)	0.004*** (0.001)	-0.185*** (0.027)
Duluth, MN-WI	0.011*** (0.003)	-0.008*** (0.001)	-0.013 (0.018)
Durham, NC	-0.048*** (0.004)	0.004*** (0.001)	-0.009 (0.015)
Eau Claire, WI	0.010** (0.005)	-0.004*** (0.001)	0.003 (0.016)
El Paso, TX-NM	0.004 (0.010)	0.003** (0.001)	-0.046** (0.026)
Elkhart, IN-MI	0.019*** (0.007)	-0.011*** (0.001)	0.188*** (0.028)
Erie, PA	0.013*** (0.004)	0.000 (0.001)	-0.071*** (0.014)
Evansville, IN-KY	-0.013*** (0.003)	-0.001* (0.001)	-0.161*** (0.017)
Fargo, ND-MN	0.012** (0.006)	-0.007*** (0.001)	-0.014 (0.020)
Fayetteville-Springdale-Rogers, AR-MO	0.008* (0.006)	-0.002* (0.001)	0.111*** (0.017)
Fayetteville, NC	-0.099*** (0.012)	0.005*** (0.002)	-0.015 (0.031)
Flint, MI	0.013** (0.006)	-0.011*** (0.001)	-0.084*** (0.035)
Fort Collins, CO	0.011*** (0.003)	-0.010*** (0.001)	0.070*** (0.014)
Fort Walton Beach-Navarre-Wright, FL	-0.012*** (0.004)	0.002*** (0.001)	-0.115*** (0.017)
Fort Wayne, IN	0.011** (0.006)	-0.005*** (0.001)	-0.021 (0.022)
Frederick, MD	-0.059*** (0.004)	0.000 (0.001)	-0.187*** (0.017)
Fresno, CA	-0.017*** (0.006)	0.004*** (0.001)	-0.026 (0.021)
Gainesville, FL	0.169*** (0.006)	0.010*** (0.001)	0.014 (0.023)
Gainesville, GA	-0.001 (0.005)	0.003*** (0.001)	-
Grand Junction, CO	-0.002 (0.009)	-0.012*** (0.002)	-
Grand Rapids, MI	0.004 (0.004)	-0.006*** (0.001)	-0.077*** (0.020)
Greeley, CO	0.070*** (0.007)	-0.019*** (0.002)	-
Green Bay, WI	0.001 (0.004)	-0.001 (0.001)	-0.072*** (0.018)
Greensboro, NC	-0.039*** (0.007)	0.004*** (0.001)	-0.077*** (0.022)
Greenville, NC	-0.035*** (0.006)	0.002** (0.001)	-
Greenville, SC	0.054*** (0.009)	-0.009*** (0.001)	0.215*** (0.024)

Urban Area	Vehicle Reg's.	Fuel Economy	Transit Ridership
Gulfport, MS	-0.012*** (0.004)	-0.002** (0.001)	0.005 (0.021)
Hagerstown, MD-WV-PA	0.130*** (0.006)	-0.006*** (0.002)	-
Harrisburg, PA	0.039*** (0.004)	-0.003*** (0.001)	-0.089*** (0.017)
Hartford, CT	-0.004 (0.005)	0.000 (0.001)	0.069*** (0.020)
High Point, NC	-0.034*** (0.004)	0.001 (0.001)	-
Holland, MI	-0.005 (0.010)	-0.014*** (0.002)	-0.014 (0.036)
Houston, TX	0.070*** (0.014)	-0.009*** (0.004)	0.041 (0.040)
Huntington, WV-KY-OH	-0.017*** (0.003)	-0.006*** (0.001)	0.065*** (0.016)
Huntsville, AL	-0.008** (0.003)	-0.003*** (0.001)	0.330*** (0.013)
Idaho Falls, ID	0.031*** (0.003)	-0.010*** (0.001)	-
Indianapolis, IN	0.007* (0.005)	-0.003*** (0.001)	0.046*** (0.014)
Indio-Cathedral City, CA	0.034*** (0.012)	0.020*** (0.002)	0.042 (0.035)
Iowa City, IA	-0.012** (0.006)	0.000 (0.001)	-0.039** (0.020)
Jackson, MS	0.002 (0.004)	-0.004*** (0.001)	-
Jacksonville, FL	0.029*** (0.004)	0.008*** (0.001)	0.064*** (0.014)
Kalamazoo, MI	-0.020*** (0.005)	-0.008*** (0.001)	0.023 (0.024)
Kansas City, MO-KS	0.006* (0.004)	-0.003*** (0.001)	-0.117*** (0.016)
Kennewick-Pasco, WA	-0.030*** (0.005)	-0.005*** (0.001)	-0.099*** (0.017)
Kenosha, WI-IL	0.005 (0.007)	0.002 (0.002)	-0.024 (0.022)
Kissimmee, FL	0.038*** (0.005)	0.014*** (0.001)	-
Knoxville, TN	-0.004 (0.004)	-0.006*** (0.001)	-0.062*** (0.017)
La Crosse, WI-MN	0.014*** (0.004)	-0.003*** (0.001)	-0.034** (0.018)
Lafayette, IN	-0.013** (0.006)	-0.006*** (0.001)	-0.038** (0.017)
Lafayette, LA	-0.046*** (0.003)	0.000 (0.001)	0.194*** (0.017)
Lakeland, FL	0.019*** (0.006)	0.013*** (0.001)	0.115*** (0.020)
Lancaster, PA	0.018*** (0.003)	-0.006*** (0.001)	-
Lansing, MI	0.071*** (0.003)	-0.003*** (0.001)	-0.036** (0.016)
Las Cruces, NM	-0.003 (0.005)	-0.005*** (0.001)	-
Las Vegas-Henderson, NV	0.018*** (0.007)	0.006*** (0.001)	0.149*** (0.025)
Lee's Summit, MO	-0.001 (0.004)	0.000 (0.001)	-
Leominster-Fitchburg, MA	0.075*** (0.003)	0.004*** (0.001)	0.001 (0.017)
Lexington-Fayette, KY	-0.016*** (0.004)	-0.001** (0.001)	-0.179*** (0.018)
Lincoln, NE	-0.015*** (0.003)	-0.008*** (0.001)	0.109*** (0.017)
Little Rock, AR	-0.017*** (0.003)	-0.002*** (0.001)	-0.013 (0.014)
Logan, UT	-0.003 (0.006)	-0.007*** (0.001)	-0.103*** (0.019)
Los Angeles-Long Beach-Anaheim, CA	0.030*** (0.008)	0.024*** (0.001)	-0.038** (0.020)
Louisville/Jefferson County, KY-IN	-0.009** (0.004)	-0.001 (0.001)	-0.061*** (0.019)
Lubbock, TX	0.004 (0.004)	-0.001** (0.001)	0.048*** (0.016)
Lynchburg, VA	-0.007** (0.003)	-0.012*** (0.001)	-0.043*** (0.018)
Macon, GA	0.016** (0.008)	-0.002* (0.001)	-

Madison, WI	0.011*** (0.005)	0.004*** (0.001)	-0.017 (0.017)	**Pittsburgh, PA**	0.006 (0.005)	0.001 (0.001)	0.060*** (0.023)	**South Bend, IN-MI**	0.010** (0.004)	-0.004*** (0.001)	-0.125*** (0.019)
Manchester, NH	0.041*** (0.005)	0.009*** (0.001)	-	**Port St. Lucie, FL**	-0.002 (0.005)	0.011*** (0.001)	0.227*** (0.031)	**South Lyon-Howell, MI**	0.041*** (0.006)	0.001 (0.001)	-
Marysville, WA	-0.003 (0.005)	-0.006*** (0.001)	-	**Portland, ME**	0.009** (0.005)	0.008*** (0.001)	0.122*** (0.020)	**Spartanburg, SC**	0.079*** (0.004)	-0.008*** (0.001)	-
Mauldin-Simpsonville, SC	0.034*** (0.007)	-0.005*** (0.002)	-	**Portland, OR-WA**	-0.007* (0.005)	-0.001 (0.002)	0.026** (0.015)	**Spokane, WA**	0.006 (0.005)	-0.018*** (0.001)	0.024* (0.018)
McKinney, TX	0.059*** (0.005)	0.010*** (0.002)	-	**Poughkeepsie-Newburgh, NY-NJ**	-0.004 (0.004)	-0.001 (0.001)	0.175*** (0.017)	**Spring Hill, FL**	-0.014** (0.008)	0.010*** (0.001)	-
Medford, OR	0.013** (0.007)	-0.005*** (0.001)	0.053** (0.024)	**Providence, RI-MA**	-0.005* (0.004)	0.001 (0.001)	-0.019 (0.015)	**Springfield, IL**	-0.006* (0.004)	-0.002** (0.001)	0.020 (0.020)
Memphis, TN-MS-AR	0.048*** (0.005)	-0.001 (0.001)	-0.090*** (0.017)	**Provo-Orem, UT**	-0.010*** (0.003)	-0.004*** (0.001)	-	**Springfield, MA-CT**	-0.066*** (0.004)	-0.006*** (0.001)	0.142*** (0.017)
Miami, FL	0.044*** (0.013)	0.004 (0.003)	-0.086*** (0.035)	**Racine, WI**	0.014* (0.010)	-0.003** (0.001)	-0.086*** (0.025)	**Springfield, MO**	-0.012*** (0.004)	-0.003*** (0.001)	-0.068*** (0.018)
Middletown, OH	0.143*** (0.008)	0.003** (0.001)	-	**Raleigh, NC**	-0.030*** (0.005)	0.004*** (0.001)	-0.117*** (0.015)	**St. Cloud, MN**	0.033*** (0.005)	-0.001 (0.001)	-0.075*** (0.015)
Midland, TX	-0.021** (0.011)	-0.025*** (0.004)	-	**Reading, PA**	0.075*** (0.004)	-0.006*** (0.001)	-	**St. Louis, MO-IL**	0.047*** (0.003)	0.006*** (0.001)	-0.042*** (0.014)
Milwaukee, WI	0.001 (0.004)	0.002*** (0.001)	-0.091*** (0.017)	**Redding, CA**	-0.115*** (0.006)	-0.002** (0.001)	-0.023 (0.033)	**Stockton, CA**	-0.002 (0.007)	0.000 (0.001)	0.099*** (0.032)
Minneapolis-St. Paul, MN-WI	0.010** (0.005)	-0.002** (0.001)	-0.014 (0.018)	**Reno, NV-CA**	0.015*** (0.006)	-0.003*** (0.001)	0.438*** (0.021)	**Syracuse, NY**	-0.007** (0.003)	0.006*** (0.001)	0.108*** (0.016)
Mobile, AL	-0.029*** (0.003)	-0.003*** (0.001)	-0.094*** (0.018)	**Richmond, VA**	-0.002 (0.005)	-0.001 (0.001)	0.008 (0.017)	**Tallahassee, FL**	0.002 (0.006)	0.005*** (0.001)	-0.152*** (0.023)
Modesto, CA	0.029*** (0.006)	0.000 (0.001)	-0.032* (0.023)	**Riverside-San Bernardino, CA**	0.040*** (0.008)	0.012*** (0.001)	-0.050** (0.021)	**Tampa-St. Petersburg, FL**	0.001 (0.006)	0.007*** (0.001)	0.005 (0.018)
Monessen-California, PA	0.032*** (0.005)	-0.007*** (0.002)	-0.021 (0.024)	**Roanoke, VA**	0.001 (0.006)	-0.008*** (0.001)	0.003 (0.021)	**Temple, TX**	-0.052*** (0.008)	0.000 (0.001)	-
Monroe, LA	-0.039*** (0.005)	-0.005*** (0.002)	-	**Rochester, NY**	-0.012*** (0.004)	0.006*** (0.001)	-0.076*** (0.016)	**Texas City, TX**	0.034*** (0.004)	-0.001* (0.001)	0.361*** (0.017)
Monroe, MI	0.015* (0.010)	-0.007** (0.003)	-	**Rockford, IL**	0.012*** (0.003)	-0.006*** (0.001)	0.016 (0.018)	**Thousand Oaks, CA**	0.028** (0.015)	0.030*** (0.002)	-
Montgomery, AL	0.028*** (0.003)	0.001 (0.001)	-0.203*** (0.014)	**Round Lake Beach-McHenry-Grayslake, IL**	0.034*** (0.005)	0.015*** (0.002)	-	**Toledo, OH-MI**	0.019*** (0.004)	-0.007*** (0.001)	-0.050** (0.023)
Muskegon, MI	-0.006 (0.005)	-0.011*** (0.001)	-	**Sacramento, CA**	-0.064*** (0.006)	0.010*** (0.001)	-0.059*** (0.015)	**Topeka, KS**	-0.010*** (0.003)	-0.013*** (0.001)	0.058*** (0.014)
Myrtle Beach-Socastee, SC-NC	0.043*** (0.004)	0.004*** (0.001)	-	**Salem, OR**	0.018*** (0.005)	-0.008*** (0.001)	0.021 (0.026)	**Trenton, NJ**	0.009** (0.005)	0.000 (0.001)	-
Nashua, NH-MA	-0.014*** (0.005)	0.008*** (0.001)	0.008 (0.018)	**Salinas, CA**	0.055*** (0.007)	0.002* (0.001)	-	**Tucson, AZ**	0.001 (0.005)	-0.003*** (0.001)	-0.047*** (0.017)
Nashville-Davidson, TN	0.011** (0.006)	0.007*** (0.001)	0.048*** (0.017)	**Salisbury, MD-DE**	-0.026*** (0.003)	0.000 (0.001)	-0.010 (0.014)	**Tulsa, OK**	0.082*** (0.004)	0.010*** (0.001)	0.076*** (0.018)
New Haven, CT	-0.018* (0.012)	0.000 (0.002)	-0.052* (0.032)	**Salt Lake City-West Valley City, UT**	-0.051*** (0.006)	-0.003** (0.002)	0.088*** (0.016)	**Turlock, CA**	0.037*** (0.006)	0.003*** (0.001)	0.149*** (0.026)
New Orleans, LA	-0.045*** (0.006)	0.004*** (0.001)	0.038** (0.019)	**San Antonio, TX**	0.037*** (0.006)	0.003** (0.001)	-0.109*** (0.021)	**Twin Rivers-Hightstown, NJ**	0.000 (0.006)	0.008*** (0.002)	-
New York-Newark, NY-NJ-CT	0.014* (0.010)	-0.008*** (0.003)	0.003 (0.029)	**San Diego, CA**	0.116*** (0.007)	0.022*** (0.002)	0.110*** (0.021)	**Urban Honolulu, HI**	0.030*** (0.006)	0.004** (0.002)	-0.006 (0.024)
Norman, OK	-0.015*** (0.004)	-0.002** (0.001)	-	**San Francisco-Oakland, CA**	0.058*** (0.008)	0.018*** (0.004)	0.028 (0.024)	**Utica, NY**	0.015*** (0.004)	0.009*** (0.001)	-
Norwich-New London, CT-RI	0.014*** (0.005)	0.003* (0.002)	-0.031* (0.022)	**San Jose, CA**	0.045*** (0.007)	0.038*** (0.004)	-0.002 (0.025)	**Virginia Beach, VA**	0.018*** (0.005)	0.000 (0.001)	-0.109*** (0.023)
Ogden-Layton, UT	-0.030*** (0.003)	-0.007*** (0.001)	-	**Santa Barbara, CA**	0.070*** (0.008)	0.016*** (0.002)	-0.055** (0.025)	**Waco, TX**	0.000 (0.004)	0.000 (0.001)	0.188*** (0.015)
Oklahoma City, OK	0.038*** (0.004)	-0.002** (0.001)	0.126*** (0.015)	**Santa Clarita, CA**	0.043*** (0.007)	0.017*** (0.001)	-0.139*** (0.017)	**Waldorf, MD**	-0.022*** (0.006)	0.009*** (0.002)	0.223*** (0.022)
Olympia-Lacey, WA	-0.028*** (0.006)	0.004*** (0.001)	-0.005 (0.020)	**Santa Cruz, CA**	0.026*** (0.006)	0.020*** (0.001)	0.030** (0.016)	**Washington, DC-VA-MD**	-0.009 (0.009)	0.000 (0.002)	-0.047* (0.031)
Omaha, NE-IA	-0.004 (0.006)	-0.008*** (0.001)	-0.044*** (0.016)	**Santa Rosa, CA**	0.021*** (0.006)	0.016*** (0.001)	-0.203*** (0.019)	**Waterloo, IA**	-0.044*** (0.004)	-0.008*** (0.001)	-
Orlando, FL	0.063*** (0.006)	0.021*** (0.001)	-0.037** (0.021)	**Sarasota-Bradenton, FL**	0.016** (0.007)	0.009*** (0.001)	-0.002 (0.024)	**Wichita, KS**	-0.006* (0.004)	-0.006*** (0.001)	-0.184*** (0.016)
Oxnard, CA	0.048*** (0.006)	0.008*** (0.001)	0.086*** (0.017)	**Savannah, GA**	-0.002 (0.006)	0.002* (0.002)	-	**Wilmington, NC**	-0.043*** (0.004)	0.004*** (0.001)	-0.137*** (0.017)
Palm Bay-Melbourne, FL	0.010*** (0.004)	0.011*** (0.001)	-	**Scranton, PA**	0.037*** (0.004)	-0.005*** (0.001)	-0.134*** (0.016)	**Winston-Salem, NC**	-0.022*** (0.004)	-0.001* (0.001)	-0.098*** (0.017)
Palm Coast-Daytona Beach-Port Orange, FL	0.029*** (0.005)	0.013*** (0.001)	0.004 (0.022)	**Seaside-Monterey, CA**	0.036*** (0.006)	0.010*** (0.001)	0.073*** (0.017)	**Winter Haven, FL**	0.036*** (0.008)	0.015*** (0.001)	-
Panama City, FL	-0.002 (0.006)	-0.002* (0.001)	-0.101*** (0.023)	**Seattle, WA**	0.011** (0.006)	0.002 (0.002)	0.092*** (0.017)	**Worcester, MA-CT**	0.026*** (0.005)	0.006*** (0.001)	0.096*** (0.019)
Pensacola, FL-AL	-0.036*** (0.012)	-0.003** (0.002)	0.653*** (0.033)	**Shreveport, LA**	-0.038*** (0.005)	0.001 (0.002)	-0.094*** (0.022)	**Yakima, WA**	-0.029*** (0.007)	-0.013*** (0.001)	-0.105*** (0.019)
Peoria, IL	-0.016*** (0.004)	-0.004*** (0.001)	-0.048*** (0.019)	**Simi Valley, CA**	0.021*** (0.008)	0.017*** (0.001)	-	**York, PA**	0.059*** (0.005)	-0.005*** (0.001)	0.162*** (0.024)
Philadelphia, PA-NJ-DE-MD	0.040*** (0.005)	0.001 (0.001)	0.036** (0.018)	**Sioux City, IA-NE-SD**	0.020*** (0.004)	-0.013*** (0.001)	-0.116*** (0.023)	**Youngstown, OH-PA**	0.006** (0.003)	0.001** (0.001)	0.083*** (0.015)
Phoenix-Mesa, AZ	0.062*** (0.007)	0.001 (0.001)	0.138*** (0.018)	**Sioux Falls, SD**	0.003 (0.005)	-0.009*** (0.001)	-0.067*** (0.015)	**Zephyrhills, FL**	0.066*** (0.003)	0.012*** (0.001)	-

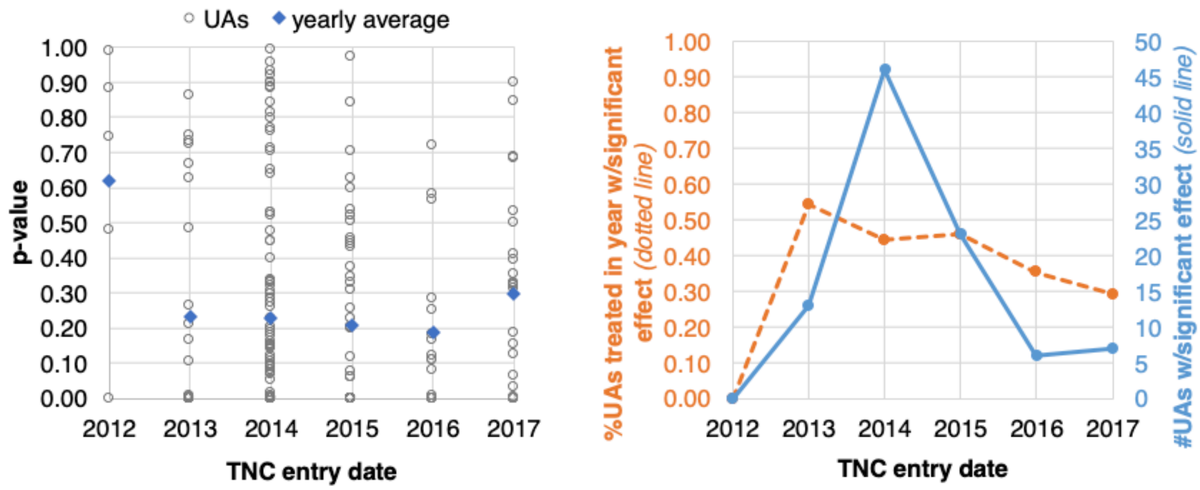


Figure 30. At left, a comparison of the level of significance (p-value) of all estimated urban area-specific TNC entry treatment effects on vehicle ownership as a function of TNC entry date into each urban area, as well as the average level of significance by TNC entry year. At right, the percentage of urban areas with a significant estimated TNC entry effect on vehicle ownership by TNC entry year as well as the distribution of the number of urban areas with a significant estimated TNC entry effect on vehicle ownership by TNC entry year.

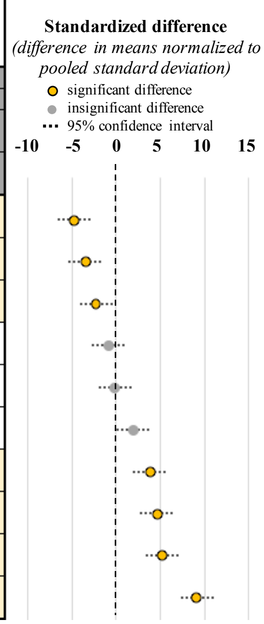
<u>Cluster analysis detailed results</u>: the following table presents estimated effects on vehicle registrations per capita and average fuel economy and descriptive statistics by cluster for clusters of urban areas estimated in a 3-cluster and 4-cluster analysis. Subsequently, a version of Figure 4 from the main text is presented as a complement and/or alternative to the 3-cluster case on which the main text focuses for discussion.

Table 31. Estimated effects on vehicle registrations per capita and average fuel economy and descriptive statistics by cluster for clusters of urban areas estimated in a 3-cluster and 4-cluster analysis.

Cluster		Largest Urban Area	n	% treated	Estimated Effect on Vehicle Registrations per capita: Estimate	Cluster s.e.	p-value	Estimated Effect on Average Fuel Economy: Estimate	Cluster s.e.	p-value
3-cluster results	1	St. Louis, MO--IL Urbanized Area	193	42.5	2.04%	0.52%	0.000	0.43%	0.11%	0.000
	2	Los Angeles--Long Beach--Anaheim, CA Urbanized Are	291	54.6	0.13%	0.46%	0.781	0.12%	0.09%	0.147
	3	New York--Newark, NY--NJ--CT Urbanized Area	1	100	-1.75%	0.32%	0.000	0.02%	0.04%	0.562
4-cluster results	1	St. Louis, MO--IL Urbanized Area	193	42.5	2.04%	0.52%	0.000	0.43%	0.11%	0.000
	2	Detroit, MI Urbanized Area	258	51.2	-0.19%	0.50%	0.711	0.13%	0.09%	0.163
	3	Los Angeles--Long Beach--Anaheim, CA Urbanized Are	33	81.8	2.58%	0.89%	0.004	-0.02%	0.24%	0.950
	4	New York--Newark, NY--NJ--CT Urbanized Area	1	100	-1.72%	0.32%	0.000	0.03%	0.04%	0.484

Cluster		Largest Urban Area	Vehicle Registrations per capita	Population	Population Density	Transit Commuting Rate	Unemployment Rate	Household income	Childless Households	Population Growth Rate
3-cluster results	1	St. Louis, MO--IL Urbanized Area	0.869	257826	1116	1.4%	8.5%	51748	68.9%	0.1%
	2	Los Angeles--Long Beach--Anaheim, CA Urbanized Are	0.805	581637	1488	2.1%	8.5%	56435	65.9%	1.2%
	3	New York--Newark, NY--NJ--CT Urbanized Area	0.545	18735857	25304	32.5%	8.7%	72644	65.5%	0.5%
4-cluster results	1	St. Louis, MO--IL Urbanized Area	0.869	257826	1116	1.4%	8.5%	51748	68.9%	0.1%
	2	Detroit, MI Urbanized Area	0.813	326420	1227	1.4%	8.6%	55002	65.7%	1.2%
	3	Los Angeles--Long Beach--Anaheim, CA Urbanized Are	0.743	2576968	3522	7.4%	7.4%	67643	67.8%	1.1%
	4	New York--Newark, NY--NJ--CT Urbanized Area	0.545	18735857	25304	32.5%	8.7%	72644	65.5%	0.5%

	Mean Value: Big, dense UAs	Mean Value: Other UAs	t-stat	p-value
# UAs in cluster	33	258		
% UAs treated	81.8	51.2		
Vehicle Reg. Effect	0.026*** (0.009)	-0.002 (0.005)		
Fuel Economy Effect	0.008 (0.008)	0.007 (0.011)		
Vehicle Reg. per cap	0.743 (0.077)	0.813 (0.090)	-4.782	0.000
Unemployment Rate	0.074 (0.017)	0.086 (0.025)	-3.515	0.000
Δ % Transit Commuters	0.008 (0.023)	0.032 (0.162)	-2.227	0.026
Δ Population Density	0.010 (0.006)	0.011 (0.009)	-0.840	0.401
Δ Population	0.011 (0.006)	0.012 (0.011)	-0.166	0.868
% Households w/no children	0.678 (0.06)	0.657 -0.074	1.854	0.064
Income	67642 (18692)	55001 (12359)	3.781	0.000
Population	2576968 (2836340)	326420 (460281)	4.550	0.000
Population Density	3522 (2513)	1227 (911)	5.201	0.000
% Transit Commuters	0.074 (0.038)	0.014 (0.013)	9.168	0.000



	Mean Value: Slow growth UAs	Mean Value: Other UAs	t-stat	p-value
# UAs in cluster	193	258		
% UAs treated	42.5	51.2		
Vehicle Reg. Effect	0.004*** (0.001)	-0.002 (0.005)		
Fuel Economy Effect	0.020*** (0.005)	0.007 (0.011)		
Δ Population Density	-0.001 (0.006)	0.011 (0.009)	-16.90	0.000
Δ Population	0.001 (0.005)	0.012 (0.011)	-13.26	0.000
Income	51748 (10606)	55001 (12359)	-3.002	0.003
Δ % Transit Commuters	0.007 (0.068)	0.032 (0.162)	-2.179	0.029
Population	257826 (343914)	326420 (460281)	-1.811	0.070
Population Density	1116 (815)	1227 (911)	-1.361	0.174
Unemployment Rate	0.085 (0.022)	0.086 (0.025)	-0.394	0.693
% Transit Commuters	0.014 (0.012)	0.032 (0.162)	-0.077	0.938
Vehicle Reg. per cap	0.869 (0.112)	0.813 (0.090)	5.677	0.000
% Households w/no children	0.689 (0.036)	0.657 (0.074)	6.110	0.000

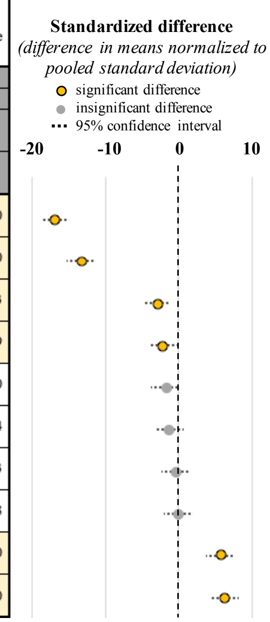


Figure 31. Cluster analysis results for the 4-cluster case: a comparison of the average characteristics of "Big, dense UAs" versus "Other UAs" (left), and "Slow growth UAs" versus "Other UAs" (right). Highlighted cells indicate significant differences (p<0.05). "Big, dense UAs" is a cluster where TNC entry results in increased per capita vehicle registrations, and "Slow growth UAs" is a cluster where TNC entry results in increased per capita registrations and increased fleet average fuel efficiency.

Interaction regression detailed results: the following series of three tables reports interaction regression results in more detail than is presented in main text Table 2. Afterwards, an additional accompanying table contrasts the mean and standard deviation for the lower and upper 50%ile for each respective interaction term: vehicle registrations per capita, population, change in population, household income, and transit commuting rate.

Table 32. Regression results estimating heterogeneity of the effect of TNC market entry ("Treatment") on vehicle ownership with a series of several estimated interaction effect coefficients: vehicle registrations per capita (here, in pre-treatment year 2010), population, population growth, household income, and transit commuting rate.

	Dependent variable: log (vehicle registrations, per cap.)				
	Vehicle Registrations per capita	Population	Population Growth	Household income	Transit Commute %
Treatment	0.004	0.004	0.007**	-0.001	0.003
	(0.004)	(0.008)	(0.003)	(0.005)	(0.005)
Treatment Interaction (upper v. lower 50%ile)	0.007**	0.003	-0.008**	0.011**	0.006
	(0.003)	(0.009)	(0.004)	(0.005)	(0.005)
Reference effect (lower 50%ile)	-0.005*	-0.031*	0.003	-0.009	-0.008
	(0.003)	(0.016)	(0.004)	(0.005)	(0.007)
Population	-0.122***	-0.121**	-0.142***	-0.127***	-0.122***
	(0.038)	(0.039)	(0.038)	(0.038)	(0.038)
Unemployment Rate	0.384	0.392	0.627	0.421	0.339
	(0.511)	(0.510)	(0.498)	(0.511)	(0.517)
Unemployment Rate^2	-5.720***	-5.636**	-6.892***	-6.086***	-5.552***
	(1.887)	(1.864)	(1.799)	(1.858)	(1.886)
Income	3.310***	3.329**	2.835**	2.992***	3.112***
	(1.148)	(1.137)	(1.133)	(1.153)	(1.131)
Childless Households	0.518	0.527	0.637*	0.490	0.522
	(0.355)	(0.356)	(0.376)	(0.365)	(0.360)
Transit Commuting	-0.432	-0.435	-0.026	-0.409	-0.318
	(0.542)	(0.548)	(0.519)	(0.545)	(0.552)
Gasoline Price	0.006***	0.006**	0.001	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	3395	3395	2910	3395	3395
Deg. Freedom	2895	2893	2409	2893	2893
Adjusted R-Sq.	0.948	0.948	0.959	0.948	0.948

Notes: *Time and group fixed effects and group time trend coefficient estimates not shown; *p<0.1; **p<0.05; ***p<0.01*

Table 33. Regression results estimating heterogeneity of the effect of TNC market entry ("Treatment") on average fuel economy with a series of several estimated interaction effect

coefficients: vehicle registrations per capita, population, population growth, household income, and transit commuting rate.

	Dependent variable: log (average fuel economy)				
	Vehicle Registrations per capita	Population	Population Growth	Household income	Transit Commute %
Treatment	0.003***	-0.004**	0.000	-0.001	-0.001
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Treatment Interaction (upper v. lower 50%ile)	-0.006***	0.005**	0.000	0.002*	0.002**
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Reference effect (lower 50%ile)	0.005***	-0.009***	0.000	-0.004***	-0.001
	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)
Population	0.025***	0.028***	0.031***	0.027***	0.028***
	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)
Unemployment Rate	0.014	0.064	0.106	0.067	0.044
	(0.108)	(0.108)	(0.099)	(0.113)	(0.113)
Unemployment Rate^2	-0.058	-0.304	-0.629	-0.461	-0.291
	(0.453)	(0.448)	(0.421)	(0.466)	(0.466)
Income	1.601***	1.634***	1.653***	1.602***	1.589***
	(0.432)	(0.426)	(0.424)	(0.449)	(0.437)
Childless Households	0.134**	0.148**	0.119**	0.137**	0.148**
	(0.061)	(0.062)	(0.051)	(0.059)	(0.063)
Transit Commuting	0.270	0.305*	0.176	0.316*	0.324*
	(0.172)	(0.171)	(0.144)	(0.174)	(0.173)
Gasoline Price	0.005***	0.005***	0.003***	0.005***	0.005***
	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Observations	3395	3395	2910	3395	3395
Deg. Freedom	2895	2893	2409	2893	2893
Adjusted R-Sq.	0.980	0.980	0.986	0.979	0.979

Notes: *Time and group fixed effects and group time trend coefficient estimates not shown; *p<0.1; **p<0.05; ***p<0.01*

Table 34. Regression results estimating heterogeneity of the effect of TNC market entry (“Treatment”) on transit ridership with a series of several estimated interaction effect

coefficients: vehicle registrations per capita, population, population growth, household income, and transit commuting rate.

	Dependent variable: log (transit trips, per cap.)				
	Vehicle Registrations per capita	Population	Population Growth	Household income	Transit Commute %
Treatment	0.002	-0.030	-0.007	-0.002	0.033
	(0.013)	(0.022)	(0.014)	(0.018)	(0.023)
Treatment Interaction (upper v. lower 50%ile)	-0.005	0.032	0.013	0.002	-0.045*
	(0.016)	(0.022)	(0.015)	(0.019)	(0.024)
Reference effect (lower 50%ile)	0.004	-0.048	0.012	-0.013	0.045**
	(0.020)	(0.049)	(0.014)	(0.020)	(0.019)
Population	0.036	0.033	-0.018	0.035	0.065
	(0.098)	(0.096)	(0.125)	(0.097)	(0.095)
Unemployment Rate	-3.807	-3.843	-3.224	-3.786	-3.171
	(2.376)	(2.394)	(2.500)	(2.387)	(2.354)
Unemployment Rate^2	16.655*	16.866*	14.705	16.045	14.611
	(9.996)	(10.048)	(9.974)	(9.979)	(9.965)
Income	7.346*	7.212*	6.771	7.287*	8.855**
	(4.024)	(4.035)	(4.366)	(4.126)	(4.200)
Childless Households	2.560**	2.551*	2.488*	2.525*	2.769**
	(1.284)	(1.311)	(1.470)	(1.367)	(1.304)
Transit Commuting	1.623	1.610	1.632	1.652	1.248
	(1.824)	(1.817)	(1.978)	(1.797)	(1.847)
Gasoline Price	0.006	0.007	0.006	0.006	0.008
	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)
Observations	1848	1848	1584	1848	1848
Deg. Freedom	1567	1567	1304	1567	1567
Adjusted R-Sq.	0.998	0.998	0.998	0.998	0.998

Notes: *Time and group fixed effects and group time trend coefficient estimates not shown; *p<0.1; **p<0.05; ***p<0.01*

Table 35. Comparison of mean and standard deviation for the lower and upper 50%ile for each respective interaction term: vehicle registrations per capita, population, change in population, household income, and transit commuting rate.

	Lower 50%ile Mean (Std. Dev.)	**Upper 50%ile Mean (Std. Dev.)**
Vehicle Registrations per capita	0.752 (0.063)	0.908 (0.095)
Population	97,482 (25,909)	883,140 (1,773,708)
Change in Population	-0.10% (1.34%)	1.58% (2.02%)
Household Income	45,254 (3,877)	63,955 (12,200)
Transit Commuting Rate	0.60% (0.27%)	3.15% (3.16%)

Main text summary figure data: this section concludes with a data table detailing the values of each data point plotted in main text Figure 5 and summary table comparing the relationships between TNC market entry effects and the dimensions of heterogeneity identified as significant in each of the HTE, cluster, and interaction regression analyses.

Table 36. Data shown in main text Figure 5 by urban area: estimated TNC entry effect on vehicle registrations per capita, household income, and population growth rate.

	Estimate	Income	Population Growth Rate		Estimate	Income	Population Growth Rate
Aberdeen--Bel Air South--Bel Air North, MD	-4.5%	$ 80,206	0.4%	Mobile, AL	-2.7%	$ 43,750	0.1%
Albuquerque, NM	-2.8%	$ 51,651	0.7%	Modesto, CA	3.8%	$ 50,630	0.7%
Allentown, PA--NJ	4.8%	$ 58,997	-0.4%	Monessen--California, PA	3.4%	$ 40,670	-0.4%
Anchorage, AK	-8.9%	$ 76,054	0.5%	Monroe, LA	-3.6%	$ 38,697	0.2%
Ann Arbor, MI	-3.0%	$ 58,261	0.9%	Montgomery, AL	2.8%	$ 48,281	-0.4%
Asheville, NC	-4.2%	$ 45,143	0.9%	Myrtle Beach--Socastee, SC--NC	3.7%	$ 43,477	2.7%
Baton Rouge, LA	-9.4%	$ 54,396	0.9%	New Orleans, LA	-5.7%	$ 46,335	1.6%
Binghamton, NY--PA	-0.9%	$ 47,083	-0.4%	Ogden--Layton, UT	-3.9%	$ 63,596	1.5%
Boise City, ID	2.1%	$ 57,052	1.7%	Oklahoma City, OK	3.6%	$ 52,290	1.6%
Bonita Springs, FL	3.2%	$ 59,313	1.7%	Olympia--Lacey, WA	-2.7%	$ 64,173	1.2%
Boulder, CO	15.5%	$ 59,312	-1.5%	Orlando, FL	6.2%	$ 54,852	1.8%
Bridgeport--Stamford, CT--NY	5.6%	$ 91,415	-0.5%	Pensacola, FL--AL	-4.0%	$ 46,631	0.6%
Cedar Rapids, IA	-2.4%	$ 57,213	0.8%	Phoenix--Mesa, AZ	6.1%	$ 57,194	1.3%
Charlotte, NC--SC	-3.5%	$ 60,899	2.3%	Raleigh, NC	-3.4%	$ 68,221	2.7%
Chattanooga, TN--GA	-3.0%	$ 48,407	0.8%	Reading, PA	7.6%	$ 53,213	-0.1%
Chicago, IL--IN	4.1%	$ 64,438	0.2%	Redding, CA	-11.2%	$ 43,760	0.0%
Clarksville, TN--KY	4.8%	$ 48,143	3.7%	Round Lake Beach--McHenry--Grayslake, IL--WI	2.8%	$ 75,308	0.3%
Colorado Springs, CO	2.7%	$ 61,701	0.8%	Sacramento, CA	-7.1%	$ 63,810	1.1%
Columbus, GA--AL	-3.6%	$ 44,194	1.4%	Salisbury, MD--DE	-2.7%	$ 50,374	1.1%
Concord, NC	-5.1%	$ 49,201	1.1%	Salt Lake City--West Valley City, UT	-6.0%	$ 62,879	2.4%
Davenport, IA--IL	-3.3%	$ 49,793	0.3%	San Diego, CA	11.0%	$ 65,008	0.2%
Dayton, OH	2.6%	$ 51,266	-0.2%	San Francisco--Oakland, CA	5.5%	$ 76,253	1.3%
Denver--Aurora, CO	5.5%	$ 64,730	1.9%	Santa Barbara, CA	6.8%	$ 69,125	0.8%
Detroit, MI	3.5%	$ 55,536	-0.1%	Santa Clarita, CA	4.5%	$ 89,521	0.8%
Durham, NC	-5.2%	$ 54,392	1.9%	Santa Rosa, CA	1.3%	$ 62,617	0.7%
Fayetteville, NC	-9.3%	$ 45,045	1.2%	Scranton, PA	3.8%	$ 45,402	-0.1%
Frederick, MD	-5.8%	$ 87,305	1.1%	Seaside--Monterey, CA	2.6%	$ 63,809	0.8%
Gainesville, FL	16.2%	$ 40,285	-2.3%	Shreveport, LA	-3.3%	$ 44,100	0.3%
Greeley, CO	6.9%	$ 44,232	1.5%	Simi Valley, CA	1.9%	$ 89,958	0.4%
Greensboro, NC	-4.8%	$ 49,000	1.2%	South Lyon--Howell, MI	3.2%	$ 73,081	0.7%
Greenville, SC	5.0%	$ 45,026	1.2%	Spartanburg, SC	6.2%	$ 43,724	-0.3%
Hagerstown, MD--WV--PA	12.6%	$ 51,198	-2.5%	Springfield, MA--CT	-6.4%	$ 56,130	0.5%
Harrisburg, PA	4.0%	$ 60,349	0.7%	St. Cloud, MN	3.2%	$ 51,034	0.7%
High Point, NC	-3.2%	$ 46,105	0.6%	St. Louis, MO--IL	4.4%	$ 59,156	0.2%
Jacksonville, FL	2.7%	$ 55,109	0.7%	Tulsa, OK	8.4%	$ 51,796	1.0%
Kennewick--Pasco, WA	-3.2%	$ 59,480	2.1%	Turlock, CA	4.7%	$ 51,028	0.9%
Kissimmee, FL	3.0%	$ 48,553	2.8%	Urban Honolulu, HI	3.6%	$ 71,684	0.9%
Lafayette, LA	-4.6%	$ 49,643	1.1%	Waldorf, MD	-4.0%	$ 94,150	1.3%
Lansing, MI	6.5%	$ 48,834	-1.1%	Waterloo, IA	-4.4%	$ 44,154	0.4%
Leominster--Fitchburg, MA	7.3%	$ 58,633	-0.7%	Wilmington, NC	-4.3%	$ 50,575	1.7%
Logan, UT	-2.2%	$ 48,046	1.6%	Winter Haven, FL	3.6%	$ 41,390	1.6%
Manchester, NH	3.9%	$ 68,718	0.3%	Worcester, MA--CT	2.1%	$ 70,396	0.0%
Mauldin--Simpsonville, SC	2.5%	$ 59,653	2.4%	York, PA	5.5%	$ 53,345	-0.8%
Memphis, TN--MS--AR	4.8%	$ 50,573	0.4%	Zephyrhills, FL	6.6%	$ 46,112	0.1%
Middletown, OH	14.1%	$ 50,523	-2.4%				

Table 37. Summary of heterogeneity in estimated TNC entry effect on vehicle registrations per capita, average fuel economy, and transit ridership along dimensions of vehicle registrations per capita, population, population growth, household income, and transit commuting rate.

	Vehicle Registrations per capita			Average Fuel Economy			Transit Ridership		
	HTE	Cluster	Interaction	HTE	Cluster	Interaction	HTE	Cluster	Interaction
Vehicle Registrations per capita	–	+	+	–	+	–			
Population	+	–		+	–	+			
Population Growth	–	–	–		–				
Household income		–	+	+	–	+			
Transit Commute %	+	–		+	–	+			–

3.6.3. Robustness Checks

This section offers detailed results and/or illustrative diagrams as evidence for the robustness check summary offered in the main text. Tables and figures are organized to align with the order of robustness checks described and summarized in the main text: randomized treatment, leave-one-out, leave-multiple-out (alternatively called discontinuity sensitivity), event study, alternative quantiles and continuous interactions, and alternative clustering.

For the first few robustness checks—randomized treatment, leave-one-out, and leave-multiple-out—an illustrative set of histograms is presented as an indication that, despite systematic perturbations to the regression specifications presented in the main text (i.e., via scrambling treatment dates, as in the randomized treatment test, systematically excluding each urban area, as in the leave-one-out test, or systematically excluding increasing numbers of urban areas with vehicle registration trends that could be labeled discontinuities), results remain consistent in terms of sign, magnitude, and level of significance.

An event study is presented in the form of a relative time model estimating in the years before and after TNC entry annual effects on vehicle registrations per capita. Neither average fuel economy nor transit ridership are analyzed, as regressions modeling those dependent variables in the main text did not identify a significant TNC entry effect to test.

The last sets of tables and figures focus on alternative approaches to the interaction regression and clustering analyses. Alternative methods for interaction regressions include variations on the number of quantiles included in the categorical interaction model described in the main text as well as an additional model using continuous measures of each interaction variable. And, for clustering, alternative approaches include variations to distance measure (i.e., Manhattan vs. Euclidean), linkage function (complete vs. Ward's), hierarchy (divisive vs. agglomerative), and features included for clustering (several reasonable approaches are contrasted).

Regression robustness checks: randomized treatment, leave-one-out, and leave-multiple out: the next few figures present a series of related histograms for an illustrative case from the main text. Each is presented as an indication that systematic perturbations to the regression specifications presented in the main text (i.e., via scrambling treatment dates, as in the randomized treatment test, systematically excluding each urban area, as in the leave-one-out test, or systematically excluding increasing numbers of urban areas with vehicle registration trends that could be labeled discontinuities) do not change the results reported in the main text in terms of sign, magnitude, and level of significance.

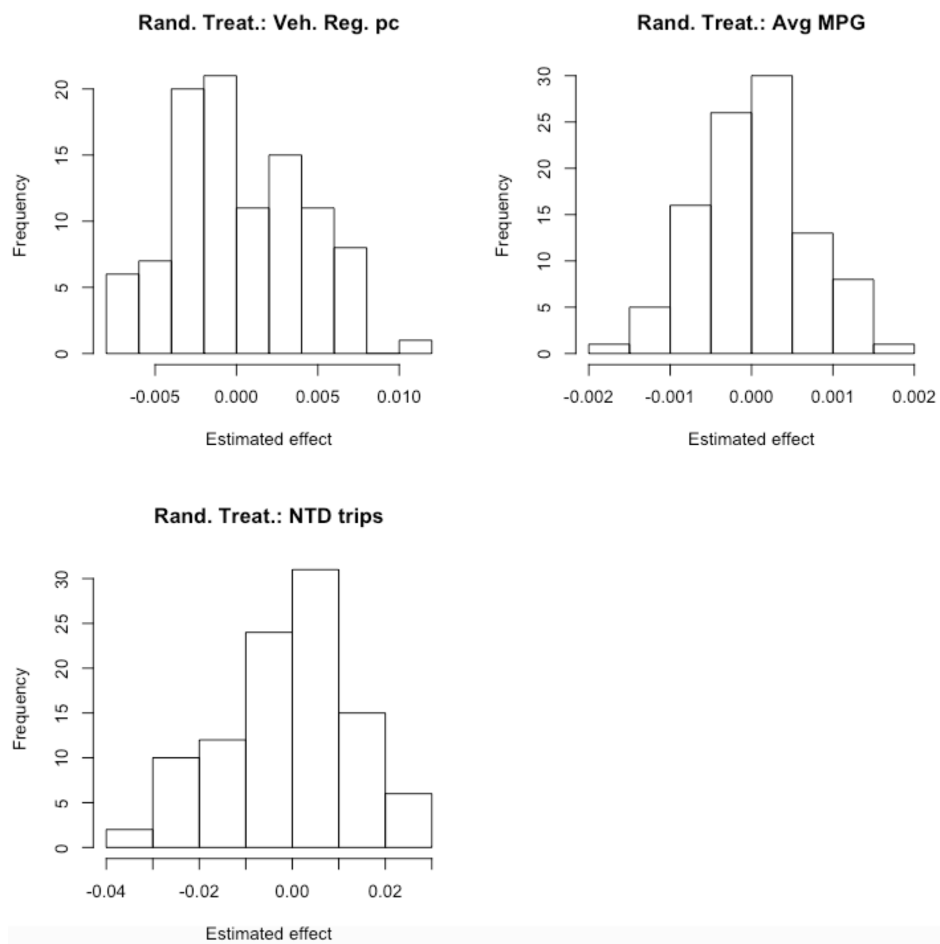


Figure 32. Distributions of average TNC entry effect estimated on vehicle registrations per capita (top left), average fuel economy (top right), and transit ridership (bottom left) after randomizing TNC entry across urban areas. In general, the distributions are centered about zero and symmetric, suggesting that the estimated effects in the main text are not the inadvertent result of model structure.

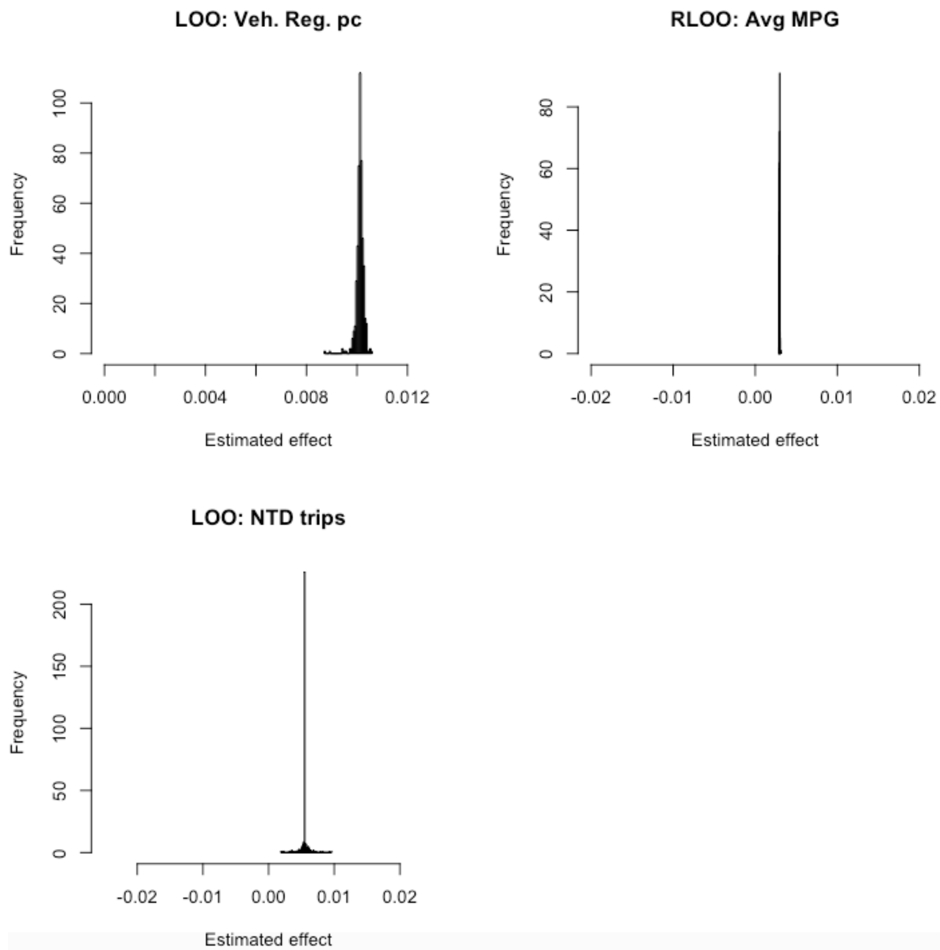


Figure 33. Distributions of average TNC entry effect estimated on vehicle registrations per capita (top left), average fuel economy (top right), and transit ridership (bottom left) after systematically excluding one urban area at time. Limits of the x-axis are set to match the confidence interval presented in the main text, such that the tightness of the distribution is meaningful. In general, the distributions are centered tightly about the value estimated including all urban areas, suggesting no one urban area has undue influence in biasing or driving the estimates presented in the main text.

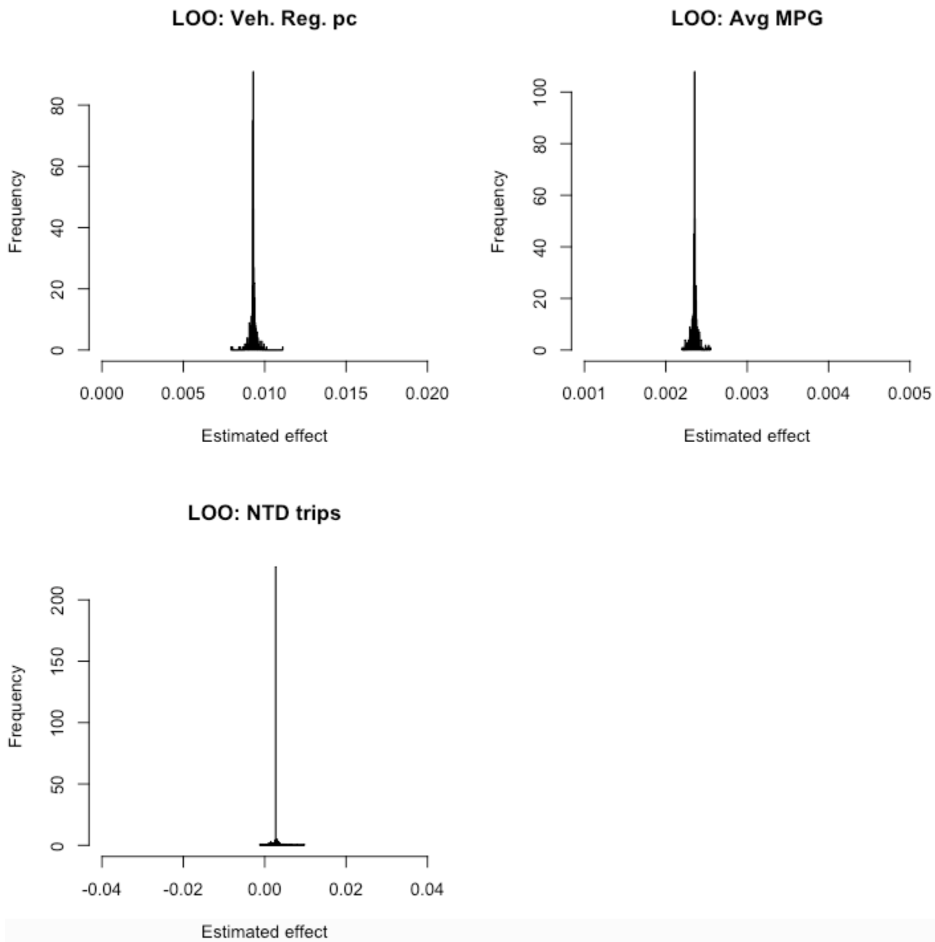


Figure 34. Distributions of the estimated interaction effect between TNC entry and a categorical measure of income on vehicle registrations per capita (top left), average fuel economy (top right), and transit ridership (bottom left) after systematically excluding one urban area at time. Limits of the x-axis are set to match the confidence interval presented in the main text, such that the tightness of the distribution is meaningful. In general, the distributions are centered tightly about the value estimated including all urban areas, suggesting no one urban area has undue influence in biasing or driving the estimates presented in the main text.

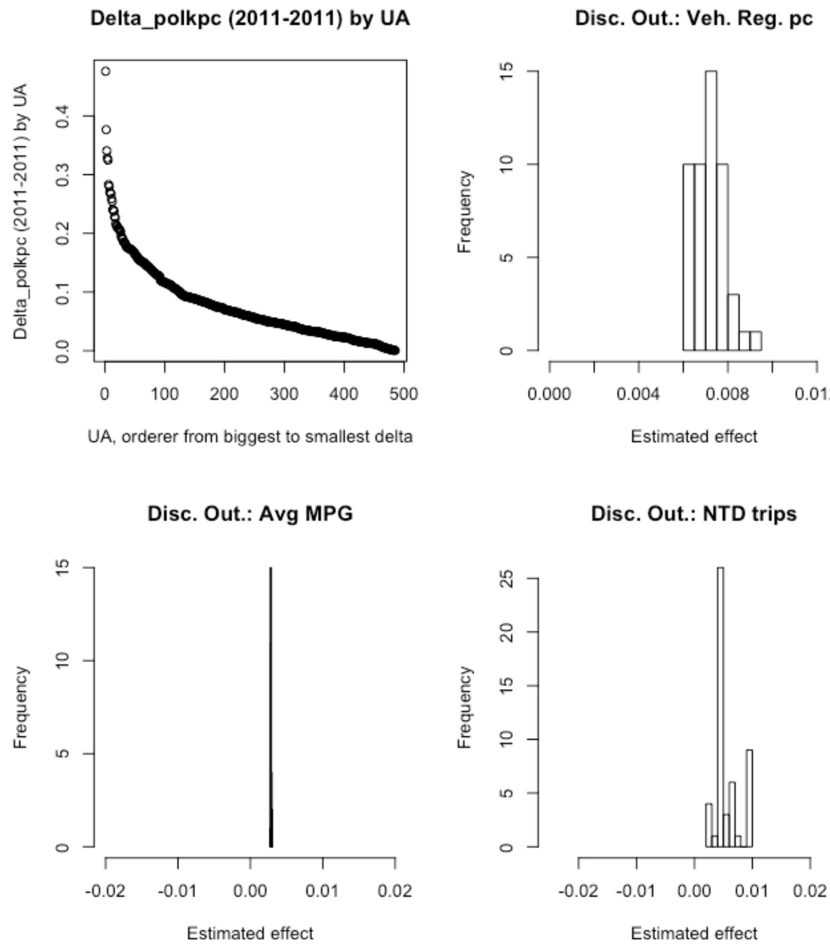


Figure 35. Distribution of changes in per-capita vehicle registrations over the 2011–2017 analysis period by urban are (top left) as well as distributions of the estimated interaction effect between TNC entry and a categorical measure of income on vehicle registrations per capita (top right), average fuel economy (bottom left), and transit ridership (bottom right) after systematically excluding an increasing number of urban areas in order greatest change in vehicle ownership over the analysis period. Limits of the x-axis are set to match the confidence interval presented in the main text, such that the tightness of the distribution is meaningful. In general, the distributions are centered tightly about the value estimated including all urban areas, suggesting no one urban area has undue influence in biasing or driving the estimates presented in the main text.

Event study: the following figure presents a relative time model estimating in the years before and after TNC entry annual effects on vehicle registrations per capita. Neither average fuel economy nor transit ridership are analyzed, as regressions modeling those dependent variables in the main text did not identify a significant TNC entry effect to test.

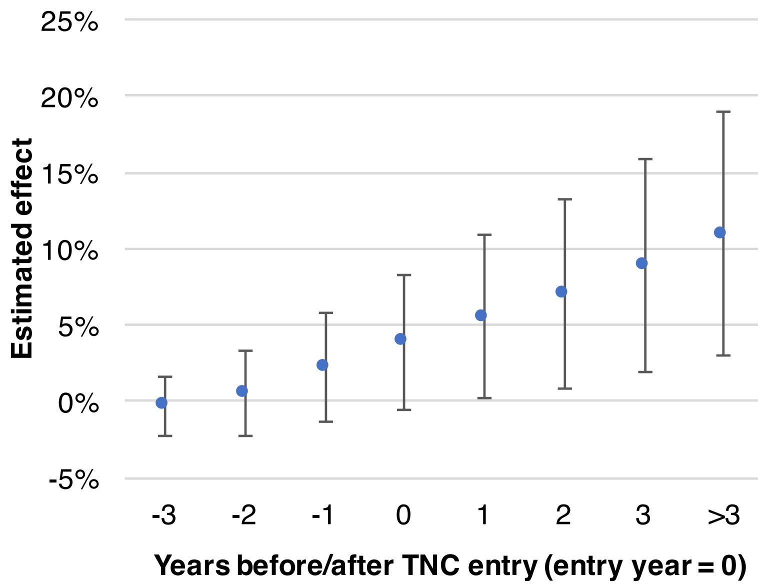


Figure 36. Event study showing estimated relative time effect of TNC entry on vehicle registrations per capita.

Alternative quantiles and continuous interactions: the next figure and table present alternative approaches to the interaction regression presented in the main text. The figure depicts variations on the number of quantiles included in the categorical interaction model described in the main text, and the table summarizes an alternative regression model using continuous measures of each interaction variable.

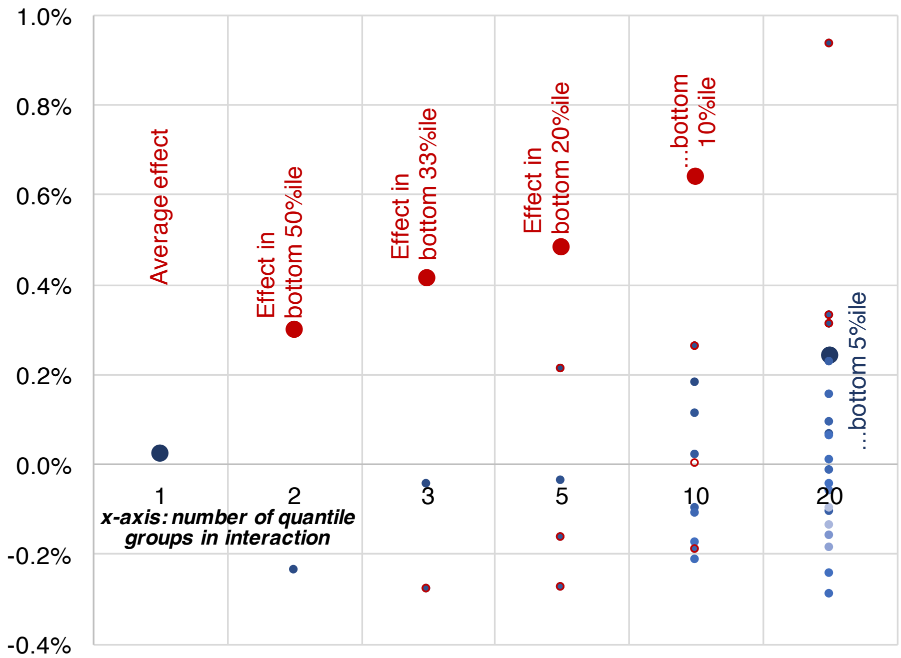


Figure 37. Estimated effect of TNC entry on average fuel economy for varying quantiles (from 2, i.e. lower vs. upper 50%ile, to 20, i.e., increments of 5 %ile) of a categorical measure of vehicle ownership. The x-axis reflects the number of quantiles included, and the bottom-most quantile (i.e., lowest vehicle ownership) is emphasized with larger points for ease of interpretation. Darker points reflect lower levels of vehicle ownership; lighter points reflect higher values. All significant effects are highlighted in red (bottom quantile as a red dot; other quantiles with a red ring). The figure makes clear that there is a significant increase in fuel economy for lower levels of vehicle ownership, and the pattern associated with that increase is fairly consistent across the varying quantiles.

Table 38. Treatment effects of TNC entry in the U.S. from regression models estimating vehicle registrations per capita, average fuel economy, and transit ridership per capita using continuous measures (rather than categorical measures) of interaction variables.

Interaction variables:	Vehicle Registrations per capita	Population	Population Growth	Household income	Transit Commute %
			Dependent variable: log(Vehicle Registrations, Per Cap.)		
Reference effect (lower 50%ile)	0.008**	0.004	0.007**	0.006*	0.005
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Continuous Interaction	0.005	2.69E-9	-0.516***	8.69E-8	0.084
	(0.006)	(1.66E-9)	(0.155)	(2.12E-7)	(0.068)
Observations	3381	3395	2910	3395	3395
Deg. Freedom	2894	2894	2409	2894	2894
Adjusted R-Sq.	0.949	0.948	0.960	0.946	0.948
			Dependent variable: log(Average Fuel Economy)		
Reference effect (lower 50%ile)	0.010	9.72E-5	-0.001	-8.60E-4	1.23E-4
	(0.008)	(0.001)	(0.001)	(6.64E-4)	(0.001)
Continuous Interaction	-0.013	1.76E-10	0.029	1.34E-7**	0.013
	(0.010)	(4.63E-10)	(0.026)	(6.94E-8)	(0.024)
Observations	3395	3395	2910	3395	3395
Deg. Freedom	2894	2894	2409	2893	2894
Adjusted R-Sq.	0.980	0.978	0.986	0.982	0.979
			Dependent variable: log(Transit Trips, Per Cap.)		
Reference effect (lower 50%ile)	-0.011	0.003	0.001	5.42E-4	0.004
	(0.029)	(0.013)	(0.011)	(0.013)	(0.014)
Continuous Interaction	0.009	-2.43E-9	-0.310	-6.38E-7	-0.115
	(0.036)	(2.50E-9)	(0.667)	(7.06E-7)	(0.133)
Observations	1848	1848	1584	1848	1848
Deg. Freedom	1568	1568	1304	1568	1568
Adjusted R-Sq.	0.998	0.998	0.998	0.998	0.998

*Notes: Covariate, time and group fixed effects, and group time trend coefficient estimates not shown; *p<0.1; **p<0.05; ***p<0.01*

Alternative clustering: the next figures depict alternative approaches to clustering, including variations to distance measure (i.e., Manhattan vs. Euclidean), linkage function (complete vs. Ward's), hierarchy (divisive vs. agglomerative), and, in a separate set of related figures, features included for clustering (several reasonable approaches are contrasted).

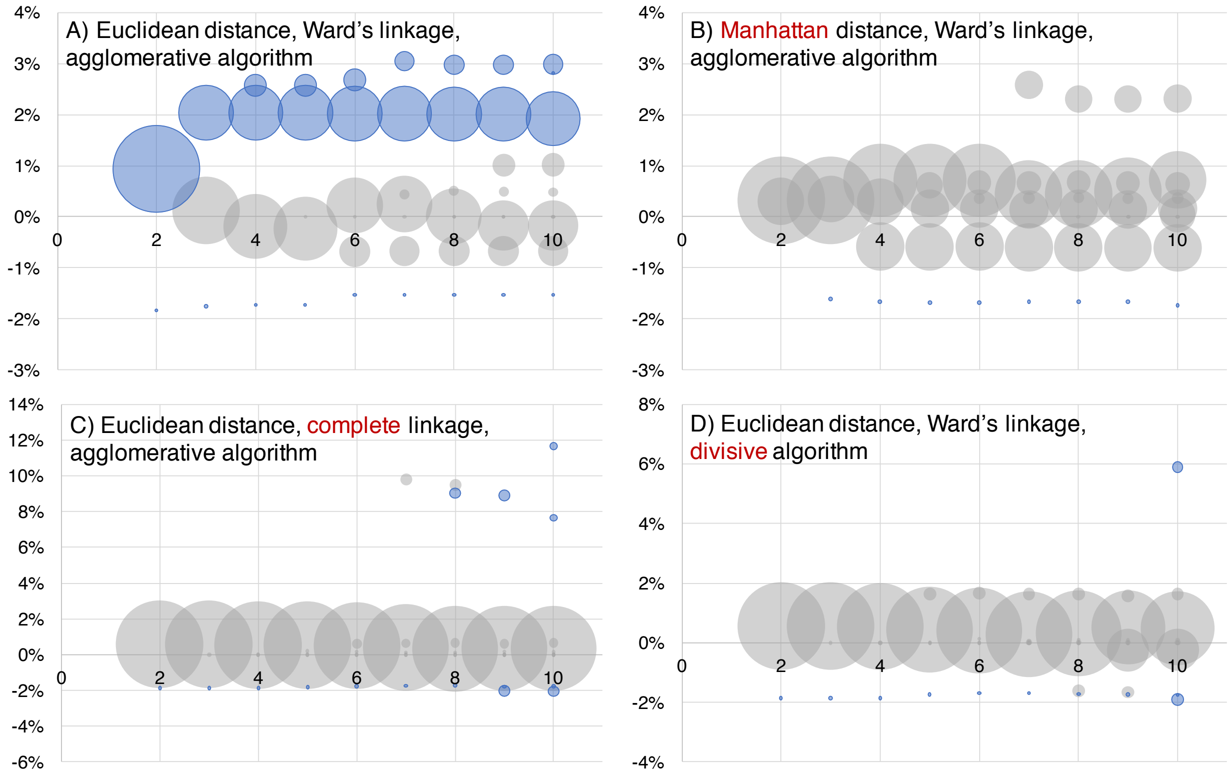


Figure 38. TNC treatment effect on the change in per-capita vehicle registrations varying by urban area typology and as a function of clustering method: Euclidean vs. Manhattan distance (panel A vs. B), Ward's vs. complete linkage (panel A vs. C), and agglomerative vs. divisive algorithm (panel A vs. D). In all cases, statistically significant effects are highlighted in blue, and estimates that are not significant are grey. The size of each circle reflects the number of urban areas in each cluster.

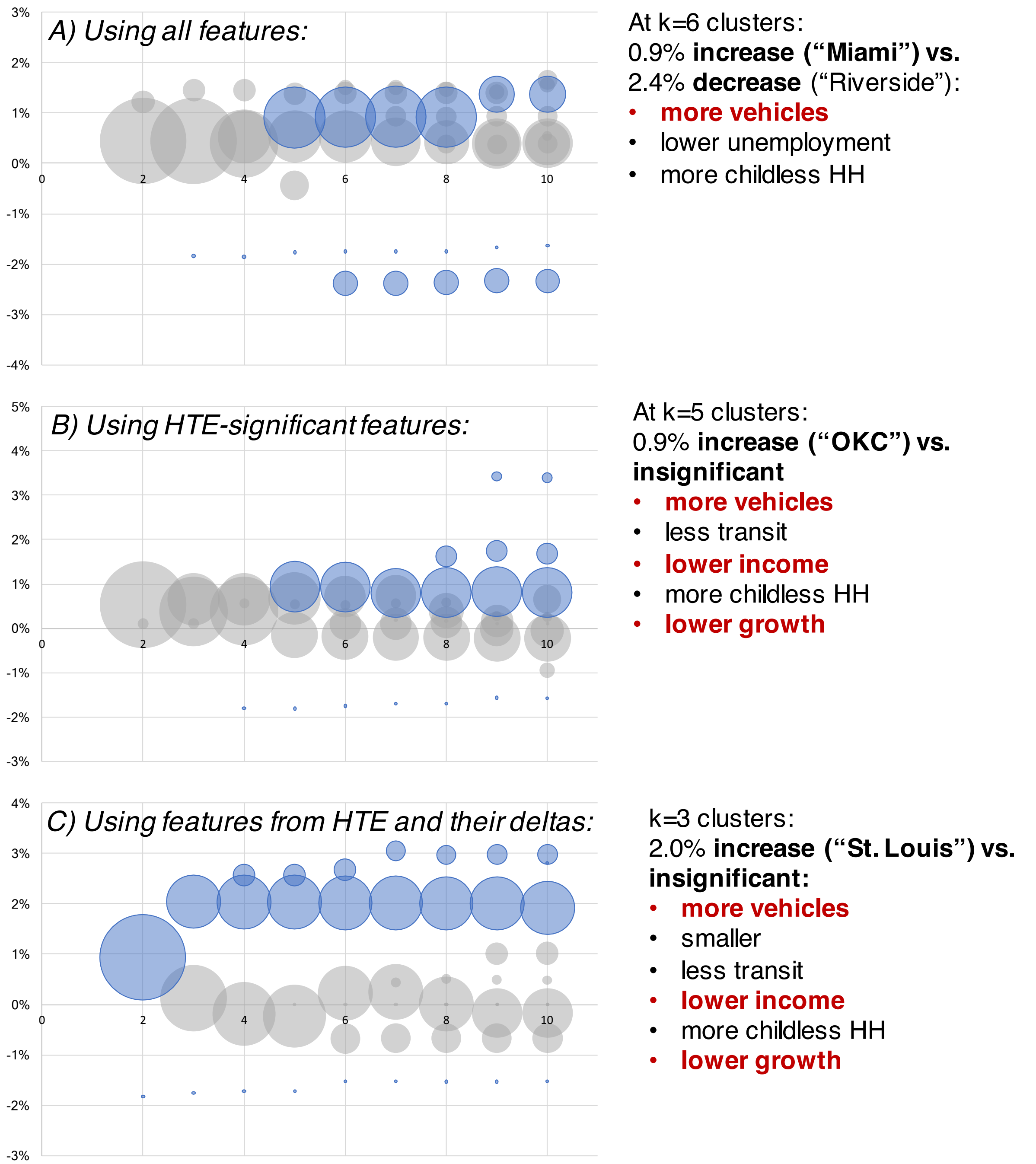


Figure 39. TNC treatment effect on the change in per-capita vehicle registrations varying by urban area typology and as a function of the urban area features used for classification: all features (panel), only significant features identified in the HTE analysis (panel B), and significant features identified in the HTE analysis plus a measure of their rates of change (panel C). In all cases, statistically significant effects are highlighted in blue, and estimates that are not significant are grey. The size of each circle reflects the number of urban areas in each cluster.

3.7. References

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Chapter 4. Cleaning Air but Clogging Streets? The Environmental Tradeoffs of Shifting Personal Travel from Private Vehicles to Transportation Network Companies

This study is based on a working paper co-authored with Jeremy Michalek and Constantine Samaras and is in preparation for journal submission.

In this chapter, I zoom in from the aggregate data and econometric tools employed at the state and urban area levels in the preceding chapters to stochastic simulation at the individual trip level to study how shifting personal travel from private to TNC vehicles affects external costs. My specific interests are firstly about net costs (or benefits) associated with changes to trip-level emissions and, secondly, how the additional consideration of travel externalities (i.e., congestion, crashes, and noise) further affects net (both emissions and traffic) external costs. These net effects are worth studying because the net effects are not obvious *a priori*: shifting personal travel from a private vehicle to a TNC vehicle can lead to both external costs and benefits. For example, on the one hand, TNCs can increase vehicle miles traveled (VMT) per passenger trip and associated emissions of greenhouse gases (GHGs) and air pollutants; on the other hand, vehicles emit far more when started cold (i.e., after the vehicle has not been used for several hours) than when driven at hot operating temperature, and TNCs can reduce cold starts per passenger mile traveled. I propose and apply a framework to quantify the external costs and benefits of shifting personal travel from private to TNC vehicles affects external costs by systematically characterizing TNC deadheading (the portion of VMT without a passenger), modeling representative TNC vehicle driving schedules and associated avoided cold starts, and quantifying the relative size of public benefits and costs from TNC vehicles for the nine largest TNC markets in the U.S. I find that shifting travel from private vehicles to TNCs offers net external benefit in some areas while incurring a net external cost in others. I conduct targeted sensitivity and policy analyses to illustrate how transportation and urban planning decisions can increase external benefits and/or reduce negative external costs.

4.1. Introduction

On-demand ridesourcing services provided by transportation network companies (TNCs), such as Uber and Lyft, have changed how many urban travelers move, and this transformation to the transportation system has been quick: in 2018, only 6 years after its first market entry, Uber served over 1.3 billion trips (over 3.5 million trips per day) in the U.S. As Figure 40 shows, shifting personal travel from a private vehicle to a TNC vehicle can lead to both external costs and benefits: on the one hand, TNCs can increase vehicle miles traveled (VMT) per passenger trip and associated emissions of greenhouse gases (GHGs) and air pollutants for two primary reasons: 1) TNC vehicles need to travel from the end of one passenger trip to the beginning of the next, and 2) many vehicles cruise additional miles while waiting for ride requests. On the other hand, vehicles emit far more when started cold (i.e., after the vehicle has not been used for several hours) than when driven at hot operating temperature (for some pollutants, one cold start produces as much emissions as 200 miles of travel at stable operating temperature), and, because Uber and Lyft often serve multiple trips each time a counterfactual private vehicle would have been started, TNCs can reduce cold starts per passenger mile traveled.

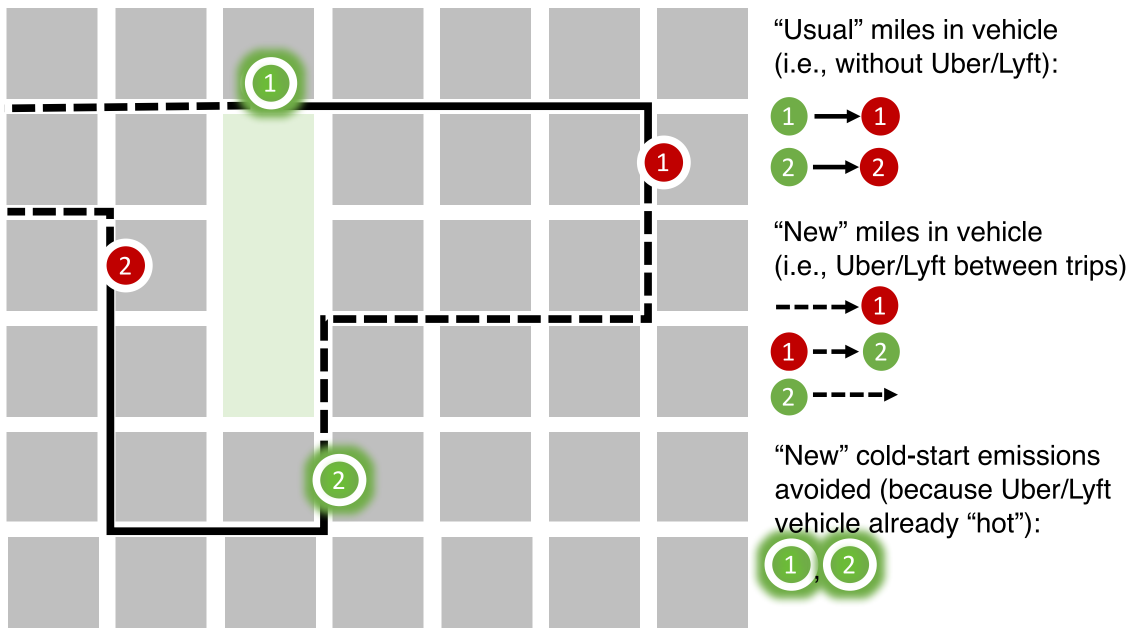

Figure 40. Illustrative schematic of personal travel in a private vehicle versus in a TNC vehicle for two example trips (denoted by the numbers 1 and 2 and the start and stop locations highlighted in green and red, respectively).

I propose and apply a framework to quantify the external costs and benefits of TNC's disruption to the transportation energy system by systematically characterizing TNC deadheading (the term commonly used to describe the portion of VMT without a passenger), modeling representative TNC vehicle driving schedules and associated avoided cold starts, and quantifying the relative size of public benefits and costs from TNC vehicles for the nine largest TNC markets in the U.S.

This chapter focuses on external costs in the U.S. to highlight and quantify changes to externalities—here, the energy-, environmental-, congestion-, crash-, and noise-related side effects and consequences not reflected in the costs of vehicle travel and that affect third parties not directly involved—that result from shifting travel from private to TNC vehicles. While oil dependency costs, the quantification and inclusion of which is still debated in recent U.S policy[1] (and which only potentially account for ~5% of relevant external costs), are not included, this set of externalities is otherwise consistent with that examined in recent literature[2]. These external costs differ from the private costs paid by a consumer to travel via private vehicle—e.g. the capital costs to purchase and finance a personal vehicle plus the operating costs of fuel and potential additional fees in the form of road tolls and parking—or TNC vehicle—e.g. TNC-imposed charges and optional tip—which have been quantified and compared previously[3,4]. External costs also differ from, but are a component of, social costs, which accounts for all private and external costs, the latter of which in a full social cost accounting would include not only the energy and emissions costs considered here but also others such as safety, privacy, discrimination, labor standards, and curb-space utilization, among possible others. Previous studies have proposed social cost frameworks to evaluate TNC travel[5,6,7]; though, exactly which external costs to include and their relative importance varies, underscoring the need for this study focused on energy and environmental external costs. And, conducting this external cost analysis for the U.S. not only affects input assumptions such as vehicle emissions and fuel economy but also the valuation of local air pollutant health damages and traffic externalities.

Additionally, this chapter focuses on the relatively near term, assuming individual trips are shifting from personal to TNC vehicles without other major changes to the transportation system. By contrast, an analysis over the slightly longer term, might consider continued TNC market growth and associated feedbacks (i.e., greater supply and demand could increase system efficiency and decrease deadheading ratios and associated externalities on a per-trip basis but increased vehicle travel overall could exacerbate damage coefficients); such dynamics are not considered here. In the even longer-term, larger-scale TNC travel could lead to fundamental changes in land use and lifestyle (as opposed to per-trip) travel behavior, which could introduce other complicating dynamics that are out of scope for this study.

4.2. Prior Literature

One the one hand, personal travel in a TNC vehicle presents an opportunity to forego turning on a private vehicle, thereby avoiding the additional air pollutant emissions and lower fuel economy associated with the period after ignition before the engine and emissions control system have heated up. On the other hand, traveling via TNC is associated with additional deadhead miles traveled and associated emissions, fuel consumption, and traffic implications. Table 39 summarizes the positive and negative tradeoffs of shifting a passenger trip from a private to a TNC vehicle, and the paragraphs that follow explore each component consideration in more detail.

Table 39. Avoided and additional emissions and fuel consumption associated with shifting a passenger trip from a private vehicle (started cold) and a TNC vehicle (started hot but incurring additional deadhead miles traveled).

Emissions and fuel consumption avoided per passenger trip shifted	Additional emissions, fuel consumption, and traffic per passenger trip shifted

• Avoided NO_x emissions: 20–30%
• Avoided $PM_{2.5}$ emissions: 18–30%
• Avoided VOC emissions: 60–75%
• Fuel economy and GHG emissions improvement from all-hot operation: 13–25%
• Fuel economy and GHG emissions improvement from more efficient TNC vehicle fleet: 14%
	• Additional miles traveled: 20–47%
	  ○ Associated NOx, $PM_{2.5}$, and VOC emissions
	  ○ Associated fuel consumption and GHG emissions
	  ○ Associated expected congestion, crashes, and noise

Starting a vehicle from ambient temperatures (i.e., roughly 25ºC) is called a "cold start", since the range of typical daily temperatures (e.g., as low as –20ºC to as high as 40ºC) are well below the operating temperatures associated with the internal combustion engine, which a coolant loop maintains around 90ºC[8], and emissions control system, optimally around 500ºC for catalytic converters[9]. Cold starts result in additional air pollutant emissions and fuel consumption for at least two principle reasons:

- First, before a catalytic converter reaches its "light-off" temperature of around 300–350ºC, little chemical reaction occurs[10,11,12] and emissions of oxides of nitrogen (NO_x)

and volatile organic compounds (VOCs)—as well as other hydrocarbons (HC)—pass into the exhaust stream largely unconverted. After the converter lights off, it converts more than 97% of carbon monoxide (CO), hydrocarbons (HC), and nitrogen oxides (NO_x) emissions to air, carbon dioxide, and water vapor[11]. NO_x and VOC emissions during cold starts have been estimated to account for 20–30% and 60–75% of total gasoline vehicle emissions, with lower proportions at warmer ambient temperatures[13], and the cold-start emissions for some VOC species have been equated to that of roughly 200 miles of hot operation (i.e., after the engine and emissions control systems reach operating temperature)[14].

- Second, when an engine starts cold, it operates less efficiently (due to higher oil pump friction, among other reasons) and starts fuel-rich, both of which contribute to elevated fuel consumption and emissions of fine particulate matter ($PM_{2.5}$)[15]. Cold starts have been estimated to decrease fuel economy by 13–25% (and 10–80% in more extreme cases)[16,17,18] during the first five to twenty minutes of operation; larger decreases are associated with lower ambient temperatures[16] and less aggressive driving behavior[17]. Similarly, cold starts increase PM2.5 emissions 18–30% per trip[13,19].

While all additional air pollutant emissions from cold starts can be problematic, I focus on the emissions of fine particulates ($PM_{2.5}$) and their precursors (SO2, NO_x, and VOCs)[20] not only because of these particles' dangerous effects on the heart and lungs but also because researchers have worked to quantify the external costs associated with these pollutant emissions, such that a comparison with other external costs is possible. In particular, AP2 (and its processor the Air Pollution Emission Experiments and Policy analysis model)[21] and InMAP (the Intervention Model for Air Pollution)[22] are reduced-complexity (so-called because they employ simplified representations of more complex and computationally demanding chemical transport models) integrated assessment models that connect emissions of local air pollutants to monetary damages associated with physical health and environmental effects. These models can effectively assign a monetary value to changes in vehicle air pollutant emissions associated with shifting a passenger trip from a private to a TNC vehicle. A third reduced-complexity model, EASIUR, is not used because the model does not offer VOC damage estimates, since those damages can vary greatly by VOC species. It is worth noting that AP2 and InMAP both assign VOC damages without differentiating VOC species.

For example, Figure 41 shows NO_x, VOC, and $PM_{2.5}$ cold start emissions from gasoline passenger car for model years 1990 through 2020, estimated using EPA's MOVES model (version 2014b)[23], and the associated AP2- and InMAP-estimated monetary damages: a MY1990 passenger car emitted 2.6 g, 3.3 g, and 0.12 g of NO_x, VOC, and $PM_{2.5}$, respectively, resulting in 22¢ in total air pollutant emissions damages per start per AP2 and 52¢ per InMAP. MY2020 cold-start emissions are more than 90% lower (as the result of EPA tightening emissions standards over time along with other voluntary emissions reductions, such as the 2001 rollout of national low emissions vehicles[24]), as are associated damages: 1.5¢ and 3¢ per start, according to AP2 and InMAP, respectively. Shifting a personal trip from a private vehicle that has to be turned on to a TNC vehicle already running hot, then, can save 1.5¢–52¢, depending on the model year of the private vehicle replaced and assuming a population-weighted average of county-level damage coefficients. For the 12 to 15 trips per TNC vehicle served daily, on average (12 according to New York City's summary statistics of for-hire vehicles[25] and 15 according to California's Clean Miles Standard base-year analysis[26]), emissions costs avoided sum to \$0.18–\$7.50, or, on a larger scale, for the 10 billion trips Uber completed between 2010 and 2018[27], that translates to between $150 million and $5.2 billion in cold-start emission damages avoided.

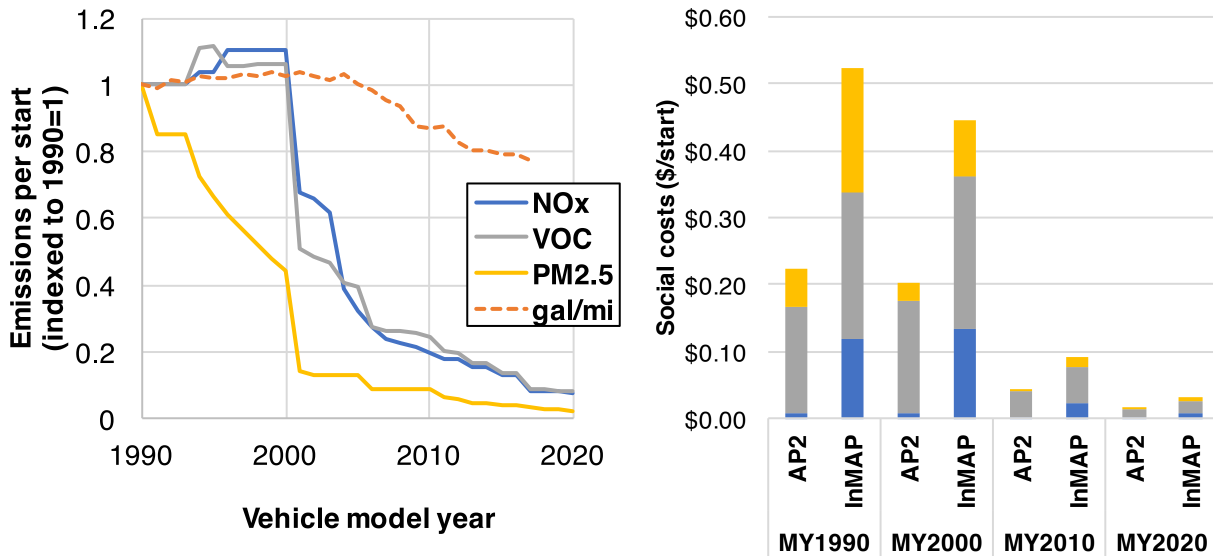

Figure 41. Average pollutant emissions per passenger car cold start, as estimated using EPA's MOVES model (edition 2014b), and fuel economy by vehicle model year (at left), indexed to 1990 values (oxides of nitrogen, NO_x = 2.6 g; volatile organic compounds, VOC = 3.3 g; fine particulate matter, $PM_{2.5}$ = 0.12 g, and fuel efficiency, 0.4 gal./mi., i.e., conventionally expressed in the U.S. as a 25.4-mpg fuel economy), and the estimated external costs associated with those emissions (at right), as estimated by the reduced-complexity models AP2 and InMAP and assuming a population-weighted average of county-level damage coefficients.

Social benefits from avoided cold-start emissions come at the expense of external costs from additional greenhouse gases emitted during additional miles traveled between passenger trips, as Figure 40 depicts. Wenzel et al.[28] and Moura et al.[29] in two separate studies summarize the range of deadheading ratios reported in other literature, spanning 20% to 47%, with specific estimates across a range of cities: including 20–44.8% for San Francisco[30,31,32,33]; 35.8–40% for Los Angeles[31,32]; 37–45% for Austin[28,34]; 40.8% for Denver[35]; 41% for New York[36]; 41–45% for Chicago[37]; 45% each for Boston and Washington, DC[31]; and 47% for Seattle[31]. But, deadhead miles (and all TNC miles) are, on average, traveled more efficiently than would be in the private vehicle replaced: Wenzel et al. also find that TNC vehicles are 14% more efficient (25.5 vs. 22.3 miles per gallon) and two years newer (model year 2011.9 vs. 2009.7)[32]. The U.S. Federal Government has estimated the social cost of carbon to be as low as 10 dollars per metric tonne and as high as 212 dollars per metric tonne (depending on discount rate, year of future damages, and central vs. high-impact estimates)[38], and more recent literature has suggested estimates could be even higher[39,40].

In addition to air pollutant and greenhouse gas external cost considerations, shifting personal travel from private to TNC vehicles has external travel cost considerations in the form of congestion, crashes and noise. In a meta-study of the external costs of transportation in the U.S., Delucchi and McCubbin explain external costs from congestion as including opportunities foregone due to travel delay (the estimates of which are described as relatively robust, given previous studies on the value of travel time) along with the discomfort of crowding and the impact of travel-time uncertainty on the reliability of arrival and delivery times (both of which are less well understood)[41]. They quantify a range of external costs ranging from 0.88–5.7 cents per passenger mile based on several previous studies[2,42,43,,44]. It is important to note that these costs are per average passenger mile, rather than marginal passenger mile, which is more relevant to shifting personal travel from a private vehicle to a higher-mileage TNC vehicle and may differ from the average value. I can look to more recent reports on variation in congestion costs by urban area as an indicator to how average costs vary and find tighter, similar, and wider proportional ranges, where the upper bound is as low as three to as high as ten times the lower bound (e.g., annual estimates of $439–$2,676[45] per commuter, $681–$2,205 per driver[46], and $100–$1,112 per person[47]).

Returning to Delucchi and McCubbin's meta-study, external costs from crashes include "medical costs, property damage, lost productivity, insurance administration, emergency services, and the nonmonetary costs of lost quality of life and pain and suffering as a result of death and serious injury," the sum of which range from 1.4–14.4 cents per passenger mile (again, average rather than marginal), also based on previous studies[2,43,44,48]. This range of uncertainty is wider than the proportional range of variability reported in a separate study on crash cost variation by urban area ($618–$3,747 per person[47]). Finally, the external costs of noise include the damages from excess noise experienced (e.g., disturbed sleep, disrupted activities, hindered work, and impeded learning) plus the cost of any defensive actions or avoidance behavior and is estimated to range from 0–0.9 cents per passenger mile[41,49].

4.3. Methods

Our analysis first extracts passenger car air pollutant emissions rates from EPA's MOVES model, extracts relevant damage estimates from two reduced-complexity integrated assessment models, and then uses those to inform a stochastic simulation of the vehicles, emissions, and travel behavior associated with what is meant to represent a TNC driver shift. Details of both my implementation of the MOVES model and my stochastic simulation follow.

4.3.1. The MOVES model

EPA's MOtor Vehicle Emissions Simulator ("MOVES") is a mobile-source emission modeling tool that quantifies criteria air pollutant, greenhouse gas, and air toxic emissions rates and inventories by transportation mode considering equipment usage and ambient conditions nationally and down to the county level. It is specifically useful for this analysis because it reports cold-start emissions for $PM_{2.5}$ and its precursors (NO_x, SO_2, and VOCs). I characterize these emissions for the set of nine U.S. counties containing the center cities that Schaller (2018) reports account for more than 70% of TNC trips in 2017[37]: Suffolk County, MA (Boston); Cook County, IL (Chicago); Los Angeles County, CA (Los Angeles); Miami-Dade County, FL (Miami); King's County, NY (New York); Philadelphia County, PA (Philadelphia); San Francisco County, CA (San Francisco); King County, WA (Seattle), and Washington, DC.

I operationalize MOVES by specifying a run specification with county-level input assumption detail. The run specification focuses on emissions rates of $PM_{2.5}$ and its precursors from gasoline-fueled passenger cars for a set of representative summer (July) and winter (January) temperatures by hour of day. For each county, I use EPA's county-specific default values for fuel formulation details, inspection and maintenance programs, and local temperature and relative humidity values. For all counties, I make arbitrary assumptions about vehicle population (50,000 passenger cars) and travel (12,000 miles annually across an even distribution of road types) since my focus is emissions per start, which is independent of both vehicle fleet size and distance traveled. I include an equal number of vehicle model years from 1990 through 2020 to ensure that cold starts are reported for each. MOVES generates a database of emissions per cold start by pollutant, vehicle model year, and temperature (by hour of day and season), which inform the stochastic simulation model posed next.

4.3.2. Air pollutant damage estimates

The reduced-complexity integrated assessment models AP2 and InMAP are used for two sets of health damages estimates for emissions of fine particulate matter and its precursors for the nine counties that will be considered in my stochastic simulation model, as are summarized in Table 40.

Table 40. Air pollutant damage estimates from reduced complexity models AP2 and InMAP for fine particulate matter ($PM_{2.5}$) and its precursors (NO_x, SO_2, and VOCs) for Suffolk County, MA (Boston); Cook County, IL (Chicago); Miami-Dade County, FL (Miami); Philadelphia County, PA (Philadelphia); and San Francisco County, CA (San Francisco).

Pollutant	RCM	Damage Coefficients by Metropolitan Area ($/tonne)				
		Boston	Chicago	Miami	Philadelphia	San Francisco
PM2.5	AP2	$ 228,457	$ 459,558	$ 164,978	$ 97,051	$ 505,105
	InMAP	$ 833,892	$ 948,086	$ 534,384	$ 117,655	$ 1,605,633
SO2	AP2	$ 45,750	$ 66,228	$ 47,403	$ 33,831	$ 128,045
	InMAP	$ 31,904	$ 53,564	$ 35,889	$ 79,137	$ 99,835
VOC	AP2	$ 680	$ 9,156	$ 1,285	$ 637	$ 3,351
	InMAP	$ 23,439	$ 44,017	$ 5,742	$ 12,651	$ 46,233
NOx	AP2	$ 21,830	$ 43,952	$ 15,659	$ 9,362	$ 47,490
	InMAP	$ 24,468	$ 27,001	$ 27,671	$ 6,256	$ 65,041

4.3.3. Stochastic scenario simulation

I structure and execute a stochastic scenario simulation tool meant to reflect personal travel via TNC vehicle at the TNC driver shift level (i.e., for a number of TNC trips taken in a single TNC vehicle over the course of one TNC driver's shift) with an explicit consideration of relevant uncertainties. Figure 42 offers a graphical summary of the model, and Table 41 summarized the model's underlying parametric distributions.

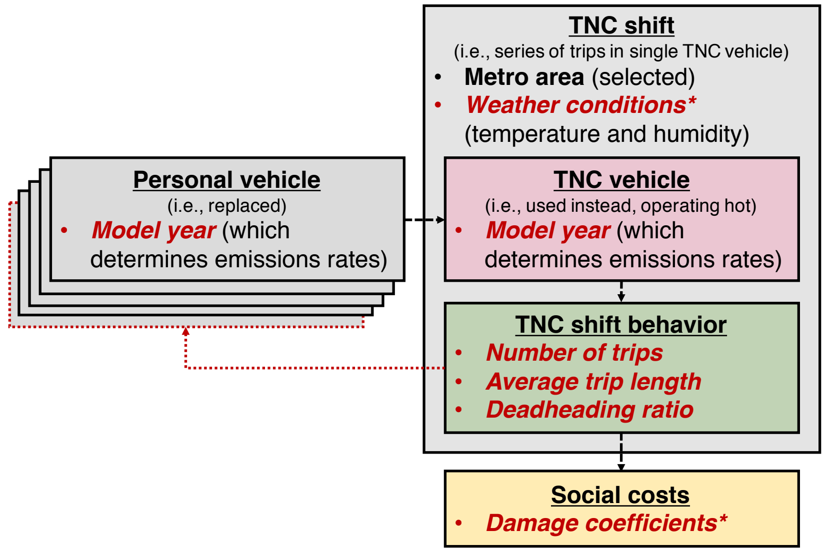

Figure 42. Schematic depicting notional flow of Monte Carlo scenario analysis at the TNC shift level. Parameters for which values are assigned stochastically are highlighted in red italics, and parameters for which underlying distributions differ by metropolitan area are denoted with an asterisk (*).

Each run begins by establishing TNC shift operating context: in which of nine metropolitan areas is the shift taking place and under what weather conditions. Next, the model year of the TNC vehicle is determined stochastically, as is the type of personal vehicle from which TNC passengers are shifting their travel. Greenhouse gas emissions rates for both the TNC vehicle and personal vehicles replaced are defined deterministically as a function of those vehicles model years. Air pollutant emission rates are defined similarly but with an additional consideration of ambient temperature (as emissions are greater at lower temperatures). Next, three parameters describing the operational details of the TNC shift—the number of trips completed during the shift, the average length of those trips, and the deadheading ratio describing the number of additional miles traveled without a passenger between those trips—are assigned. Total shift emissions, both avoided and emitted, are calculated as the product of the emissions rates and relevant characteristics describing shift operation. In a final step, external cost coefficients are assigned stochastically for air pollutants, greenhouse gas emissions, and travel (i.e., congestion, crashes, and noise) and used to convert total shift externalities to total shift external costs. Air pollutant emissions damage coefficient distributions differ by urban area; greenhouse gas emission and travel cost coefficient distributions are identical. The simulation is repeated 10,000 times for each urban area.

Table 41. Details of parameterization.

Parameter	Unit	Low	Mid	High	Distr.	Notes
TNC Vehicle						
Model year	MY	2005	2012	2020	triangular	Wenzel et al.
Fuel economy	mpg	25	31	35		Distribution from fueleconomy.gov
GHG/mi	g/mi	352.0	283.9	251.4		EPA, based on fuel economy
Replaced Vehicle Age						
Model year	MY	1990	2010	2020	triangular	Wenzel et al.
Fuel economy	mpg	20	30	35		Distribution from fueleconomy.gov
GHG/mi	g/mi	440.0	293.3	251.4		EPA, based on fuel economy
FE cold oprtn penalty	%	11		22	uniform	From ANL
TNC shift characteristics						
Passenger trips	#	2	15	30	triangular	Based on CA, Chicago values
Deadhead ratio	%	27	38.5	47	triangular	Based on CA, Chicago values
Average trip length	mi.	3	6	20	triangular	Distribution from RideAustin and Chicago
Policy factors						
Carbon damages	$/tonne	10	50	200	triangular	Inter-Agency Working Group
Congestion damages	$/PMT	0.88		7.5	uniform	From Delucchi and McCubbin
Accidents	$/PMT	1.4		14.4	uniform	From Delucchi and McCubbin
Noise	$/PMT	0		3.5	uniform	From Delucchi and McCubbin

4.4. Results

On average, shifting personal travel from a private to a TNC vehicle increases net external costs approximately 50%, or 57¢ per trip, as Figure 43 depicts. Compared to a private vehicle trip, traveling via TNC does reduce cold-start emissions almost entirely (from 5–10¢ per trip to near zero), but that benefit is almost or fully counterbalanced by an increase in operating emissions (from 25–30¢ per trip to 30–40¢ per trip). Traffic externalities account for the bulk of external costs, increasing from about 80¢ per private vehicle trip to $1.40 per TNC vehicle trip.

There is considerable heterogeneity in the results across the five counties modeled, as Figure 44 shows, and there are locations where shifting to TNCs yields net emissions benefits. In some urban areas, like Chicago and San Francisco, the external benefits of avoided pollutant emissions more than outweigh the external costs of additional GHG emissions in 80–85% of model runs. In other urban areas, Miami and Philadelphia, the emissions external costs outweigh the benefits (in 75% and 90% of runs, respectively). Across all metro areas, external benefits from emissions are greater than respective costs in 65% of model runs. Adding congestion, crashes, and noise increases expected external costs across all urban areas to roughly 50¢ per trip, which is an order of magnitude greater than the emissions-only results. Total net external costs from emissions and travel externalities combined outweigh benefits in more than 95% of model runs, both within each county and across all counties considered.

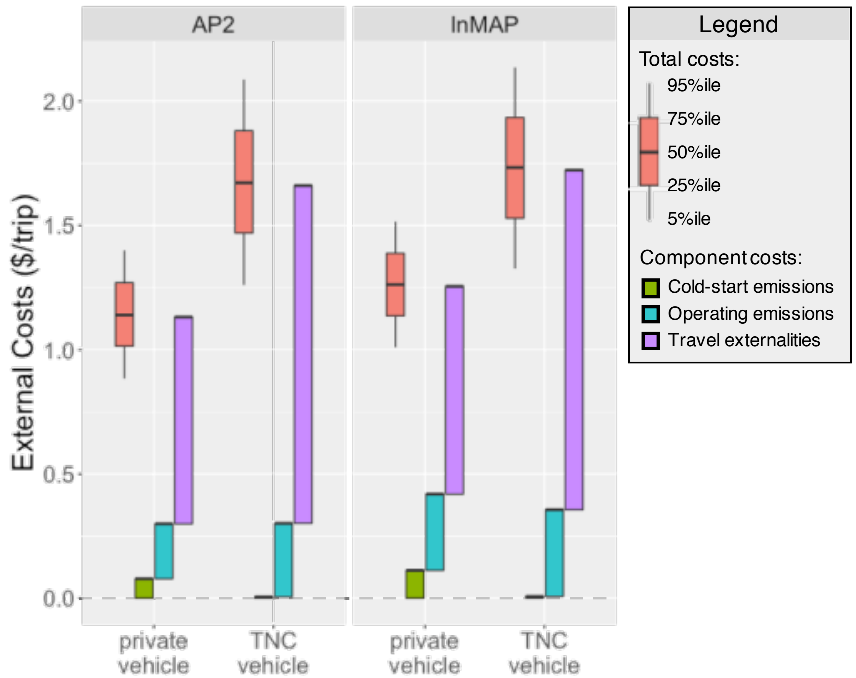

Figure 43. Simulation results for TNC travel-induced changes to external costs in total and from cost start emissions, operating emissions, and travel externalities using air pollutant damage coefficients from the AP2 and InMAP reduced-complexity models at left and right, respectively.

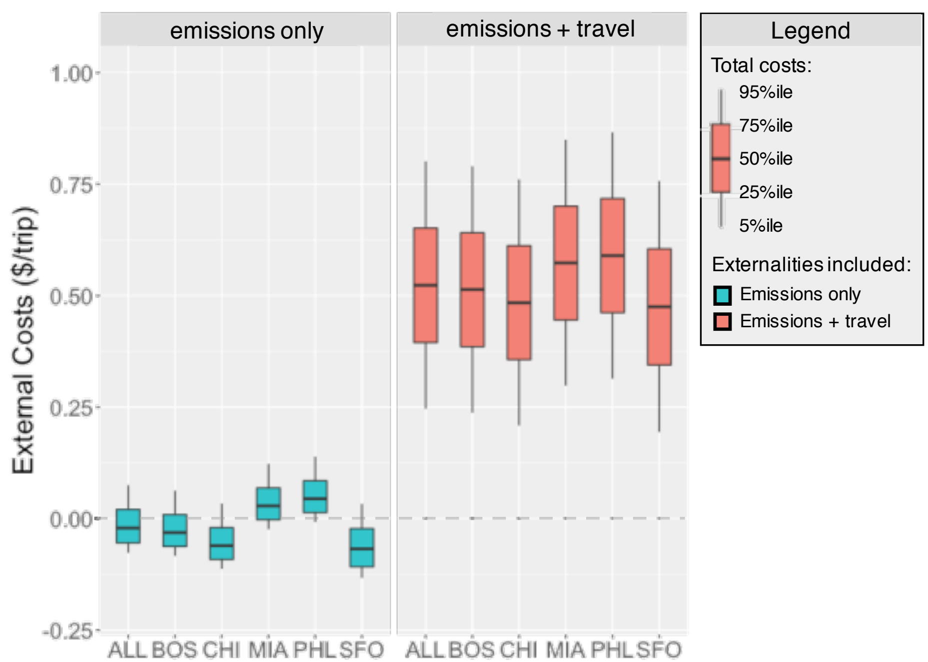

Figure 44. Changes to external costs of shifting personal trip from a private to a TNC vehicle, by county containing the following major metropolitan areas: Boston (BOS), Chicago (CHI), Miami (MIA), Philadelphia (PHL), and San Francisco (SFO).

Heterogeneity by vehicle age also matters: shifting from older private to newer TNC vehicles offers greater emissions benefits (and vice-versa: shifting from newer private to older TNC vehicles results in greater emissions costs). As Figure 45 shows, from a "passenger perspective", for the oldest private vehicles considered here (i.e., MY1990), traveling instead in a newer TNC vehicle results in emissions benefits in all scenarios (i.e., across all model years of TNC vehicle); whereas, for the newest private vehicles considered here (i.e., MY2020), traveling instead in any TNC vehicle results in increase emissions external costs. So, owners of MY1990 and MY2020 vehicles can always and never reduce emissions by taking a TNC instead, respectively. From a "TNC driver perspective", emissions benefits or cost are not as categorically obvious: whether a TNC vehicle is relatively old or new, the direction of the change in emissions costs still depends on the private vehicle replaced.

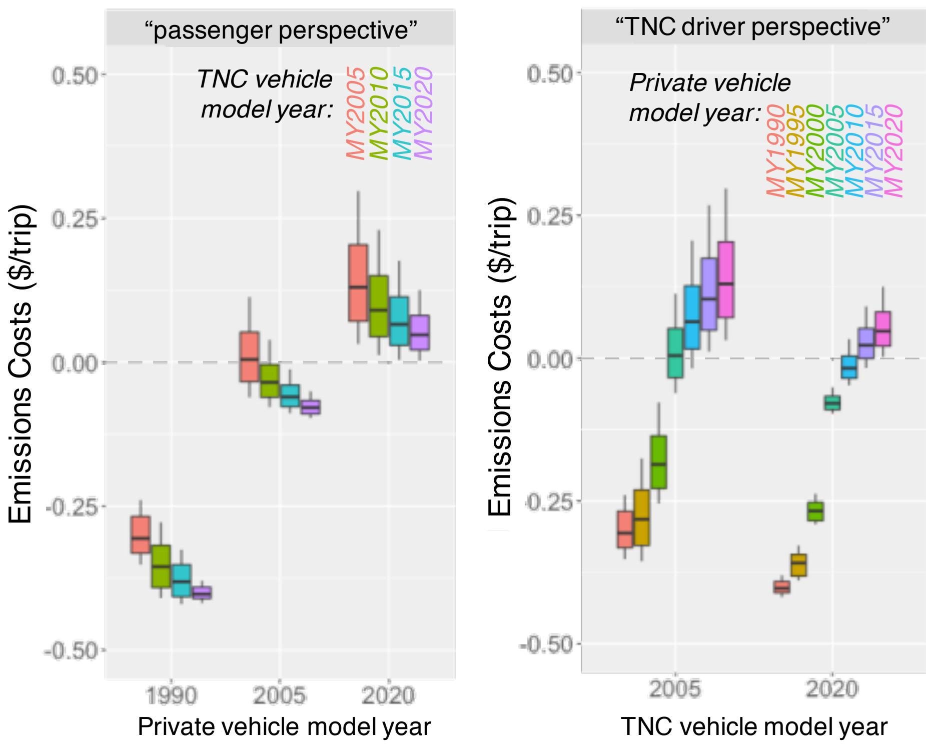

Figure 45. Sensitivity of emissions external costs across all urban areas in this study as a function of TNC vehicle model year and private vehicle (replaced) model year.

My simulation model oversimplifies its representation of a TNC shift in at least two important ways. First, all TNC trips are assumed to replace a trip that would otherwise happen in a private automobile; when, in reality, some trips are shifted from other modes (transit, walking, biking) or would not have been taken at all. Our doing so overestimates the benefit of avoided cold starts, since not every TNC trip involves not starting a private vehicle. Second, I ignore ride-pooling, wherein multiple TNC trips are provided in the same TNC vehicle at the same time, or with at least some overlap. Doing so potentially underestimates avoided cold starts, assuming pooled trips are shifted from multiple different private vehicles, and potentially overestimates deadheading costs, since multiple passenger miles can be traveled at a time.

I pose two illustrative variations on my simulation to explore what incorporating a measure of mode shifting and ride-pooling would have. First, I designate a range of 33%–66% of

TNC trips shifted from what I call zero-emission modes (as an illustrative bound) based on survey results from several survey studies[35,37,50,51,52,53] compiled and reported in Anair et al. (2020)[29]. Next, I consider the case where 10%–40% of TNC trips are pooled, borrowing the extreme values from two separate datasets on pooled rides[29,54]. Figure 46 presents the results of the modified simulation model. With a consideration of mode shifts, TNC trips always incur external costs (across all urban areas modeled), both for emissions alone and for emissions and travel externalities taken together, and a TNC trip diverted from a zero-emissions mode can incur up to three times the incremental external costs of shifting that trip from a private vehicle. Separately, considering trip pooling can increase both emissions- and traffic related external benefits (by taking multiple private vehicles off the road per pooled TNC trip), such that total incremental external benefits and costs are counterbalanced when 40% of trips are pooled (costs are higher for lower levels, and benefits are higher for higher levels). When a TNC trip is known to be pooled, the net incremental benefit is 75¢ per trip, an increase of 250% compared to the base case.

In an additional illustrative modeling sensitivity, all TNC vehicles are electric and powered by a zero-carbon grid. In this scenario, travel externality costs still swamp any zero-carbon grid EV emission benefits. This observation has potential regulatory ramifications: while electrification offers external benefits in an emissions-only framework, it overlooks the larger problem (at least in terms of the relative magnitudes of respective external cost) of travel externalities.

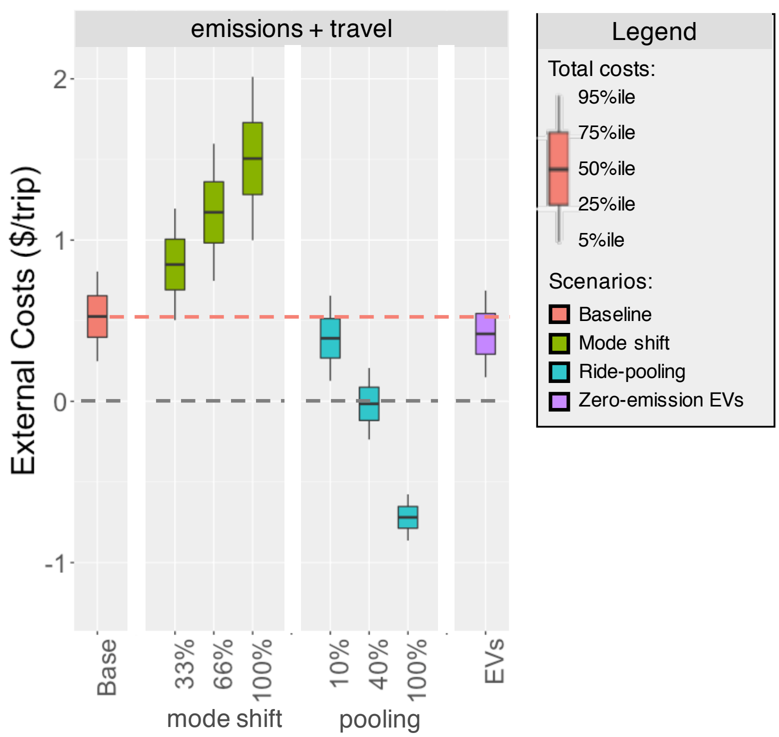

Figure 46. Distributions of external costs considering trips not replacing a personal vehicle (i.e., diverted from an assumed zero-carbon transportation mode or induced from no travel) (a) from

emissions only and (b) from emissions and travel along with distributions of external costs considering pooled trips (c) from emissions only and (d) from emissions and travel.

In a final sensitivity variation on my simulation (not shown), I consider an additional dimension of uncertainty: whether adding an additional vehicle to a potentially already crowded road (i.e., shifting a trip to a TNC vehicle and associated marginal VMT) incurs greater external costs than does a vehicle on the road on average (i.e., average VMT). Published literature suggests marginal VMT external costs could be equivalent to those of average VMT[55], as much as 60% higher[56], or somewhere in between[57]. Adding a marginal-mile multiplier (from a uniform distribution from 0 to 60%) increases expected net external costs by 27% and the proportion of runs where external costs exceed benefits to 99% across model runs (and in at least 97% of runs for each respective metro area).

4.5. Discussion

Our results suggest that shifting personal travel from a private to a TNC vehicle can causes changes in vehicle emissions that correspond to both net external costs and benefits. In some urban areas, and on average overall, the benefits associated with avoided cold-start emissions do not outweigh the costs associated with additional emissions from deadhead travel; however, in other urban areas, like Chicago and San Francisco, they do. In general, emissions benefits are greater when the TNC vehicle is newer, and, according to my analysis, TNC companies could ensure travel shifted from a non-TNC vehicle always offers expected net emissions benefits by restricting the TNC vehicle fleet to model years 2018 and newer. Similarly, emissions benefits are greater when the private vehicle replaces is older, and, owners of private vehicles from model year 2008 or older can always reduce their expected net emissions costs by shifting a trip from their private to a TNC vehicle. Additionally, avoided cold-start emissions benefits increase at lower temperatures, such that shifting travel to a TNC vehicle in colder weather increases net emissions benefits (or decrease costs).

We find the net effect of ridesourcing on energy use is, on average (without mode shifting, ride-pooling, or vehicle electrification), a 45% increase (with a 95% quantile interval ranging from 42% to 48%), which agrees generally with Wenzel et al.'s finding that, despite a more efficient TNC fleet (compared to the overall fleet of private vehicles), shifting travel to a TNC vehicle increases net energy use 41–90%. This analysis also adds an explicit consideration of cold starts and offers a broader framework that also includes traffic externalities and finds that while shifting travel from a private to a TNC vehicle reduced cold-start emissions, the net effect is a 50% increase in total external costs (95% quantile interval: 39% to 62%).

Emissions benefits and costs are only part of the story, as costs associated with travel externalities (congestion, crashes, and noise) are greater than monetized emissions externalities by an order of magnitude. In fact, no emissions benefit in my analysis is sufficiently large to outweigh the external costs of travel. The biggest lever to pull, then, in working towards increasing TNC external benefits (and/or reducing external costs) is targeting and reducing congestion, crashes, and noise, for example, through special training for TNC drivers to help avoid fatal accidents (though, recent research suggests the opposite is happening: the arrival of ridehailing is associated with an increase of approximately 3% in the number of fatalities and fatal accidents for both vehicle occupants and pedestrians[58]).

4.6. Conclusions

I model the external costs and benefits of changes to air pollutant and greenhouse gas emissions and travel externalities associated with shifting personal trips from private vehicles to TNC vehicles and find that, while external benefits from avoided cold-start air pollutant emissions outweigh external costs from additional greenhouse gas emissions from deadhead VMT in some metro areas, travel externality considerations are sufficiently large to result in net external costs in almost all cases ($0.50/trip, on average). Avoided cold-start emissions benefits are higher in colder temperatures and denser metropolitan areas, and these benefits generally counterbalance costs from greenhouse gas emissions in metro areas with relatively cold winters or high population densities (or both). Shifting personal travel from older private vehicles to newer TNC vehicles also increases avoided cold-start emissions benefits: replacing a private with a TNC vehicle generally yields emissions benefits when the private vehicle is model year 2007 or older (independent of TNC model year) or when the TNC vehicle is model 2014 or newer (independent of the model year of the private vehicle replaced). When travel externalities (congestion, crashes, and noise) are included, lower deadheading ratios can reduce net external costs, but even at the lowest deadheading ratio considered (20%), external costs from travel externalities are generally not counterbalanced by potential emissions benefits.

Two variations on shifting a trip from one private vehicle to a TNC vehicle—ride pooling and mode shifting—can both increase and decrease external costs. On the one hand, mode shifting eliminates some number of avoided cold starts and increases net external costs, since TNC travel is shifted from a mode without trip-specific cold starts. On the other hand, ride pooling avoids additional cold-start emissions and effectively decreases deadheading external costs (by allocating some portion of those costs over multiple trips), such that TNC travel offers a net benefit (vs. private vehicle travel) in 25% of overall cases and 95% of cases that are pooled. Adding a consideration of mode shifting reduces the number of cases with net benefits to 13% of all trips and 50% of pooled trips, respectively.

Electrifying the TNC fleet and powering it with zero-carbon electricity reduces cold-start and greenhouse gas emissions (for a net emissions external benefit in all cases), but associated external benefits are not sufficiently large to overcome external costs from travel externalities. Net external costs are reduced only 10% compared to a baseline scenario without EVs: shifting personal travel to zero-emission EV TNC trips still incurs a net 47-cent external cost, on average, due to congestion, crashes, and noise. In fact, congestion and crashes are the largest component of net external costs across all cases modeled, such that they present the biggest target for potential policies to reduce external costs (and increase external benefits). Ride pooling can do so by distributing travel externalities across multiple trips and potentially offers the added benefit of avoiding cold starts from multiple private vehicles. Other policies to minimize deadheading (e.g., efficient routing and/or limited-to-zero unnecessary in-between passenger trip travel) can also help. Policies can also consider novel approaches to targeting travel externalities, such as training professional (or semi-professional) TNC drivers to avoid crashes (for example, recent literature has suggested that crash avoidance technology can reduce crashes by over 20%[59]) and/or to select routes that minimize system-level congestion. Alternatively, policies could internalize external costs by levying a per-trip fee, such as the $0.50/trip average external cost from the base case modeled in this study or a more sophisticated price model that considers external cost factors in real time (e.g., emissions as a function of ambient conditions, traffic congestion, etc.).

Our models do not consider several factors that could affect external benefit and cost factors and calculations. For example, I assume similar vehicle fleets—both for TNC vehicles and the private vehicles they replace—across metro areas; when, in reality, systemic differences in certain cities could increase net external benefits (either due to newer TNC vehicles, older private vehicles, or both) or vice versa. I also assume similar road networks and travel patterns across metro areas; when, again, systemic differences could increase net external benefits (due to shorter average trip distances or deadheading ratios, more trips per TNC shift or pooled trips, and/or fewer trips induced or diverted from zero-emission modes). Travel externality costs (both on average and on the margin) could also vary (e.g., as a function of differences in traffic congestion or accident rates); though, I use similar distributions across cities. Future work can build on the findings here by incorporating these factors and offer complementary insights by considering potential implementations of and associated effectiveness, efficiency, and equity of potential policies mentioned above. Finally, better data to inform the parameters modeled using uncertainty distributions here will offer broad benefits for future work, policy and policymakers, and knowledge about TNC effects on personal travel more generally.

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Chapter 5. Conclusions and Contributions

This thesis provided an initial understanding of the potentially fundamental changes to the way Americans travel given the introduction of ridesourcing via transportation network companies (TNCs), like Uber and Lyft and the effects those changes have on energy and environmental outcomes. Complementary econometric analyses in Chapters 2 and 3 found that TNCs have already significantly affected transportation outcomes, and that the direction and magnitude of these effects depends on how the data are sliced. A first analysis in Chapter 2 found a decline in vehicle ownership, on average at the state level after TNC market entry; whereas, a second analysis in Chapter 3 found an average increase in vehicle ownership in urban areas, with the difference in sign explained by heterogeneity of effects across urban areas. The data and estimated effects look different depending how they are sliced: TNC entry tends to increase vehicle ownership more in urban areas with higher initial vehicle ownership and lower population growth rates, increase overall fleet efficiency more in urban areas with fewer childless households, and decrease transit ridership more in urban areas with higher median incomes and more childless households. A third study in Chapter 4 explored how displacing a personal with a TNC vehicle trips affects external costs and concluded that such shifts in personal travel can offer emissions benefits but always incurs a net external cost when travel externalities are included. Additional concluding contextual details for each of these studies are provided below.

Chapter 2 studied whether TNC entry increased or decreased vehicle ownership, personal travel, energy use, and emissions outcomes at the state level and whether those effects were larger in more urbanized states. Using publically available data, and two sets of inverse-probability-of-treatment-weighted (IPTW) difference-in-difference (DiD) regression models—one to estimate the average effect of TNC market entry across outcomes of interest and a second to explore heterogeneity in those effects as a function of the level of urbanization within a state—on average, I found a 3.1% decrease in vehicle registrations (based on real-world measurements), a 4.5% decline in VOC emissions (based on EPA-modeled values), and no significant effect on other outcomes. I did estimate a decline in VOC emissions but viewed that finding as exploratory, since the estimate used EPA-modeled values rather than real-world measurements and therefore warrants repeated modeling and/or other study with real-world data. Monetizing the exploratory value translated to external benefits totaling \$300 million to \$900 million. Despite these potential benefits, other external costs associated with other uncertainty outcomes meant that the net effect was also uncertain: external costs associated with VMT ranged from −\$1 billion to \$6 billion, and those for gasoline consumption ranged from −\$600 million to \$500 million, such that the net effect could range from −\$600 million to \$500 million, spanning zero and underscoring the need for further study.

Chapter 2 did not find significant effects on personal travel, energy use, or emissions outcomes in U.S. states, possibly because available data constrained the analysis to net effects at the state level (such that opposing effects could offset, such as increased miles per-trip counteracted by fewer vehicle trips, more efficient vehicles counterbalanced by more travel in those vehicles, or increased operating emissions offset by avoided cold starts), using state-level observational units could have obscured potential underlying nuance at the urban area (or finer) resolution, and TNC effects are still a relatively small component of overall state-level indicators and trends. These outcomes warrant further study, as each has policy ramifications, sometimes at multiple levels of government: increased vehicle travel intensity and associated external costs could affect federal, state, and local planning policies; changes to fuel use could affect national policies surrounding energy security as well as regional economic interests; and air pollution and GHG emissions warrant consideration by EPA and the California Air Resources Board, potentially among others.

Using higher-resolution data at the urban area level, Chapter 3 examined whether TNC entry increased or decreased vehicle ownership, fuel economy, and transit ridership outcomes as well as in what kinds of cities Uber caused vehicle ownership and efficiency and transit ridership outcomes to increase versus decrease. I again used a series of IPTW DiD regression models to estimate average effects and found evidence that TNC entry causes an average 0.7% increase in vehicle registrations and no average effect on overall fleet efficiency or transit ridership. Complementary methods to explore heterogeneity (heterogeneous treatment effects (HTE), cluster analyses, and interaction regressions) found effects different across all outcomes. First, TNC entry tended to increase vehicle ownership more in urban areas with higher initial vehicle ownership and lower population growth rates. That more vehicles prior to TNC entry resulted in even more vehicles after suggested already relatively car-dependent areas remained or became even more so after the introduction of TNCs as a travel option. And, while TNC-stimulated new vehicle acquisitions outpaced new residents in slower-population-growth areas, in faster-population-growth areas, more new residents afforded the flexibility of being a potential TNC passenger depressed overall private vehicle registrations. Second, TNC entry was estimated to increase overall fleet efficiency more in urban areas with fewer childless households; though, TNC drivers may, in reality, have bought similarly efficient vehicles across urban areas, such that in urban areas with more childless households where pre-existing vehicle stock was smaller-sized and already disproportionately efficient, TNC entry resulted in a smaller relative efficiency increase. Finally, TNC entry was found to decrease transit ridership more in urban areas with higher median incomes, where potential passengers possess a greater ability to pay for a more expensive travel mode, and more childless households, where a greater number of potential passengers are in a life stage that affords flexibility in making travel choices (i.e., TNC versus other modes).

Heterogeneity in TNC effects on vehicle ownership, fleet efficiency, and transit ridership motivates concomitant heterogeneity in policy implications. Firstly, while vehicle ownership does not translate clearly to energy and emissions outcomes, vehicle ownership is generally correlated with higher energy use, which, in turn, is entails higher emissions. In historically vehicle-reliant, slower-growth urban areas where vehicle ownership was estimated to increase, then, special attention to and further study of potential increases in energy use and associated emissions may be warranted. Secondly, increases in average fuel economy can reduce fuel consumption (though a net effect also depends on potential changes in travel behavior, which was not studied here) and associated emissions. So, while urban areas with more households with children where fuel economy was estimated to increase may see declines in fuel consumption and local emissions, other urban areas with fewer households with children may consider how to incentivize similar efficiency gains. Finally, potential declines in transit ridership are potentially worrying for higher-income localities with more childless households where ridership was estimated to decline more (relative to lower-income urban areas with fewer childless households), as transit revenues could be affected. Additionally, if changes to transit ridership are the result of trips diverted to TNC travel, then increases in energy use, emissions, and road congestion could be local concerns, too.

Chapter 4 focused at the vehicle level to examine how shifting personal travel from private to TNC vehicles affects external benefits, with a specific eye whether the external benefits of avoided cold-start emissions from private vehicles outweigh the additional external costs of deadheading between trips and how the additional consideration of travel externalities (i.e., congestion, crashes, and noise) affect total external benefits (considering both emissions and traffic effects). I proposed and applied a framework to quantify the external benefits of TNC disruption to the transportation energy system by systematically characterizing the avoided cold start emissions and additional deadhead miles and associated emissions and quantifying the relative size of external benefits from TNC vehicles for several of the largest TNC markets in the U.S. I found that shifting travel from private vehicles to TNCs offered net emissions benefits in some areas but net costs in others, and including congestion, crashes, and noise yielded net benefits everywhere ($0.50 per TNC passenger trip, on average).

Findings from Chapter 4 suggested the roughly net-zero emissions effect of shifting personal travel from private to TNC vehicles do not necessitate major policy action from a combined emissions-only perspective; however, separating air quality (a relatively local issue) from GHGs (a global issue) may raise different implications for local versus national policymakers. Additionally, travel externalities incur relatively large incremental external costs at the local level that at least warrant a consideration of changes to traffic planning and roadway allocation and passing associated costs along to TNCs to correct the market failures associated with unpriced externalities. Mode shifting exacerbated negative outcomes (and perhaps underscores a greater need for congestion controls in cities where more TNC trips are shifted from non-vehicle modes); while, ride-pooling emerged as the biggest lever for minimizing external costs (and even maximizing external benefits). Electric vehicles powered by zero-emission electricity, for example, were found to offer net emissions benefits but fail to address still relatively large travel externalities. Promising policies for increasing overall external benefits associated with TNC travel include some combination of incentivizing ride pooling and avoiding deadheading miles (when possible), targeting a reduction in the external costs of deadheading (e.g., potentially through semi-professional driver training), and/or internalizing external costs with user fees (such as the $0.50 per TNC passenger trip, on average, the study estimated).

Future work can build on the findings in this thesis by addressing specific limitations acknowledged herein and offer complementary insights by considering possible implementations of potential policy options mentioned. For example, since Chapters 2 and 3 were limited to examining net effects estimated econometrically using aggregate data, additional study could explore the component effects underlying overall increases or decreases in vehicle ownership (e.g., what are the individual relative dynamics of aspiring drivers registering new vehicles versus increasingly TNC-reliant passengers shedding old vehicles?). Similarly, Chapter 2 did not estimate significant effects on fuel consumption or several emissions species just as Chapter 3 did not estimate significant average effects for fuel economy and transit ridership, suggesting a role for further study via better-powered econometric models (i.e., with more data) or using alternative methods. In Chapter 4, TNC vehicle travel patterns and non-revenue miles in particular were critically important in driving net external cost calculations, but assumptions were based on the limited data available (i.e., for a limited number of geographic regions and/or for a limited number of vehicles). Targeted studies on fleet-wide operations to minimize deadheading promises to offer social benefits, and more and better data collection and dissemination can benefit both those studies and knowledge and policy more broadly. Additionally, external costs associated with vehicular travel and air pollutants involved large uncertainty ranges, and travel costs, in particular, weighed heavily on my results. Focused study on external travel costs at the margin (i.e., either shifting a non-vehicle trip to a TNC vehicle or a private vehicle trip to a TNC vehicle trip involving more miles traveled) and heterogeneity across urban forms by time of day is needed. Finally, and in general, more and better data on personal travel via TNCs (especially about deadheading, mode-shifting, and ride-pooling, but also about TNC vehicle characteristics and travel patterns, all with sufficient geographic and temporal resolution to be representative of any potentially important differences in effects and outcomes by location and/or time) will offer broad benefits for future work, policy and policymakers, and knowledge about TNC effects on energy consumption, greenhouse gas and air pollutant emissions, and personal travel overall.