The Cost of Convenience: Ridesharing and Traffic Fatalities

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We examine the effect of the introduction of ridesharing services in U.S. cities on fatal traffic accidents. The arrival of ridesharing is associated with an increase of 2-3% in the number of motor vehicle fatalities and fatal accidents. This increase is not only for vehicle occupants, but also for pedestrians. We propose a simple conceptual model to explain the effects of ridesharing's introduction on accident rates. Consistent with the notion that ridesharing increases congestion and road utilization, we find that the introduction of ridesharing is associated with an increase in arterial vehicle miles traveled, excess gas consumption, and annual hours of delay in traffic. On the extensive margin, ridesharing arrival is also associated with an increase in new car registrations. These effects are higher in cities with higher ex-ante use of public transportation and carpools, consistent with a substitution effect, and in larger cities and cities with high ex-ante vehicle ownership. The increase in accidents appears to persist (and even increase) over time. Back-of-the-envelope estimates of the annual cost in human lives range from $5.33 billion to $13.24 billion per year.

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1. Introduction

The introduction of ridesharing services such as Uber and Lyft have fundamentally changed how many individuals are transported in cities and towns across the U.S. on a daily basis. While the ability to easily hail a ride through an app has undoubtedly increased convenience for passengers seeking car transportation from origin to destination, critics increasingly argue that ridesharing creates other, offsetting effects, such as increases in traffic congestion and car-exhaust pollution. Are there, in fact, significant costs that come with the convenience of ridesharing? In this paper, we present evidence suggesting that such costs exist, are not trivial and can be measured in human lives—specifically, in increased rates of major traffic accidents and traffic fatalities. Using the staggered introduction of ridesharing services across U.S. cities, we show that the advent of ridesharing in a metropolitan area leads to an economically meaningful increase in motor vehicle fatalities. This increase is consistent with acknowledged macro trends in motor vehicle accidents, which had been falling steeply in the U.S. over the period 1985 to 2010 when ridesharing first launched, and have since reversed course and increased (Figure 1).[1]

Whether ridesharing should lead to higher accident rates is not apparent at first glance. A naïve view of the effects of ridesharing merely views ridesharing as removing drivers who would have driven themselves with their car and replacing them with rideshare drivers. Under this naïve view, ridesharing substitutes self-drivers with rideshare drivers on a one-to-one basis. Moreover, one might argue that many of the users who are substituting away from driving themselves into being driven are often doing so because they are (or will be) inebriated or otherwise impaired. This substitution of impaired drivers with sober rideshare drivers potentially increases the quality of driving while holding car utilization fixed. Under this view, there is no increase in the vehicle miles traveled and a possible increase in driver quality, and consequently, there should be no effect on, or even a reduction, in accident rates.

This naïve view, however, ignores many of the nuanced effects of substituting driving oneself with being driven by a rideshare driver. For example, rideshare drivers have riders in their car for only a fraction of the time that they are driving on the road: they must drive from fare to fare, and they drive from location to location in the city looking for better fare prospects as there is not

[1] Figure 1 was created by Dennis Bratland and is reproduced under creative commons license. The figure uses NHTSA FARS and CrashStats data to depict total U.S. motor vehicle deaths, deaths per VMT, deaths per capita, VMT and population for the period 1920-2017.

always a fare available. Moreover, rideshare companies often subsidize drivers to stay on the road even when utilization is low, to ensure that supply is quickly available.

Furthermore, the naïve substitution view assumes that only those that would have otherwise driven themselves are now utilizing the ridesharing system, which is unlikely. The convenience and lower pricing of ridesharing apps suggest that there may be a significant substitution away from other modes of previously available transportation, such as subways, buses, biking or walking. These individuals would have utilized these modes of transport in the absence of the convenience and low cost of ridesharing. Indeed, surveys report that fewer than half of rideshare rides in nine major metro areas actually substitute a trip someone would have made in his or her car (Schaller, 2018). Moreover, a survey conducted by UC Davis on over 4,000 residents in seven major metros areas found that only 39% of respondents would drive themselves, carpool, or take a taxi if ride-hailing had not been available. The rest substitute from rail, biking, walking or not traveling at all (Clewlow and Mishra, 2017).[2]

The survey evidence suggests that the utilization substitution is not likely to be one-for-one, as assumed in the naïve view. From a supply perspective, a local report that examines detailed ridesharing data in New York City suggests that ridesharing companies put 2.8 new vehicle miles on the road for each mile of personal driving that they eliminated (a 180% overall increase). Moreover, the same report suggests that ridesharing has added 5.7 billion miles of annual driving in the Boston, Chicago, Los Angeles, Miami, New York, Philadelphia, San Francisco, Seattle and Washington DC metro areas (Schaller, 2018). While pooling services such as UberPool and LyftLine have the potential to reduce the overall increase in vehicle miles, these modes of ridesharing currently represent a relatively small (20%) share of overall rides.

The survey data suggests a more nuanced view of the overall effect of ridesharing on road safety. We incorporate these general ideas into a conceptual framework for thinking about how ridesharing's introduction may affect accident rates.[3] While the naïve view of ridesharing holds the utilization and supply of drivers constant, our nuanced view incorporates rational choice theory to drivers and riders' decisions in the context of ridesharing. Our framework models accidents as

[2] Similar numbers emerge from studies conducted by the Boston Metropolitan Area Planning Commission (MAPC, 2018), the New York Department of Transportation (NYDOT, 2018), and other researchers (Clewlow and Mishra, 2017; Henao, 2017; Circella et al., 2018).
[3] Our theoretical analysis of ridesharing's effect on safety can be thought of along the lines of the traditional offsetting behaviors literature (Peltzman, 1975).

a function of vehicle miles traveled and average driver quality. The advent of ridesharing makes car transportation easier for riders, which should, in turn, lead to a decrease in the marginal cost of making a trip for riders, thus spurring more rides. In the case of potential drivers, the monetary value assigned to driving via the platform also increases the net benefit for individuals with vehicles of heading out to give rides. These two forces should lead to overall increases in the number of cars on the roads. Depending on the quality of the new rideshare driver entrants as compared to the driving quality of former drivers who now ride in a rideshare, this may also lead to a potential change in the average quality of drivers on the road. We outline the potential effects of the introduction of ridesharing through each of these two components.

We then turn to an empirical analysis of the effects of ridesharing on accident rates. We define the entry of ridesharing into cities using rollout dates obtained directly from Uber and Lyft. The companies provided separate launch dates for the different services offered. Thus, for each city, we have a separate launch date for UberBlack/UberTaxi, UberX, UberPool, Lyft, and LyftLine. We use the launch date of the first service to arrive in each city to determine the first quarter of treatment. Our outcome measures are a variety of fatal traffic accident related measures from the Fatal Accident Reporting system maintained by the National Highway Traffic Safety Administration (NHTSA).

We begin our analysis by examining changes in the level of accidents in the treated cities around the introduction of ridesharing. Figure 2 plots the quarterly average accident rate per 100,000 people over event time in rideshare cities. At the time of rideshare initiation—time zero—we see a distinct break in the trend of accident rates in the cities: accident rates begin to rise sharply relative to the pre-event time trend, a noticeable increase. We investigate this increase formally using a difference-in-differences specification with fixed effects for location, time (quarter-year). Consistent with the raw data plotted in Figure 2, the difference-in-differences specification documents a 2 to 4% increase in the number of fatal accidents and fatalities: throughout the week, on weekends, at night, and on weekend nights. The estimates are robust to the inclusion of a variety of control variables as well as to the addition of a location-specific linear time trend, and are similar for a variety of different specifications of the left hand side accident measure.

When we separate the accidents into those that do and do not involve a drunk driver, we find that the estimates for non-drunk accidents are similar: a 2-4% increase in accidents, across a variety of measures. For drunk accidents, estimates of the model without accounting for a location-specific time trend suggest a decrease in accidents and fatalities that is much smaller in magnitude and only weakly significant if at all. This estimate is consistent with existing studies utilizing smaller samples that limit their analysis to fatal drunk driving accidents and that do not account for city-specific time trends; when we include the location-specific trend, however, the sign and magnitudes of the estimate of the effects of ridesharing on drunk accidents are similar to those in our other specifications.[4]

Having established our primary finding, we proceed to examine differentials in outcomes for the various rideshare product lines using the launch dates for pooled versus non-pooled services: Despite allowing for more utilization of carpools, and therefore potentially reducing total vehicle miles traveled, the introduction of UberPool and LyftLine do not reverse the documented increase in fatal accidents. Instead, the estimates suggest that either the share of pooled rides is insufficiently large enough relative to single rides or that any positive effects of pooled services in reducing VMT—and accordingly, accidents—may be offset by an increase in overall ridership due to the lower cost of the pool service.

We then examine the effects of the intensity of rideshare use on accident rates. We proxy for the intensity of rideshare use by the intensity of Google searches for terms such as "Uber" and "Lyft," in the treatment cities. When we substitute the indicator for city treatment with our Google intensity proxy for the adoption and spread of rideshare services within a city, we obtain similar results to those in our main specifications: fatal accidents and fatalities increase with the intensity of adoption, as proxied for by the Google Trends measure.

Next, we separate traffic accidents and fatalities into those of car occupants and non-occupants (pedestrians, bicycle riders, etc.). Doing so allows us to examine externalities to pedestrians in the advent of ridesharing in the city. Pedestrians represent a population that is neither an occupant of a rideshare car nor driving or riding in a private vehicle. Here, we find a similar magnitude increase in the number of fatal accidents involving pedestrians, the number of pedestrians involved in such

[4] The inclusion of a location-specific linear time trend is important: accident rates, particularly drunk driving related accident rates, have been falling steeply in the U.S. over the period 1985 to 2010 when ridesharing first launched, and have since reversed course and increased. Moreover, we document that ridesharing launched first in cities that had been experiencing steeper declines in drunk accident rates. For example, cities in which ridesharing launched in 2011 had been experiencing significant declines in accident rates over the preceding five years, while cities in which ridesharing launched in 2013 were not experiencing much of a decline, and cities in which ridesharing launched in 2015 were actually experiencing increases in drunk accident rates. In the absence of accounting for these location-specific trends, a difference-in-differences model can erroneously estimate a negative effect on accidents; this estimate, however, will be driven by order of entry and the pre-existing trends, rather than an actual drop in drunk accidents.

accidents, and the number of fatalities in such accidents, suggesting that the introduction of ridesharing imposes a negative externality on pedestrians in addition to affecting vehicle occupants.

Presumably, the effects of ridesharing on accident rates may vary with city characteristics. We explore this next. We find that the accident increases are concentrated in large cities (high population), and more impoverished cities (as measured by per capita income). They are primarily concentrated in cities where the ex-ante use of public transportation is higher, consistent with substitution away from the alternative mode of public transportation. Moreover, the effects are concentrated in cities with high ex-ante levels of vehicle ownership, consistent with increasing usage of existing vehicles.

We then turn to examine the quantity mechanism suggested by our conceptual framework. We first document that at the intensive margin, VMT, measures of excess gas consumption, and annual hours spent in traffic go up following the entry of ridesharing. Furthermore, at the extensive margin, we find a 3% increase in new car registrations. Consistent with our estimates for fatal accident rates, this increase in new car registrations is more substantial in cities with high ex-ante use of public transportation, further strengthening the evidence for substitution away from public transport.

We note that the documented effects may be short-term, as pooling services such as LyftLine and UberPool increase ridership. Furthermore, as rideshare driver-partners become more experienced, both the VMT effects and the driver quality effects may be attenuated. In our sample through 2016, however, we observe no reversion of the effect; instead, the estimates appear to be increasing with time since rideshare launch, and the persistence is statistical significance. Still, many cities only saw the introduction of ridesharing services in the last three years, and pooling services are not available in all cities. It may be too soon to tell whether the effect we document is a short-term adjustment or a longer-term pattern; our initial evidence suggests that the effect is still present three years after the entrance of ridesharing.

An examination of ridesharing's effects on accident rates is particularly useful in providing insights into changes in motor vehicle fatality trends. Prior to 2011, and for the preceding twenty plus years, motor vehicle accident fatalities, in total, per population, and per VMT, had been falling. The 2010s saw a halt to the decrease in fatal accidents and a reversal of the trend. If this reversal is partly related to the increased quantity of vehicle miles on the road due to the introduction of ridesharing, this may have implications for policy discussions around decreasing motor vehicle accident rates.

Our paper is not the first to attempt to examine the effects of ridesharing's introduction on traffic accidents. A number of recent papers have explored this issue, primarily through the lens of drunk driving and the potential for reduction in drunk driving as a result of the availability of ridesharing (Brazil and Kirk, 2016; Martin-Buck, 2016; Greenwood and Wattal, 2016; Dills and Muholland, 2018). These studies are primarily focused on measures of alcohol-related fatal accidents, fatalities, and DUIs. They typically use the introduction of UberX as their measure of treatment and find either a reduction or no significant change in drunk accidents or fatalities. In contrast, we do not place our focus solely on fatalities resulting from drunk driving or alcohol consumption. Rather, in this study, we focus on the totality of fatal accidents, using a broad sample, and we account for the introduction of both Uber and Lyft, including the different types of Uber and Lyft service types. When we do not account for location-specific trends in our sample, we too observe a negative coefficient for alcohol-related accident measures. However, the inclusion of the location-specific trend aligns our results for these measures with those we obtain for all other accident measures: an increase in overall accidents and fatalities, for vehicle occupants and pedestrians. While ridesharing indeed may be displacing some drunk drivers, our findings suggest that overall accident rates and fatalities increase in the wake of rideshare introduction, despite the possible benefits from limiting impaired driving.

Our study contributes to several literatures. First, our paper contributes to a growing literature exploring the ridesharing industry and its workers. Hall and Krueger (2018) use survey and administrative data and find that drivers who partner with Uber appear to be attracted to the platform primarily because of the flexibility it offers, the level of compensation, and the fact that earnings per hour do not differ much with the number of hours worked. In related work, Chen et al. (2018) estimate how drivers reservation wages relate to the flexibility of rideshare work arrangements. They find that while the Uber relationship may have other drawbacks, Uber drivers benefit significantly from real-time flexibility, earning more than twice the surplus they would in less flexible arrangements. Cook et al. (2018) examine the gender earnings gap between male and female Uber drivers and show that it can be entirely attributed to three factors: experience on the platform, preferences over where to work, and preferences for driving speed. Liu et al. (2018) compare Uber drivers to taxi drivers and find that the Uber platform reduces moral hazard in the form of fewer detours by drivers on Manhattan to airport routes, except during times of surge pricing. Relatedly, Cramer and Krueger (2016) point to the higher level of efficiency of Uber's matching algorithm between drivers and riders and the resulting lower transaction costs.

Other work in this category has focused on ridesharing's effect on other modes of transportation, finding mixed evidence. Nie (2017) finds Uber has reduced taxi ridership, while Cramer (2016) finds that the wages of taxi drivers and chauffeurs have not decreased. Finally, using Uber's individual-level data and its unique use of surge pricing, Cohen et al. (2016) estimate that UberX created $6.8 billion of consumer surplus in 2015.

Our paper also relates to a larger literature that explores technology diffusion and the struggle between such diffusion and the interest and resistance of entrenched incumbents (Parente and Prescott, 1994). In many ways, ridesharing has become the modern poster-child for the classic battle between what is argued to be outdated regulatory environments and rent-seeking incumbents, and the adoption of welfare-enhancing technology. Many new technologies face frictions that slow their diffusion (Grubler, 1991). Parente and Prescott (1994) argue that one such friction is resistance on the part of sectoral interests. Indeed, emphasizing barriers to technology adoption, economic historians such as Rosenberg and Birdzell (1986) argue that the reason why the West grew rich first was that active resistance to technology adoption was weaker there. Most economic histories of technological adoptions provide cases in which the adoption of technologies was met with fierce resistance (Mokyr 1990).

Our findings may be cause to reframe the discussion around city response to the rapid growth of ridesharing. While much of the resistance to ridesharing has been presented as a case of entrenched incumbents (taxis) seeking rents, our findings suggest that more considerable societal costs are also at play. In ridesharing's case, delays in the diffusion of new technology may be optimal, if we consider offsetting costs such as increased accident rates or pollution or the need for learning-by-doing on the part of users. Introduction of new technology can have unintended effects: it may impose externalities not priced into the cost for the individual user. Overall, whether ridesharing is welfare-enhancing or decreasing depends on the value of the increase in convenience and other consumer surplus effects versus the offsetting costs in time, material and human life of increased accidents and traffic-related fatalities. Studies have suggested that ridesharing may create considerable customer surplus (Cohen et al., 2016), as well as provide job opportunities for groups facing unusually high unemployment risk (Landier et al., 2016).

The paper proceeds as follows. Section 2 provides a brief overview of ridesharing services and lays out our conceptual framework. Section 3 describes our sample and data sources. Section 4 presents our main empirical results on accidents and fatalities. Section 5 explores the quantity mechanism described in our conceptual framework. Section 6 presents an estimate of costs and discusses welfare considerations. Section 7 concludes.

2. Ridesharing & Conceptual Framework

Before the advent of ridesharing services, the primary forms of private for-hire transportation were limited to traditional taxis, limousines, and larger vehicles such as bus and van services. Of these, only traditional taxis did not need to be reserved in advance, and all came at fairly substantial costs, and the number of cars available varied widely from city to city. Most municipalities heavily regulate the traditional taxi industry, placing restrictions on the number of vehicles that can operate, the prices they can charge, and the licensing and insurance requirements for the drivers and cars. Quantity restrictions, in particular, where thought to lead to shortages of taxis during periods of high demand and an inconvenience to riders.

Uber was the first ridesharing firm in the U.S., launching in San Francisco in May 2010, and was followed two years later by Lyft and Sidecar. Ridesharing then expanded rapidly across the U.S. By the end of 2014, ridesharing firms operated in 80% of U.S. cities with a population of 100,000 or more. Much of the spread in ridesharing was driven by the convenience for users, stemming from new technology making it easier for riders to match with drivers and both quickly hail a ride and seamlessly pay through the app. Ridesharing firms' exemptions from (or willful disregard for) taxi and livery restrictions allowed them to expand supply during periods of high demand and adjust prices to encourage more riders and/or drivers to participate in the market.[5]

To better understand the expected effects of ridesharing on accident rates, we develop a simple conceptual model in which accident rates are a function of two elements: the number of vehicle miles traveled (VMT) on roads and the average quality of drivers. For notational purposes, we denote the accident rate for city i in period t as $A_{i,t}$ and the new technology (ridesharing) as θ. Accident rates can then be thought of as:

[5] Many major ridesharing companies adjust pricing in real time to better match supply and demand, charging higher "Surge Pricing" fares during periods with high demand relative to supply.

$$A_{i,t} = f(VMT_i(\theta);\ Q_{i,t}(\theta)),$$

where $VMT_i(\theta)$ is the number of vehicle miles traveled on the road in city i in period t (potentially a function of whether ridesharing is available or not) and $Q_{i,t}(\theta)$ is the quality of the average driver on the road in city i in period t.

The number of VMTs can further be broken down into three sub-categories: (i) the number of VMTs generated by people driving themselves from origin to destination (which we denote by VMT^{own}; (ii) the number of VMTs generated by rideshare driver-partners driving passengers from origin to destination, denoted by VMT^{RS}; and (iii) the number of VMTs generated by rideshare driver-partners while driving in-between rideshare passengers, denoted by VMT^{btwnRS}. Thus,

$$VMT_i = VMT^{own} + VMT^{RS} + VMT^{btwnRS}.$$

Note that even if VMT^{own} and VMT^{RS} simply offset as people move from driving themselves to their destination to being driven in a rideshare vehicle, there is still "between driving" (between fares, waiting for fares, going from fare location to fare location) that is introduced by the advent of ridesharing in a city. While VMT^{own} is almost certainly decreased by the introduction of ridesharing in a city, the ridesharing technology leads to the introduction of additional vehicle miles in the form of VMT^{RS} and VMT^{btwnRS}. Thus, the effect of the introduction of ridesharing in a city on the number of VMTs on the road depends on whether the decrease in VMT^{own} is more than offset by VMT^{RS} and VMT^{btwnRS} that are introduced with the technology. Taking the model naively (and ignoring for the moment the UberPool and LyftLine services), each person who no longer chooses to drive themselves is now driven by a rideshare driver, thus precisely offsetting the effect on the overall vehicle miles traveled. But unless there is absolutely no between-fare miles driven by a ride-sharing driver, we would expect to see an increase in the overall number of VMTs after ridesharing arrives. The limited evidence to date suggests that there is considerable between-fare travel by drivers. Henao (2016) reports statistics suggesting ridesharing drivers only have passengers in the car 39% of the time and 59% of the miles they drive while active on the app. Schaller (2018), using detailed data from New York City, shows that rideshare drivers on average drive 2.8 miles while waiting for a fare, 0.7 miles to pick up the fare, and 5.1 miles with a passenger in the car, implying a 59% utilization rate. Both Lyft and Uber offer subsidies designed to induce drivers to spend more time out on the road active in the app, so as to decrease wait time for passengers. Finally, while not the focus of their study, the analysis in Chen et al. (2018) is consistent with a mismatch between rider demand and the supply of drivers.

More formally, we write the first order condition for the effects on accident rate A_i from the introduction of ridesharing technology θ as:

$$\frac{\partial A_i}{\partial \theta} = \frac{\partial A_i}{\partial VMT_i}\frac{\partial VMT_i}{\partial \theta} + \frac{\partial A_i}{\partial Q_i}\frac{\partial Q_i}{\partial \theta}$$

where

$$\frac{\partial VMT_i}{\partial \theta} = \frac{\partial {VMT^{own}}_i}{\partial \theta} + \frac{\partial {VMT^{RS}}_i}{\partial \theta} + \frac{\partial {VMT^{btwnRS}}_i}{\partial \theta}.$$

Clearly, $\frac{\partial A_i}{\partial VMT_i}$ is positive, as every additional vehicle mile travelled will increase the likelihood of an accident, and thus, the overall accident rate. $\frac{\partial {VMT^{own}}_i}{\partial \theta}$ is negative. $\frac{\partial {VMT^{RS}}_i}{\partial \theta}$, however, will either equal or (more likely, due to substitution away from other forms of transport) larger in magnitude than $\frac{\partial {VMT^{own}}_i}{\partial \theta}$, and $\frac{\partial {VMT^{btwnRS}}_i}{\partial \theta}$ is positive. Thus, the overall effect $\frac{\partial VMT_i}{\partial \theta}$ is positive: vehicle miles traveled are increasing in the introduction of rideshare technology.

Of course, in some cities, at later dates, the option to "carpool" in a rideshare was introduced into the mix, in the form of Uber Pool and Lyft Line. With the introduction of these services, the reduction in own drive car hours may not be fully offset by rideshare drive hours, as multiple people may be substituting away from driving themselves into a single rideshare car. While Uber and Lyft have both heavily invested in promoting their shared services, Uber reports that UberPool accounts for only 20% of trips in cities where it is offered, and Lyft reports that 37% of users in cities with LyftLine request a Line trip, and many trips are not matched, thus leaving a single rider (Schaller, 2018). Pooled rides are also cheaper, potentially inducing more substitution from other modes of transport. It is not clear what fraction of rides must be pooled to counteract VMT^{btwnRS}, but Schaller (2018) suggests that even if half of rides were pooled, total VMT would still increase.

Furthermore, stepping away from the naïve model, survey evidence suggests that $\frac{VMT^{RS}}{VMT^{own}} > 1$, as many riders are substituting away not from driving themselves, but rather from other forms of transportation, including walking, biking, and more importantly, public transportation (Clewlow and Mishra, 2017).

Assessing the effects of the introduction of ridesharing technology on the quality of the average driver on the road is less straightforward. On the one hand, the people substituting into a rideshare ride rather than driving themselves may be low quality drivers (impaired or inebriated, not skilled at driving and prefer not to), but they may be high quality drivers who simply dislike driving. On the other hand, there is no guarantee that the driver that substitutes them is of higher quality. Put another way, the introduction of ridesharing makes it less costly to have someone else drive you from place to place, but also makes the gains from getting out on the road as a driver greater (as you can make money by doing so). Lower quality drivers who in the absence of compensation may not have driven now have an incentive to drive. More affluent people are more likely to use ridesharing, and the less affluent are more likely to become rideshare drivers. To the extent that this substitution leads to more vehicle miles driven by lower quality drivers or in lower quality cars, this may positively affect accident rates. Yet, rideshare drivers, especially with more experience from more hours driven, may in fact be of improved quality. To the extent that the substitution goes the other way, and lower quality drivers are substituted by better drivers, this may lead to a reduction in accident rates if the increase in quality offsets the increase in VMT.

Formally, $\frac{\partial A_i}{\partial Q_i}$ is negative: better quality drivers should lead to a reduction in accident rates, all else equal. The effect of rideshare technology on the quality of the average driver on the road, $\frac{\partial Q_i}{\partial \theta}$, however, is ambiguous. If the quality of the average driver increases, it could offset the quantity effect above. If it decreases or does not change, the quantity effect will prevail. Which effect dominates, of course, is an empirical question.

Many indicators suggest that both total VMT and driver quality may adjust over time. Cook et al. (2018) note that even in the relatively simple production of a passenger's ride, experience is valuable for drivers. A driver with more than 2,500-lifetime trips completed earns 14% more per hour than a driver who has completed fewer than 100-lifetime trips, in part because he learns where and when to drive, which may decrease VMT^{btwnRS}. Similarly, Haggag et al. (2017) show that learning-by-doing and experience are important for New York City taxi drivers. At the same time, not all learning-by-doing is necessarily good for accident rates. For example, learning by doing to maximize earnings could lead to behavior, on the part of certain driver populations, that increases the probability of accidents as driving faster is associated with higher earnings.

3. Data and Sample

Our sample consists of all incorporated "places" in the U.S.[6] with population greater than or equal to ten thousand in 2010,[7] and which experienced at least one motor vehicle accident that results in a fatality ("fatal accident") during the period 2001 to 2016. Our list of incorporated places is obtained from the Census Bureau, and covers all self-governing cities, boroughs, towns and villages in the U.S.[8] (for ease of interpretation, we interchangeably refer to these as "cities" or "locations" throughout the following text). Our observations are measured at the quarterly level. The sample thus contains 189,120 quarterly observations on 2,955 "places" from 2001 to 2016, among which 1,185 adopt ridesharing prior to 2017. Figure 3 shows the diffusion of ridesharing across the U.S., by cities/places and by population. Diffusion of ridesharing across U.S. cities/places began slowly, accelerating rapidly after 2013. Diffusion by population follows a standard S-curve, consistent with general historical patterns of new technology diffusion.

3.1. Fatal Accidents

We obtain data on fatal accidents from the National Highway Traffic Safety Administration (NHTSA) Fatal Accident Reporting System (FARS). To qualify as a FARS case, crash has to involve a motor vehicle traveling on a traffic way customarily open to the public, and must have resulted in the death of a motorist or a non-motorist within 30 days of the crash. Importantly, the data identify whether any drivers involved are under the influence of alcohol. We aggregate the incident-level FARS data into quarterly totals for each place/city. When the data contain geographic coordinates, we use Google Map's Geocoding API service to determine the corresponding place/city. When the coordinates are not available, we use the city and state

[6] We use incorporated places rather than Census Designated Places (CDPs) because CDP annual population estimates are not readily available except by individual place download, whereas population data is available for incorporated places for mass download through Census.
[7] Some places in our sample had lower populations than 10K during the sample period, most notably during the period 2001-2010. We impose the cutoff on population as measured in 2010. As an example, consider Hutto, TX, a suburb of the Austin-RoundRock metro area. In 2001, Hutto had a population of 3,030, the lowest population observation in our sample. By 2010, it had grown to over 14,000 in population, mimicking the growth of the Austin metro area. As it has population above 10,000 in 2010, it is included in our sample.
[8] https://www.census.gov/content/dam/Census/data/developers/understandingplace.pdf

identifier codes to assign observations to the appropriate place. Geographic coordinates are present in 98% of FARS' observations, and we successfully match more than 99% to a city in our sample.

We construct a number of measures of accident volume from the FARS data. *Total Accidents* is the total number of fatal accidents according to the definition used by NHTSA. *Total Fatalities* is the total number of fatalities across all fatal accidents. *Total Drunk Accidents* is the total number of fatal accidents involving any drunk drivers. *Total Drunk Fatalities* is the total number of fatalities in all drunk accidents. *Total Non-Drunk Accidents* is the total number of fatal accidents not involving any drunk drivers. *Total Non-Drunk Fatalities* is the total number of fatalities in all non-drunk accidents. We measure accident "rates" as the number of accidents per 10,000 people or the number of accidents per billion city VMT.

We further classify our various categories of accidents based on their time of occurrence: (i) Weekday: Monday through Thursday; (ii) Weekend: Friday through Sunday; (iii) Night: After 5pm and before 2am; (iv) Friday and Saturday night: After 5pm and before 6am on Friday and Saturday.

We additionally separate out accidents involving pedestrians, and calculate three measures of pedestrian-involved accidents. *Pedestrian-Involved Accidents* is the number of fatal accidents involving at least one pedestrian. *Pedestrian-Involved Fatalities* is the total number of fatalities in all accidents involving at least one pedestrian. Finally, *Pedestrians Involved in Fatal Accidents* is the total number of pedestrians involved in fatal accidents.

For all our accident measures, we use log search volume in our intensity specifications, and so we interpret our coefficients in terms of percentage change in search volume.

3.2. Ridesharing Launch and Adoption Intensity

Data on ridesharing launch dates for each city are obtained directly from Uber and Lyft.[9] The companies provided dates of service launch for each type of service launched: (i) UberBlack/UberTaxi, which allows customers to hail a livery or taxi vehicle; (ii) UberX/Lyft,

[9] In this version we use the exact cities indicated by Uber and Lyft, even if we suspect or believe that the launch covered adjacent cities as well (e.g. San Francisco launched in 2010, and there is no separate launch date for San Jose or Palo Alto). Since this means some places we include in our control may in fact be treated in later years in the sample as service expands slowly out beyond original boundaries, we are biasing against finding an effect of treatment.

which allow customers to hail regular cars driven by driver-partners; and (iii) UberPool/Lyft Line, which allow customers to share a hailed vehicle with others riding in the same general direction. We merge these dates with Census Bureau's incorporated place directory in 2010.

While Uber and Lyft declined to provide data on driver enrollment and usage for this project, other researchers have shown a strong correlation between google trends for searches for rideshare keywords and actual driver uptake (Cramer and Krueger, 2016). To measure of the intensity of rideshare adoption, we thus follow the spirit of Cramer and Krueger (2016) and Hall et al. (2018) and utilize Google search volume for the terms "Uber," "Lyft," and "Rideshare."[10] We track trends for these terms using the Google Health Trends API for all Nielsen Designated Market Areas (DMAs) at monthly frequency from January 2004 to December 2016. We aggregate the data to the quarter level, and match the DMAs to census incorporated places using a crosswalk provided to us by Nielsen.

3.3. Other Data

We use a number of measures to explore heterogeneity by city characteristics and as control variables in our models. We obtain annual city population estimates and population density from the U.S. Census, and annual county income per capita from the Bureau of Economic Analysis. Household vehicle ownership and means of transportation to work at the city level are gathered from the 2010 American Communities Survey.

To explore mechanisms that may drive any change in accident rates upon arrival of ridesharing, we utilize a variety of data sources. We obtain data on new car registrations by zip code on a monthly level from Polk Automotive. We aggregate the data at city and quarter level using UDS Mapper's zip code-to-ZCTA crosswalk[11] and Census' ZCTA-to-place crosswalk. We obtain estimates of city and freeway vehicle miles traveled, total annual excess fuel consumption, and total annual hours of traffic delay for a sample of 101 urban areas from the Texas A&M Transportation Institute Urban Mobility Scorecard, covering the period 1982-2014. Of the 101 urban areas covered by TAMU in their report, 99 fall into our sample of continental

[10] We use the freebase identifiers for term "Uber" (/m/0gx0wlr) and "Lyft" (/m/0wdpqnj). Freebase identifiers denote all searches that were classified to be about this topic.
[11] The crosswalk can be found at https://www.udsmapper.org/zcta-crosswalk.cfm. The crosswalk is recommended by Missouri Census Data Center http://mcdc.missouri.edu/geography/zipcodes_2010supplement.shtml.

U.S. cities. For a set of tests regarding road utilization and driver quality, we use census' urban area-to-place crosswalk to aggregate our main sample at urban area and annual level in order to merge them with TAMU's dataset.

3.4. Summary Statistics

Table 1 presents summary statistics for the places in our sample over the sample period. Places in our sample average 54.65 thousand in population, have an income per capita of $39,710, and population density of roughly 3000 people per square mile. Prior to the arrival of ridesharing, 2.97% of residents in our average city/place used public transportation to commute, 10.6% commuted by carpool, and 33% owned vehicles. The average city in our sample had 672 new car registrations per year. As can be seen from the distributional statistics in the table, there is wide variation across all these characteristics across the sample.

Table 2 presents summary statistics on number and rate (per 100K population) of accidents for the cities in our sample over the sample period. Panel A presents accident and fatality levels, while Panel B presents the same measures scaled to be per 100K population. We present statistics for total accidents and fatalities, total drunk and non-drunk accidents and fatalities, and total pedestrian accidents and fatalities. Drunk accidents and fatalities represent approximately 1/3 of the total accidents and fatalities. Pedestrian accidents and fatalities are approximately 20% of the total. Precise numbers can be seen in the table.

4. Empirical Analysis

To assess the impact of ridesharing arrival on fatal accident rates, we employ a standard generalized difference-in-differences approach. We index cities by c and time by t. We estimate models of the form:

$$\log(1 + accidents_{t,c}) = \propto_c + \gamma_t + \beta' X_{t,c} + \theta_c t + \delta POST_t * TREATED_c + \varepsilon_{t,c},$$

where $accidents_{t,c}$ is our measure of accidents in city c in quarter t, $\propto_c$ is a city fixed effect, γ_t is quarter-year fixed effect, $X_{t,c}$ is a vector of time-varying, city specific control variables, and $\theta_c t$ is a city-specific linear time trend. We use robust standard errors, clustered at the city level. Our observations are at the quarterly level, and cover 2000Q1 through 2016Q4. Our control variables include the log of city population, and county income per capita.

The inclusion of a location-specific linear time trend is motivated by descriptive evidence on the relation between accident trends and ridesharing entry. We document that ridesharing launched first in cities that had been experiencing steeper declines in (drunk) accident rates. Figure 4 shows drunk accidents per 100K population for early-adopter cities (2010-2011), mid-adopters (2012-2014) and late adopter cities (2015-2016) in the five years preceding ridesharing entry. As can be seen from the figure, drunk accident rates had been falling in the five years preceding entry in early adopter cities, in contrast, they were stable (and much higher) in mid-to-late adopter cities.

These patterns are further confirmed in an unreported multinomial logit estimation. In the Appendix, we estimate a multinomial logit model where the outcome variable is defined as 0 if the city adopted ridesharing in 2010 or 2011, 1 if the city adopted ridesharing in 2012 through 2014 (the base outcome), and 2 if the city adopted ridesharing after 2015. We control for the natural logarithm of population and per capita income. Relative to the base outcome group, the estimates suggest that cities were more likely to be in the early launch group if they had been experiencing strong *declines* in drunk accident rates (negative drunk accident rate trends) in the five years prior, and were more likely to receive ridesharing services later if they were experiencing drunk accident rate *increases*. A dynamic hazard rate estimation provides qualitatively similar results.

In the absence of accounting for these location-specific trends, a difference-in-differences model can erroneously estimate a negative effect on accidents; this estimate, however, will be driven by order of entry and the pre-existing trends, rather than an actual drop in drunk accidents. Thus, while it is unlikely that Uber and Lyft were specifically selecting cities to roll out services based on trends in fatal accident rates, what they *were* selecting on (which may have been population, density, income, or some other variable) appears to be systematically correlated with trends in (drunk) accident rates. As a result, we focus our discussion on models that include location-specific trends to get as close as possible to a quasi-experimental setting.[12]

[12]We stress that the staggered rollout of ridesharing across U.S. cities does not represent an ideal experiment or quasi-experimental setting, as we have no random or exogenous assignment. We rely on a tight fixed effect structure and the staggered nature of the adoption to make inferences.

4.1. Main Results

We use a number of measures for $accidents_{t,c}$. In Panel A of Table 3, we employ six measures of total fatal accidents. Columns (1) and (2) utilize total accidents, columns (3) and (4) utilize total fatalities, columns (5) and (6) utilize drunk accidents, columns (7) and (8) utilize drunk fatalities, and columns (9) and (10) and (11) and (12) utilize non-drunk accidents and fatalities, respectively. The first column of each pair reports estimates without the inclusion of the city-specific linear time trend, while the second column of each pair includes the trend. For brevity, we report only the coefficient on the variable of interest— $POST_t * TREATED_c$ in the table. Here, we report OLS specifications, but our results remain robust to the use of count models instead.

For total accidents, total fatalities, total non-drunk accidents and total non-drunk fatalities, we observe a consistent positive and significant coefficient on the $POST_t * TREATED_c$ variable. Before accounting for the location specific time trend, the effect ranges in magnitude from an increase of 1.31% in total fatalities (column (3)) to 3.36% increase in non-drunk fatal accidents (column (9)). For the measures of drunk accidents and drunk fatalities, the coefficients are negative. However, as demonstrated by Figure 1, (drunk) fatal accident rates had been falling dramatically for over a decade prior to ridesharing launching, and, more importantly, had been falling faster for cities in which ridesharing launched earlier. It is therefore important to account for location-specific time trends when estimating these models. In the second column of each pair, we do just that. Once we include the location-specific time trend, we observe a positive and significant coefficient on $POST_t * TREATED_c$ for all twelve specifications. The magnitudes of the increase range from 2% to 3.5%, depending on the measure of accident used, with the smallest magnitude increases (~2%) from drunk accidents and drunk fatalities. Figure 5, Panels A, B and C graphically present the difference-in-differences estimators (with each dot representing 2 quarter-coefficients) for the eight quarters preceding and following rideshare adoption for total accidents and total fatalities, and drunk fatalities. In all three panels the counter-factual treatment effects in the pre-ridesharing periods are statistically indistinguishable from zero, providing support for our inferences (parallel trends in the pre-period).[13] Similar patterns are present for our other outcome measures.

In Panels B and C of Table 3, we break out weekend accidents, nighttime accidents, weekday accidents and weekend night accidents for total accidents (Panel B) and total fatalities (Panel C). We observe similar patterns to those exhibited in the models in Panel A. Accident and fatality increases are lowest on weekend nights (Friday and Saturday, after 5pm and before 6am) at 2.43% and 2.62% respectively. For total weekend and nighttime accidents and fatalities, the magnitudes of the estimated increases are between 3 and 4%.

We examine the persistence of the documented ridesharing effect by breaking the post ridesharing variables into quarters past. Doing this allows us to examine the dynamics of the effect up to two years after the introduction of ridesharing in the cities. Table 4 reports the estimates of the dynamics of ridesharing. It is clear from the table that ridesharing's increase in accidents and fatalities persists over time, and in fact appears to be increasing after 6 quarters of being introduced in the city, consistent with a time gap between launch of services and widespread adoption in a location.

4.2. Variation in Services

In Table 5 Panel A, we separate out the treatment effect of the different types of services: those that are single rides (UberBlack/Taxi/X, Lyft) versus pooled rides (UberPool, LyftLine. We pool UberBlack/Taxi with UberX due to the very small number of cities that have (had) UberBlack/Taxi service. We thus report the treatment effect for pooled versus non-pooled service. The estimates in the table suggest that the rollout of pooled ride services does not reverse the overall treatment effect of non-pool rideshare. The coefficients for pool launch are roughly half the magnitude of those for single ride (non-pool) rideshare launch, but negative, and are not statistically significant

[13] As an additional (closely related) way to assess the validity of the parallel trends assumption, we plot univariate trends separately for the treatment and control groups in the pre-ridesharing period (unreported, available upon request). A visual inspection provides no indication of differential trends between the groups for any of the four primary outcome variables, which provides further reassurance that the parallel trends assumption is valid in our analyses.

at conventional levels. This may be consistent with relatively low adoption rates for pooled rides, even in cities that often the service.

In Panel B of Table 5, we explore the intensity of service adoption. In the main models we just discussed, we employ the first launch of a ridesharing service, irrespective of type of service, as our treatment date. Take up on these services, however, is likely to intensify over time. To explore this issue, we now interact our $TREATMENT$ indicator with the intensity of Google searches measure, and re-estimate our models. Table 5 Panel A presents the results of this estimation where $accidents$ is measures as total accidents in column (1), total fatalities in column (2), total drunk accidents in column (3), total drunk fatalities in column (4) and total non-drunk accidents and non-drunk fatalities in columns (5) and (6), respectively. For brevity, we display only the estimates from models including the location-specific trends. The estimates are consistent with an increase in accidents following an increase in our Google Trends intensity measure. For all six models, the coefficient estimate on $POST * INTENSITY$ is positive and statistically significant. Thus, as our proxy for adoption intensity (Google trends search intensity) increases, so do fatal accidents.

4.3. Pedestrians versus Vehicle Occupants

An important question is whether the increase in accidents and fatalities suggested by the estimates in Table 3 are concentrated in vehicle occupants, versus the alternative of potentially imposing an externality on pedestrians (non-vehicle occupants). The increase in accidents could primarily affect vehicle occupants, or it could additionally affect bystanders. The FARS data allows us to separate out accidents in which pedestrians were involved. We code an accident as pedestrian-involved if the FARS database indicates it involves persons that are not motor vehicle occupants or riders (motorcycle).[14]

In Table 6, we present the estimates from models similar to those in Table 3, substituting our measures of total accidents with similar measures that solely count accidents in which a pedestrian was involved. Our $accidents$ measure in columns (1) and (2) is the total number of accidents in which a pedestrian was involved, in columns (3) and (4) it is the total number of fatalities in accidents that involved a pedestrian, and in columns (5) and (6) it is the number of pedestrians involved in fatal accidents. The estimates from these models follow the same pattern as the

[14] FARS defines a pedestrian as “Any Person Not In Or Upon A Motor Vehicle Or Other Vehicle.”

estimates in our main models, suggesting that the increase in accidents following rideshare entry imposes an externality on non-vehicle occupants, not just on occupants of vehicles. The magnitudes of these increases mirror those in our main models, ranging from a 2.5% increase in total accidents involving a pedestrian and in fatalities in accidents involving a pedestrian, to an increase of 2.8% in the number of pedestrians that are involved in fatal accidents. The magnitudes of the coefficients are higher, in the range of 3.2%, if we do not account for the location-specific trends.

Figure 5 Panel D graphically presents the difference-in-differences estimators (with each dot representing 2 quarter-coefficients) for the eight quarters preceding and following rideshare adoption for pedestrian fatalities. As in our main models for total fatalities and accidents, the counter-factual treatment effects in the pre-ridesharing periods are statistically indistinguishable from zero, again providing support for our inferences (parallel trends in the pre-period).

4.4. Heterogeneity of effects

In Table 7, we break out our results across a variety of city characteristics: population, income, and population density, as well as by ex-ante vehicle ownership, ex ante public transport usage, and ex ante car pool usage, as reported by the American Community Survey. For each characteristic, we divide cities into quartiles, and re-estimate our models, interacting $POST * TREATMENT$ with the four quartile indicators for the city characteristic. For each city characteristic, we estimate four models, in which the $accidents$ measures are total accidents, total fatalities, total accidents involving a pedestrian, and total fatalities in accidents involving a pedestrian. As before, all models include location and year-quarter fixed effects, a location-specific linear time trend, and control variables.

Panel A presents the estimates for the models using quartiles of city characteristics. Column (1) presents the estimates where the city characteristic of interest is city population. For both measures of total accidents and fatalities and for measures of pedestrian accidents and fatalities, the estimates suggest that the increase in accidents observed in our main models is concentrated in large cities (Q4). The estimates for $POST * TREATED * Q4$ are significant and range from 6.5% to 7.5%; in contrast, the estimates for the bottom three quartiles of city population are an order of magnitude smaller and insignificant at conventional levels.

Column (2) repeats this exercise breaking cities into quartiles by income per capita. Here, we see limited heterogeneity across income per capita quartiles for total accidents and fatalities, whereas for pedestrian-involved accidents and fatalities, the effect appears to be concentrated in the top three quartiles of city income per capita.

In column (3), we break cities into quartiles by population density. Here, we observe no clear pattern; the only outliers are the estimates for the coefficients for the least dense cities in the models for pedestrian accidents and fatalities, which, unlike the rest of the coefficients, are insignificant and much smaller in magnitude.

Panel B turns to measures of ex ante vehicle ownership, public transport usage and car pool usage from the ACS. Some interesting patterns emerge. First, from column (1), we see that the increase in accidents following the launch of ridesharing services appear to be concentrated in cities in the top quartile of ex ante vehicle ownership. This is consistent with a lower cost of driving for those individuals that already had a car with which to drive for ridesharing. This is also consistent with many of the rideshare firms' arguments that ridesharing allows for better utilization of cars already present in the cities, inducing those cars to be on the road instead of sitting idle.

Second, in column (2), we see that the increase in accidents is concentrated in cities with higher ex ante usage of public transportation; the coefficients of interest are positive and significant for the top two quartiles of public transport use, are insignificant for Q2, and are even *negative* and significant at the 5% level for cities in the lowest quartile of public transport use when the dependent variable is calculated using pedestrian accidents or pedestrian accident fatalities. Finally, consistent with the estimates for the prior two columns, column (3) suggests that the increase in accidents post-ridesharing is concentrated in cities that ex ante had above-median carpool usage. These estimates would be consistent with a substitution effect: to ridesharing, away from public transport and away from carpooling.

5. Mechanisms: Quantity

Having established a robust pattern of estimates consistent with an increase in fatal accidents and fatalities following the launch of ridesharing services in a city, we turn now to consideration of one of the two mechanisms discussed in our conceptual framework: increases in quantity (road utilization in the form of VMT).

Road utilization data and congestion data for city roads are not readily available for most cities (in contrast to highway VMT, which are readily available from the department of transportation). To examine this channel, first, on the intensive margin, we utilize annual estimates of Arterial Vehicle Miles Traveled, Excess Gas Consumption and Hours Delay in Traffic for 99 urban areas reported by the TAMU Transportation Institute for the years 2000-2014.

In Table 8, we estimate similar models to our main specification, replacing the $accidents$ variable as our dependent variable with Arterial Street Daily VMT (column (1)), Annual Excess Fuel Consumption (column (2)), and Annual Hours of Delay (column (3)). Due to the limited availability of data relative to the full sample, the models in table 7 aggregates locations up to the urban area.[15] Moreover, we can estimate only for the years up to 2014, for these 99 urban areas, leaving us with 1,386 observations (as compared to 189,120 in our other models). Still, for all three models, we obtain a positive and significant estimate for the coefficient on our variable of interest, $POST * TREATMENT$, though with lower statistical significance levels (5%). The economic magnitudes vary from a roughly 3% increase in daily VMT to a 1.7% increase in excess fuel consumption and annual hours of delay.

Next, in Table 9 we examine the extensive margin in usage by estimating similar models to those on Table 8, but where the dependent variable is the logarithm of new car registrations as reported by Polk Automotive. Both Lyft and Uber often report numbers from surveys of users suggesting some of their riders forgo owning their own cars, and thus argue that they are removing vehicles from the road. These surveys, however, do not account for the possibility that at the same time as some of the *rider* population is forgoing owning a vehicle, others may be purchasing cars in order to work as a *driver* of the ridesharing platform. Which effect ultimately dominates is an empirical question. Panel A reports the estimates from models with and without the location-specific linear trend. The estimates suggest that the initiation of ridesharing leads to an increase in

[15] TAMU uses the Department of Transportation (DOT) urban area boundaries. DOT urban areas were adopted from Census urban areas but have slight adjustments for transportation purposes. See e.g. https://www.fhwa.dot.gov/planning/census_issues/archives/metropolitan_planning/faqa2cdt.cfm#q24 and https://www.fhwa.dot.gov/legsregs/directives/fapg/g406300.htm.

new car registrations, rather than an overall decrease. This increase is in the range of 5% when including the location-specific time trend.

In Panel B, we further the intuition of this extensive margin effect by examining how new car registrations respond to the interaction of ridesharing intensity as proxied by the Google search intensity variable used in Section 4.2. The estimates suggest that new car registrations increase in the intensity of Google searches for Uber/Lyft/Rideshare. This relationship intensifies upon the entry of ridesharing into a treated city. These results suggest that there is an increase in new vehicle purchases as ridesharing services become more intensely used.

Turning to Panel C of the table, the heterogeneity in this increase along city characteristics lines up with the heterogeneity in the increase in accidents documented in Section 4.4: the new car registrations are concentrated in cities with above median population and in cities with above median ex ante vehicle ownership. Moreover, the increase in new car registrations is larger in cities with high ex ante public transport usage and car pool usage. They are decreasing only in the cities with the *lowest* quartile of ex ante carpool usage. These results further reinforce the likelihood that ridesharing serves to substitute riders away from other non-car forms of transportation.

Interestingly, the estimates in Panel C of Table 9 suggest that the increase in new car registrations is higher in cities with high population density: the estimates imply a 9.6% increase in new registrations in the cities in the highest quartile of density, a 5.8% increase in cities in Q3 of density, a 2% increase for cities in Q2, and a statistically insignificant 2% *decrease* in cities in the lowest quartile of population density. Overall, this fact pattern is suggestive of increases in congestion driven by ridesharing.

6. Discussion and Welfare

Up until this point, our study has documented the cost associated with the introduction of ridesharing. In order to make a welfare calculation, we must consider not only the costs that ridesharing imposes but also its benefits. Benefits come from, for example, the consumer surplus gained by the convenience of ridesharing. Cohen et al. 2018 use Uber's "surge" pricing algorithm and the richness of its individual-level data to estimate demand elasticities at several points along the demand curve, and then use these elasticity estimates to estimate consumer surplus. They estimate that in 2015 the UberX service generated about $2.9 billion in consumer surplus in the four U.S. cities they examine. Moreover, their back-of-the-envelope calculations suggest that the overall consumer surplus generated by the UberX service in the United States in 2015 was $6.8 billion. Here, we use their measure of consumer surplus to examine potential welfare effects.

First, we quantify the cost of ridesharing's increase in fatal accidents using estimates of the value of a statistical life. Assuming ridesharing services are eventually made available across the entire U.S., we can do a back-of-the-envelope calculation of the costs of the increase in accidents we document. In 2010, the year before ridesharing began, there were 32,885 motor vehicle fatalities in the U.S.[16] The 3% annual increase associated with the introduction of ridesharing in fatalities represents an additional 987 lives lost each year.[17] The U.S. Department of Transportation estimates the Value of a Statistical Life (VSL) at $9.6 million for 2015; the DOT recommends analysts use a test range of $5.4 million (low) to $13.4 million (high) in 2015 dollars. Applying the VSL and assuming an annual increase of 987 lives lost per year, the annual cost of the increase in fatalities associated with ridesharing can be estimated as roughly $9.48 billion per year, with a range of $5.33 billion (low) to $13.24 billion (high).

A comparison of our cost estimate with Cohen et al. (2018)'s estimates of consumer surplus generated by ridesharing services suggests that the costs of the new technology from fatal accident increases match or surpass the benefits to direct consumers of ridesharing. Our estimates, moreover, do not include the costs imposed by non-fatal accidents, for which data is not readily available. We can assume that the pattern for fatal accidents is also repeated for non-fatal accidents, leading to costs in material and healthcare which may dwarf these VSL estimates. The incremental cost derives from the externalities associated with driving and traffic congestion where riders of ridesharing due not bear the full cost of being on the road—some of this cost is borne by pedestrians, as we document above. Overall, these welfare calculations suggest the need for more research on the overall impact of ridesharing technology in the economy.

7. Conclusion

Beginning in the mid -1980s the United States experienced a dramatic decrease in fatal accidents per capita and per vehicle mile driven. In 2010, 32,885 people died in motor vehicle

[16] https://crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/811552
[17] We round the estimated number of fatalities to the nearest whole number.

traffic crashes in the United States—the lowest number of fatalities since 1949 (NHTSA, 2012). This decline halted and then reversed shortly after the introduction of ridesharing into U.S. cities. In 2017, the NHTSA noted that:

> “There were 37,461 people killed in crashes on U.S. roadways during 2016, an increase from 35,485 in 2015.….Fatalities increased from 2015 to 2016 in almost all segments of the population—passenger vehicle occupants, occupants of large trucks, pedestrians, pedal cyclists, motorcyclists, alcohol-impaired driving, male/female, and daytime/nighttime….with the large increases in fatalities in 2015 and 2016, [the] decade-long downward trend of 21 percent has been reduced by more than one-third.”

In this paper, we provide evidence consistent with ridesharing imposing an increase in fatal accidents and fatalities on the motor vehicle occupants and pedestrians of the cities it serves. We document a roughly 2 to 4% increase in the number of fatal accidents: throughout the week, on weekends, at night, and on weekend nights. We develop a conceptual framework for thinking about how the introduction of ridesharing may affect accident rates, which model’s accidents as a function of vehicle miles traveled and average driver quality. We document increases in the intensive margin of quantity. For example, VMT, measures of excess gas consumption, and annual hours spent in traffic go up following the entry of ridesharing. Furthermore, at the extensive margin, we find a 3% increase in new car registrations. Consistent with our estimates for fatal accident rates, this increase in new car registrations is more substantial in cities with high ex-ante use of public transportation, further strengthening the evidence for substitution away from public transport.

While our documented effects alone are unlikely to fully explain the reversal of accident rate trends in recent years, it is a component worth more investigation and discussion. Moreover, while ridesharing appears to be associated with an increase in motor vehicle deaths, it is important to note that this cost comes with many benefits that accrue from the presence of ridesharing in a city. These include improved mobility for the disabled and for minorities, flexible job opportunities that are especially valuable to those otherwise at high risk of unemployment, and customer convenience and resulting consumer surplus. The annual cost in human lives is non-trivial, and it is higher than estimates for annual consumer surplus generated. Our estimates, moreover, do not include the costs imposed by non-fatal accidents, for which data is not readily available. We can assume that the pattern for fatal accidents is also repeated for non-fatal accidents, leading to costs in material and healthcare which may dwarf the costs in human lives. An essential contribution of our study is to point to the need for further research and debate about the overall cost-benefit tradeoff of ridesharing services and further mechanisms to increase the benefits or reduce the costs.

Finally, given the relatively short period in which ridesharing has been in effect, our results are short-term in nature. The long-term consequences of ridesharing may differ, as individuals may change behavior in the long term. For example, drivers may learn the realized price for their driving, causing some to exit the market. Moreover, those that stay may gain knowledge over time and improve their driving quality with the platform. Additionally, as competition increases in the market, the massive subsidies provided by ridesharing companies for drivers and riders may decline, reducing the number of riders. If usage of pooled ride services increase, car utilization may rise, lowering the number of vehicle miles traveled overall. Thus, any regulatory actions must consider the documented short-term effects and advance further research on the outcomes of ridesharing.

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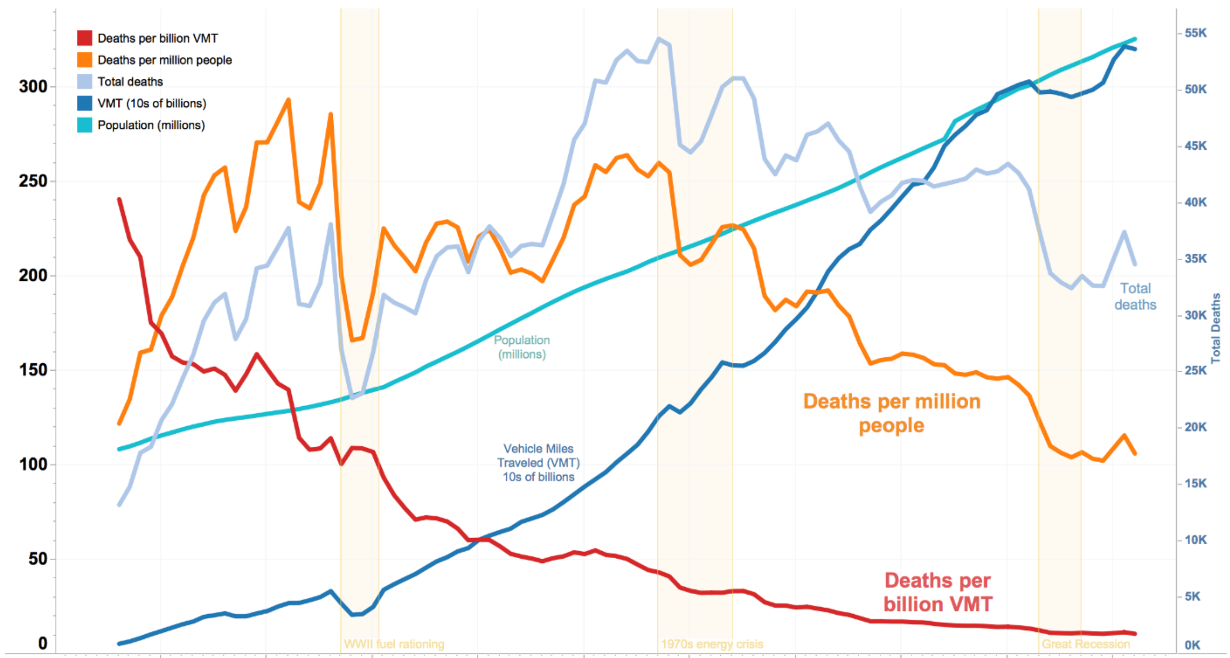


Figure 1: U.S. Motor Vehicle Death per VMT, Death per Capita, Total Death, VMT and Population
This figure was produced by Dennis Bratland and is reproduced here under creative commons license. The figure uses NHTSA FARS and CrashStats data to depict total U.S. motor vehicle deaths, deaths per VMT, deaths per capita, VMT and population for the period 1920-2017.

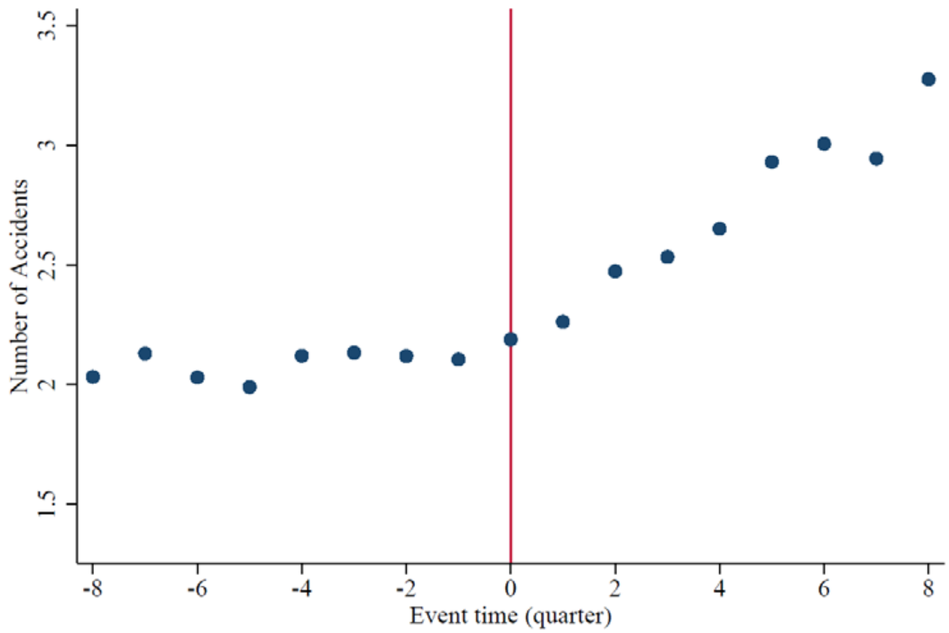


Figure 2: Accidents for Treated Cities in Event Time

This figure shows the trend of accidents for treated cities in the eight quarters preceding and after ridesharing entry. The red vertical line at event time zero indicates the quarter of ridesharing entry.

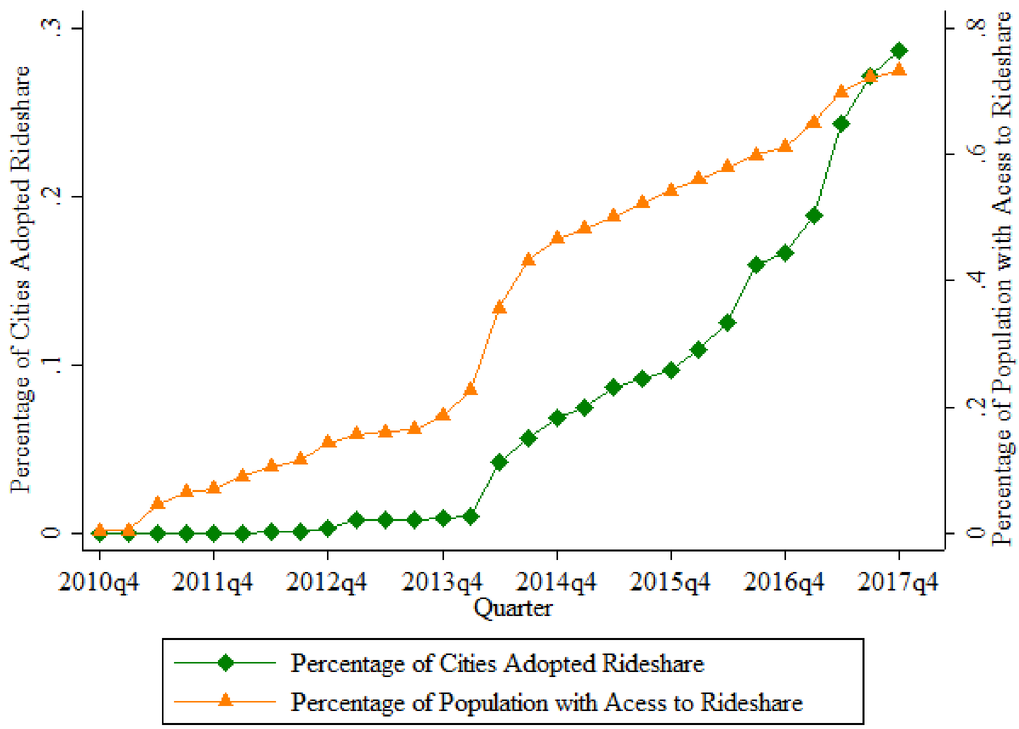


Figure 3 Rideshare Diffusion

This figure shows the diffusion of ridesharing across the U.S. by cities and by population. The sample consists of all census incorporated places in the United States. The green (orange) line graphs the percentage of cities (population) that adopted ridesharing in each quarter between the fourth quarter of 2010 and fourth quarter of 2017.

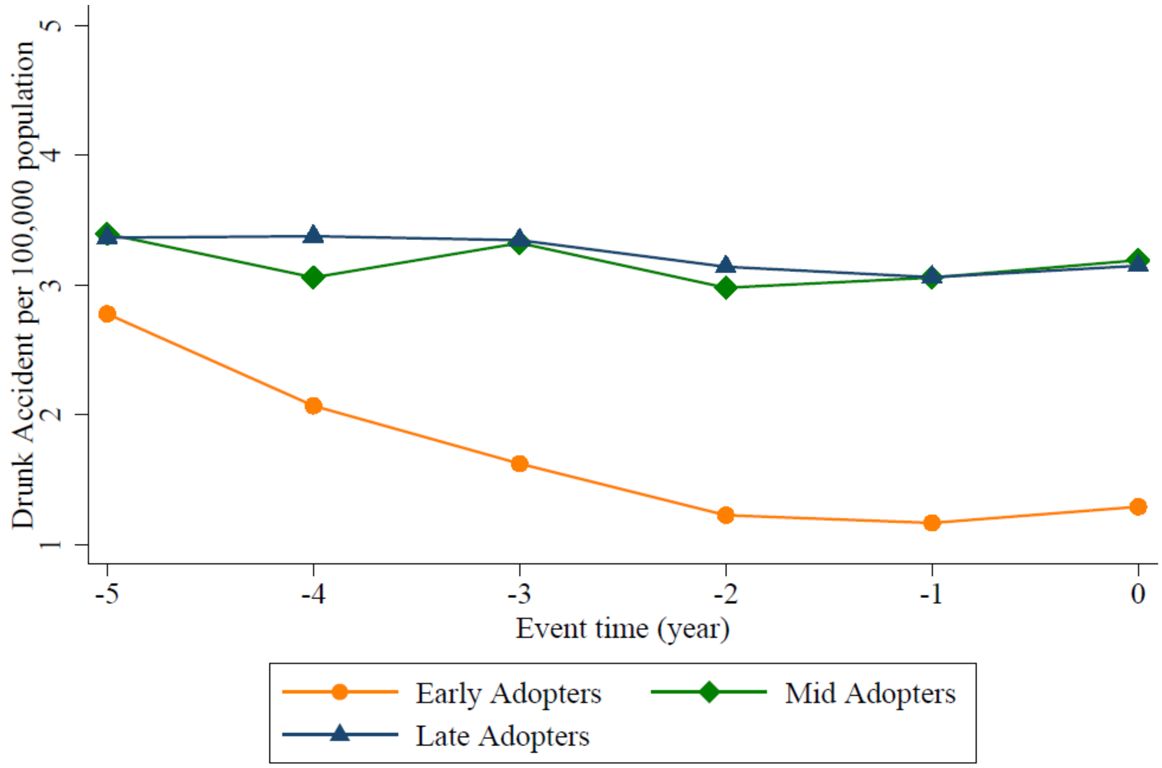
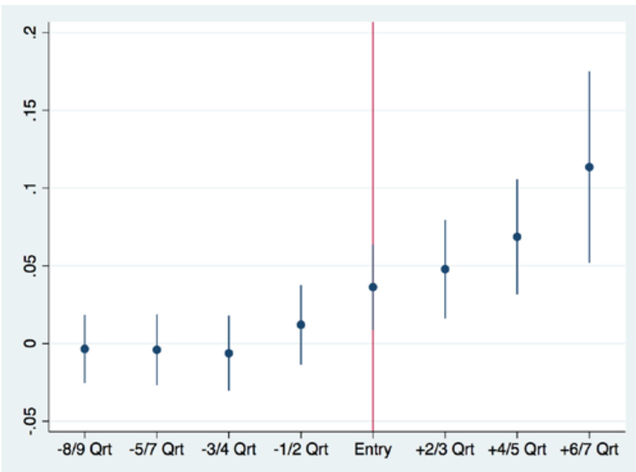
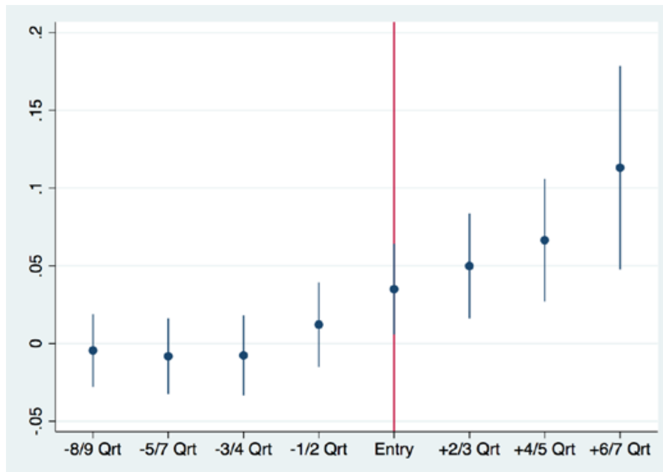


Figure 4 Drunk Accident Rate for Treated Cities Before Rideshare Adoption
This figure shows the trend of drunk accidents per 100K population in the five years preceding ridesharing entry. Early-adopter cities are cities that adopted ridesharing in 2010 or 2011, mid-adopters are cities that adopted ridesharing in 2012-2014, and late-adopter cities are cities that adopted ridesharing in 2015-2016.

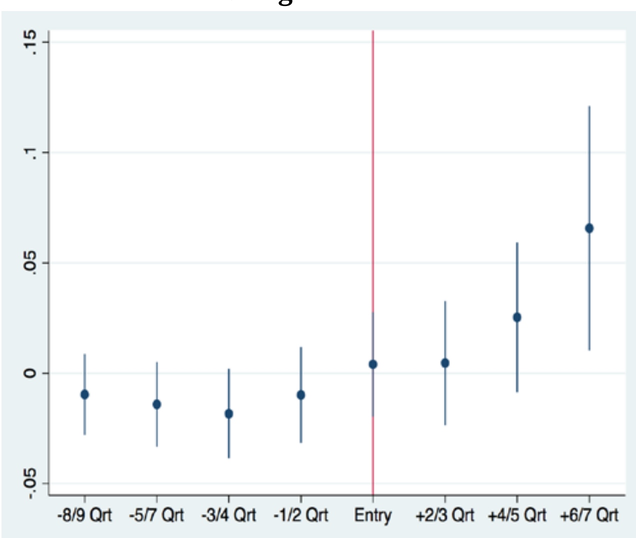
Panel A: Log Total Accidents



Panel B: Log Total Fatalities



Panel C: Log Drunk Fatalities



Panel D: Log Pedestrian-Involved Accidents

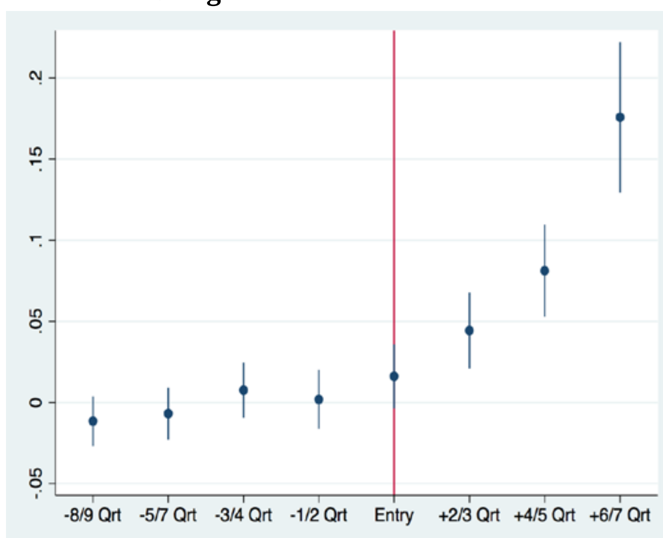


Figure 5 Difference-in-Differences Estimators

This figure displays the regression coefficient estimates and two-tailed 90% confidence intervals based on standard errors clustered at the city level. To map out the pattern in the counterfactual treatment effects we regress the various outcome measures on lag and lead indicators (bunched by 2 quarters) for the entry of rideshare. We provide a description of the variables in section 2.

Table 1: Summary Statistics: City Characteristics

City characteristic	Mean	Std. Dev.	Min.	Median	Max.	Number of cities
Population (thousands)	54.65	200.48	3.03	23.58	8,537.67	2,955
Income per capita (thousands $)	39.71	12.17	12.24	37.47	156.05	2,955
Population density	2,998.42	3,161.15	11.60	2,169.80	57,116.00	2,955
Carpool usage	10.63	3.98	1.52	10.06	48.23	2,955
Public transportation usage	2.97	4.97	0.00	1.19	56.30	2,955
Household vehicle onwership (thousands)	32.81	80.81	1.67	15.54	2,074.43	2,955
New car registration	672	2,346	0	265	181,433	2,955

Notes: The sample contains 189,120 quarterly observations on 2,955 census incorporated places from 2001 to 2016. Population density measures population per square mile. Carpool usage measures the percentage of population commuting to work using carpool. Public transportation usage measures the percentage of population commuting to work using public transportation. Household vehicle ownership measures the total number of available vehicles in households. New car registration measures the total number of new vehicle registrations.

Table 2: Summary Statistics: Accidents and Fatality Rates

Accident and fatality rates	Mean	Std. Dev.	Min.	Median	Max.	Number of cities
Accident rate	3.51	1.00	5.67	0.00	99.11	2,955
Fatality rate	3.86	1.02	6.52	0.00	122.05	2,955
Drunk accident rate	1.10	0.00	2.72	0.00	61.72	2,955
Drunk fatality rate	1.23	0.00	3.20	0.00	81.23	2,955
Drunk driver rate	1.21	0.00	3.11	0.00	69.44	2,955
Non-drunk accident rate	2.40	0.00	4.43	0.00	67.46	2,955
Non-drunk fatality rate	2.62	0.00	5.08	0.00	122.05	2,955
Pedestrian-involved accident rate	0.58	0.00	1.80	0.00	37.35	2,955
Pedestrian-involved fatality rate	0.60	0.00	1.86	0.00	38.64	2,955
Pedestrians Involved in Fatal Accidents	0.64	0.00	2.11	0.00	97.99	2,955

Notes: The sample contains 189,120 quarterly observations on 2,955 census incorporated places from 2001 to 2016. All rates are measured as of per 100,000 populations. Accident is the number of fatal accidents according to the definition used by NHTSA. Fatality is the total number of fatalities across all fatal accidents. Drunk accident is the number of fatal accidents involving any drunk drivers. Drunk fatality is the total number of fatalities in all drunk accidents. Non-drunk accident is the number of fatal accidents not involving any drunk drivers. Non-drunk fatality is the total number of fatalities in all non-drunk accidents. Pedestrian-involved accident is the number of fatal accidents involving at least one pedestrian. Pedestrian-involved fatalities is the total number of fatalities in all accidents involving at least one pedestrian. Pedestrians involved in fatal accidents is the total number of pedestrians involved in fatal accidents.

Table 3 Effect of Ridesharing on Traffic Safety

Panel A: Overall Effect

	(1) Log Total Accidents	(2) Log Total Accidents	(3) Log Total Fatalities	(4) Log Total Fatalities	(5) Log Drunk Accidents	(6) Log Drunk Accidents	(7) Log Drunk Fatalities	(8) Log Drunk Fatalities	(9) Log Non-Drunk	(10) Log Non-Drunk	(11) Log Non-Drunk	(12) Log Non-Drunk
$Post_t * Treated_c$	0.0141**	0.0356***	0.0131**	0.0354***	-0.0302***	0.0201***	-0.0315***	0.0200***	0.0336***	0.0309***	0.0332***	0.0309***
	(0.0064)	(0.0075)	(0.0067)	(0.0079)	(0.0055)	(0.0061)	(0.0059)	(0.0065)	(0.0063)	(0.0074)	(0.0065)	(0.0077)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.61	0.62	0.59	0.60	0.47	0.49	0.46	0.47	0.55	0.56	0.54	0.55

Panel B: Effect on Total Accidents by Day and Time

	(1) Log Weekday Accidents	(2) Log Weekday Accidents	(3) Log Weekend Accidents	(4) Log Weekend Accidents	(5) Log Accidents at Night	(6) Log Accidents at Night	(7) Log Accidents at Fri. and Sat. Night	(8) Log Accidents at Fri. and Sat. Night
$Post_t * Treated_c$	0.0103*	0.0277***	0.0122**	0.0340***	0.0225***	0.0387***	0.0113**	0.0243***
	(0.0057)	(0.0070)	(0.0056)	(0.0066)	(0.0058)	(0.0066)	(0.0047)	(0.0055)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	No	Yes	No	Yes	No	Yes	No	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.51	0.52	0.52	0.53	0.54	0.55	0.44	0.45

Panel C: Effect on Total Fatalities by Day and Time

	(1) Log Weekday Total Fatalities	(2) Log Weekday Total Fatalities	(3) Log Weekend Total Fatalities	(4) Log Weekend Total Fatalities	(5) Log Total Fatalities at Night	(6) Log Total Fatalities at Night	(7) Log Total Fatalities at Fri. and Sat. Night	(8) Log Total Fatalities at Fri. and Sat. Night
$Post_t * Treated_c$	0.0122**	0.0340***	0.0111*	0.0345***	0.0211***	0.0398***	0.0108**	0.0262***
	(0.0056)	(0.0066)	(0.0059)	(0.0070)	(0.0061)	(0.0070)	(0.0049)	(0.0059)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	No	Yes	No	Yes	No	Yes	No	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.52	0.53	0.51	0.52	0.53	0.54	0.43	0.44

Notes: This table presents results from generalized difference-in-difference regressions. The dependent variables are the natural logarithm of various traffic safety measures listed at the top of each column. $Post_t * Treated_c$ is a dummy variable that equals one if city c adopted at least one rideshare service at time t. City-specific linear trends are excluded in odd-numbered columns and included in even-numbered columns. Panel A presents the overall effect of ridesharing on 6 traffic safety measures. Total Accidents is the number of fatal accidents according to the definition used by NHTSA. Total Fatalities is the total number of fatalities across all fatal accidents. Drunk accident is the number of fatal accidents involving any drunk drivers. Drunk fatality is the total number of fatalities in all drunk accidents. Non-drunk accident is the number of fatal accidents not involving any drunk drivers. Non-drunk fatality is the total number of fatalities in all non-drunk accidents. Panel B and C presents the effect of ridesharing on accidents and fatalities, respectively, by day and time. Weekday is defined as Monday through Thursday. Weekend is defined as Friday through Sunday. Night is defined as 5pm through 2am. Friday and Saturday Night is defined as 5pm through 6am on Friday and Saturday. Control variables in all regressions include the natural logarithm of population and the level of income per capita. Standard errors, adjusted for clustering at the city level, are reported in parentheses. $***$, $**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4 Dynamic Effect of Ridesharing on Traffic Safety

	(1) Log Total Accidents	(2) Log Total Fatalities	(3) Log Drunk Accidents	(4) Log Drunk Fatalities	(5) Log Non-Drunk Accidents	(6) Log Non-Drunk Fatalities
Rideshare Tenure						
1 - 2 Quarters	0.0326***	0.0323***	0.009	0.0092	0.0291***	0.0291***
	(0.0101)	(0.0105)	(0.0085)	(0.0091)	(0.0098)	(0.0102)
3 - 4 Quarters	0.0375***	0.0359***	0.0295***	0.0309***	0.0265**	0.0236**
	(0.0114)	(0.0119)	(0.0091)	(0.0098)	(0.0111)	(0.0115)
5 - 6 Quarters	0.0329***	0.0356***	0.0149	0.0154	0.0354***	0.0370***
	(0.0128)	(0.0135)	(0.0095)	(0.0103)	(0.0126)	(0.0132)
7 - 8 Quarters	0.0409***	0.0421***	0.0231**	0.0215*	0.0384***	0.0419***
	(0.0132)	(0.0139)	(0.0107)	(0.0114)	(0.0131)	(0.0137)
9 - 10 Quarters	0.0372**	0.0332**	0.0440***	0.0391***	0.0251	0.0249
	(0.0157)	(0.0164)	(0.0126)	(0.0134)	(0.0154)	(0.0161)
11 - 12 Quarters	0.0466**	0.0475*	0.0263	0.0277	0.0500**	0.0501**
	(0.0232)	(0.0243)	(0.0204)	(0.0222)	(0.0216)	(0.0223)
> 12 Quarters	0.0838**	0.0826**	0.0545*	0.0557*	0.0829**	0.0807**
	(0.0358)	(0.0367)	(0.0314)	(0.0332)	(0.0340)	(0.0346)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.62	0.60	0.49	0.47	0.56	0.55

Notes: This table presents the dynamic effects of ridesharing on traffic safety. The dependent variables are the natural logarithm of various traffic safety measures listed at the top of each column. Total Accidents is the number of fatal accidents according to the definition used by NHTSA. Total Fatalities is the total number of fatalities across all fatal accidents. Drunk accident is the number of fatal accidents involving any drunk drivers. Drunk fatality is the total number of fatalities in all drunk accidents. Non-drunk accident is the number of fatal accidents not involving any drunk drivers. Non-drunk fatality is the total number of fatalities in all non-drunk accidents. Rideshare tenure variables are dummy variables that take the value of one if rideshare has been in effect for the specified periods of time. All columns include city-specific linear trends. Control variables in all regressions include the natural logarithm of population and the level of income per capita. Standard errors, adjusted for clustering at the city level, are reported in parentheses. $***$, $**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 5 Variation of Ridesharing Service

Panel A: Single Ride Services vs. Pooled Ride Services

	(1) Log Total Accidents	(2) Log Total Fatalities	(3) Log Drunk Accidents	(4) Log Drunk Fatalities	(5) Log Non-Drunk Accidents	(6) Log Non-Drunk Fatalities
Single Ride Service (UberBlack/Taxi/X, Lyft)	0.0368***	0.0365***	0.0214***	0.0214***	0.0312***	0.0310***
	(0.0077)	(0.0080)	(0.0062)	(0.0067)	(0.0075)	(0.0078)
Pooled Ride Service (Uber Pool, Lyft Line)	-0.0140	-0.0128	-0.0113	-0.0123	-0.0064	-0.0046
	(0.0151)	(0.0159)	(0.0127)	(0.0134)	(0.0148)	(0.0155)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.62	0.60	0.49	0.47	0.56	0.55

Panel B: Google Trends Intensity

	(1) Log Total Accidents	(2) Log Total Fatalities	(3) Log Drunk Accidents	(4) Log Drunk Fatalities	(5) Log Non-Drunk Accidents	(6) Log Non-Drunk Fatalities
$Post_t * Treated_c * \ Log\ Rideshare - Related\ Google\ Search\ Volume_{ct}$	0.0049***	0.0050***	0.0035***	0.0034***	0.0039***	0.0041***
	(0.0010)	(0.0010)	(0.0008)	(0.0008)	(0.0009)	(0.0010)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	153,660	153,660	153,660	153,660	153,660	153,660
R2	0.62	0.61	0.49	0.48	0.57	0.56

Notes: This table shows how the effect of ridesharing on traffic safety varies with the intensity of service. In all panels, the dependent variables are the natural logarithm of various traffic safety measures listed at the top of each column. Total Accidents is the number of fatal accidents according to the definition used by NHTSA. Total Fatalities is the total number of fatalities across all fatal accidents. Drunk accident is the number of fatal accidents involving any drunk drivers. Drunk fatality is the total number of fatalities in all drunk accidents. Non-drunk accident is the number of fatal accidents not involving any drunk drivers. Non-drunk fatality is the total number of fatalities in all non-drunk accidents. In Panel A, Single (Pooled) Ride Service is a dummy variable that takes the value of one if any single (pooled) ride services is adopted. In Panel B, Log Rideshare-Related Google Search Volume is the natural logarithm of Google search volume for the terms "Uber," "Lyft," and "Rideshare." All columns include city-specific linear trends. Control variables in all regressions include the natural logarithm of population and the level of income per capita. Standard errors, adjusted for clustering at the city level, are reported in parentheses. $***$,$**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 6 Externality of Ridesharing on Pedestrians

	(1) Log Pedestrian-Involved Accident	(2) Log Pedestrian-Involved Accident	(3) Log Pedestrian-Involved Fatalities	(4) Log Pedestrian-Involved Fatalities	(5) Log Pedestrians Involved in Fatal Accidents	(6) Log Pedestrians Involved in Fatal Accidents
$Post_t * Treated_c$	0.0318***	0.0249***	0.0319***	0.0250***	0.0325***	0.0280***
	(0.0051)	(0.0058)	(0.0052)	(0.0059)	(0.0054)	(0.0063)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	No	Yes	No	Yes	No	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.53	0.54	0.53	0.54	0.51	0.52

Notes: This table presents results from generalized difference-in-difference regressions. The dependent variables are the natural logarithm of pedestrian-related traffic safety measures listed at the top of each column. Pedestrian-involved accident measures the number of fatal accidents involving at least one pedestrian. Pedestrian-involved fatalities measures the total number of fatalities in all accidents involving at least one pedestrian. Pedestrians involved in fatal accidents measures the total number of pedestrians involved in fatal accidents. $Post_t * Treated_c$ is a dummy variable that equals one if city c adopted at least one rideshare service at time t. City-specific linear trends are excluded in odd-numbered columns and included in even-numbered columns. Control variables in all regressions include the natural logarithm of population and the level of income per capita. Standard errors, adjusted for clustering at the city level, are reported in parentheses. $***$, $**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7 Heterogeneous Effects

Panel A: City Characteristics

	Population				Per Capita Income				Population Density			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Log Total Accidents	Log Total Fatalities	Log Pedestrian-Involved Accident	Log Pedestrian-Involved Fatalities	Log Total Accidents	Log Total Fatalities	Log Pedestrian-Involved Accident	Log Pedestrian-Involved Fatalities	Log Total Accidents	Log Total Fatalities	Log Pedestrian-Involved Accident	Log Pedestrian-Involved Fatalities
$Post_t * Treated_c * Q4$	0.0752***	0.0775***	0.0649***	0.0655***	0.0375***	0.0372***	0.0261***	0.0274***	0.0364***	0.0358***	0.0178**	0.0178*
	(0.0115)	(0.0119)	(0.0102)	(0.0103)	(0.0114)	(0.0119)	(0.0088)	(0.0090)	(0.0111)	(0.0114)	(0.0091)	(0.0092)
$Post_t * Treated_c * Q3$	0.0032	0.0032	-0.0065	-0.0071	0.0126	0.0122	0.0214**	0.0206**	0.0263*	0.0263*	0.0328***	0.0320***
	(0.0138)	(0.0146)	(0.0095)	(0.0097)	(0.0116)	(0.0121)	(0.0092)	(0.0093)	(0.0141)	(0.0148)	(0.0111)	(0.0112)
$Post_t * Treated_c * Q2$	0.0025	-0.0021	-0.0144	-0.0152*	0.0539***	0.0552***	0.0385***	0.0381***	0.0507***	0.0491***	0.0513***	0.0524***
	(0.0159)	(0.0168)	(0.0091)	(0.0092)	(0.0144)	(0.0151)	(0.0121)	(0.0121)	(0.0164)	(0.0171)	(0.0133)	(0.0136)
$Post_t * Treated_c * Q1$	0.0036	0.0009	-0.0031	-0.0025	0.0525***	0.0512***	0.0118	0.0121	0.0307*	0.0332*	-0.0020	-0.0014
	(0.0131)	(0.0136)	(0.0077)	(0.0079)	(0.0178)	(0.0186)	(0.0140)	(0.0142)	(0.0167)	(0.0179)	(0.0117)	(0.0119)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.62	0.60	0.54	0.54	0.62	0.60	0.54	0.54	0.62	0.60	0.54	0.54

Panel B: Ex-ante Behavior

	Ex Ante Vehicle Ownership				Ex Ante Public Transportation Usage				Ex Ante Car Pool Usage			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Log Total Accidents	Log Total Fatalities	Log Pedestrian-Involved Accident	Log Pedestrian-Involved Fatalities	Log Total Accidents	Log Total Fatalities	Log Pedestrian-Involved Accident	Log Pedestrian-Involved Fatalities	Log Total Accidents	Log Total Fatalities	Log Pedestrian-Involved Accident	Log Pedestrian-Involved Fatalities
$Post_t * Treated_c * Q4$	0.0781***	0.0803***	0.0678***	0.0683***	0.0367***	0.0364***	0.0432***	0.0428***	0.0467***	0.0466***	0.0317**	0.0319**
	(0.0113)	(0.0117)	(0.0100)	(0.0102)	(0.0115)	(0.0119)	(0.0096)	(0.0097)	(0.0153)	(0.0164)	(0.0128)	(0.0129)
$Post_t * Treated_c * Q3$	-0.0003	-0.0023	-0.0084	-0.0091	0.0517***	0.0548***	0.0267**	0.0280***	0.0624***	0.0611***	0.0482***	0.0480***
	(0.0147)	(0.0155)	(0.0108)	(0.0109)	(0.0126)	(0.0132)	(0.0104)	(0.0105)	(0.0136)	(0.0138)	(0.0115)	(0.0116)
$Post_t * Treated_c * Q2$	-0.0051	-0.0068	-0.0100	-0.0102	0.0194	0.0143	0.0211*	0.0194	0.0184	0.0210	0.0140	0.0144
	(0.0152)	(0.0162)	(0.0098)	(0.0100)	(0.0158)	(0.0166)	(0.0118)	(0.0120)	(0.0131)	(0.0135)	(0.0095)	(0.0097)
$Post_t * Treated_c * Q1$	0.0113	0.0090	-0.0096	-0.0094	0.0207	0.0219	-0.0284**	-0.0264**	0.0087	0.0071	0.0002	0.0002
	(0.0141)	(0.0146)	(0.0072)	(0.0073)	(0.0192)	(0.0202)	(0.0117)	(0.0121)	(0.0131)	(0.0139)	(0.0095)	(0.0097)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.62	0.60	0.54	0.54	0.62	0.60	0.54	0.54	0.62	0.60	0.54	0.54

Notes: This table presents heterogeneous effects of ridesharing on traffic safety. In all panels, the dependent variables are the natural logarithm of various traffic safety and externality measures listed at the top of each column. Panel A and Panel B breaks out results across a variety of city characteristics and ex-ante behaviors, respectively. The variables used for sample cut is listed at the top of each panel. Population density measures population per square mile. Vehicle ownership measures the total number of available vehicles in households. Public transportation usage measures the percentage of population commuting to work using public transportation. Carpool usage measures the percentage of population commuting to work using carpool. Total Accidents is the number of fatal accidents according to the definition used by NHTSA. Total Fatalities is the total number of fatalities across all fatal accidents. Pedestrian-involved accident measures the number of fatal accidents involving at least one pedestrian. Pedestrian-involved fatalities measures the total number of fatalities in all accidents involving at least one pedestrian. The independent variables of interest are the interaction of $Post_t * Treated_c$, a dummy variable that equals one if city c adopted at least one rideshare service at time t, and an indicator for the quartile the observation falls in. Apart from the natural logarithm of population and the level of income per capita, all interacted variables are included separately as control variables. All columns include city-specific linear trends. Standard errors, adjusted for clustering at the city level, are reported in parentheses. $***$,$**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 8 Effect of Ridesharing on Road Utilization and Congestion

	(1)	(2)	(3)
	Log Arterial Street VMT	Log Excess Fuel Consumption	Log Hours of Delay
$Post_t * Treated_u$	0.0296*	0.0170**	0.0170**
	(0.0158)	(0.0075)	(0.0075)
Urban Area and Year Fixed Effects	Yes	Yes	Yes
Urban Area Linear Trend	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes
Observations	1,386	1,386	1,386
R2	0.998	0.999	0.999

Notes: The sample contains 1,386 annual observations on 99 urban areas from 2001 to 2014. The dependent variables are the natural logarithm of congestion-related measures listed at the top of each column. Arterial Street VMT measures the total number of vehicle-miles-traveled in arterial streets in an urban area. Excess fuel consumption measures the extra fuel consumed due to inefficient operation in slower stop-and-go traffic. Hours of delay measures the amount of extra time spent traveling due to congestion. $Post_t * Treated_u$ is a dummy variable that equals one if urban area u adopted at least one rideshare service at year t. Urban area-specific linear trends are included in all regressions. Control variables include the natural logarithm of population and the level of income per capita. Standard errors, adjusted for clustering at the urban area level, are reported in parentheses. $***$, $**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively. For more detailed information on the dependent variables, please refer to https://static.tti.tamu.edu/tti.tamu.edu/documents/mobility-scorecard-2015-wappx.pdf.

Table 9 The Effect of Rideshare on New Car Registrations

Panel A: Overall Effect

	(1) Log New Car Registrations	(2) Log New Car Registrations
$\text{Post}_t * \text{Treated}_c$	0.0205**	0.0518***
	(0.0083)	(0.0070)
Quarter and City Fixed Effects	Yes	Yes
City Linear Trend	No	Yes
Control Variables	Yes	Yes
Observations	189,120	189,120
R2	0.94	0.97

Panel B: Intensity

	(1) Log New Car Registrations	(2) Log New Car Registrations
Google Search Volume		
$\text{Post}_t * \text{Treated}_c * \text{ Log Rideshare} - \text{Related Google Search Volume}_{ct}$	0.0083***	
	(0.0009)	
Rideshare Service Type		
Single Ride Service (UberBlack/Taxi/X, Lyft)		0.0496***
		(0.0069)
Pooled Ride Service (Uber Pool, Lyft Line)		0.0294***
		(0.0106)
Quarter and City Fixed Effects	Yes	Yes
City Linear Trend	Yes	Yes
Control Variables	Yes	Yes
Observations	153,660	189,120
R2	0.97	0.97

Panel C: Heterogeneous Effects

Dep: Log New Car Registration	(1) Population	(2) Pop Density	(3) Public Transport	(4) Carpool	(5) Vehicle Ownership
$Post_t * Treated_c * Q4$	0.0873***	0.0958***	0.0624***	0.1612***	0.0832***
	(0.0099)	(0.0119)	(0.0110)	(0.0148)	(0.0095)
$Post_t * Treated_c * Q3$	0.0371**	0.0575***	0.0737***	0.0707***	0.0484***
	(0.0146)	(0.0113)	(0.0127)	(0.0111)	(0.0135)
$Post_t * Treated_c * Q2$	0.0204	0.0207*	0.0334**	0.0238**	0.0034
	(0.0170)	(0.0121)	(0.0145)	(0.0112)	(0.0197)
$Post_t * Treated_c * Q1$	0.0077	-0.0239	0.0024	-0.0580***	0.0254
	(0.0150)	(0.0191)	(0.0201)	(0.0128)	(0.0157)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes	Yes
Observations	189,120	189,120	189,120	189,120	189,120
R2	0.97	0.97	0.97	0.97	0.97

Notes: This table presents the effect of ridesharing on new car registrations. In all panels, the dependent variables are the natural logarithm of new car registrations. $Post_t * Treated_c$ is a dummy variable that equals one if city c adopted at least one rideshare service at time t. Panel A presents results from generalized difference-in-difference regressions. Panel B shows how the effect varies with the intensity of rideshare service. Log Rideshare-Related Google Search Volume is the natural logarithm of Google search volume for the terms “Uber,” “Lyft,” and “Rideshare.” Single (Pooled) Ride Service is a dummy variable that takes the value of one if any single (pooled) ride services is adopted. Panel C breaks out results across a variety of city characteristics and ex-ante behaviors. The variables used for sample cut is listed at the top of each column. Pop density measures population per square mile. Public transportation usage measures the percentage of population commuting to work using public transportation. Carpool usage measures the percentage of population commuting to work using carpool. Household vehicle ownership measures the total number of available vehicles in households. Apart from the natural logarithm of population and the level of income per capita, all interacted variables are included separately as control variables. All columns include city-specific linear trends. Standard errors, adjusted for clustering at the city level, are reported in parentheses. $***$,$**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.