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 approximately 3% in the number of motor vehicle fatalities and fatal accidents. This increase is not only for vehicle occupants but also 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 use, we find that its introduction 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's arrival is also associated with an increase in new car registrations. These effects are higher in cities with prior higher use of public transportation and carpools, consistent with a substitution effect, and in larger cities and cities with high 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, has fundamentally changed how many individuals are transported in cities and towns across the United States. While the ability to easily hail a ride through a smartphone app has undoubtedly increased convenience for people seeking transportation and flexible employment, critics increasingly argue that ridesharing creates offsetting negative effects, such as increases in traffic congestion and car-exhaust pollution. Are there, in fact, significant externalities that accompany 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 across U.S. cities, we show that its introduction in a metropolitan area leads to an economically meaningful increase in overall motor vehicle fatalities. This increase is consistent with acknowledged macro trends in motor vehicle accidents, which had been falling steeply in the United States over the period 1985 to 2010, when ridesharing first launched, and have since reversed course (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 sees it as removing drivers who would have driven themselves with their cars and replacing them with rideshare drivers for the same mileage. Under this view, ridesharing substitutes self-drivers with rideshare drivers on a one-to-one basis. One might also argue that many of the users who are substituting to 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 thus potentially increases the quality of driving, while (in theory) holding car utilization fixed. Under this view, there will be no increase in the vehicle miles traveled and a possible increase in driver quality, and consequently there should be no increase in accident rates—in fact, there might even be a reduction.

This naïve view, however, ignores some of the more subtle 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 on the road: they must drive from fare to fare, and they drive from location to location in the city seeking better fare prospects, as there is not always

[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.

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.

The naïve view also assumes that only those who would have otherwise driven themselves are now using ridesharing, which is unlikely. The convenience and lower pricing of ridesharing apps suggest that there may be a significant number of additional riders substituting away from other modes of transportation, such as subways, buses, biking or walking; persons who would have used these modes 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 a car (Schaller, 2018). Moreover, a survey conducted by the University of California at Davis of over 4,000 residents in seven major metros areas found that only 39% of respondents would drive themselves, carpool, or take a taxi if ridesharing had not been available. The rest substitute from rail, biking, walking or not traveling at all (Clewlow and Mishra, 2017).[2] This survey evidence runs counter to the naïve view's notion that utilization remains fixed.[3]

Here, we take a more nuanced view of the overall effect of ridesharing on road safety. We begin by proposing a conceptual framework for considering how ridesharing's introduction may affect accident rates.[4] While the naïve view holds the utilization and supply of drivers constant, our view incorporates rational choice theory to drivers' and riders' decisions. Our framework models accidents as a function of vehicle miles traveled and average driver quality, both of which are in turn affected by the introduction of ridesharing technology. The advent of ridesharing makes car travel easier for riders, which, in turn, should decrease the marginal cost of making a trip for them, 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, combined with the addition of driving from fare to fare, should

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[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] 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 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, D.C., metro areas (Schaller, 2018). While pooling services, such as UberPool and LyftLine, can reduce the overall increase in vehicle miles, these modes of ridesharing currently represent a relatively small (20%) share of overall rides.

[4] Our theoretical analysis of ridesharing's effect on safety can be thought of along the lines of the traditional offsetting behaviors literature (Peltzman, 1975).

lead to overall increases in vehicle miles traveled. Depending on the quality of the new rideshare drivers, as compared to the driving quality of former drivers who now become riders, this may also lead to a 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. 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). The data does not distinguish accidents in which a rideshare driver-partner car was involved from those where one was not; rather, we examine overall motor vehicle fatalities in a city. From a policy perspective, this distinction is not critical—we wish to explore how the introduction of ridesharing as a phenomenon shapes *total* accident rates.

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 raw 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. We investigate this increase formally using a difference-in-differences specification with fixed effects for location and time (quarter-year) and a location-specific linear trend. 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 that persists 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 inclusion of a location-specific quadratic trend, and are similar for a variety of different specifications of the left-side accident measure, including separating out drunk and nondrunk accidents.

Having established our primary finding, we proceed to examine differentials in outcomes for pooled versus nonpooled services. Despite allowing for more carpools and therefore a potential reduction of 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 high 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.

Presumably, the intensity of rideshare use should be related to the documented increase in 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 of ridesharing more directly: pedestrians and bike riders represent a population that is neither using a rideshare car nor riding in or driving a private vehicle. Here, we find a similar magnitude increase in the number of fatal accidents, the number of pedestrians and bike riders involved in these accidents, and the number of fatalities in such accidents, suggesting that the introduction of ridesharing imposes a negative externality on pedestrians and bike riders, in addition to affecting vehicle occupants.

Of course, 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 cities with higher levels of income inequality (as measured by Gini coefficient). They are primarily concentrated in cities where the ex ante use of public transportation was higher, consistent with substitution away from an alternative mode of transportation, and in cities with high ex ante levels of vehicle ownership, consistent with increasing the 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 rise, 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 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 drivers become more experienced, both the VMT effects and the driver quality effects may be attenuated. In our sample through the end of 2016, however, we observe no such reversion; instead, the estimates appear to be increasing with time since rideshare launch, and the persistence is statistically significant. 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.

We conclude our study with a back-of-the-envelope discussion of potential costs from an increase in fatalities of the magnitude estimated in this paper. Utilizing estimates of the value of a statistical life from the Department of Transportation, we estimate a potential cost of just under $10 billion. We end with a discussion of other potential societal costs suggested by some of our findings, which suggest the need for further research.

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 citations for driving while under the influence of alcohol (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/fatalities and DUIs. In contrast, we do not place our focus solely on fatalities resulting from drunk driving or alcohol consumption. Rather, we focus on total fatal accidents, using a broad sample, accounting for the introduction of both Uber and Lyft, including the different types of Uber and Lyft service.[5] While ridesharing may indeed displace 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.

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 20+ years, motor vehicle accident fatalities, in total, per population, and per VMT, had been falling. The 2010s saw a reversal of these trends. If this reversal relates partly to increased vehicle miles

[5] 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.

on the road, due to the introduction of ridesharing, this may have implications for policy discussions around decreasing motor vehicle accident rates.

Our study offers several contributions to the existing literature. First and foremost, our work speaks to the importance of considering externalities—both positive and negative—associated with the introduction of new technologies. Often, discussion of externalities of new technologies focuses on positive externalities and benefits to society (e.g. Klenow and Rodriguez-Clare, 2005). Here, in contrast, we consider that some technologies may also impose negative externalities. When new technologies are introduced in markets that account for these externalities, they often induce competition with existing products and services that enhances welfare. If these negative externalities are not accounted for, even if the private costs are exceeded by the private benefits for the individual user, the social costs may not be. It is the *sum* of social and private costs as compared to the *sum* of social and private benefits that is key to welfare effects.

In the case of ridesharing, whether there were associated negative externalities was not clear to economists ex ante. In a 2014 University of Chicago Initiatives on Global Markets (IGM) survey of a panel of 43 top academic economists,[6] all the panelists either agreed or strongly agreed with the statement "Letting car services such as Uber or Lyft compete with taxi firms on equal footing regarding genuine safety and insurance requirements, but without restrictions on prices or routes, raises consumer welfare." Many commented on the contribution of competition to consumer welfare; none suggested any potential negative externalities (one Nobel Prize winner noted specifically that he did not see any externalities involved). The comments were consistent with the panelists considering private welfare, rather than social welfare. Here, we shed light on the potential social costs of ridesharing. Our results speak to a growing literature on the social and economic impacts of digitization (Brynjolfsson and McAfee, 2014). In this spirit, our paper joins contemporaneous work by Hasan and Kumar (2018), who also explore social costs of technology adoption, but in the setting of online school ratings and their effects on educational inequality.

Second, our study 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

[6] http://www.igmchicago.org/surveys/taxi-competition

to the flexibility of rideshare work arrangements, and find that 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 in 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, Hall et. al. (2018) finds ridesharing complements public transit, 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 findings may be a reason to reframe the discussion around cities' 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 considerable societal costs are also at play. In ridesharing's case, delays in the diffusion of this 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.

The paper proceeds as follows. Section 2 provides an overview of ridesharing services and outlines our conceptual framework. Section 3 describes our sample and data sources. Section 4 presents our main empirical results. 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 and Conceptual Framework

Before the advent of ridesharing, the primary forms of private for-hire transportation were 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, 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, prices charged, and the licensing and insurance requirements for drivers and cars. Quantity restrictions, in particular, were thought to lead to shortages of taxis during periods of high demand and inconveniences for riders.

Uber was the first ridesharing firm in the United States, launching in San Francisco in May 2010, and was followed two years later by Lyft and Sidecar. Ridesharing then expanded rapidly across the country. 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 easing the matching of riders and drivers and enabling seamless payment through an 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 drivers to participate in the market.[7] This has in turn engendered backlash from advocacy groups and policymakers concerned with the effects of ridesharing technology in their cities.[8]

2.1. Conceptual Model

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 that are impacted by the introduction of ridesharing technology: 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

[7] 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.

[8] In many ways, ridesharing has become the modern poster child for the classic battle between what are argued to be outdated regulations, supported by rent-seeking incumbents, and the introduction of a welfare-enhancing technology. Many new technologies face frictions that slow 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 before the rest of the world was that active resistance to technology adoption was weaker there. Most economic histories of technological adoptions provide cases in which adoption was met with fierce resistance (Mokyr 1990).

period t as $A_{i,t}$ and the new technology (ridesharing) as θ. Accident rates can then be thought of as:

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

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) 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, denoted by VMT^{own}; (ii) the number of VMTs generated by rideshare drivers carrying passengers from origin to destination, denoted by VMT^{RS}; and (iii) the number of VMTs generated by rideshare drivers while driving in-between 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 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, the 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 naïvely (and ignoring for the moment the UberPool and LyftLine services), each person who no longer chooses to drive himself or herself is now driven by a rideshare driver, thus precisely offsetting the effect on the overall vehicle miles traveled. But unless there are absolutely no between-fare miles driven by a ride-sharing driver, we would expect to see an increase in overall 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 of Chen et al. (2018) is consistent with a mismatch between rider demand and the supply of drivers.

More formally, we can 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, be larger in absolute 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 increase with the introduction of ridesharing.

Of course, in some cities, at later dates, the option to "carpool" in a rideshare was introduced, 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). Thus, it is likely that pooled ride adoption would need to be extremely high to offset such substitution effects.

Assessing the effects of the introduction of ridesharing on the quality of the average driver on the road is less straightforward. On the one hand, the people substituting into a rideshare, rather than driving themselves, may be low quality drivers (impaired or unskilled or may just prefer not to drive), but they may be high quality drivers who simply dislike driving. On the other hand, there is no guarantee that the driver who substitutes for them is of higher quality. Put another way, the introduction of ridesharing makes it less costly to have someone else drive you 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. Moreover, more affluent people are more likely to use ridesharing (Pew Research Center, 2016), 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 those with more experience from more hours driven, may in fact represent improved quality. To the extent that the substitution goes the other way and lower quality drivers are substituted by better drivers, this may reduce accident rates if the increase in quality offsets the increase in VMT.

Formally, $\frac{\partial A_i}{\partial Q_i}$ is negative: better drivers reduce accident rates, all else equal. The effect of ridesharing 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, this 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 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 directly or indirectly increases the probability of accidents, such as gaming time-and-distance pay algorithms by taking longer routes, etc.

3. Data and Sample

Our sample consists of all incorporated "places"[9] in the continental United States with population greater than or equal to 10,000 in 2010,[10] and that experienced at least one motor vehicle accident that results in a fatality ("fatal accident") during the period of 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 United States.[11] (For ease of interpretation, we interchangeably refer to these as "cities" or "locations" throughout the 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 United States, by cities/places and 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, a crash must 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 nonmotorist 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

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[9] 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 the census.

[10] Some places in our sample had lower populations than 10,000 during the sample period, most notably during the period of 2001–2010. We impose the cutoff on population as measured in 2010. As an example, consider Hutto, Texas, a suburb of the Austin-RoundRock metro area. In 2001, Hutto had a population of 3,030, the lowest in our sample. By 2010, it had grown to over 14,000, mimicking the growth of the Austin metro area. As it has population above 10,000 in 2010, it is included in our sample. Our results are robust to permutations to this cutoff.

[11] https://www.census.gov/content/dam/Census/data/developers/understandingplace.pdf

corresponding place/city. When the coordinates are not available, we use the city and state identifier codes to assign observations to the appropriate place. Geographic coordinates are present in 98% of FARS's 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-driver 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 nondrunk-driver accidents. We measure accident "rates" as the number of accidents per 100,000 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 5 pm and before 2 am; (iv) Friday and Saturday night: after 5 pm and before 6 am 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.[12] 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, which allow customers to hail regular cars driven by driver-partners; and (iii) UberPool/Lyft Line,

[12] 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 share a hailed vehicle with others. 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 the work of Cramer and Krueger (2016) and Hall et al. (2018) and use Google search volume for the terms "Uber," "Lyft," and "rideshare."[13] 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 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 use 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[14] 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 of 1982–2014. Of the 101 urban areas covered by TAMU in their report, 99 fall into our sample of continental U.S. cities. For a set of tests regarding road use and driver quality, we use the census's urban area-to-place crosswalk to aggregate our main sample at urban area and annual level to merge the information with TAMU's dataset.

[13] 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.

[14] 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.

3.4. Summary Statistics

Table 1 presents summary statistics for the places in our sample over the sample period. The places average 54,650 thousand in population, have an income per capita of $39,710, and population density of roughly 3,000 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 100,000 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 100,000 population. We present statistics for total accidents and fatalities, total drunk- and nondrunk-driver accidents and fatalities, and total pedestrian-related accidents and fatalities. Drunk-driver accidents and fatalities represent approximately one-third of the total accidents and fatalities. Pedestrian accidents and fatalities are approximately 20% of the total.

4. Empirical Analysis

To assess the impact of ridesharing 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 following 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 the first quarter of 2000 through the fourth quarter of 2016. 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-driver) accident rates. Figure 4 shows drunk-driver accidents per 100,000 population for early-adopter cities (2010–2012), mid-adopters (2013–2014) and late adopter cities (2015–2016) in the five years preceding ridesharing entry. As can be seen from the figure, drunk-driver 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, Table A5, we estimate a multinomial logit model, where the outcome variable is defined as 0 if the city adopted ridesharing in 2010 through 2012, 1 if the city adopted ridesharing in 2013 through 2014 (the base outcome), and 2 if the city adopted ridesharing in 2015 or 2016. 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.[15]

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) use total accidents, columns (3) and (4) use total fatalities, columns (5) and (6) use drunk-driver accidents, columns (7) and (8) use drunk-driver fatalities, and columns (9), (10), (11), and (12) use nondrunk-driver accidents and fatalities,

[15]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.

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 nondrunk-driver accidents and total nondrunk-driver 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 nondrunk-driver fatal accidents (column (9)). For the measures of drunk-driver accidents and drunk-driver fatalities, the coefficients are negative. However, as demonstrated by Figure 1, (drunk-driver) fatal accident rates had been falling dramatically for over a decade prior to ridesharing 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 that. Once we include the location-specific time trend, we observe a positive and significant coefficient on $POST_t * TREATED_c$ for all 12 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.

We note that for drunk-driver 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 studies using 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 resemble those in our other specifications.[16]

[16] The inclusion of a location-specific linear time trend is important: accident rates, particularly drunk driving-related accident rates, were falling steeply in the United States over the period of 1985 to 2010, when ridesharing was launched, and have since reversed course. Moreover, we document that ridesharing launched first in cities that had experienced steeper declines in drunk-driver 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-driver 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-driver accidents.

All our results are robust to inclusion of a city-specific quadratic trend as well. We demonstrate this in the appendix, Table A1, where we additionally break out the effect of introducing each element of our main specification in turn. Figure A1 graphs the coefficient and associated confidence interval for the variable $POST_t * TREATED_c$, first itself, then adding year fixed effects, city fixed effects, and the city-specific linear trend, and city-specific quadratic trend, each in turn.

Figure 5, Panels A and B, graphically present the difference-in-differences estimators (with each dot representing two quarter-coefficients) for the eight quarters preceding and following rideshare adoption for total accidents, and drunk-driver accidents. In both panels, the counterfactual treatment effects in the pre-ridesharing periods are statistically indistinguishable from zero, providing support for our inferences (parallel trends in the pre-period).[17] Post-ridesharing, we see a clear increasing treatment effect. 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 5 pm, and before 6 am) 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 graph these estimates in Figure 6. Panel A presents the estimates and confidence intervals for total accidents, total fatalities and total drunk accidents, on weekends and nights. Panel B further splits the sample into large (highest quartile) and small (lowest quartile) cities by population, and graphs the estimates for accidents and drunk accidents on weekends and nights for each. As can be seen from the panel, the effects of ridesharing are larger in larger cities.

We then 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. The table shows that ridesharing's increase in accidents and fatalities

[17] 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 assurance that the parallel trends assumption is valid in our analyses.

persists and, in fact, appears to be increasing six quarters after introduction in the city, consistent with a time gap between launch and widespread adoption in a location.

In Appendix Table A2, we further examine the robustness of our results to variations in how we measure the outcome variables. Column (1) of the table presents the estimates from our main model, column (2) drops all observations with zero accidents, column (3) utilizes the level of accidents per 10,000 population, and column (4) drops the zero accident observations and takes the log of the measure in column (3). We graph these results in Appendix Figure A2. Appendix Table A3 similarly demonstrates robustness to population-weighting our main model specification, and we present the coefficients graphically in Figure A3. In Appendix Table A4, we further demonstrate robustness to controlling for the population growth rate, retail gas prices, the change in retail gas prices, and the unemployment rate. The coefficients are graphically presented in Figure A4.

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 nonpooled service. The estimates in the table suggest that the rollout of pooled ride services does not reverse the overall treatment effect of nonpool rideshare. The coefficients for pool launch are roughly half the magnitude of those for single ride (nonpool) rideshare launch—but negative—and are not statistically significant at conventional levels. This may be consistent with relatively low adoption rates for pooled rides, even in cities that offer the service.

In Panel B of Table 5, we explore the effect of the intensity of service adoption. In the main models presented in Table 3, we employ the first launch of a ridesharing service, irrespective of type of service, as our treatment date. Take up of 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 $accidents$ measure as total accidents in column (1), total fatalities in column (2), total drunk-driver accidents in column (3), total drunk-driver fatalities in column (4), and total nondrunk-driver accidents and nondrunk-driver 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 among vehicle occupants versus the alternative of potentially imposing an externality on pedestrians (nonvehicle occupants). The increase in accidents could primarily affect vehicle occupants, or it could additionally affect bystanders. The FARS data allow 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).[18] Thus, "pedestrian" in our context refers to both pedestrians in the usual sense, as well as bicycle, skateboard and scooter riders, etc.

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 estimates of our main models, suggesting that the increase in accidents, following rideshare entry, imposes an externality on nonvehicle occupants. 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 who 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 C graphically presents the difference-in-differences estimators (with each dot representing two quarter-coefficients) for the eight quarters preceding and following rideshare adoption for pedestrian accidents. As in our main models, the counterfactual treatment effects in

[18] FARS defines a pedestrian as "any person not in or upon a motor vehicle or other vehicle."

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 inequality, and population density—as well as by ex ante vehicle ownership, public transport usage, and 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 $accidents$ measures 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 (fourth quartile). 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 Gini coefficient. Here, we see stronger effects for cities in the top quartile of income inequality, and for pedestrian-involved accidents and fatalities, the effect appears to be fully concentrated in the top three quartiles of city income inequality. 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. These estimates and the associated confidence intervals are also graphically presented in Figure 7, Panel A.

Panel B of Table 7 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 appears 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 who 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 the second quartile, 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 had above-median carpool usage. These estimates would be consistent with a substitution effect to ridesharing and away from public transport and carpooling. We graph the estimates and associated confidence intervals in Figure 7, Panel B.

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 now consider one of the two mechanisms discussed in our conceptual framework: increases in quantity (road utilization in the form of VMT). Road-utilization 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 use 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.[19]

[19] 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

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, while some of the *rider* population is forgoing owning a vehicle, others may be purchasing cars to work as rideshare *drivers*. Which effect 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 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 advance 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 with the intensity of Google searches for Uber/Lyft/rideshare. This relationship intensifies when ridesharing begins in a treated city. These results suggest that new vehicle purchases increase 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

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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.

with the *lowest* quartile of ex ante carpool usage. These results further reinforce the likelihood that ridesharing pulls riders away from noncar 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 the third quartile of density, a 2% increase for cities in the second quartile, and a statistically insignificant 2% *decrease* in cities in the lowest quartile of population density. Overall, this fact pattern suggests increases in congestion prompted by ridesharing. The increase in new car registration also appears to be concentrated in cities with higher population levels in general, consistent with our findings regarding VMT (the intensive margin). In contrast, the increase in new car registrations is stronger for cities with lower income inequality. This is perhaps unsurprising; while the more affluent are more likely to use ridesharing, the less affluent the lower tiers of society who are the likely rideshare drivers, the less likely they are to be able to purchase or lease new cars in order to become drivers. Overall, this may then lead to rideshare driving being done in existing older or lower quality cars, leading to a decrease in the quality channel, consistent with our finding that accident rates increase more in cities with higher income inequality.

6. Discussion and Welfare

Up until this point, our study has documented the cost associated with the introduction of ridesharing. To make a welfare calculation, we must also consider its benefits. Benefits come from, for example, the consumer surplus provided by convenience. 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 UberX 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 United States, 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.[20] The 3% annual increase associated with the introduction of ridesharing in fatalities represents an additional 987 lives lost each year.[21] 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.'s (2018) estimates of consumer surplus generated by ridesharing services suggests that the costs from the increase in fatal accidents match or surpass the benefits to direct consumers of ridesharing. Our estimates, moreover, do not include the costs imposed by nonfatal accidents, for which data is not readily available. We can assume that the pattern for fatal accidents is repeated for nonfatal accidents, leading to costs in material and healthcare that 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 in the economy. Importantly, this study documents only one particular social cost associated with ridesharing, much as Cohen et al. (2018) documents a particular type of surplus. Our findings, however, suggest significant additional costs beyond the loss of life associated with increased traffic fatalities. Nationally, the number of traffic accidents in which individuals are injured is an order of magnitude higher than the number of those in which there is a fatality. Detailed data on such accidents and the associated costs associated with medical care and property damage is generally unavailable, but our findings would suggest that an increase in such accidents is also likely to be present, with large associated societal costs.

Additionally, even ignoring the contribution of increased road utilization to accident rates, our findings suggest an increase in road utilization and congestion that imposes additional costs on society. While an increase in congestion may impose incremental costs on individuals driving to

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

work or to a social event, it can impose much greater costs on first responders and those being assisted by them. For illustration, suppose there are 100 heart attack victims transported to the emergency room each day by car or ambulance. These individuals face much higher costs from congestion and road delays. As congestion increases, a higher proportion of these 100 cases may encounter a delay in receiving life-saving medical attention. The disutility of the externality imposed by congestion is heterogeneous, however, unlike, say, the case of congestion in broadband telecommunication services, it is not straightforward to solve this with differential pricing.

In sum, our findings suggest more research will be needed to better quantify both the societal cost and benefit of ridesharing. More generally, our work points to the need for better consideration of societal costs and externalities associated with the introduction of new technologies.

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 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:

> 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 … [W]ith 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 in the cities it serves. We document a roughly 2% to 4% increase in the number of fatal accidents, which persists throughout the week, on weekends, at night, and on weekend nights. We develop a conceptual framework for analyzing how the introduction of ridesharing may affect accident rates, which models 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 rise 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 registrations is more substantial in cities with high ex-ante use of public transportation, 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, they are worth further investigation and discussion. Moreover, while ridesharing appears associated with more motor vehicle deaths, it does also bring many benefits. These include improved mobility for the disabled and minorities, flexible job opportunities that are especially valuable to those otherwise at high risk of unemployment, and customer convenience and resulting consumer surplus.

Still, the annual cost in human lives is nontrivial, and it is higher than estimates for annual consumer surplus generated. And on top of this, our estimates do not include the costs imposed by nonfatal accidents, for which data is not readily available. We can assume that the pattern for fatal accidents is also repeated for nonfatal accidents, leading to costs in material and healthcare that 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 and ways to increase the benefits or reduce the costs. Further research on this issue will likely necessitate unrestricted access to private data generated by rideshare companies.

Finally, given the relatively short period in which ridesharing has existed, our results are necessarily short term. The long-term consequences of ridesharing may differ, as individuals may change behavior as time passes. For example, some drivers may exit the market, and those who remain may gain knowledge and improve their driving with the platforms. 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 increases, car utilization may rise, lowering the number of vehicle miles traveled overall. Thus, any regulatory actions should proceed cautiously, considering the short-term effects of ridesharing documented here, the real and potential benefits and the necessity for further research on the outcomes.

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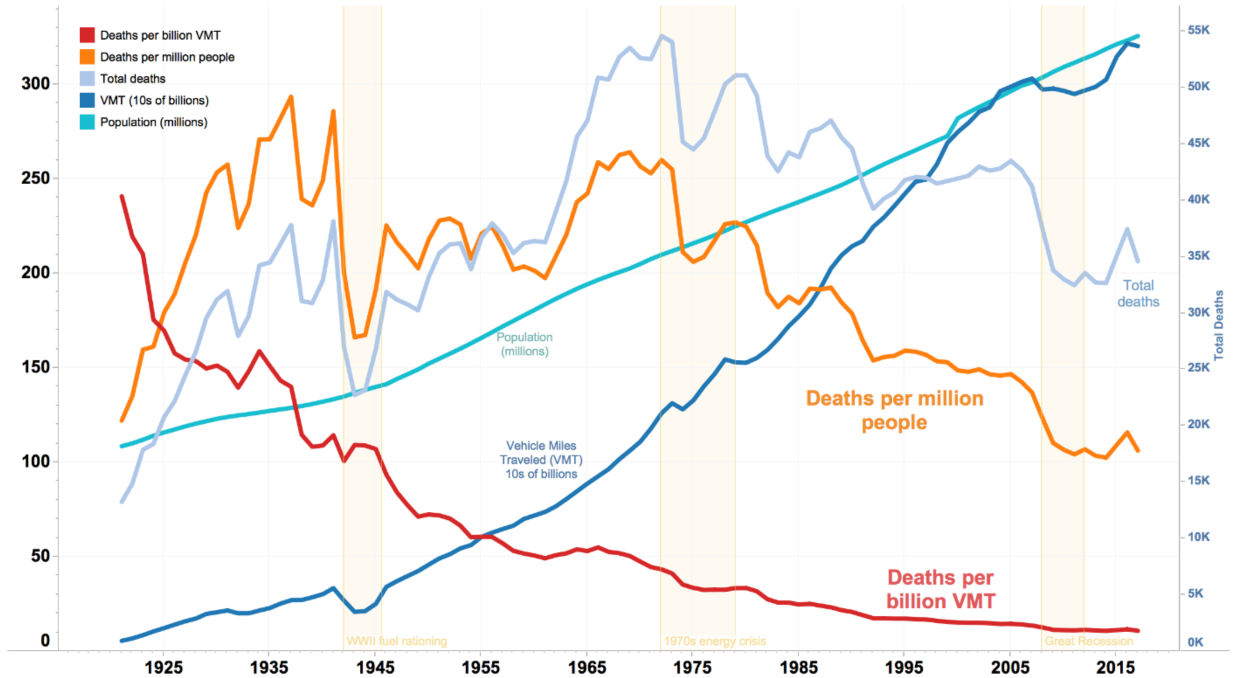


Figure 1. U.S. Motor Vehicle Death per VMT, Death per Capita, Total Death, VMT and Population
This figure was produced by Dennis Bratland and is reproduced here under creative commons license. The figure uses NHTSA FARS and CrashStats data to depict total U.S. motor vehicle deaths, deaths per VMT, deaths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

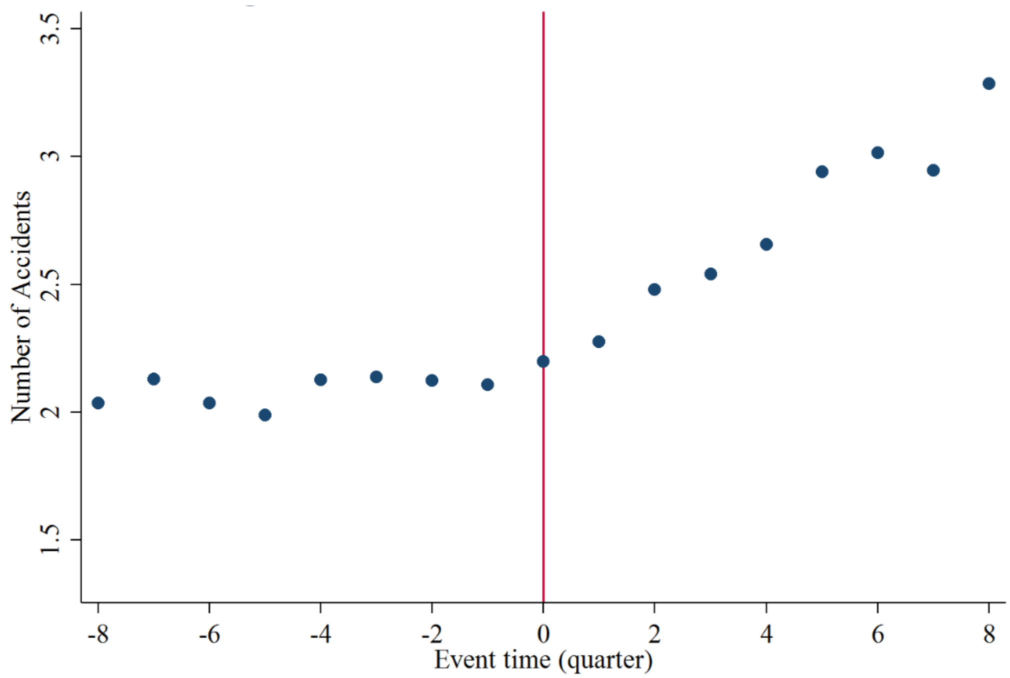


Figure 2. Average Accidents for Treated Cities in Event Time
This figure shows the trend of accidents for treated cities in the eight quarters before and after ridesharing entry. The red vertical line at event time zero indicates the quarter of ridesharing entry.

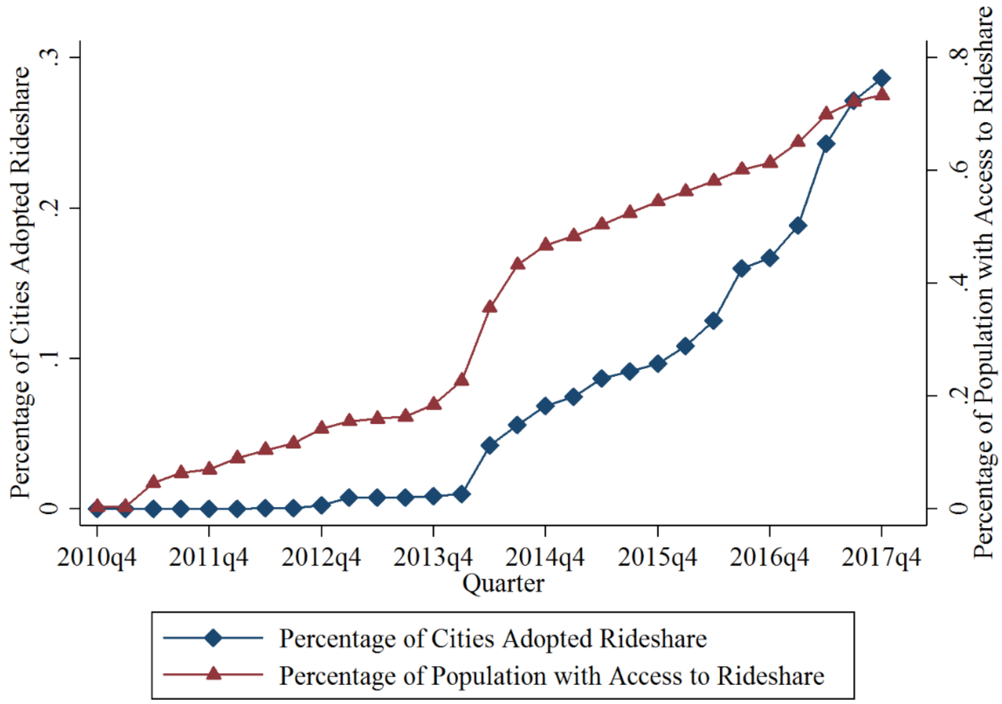


Figure 3. Rideshare Diffusion

This figure shows the diffusion of ridesharing across the U.S. by cities and 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 the fourth quarter of 2017.

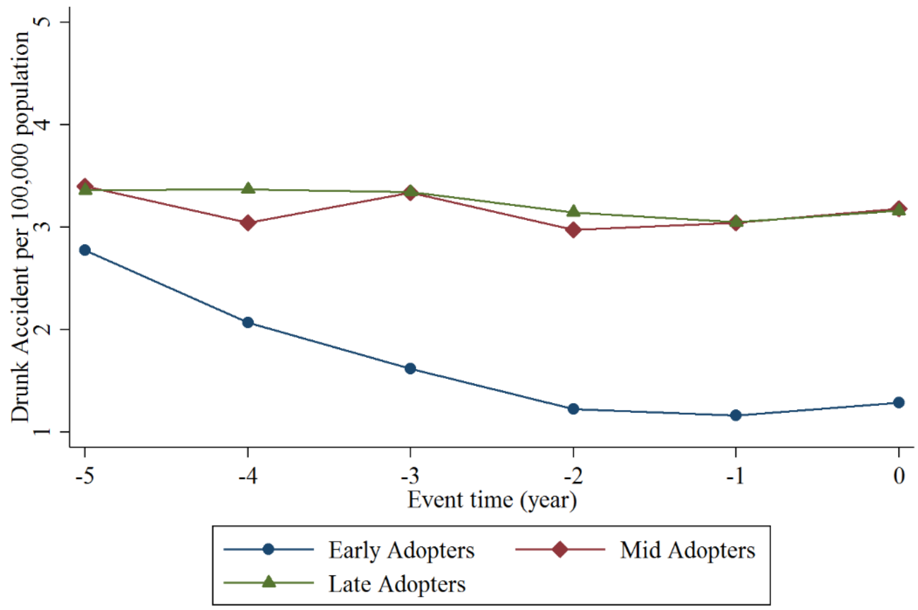


Figure 4. Drunk Accident Rate for Treated Cities Before Rideshare Adoption
This figure shows the trend of drunk accidents per 100,000 population in the five years preceding ridesharing entry. Early adopters are cities that adopted ridesharing in 2010 or 2011, mid-adopters are cities that adopted ridesharing in 2012–2014, and late-adopter cities are cities that adopted ridesharing in 2015–2016.

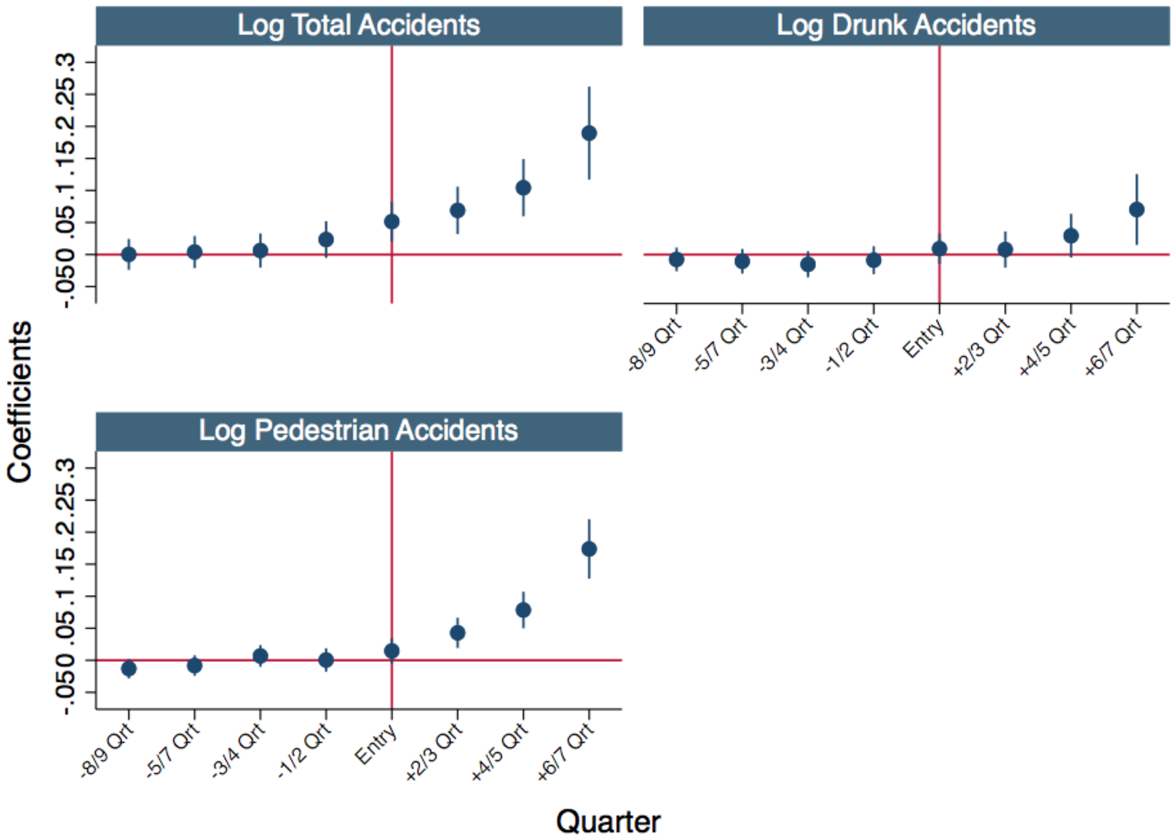
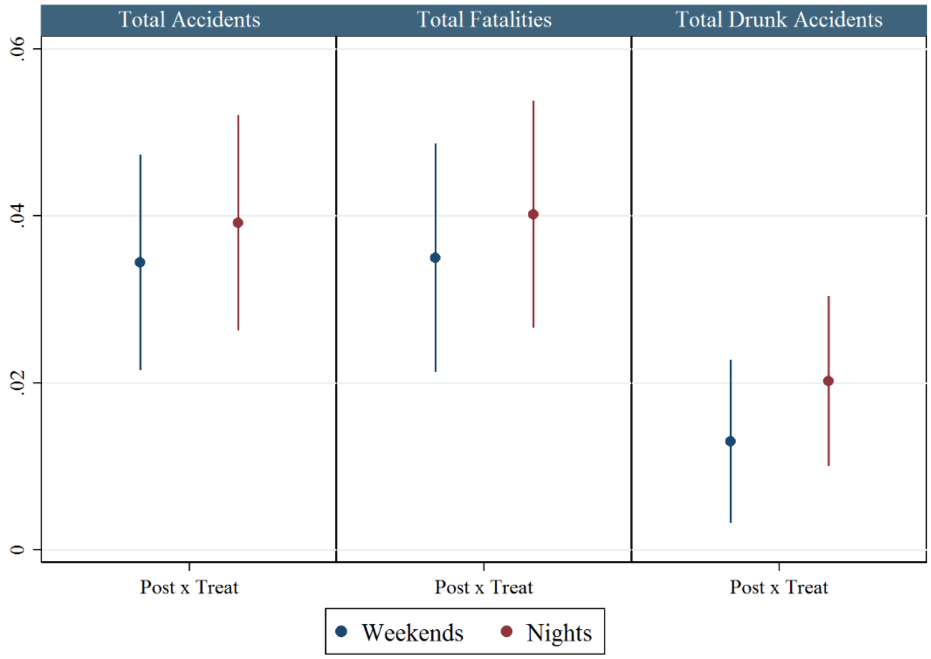


Figure 5. Difference-in-Differences Estimators

This figure displays the regression coefficient estimates and two-tailed 95% 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 two quarters) for the entry of rideshare. We provide a description of the variables in section 2.

Panel A: Weekends and Nights – Accidents, Fatalities, Drunk Accidents



Panel B: Small vs. Large Cities.

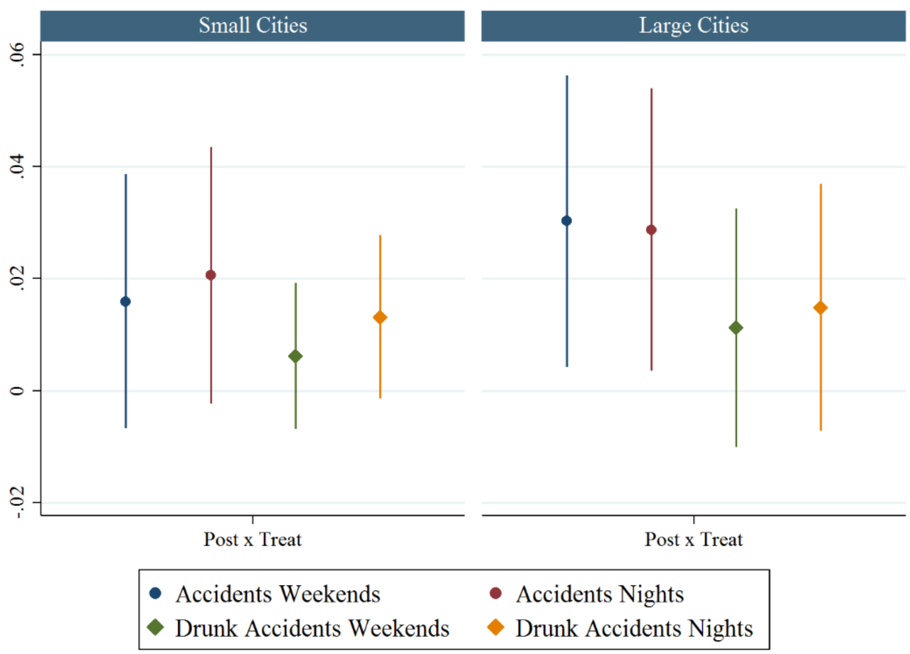
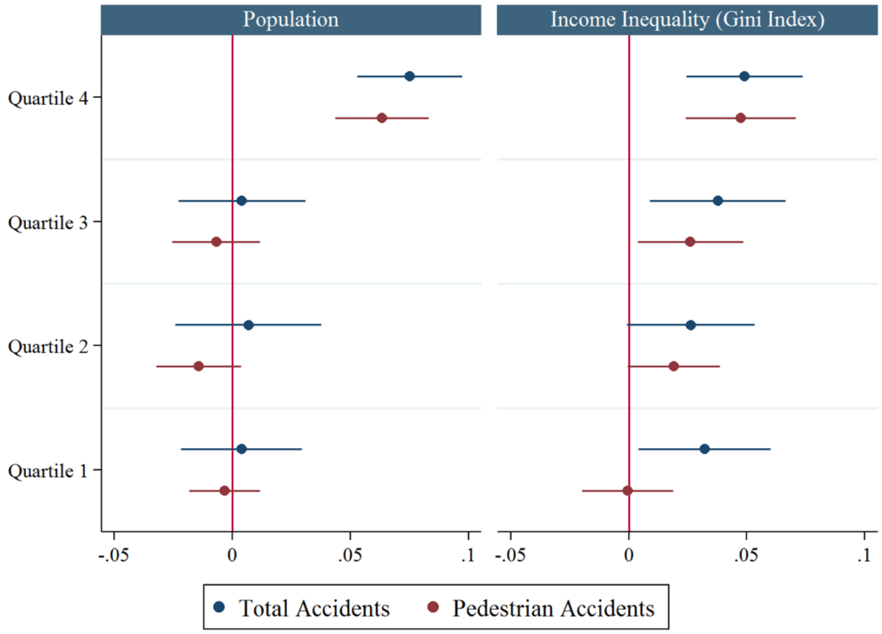


Figure 6. Nights and Weekends
This figure displays the regression coefficient estimates and two-tailed 95% confidence intervals based on standard errors clustered at the city level, broken down by accidents at night and on the weekend. Panel A presents estimates for accidents, fatalities and drunk accidents, while Panel B presents the coefficients for accidents and drunk accidents separately for small and large cities. We provide a description of the variables in Section 2.

Panel A: Heterogeneity by City Population, Income Inequality



Panel B: Heterogeneity by Ex Ante Usage of Public Transport and Carpools

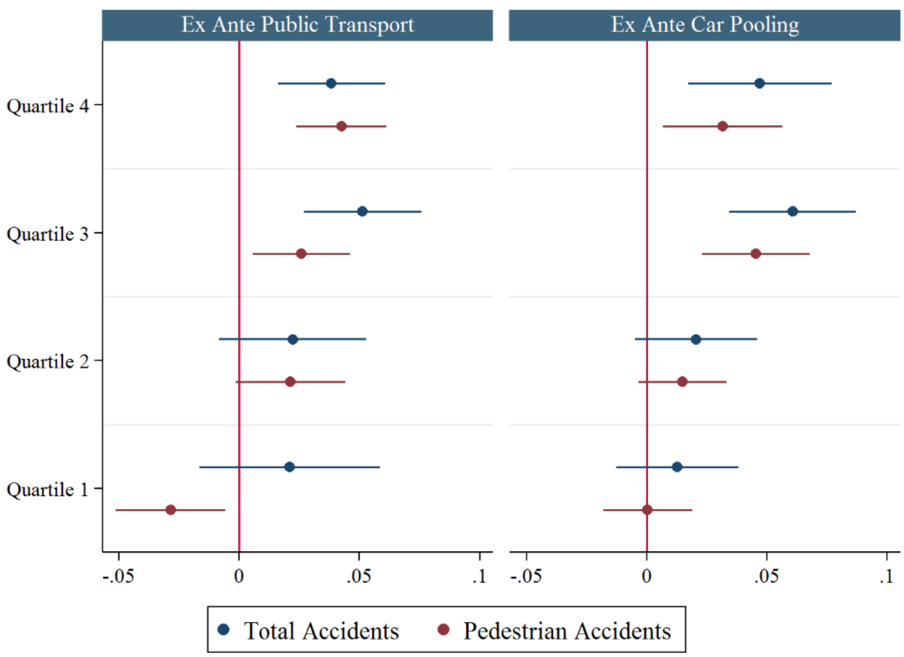


Figure 7. Heterogeneity by City Characteristics

This figure displays the regression coefficient estimates and two-tailed 95% confidence intervals based on standard errors clustered at the city level, broken down by quartiles for four city characteristics: population, income inequality (measured by Gini Index), ex ante usage of public transportation, and ex ante use of carpooling. 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 a 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	5.67	0.00	1.00	99.11	2,955
Fatality rate	3.86	6.52	0.00	1.02	122.05	2,955
Drunk accident rate	1.10	2.72	0.00	0.00	61.72	2,955
Drunk fatality rate	1.23	3.20	0.00	0.00	81.23	2,955
Drunk driver rate	1.21	3.11	0.00	0.00	69.44	2,955
Non-drunk accident rate	2.40	4.43	0.00	0.00	67.46	2,955
Non-drunk fatality rate	2.62	5.08	0.00	0.00	122.05	2,955
Pedestrian-involved accident rate	0.58	1.80	0.00	0.00	37.35	2,955
Pedestrian-involved fatality rate	0.60	1.86	0.00	0.00	38.64	2,955
Pedestrians Involved in Fatal Accidents	0.64	2.11	0.00	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 population. 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. Nondrunk accident is the number of fatal accidents not involving any drunk drivers. Nondrunk fatality is the total number of fatalities in all nondrunk 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 Accidents	(10) Log Non-Drunk Accidents	(11) Log Non-Drunk Fatalities	(12) Log Non-Drunk Fatalities
$Post_t * Treated_c$	0.0148**	0.0367***	0.0137**	0.0367***	-0.0296***	0.0214***	-0.0309***	0.0215***	0.0338***	0.0312***	0.0333***	0.0312***
	(0.0063)	(0.0075)	(0.0066)	(0.0078)	(0.0055)	(0.0060)	(0.0058)	(0.0064)	(0.0063)	(0.0073)	(0.0065)	(0.0076)
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
$Post_t * Treated_c$	0.0123**	0.0344***	0.0110*	0.0285***	0.0230***	0.0392***	0.0113**	0.0247***
	(0.0056)	(0.0066)	(0.0056)	(0.0069)	(0.0057)	(0.0066)	(0.0046)	(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	0.52	0.53	0.51	0.52	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
$Post_t * Treated_c$	0.0110*	0.0350***	0.0123**	0.0344***	0.0215***	0.0402***	0.0107**	0.0265***
	(0.0059)	(0.0070)	(0.0056)	(0.0066)	(0.0060)	(0.0069)	(0.0049)	(0.0058)
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	0.53	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 six 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. Nondrunk accident is the number of fatal accidents not involving any drunk drivers. Nondrunk fatality is the total number of fatalities in all nondrunk accidents. Panels B and C present 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 5 pm through 2 am. Friday and Saturday Night is defined as 5 pm Friday through 6 am Saturday and 5 pm Saturday through 6 am Sunday. 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	0.0344***	0.0342***	0.0124	0.0126	0.0286***	0.0287***
	(0.0100)	(0.0105)	(0.0084)	(0.0090)	(0.0097)	(0.0101)
3 - 4 Quarters	0.0382***	0.0369***	0.0304***	0.0318***	0.0273**	0.0246**
	(0.0113)	(0.0118)	(0.0091)	(0.0097)	(0.0110)	(0.0114)
5 - 6 Quarters	0.0342***	0.0371***	0.0147	0.0157	0.0366***	0.0383***
	(0.0126)	(0.0133)	(0.0094)	(0.0102)	(0.0125)	(0.0131)
7 - 8 Quarters	0.0409***	0.0418***	0.0232**	0.0216*	0.0379***	0.0411***
	(0.0131)	(0.0138)	(0.0106)	(0.0113)	(0.0130)	(0.0136)
9 - 10 Quarters	0.0380**	0.0341**	0.0443***	0.0396***	0.0261*	0.0259
	(0.0156)	(0.0162)	(0.0125)	(0.0132)	(0.0153)	(0.0159)
11 - 12 Quarters	0.0479**	0.0490**	0.0271	0.0286	0.0509**	0.0510**
	(0.0231)	(0.0242)	(0.0203)	(0.0221)	(0.0214)	(0.0221)
> 12 Quarters	0.0847**	0.0836**	0.0552*	0.0566*	0.0834**	0.0812**
	(0.0355)	(0.0364)	(0.0311)	(0.0329)	(0.0337)	(0.0343)
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. Nondrunk accident is the number of fatal accidents not involving any drunk drivers. Nondrunk fatality is the total number of fatalities in all nondrunk 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)	0.0379***	0.0378***	0.0228***	0.0230***	0.0315***	0.0314***
	(0.0076)	(0.0079)	(0.0062)	(0.0066)	(0.0074)	(0.0078)
Pooled Ride Service (Uber Pool, Lyft Line)	-0.0145	-0.0132	-0.0119	-0.0128	-0.0064	-0.0047
	(0.0150)	(0.0158)	(0.0126)	(0.0133)	(0.0147)	(0.0154)
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
$Post_t * Treated_c * Log\ Rideshare - Related\ Google\ Search\ Volume_{ct}$	0.0050***	0.0051***	0.0037***	0.0036***	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. Nondrunk accident is the number of fatal accidents not involving any drunk drivers. Nondrunk fatality is the total number of fatalities in all nondrunk accidents. In Panel A, Single (Pooled) Ride Service is a dummy variable that takes the value of one if any single (pooled) ride service 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
$Post_t * Treated_c$	0.0317***	0.0244***	0.0318***	0.0244***	0.0324***	0.0274***
	(0.0051)	(0.0058)	(0.0051)	(0.0059)	(0.0053)	(0.0062)
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				Income Inequality (Gini Index)				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***	0.0491***	0.0496***	0.0475***	0.0476***	0.0370***	0.0364***	0.0183**	0.0183**
	(0.0115)	(0.0119)	(0.0102)	(0.0103)	(0.0125)	(0.0131)	(0.0118)	(0.0120)	(0.0110)	(0.0113)	(0.0090)	(0.0091)
$Post_t * Treated_c * Q3$	0.0032	0.0032	-0.0065	-0.0071	0.0377**	0.0393**	0.0262**	0.0269**	0.0307**	0.0308**	0.0331***	0.0323***
	(0.0138)	(0.0146)	(0.0095)	(0.0097)	(0.0147)	(0.0153)	(0.0114)	(0.0117)	(0.0139)	(0.0146)	(0.0110)	(0.0112)
$Post_t * Treated_c * Q2$	0.0025	-0.0021	-0.0144	-0.0152*	0.0262*	0.0229	0.0190*	0.0189*	0.0510***	0.0494***	0.0512***	0.0522***
	(0.0159)	(0.0168)	(0.0091)	(0.0092)	(0.0138)	(0.0145)	(0.0100)	(0.0101)	(0.0163)	(0.0170)	(0.0132)	(0.0135)
$Post_t * Treated_c * Q1$	0.0036	0.0009	-0.0031	-0.0025	0.0322**	0.0336**	-0.0005	-0.0011	0.0288*	0.0319*	-0.0062	-0.0058
	(0.0131)	(0.0136)	(0.0077)	(0.0079)	(0.0142)	(0.0148)	(0.0098)	(0.0099)	(0.0166)	(0.0177)	(0.0119)	(0.0121)
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***	0.0805***	0.0663***	0.0667***	0.0383***	0.0382***	0.0425***	0.0420***	0.0470***	0.0469***	0.0316**	0.0318**
	(0.0112)	(0.0116)	(0.0099)	(0.0101)	(0.0113)	(0.0117)	(0.0096)	(0.0097)	(0.0152)	(0.0163)	(0.0127)	(0.0128)
$Post_t * Treated_c * Q3$	0.0000	-0.0019	-0.0086	-0.0092	0.0513***	0.0546***	0.0259**	0.0271***	0.0607***	0.0597***	0.0453***	0.0450***
	(0.0145)	(0.0153)	(0.0107)	(0.0108)	(0.0125)	(0.0131)	(0.0103)	(0.0105)	(0.0135)	(0.0137)	(0.0115)	(0.0116)
$Post_t * Treated_c * Q2$	-0.0025	-0.0038	-0.0098	-0.0100	0.0222	0.0172	0.0213*	0.0196*	0.0207	0.0233*	0.0150	0.0153
	(0.0151)	(0.0161)	(0.0098)	(0.0100)	(0.0157)	(0.0164)	(0.0116)	(0.0119)	(0.0129)	(0.0133)	(0.0094)	(0.0095)
$Post_t * Treated_c * Q1$	0.0141	0.0116	-0.0097	-0.0095	0.0209	0.0222	-0.0285**	-0.0266**	0.0127	0.0111	0.0004	0.0003
	(0.0138)	(0.0143)	(0.0072)	(0.0072)	(0.0191)	(0.0200)	(0.0116)	(0.0120)	(0.0129)	(0.0137)	(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. Panels A and B break out results across a variety of city characteristics and ex-ante behaviors, respectively. The variable used for sample cut is listed at the top of each panel. Population measures annual city population. Income inequality is measured using city Gini index. 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) Log Arterial Street VMT	(2) Log Excess Fuel Consumption	(3) Log Hours of Delay
$Post_t * Treated_u$	0.0165*	0.0172**	0.0172**
	(0.0084)	(0.0073)	(0.0073)
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 on 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
$Post_t * Treated_c$	0.0211**	0.0533***
	(0.0082)	(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		
$Post_t * Treated_c *$ Log Rideshare-Related Google Search Vol_{ct}	0.0084***	
	(0.0009)	
Rideshare Service Type		
Single Ride Service (UberBlack/Taxi/X, Lyft)		0.0506***
		(0.0068)
Pooled Ride Service (UberPool, Lyft Line)		0.0290***
		(0.0105)
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) Income Inequality (Gini Index)	(3) Pop Density	(4) Public Transport	(5) Carpool	(6) Vehicle Ownership
$Post_t * Treated_c * Q4$	0.0882***	0.0017	0.0961***	0.0637***	0.1614***	0.0841***
	(0.0098)	(0.0105)	(0.0118)	(0.0110)	(0.0147)	(0.0094)
$Post_t * Treated_c * Q3$	0.0378***	0.0603***	0.0596***	0.0742***	0.0715***	0.0487***
	(0.0145)	(0.0140)	(0.0113)	(0.0125)	(0.0110)	(0.0134)
$Post_t * Treated_c * Q2$	0.0214	0.0798***	0.0210*	0.0347**	0.0250**	0.0039
	(0.0170)	(0.0132)	(0.0120)	(0.0144)	(0.0111)	(0.0197)
$Post_t * Treated_c * Q1$	0.0079	0.0789***	-0.0219	0.0026	-0.0575***	0.0266*
	(0.0149)	(0.0161)	(0.0189)	(0.0199)	(0.0128)	(0.0157)
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	0.97	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 service is adopted. Panel C breaks out results across a variety of city characteristics and ex-ante behaviors. The variable used for sample cut is listed at the top of each column. Population measures annual city population. Income inequality is measured using city Gini index. Population density measures population per square mile. Pop Density measures the population per square mile. Public Transport measures the percentage of the population commuting to work using public transportation. Carpool measures the percentage of the population commuting to work using carpools. 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.

Appendix

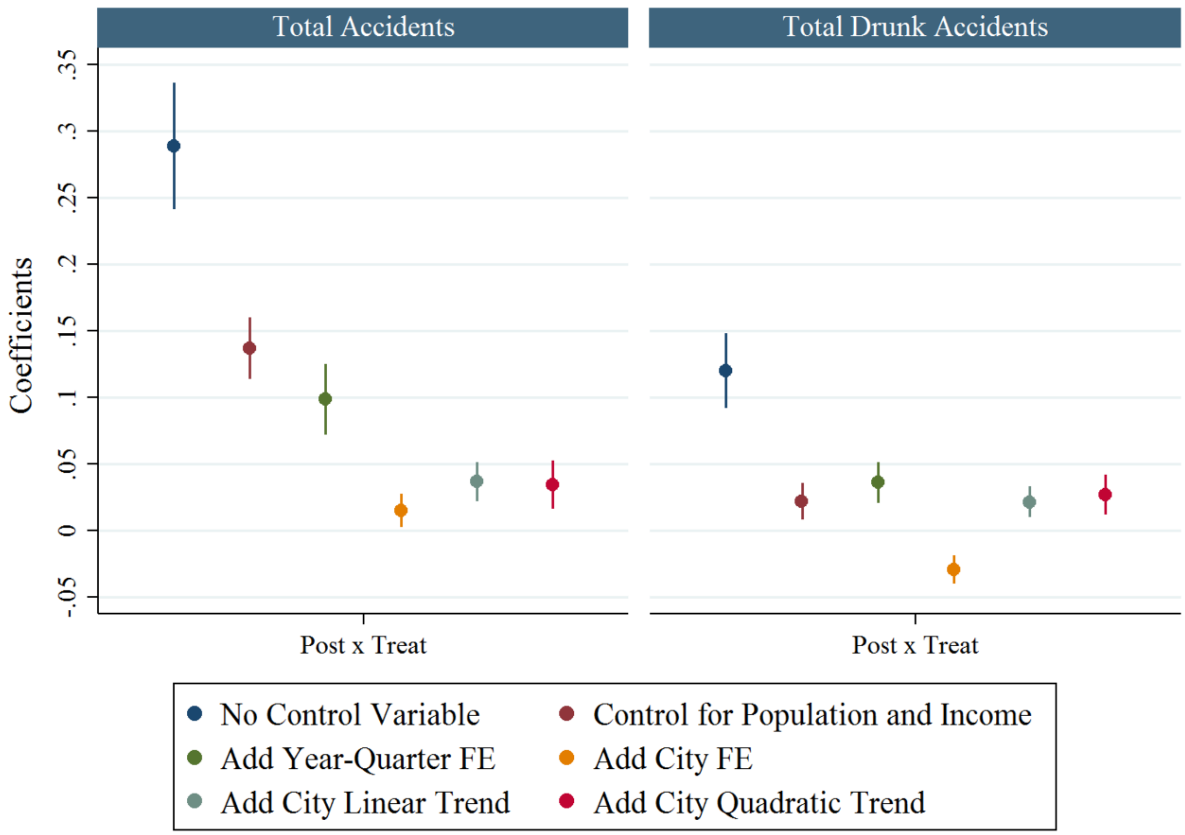


Figure A1. Effect of Ridesharing on Traffic Safety in Different Model Specifications

This figure shows the regression coefficient estimates and two-tailed 95% confidence intervals based on standard errors clustered at the city level. To demonstrate how the point estimates and standard errors change across different model specifications, we incrementally add control variables, fixed effects and city-specific linear and quadratic time trends into the difference-in-difference regression specification. The outcome variables are displayed at the top of each graph. We provide a description of the variables in Section 2.

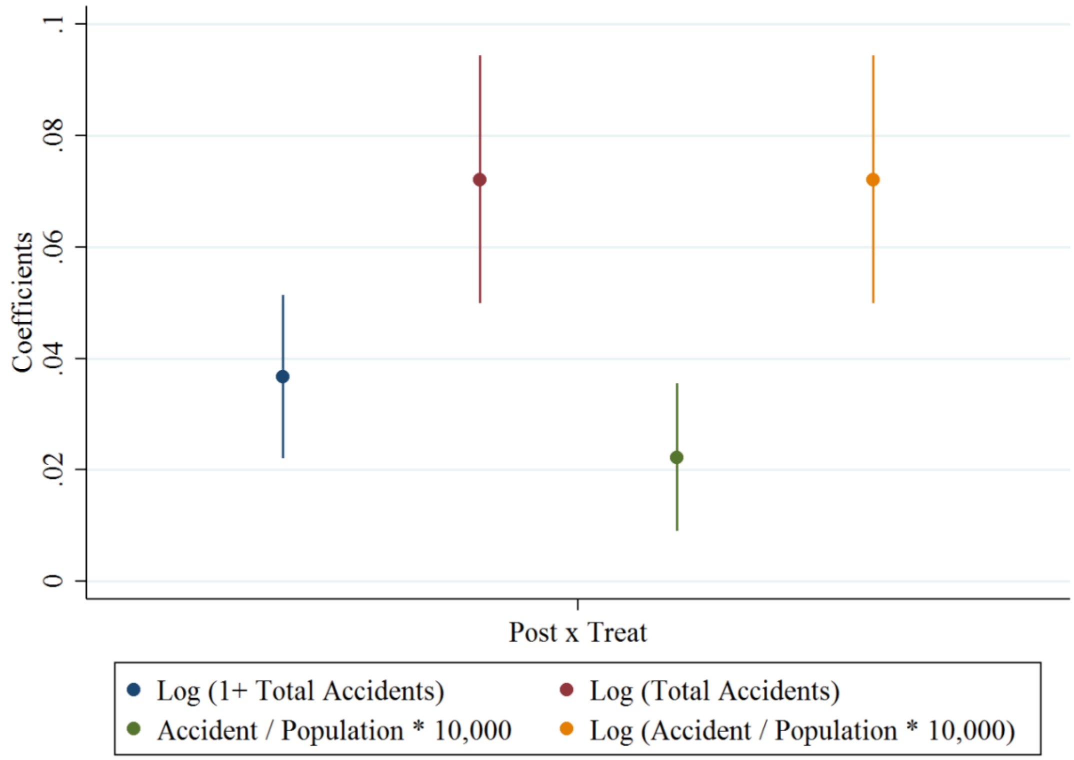


Figure A2. Robustness to Variations in Outcome Measures

This figure illustrates the variation in the regression coefficient estimates across different measures of fatal accidents.. The regression coefficient estimates and two-tailed 95% confidence intervals are displayed in the figure. The outcome measures for each regression are shown in the legend at the bottom of the figure. Standard errors are clustered at the city level. We provide a description of the variables in Section 2.

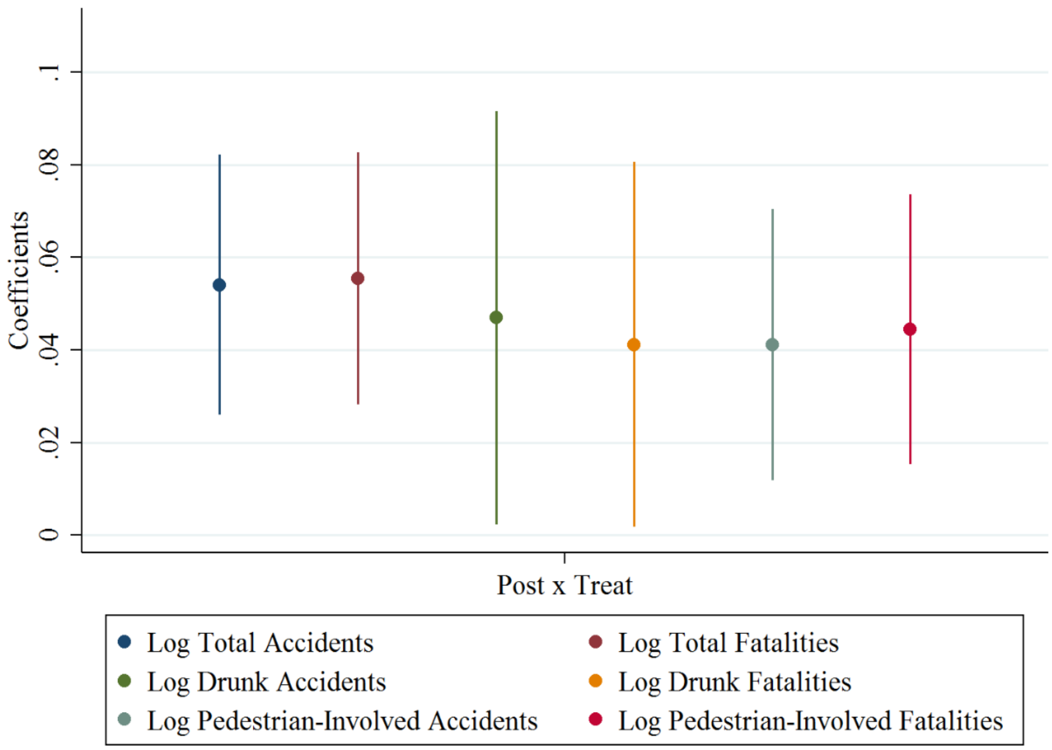


Figure A3. Population Weighted Regression Results

This figure displays coefficient estimates in regressions weighted by city population. Two-tailed 95% confidence intervals based on standard errors clustered at the city level are displayed in the figure. The outcome measures for each regression are shown in the legend at the bottom of the figure. We provide a description of the variables in Section 2.

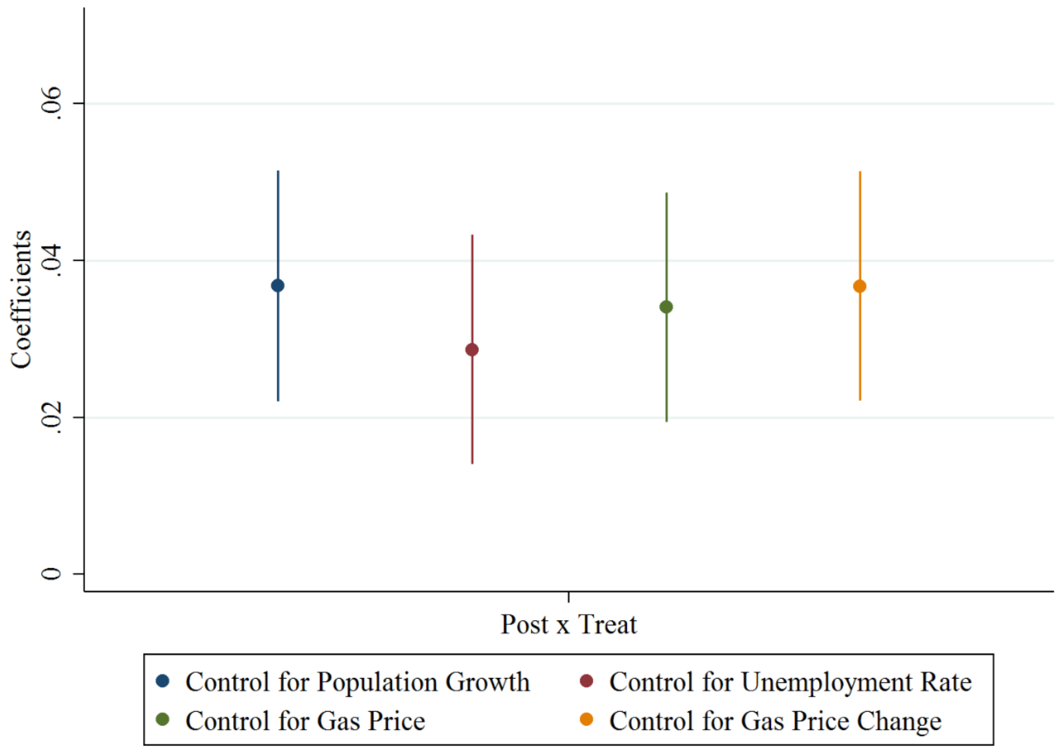


Figure A4. Robustness to Additional Control Variables

This figure illustrates how the regression coefficients vary when we include additional control variables. The outcome measures for all regressions are Log Total Accidents. Two-tailed 95% confidence intervals based on standard errors clustered at the city level are displayed in the figure. We provide a description of the variables in Section 2.

Table A1 Effect of Ridesharing on Traffic Safety in Different Model Specifications

Panel A: Total Accidents

	(1) Log Total Accidents	(2) Log Total Accidents	(3) Log Total Accidents	(4) Log Total Accidents	(5) Log Total Accidents	(6) Log Total Accidents	(7) Log Total Accidents
$Post_t * Treated_c$	0.2887***	0.1369***	0.0402***	0.0984***	0.0148**	0.0367***	0.0344***
	(0.0243)	(0.0118)	(0.0062)	(0.0135)	(0.0063)	(0.0075)	(0.0092)
Control Variables	No	Yes	Yes	Yes	Yes	Yes	Yes
City FE	No	No	Yes	No	Yes	Yes	Yes
Year-Quarter FE	No	No	No	Yes	Yes	Yes	Yes
City Linear Trend	No	No	No	No	No	Yes	Yes
City Quadratic Trend	No	No	No	No	No	No	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.009	0.338	0.603	0.343	0.608	0.617	0.624

Panel B: Drunk Accidents

	(1) Log Drunk Accidents	(2) Log Drunk Accidents	(3) Log Drunk Accidents	(4) Log Drunk Accidents	(5) Log Drunk Accidents	(6) Log Drunk Accidents	(7) Log Drunk Accidents
$Post_t * Treated_c$	0.1202***	0.0217***	-0.0281***	0.0359***	-0.0296***	0.0214***	0.0269***
	(0.0144)	(0.0071)	(0.0052)	(0.0077)	(0.0055)	(0.0060)	(0.0076)
Control Variables	No	Yes	Yes	Yes	Yes	Yes	Yes
City FE	No	No	Yes	No	Yes	Yes	Yes
Year-Quarter FE	No	No	No	Yes	Yes	Yes	Yes
City Linear Trend	No	No	No	No	No	Yes	Yes
City Quadratic Trend	No	No	No	No	No	No	Yes
Observations	189,120	189,120	189,120	189,120	189,120	189,120	189,120
R2	0.004	0.245	0.468	0.249	0.473	0.488	0.497

Notes: This table illustrates the changes in the generalized difference-in-difference regression coefficient estimates when using different model specifications. The dependent variables are in Panel A are the natural logarithm of total fatal accidents, and in Panel B are the natural logarithm of fatal dunk accidents. We provide a detailed description of these variables in section 2. $Post_t * Treated_c$ is a dummy variable that equals one if city c adopted at least one rideshare service at time t. Control variables 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 A2 Robustness to Variations in Outcome Measures

	(1) Log (1+ Total Accidents)	(2) Log (Total Accidents)	(3) Accident / Population * 10,000	(4) Log (Accident / Population * 10,000)
$Post_t * Treated_c$	0.0367***	0.0721***	0.0223***	0.0721***
	(0.0075)	(0.0113)	(0.0068)	(0.0113)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes
Observations	189,120	97,577	189,120	97,577
R2	0.62	0.65	0.40	0.73

Notes: This table illustrates the variation in the regression coefficient estimates across different measures of fatal accidents. The dependent variables are 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. Control variables 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 A3 Population Weighted Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Total Accidents	Log Total Fatalities	Log Drunk Accidents	Log Drunk Fatalities	Log Pedestrian-Involved Accident	Log Pedestrian-Involved Fatalities
$Post_t * Treated_c$	0.0541***	0.0554***	0.0469**	0.0411**	0.0412***	0.0444***
	(0.0143)	(0.0139)	(0.0227)	(0.0201)	(0.0149)	(0.0148)
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	0.92	0.91	0.85	0.84	0.91	0.90

Notes: This table presents results from generalized difference-in-difference regressions weighted by city population. The dependent variables are the natural logarithm of six 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. 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. 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. 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 A4 Robustness to Additional Control Variables

	(1) Log Total Accidents	(2) Log Total Accidents	(3) Log Total Accidents	(4) Log Total Accidents
$Post_t * Treated_c$	0.0368***	0.0287***	0.0341***	0.0367***
	(0.0075)	(0.0075)	(0.0075)	(0.0075)
Population Growth Rate	0.1153			
	(0.1051)			
Unemployment Rate		-0.0140***		
		(0.0013)		
Retail Gas Price			0.1176***	
			(0.0202)	
Retail Gas Price Change (%)				0.0012***
				(0.0004)
City and Quarter Fixed Effects	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes
Control Variables	Yes	Yes	Yes	Yes
Observations	186,165	189,105	189,120	189,120
R2	0.62	0.62	0.62	0.62

Notes: This table presents results from generalized difference-in-difference regressions when controlling for various additional control variables. $Post_t * Treated_c$ is a dummy variable that equals one if city c adopted at least one rideshare service at time t. Population growth rate is the annual percentage growth in city population. Unemployment rate is the quarterly average county unemployment rate. Retail Gas Price is average quarterly retail gasoline price (dollars per gallon). Retail Gas Price Change is the quarterly percentage change in retail gasoline price. 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 A5 Multinomial Logit Estimation of Rideshare Entry Decisions

		(1) Adoption Timeliness	(2) Adoption Timeliness
5-Year Accidents Trend	0	-2.0299*	
		(1.0918)	
5-Year Drunk Accidents Trend			-4.4109***
			(1.6628)
	1	Base Outcome	
5-Year Accidents Trend	2	1.4001**	
		(0.6558)	
5-Year Drunk Accidents Trend			2.5893**
			(1.1344)
Control Variables		Yes	Yes
Observations		1,190	1,190

Notes: In this table we estimate multinomial logit models of the relationship between accident trends and ridesharing entry. The outcome variable, Adoption Timeliness, is defined as 0 if the city adopted ridesharing in 2010 through 2012, 1 if the city adopted ridesharing in 2013 and 2014 (the base outcome), and 2 if the city adopted ridesharing in 2015 or 2016. 5-Year Accidents (Drunk Accidents) Trend is the 5-year average quarterly change in accidents (drunk accidents) before ridesharing entry. We control for the natural logarithm of population and per capita income. Standard errors are reported in parentheses. $***$, $**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.