Do Uber and Lyft Reduce Drunk-Driving Fatalities?*

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This paper investigates whether Uber and Lyft lead to reductions in drunk driving, as measured by city-level drunk-driver-related motor vehicle fatalities and fatal crashes. I use a difference-in-differences method that exploits the variation in the timing of Uber and Lyft entry for the 100 most populous U.S. cities and a Poisson model to account for the fact that crashes and fatalities are count data. Using monthly city-level Fatality Analysis Reporting System (FARS) data for 2006 to 2016, I find small declines in drunk-driver-related fatal motor vehicle incidents and small increases in overall fatal motor vehicle incidents, but I cannot reject the null hypothesis of no effect of Uber or Lyft on these outcomes. Event studies suggest that drunk-driver-related and overall fatal motor vehicle incidents decline several years after the entry of Uber or Lyft into a city.

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1 Introduction

Approximately 30% of motor vehicle fatalities in the U.S. involved a drunk driver between 2006 and 2016 (National Highway Traffic Safety Administration, 2017).[1] In 2016 alone, 10,497 people died from a motor vehicle crash involving a drunk driver (National Highway Traffic Safety Administration, 2017). In addition, motor vehicle fatalities are a leading cause of death for young people (Centers for Disease Control and Prevention, 2018). Among 15 to 24 year-olds in 2016, unintentional motor vehicle fatalities were the leading cause of death.[2] For 25 to 34 year-olds in 2016, unintentional motor vehicle fatalities were the third leading cause of death, behind unintentional poisoning and suicide (Centers for Disease Control and Prevention, 2018). Moreover, in 2016 over 1 million arrests were made for Driving Under the Influence (DUI) (Department of Justice, 2017). Drunk-driving crashes also generate an estimated cost of $44 billion per year (National Highway Traffic Safety Administration, 2017).

Drunk driving poses a significant negative externality in several ways. When a drunk driver crashes, there may be externalities in the form of deaths or injuries of other passengers in the drunk driver's car, or pedestrians, cyclists, and occupants of any other vehicles involved in the crash. Drunk driving also generates externalities from expenditures due to auto or health insurance claims (increases in costs are partially borne by higher premiums for everybody) and from increased expenditures on public safety (e.g. DUI enforcement). Negative externalities such as these have traditionally been an economic rationale for gov-

[1] A drunk driver is defined as someone with a blood alcohol concentration greater than or equal to 0.08 g/dL, which was the legal limit for driving under the influence (DUI) in all 50 states and Washington, D.C. during this time period.

[2] The next four leading causes of death were suicide, homicide, unintentional poisoning (includes drug and alcohol overdoses), and malignant neoplasms (cancer).

ernment intervention. But what if the "free market" could reduce the size of this negative externality? Uber often makes this claim. In a post on their website, Uber claims that as Uber use in Pennsylvania increases, DUI rates fall (Uber, 2014).

As part of their supporting evidence for the claim that Uber is associated with a reduction in drunk driving, they provide a graph of Saturday night ride requests in Pittsburgh by time of day (see Figure 1). The graph does show a spike in requests around the time the bars close; however, that's not conclusive evidence that consumers are substituting toward Uber (or Lyft) and away from drunk driving. These ride requests could be coming from individuals who would have taken a taxi, walked, rode the bus, or bicycled, as opposed to driving drunk.

This paper answers the following question: have the introduction of ridesharing services such as Uber and Lyft led to a reduction in drunk driving, as measured by city-level drunk-driver-related motor vehicle fatalities and fatal crashes? I also examine whether ridesharing services affect overall crashes and fatalities, because if Uber and Lyft create more cars on the road then any effect on drunk driving could potentially be offset by heavier congestion.

Figure 2 illustrates a simplified, hypothetical market for drunk driving, with a downward-sloping "perceived marginal benefit of drunk driving" curve, an upward sloping "perceived marginal private cost of drunk driving" curve, and a much higher upward sloping "marginal social cost of drunk driving" curve. I have drawn the marginal cost curves as upward sloping to denote the fact that when there are more drunk drivers on the road, the roads become more dangerous (hence there is a higher marginal cost). The initial equilibrium quantity of drunk driving is represented by Q_0, while the initial socially efficient quantity of drunk driving is much lower and represented by Q_{soc}.[3] The entry of Uber or Lyft into a city

[3]Note: this illustration is stylized, and it is entirely possible that the actual marginal social cost of drunk

represents a reduction in the price of a substitute for drunk driving. Under a comparative statics analysis, the entry of Uber or Lyft into a city will shift the "perceived marginal benefit of drunk driving" curve inward, leading to a new (partial) equilibrium quantity of drunk driving, denoted $Q_{post-Uber/Lyft}$.[4]

But what are the general equilibrium effects of Uber and Lyft? While these services represent a reduction in the price of a substitute to drunk driving, they also represent a reduction in the cost of drinking. There is evidence that Uber has led to increases in alcohol consumption (Teltser, Lennon, and Burgdorf, 2021; Zhou, 2020).[5] There may also be more cars on the road post-Uber and Lyft entry. The first effect may lead to an increase in the quantity of drunk driving. The second effect may lead to an increase in the quantity of drunk-driver-related motor vehicle crashes, as more cars on the road, ceteris paribus, means more cars for a drunk driver to potentially crash into. These offsetting potential effects make the impact of Uber and Lyft on drunk driving theoretically ambiguous and therefore an empirical question. A secondary economic motivation is that Uber and Lyft represent an unusual case of how health can be affected by firm entry and innovation.

Uber and Lyft are ridesharing services that operate through smartphone apps. Riders open the app, select their pickup location on a map, and request a ride. The driver transports the rider to the rider's destination. The apps require a credit card on file, and the app

driving is so high that the socially efficient quantity of drunk driving equals 0. The precise socially efficient quantity of drunk driving is beyond the scope of this paper.

[4] In this partial equilibrium scenario, the entry of Uber/Lyft does not affect the perceived marginal cost of drunk driving. Uber and Lyft may have a general equilibrium effect on the perceived marginal cost, which is described in more detail below.

[5] Thinking of the general equilibrium version of Figure 2, increased alcohol consumption could lower the perceived marginal cost of drunk driving, which may lead to more drunk driving. This outcome could arise if people drive to the bar (sober), drink more because they can rely on Uber/Lyft to transport them home, but then drunkenly decide that they are capable of driving themselves.

automatically charges the rider's credit card at the end of the ride. The main differences between Uber or Lyft and a taxi is that riders can request a ride through an easy-to-use app on their phone (they don't have to call a cab company or stand on a street corner), they can track the driver's realtime progress to the pickup location through the app, and payment occurs automatically, which means riders do not have to carry cash. In other words, Uber and Lyft reduce the time cost and increase the convenience of transportation.

Uber was founded in 2009, and in July 2010, it launched in San Francisco. The initial service only had black cars (known today as UberBlack), which are more expensive than taxis. In 2011 Uber expanded to New York City. In June 2012, Lyft started in San Francisco. Lyft typically enters cities after Uber, although it did launch before Uber in a few cities. In July 2012, Uber launched UberX, a cheaper version of Uber. UberX, when not in surge-pricing mode, is usually cheaper than a taxi. By early 2014, Uber had expanded to 50 of the 100 largest U.S. cities, and by late 2015, it had expanded to all but 2 of the 100 largest U.S. cities (Uber and Lyft, 2017).

Taking advantage of the staggered rollout of these ridesharing programs across cities, this paper uses a difference-in-differences approach to test whether there were reductions in drunk-driver-related motor vehicle fatalities and fatal crashes after the introduction of Uber and Lyft into a city. I use city-level motor vehicle fatality data for 2006 to 2016 from the Fatality Analysis Reporting System (FARS). Figure 3 shows the variation in Uber and Lyft entry across the 100 most populous U.S. cities over my sample period (2006 to 2016). There are two important features of the histogram to note. First, there is variation in the timing of Uber and Lyft entry. Second, there does not appear to be seasonality in the timing of Uber and Lyft entry (drunk driving fatalities and fatal crashes do exhibit some seasonality).

One source of endogeneity would be if Uber and Lyft timed their entry into a city with peak drunk driving incidents. I test for parallel pre-trends with event studies to address whether this particular source of endogeneity is likely to be a concern.

This paper contributes to the literature on the effect of ridesharing on drunk driving. The most closely related paper estimates the impact of ridesharing on drunk driving for all U.S. cities with a population of at least 100,000 (Martin-Buck, 2017). He finds that for the period 2000-2014, ridesharing leads to reductions in drunk-driving-related crashes. Brazil and Kirk (2016) use a difference-in-differences method on county-level Fatality Analysis Reporting System (FARS) data for the counties containing the 100 largest metropolitan areas, and they do not reject the null hypothesis of no effect on motor vehicle fatalities. Dills and Mulholland (2018) use a difference-in-differences method on county-level FARS data for all U.S. counties, and they find that the decline in motor vehicle fatalities and fatal crashes becomes larger the longer Uber has been in a county. Greenwood and Wattal (2017) study the arrival of UberX in California and find that it leads to a 3.6% to 5.6% decline in motor vehicle fatalities per quarter. Peck (2017) finds a 25-35% reduction in the alcohol-related crash rate in New York City. In contrast, Barrios, Hochberg, and Yi (2020) find a 3% increase in overall traffic fatalities. In Brazil, Barreto, Neto, and Carazza (forthcoming) find that Uber leads to a 10% reduction in traffic fatalities.

Another strand of the literature has examined the effects of other drunk driving substitutes on measures of drunk driving. Chung, Joo, and Moon (2014) examine the impact of designated driver services in South Korea, and they find that an increase in the number of companies is associated with a reduction in alcohol-involved and overall traffic fatalities in 4 metropolitan areas and 8 provinces (Chung, Joo, and Moon, 2014). Jackson and Owens

(2011) exploit the D.C. metro's late-night service expansions to examine the effect of public transportation on drunk driving. They find that the later operating hours of the metro reduced the probability of a DUI arrest in neighborhoods with bars near a Metro station, but that there was no effect on the probability of being arrested for DUI over all neighborhoods (Jackson and Owens, 2011).

This paper contributes to the existing literature in several ways. First, by restricting my sample to the 100 most populous U.S. cities, 98 of which have Uber or Lyft by the end of my sample period, I rely almost exclusively on the variation in the timing rather than whether entered. Cities that have never had Uber or Lyft might not be good controls: they are smaller, less population dense, and more rural than treated cities. Second, I examine city-level outcomes, which is arguably a more accurate measure of the treatment effect than county-level outcomes, because Uber and Lyft entry happens at the city level. Third, compared to most of the other papers on ridesharing in the U.S., (Brazil and Kirk, 2016; Dills and Mulholland, 2018; Martin-Buck, 2017) I use at least one additional year of data in the post-period. Finally, I contribute to the broader literature on determinants of drunk driving (Carpenter, Dobkin, and Warman, 2016; Lovenheim and Steefel, 2011; Dee, 1999; Eisenberg, 2003; Freeman, 2007; Hansen, 2015; Kenkel and Koch, 2001).

I find that the presence of Uber or Lyft in a city has mixed effects on motor vehicle fatalities and fatal crashes. Event study specifications provide suggestive evidence of longer-term effects of Uber and Lyft on fatal incidents, particularly for drunk-driver-related incidents (Figures 5 through 8). However, using the standard difference-in-differences method, I cannot reject the null hypothesis of no effect of Uber or Lyft on either drunk-driver-related or all fatal motor vehicle incidents.

The remainder of the paper proceeds as follows: Section 2 outlines the conceptual framework and the method I use, Section 3 describes the data, Section 4 presents the results of the difference-in-differences estimation and event studies, Section 5 incorporates robustness checks and alternative specifications, and Section 6 concludes.

2 Model, Identification & Methods

2.1 Model of Individual's Decision to Drive After Drinking

An individual's decision to drive drunk can be modeled with the following equation:

$$Prob(DD) = f(P_{DD}, P_{complements}, \mathbf{P_{substitutes}}, alc) \tag{1}$$

$Prob(DD)$ represents the probability an individual drives drunk. P_{DD} represents the implicit price of drunk driving, which includes the perceived risks of being arrested and crashing. $P_{complements}$ represents the implicit price of complements (e.g. alcohol). $\mathbf{P_{substitutes}}$ represents the implicit price of substitutes (e.g., walking, bicycling, taking public transit, hailing a taxi, **or using Uber or Lyft**). alc represents alcohol consumption, which could affect one's perception of one's cognitive and motor skills (e.g., perceived ability to drive safely). If the individual has already decided to drive, increasing alc increases the probability of driving drunk. Risk aversion affects P_{DD} by affecting the perceived risks of being arrested and crashing. The relative prices of drunk driving and its substitutes are also affected by the distance one has to travel.

I am unable to directly observe the probability that an individual drives drunk, but I do observe a measure of drunk-driving-related fatalities and fatal crashes. Drunk-driving fatalities can be modeled with the following equation:

$$DD\ fatalities = f(miles\ DD, \frac{fatal\ crash\ rate}{mile}, \frac{fatalities}{fatal\ crash}) \tag{2}$$

$DD\ fatalities$ represent drunk driving fatalities. $miles\ DD$ represent miles driven drunk. Increasing $\mathbf{Prob(DD)}$ leads to an increase in $\mathbb{E}[\mathbf{miles\ DD}]$. I am able to observe the left-hand side of equation 2 as well as $\frac{fatalities}{fatal\ crash}$.

I estimate a reduced-form equation of equation 2:

$$DD\ F = f(ridesharing,\ city\ characteristics,\ city\ +\ time\ FE) \tag{3}$$

$city\ characteristics$ represents population characteristics and the unemployment rate. $city + time\ FE$ represent city and time fixed effects. I have not included alcohol consumption because alcohol consumption is an intermediate outcome (Uber and Lyft lead to increased alcohol consumption, which may lead to increases in drunk driving), and Uber or Lyft's effect on alcohol consumption could be a mechanism for how they affect drunk driving.[6]

2.2 Difference-in-Differences Identification and Assumptions

I estimate a difference-in-differences model in which an indicator for the presence of Uber or Lyft is my treatment variable and motor vehicle fatalities and fatal crashes are my outcome variables. Identification rests on two assumptions:

[6]Including intermediate outcomes in a regression can lead to collider bias, which would provide an inaccurate estimate of the effect of Uber and Lyft on fatal motor vehicle incidents.

1. *Parallel trends:* in the absence of Uber and Lyft, trends in motor vehicle fatalities and fatal crashes would be the same across treated and untreated cities

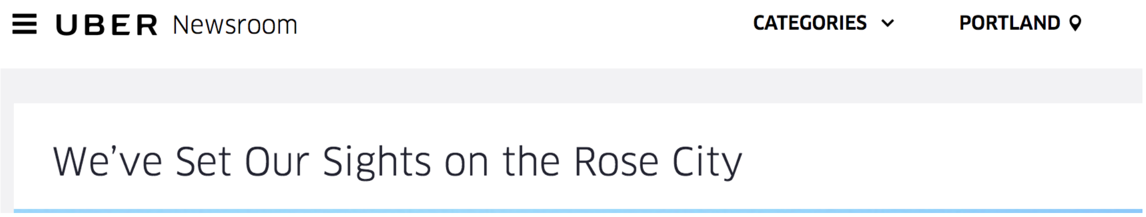
2. There are no other concurrent changes at the time of Lyft or Uber's entry into the treated cities that affect motor vehicle fatalities

As with all difference-in-differences studies, the greatest threat to identification is policy endogeneity. If Uber and Lyft are not entering cities randomly, and are in fact systematically targeting cities where drunk driving is increasing at faster rates than other cities, then the difference-in-differences model's results would be biased. However, if the pre-implementation trends in the outcome variables are parallel relative to cities without Lyft or Uber, then this type of policy endogeneity may not be not an issue.

According to an employee at Lyft, the decision to enter a given city was primarily influenced by the population density and response to competition from Uber (Gigante, phone interview, October 10, 2017). In some cities, Lyft decided to enter the market because the city explicitly welcomed ridesharing companies. In Indianapolis, the mayor's office and the chief of police were concerned with drunk driving and viewed ridesharing companies as a solution for reducing drunk driving.[7]

However, cities were not always in favor of Uber and Lyft, and in some cases, they banned them outright or created restrictions to delay their arrival. In these cities, Uber and Lyft wanted to operate months or years before they were legally allowed to do so. Portland, Oregon is one such example.

[7]If such a scenario were happening systematically, that would be concerning from a policy endogeneity standpoint.



https://newsroom.uber.com/us-oregon/hello_portland/

In July 2013, Uber *wanted* to operate in Portland but was barred due to regulations. 21 months later, in April 2015, Uber was legally able to begin operating after the regulations were revised. A similar situation arose in upstate New York: it was not until April 2017 that New York State passed a budget allowing Uber and Lyft to operate in upstate New York (Kim, 2017).

The fact that there are several cities where Uber and Lyft wanted to operate but were delayed while they worked with city officials or regulatory agencies introduces an element of randomness into the timing of their arrival. Even though the policy might be endogenous in some cities, in the aggregate, the timing of Uber and Lyft entry may not be correlated with trends in fatal drunk-driving incidents.

To test for the presence of policy endogeneity (and to test for dynamic treatment effects), I conduct event studies to test the validity of the parallel pre-trends assumption. Figures 5 through 8 shows the results of the event study specifications and are described in detail in Section 4.1.

2.3 Reduced-Form Drunk Driving Equation

The difference-in-differences model is a Poisson model estimated with control variables and city and month-year fixed effects. The main specification is equation 4.

$$\mathbb{E}[F_{it} \mid Ride,\ \mathbf{X}] = exp\{\alpha + \beta \cdot Ride_{it} + \mathbf{X}'_{it} \cdot \gamma + \eta_i + \delta_t\} \tag{4}$$

F_{it} represents the count of monthly city-level motor-vehicle fatalities or fatal crashes. $Ride_{it}$ represents a monthly city-level indicator for the presence of Uber or Lyft. $\mathbf{X}'_{it}$ represents a vector of characteristics that change over time. These include the monthly city-level unemployment rate as well as annual county-level demographic characteristics: the percent of the population that is African-American, Native American, Asian, or Hispanic, male, male aged 20 to 24, aged 20 to 24, 25 to 34, 35 to 54, and 55 and older. η_i represents city fixed effects. δ_t represents time fixed effects. The standard errors are clustered at the city level. I weight all regressions using the 2010 Census city population, so that the results are interpretable as the effect of Uber or Lyft on the average person, as opposed to the effect on the average city.

3 Data

3.1 Outcome Variables: Fatal Motor Vehicle Incidents

The outcome variables come from the National Highway Traffic Safety Administration (NHTSA) Fatality Analysis Reporting System (FARS) data. FARS contains the universe of motor vehicle crashes on public roadways in the United States (50 states and Washington, D.C.) that result in a fatality within 30 days of the crash (National Center for Statistics and Analysis, 2021). State governments send the crash data to the NHTSA each year, and NHTSA analysts aggregate and clean the data. FARS is the only source of data for fatal crashes for the entire United States. The case listings include information on the location and time of the crash, the number of fatalities, and the drivers' blood alcohol content, in addition to numerous other variables.

The sample includes monthly crash data from 2006 to 2016 for 99 of the 100 most populous U.S. cities (United States Census Bureau, 2012). I exclude San Juan, Puerto Rico because there are no FARS data for Puerto Rico.

I define fatalities and fatal crashes as drunk driver related if at least one vehicle driver had a blood alcohol concentration recorded in the FARS data of at least 0.08 g/dL (the legal limit for individuals 21+ for Driving Under the Influence in all 50 states and Washington, D.C. during my sample period, 2006 to 2016).

Given the findings that Uber and Lyft lead to increases in alcohol consumption (Teltser, Lennon, and Burgdorf, 2021; Zhou, 2020), I also examine all alcohol-related incidents. I define fatalities and fatal crashes as alcohol related if at least one vehicle driver had a recorded blood alcohol concentration greater than 0 g/dL. Note that this measure excludes fatalities and fatal crashes involving an intoxicated pedestrian, cyclist, or passenger.

If Uber and Lyft are primarily substitutes for drunk driving, then I would expect to see changes in drunk driving. However, if they are primarily substitutes for walking or bicycling home drunk, then an analysis of the effect of Uber and Lyft entry on drunk driving would not pick up the true effect of Uber and Lyft on drunk transportation choices.

Lyft and Uber may have an effect on fatal crashes if they lead to an increase in the number of cars on the road, independent of their effect on drunk driving. To address this possibility, I examine total (alcohol and non-alcohol-related) fatalities and fatal crashes.

Time of day of each crash is known, so I separate crashes into daytime and nighttime, classifying daytime crashes as occurring between 4 a.m. and 8 p.m and nighttime crashes as occurring between 8 p.m. and 4 a.m. This cutoff was chosen because the hours between 8 p.m. and 4 a.m. contain most of the alcohol-related crashes. In addition, they exclude standard rush hour when people would be commuting to or from work.

Figure 4 shows the distribution of monthly drunk-driver-related fatal crashes. Summary statistics for monthly city-level fatal motor vehicle *crashes* are shown in Table 1. Summary statistics for monthly city-level motor vehicle *fatalities* are shown in Table 2.

There are more drunk-driver related crashes at night compared to during the day. For nighttime hours, fatalities per fatal crash are slightly higher than daytime fatalities per fatal crash ($0.88/0.76 = 1.16$ for night; $0.26/0.24 = 1.08$ for day).

Similar to drunk-driver-related crashes, the vast majority of alcohol-related crashes occur at night (between the hours of 8 p.m. and 4 a.m.). Alcohol-related crashes are slightly more lethal at night compared to during the day (1.15 fatalities per fatal crash vs. 1.10). In this sample of cities, drunk-driver-related crashes and fatalities make up 85% of alcohol-related crashes and fatalities.

Total fatal crashes are roughly evenly split between nighttime and daytime crashes, and there is quite a bit of variation across cities in the monthly number of fatal crashes. The means of daytime crashes and fatalities are slightly higher than nighttime crashes and fatalities, but fatalities per fatal crash are slightly higher for nighttime compared to daytime (1.09 vs. 1.06). Also of note is the fact that in this sample, 20% of fatal crashes and 21% of fatalities are drunk-driver-related, which is less than the national average of approximately 30% of motor vehicle fatalities during the same time period.

The three main limitations of the outcome variables/FARS data are that there is no information on the number of vehicle miles driven, alcohol-related crashes are measured with error, and there is no information on non-fatal crashes.

First, FARS does not contain information on the number of vehicle miles driven. To see why this is a problem, observe that the number of crashes in city i at time t can be represented by the following equation:

$$crashes_{it} = \frac{crashes_{it}}{vehicle\ mile\ driven_{it}} * (\#\ vehicle\ miles\ driven_{it}) \tag{5}$$

For example, if Uber and Lyft have no overall effect on the number of crashes, I could not distinguish between the following two scenarios. One, that Uber and Lyft have no effect on the number of crashes per vehicle mile driven and on the number of vehicle miles driven.[8] Two, that Uber and Lyft drivers are better drivers than average, leading to a reduction in the crash rate per vehicle mile driven, but they also lead to an increase in the number of vehicle miles driven, which exactly offsets the reduction in the crash rate.[9]

Second, there is measurement error for alcohol involvement in fatal crashes. One source of measurement error arises because states have different laws (and levels of enforcement) regarding BAC tests for drivers involved in fatal crashes (National Highway Traffic Safety

[8] If this scenario were true, it would imply that for every mile driven by Lyft and Uber, one mile is not driven by taxis or individuals driving their own cars/carpooling. But recall that Uber and Lyft are cheaper than a taxi, on average, implying that a reduction in the price of transportation is associated with no change in quantity demanded, which would imply that demand for transportation is perfectly inelastic.

[9] Uber and Lyft drivers could alternatively be worse drivers on average, leading to increases in the crash rate, but if there is a decrease in the number of vehicle miles driven, that could offset the increase in the crash rate.

Administration, 2012). Numerous states require probable cause for administering BAC tests to drivers involved in fatal crashes. As a result, the rates of known BAC test results vary across states (National Highway Traffic Safety Administration, 2012). However, when the alcohol test results are unknown, the National Highway Traffic Safety Administration estimates alcohol involvement (National Highway Traffic Safety Administration, 2017). After including the NHTSA alcohol-involvement estimates, only 0.3% of U.S. fatalities had an unknown or unreported highest driver blood alcohol concentration from 2006 to 2016. However, it is unclear how accurate the imputed BAC test results are, or how they may be biased.

Another source of measurement error comes from the breathalyzer tests themselves. An investigation by New York Times reporters found that breathalyzer machines in police departments across the country have not been properly calibrated or maintained, and the software in some of these machines had programming errors (Cowley and Silver-Greenberg, 2019). The combined effect of these errors yields breathalyzer test results that can be up to 40% higher than an individual's true blood alcohol concentration. A 40% overestimate of BAC would mean that an individual whose blood alcohol concentration was 0.057 g/dL (less than three-fourths the legal limit in all states during my sample period) could have a recorded BAC of 0.08 g/dL, rendering them drunk in the eyes of the law.

I address these measurement error issues in several ways. First, I separate crashes into nighttime and daytime, using nighttime crashes as a proxy for drunk-driver-related crashes. To address the measurement error issue arising from BAC imputation, I conduct a robustness check in which I restrict the sample to cities in the 18 states that test the BAC of at least 80% of deceased drivers (Kim et al, 2016; Appendix Table *A.2*). These results are presented in Section 5.2). To address the second source of measurement error, I estimate effects on alcohol-related fatal motor vehicle incidents. To the extent that errors in the breathalyzer machines are largely errors on the intensive margin, examining the effects on alcohol-related crashes avoids the issues arising from recorded BACs that are too high.[10]

Third, FARS does not contain information on non-fatal motor vehicle crashes. Suppose there are individuals who substitute away from drunk driving to Lyft and Uber, but they are the individuals who used to become involved only in non-fatal crashes. I would not be able to observe the reduction in non-fatal crashes in the data I have. Nevertheless, a reduction in non-fatal crashes is a desirable policy outcome. Consequently, the effect of Lyft and Uber on non-fatal crashes is beyond the scope of this paper.

3.2 Treatment Variable: Introduction of Ridesharing

I obtain data on the introduction of Uber or Lyft into a city from the respective company websites or from news articles. The month and year of each city's Uber or Lyft entry is listed in Appendix Table *A*.1.

The primary limitation of the treatment variable is that it requires the assumption of a constant, immediate treatment effect. The indicator may not accurately capture the effect of Uber and Lyft on drunk driving if, for example, it takes months or years for those services to become popular in a city. I conduct some robustness checks using alternative specifications to test the sensitivity of my results to this simplifying assumption. Nevertheless, assuming a constant treatment effect misses any measure of dose-response. The event studies in Section 4.1 provide suggestive evidence of longer-term effects.

[10] To the extent that toothpaste and mouthwash can also trigger a BAC above 0 for a roadside breathalyzer, there may still be some measurement error in alcohol-related crashes (Cowley and Silver-Greenberg, 2019).

3.3 Control Variables

I acquire monthly city-level unemployment data from the Bureau of Labor Statistics' Local Area Unemployment Statistics series.[11] Annual county-level population data come from the Surveillance, Epidemiology, and End Results (SEER) program. The population data break down the county-level populations by gender, race, and age. The 2010 city-level population data come from the U.S. Census Bureau. Summary statistics for selected control variables are shown in Table 3.

In this sample of cities, 30% of cities had Uber or Lyft in a given month-year. Also of note is the variation in city population size: of the 100 most populous U.S. cities, the average 2010 Census population was roughly 600,000 people. The 100th most populous city had a population just over 200,000, while the most populous city had nearly 8.2 million people.

The two main limitations of the SEER data are that county-level population estimates are imperfect measures of city-level population estimates, and annual population data are imperfect measures of monthly population data. Given the geographic mismatch of counties and cities, how closely the city population data line up with the county population data will vary across cities. With regard to annual population data, as long as the city population is not changing much month to month, the annual population data will be a good approximation of the monthly population.

In a robustness check I analyze heterogeneous effects of Uber and Lyft by public transit accessibility. I use rankings from a study conducted by WalletHub that ranked the 100 most

[11] The first six months of unemployment data for 2006 are unavailable for New Orleans, as the Bureau of Labor Statistics did not publish labor force estimates for New Orleans due to lingering data quality concerns from the effects of Hurricane Katrina (BLS, 2006). I therefore exclude the first six months of 2006 for New Orleans from my analysis.

populous U.S. cities on a variety of public transportation measures (McCann, 2019). One of the dimensions they ranked cities by was public transit accessibility and convenience. I define cities ranked 1-33 on this measure as "high accessibility," cities ranked 34-67 as "medium accessibility," and cities ranked 68-100 as "low accessibility". Appendix Table A.3 lists cities by accessibility type (high, medium, or low). The main limitation of these data are that they are from a study conducted in 2019, which is after my sample period.

4 Difference-in-Differences Results

Table 4 shows the results for monthly fatal motor vehicle incidents using a Poisson estimation. Panel A shows the results for drunk-driver-related fatal crashes. A fatal crash is defined as drunk driver related if the highest recorded Blood Alcohol Concentration of any involved driver was above the legal threshold for Driving Under the Influence (0.08 g/dL). The presence of Uber or Lyft is associated with a decrease in all drunk-driver-related fatal crashes of 0.03 crashes per month, a 3% decrease. Uber or Lyft lead to a reduction in nighttime drunk-driver-related fatal crashes (crashes recorded as occurring between 8 p.m. and 4 a.m.) of 0.01 crashes per month, a 1.3% decrease. They lead to a decrease in daytime drunk-driver-related crashes (recorded as occurring between 4 a.m. and 8 p.m.) of 0.02 crashes per month, an 8.3% decrease. None of these coefficients are statistically significantly different than 0.

Panel B of Table 4 shows the results for drunk-driver-related fatalities. Uber or Lyft leads to a reduction in all such fatalities of 0.09 people per month, an 8.0% decrease. They are associated with a 0.06-person-per-month decrease for nighttime drunk-driver-related fatalities (6.8%), and a 0.03-person-per-month decrease for daytime drunk-driver-related fatalities (11.9%). None of these effects are statistically significant, however.

Panel C shows the results for alcohol-related fatal crashes. A fatal crash is defined as alcohol related if the highest recorded Blood Alcohol Concentration of any involved driver was greater than zero. Uber or Lyft lead to monthly reductions of 0.02 alcohol-related crashes per month (1.2% decrease), 0.01 nighttime alcohol-related crashes (1.1% decrease), and 0.01 daytime alcohol-related crashes (3.3% decrease). Again, none of these effects are statistically significantly different than 0.

Panel D shows the results for alcohol-related fatalities. Uber or Lyft lead to reductions in all alcohol-related fatalities of 0.08 people per month, which is a 6% decrease. They are associated with 0.06 fewer nighttime alcohol-related fatalities per month (a 5.9% decrease) and 0.02 fewer daytime alcohol-related fatalities per month (a 6.1% decrease). None of these coefficients are statistically significant.

Panel E presents the results for all fatal crashes. Uber or Lyft leads to an increase of 0.15 fatal crashes per month (a 3% increase), a decrease of 0.03 nighttime fatal crashes per month (a 1.3% decrease), and an increase of 0.17 daytime fatal crashes per month (a 6.5% increase). However, none of these coefficients are significant.

Panel F presents the results for all fatalities. Uber or Lyft is associated with an increase of 0.16 fatalities per month (a 3% increase), no change in nighttime fatalities per month, and an increase of 0.15 daytime fatalities per month (a 5.5% increase). None of these effects are statistically significant.

4.1 Event Studies

A standard difference-in-differences model assumes a constant immediate treatment effect. However, this assumption may not be appropriate in the context of ridesharing. The adoption of this new technology takes time, meaning demand for Uber and Lyft may increase over time as more people learn about these apps and become more familiar with them. In addition, the number of drivers may change over time. Outward shifts in both the supply and demand curves for rides should in theory lead to an increase in the quantity of Uber and Lyft rides. As ridership increases, the effect of Uber and Lyft on drunk driving may change. Alternatively, driver skill may also play a role. If there are returns to experience for being a Lyft or Uber driver, whether through improved driving skill or improved knowledge of city geography (so drivers are not constantly checking their smartphones for driving directions), then there may not be a change in fatal incidents in the short run (there may even be an increase), but there could be longer-run changes.

To test for these heterogenous treatment effects, I conduct an event study. I focus on drunk-driver-related and overall fatal incidents because in the standard difference-in-differences specification, the results for alcohol-related incidents are quite similar to the results for drunk-driver-related incidents. To reduce the noise associated with monthly observations, I aggregate the data to the annual level. I use a pre-period window of 4 years and a post-period window of 6 years. I omit the year prior to Uber or Lyft's arrival in a city as the reference point. The Poisson event-study equation is as follows:

$$\mathbb{E}[(F_{iy}|\ Ride,\ \mathbf{X}] = exp\{\alpha + \sum_{k\neq -1, k=-4}^{k=6} \beta_k Ride_{kiy} + \mathbf{X}'_{iy}\gamma + \eta_i + \delta_y\} \tag{6}$$

$Ride_{kiy}$ is an indicator equal to 1 if Uber or Lyft has been in city i at year y for k years. β_k is the effect of Uber or Lyft having been in a city for k years.

The results indicate a delayed effect of Uber and Lyft on drunk driving. For drunk-driver-related fatal crashes (Figure 5), in the pre period the coefficients are small, positive, and not statistically significant. In the post period, the results become more negative and statistically significant the longer Uber or Lyft has been in a city. For drunk-driver-related fatalities (Figure 6), the results are nearly identical. The coefficients are small, positive, and not significant in the pre period, and trend downward and become statistically significant after several years in the post period. These event studies indicate that there are heterogeneous treatment effects of Uber and Lyft on drunk driving, consistent with a delayed adoption of this technology by riders and or drivers.

For overall crashes and fatalities (Figures 7 and 8), there may be a slight upward pre trend, as the coefficients are negative and generally not statistically significant but become more attenuated. In the first few years of the post period for both, the coefficients are small, positive, and not statistically significant, but over time they trend downward, though they are still generally not statistically significant.

5 Extensions

5.1 Quarterly and Annual Outcomes

Given that city-level fatal motor vehicle incidents are relatively rare occurrences, it is possible that the way one aggregates the data could matter for the results. Consequently, I conduct robustness checks where I aggregate the data to the quarterly or annual level as opposed to the monthly level. At these higher-level aggregations, there will be fewer zeroes in the outcome data.

The results for quarterly-level fatal incidents are shown in Table 5. There are some sign changes for drunk-driver-related fatal crashes and alcohol-related fatal crashes, but in general the estimated quarterly effects are similar to the monthly effects in percentage terms. None of the coefficients are statistically significantly different than 0.

For drunk-driver-related crashes (Panel A), the effect on all drunk-driver-related crashes changes sign relative to the monthly estimates (0.05 versus -0.03, the former of which is a 1.7% increase). The effect of Uber or Lyft on quarterly nighttime drunk-driver-related crashes also changes sign relative to the monthly estimate: it's an increase of 0.08 (3.3%) compared to -0.01 for the monthly estimate. For daytime drunk-driver-related crashes, the effect is attenuated (in percentage terms) relative to the monthly effect: -0.03 (-4.2%) compared to a monthly estimate of -0.02 (-8.33%). For drunk-driver-related fatalities (Panel B), Uber or Lyft leads to reductions of 0.19 drunk-driver-related fatalities (5.6% decrease), 0.12 nighttime drunk-driver-related fatalities (4.1% decrease), and 0.07 daytime drunk-driver-related fatalities (9.1% decrease) per quarter. These effects are similar to the monthly estimates in terms of percent changes.

Panel C shows the effect of Uber and Lyft on alcohol-related fatal crashes. There are increases of 0.09 fatal alcohol-related crashes per quarter (2.6% increase), 0.08 nighttime fatal alcohol-related crashes per quarter (3% increase), and 0.01 daytime fatal alcohol-related crashes per quarter (1.1% increase). Compared to the monthly estimates, these effects are small and positive as opposed to small and negative. For alcohol-related fatalities (Panel

D), the quarterly effects are half as large as the monthly effects in percentage terms. Uber and Lyft are associated with reductions of 0.10 alcohol-related fatalities (2.5% decrease), 0.08 nighttime alcohol-related fatalities (2.7% decrease), and 0.02 daytime alcohol-related fatalities (3% decrease) per quarter.

For overall crashes (Panel E), Uber and Lyft lead to an increase of 0.58 fatal crashes per quarter (a 3.9% increase), a decrease of 0.03 nighttime fatal crashes (0.4% decrease), and an increase of 0.61 daytime fatal crashes (a 7.8% increase). The effects are similar in percentage terms to the monthly estimates. For overall fatalities (Panel F), Uber and Lyft again lead to small increases. All fatalities increase by 0.61 people per quarter (3.8% increase), nighttime fatalities increase by 0.04 people (a 0.5% increase), and daytime fatalities increase by 0.55 people (a 6.2% increase). The effects are are again similar to the monthly estimates for overall fatalities when measured in percentage terms.

Table 6 presents the results for annual fatal incidents. The annual results are generally similar to the quarterly results, with the exception of the results for alcohol-related motor vehicle fatalities. None of the estimates are statistically significant.

For drunk-driver-related crashes (Panel A), Uber and Lyft lead to an increase of 0.40 crashes per year (a 3.3% increase). They are associated with an additional 0.65 nighttime drunk-driver-related fatal crashes per year, a 7.1% increase, and a reduction in daytime drunk-driver-related fatal crashes of 0.25 crashes per year (an 8.9% decrease). Panel B presents the results for drunk-driver-related fatalities. Uber or Lyft leads to reductions of 0.67 drunk-driver-related fatalities per year (4.9% decrease), 0.29 nighttime drunk-driver-related fatalities per year (2.8% decrease), and 0.39 daytime drunk-driver-related fatalities per year (12.7% decrease).

The results for alcohol-related crashes are shown in Panel C. Uber or Lyft is associated with an increase of 0.88 alcohol-related crashes per year (6.2% increase). They lead to an increase of 0.97 nighttime alcohol-related crashes per year (9.2% increase), and a decrease of 0.10 daytime alcohol-related crashes per year (2.8% decrease). Panel D shows the effect of Uber and Lyft on alcohol-related fatalities. They are associated with an increase of 0.09 alcohol-related fatalities per year (0.6% increase), an increase of 0.29 nighttime alcohol-related fatalities per year (2.4% increase), and a decrease of 0.22 alcohol-related fatalities per year (5.6% decrease). Compared to the monthly and quarterly effects, the sign changes for all and nighttime alcohol-related fatalities.

For overall crashes (Panel E), Uber and Lyft lead to increases of 3.54 crashes per year (6% increase), 0.62 nighttime crashes per year (2.2% increase), and 2.93 daytime crashes per year (9.4% increase). For overall fatalities (Panel F), Uber and Lyft again lead to increases: 7.12 fatalities per year (11.2% increase), 0.89 nighttime fatalities per year (2.9% increase), and 6.17 daytime fatalities per year (18.7% increase). The annual estimates for all fatalities are noticeably larger in percentage terms than the quarterly or monthly estimates; this difference is driven by larger effects on daytime fatalities.

5.2 Subsample of Majority Testing States

As mentioned previously, states vary in the percentage of drivers whose BAC they test, due to probable cause laws and inconsistent testing practices. Some states test the blood alcohol concentration of most deceased drivers, however (see Appendix Table A.2 for the list of states). These states tend to be on the West Coast, in the Mountain West, or in the

Northeast, and the subsample of cities in these states have more fatal motor vehicle incidents than the sample average.

In theory, the effect of Uber and Lyft on overall crashes and fatalities in this subsample should be similar to the effect on the full sample, unless there are time-varying omitted variables that differentially affect motor vehicle crashes, or Uber and Lyft have heterogeneous effects across cities.[12] If the overall results are similar, then any difference in the effects on drunk-driver-related incidents may be due to the measurement error in BAC in the full sample.[13]

When I restrict the sample to the 18 states that test at least 80% of drivers who died in the motor vehicle crash (Table 7), the effects for nighttime fatal motor vehicle incidents, as well as overall crashes and fatalities, are similar to the monthly estimates (Table 4). The estimates for all and daytime drunk-driver-related and alcohol-related incidents change signs.

Uber or Lyft is associated with an increase in overall drunk-driver-related fatal motor vehicle crashes of 0.10 crashes per month (6.8%), which is not statistically significant (Panel A of Table 7). They are associated with a 0.03-crash decrease in nighttime drunk-driver-related fatal motor vehicle crashes (-2.7%), which is also not statistically significant. Daytime drunk-driver-related fatal crashes increase by 0.13 crashes per month (37.1%), which is marginally statistically significant (10% significance level) and a large effect in percentage terms. The coefficients for drunk-driver-related motor vehicle fatalities (Panel B of Table 7) are the same as the coefficients for fatal crashes (Panel A), although none of the coefficients are

[12]The effects should be similar to the extent that overall crashes and fatalities do not suffer from measurement error in BAC.

[13]That is, the measurement error that arises from the NHTSA's BAC imputation procedure or from some states having greater discretion or probable cause requirements for law enforcement to enforce drunk-driving laws or measure BAC. Differences could also arise if Uber and Lyft have heterogeneous effects on drunk driving across cities.

statistically significant.

For alcohol-related fatal motor vehicle crashes (Panel C), Uber and Lyft are associated with an increase of 0.06 crashes per month (3%), a decrease of 0.04 nighttime crashes per month (-3.1%), and an increase of 0.10 daytime crashes per month (22.7% increase). For alcohol-related fatalities (Panel D), Uber or Lyft leads to an increase of 0.04 fatalities per month (2% increase), a decrease of 0.04 nighttime fatalities per month (2.7% decrease), and an increase of 0.09 daytime fatalities per month (18.8% increase). None of the coefficients for alcohol-related fatal crashes or fatalities are statistically significant.

For overall crashes (Panel E), Uber or Lyft leads to an increase of 0.03 fatal crashes per month (0.4% increase), a decrease of 0.09 nighttime crashes per month (2.7% decrease), and an increase of 0.13 daytime fatal crashes per month (3.5% increase). For overall fatalities (Panel F), they are associated with an increase of 0.05 fatalities per month (0.7% increase), a decrease of 0.01 nighttime fatalities per month (0.3% decrease), and an increase of 0.06 daytime fatalities per month (1.6% increase). None of these coefficients are statistically significant.

The similarities in results for nighttime fatal motor vehicle incidents, and overall crashes and fatalities, while not statistically significant, suggest that Uber and Lyft may lead to small reductions in nighttime fatal incidents (alcohol related and overall), but small increases in daytime crashes and fatalities. The discrepancy for daytime drunk-driver-related and alcohol-related crashes (which drives the difference in total drunk-driver-related and alcohol-related incidents) could be a result of several factors. As mentioned above, the effect of Uber and Lyft on daytime drunk driving for the subsample of cities in majority testing states could be different than their effect on other cities. Alternatively, there could be measurement error in imputed BAC for daytime drunk drivers. For example, the imputed BAC may undercount the true extent of daytime drunk driving. Uber and Lyft may have an effect on the behavior of individuals who would always be recorded (or imputed) as having a BAC above 0.08, but they may not have an effect on individuals who would be undercounted in the imputation. It could be that Uber and Lyft are not substitutes for daytime drunk driving but they are substitutes for nighttime drunk driving, particularly if these daytime incidents are occurring in the early morning hours when people wake up drunk, believe they are sober, and drive home from wherever they spent the night.

5.3 Heterogeneity by Quality of Public Transportation

The effect of Uber and Lyft on motor vehicle crashes and fatalities may depend on the quality of potential substitutes to driving, as well as the quality of potential complements to Uber and Lyft. In cities that are more walkable or have more robust public transportation, people may not be substituting from driving their own car to Uber and Lyft. They may either substitute from using public transit, or they may not use Uber and Lyft much. In this instance, Uber and Lyft may not lead to declines in drunk driving and may even lead to increases in total crashes if there are more cars on the road. Conversely, Uber and Lyft combined with public transportation may encourage people to use their own cars less frequently, if they use Uber and Lyft to get from the bus or subway stop to their home, for example. Empirically, Uber appears to be a complement for public transit, particularly in larger cities and in cities with smaller transit agencies (Hall, Palsson, and Price, 2018). To analyze heterogeneity by public transportation, I split cities into three approximately equally sized groups: high, medium, and low public transit accessibility, using rankings from a study conducted by WalletHub (McCann, 2019).

The effects of Uber and Lyft on drunk driving by public transit accessibility are presented in Table 8 and the results for overall crashes and fatalities are presented in Table 9. Panel A of Table 8 shows the effect of Uber and Lyft on drunk-driver-related fatal crashes. For cities with high public transit accessibility, Uber and Lyft lead to a reduction of 0.06 drunk-driver-related fatal crashes per month (5%), 0.06 nighttime drunk-driver-related fatal crashes per month (6.5%), and 0.01 daytime drunk-driver-related fatal crashes per month (3.5%). None of these effects are statistically significant. For cities with medium public transit accessibility, Uber or Lyft is associated with an increase of 0.07 fatal drunk-driver-related crashes per month (8%), 0.09 nighttime fatal drunk-driver-related crashes per month (12.9%), and a decrease of 0.02 daytime drunk-driver-related fatal crashes per month (10.5%). These effects are also not statistically significant. For cities with low public transit accessibility, the entry of Uber or Lyft leads to a reduction of 0.08 drunk-driver-related fatal crashes per month (15.7%), an increase of 0.01 nighttime drunk-driver-related crashes per month (2.6%), and a reduction of 0.09 daytime fatal drunk-driver-related crashes per month (69.2%). The effect for daytime is marginally statistically significant.

Panel B of Table 8 presents results for drunk-driver-related fatalities. For cities with high public transit accessibility, Uber and Lyft lead to a reduction of 0.14 drunk-driver-related fatalities per month (10.1%), 0.12 nighttime fatalities per month (11.3%), and 0.02 daytime fatalities per month (6.3%). The effect on nighttime drunk-driver-related fatalities is statistically significant at the 5% level. For cities with medium public transit accessibility, the arrival of Uber or Lyft is associated with an increase of 0.04 drunk-driver-related fatalities per month (4%), 0.08 nighttime drunk-driver-related fatalities per month (10.1%), and a reduction of 0.04 daytime fatalities per month (20%). None of these effects are statistically significant. For cities with low public transit accessibility, Uber and Lyft lead to reductions of 0.15 drunk-driver-related fatalities per month (26.8%), 0.04 nighttime fatalities per month (9.5%) and 0.10 daytime fatalities per month (71.4%). The effect on daytime drunk-driver-related fatalities is marginally statistically significant.

Panel A of Table 9 shows results for overall crashes. For cities with high public transit accessibility, Uber and Lyft lead to an increase of 0.02 fatal crashes per month (0.3%), a decrease of 0.12 nighttime fatal crashes per month (-4%), and an increase of 0.13 daytime fatal crashes per month (3.8%). None of these coefficients are statistically significant. For cities with medium public transit accessibility, Uber or Lyft is associated with an increase of 0.47 fatal crashes per month (12.7%), 0.18 nighttime fatal crashes per month (9.7%), and 0.29 daytime fatal crashes per month (15.6%). The effects for overall and daytime crashes are statistically significant at the 5% level. For cities with low public transit accessibility, Uber and Lyft lead to increases of 0.26 fatal crashes per month (11.5%), 0.07 nighttime fatal crashes per month (6.5%), and 0.19 daytime fatal crashes per month (15.8%). None of these effects are statistically significant.

Results for overall fatalities are shown in Panel B of Table 9. For cities with high public transit accessibility, Uber and Lyft lead to a decrease of 0.01 fatalities per month (0.2%), a decrease of 0.12 nighttime fatalities per month (3.7%), and an increase of 0.10 daytime fatalities per month (2.8%). None of these effects are statistically significant. For cities with medium public transit accessibility, Uber and Lyft lead to an increase of 0.57 fatalities per month (14.3%), 0.27 nighttime fatalities per month (13.3%), and 0.29 daytime fatalities per month (14.9%). The effect on overall fatalities is significant at the 1% level, the effect on nighttime fatalities is marginally significant, and the effect on daytime fatalities is significant at the 5% level. For cities with low public transit accessibility, Uber and Lyft are associated with increases of 0.33 fatalities per month (13.6%), 0.11 nighttime fatalities per month (9.5%), and 0.23 daytime fatalities per month (18.3%). None of these effects are statistically significant.

In general, there are moderate declines for drunk-driver-related crashes and fatalities in cities with high or low public transit accessibility, and daytime incidents in cities with medium accessibility, although only some of these declines are statistically significant. The declines for daytime incidents in cities with low accessibility are quite large in percentage terms as the underlying number of crashes and fatalities in these cities is quite small. There are moderate increases in overall and nighttime drunk-driver-related incidents in cities with medium accessibility but these effects are not statistically significant.

For all crashes and fatalities, there are moderate increases in cities with medium or low public transit accessibility, although only the effects for medium cities are statistically significant. For cities with medium public transit accessibility, a potential mechanism for the increase in fatal incidents is more cars on the road as a result of Uber and Lyft. There are small declines for nighttime incidents in cities with high public transit accessibility that are offset by small increases in daytime incidents, although the effects are not statistically significant.

5.4 Negative Binomial Regression

The Poisson distribution is a special case of the negative binomial distribution. A Poisson model is appropriate when the outcome is a count variable and follows a Poisson distribution, which requires the mean and variance of the distribution to be equal. A negative binomial model is an alternative specification that does not require the mean to equal the variance. In my sample, the variance of fatal motor vehicle incidents is larger than the mean (Tables 1 and 2), so in this section I conduct an alternative specification using a negative binomial model. The equation for a negative binomial model is the same as the equation for the Poisson model (Equation 4), but assumes a different distribution of the outcome variable.

Table 10 presents the results from the negative binomial estimation. These estimates are unweighted because the estimates did not converge when I weighted by city population. The unweighted results for drunk-driver-related and alcohol-related fatal incidents are nearly identical to the population-weighted monthly Poisson estimates. The effects for overall fatal incidents are zero in the negative binomial specification so they are attenuated relative to the Poisson specification. None of the coefficients are statistically significant.

Panel A shows the effect on drunk-driver-related fatal crashes. Uber or Lyft is associated with reductions of 0.03 drunk-driver-related fatal crashes (3% decrease), 0.04 nighttime drunk-driver-related fatal crashes (5.3% decrease), and 0.00 daytime drunk-driver-related fatal crashes. Panel B shows the effect on drunk-driver-related fatalities: a decrease of 0.08 drunk-driver-related fatalities (7.1% decrease), a decrease of 0.09 nighttime fatalities (-10.2%), and a decrease of 0.06 daytime fatalities (-23.1%).

The results for alcohol-related fatal crashes are in Panel C. Uber or Lyft leads to decreases of 0.02 alcohol-related fatal crashes per month (-1.7%), 0.02 nighttime alcohol-related fatal crashes per month (-2.3%), and no change for daytime alcohol-related fatal crashes. For alcohol-related fatalities (Panel D), they lead to reductions of 0.05 fatalities per month (-3.8%), 0.04 nighttime fatalities per month (-4%), and 0.01 daytime fatalities per month (-3%).

For overall crashes and fatalities, the point estimates are 0.00 for daytime crashes, overall fatalities, and nighttime fatalities. Uber and Lyft are associated with an increase of 0.01 crashes per month (0.2% increase). The estimation for nighttime crashes did not converge, so no result is reported for that specification. Uber and Lyft are associated with a reduction of 0.01 daytime fatalities per month (-0.4% decrease).

5.5 Log Regression

To analyze whether the results are consistent using other functional forms, in this section I estimate an OLS model using the log of monthly fatal motor vehicle incidents (plus 1) as the outcome. This specification assumes a lognormal distribution of the outcome variable, which occurs when the logarithm of a continuous variable is normally distributed.

$$log(F_{it} + 1) = \alpha + \beta \cdot Ride_{it} + \mathbf{X}'_{it} \cdot \gamma + \eta_i + \delta_t + \varepsilon_{it} \tag{7}$$

$log(F_{it} + 1)$ represents the log of 1 + monthly city-level motor vehicle crashes or fatalities. I add 1 to the count of incidents because many cities (fortunately) have 0 fatal incidents in a month, and the log of 0 is undefined. The treatment and control variables are the same as in Equation 4, and as before, the standard errors are clustered at the city level and I weight all regressions using the 2010 Census city population.

The results for the log specification are similar to the Poisson specification, although in some instances the log coefficients are simultaneously attenuated and more precisely estimated than the Poisson coefficients. The impact of Uber and Lyft on the log of fatal drunk-driver-related motor vehicle crashes is shown in Panel A of Table 11. The presence of Lyft or Uber in a city is associated with approximately a 2% decline in all drunk-driver-related motor vehicle crashes, a 3% decline in nighttime drunk-driver-related fatal crashes, and a 1% decline in daytime drunk-driver-related crashes declined. However, none of these effects are statistically significant. The log estimates are similar to the Poisson estimates for all and nighttime drunk-driver-related crashes (-2% vs. -3% and -3% vs. -1%), and they are attenuated for daytime crashes (-1% vs. -8%).

Turning to the results for the log of drunk-driver-related motor vehicle fatalities (Panel B of Table 11), I find that the presence of Lyft or Uber leads to a 4% decline in such fatalities, which is marginally statistically significant (10% significance level). This decline is driven by nighttime drunk-driver-related motor vehicle fatalities, which decline by 4% after Uber or Lyft entered a city (statistically significant at the 5% level). Daytime fatalities declined by 1%, although this difference is not statistically significant. The log estimates for all and nighttime drunk-driver-related fatalities are approximately half as large as the Poisson estimates (-4% vs. -8%, -4% vs. -7%) and the daytime estimates are noticeably attenuated in the log specification (-1% vs. -12%).

The results for alcohol-related fatal motor vehicle crashes are smaller in magnitude and not statistically significant (Panel C of Table 11). The presence of Lyft or Uber leads to a 1% decline in all alcohol-related fatal motor vehicle crashes, a 1% decline in nighttime alcohol-related fatal crashes, and no decline in daytime alcohol-related fatal crashes. The estimates are similar to the Poisson specification for all and nighttime alcohol-related crashes (-1% for all), but attenuated for daytime crashes (no change vs. -3%).

For alcohol-related motor vehicle fatalities (Panel D of Table 11), Uber or Lyft leads to a 2% decline in all alcohol-related motor vehicle fatalities. They also lead to a 3% reduction in nighttime alcohol-related fatalities and a 1% decline in daytime alcohol-related fatalities. None of these coefficients are statistically significant and they are attenuated relative to the Poisson specification (-2% vs. -6%, -3% vs. -6%, and -1% vs. -6%).

For all fatal motor vehicle crashes (Panel E of Table 11), some of the coefficients become positive, but none of them are statistically significant. Uber or Lyft leads to a 2% increase in all fatal motor vehicle crashes, a 1% decrease in nighttime fatal motor vehicle crashes, and a 3% increase in daytime fatal motor vehicle crashes. The coefficients on motor vehicle fatalities (Panel F of Table 11) are virtually identical: Lyft or Uber leads to a 2% increase in all motor vehicle fatalities, a 1% decrease in nighttime fatalities, and a 2% increase in daytime fatalities. As with the fatal motor vehicle crash coefficients, none of these coefficients are statistically significant. The log estimates for all and nighttime crashes and fatalities are similar to the Poisson estimates (2% vs. 3%, -1% vs. -1%, 2% vs. 3%, and -1% vs. 0%) and attenuated for the daytime estimates (3% vs. 7% and 2% vs. 5%).

6 Discussion

The externalities associated with drunk driving are a serious problem in the United States. Consequently, many policies have been enacted to reduce the incidence of drunk driving, such as the Minimum Legal Drinking Age and a lower BAC limit. The former has been relatively successful at reducing motor vehicle fatalities (Carpenter, Dobkin, and Warman, 2016). The latter's effectiveness has been debated in the literature (Eisenberg, 2003 and Freeman, 2007).

In this paper, I analyze whether the "free market" may also have a role to play in combating drunk driving; specifically, whether the entry of Uber and Lyft into cities led to reductions in drunk-driver-related fatal motor vehicle incidents. I use Fatality Analysis Reporting System data from 2006 to 2016 for 99 of the 100 most populous cities in the U.S. to estimate a difference-in-differences model. In the standard difference-in-differences specification, I am unable to reject the null hypothesis of no effect of Uber and Lyft on monthly city-level drunk-driver-related fatalities or fatal crashes. The coefficients are small and negative, but they are not precisely estimated, meaning I cannot rule out moderate to large decreases or increases in fatal drunk-driving incidents. However, event study specifications indicate that there are statistically significant declines in annual drunk-driver-related crashes and fatalities 2-6 years after Uber or Lyft start operating in a city. The simple difference-in-differences results are similar when I estimate a negative binomial or a log specification. The small declines in drunk-driving-related fatal incidents are driven by cities with high or low public transit accessibility. When I restrict the sample to cities in states that test the BAC of at least 80% of deceased drivers, the effect on nighttime drunk-driving-related fatal incidents is similar but the effect on daytime drunk-driving-related fatal incidents becomes positive.

As a secondary analysis, I estimate the effect of Uber and Lyft on overall crashes and fatalities. I am unable to reject the null hypothesis of no effect of Uber or Lyft on overall crashes and fatalities. The coefficients are small and generally positive but imprecisely estimated. The event studies show small increases in crashes and fatalities for the first couple of years after Uber and Lyft arrive in a city followed by large declines after 5 to 6 years. The simple difference-in-differences results are slightly attenuated when I estimate a negative binomial or log specification. The small increases in overall fatal incidents are concentrated in cities with medium public transit accessibility.

Given that the standard difference-in-differences coefficients are not statistically significant, caution is warranted when trying to translate the results of this paper into specific policy implications. That being said, the opposite-signed results for drunk-driving-related incidents and overall incidents suggest that there is the potential for certain regulations to keep the potentially beneficial effects of Uber and Lyft (reductions in nighttime drunk-driving fatal incidents) while minimizing the potentially detrimental effects (increases in overall fatal incidents). Longer-run declines in both drunk-driver-related and overall fatal incidents are consistent with both outward shifts in the supply and or demand curves for Uber and Lyft, as well as driver skill improving with experience. If the mechanism is one of driver skill, then stricter regulations concerning driver qualifications may be warranted as a way to reduce overall crashes and fatalities. If the increase in daytime incidents is occurring due to more cars on the road, then a tax on daytime rides may reduce demand for daytime Uber or Lyft rides (a similar concept to congestion pricing).

Future work should include more recent years of data to estimate longer-run effects. Another interesting avenue of inquiry would be to distinguish between possible mechanisms for the longer-run effects that I find, specifically increased adoption of ridesharing versus improvements in driver skill.

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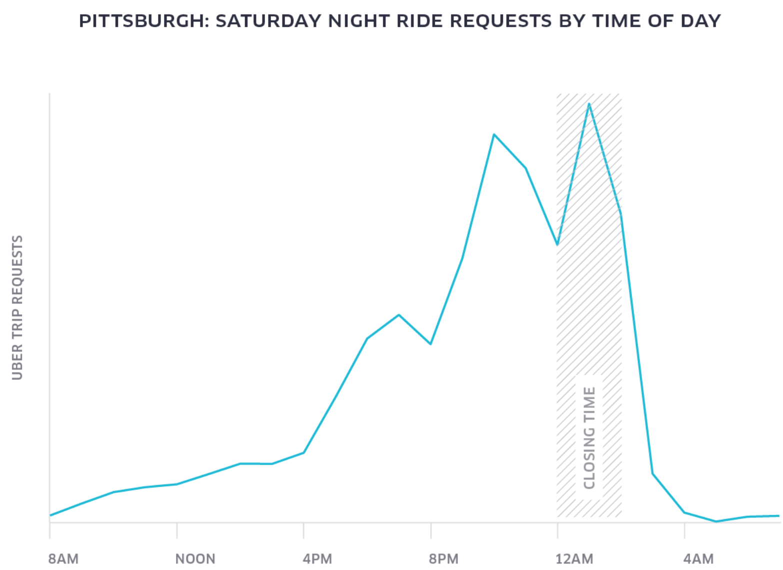
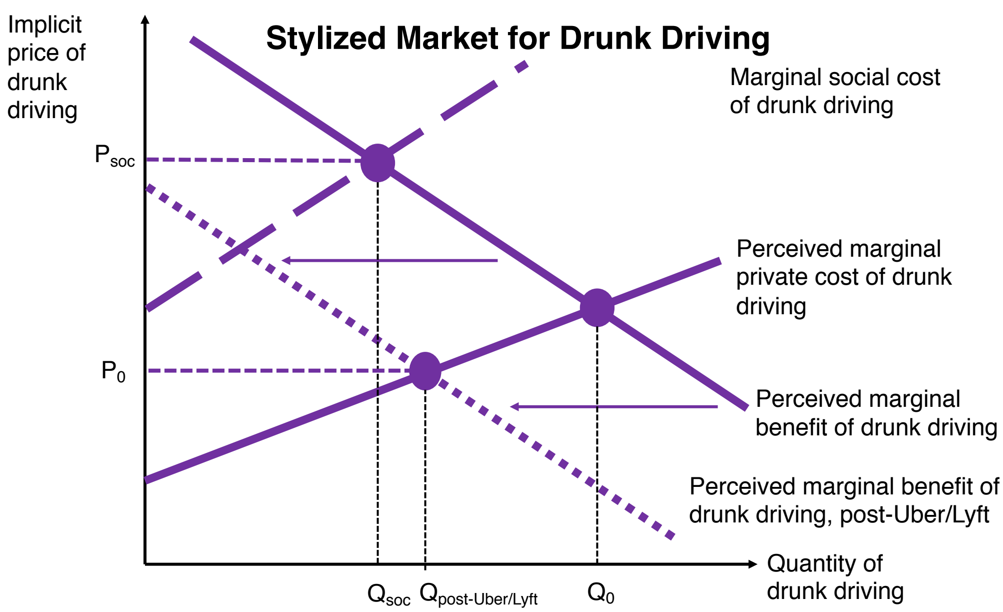
8 Figures and Tables

Figure 1



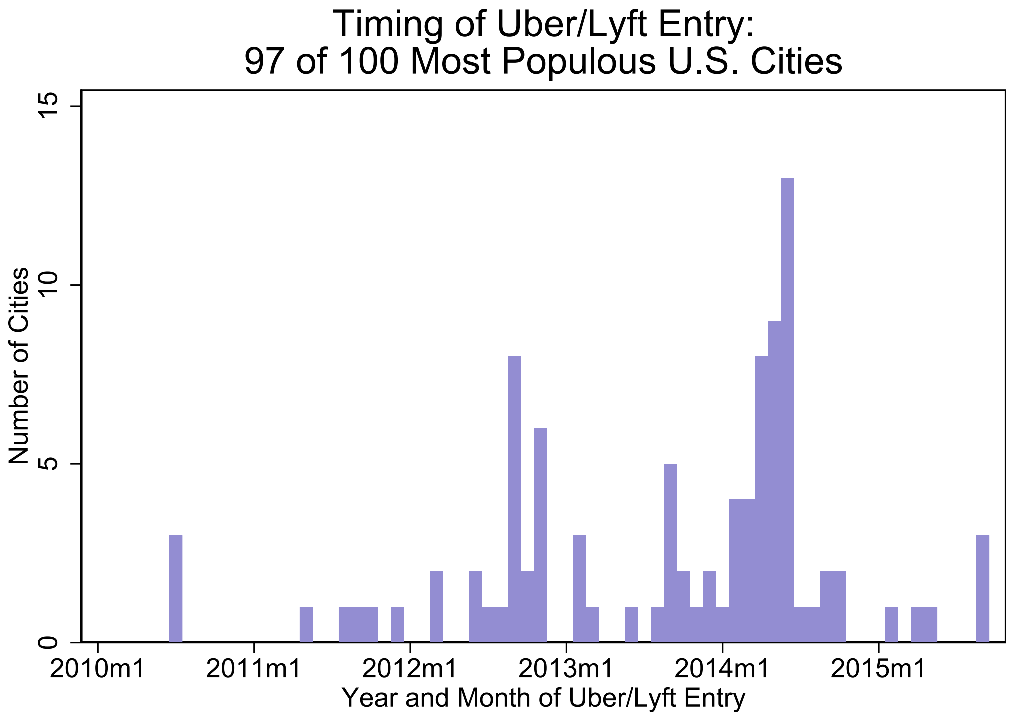
Source: Uber.

Figure 2



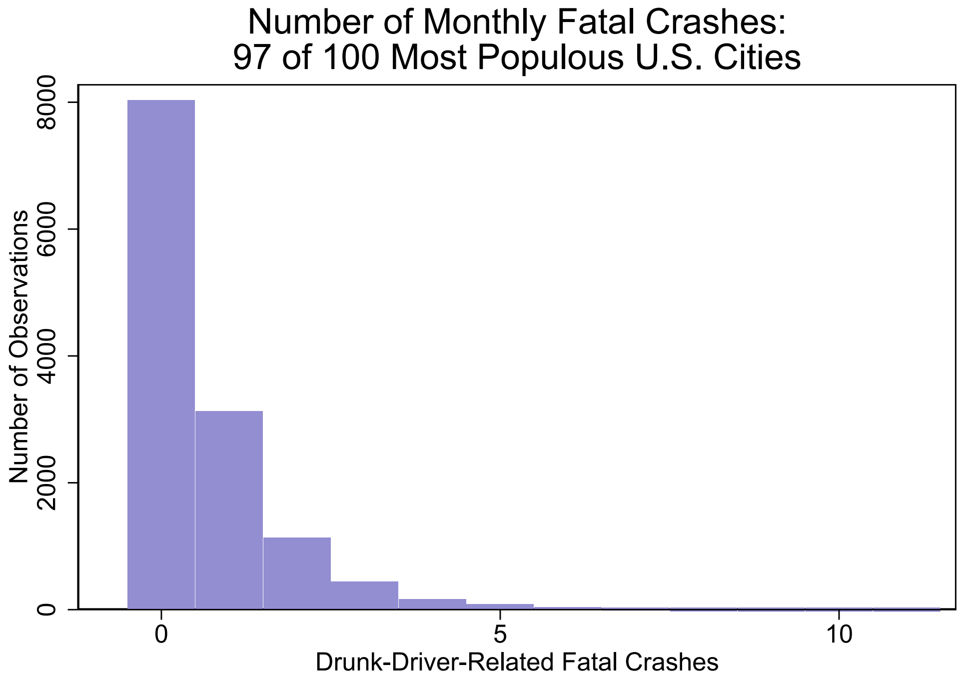
Source: Author's own illustration.

Figure 3



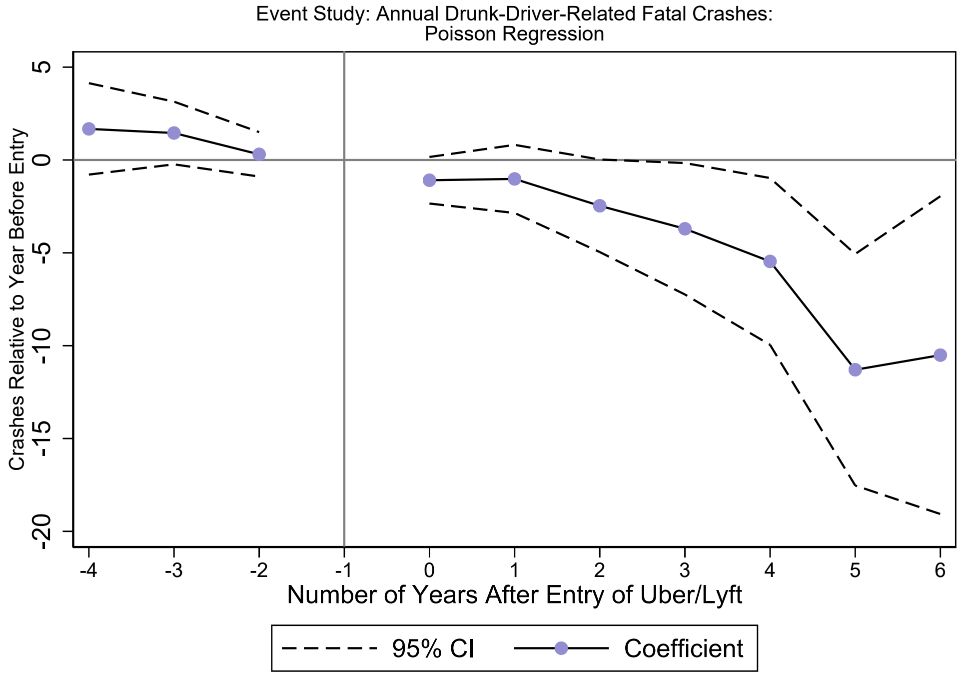
Data source: Author's hand-collected data from Uber, Lyft, and news articles.

Figure 4



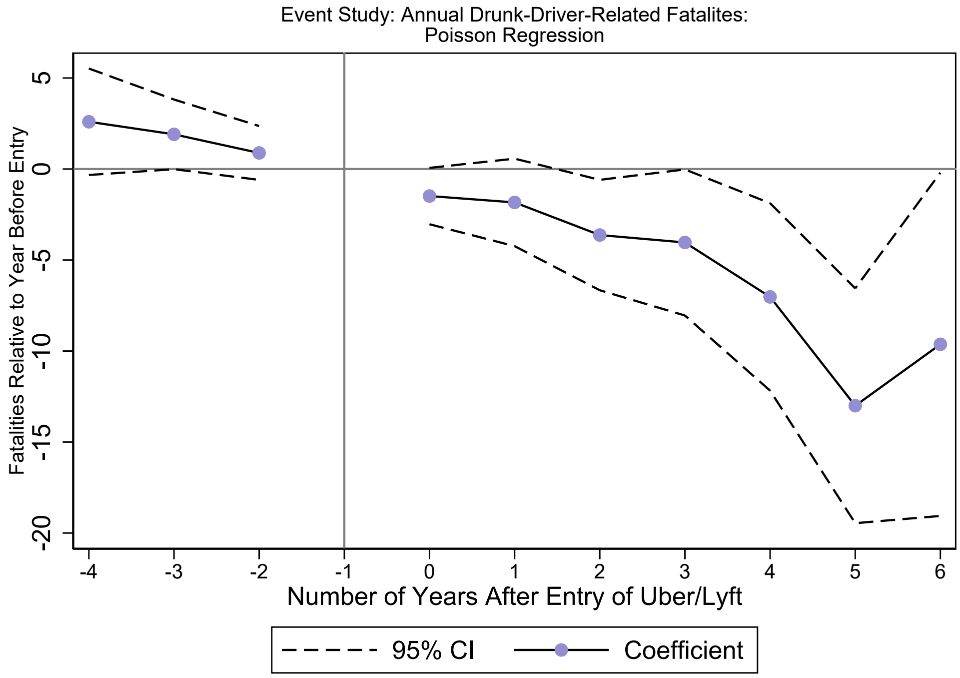
Data source: FARS 2006-2016.

Figure 5



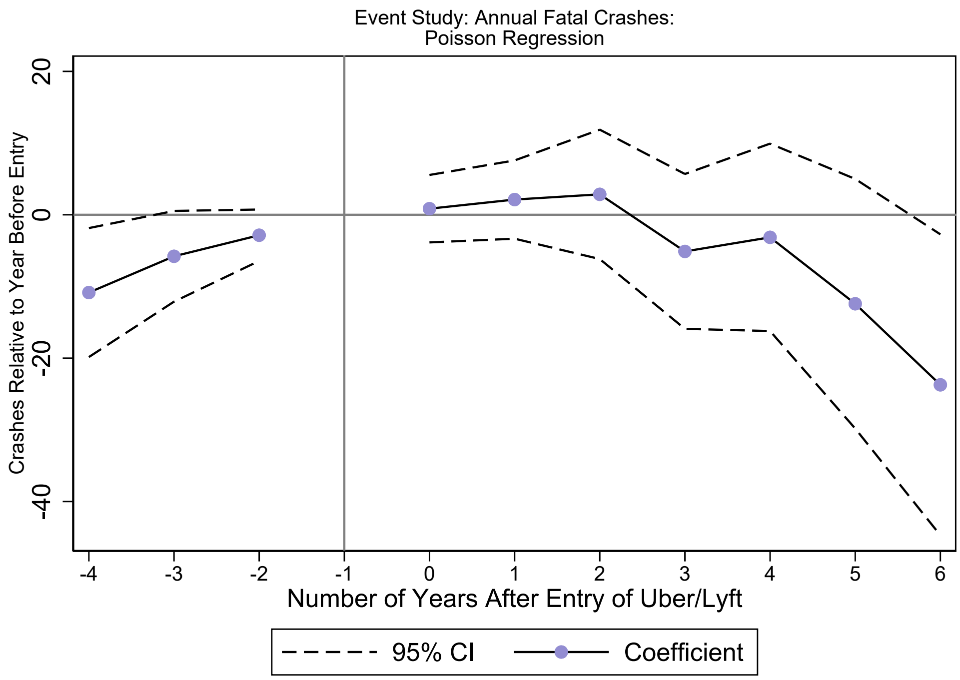
Note: Results from the estimation specified in Equation 6. Controls include the annual city unemployment rate, county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Figure 6



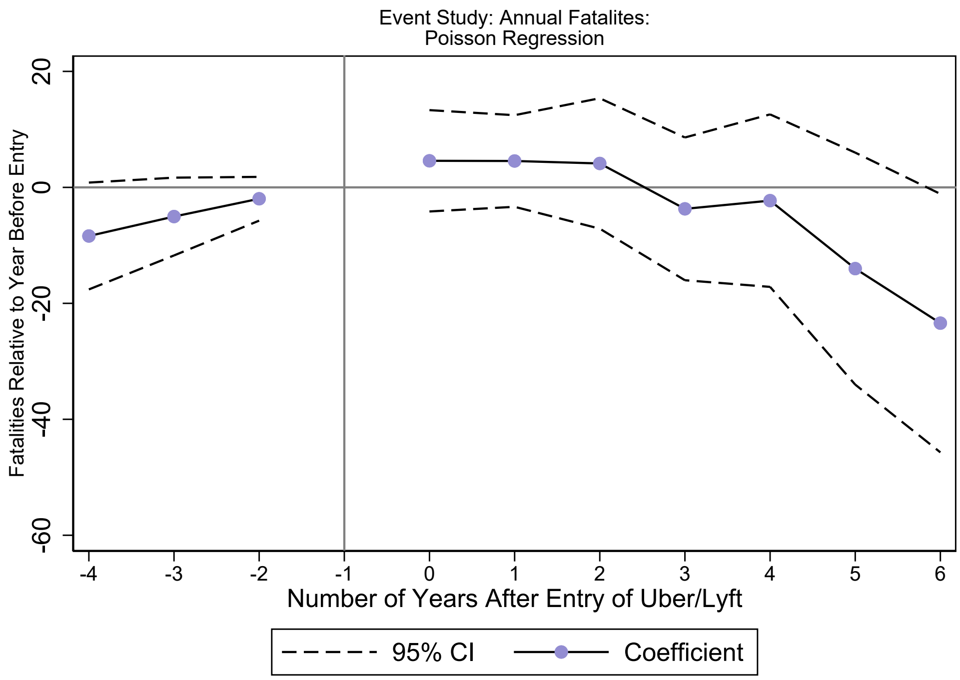
Note: Results from the estimation specified in Equation 6. Controls include the annual city unemployment rate, county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Figure 7



Note: Results from the estimation specified in Equation 6. Controls include the annual city unemployment rate, county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Figure 8



Note: Results from the estimation specified in Equation 6. Controls include the annual city unemployment rate, county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

Table 1: Summary Statistics of Fatal Motor Vehicle **Crashes:** 99 of the 100 Most Populous U.S. Cities

Variable	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)	N (5)
Drunk Driver	**1.00**	1.50	0	11	13,062
Nighttime	0.76	1.23	0	9	13,062
Daytime	0.24	0.55	0	4	13,062
Alcohol-Related	**1.18**	1.73	0	13	13,062
Nighttime	0.88	1.36	0	9	13,062
Daytime	0.30	0.65	0	5	13,062
Total	**4.95**	5.66	0	34	13,062
Nighttime	2.35	2.91	0	20	13,062
Daytime	2.60	3.21	0	21	13,062

Note: each observation is a city-month-year, e.g. New York City, May 2006. Data are from 2006 to 2016 for the 100 largest U.S. cities excluding San Juan, Puerto Rico, which does not have FARS data. The first 6 months of 2006 for New Orleans are not included because the BLS did not publish labor force estimates due to lingering data quality concerns post-Hurricane Katrina (BLS, 2006). Statistics are weighted by the 2010 Census city population. Drunk driver means at least one driver was recorded as having a BAC ≥ 0.08 g/dL. Alcohol-related means at least one driver was recorded as having a BAC > 0.00 g/dL.
Data Source: FARS 2006-2016.

Table 2: Summary Statistics of Motor Vehicle **Fatalities:** 99 of the 100 Most Populous U.S. Cities

Variable	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)	N (5)
Drunk Driver	**1.13**	1.74	0	12	13,062
Nighttime	0.88	1.45	0	10	13,062
Daytime	0.26	0.63	0	7	13,062
Alcohol-Related	**1.33**	1.99	0	14	13,062
Nighttime	1.01	1.60	0	10	13,062
Daytime	0.33	0.73	0	7	13,062
Total	**5.30**	6.08	0	36	13,062
Nighttime	2.55	3.22	0	21	13,062
Daytime	2.75	3.41	0	22	13,062

Note: each observation is a city-month-year, e.g. New York City, May 2006. Data are from 2006 to 2016 for the 100 largest U.S. cities excluding San Juan, Puerto Rico, which does not have FARS data. The first 6 months of 2006 for New Orleans are not included because the BLS did not publish labor force estimates due to lingering data quality concerns post-Hurricane Katrina (BLS, 2006). Statistics are weighted by the 2010 Census city population. Drunk driver means at least one driver was recorded as having a BAC ≥ 0.08 g/dL. Alcohol-related means at least one driver was recorded as having a BAC > 0.00 g/dL.
Data Source: FARS 2006-2016.

Table 3: Summary Statistics of Selected Control Variables (Unweighted): 99 of the 100 Most Populous U.S. Cities

Variable	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)	N (5)
= 1 if Uber/Lyft	**0.30**	0.46	0	1	13,062
UE Rate (%)	6.98	3.05	1.50	28.40	13,062
% African-American†	17.09	14.53	0.21	65.62	13,062
% Asian†	7.70	9.22	0.53	71.10	13,062
% Hispanic†	23.35	18.78	1.27	95.73	13,062
% White†	51.08	17.35	3.47	87.47	13,062
% Male†	49.08	0.97	46.89	52.31	13,062
% Male 20-24†	3.91	0.93	2.54	9.47	13,062
% 20-24†	7.70	1.59	5.09	15.64	13,062
% 25-34†	15.30	2.05	10.61	23.59	13,062
% 35-54†	27.24	1.90	19.33	32.23	13,062
% 55+†	22.73	3.28	14.48	39.54	13,062
2010 Pop.	**602,532**	920,435	208,453	8,175,133	13,062

Note: each observation is a city-month-year, e.g. New York City, May 2006. Data are from 2006 to 2016 for the 100 largest U.S. cities excluding San Juan, Puerto Rico, which does not have FARS data. The first 6 months of 2006 for New Orleans are not included because the BLS did not publish labor force estimates due to lingering data quality concerns post-Hurricane Katrina (BLS, 2006). † refers to county population. Data sources: Uber, Lyft, news articles, BLS, SEER, Census.

Table 4: Effect of Uber and Lyft on **Monthly** Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.03	-0.01	-0.02
	(0.06)	(0.04)	(0.04)
Dependent Variable Mean	1.00	0.76	0.24
% of Mean	-3.00%	-1.32%	-8.33%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.09	-0.06	-0.03
	(0.08)	(0.05)	(0.05)
Dependent Variable Mean	1.13	0.88	0.26
% of Mean	-7.96%	-6.82%	-11.94%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.02	-0.01	-0.01
	(0.06)	(0.04)	(0.04)
Dependent Variable Mean	1.18	0.88	0.30
% of Mean	-1.16%	-1.14%	-3.33%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.08	-0.06	-0.02
	(0.07)	(0.05)	(0.04)
Dependent Variable Mean	1.33	1.01	0.33
% of Mean	-6.02%	-5.94%	-6.06%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.15	-0.03	0.17
	(0.16)	(0.08)	(0.08)
Dependent Variable Mean	4.95	2.35	2.60
% of Mean	3.03%	-1.28%	6.54%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft	0.16	0.00	0.15
	(0.16)	(0.08)	(0.13)
Dependent Variable Mean	5.30	2.55	2.75
% of Mean	3.02%	0.00%	5.45%
N	13,062	13,062	13,062

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 4. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 5: Effect of Uber and Lyft on **Quarterly** Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of quarter)	0.05	0.08	-0.03
	(0.18)	(0.13)	(0.15)
Dependent Variable Mean	3.00	2.29	0.71
% of Mean	1.67%	3.29%	-4.23%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of quarter)	-0.19	-0.12	-0.07
	(0.24)	(0.15)	(0.17)
Dependent Variable Mean	3.40	2.63	0.77
% of Mean	-5.59%	-4.06%	-9.09%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of quarter)	0.09	0.08	0.01
	(0.18)	(0.13)	(0.15)
Dependent Variable Mean	3.54	2.63	0.91
% of Mean	2.54%	3.04%	1.10%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of quarter)	-0.10	-0.08	-0.02
	(0.23)	(0.15)	(0.17)
Dependent Variable Mean	4.00	3.02	0.98
% of Mean	-2.50%	-2.65%	-3.04%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of quarter)	0.58	-0.03	0.61
	(0.64)	(0.26)	(0.49)
Dependent Variable Mean	14.85	7.04	7.81
% of Mean	3.91%	-0.43%	7.81%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of quarter)	0.61	0.04	0.55
	(0.66)	(0.27)	(0.47)
Dependent Variable Mean	15.91	7.66	8.95
% of Mean	3.83%	0.52%	6.15%
N	4,354	4,354	4,354

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 4 with quarterly instead of monthly observations. Controls include the quarterly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 6: Effect of Uber and Lyft on **Annual** Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of year)	0.40	0.65	-0.25
	(1.06)	(0.80)	(0.58)
Dependent Variable Mean	11.99	9.16	2.82
% of Mean	3.34%	7.10%	-8.87%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of year)	-0.67	-0.29	-0.39
	(1.33)	(0.93)	(0.65)
Dependent Variable Mean	13.59	10.52	3.07
% of Mean	-4.93%	-2.76%	-12.70%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of year)	0.88	0.97	-0.10
	(1.01)	(0.78)	(0.56)
Dependent Variable Mean	14.16	10.53	3.63
% of Mean	6.21%	9.21%	-2.75%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of year)	0.09	0.29	-0.22
	(1.28)	(0.91)	(0.63)
Dependent Variable Mean	16.00	12.08	3.92
% of Mean	0.56%	2.40%	-5.61%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft (by fraction of year)	3.54	0.62	2.93
	(2.86)	(1.30)	(2.43)
Dependent Variable Mean	59.40	28.15	31.25
% of Mean	5.96%	2.20%	9.38%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft (by fraction of year)	7.12	0.89	6.17
	(5.22)	(1.41)	(4.65)
Dependent Variable Mean	63.62	30.65	32.98
% of Mean	11.19%	2.90%	18.71%
N	1,089	1,089	1,089

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 4 with annual instead of monthly observations. Controls include the annual city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 7: Effect of Uber and Lyft on Monthly Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression, Majority Testing Subsample (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.10	-0.03	0.13*
	(0.16)	(0.13)	(0.07)
Dependent Variable Mean	1.47	1.13	0.35
% of Mean	6.80%	-2.65%	37.14%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft	0.10	-0.03	0.13
	(0.16)	(0.13)	(0.08)
Dependent Variable Mean	1.67	1.29	0.38
% of Mean	5.99%	-2.33%	34.21%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.06	-0.04	0.10
	(0.20)	(0.16)	(0.08)
Dependent Variable Mean	1.76	1.31	0.44
% of Mean	3.01%	-3.05%	22.73%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft	0.04	-0.04	0.09
	(0.20)	(0.16)	(0.09)
Dependent Variable Mean	1.99	1.50	0.48
% of Mean	2.01%	-2.67%	18.75%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.03	-0.09	0.13
	(0.28)	(0.23)	(0.23)
Dependent Variable Mean	7.03	3.35	3.69
% of Mean	0.43%	-2.69%	3.52%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft	0.05	-0.01	0.06
	(0.28)	(0.24)	(0.26)
Dependent Variable Mean	7.50	3.66	3.84
% of Mean	0.67%	-0.27%	1.56%
N	4,752	4,752	4,752

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 4 using the subsample of cities in states that test the BAC of at least 80% of deceased drivers. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 8: Effect of Uber and Lyft on Monthly Drunk-Driver-Related Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression, Stratified by Public Transit Accessibility (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft High	-0.06	-0.06	-0.01
	(0.06)	(0.05)	(0.05)
Dependent Variable Mean	1.21	0.92	0.29
% of Mean	-4.96%	-6.52%	-3.45%
Uber and/or Lyft Medium	0.07	0.09	-0.02
	(0.09)	(0.06)	(0.05)
Dependent Variable Mean	0.88	0.70	0.19
% of Mean	7.95%	12.86%	-10.53%
Uber and/or Lyft Low	-0.08	0.01	-0.09*
	(0.15)	(0.12)	(0.05)
Dependent Variable Mean	0.51	0.38	0.13
% of Mean	-15.69%	2.63%	-69.23%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft High	-0.14	-0.12**	-0.02
	(0.08)	(0.05)	(0.06)
Dependent Variable Mean	1.38	1.06	0.32
% of Mean	-10.14%	-11.32%	-6.25%
Uber and/or Lyft Medium	0.04	0.08	-0.04
	(0.11)	(0.07)	(0.05)
Dependent Variable Mean	0.99	0.79	0.20
% of Mean	4.04%	10.13%	-20.00%
Uber and/or Lyft Low	-0.15	-0.04	-0.10*
	(0.16)	(0.12)	(0.06)
Dependent Variable Mean	0.56	0.42	0.14
% of Mean	-26.79%	-9.52%	-71.43%
N	13,062	13,062	13,062

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 4 with treatment interacted with public transit accessibility. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 9: Effect of Uber and Lyft on Monthly Motor Vehicles Fatalities and Fatal Crashes: Poisson Regression, Stratified by Public Transit Accessibility (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft High	0.02	-0.12	0.13
	(0.20)	(0.09)	(0.16)
Dependent Variable Mean	6.41	2.99	3.42
% of Mean	0.31%	-4.01%	3.80%
Uber and/or Lyft Medium	0.47**	0.18	0.29**
	(0.19)	(0.12)	(0.14)
Dependent Variable Mean	3.71	1.85	1.86
% of Mean	12.67%	9.73%	15.59%
Uber and/or Lyft Low	0.26	0.07	0.19
	(0.23)	(0.15)	(0.14)
Dependent Variable Mean	2.26	1.07	1.20
% of Mean	11.50%	6.54%	15.83%
Panel B. Overall Motor Vehicle Fatalities			
Uber and/or Lyft High	-0.01	-0.12	0.10
	(0.21)	(0.09)	(0.15)
Dependent Variable Mean	6.86	3.25	3.61
% of Mean	-0.15%	-3.69%	2.77%
Uber and/or Lyft Medium	0.57***	0.27*	0.29**
	(0.21)	(0.14)	(0.14)
Dependent Variable Mean	3.98	2.03	1.95
% of Mean	14.32%	13.30%	14.87%
Uber and/or Lyft Low	0.33	0.11	0.23
	(0.25)	(0.17)	(0.16)
Dependent Variable Mean	2.42	1.16	1.26
% of Mean	13.64%	9.48%	18.25%
N	13,062	13,062	13,062

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 4 with treatment interacted with public transit accessibility. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

Table 10: Effect of Uber and Lyft on Monthly Motor Vehicles Fatalities and Fatal Crashes: Negative Binomial Regression (Marginal Effects)

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.03	-0.04	0.00
	(0.06)	(0.07)	(0.13)
Dependent Variable Mean	1.00	0.76	0.24
% of Mean	-3.00%	-5.26%	0.00%
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.08	-0.09	-0.06
	(0.07)	(0.07)	(0.14)
Dependent Variable Mean	1.13	0.88	0.26
% of Mean	-7.08%	-10.23%	-23.08%
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.02	-0.02	-0.00
	(0.04)	(0.03)	(0.02)
Dependent Variable Mean	1.18	0.88	0.30
% of Mean	-1.69%	-2.27%	-0.00%
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.05	-0.04	-0.01
	(0.05)	(0.04)	(0.02)
Dependent Variable Mean	1.33	1.01	0.33
% of Mean	-3.76%	-3.96%	-3.03%
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.01	†	0.00
	(0.07)		(0.05)
Dependent Variable Mean	4.95	2.35	2.60
% of Mean	0.20%	†	0.00%
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft	0.00	0.00	-0.01
	(0.08)	(0.08)	(0.05)
Dependent Variable Mean	5.30	2.55	2.75
% of Mean	0.00%	0.00%	-0.36%
N	13,062	13,062	13,062

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 4 using a negative binomial estimation instead of a Poisson estimation. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population. Data source: FARS 2006-2016.

†This specification did not converge so no estimate is reported.

Table 11: Effect of Uber and Lyft on Log of Monthly Motor Vehicles Fatalities and Fatal Crashes

	Overall (1)	Nighttime (2)	Daytime (3)
Panel A. Drunk-Driver-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.02	-0.03	-0.01
	(0.02)	(0.02)	(0.02)
Panel B. Drunk-Driver-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.04*	-0.04**	-0.01
	(0.02)	(0.02)	(0.02)
Panel C. Alcohol-Related Fatal Motor Vehicle Crashes			
Uber and/or Lyft	-0.01	-0.01	-0.00
	(0.02)	(0.02)	(0.02)
Panel D. Alcohol-Related Motor Vehicle Fatalities			
Uber and/or Lyft	-0.02	-0.03	-0.01
	(0.02)	(0.02)	(0.02)
Panel E. Overall Fatal Motor Vehicle Crashes			
Uber and/or Lyft	0.02	-0.01	0.03
	(0.02)	(0.02)	(0.03)
Panel F. Overall Motor Vehicle Fatalities			
Uber and/or Lyft	0.02	-0.01	0.02
	(0.02)	(0.02)	(0.03)
N	13,062	13,062	13,062

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Note: Results from the estimation specified in Equation 7. Controls include the monthly city unemployment rate, annual county-level % of the population that is African-American, Native American, Asian, Hispanic, male, male aged 20-24, 20-24, 25-34, 35-54, 55+, city and month-year fixed effects. Standard errors are clustered at the city level and regressions are weighted using the 2010 Census city population.
Data source: FARS 2006-2016.

A Appendix Table

Table A.1: List of 100 Most Populous U.S. Cities, by Date of Lyft/Uber Entry

City	State	Month	Year	City	State	Month	Year
Oakland	CA	July	2010	San Francisco	CA	July	2010
San Jose	CA	July	2010	New York City	NY	May	2011
Seattle	WA	August	2011	Chicago	IL	September	2011
Boston	MA	October	2011	Washington	DC	December	2011
Long Beach	CA	March	2012	Los Angeles	CA	March	2012
Philadelphia	PA	June	2012	San Diego	CA	June	2012
Fremont	CA	July	2012	Atlanta	GA	August	2012
Arlington	TX	September	2012	Aurora	CO	September	2012
Dallas	TX	September	2012	Denver	CO	September	2012
Fort Worth	TX	September	2012	Garland	TX	September	2012
Irving	TX	September	2012	Plano	TX	September	2012
Minneapolis	MN	October	2012	St. Paul	MN	October	2012
Chandler	AZ	November	2012	Gilbert	AZ	November	2012
Glendale	AZ	November	2012	Mesa	AZ	November	2012
Phoenix	AZ	November	2012	Scottsdale	AZ	November	2012
Baltimore	MD	February	2013	Sacramento	CA	February	2013
Stockton	CA	February	2013	Detroit	MI	March	2013
Indianapolis	IN	June	2013	Honolulu	HI	August	2013
Anaheim	CA	September	2013	Charlotte	NC	September	2013
Chula Vista	CA	September	2013	Irvine	CA	September	2013
Santa Ana	CA	September	2013	Oklahoma City	OK	October	2013
Tucson	AZ	October	2013	Jersey City	NJ	November	2013
Columbus	OH	December	2013	Nashville	TN	December	2013
Jacksonville	FL	January	2014	Fresno	CA	February	2014
Houston	TX	February	2014	Milwaukee	WI	February	2014
Pittsburgh	PA	February	2014	Cincinnati	OH	March	2014
Madison	WI	March	2014	San Antonio	TX	March	2014
Tulsa	OK	March	2014	Albuquerque	NM	April	2014
Cleveland	OH	April	2014	Lincoln	NE	April	2014
Louisville	KY	April	2014	Memphis	TN	April	2014
Raleigh	NC	April	2014	St. Petersburg	FL	April	2014
Tampa	FL	April	2014	Chesapeake	VA	May	2014
Colorado Springs	CO	May	2014	Kansas City	MO	May	2014
Newark	NJ	May	2014	Norfolk	VA	May	2014
Omaha	NE	May	2014	Riverside	CA	May	2014
San Bernardino	CA	May	2014	Virginia Beach	VA	May	2014
Austin	TX	June	2014	Bakersfield	CA	June	2014
Corpus Christi	TX	June	2014	Durham	NC	June	2014
El Paso	TX	June	2014	Greensboro	NC	June	2014

Hialeah	FL	June	2014	Lexington	KY	June	2014
Lubbock	TX	June	2014	Miami	FL	June	2014
Orlando	FL	June	2014	Toledo	OH	June	2014
Winston-Salem	NC	June	2014	Baton Rouge	LA	July	2014
Wichita	KS	August	2014	Anchorage	AK	September	2014
New Orleans	LA	September	2014	Reno	NV	October	2014
St. Louis	MO	October	2014	Birmingham	AL	February	2015
Portland	OR	April	2015	Fort Wayne	IN	May	2015
Henderson	NV	September	2015	Las Vegas	NV	September	2015
North Las Vegas	NV	September	2015				

Note: Buffalo, NY and Laredo, TX did not have UberX before December 31, 2016. San Juan, Puerto Rico is excluded because there are no FARS data available.
Data source: Author's hand-collected data from Uber and Lyft's websites and news articles.

Table A.2: States that Test Blood Alcohol Concentration for at Least 80% of Deceased Drivers

State (1)	FIPS Code (2)
California	6
Colorado	8
Connecticut	9
Hawaii	15
Illinois	17
Maryland	24
Massachusetts	25
Montana	30
New Hampshire	33
New Jersey	34
North Dakota	38
Ohio	39
Pennsylvania	42
Rhode Island	44
Vermont	50
Virginia	51
Washington	53
West Virginia	54

Data source: Kim et al. (2016).

Table A.3: Cities by Public Transit Accessibility

High		Medium		Low	
City (1)	Rank (2)	City (3)	Rank (4)	City (5)	Rank (6)
San Francisco, CA	1	Houston, TX	34	Scottsdale, AZ	68
Boston,	2	Santa Ana, CA	35	Omaha, NE	69
Washington, D.C.	3	Durham, NC	36	Irvine, CA	70
Jersey City, NJ	4	Cincinnati, OH	37	Fresno, CA	71
New York City, NY	5	Long Beach, CA	38	Tulsa, OK	72
Chicago, IL	6	Pittsburgh, PA	39	St. Petersburg, FL	73
Seattle, WA	7	Buffalo, NY	40	Anaheim, CA	74
Minneapolis, MN	8	Raleigh, NC	41	Fort Worth, TX	75
Philadelphia, PA	9	Milwaukee, WI	42	Reno, NV	76
Oakland, CA	10	Fremont, CA	43	Toledo, OH	77
Albuquerque, NM	11	Las Vegas, NV	44	Plano, TX	78
Baltimore, MD	12	Columbus, OH	45	Colorado Springs, CO	79
Portland, OR	13	Madison, WI	46	Chandler, AZ	80
Denver, CO	14	Virginia Beach, VA	47	Aurora, CO	81
Los Angeles, CA	15	Norfolk, VA	48	Boise, ID	82
Newark, NJ	16	San Antonio, TX	49	Hialeah, FL	83
St. Louis, MO	17	Anchorage, AK	50	Garland, TX	84
Cleveland, OH	18	Sacramento, CA	51	Winston-Salem, NC	85
Austin, TX	19	Detroit, MI	52	Mesa, AZ	86
Miami, FL	20	Charlotte, NC	53	Indianapolis, IN	87
San Jose, CA	21	Corpus Christi, TX	54	Fort Wayne, IN	88
Atlanta, GA	22	Oklahoma City, OK	55	Chula Vista, CA	89
Kansas City, MO	23	Irving, TX	56	Stockton, CA	90
Lincoln, NE	24	Greensboro, NC	57	Baton Rouge, LA	91
Orlando, FL	25	Memphis, TN	58	Lubbock, TX	92
Phoenix, AZ	26	Bakersfield, CA	59	Chesapeake, VA	93
St. Paul, MN	27	San Bernardino, CA	60	North Las Vegas, NV	94
New Orleans, LA	28	El Paso, TX	61	Henderson, NV	95
Dallas, TX	29	Jacksonville, FL	62	Arlington, TX	96
Honolulu, HI	30	Lexington-Fayette, KY	63	Birmingham, AL	97
Nashville, TN	31	Louisville, KY	64	Glendale, AZ	98
San Diego, CA	32	Riverside, CA	65	Laredo, TX	99
Tucson, AZ	33	Wichita, KS	66	Gilbert, AZ	100
		Tampa, FL	67		

Note: Author's ranking of high, medium, and low public transit accessibility based on rankings conducted by WalletHub. Data source: McCann (2019).