Do Uber and Lyft Reduce Drunk Driving Fatalities?

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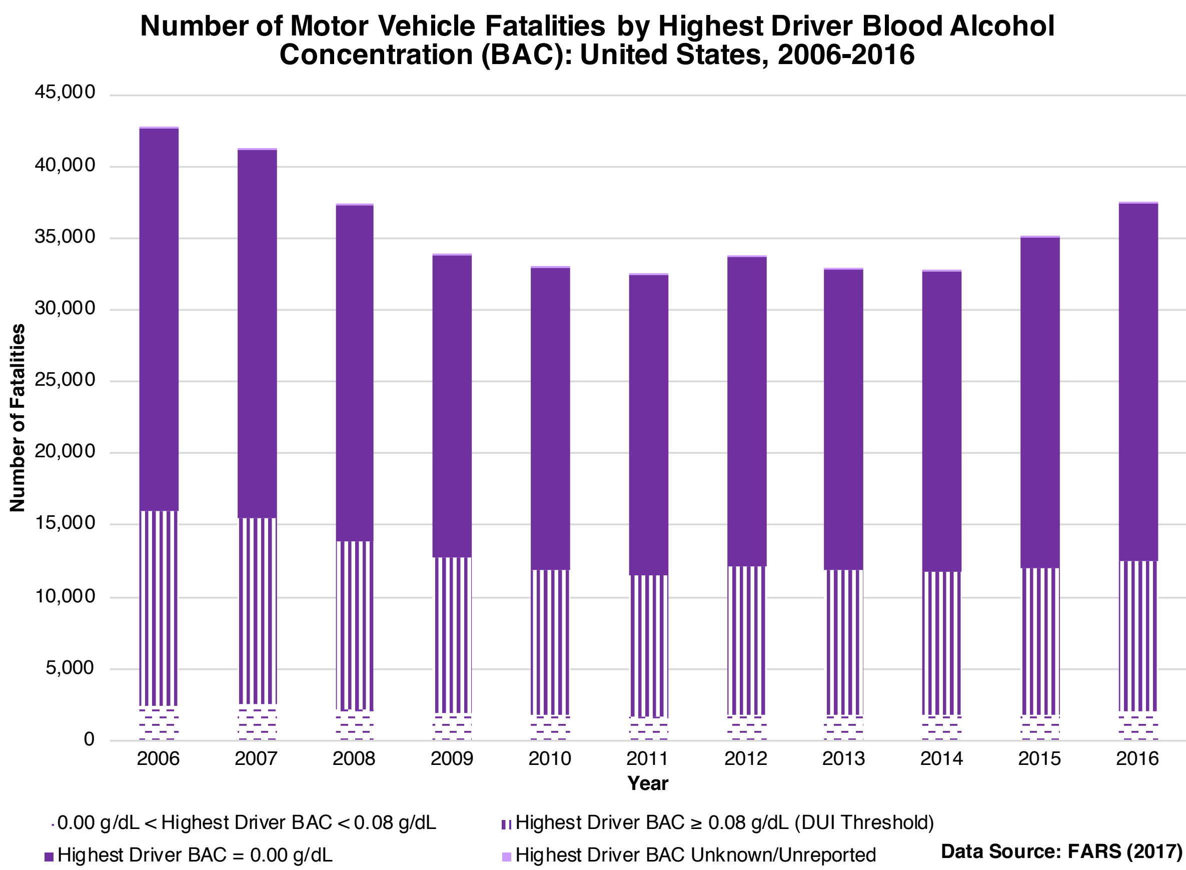
This paper investigates whether Uber and Lyft lead to reductions in drunk driving, as measured by city-level drunk-driver-related motor vehicle fatalities and fatal crashes. I use a differences-in-differences method that exploits the variation in the timing of Uber and Lyft entry for the 100 most populous U.S. cities. Using monthly city-level Fatality Analysis Reporting System (FARS) data for 2006 to 2016, I find that Lyft and Uber lead to a statistically significant reduction in nighttime drunk-driver-related fatal motor vehicle incidents. This effect appears to be driven by reductions in incidents several years after the entry of Uber or Lyft into a city.

1 Introduction

Approximately 30% of motor vehicle fatalities in the U.S. involved a legally drunk driver between 2006 and 2016 (Figure 1). In 2016 alone, 10,497 people died from a motor vehicle crash involving a legally drunk driver (National Highway Traffic Safety Administration, 2017).

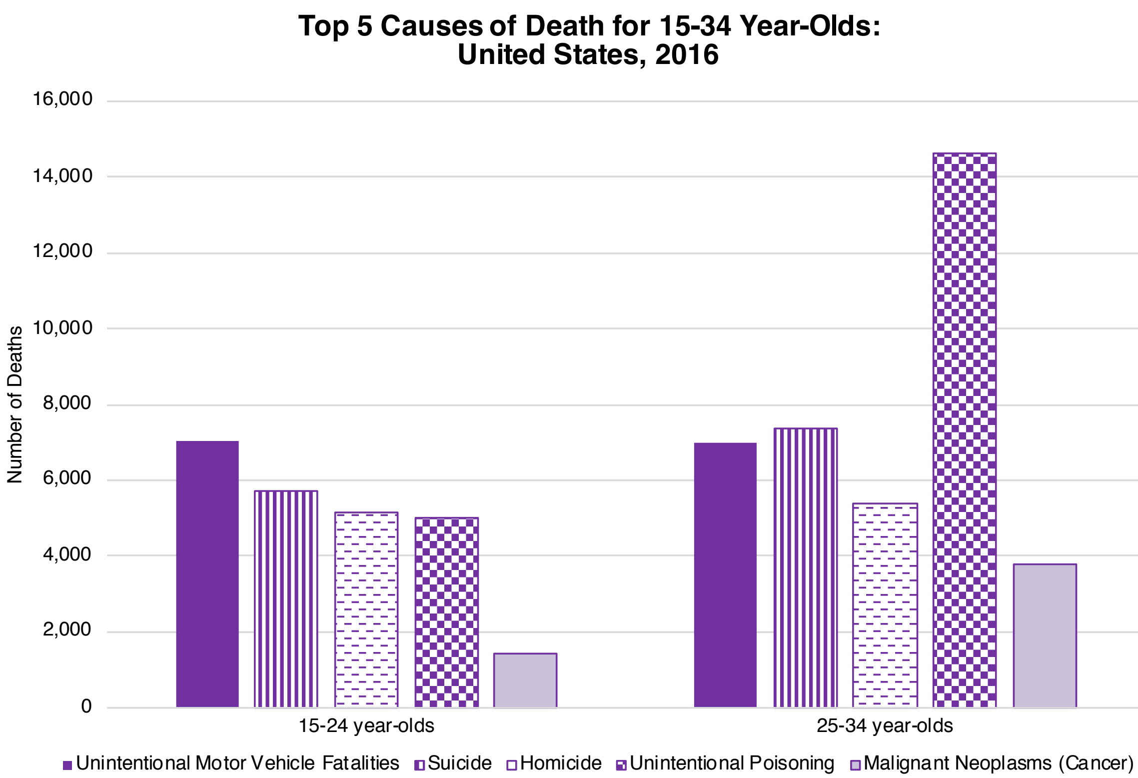
Figure 1



In addition, motor vehicle fatalities are a leading cause of death for young people

(Figure 2). Among 15 to 24 year-olds in 2016, unintentional motor vehicle fatalities were the leading cause of death; the next four leading causes of death were suicide, homicide, unintentional poisoning (includes drug and alcohol overdoses), and malignant neoplasms (cancer). 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).

Figure 2



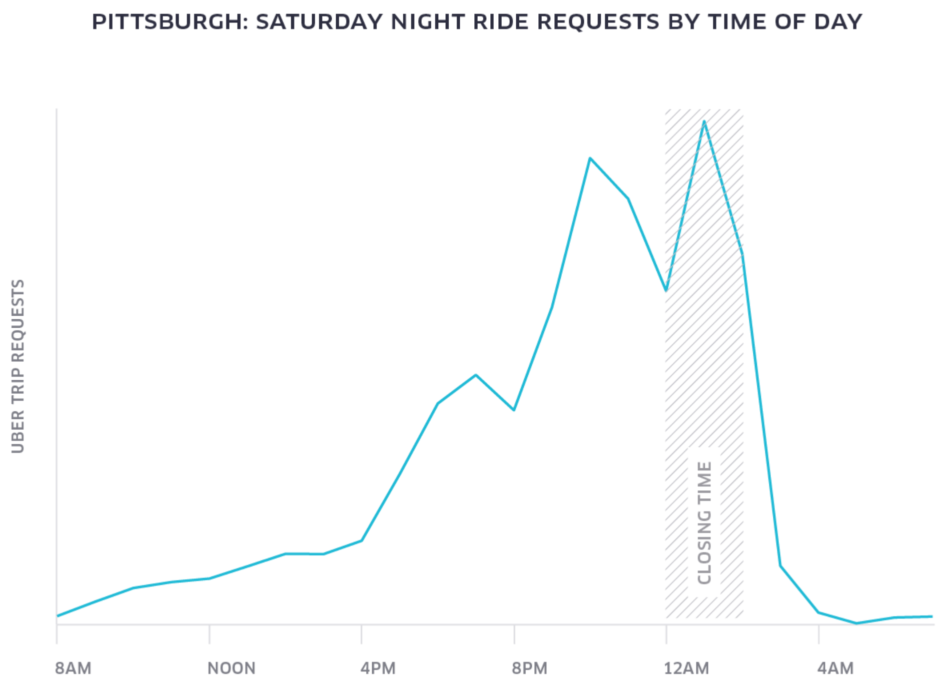
Data Source: National Center for Health Statistics, National Vital Statistics System, Centers for Disease Control (CDC). Based on tables produced by National Center for Injury Prevention and Control, CDC, using Web-based Injury Statistics Query and Reporting System (WISQARS™)

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, which has traditionally been an economic rationale for government intervention. But what if the "free market" could reduce the size of this negative externality? Uber especially loves to make 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 3). 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.

Figure 3



Source: https://www.uber.com/blog/pittsburgh/making-pennsylvania-safer-as-uber-use-goes-up-dui-rates-go-down/

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? The entry of Uber or Lyft into a city represents a reduction in the price of a substitute for drunk driving.

Figure 4

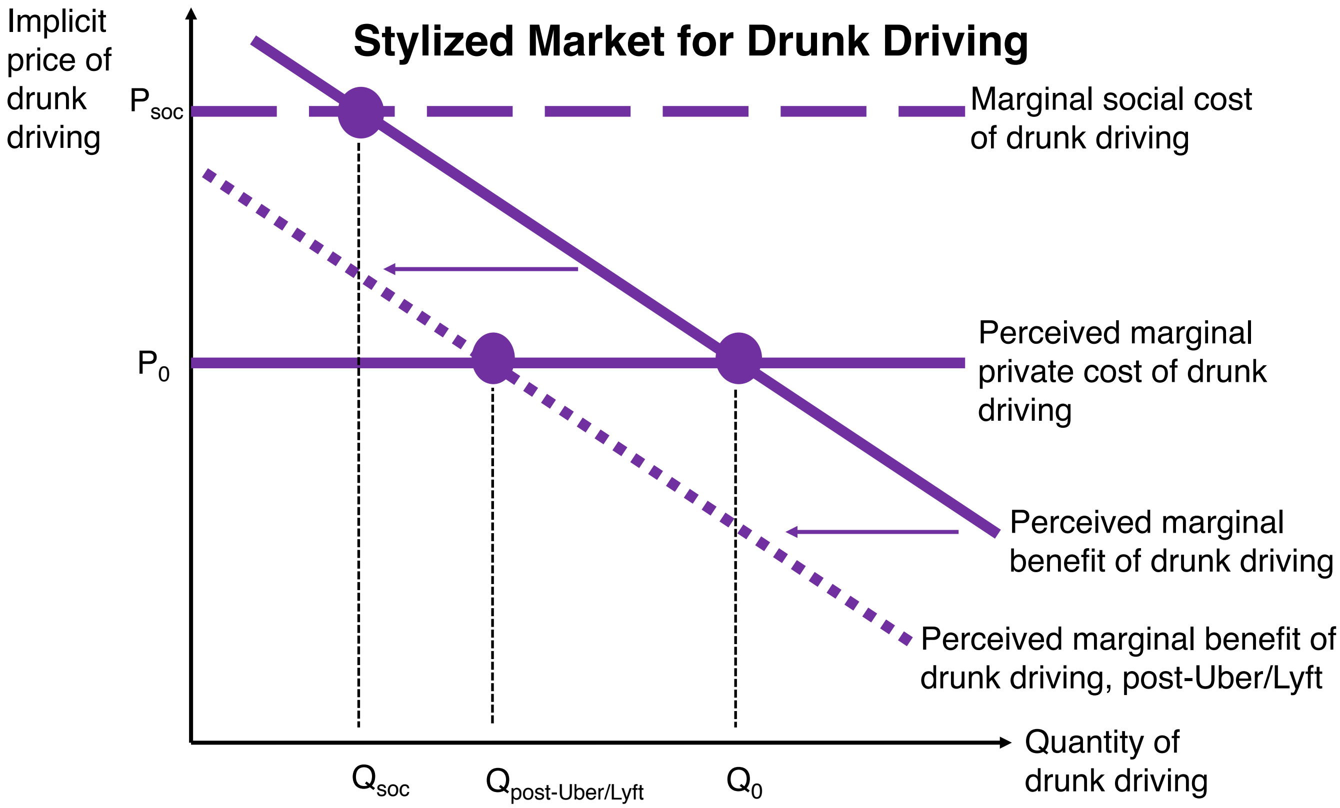


Figure 4 illustrates a simplified, hypothetical market for drunk driving, with a downward-sloping "perceived marginal benefit of drunk driving" curve, a horizontal "perceived marginal private cost of drunk driving" curve, and a much higher horizontal "marginal social cost of drunk driving" curve. The initial equilibrium quantity of drunk driving is represented by Q_0, while the socially efficient quantity of drunk driving is much lower and represented by Q_{soc} (Note: this illustration is stylized, and it is entirely possible that the actual marginal social cost of drunk 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). 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 $Q_{post-Uber/Lyft}$.

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 may also be more cars on the road post-Uber and Lyft entry. Both of these effects may lead to an increase in the quantity of drunk driving, making 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.

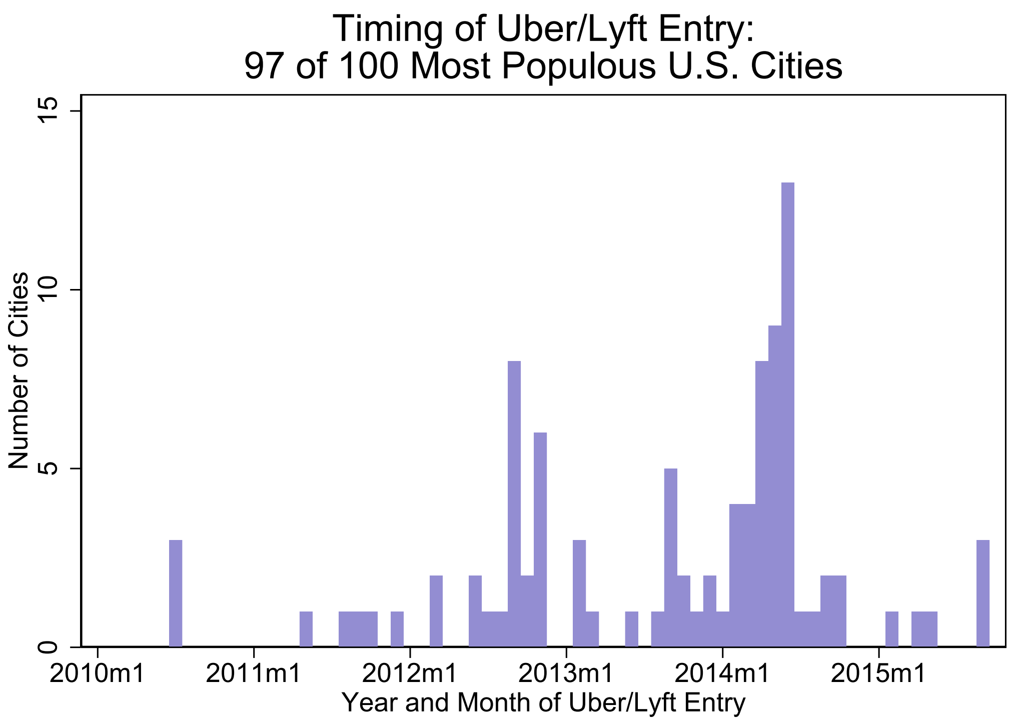
Uber and Lyft are ridesharing services that operate through smartphone apps. Riders open the app, select their pickup location on a map, and request a ride. The driver transports the rider to the rider's destination. The apps require a credit card on file, and the app automatically charges the rider's 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 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 differences-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 5 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, but this particular source of endogeneity does not appear to be relevant.

Figure 5



This paper contributes to the nascent literature on the effect of ridesharing on drunk driving. In the paper most similar to mine, Martin-Buck (2017 working paper) estimates the impact of ridesharing on drunk driving for all U.S. cities with a population of at least 100,000. He finds that for the period 2000-2014, ridesharing leads to reductions in drunk-driving-related crashes. Brazil and Kirk (2016) use a differences-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 differences-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 working paper) finds a 25-35% reduction in the alcohol-related crash rate in New York City.

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. Uber and Lyft entry happens at the city level, and given the geographic mismatch between cities and counties, city-level outcomes are either a more accurate measure of treatment or an alternative measure. Third, compared to the other papers on ridesharing, (Brazil and Kirk, 2016; Dills and Mulholland, 2018; Martin-Buck, 2017 working paper) 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. There appear to be longer-term effects of Uber and Lyft on drunk-driver-related fatal incidents, with larger effects for nighttime incidents. However, when I restrict the sample to cities in the 18 states that test over 80% of deceased drivers, I find that Uber and Lyft are associated with statistically significant increases in daytime drunk-driver-related incidents.

The remainder of the paper proceeds as follows: Section 2 outlines the data, Section 3 details the method I use, Section 4 presents the results of the differences-in-differences estimation, Section 5 incorporates some robustness checks, and Section 6 concludes.

2 Data

2.1 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 fatal motor vehicle crashes on public roadways in the United States (50 states and Washington, D.C.). 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 recorded blood alcohol concentration 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).

To address the possibility that the availability of ridesharing lowers the price of drinking and this induces individuals to consume more alcohol, 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.

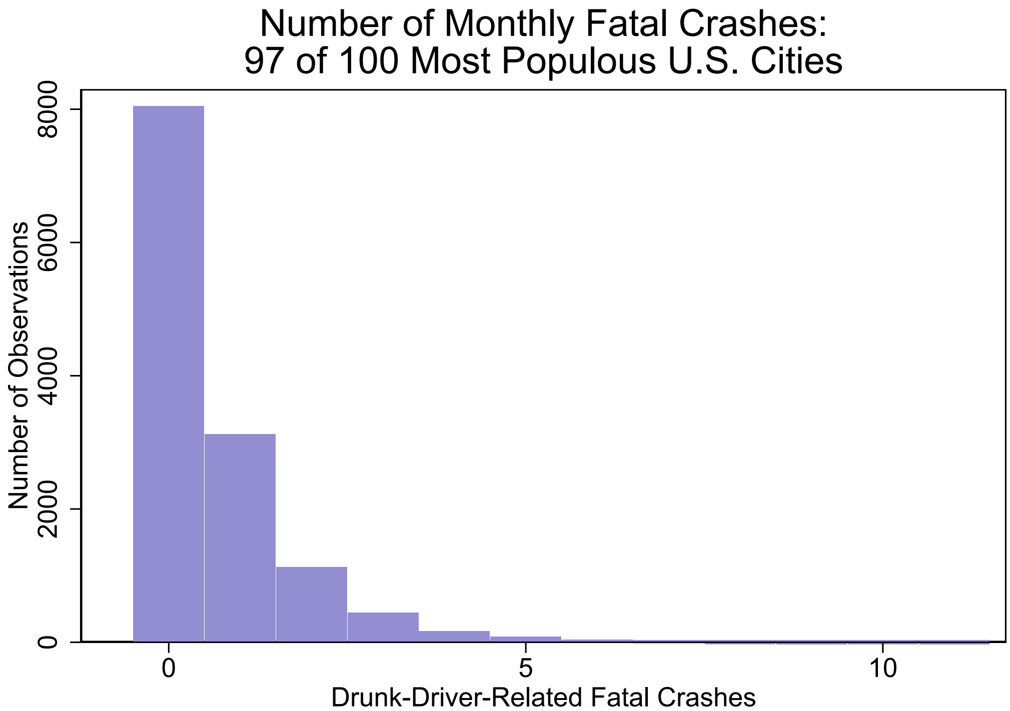
If choosing between drunk driving and taking an Uber or Lyft is the relevant choice for individuals, then I would expect to see changes in drunk driving. However, if the relevant choice is between taking a Lyft or Uber home and walking home drunk, then an analysis of Uber and Lyft entry on drunk driving would not pick up the true effect of Uber and Lyft on drunk transportation choices.

To address whether Lyft and Uber lead to an increase in the number of cars on the road, I also 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 6 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.

Figure 6



There are more drunk-driver related crashes at night compared to during the day. The fatality to fatal crash ratio is slightly higher for nighttime drunk-driver-related crashes (0.86/0.75 = 1.15) relative to daytime drunk-driver-related crashes (0.25/0.23 = 1.09).

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. 1.08). In this sample of cities, drunk-driver-related crashes and fatalities

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

Variable	Mean	Std. Dev.	Min.	Max.	N
Drunk Driver	**0.98**	1.47	0	11	13,068
Nighttime	0.75	1.20	0	9	13,068
Daytime	0.23	0.55	0	4	13,068
Alcohol-Related	**1.14**	1.69	0	13	13,068
Nighttime	0.85	1.34	0	9	13,068
Daytime	0.29	0.64	0	5	13,068
Total	**4.94**	5.65	0	34	13,068
Nighttime	2.34	2.91	0	20	13,068
Daytime	2.60	3.20	0	21	13,068

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. Statistics are weighted by the 2010 Census city population. Drunk driver means at least one driver was recorded as having a BAC $\geq$ 0.08 g/dL. Alcohol-related means at least one driver was recorded as having a BAC $>$ 0.00 g/dL.
Data Source: FARS

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

Variable	Mean	Std. Dev.	Min.	Max.	N
Drunk Driver	**1.11**	1.72	0	12	13,068
Nighttime	0.86	1.42	0	10	13,068
Daytime	0.25	0.63	0	12	13,068
Alcohol-Related	**1.29**	1.95	0	14	13,068
Nighttime	0.98	1.58	0	10	13,068
Daytime	0.32	0.72	0	12	13,068
Total	**5.29**	6.05	0	35	13,068
Nighttime	2.55	3.20	0	21	13,068
Daytime	2.74	3.40	0	22	13,068

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. Statistics are weighted by the 2010 Census city population. Drunk driver means at least one driver was recorded as having a BAC $\geq$ 0.08 g/dL. Alcohol-related means at least one driver was recorded as having a BAC $>$ 0.00 g/dL.
Data Source: FARS

make up 86% 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 the ratio of fatalities to fatal crashes is 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 in month-year $m-y$ can be represented by the following equation:

$$crashes_{imy} = \frac{crashes_{imy}}{vehicle\ mile\ driven_{imy}} * (\#\ vehicle\ miles\ driven_{imy}) \tag{1}$$

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. Two, that Uber and Lyft drivers are better (worse) drivers than average, leading to a reduction (increase) in the crash rate per vehicle mile driven, but they also lead to an increase (reduction) in the number of vehicle miles driven, which exactly offsets the reduction (increase) in the crash rate. If the first scenario were true, it would imply that Lyft and Uber completely crowd out 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, implying demand for transportation is perfectly inelastic.

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

It is unclear how precise the imputed BAC tests are. Further, it seems reasonable to assume that the accuracy of the imputation may be positively correlated with the fraction of drivers whose BAC is tested. Therefore, I employ a robustness check where I restrict the sample to the 18 states that test over 80% of deceased drivers. See section 5.1 for more details.

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 could be a desirable policy outcome. Consequently, the effect of Lyft and Uber on non-fatal crashes is beyond the scope of this paper.

2.2 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.

2.3 Control Variables

I acquire monthly city-level unemployment data from the Bureau of Labor Statistics. The first six months of unemployment data for 2006 are missing for New Orleans. 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 below.

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

Variable	Mean	Std. Dev.	Min.	Max.	N
= 1 if Uber/Lyft	**0.30**	0.46	0	1	13,068
UE Rate (%)	6.98	3.05	1.50	28.40	13,062
% African-American†	17.11	14.55	0.21	65.62	13,068
% Asian†	7.70	9.21	0.54	71.10	13,068
% Hispanic†	23.34	18.77	1.27	95.73	13,068
% White†	51.08	17.36	3.47	87.47	13,068
% Male†	49.07	0.96	46.89	52.19	13,068
% Male 20-24†	3.91	0.93	2.54	9.52	13,068
% 20-24†	7.71	1.59	5.05	15.53	13,068
% 25-34†	15.29	2.05	10.61	23.42	13,068
% 35-54†	27.25	1.90	19.13	32.23	13,068
% 55+†	22.73	3.28	14.48	39.65	13,068
2010 Pop.	**602,413**	920,240	208,453	8,175,133	13,068

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 unemployment rate variable is missing the first 6 months of 2006 for New Orleans.
† refers to county population
Data sources: Uber, news articles, BLS, SEER, Census

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 one buys the assumption that city population is not changing much month-to-month, the annual population data will be a good approximation of the monthly population.

3 Model, Identification & Methods

3.1 Model of Individual's Decision to Drive After Drinking

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

$$Prob(DD) = f(P_{DD}, P_{complements}, \mathbf{P_{substitutes}}, alc) \tag{2}$$

$Prob(DD)$ 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 (walking, bicycling, taking public transit, hailing a taxi, **using Uber or Lyft**). alc represents alcohol consumption: if the individual has already decided to drive, increasing alc increases the probability of driving drunk. Note that risk aversion affects the perceived risks of being arrested and crashing. Also note that the distance to travel affects the relative prices of drunk driving and its substitutes.

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

$$DD\ fatalities = f(miles\ DD, \frac{fatal\ crash\ rate}{mile}, \frac{fatalities}{fatal\ crash}) \tag{3}$$

$DD\ fatalities$ represent drunk driving fatalities. $miles\ DD$ represent miles driven drunk. Increasing $\mathbf{Prob(DD)}$ leads to an increase in $\mathbb{E}[\mathbf{miles\ DD}]$. Note that I am able to observe the left-hand side of equation 3 as well as $\frac{fatalities}{fatal\ crash}$.

I estimate a reduced-form equation of equation 3:

$$DD\ F = f(ridesharing, UE\ rate, pop.\ characteristics, city\ +\ time\ FE) \tag{4}$$

$UE\ rate$ represents the unemployment rate. *pop. characteristics* represents population characteristics. $city\ +\ time\ FE$ represent city and time fixed effects. Note that I am making the assumption that within-city alcohol consumption is time-invariant.

3.2 Differences-in-Differences Identification and Assumptions

I estimate a differences-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:

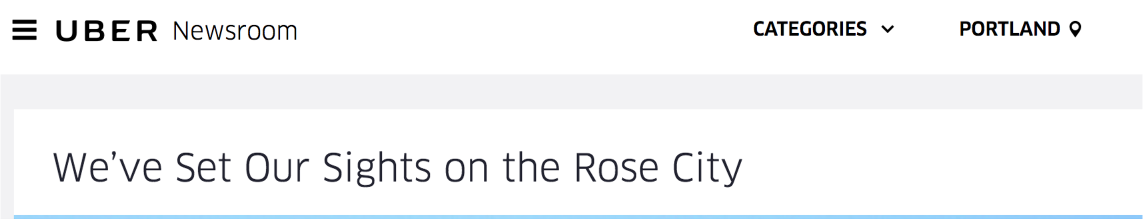
1. There are no other concurrent changes at the time of Lyft or Uber's entry into the treated cities that affect motor vehicle fatalities

2. *Parallel trends:* in the absence of Uber and Lyft, trends in motor vehicle fatalities and fatal crashes would be the same across treatment and control cities

As with all differences-in-differences studies, the greatest threat to identification is policy endogeneity. If Uber and Lyft are not entering cities as-if randomly, and are in fact systematically targeting cities with higher rates of drunk driving, then the differences-in-differences model's results would be biased. However, if the pre-implementation trends in the outcome variables are the same in cities with and without Lyft or Uber, it would strengthen my claim of policy exogeneity.

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.

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.



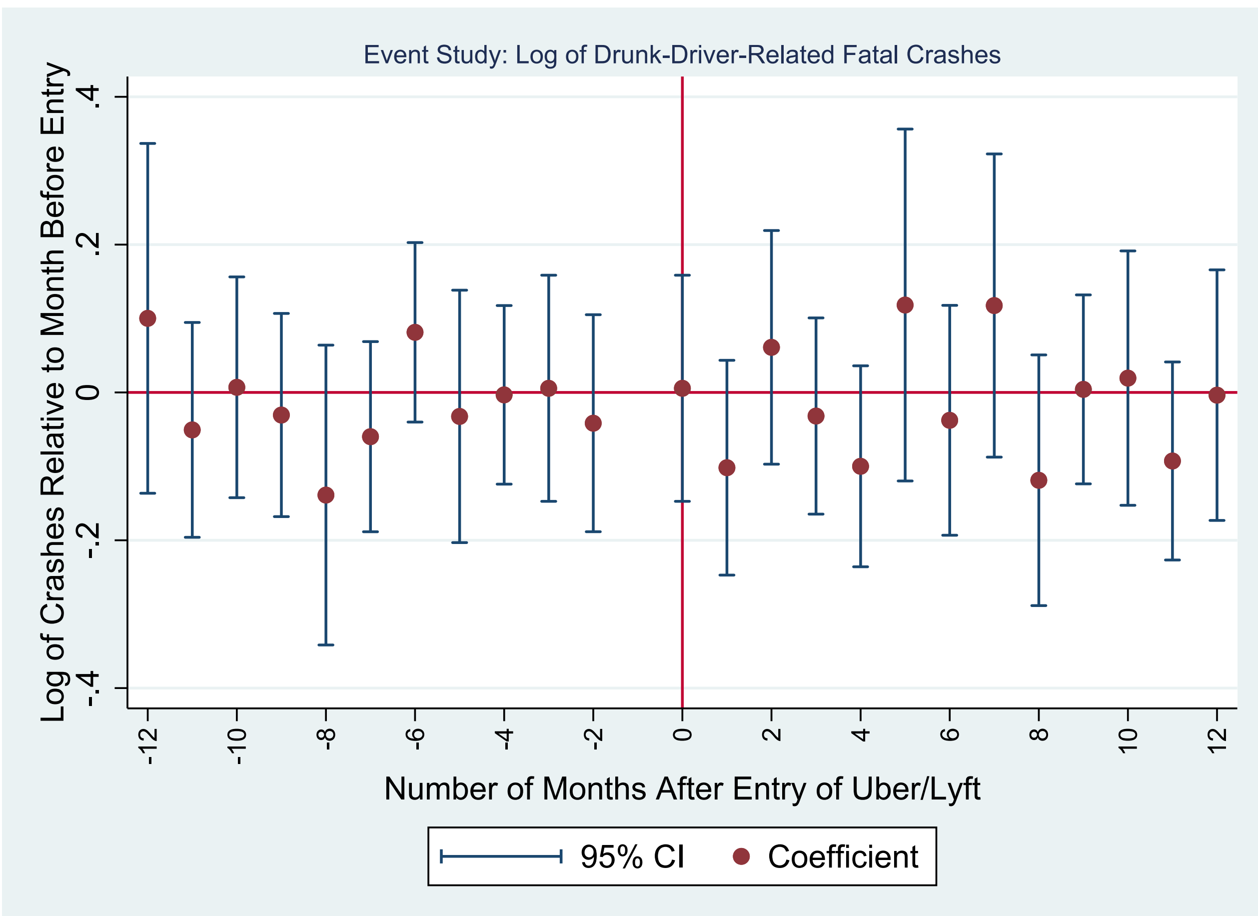
https://newsroom.uber.com/us-oregon/hello_portland/

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 wasn't 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 is likely not correlated with trends in drunk driving fatal incidents.

To provide further evidence against policy endogeneity, I conduct an event study to test the parallel trends assumption. Figure 7 plots the log of drunk-driver-related fatal crashes relative to the month prior to Lyft/Uber entry, conditional on the controls (which are detailed below in the Method subsection).

Figure 7



As seen in Figure 7, there is neither a clear nor statistically significant pre-trend in the log of drunk-driver-related fatal crashes. After controlling for city-level characteristics, city fixed effects, and time fixed effects, Uber and Lyft do not appear to be systematically timing their entry into cities with trends in drunk-driver-related fatal motor vehicle crashes. They are not entering at a time when drunk-driver-related crashes are becoming more or less pervasive, which provides suggestive evidence that policy endogeneity is not biasing my results. This pattern is not unique to a 12-month window: there are still no pre-trends when I extend the pre-period window to 24 months or to the entire sample period (see Appendix Figures $A.1$ and $A.2$).

3.3 Reduced-Form Drunk Driving Equation

The differences-in-differences model is a cluster-robust population-weighted Ordinary Least Squares model with city and month-year fixed effects. The main specification is equation 5.

$$log(F_{imy} + 1) = \alpha + \beta \cdot Ride_{imy} + \gamma \cdot uerate_{imy} + \mathbf{X}'_{iy} \cdot \theta + \eta_i + \delta_{my} + \varepsilon_{imy} \qquad (5)$$

$log(F_{imy} + 1)$ 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. $Ride_{imy}$ represents a monthly city-level indicator for the presence of Uber or Lyft. $uerate_{imy}$ represents the monthly city-level unemployment rate. $\mathbf{X}'_{iy}$ represents a vector of population characteristics: annual county-level 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_{my} represents time fixed effects. ε_{imy} represent the standard errors, which are clustered at the city level. I weight all regressions using the 2010 Census city population.

4 Differences-in-Differences Results

I first estimate the impact of Uber and Lyft on the log of fatal drunk-driver-related motor vehicle crashes, as shown in Table 4. 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 Lyft or Uber in a city is associated with approximately a 2% decline in drunk-driver-related motor vehicle crashes, but this decline is not statistically significant. For nighttime drunk-driver-related fatal crashes (crashes recorded as occurring between 8 p.m. and 4 a.m.), Uber and Lyft also lead to an approximate 2% decrease in such crashes, but again this decline is not statistically significant. Daytime drunk-driver-related crashes declined by roughly 1%, although this decline is not statistically significant.

Table 4: Effect of Uber and Lyft on Log of **Drunk-Driver-Related** Fatal Motor Vehicle **Crashes**

	Overall	Nighttime	Daytime
Uber and/or Lyft	-0.02	-0.02	-0.01
	(0.02)	(0.02)	(0.02)
R^2	0.53	0.48	0.24
N	13,062	13,062	13,062

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

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

Turning to the results for the log of drunk-driver-related motor vehicle fatalities (Table 5), I find that the presence of Lyft or Uber leads to a 3% decline in such fatalities, which is not statistically significant. This decline is driven by nighttime drunk-driver-related motor vehicle fatalities, which declined by 4% after Uber or Lyft entered a city. The decline in nighttime fatalities is statistically significant at the 5% level. Daytime fatalities declined by 1%, although this difference is not statistically significant.

Table 5: Effect of Uber and Lyft on Log of **Drunk-Driver-Related** Motor Vehicle **Fatalities**

	Overall	Nighttime	Daytime
Uber and/or Lyft	-0.03	-0.04**	-0.01
	(0.02)	(0.02)	(0.02)
R^2	0.51	0.46	0.24
N	13,062	13,062	13,062

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

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

The results for alcohol-related fatal motor vehicle crashes are less economically and statistically significant (Table 6). A fatal crash is defined as alcohol related if the highest recorded Blood Alcohol Concentration of any involved driver was nonzero but below the legal limit (0.08 g/dL). The presence of Lyft or Uber leads to no decline in alcohol-related fatal motor vehicle crashes, which is not statistically significant. Uber or Lyft lead to a 1% decline in nighttime alcohol-related fatal crashes and no decline in daytime alcohol-related fatal crashes, although neither of these coefficients are statistically significant.

Table 6: Effect of Uber and Lyft on Log of **Alcohol-Related** Fatal Motor Vehicle **Crashes**

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.00	-0.01	0.00
	(0.02)	(0.02)	(0.02)
R^2	0.56	0.51	0.28
N	13,062	13,062	13,062

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

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

The results for alcohol-related motor vehicle fatalities (Table 7) are similar to those for crashes. Uber or Lyft leads to a 2% decline in alcohol-related motor vehicle fatalities, although this decline is not statistically significant. They also lead to a 2% reduction in nighttime alcohol-related fatalities and a 1% decline in daytime alcohol-related fatalities, although neither of these coefficients are statistically significant.

Table 7: Effect of Uber and Lyft on Log of **Alcohol-Related** Motor Vehicle **Fatalities**

	Overall	Nighttime	Daytime
Uber and/or Lyft	-0.02	-0.02	-0.01
	(0.02)	(0.02)	(0.02)
R^2	0.54	0.50	0.27
N	13,062	13,062	13,062

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

For all fatal motor vehicle crashes (Table 8), some of the coefficients become positive. Uber or Lyft leads to a 2% increase in overall fatal motor vehicle crashes, a 1% decrease in nighttime fatal motor vehicle crashes, and a 3% increase in daytime fatal motor vehicle crashes. None of these estimates are statistically significant, however. The coefficients on motor vehicle fatalities (Table 9) are virtually identical: Lyft or Uber leads to a 2% increase in overall 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.

Table 8: Effect of Uber and Lyft on Log of Fatal Motor Vehicle **Crashes**

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.02	-0.01	0.03
	(0.02)	(0.02)	(0.03)
R^2	0.77	0.68	0.66
N	13,062	13,062	13,062

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

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

Table 9: Effect of Uber and Lyft on Log of Motor Vehicle **Fatalities**

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.02	-0.01	0.02
	(0.02)	(0.02)	(0.03)
R^2	0.76	0.67	0.65
N	13,062	13,062	13,062

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

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

5 Extensions

5.1 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. However, 18 states test at least 80% of drivers who died in the motor vehicle crash (Kim et al, 2016). While FARS imputes the BAC for the drivers who weren't tested, there may be measurement error in the imputed BAC.

When I restrict the sample to these 18 states, the coefficients for drunk-driver-related fatal motor vehicle crashes (Table 10), become positive. Uber or Lyft is associated with a 7% increase in overall drunk-driver-related fatal motor vehicle crashes, which is marginally statistically significant (10% level). They are associated with a 1% increase in nighttime drunk-driver-related fatal motor vehicle crashes, although this increase is not statistically significant. Daytime drunk-driver-related fatal crashes increased by 6%, which is statistically significant at the 5% level.

Table 10: Effect of Uber and Lyft on Log of **Drunk-Driver-Related** Fatal Motor Vehicle **Crashes** for Majority Testing States

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.07*	0.01	0.06**
	(0.04)	(0.04)	(0.03)
R^2	0.59	0.53	0.31
N	4,752	4,752	4,752

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

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

Turning to drunk-driver-related motor vehicle fatalities (Table 11), Uber or Lyft is associated with a 6% increase in overall drunk-driver-related fatalities, which is not statistically significant. They are associated with a 1% increase in nighttime drunk-driver-related fatalities and a 6% increase in daytime drunk-driver-related fatalities. The former is not statistically significant while the latter is statistically significant at the 5% level.

Table 11: Effect of Uber and Lyft on Log of **Drunk-Driver-Related** Motor Vehicle **Fatalities** for Majority Testing States

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.06	0.01	0.06**
	(0.04)	(0.04)	(0.03)
R^2	0.58	0.52	0.31
N	4,752	4,752	4,752

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

5.2 Differences-in-Differences with Linear Time Trend

As a second extension, to account for the possibility that the effect of Uber and Lyft on motor vehicle fatalities may depend on how long they have been in a city, I estimate a second model with an interaction between Uber or Lyft and a linear time trend. This specification is motivated by the fact that I expect the number of drivers and rides to increase in a city over time, and also due to previous work's findings of delayed effects of Uber (Greenwood and Wattal, 2017; Dills and Mulholland, 2018). Let k equal the number of months Uber or Lyft has been in a given city. I estimate the following equation:

$$log(F_{imy} + 1) = \alpha + \beta_1 Ride_{imy} + \beta_2 (Ride * k)_{imy} + \mathbf{X}'_{iy}\gamma + \eta_i + \delta_m y + \varepsilon_{imy} \quad (6)$$

β_1 can be interpreted as the change in the intercept, or the baseline percentage change in motor vehicle fatalities due to Uber and Lyft, while β_2 can be interpreted as the change in the slope, or the additional percentage change in motor vehicle fatalities per month.

Table 12: Effect of Uber and Lyft on Log of **Drunk-Driver-Related** Fatal Motor Vehicle **Crashes**

	Overall	Nighttime	Daytime
Uber and/or Lyft	-0.001	-0.008	-0.004
	(0.027)	(0.018)	(0.021)
(Uber and/or Lyft)*k	-0.005**	-0.005**	-0.002***
	(0.002)	(0.002)	(0.001)
R^2	0.53	0.48	0.24
N	13,062	13,062	13,062

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

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

For drunk-driver-related crashes (Table 12), there is a 0.1% reduction in the overall baseline, with a decrease in crashes of 0.5% per additional month of Uber or Lyft.

The coefficient on the baseline is not statistically significant but the coefficient on the trend is statistically significant at the 5% level. For nighttime drunk-driver-related crashes, the baseline in crashes falls by 0.8%, with a decrease of 0.5% per additional month of Uber or Lyft. Only the coefficient on the trend is statistically significant (5% level). For daytime drunk-driver-related crashes, Uber and Lyft are associated with a baseline decrease in crashes of 0.4% with an additional 0.2% decrease in crashes per additional month of Uber or Lyft. The coefficient on the trend is statistically significant at the 1% level.

Table 13: Effect of Uber and Lyft on Log of **Drunk-Driver-Related** Motor Vehicle **Fatalities**

	Overall	Nighttime	Daytime
Uber and/or Lyft	-0.018	-0.023	-0.009
	(0.029)	(0.019)	(0.022)
(Uber and/or Lyft)*k	-0.005**	-0.005**	-0.002***
	(0.002)	(0.002)	(0.001)
R^2	0.52	0.47	0.24
N	13,062	13,062	13,062

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

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

Table 13 shows the results for drunk-driver-related motor vehicle fatalities. The baseline effect for drunk-driver-related fatalities is a 1.8% decrease, with a 0.5%

decrease for each additional month of Uber and Lyft. The baseline effect is not statistically significant but the trend is statistically significant at the 5% level. For nighttime drunk-driver-related motor vehicle fatalities, Uber and Lyft are associated with a 2.3% reduction in fatalities at the baseline, and an additional 0.5% reduction in fatalities per month of Uber or Lyft. Only the trend coefficient is statistically significant (5% level). For daytime drunk-driver-related motor vehicle fatalities, Uber and Lyft are associated with a baseline reduction in fatalities of 0.9%, with a decline of 0.2% for each additional month of Uber or Lyft. Again, only the coefficient on the trend is statistically significant (1% level).

Overall, the results for drunk-driver-related crashes and fatalities are generally consistent with the simple differences-in-differences specification, in the sense that in both cases Uber and Lyft are associated with declines in fatal motor vehicle incidents, largely driven by the reduction in nighttime incidents. However, in the simple differences-in-differences specification, only the coefficient for nighttime drunk-driver-related fatalities is statistically significant, whereas in the linear time trend model, the coefficient on the trend is statistically significant for each drunk-driver-related specification. For the results for alcohol-related and all motor vehicle fatalities and fatal crashes, see Appendix Tables $A.2$, $A.3$, $A.4$, and $A.5$.

Restricting attention to the 18 states that majority test deceased drivers, the

Table 14: Effect of Uber and Lyft on Log of **Drunk-Driver-Related** Fatal Motor Vehicle **Crashes** for **Majority Testing States**

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.070*	0.011	0.062**
	(0.036)	(0.036)	(0.026)
(Uber and/or Lyft)*k	-0.005**	-0.006***	0.000
	(0.002)	(0.002)	(0.001)
R^2	0.59	0.53	0.31
N	4,752	4,752	4,752

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

results are broadly consistent with the simple differences-in-differences specification and the linear time trend model for the entire sample. The baseline effect for drunk-driver-related fatal crashes is a 7% increase, which is marginally statistically significant at the 10% level (Table 14). The coefficient on the trend is -0.5%, which is statistically significant at the 5% level. For nighttime drunk-driver-related fatal crashes, the baseline coefficient is a statistically insignificant 1.1% increase, with a 0.6% decrease for each additional month of Uber or Lyft. The coefficient on the trend is statistically significant at the 1% level. The baseline coefficient on daytime drunk-driver-related fatal crashes is 6.2%, which is statistically significant at the 5% level. There is no change for each additional month of Lyft or Uber.

Table 15: Effect of Uber and Lyft on Log of **Drunk-Driver-Related** Motor Vehicle **Fatalities** for **Majority Testing States**

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.061	0.007	0.058**
	(0.037)	(0.037)	(0.028)
(Uber and/or Lyft)*k	-0.005*	-0.006***	0.000
	(0.003)	(0.002)	(0.001)
R^2	0.58	0.52	0.31
N	4,752	4,752	4,752

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

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

For drunk-driver-related motor vehicle fatalities (Table 15), the baseline coefficient is 6.1%, with a 0.5% decline for each additional month of Uber or Lyft. The baseline coefficient is not statistically significant but the trend coefficient is marginally significant (10% level). For nighttime drunk-driver-related fatalities, the baseline coefficient is 0.7% (not statistically significant) and the trend is a 0.6% decline (statistically significant at the 1% level). For daytime drunk-driver-related fatalities, the baseline coefficient is a statistically significant 5.8% (5% level) while there is no change for each additional month of Lyft or Uber.

5.3 Dynamic Effects (Non-Parametric Specification)

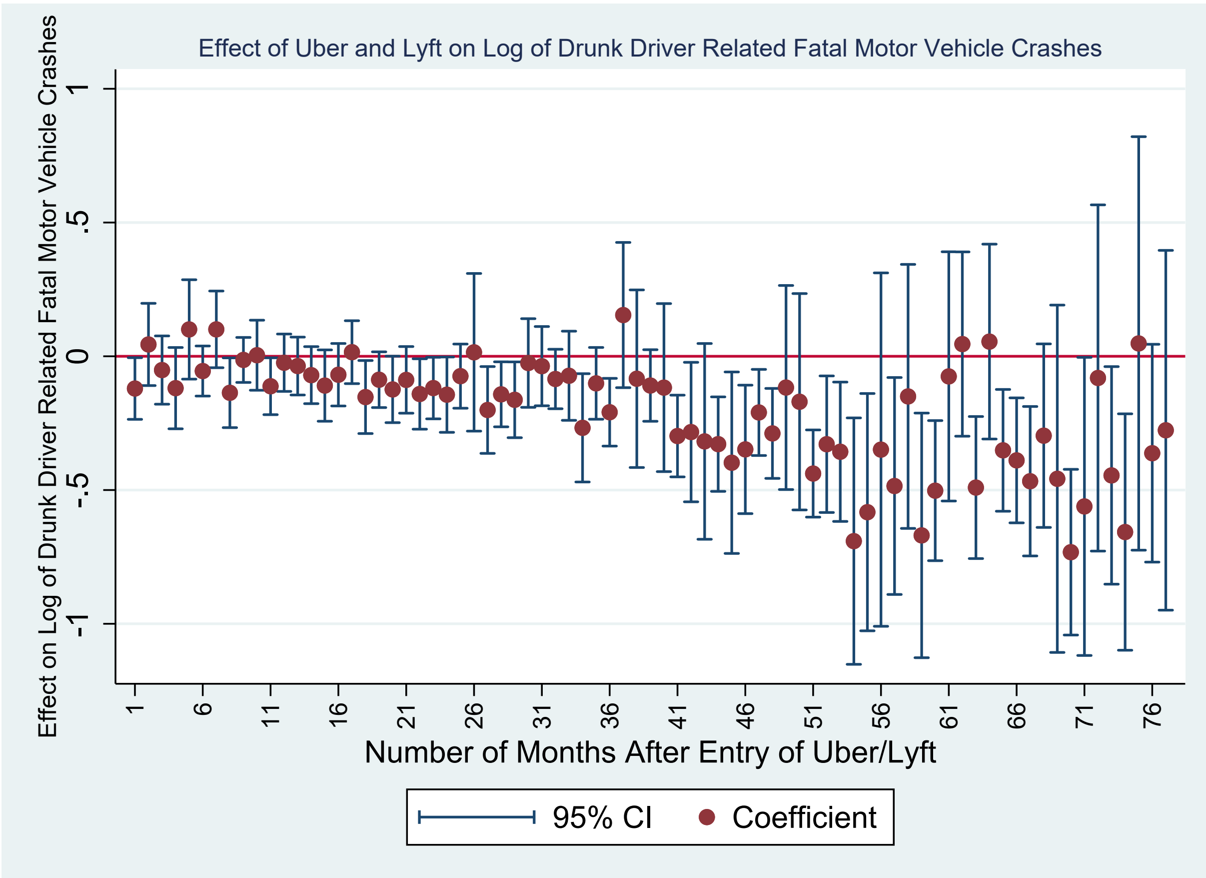
As another extension, I relax the assumption of a linear trend in equation 6 by allowing the coefficient to vary for each month that Uber or Lyft has been in place. This third model is a dynamic effects specification that is equivalent to an event study that has been restricted to the post period (as opposed to pre and post).

$$log(F_{imy} + 1) = \alpha + \sum_{k \geq 1} \beta_k Ride_{kimy} + \mathbf{X}'_{iy}\gamma + \eta_i + \delta_m y + \varepsilon_{imy} \tag{7}$$

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

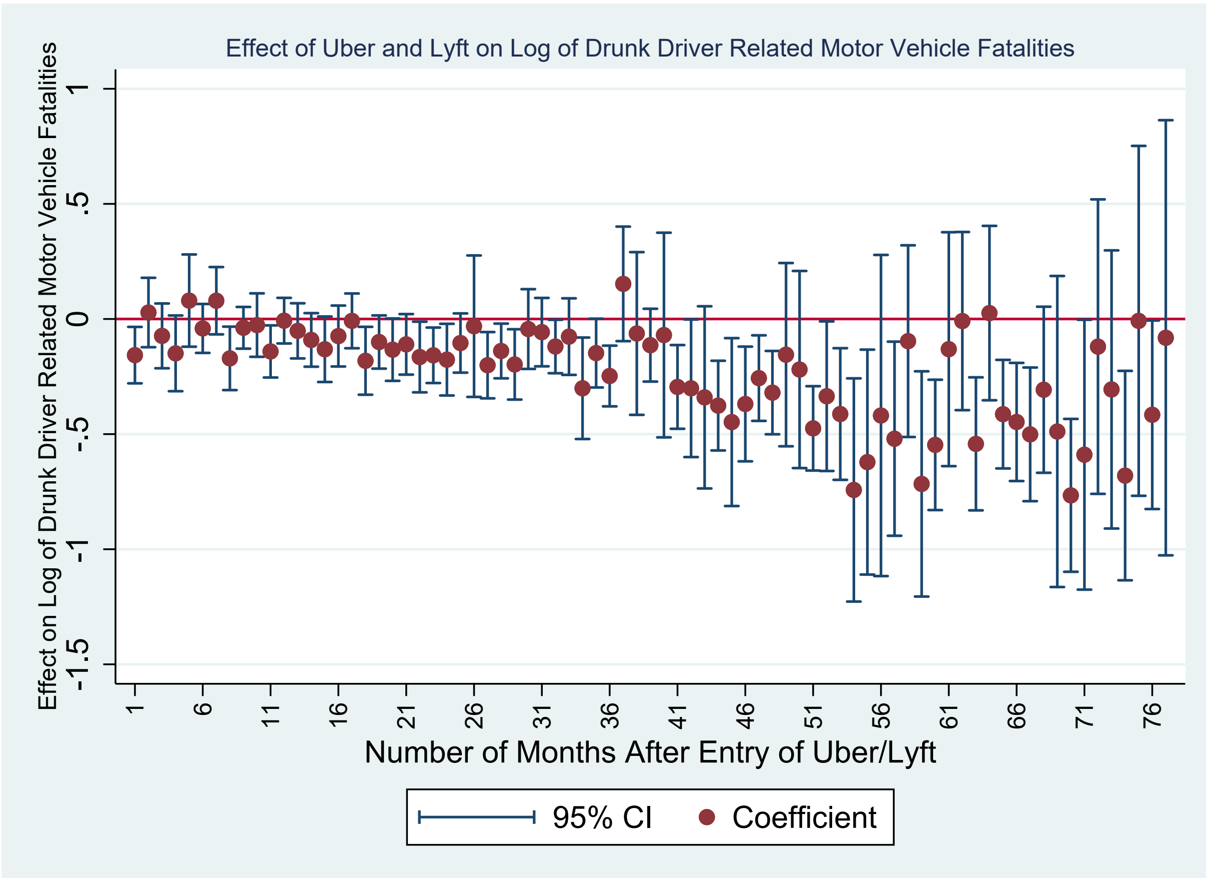
When I relax the assumption of a linear trend, the results are qualitatively similar. For drunk-driver-related crashes, shown in Figure 8, the coefficients are small and not statistically significantly different from 0 in the first 2-3 years (24 to 36 months). After 3 years though, the coefficients become larger in magnitude, negative in sign, and tend to be statistically significantly different from 0, although the estimates are noisier (as shown by the larger bands for the 95% confidence intervals). It's worth noting that the estimates are noisier after 3 years because there are fewer observations (fewer cities had had Lyft or Uber for more than 3 years as of 2016).

Figure 8



In addition, the early adopters of ridesharing were much larger cities in terms of population (San Francisco, New York City, Washington D.C., Los Angeles, Chicago, etc.). Because the regressions in the main specification and the linear time trend extension are population weighted, it's possible that the effects of Uber and Lyft for these larger cities are driving the results for the regressions.

Figure 9



The results for drunk-driver-related fatalities are virtually identical to those for crashes (see Figure 9). The coefficients are close to 0 for the first several years, then gradually tend toward a statistically significant decline, although the estimates for the later months are much noisier, likely due to the smaller number of observations.

The results for nighttime and daytime drunk-driver-related crashes and fatalities are similar to the results for all hours of the day; see Appendix Figures $A.3$, $A.4$, $A.5$, and $A.6$.

6 Discussion

Drunk driving is 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). The free market may also have a role to play in combating drunk driving, although the results are far from conclusive.

While I find some evidence of medium-run declines in drunk-driver-related incidents (after 3 or more years), the debate over whether Uber and Lyft reduce drunk driving is far from settled. When restricting the sample to the 18 states that conduct a BAC test on over 80% of deceased drivers, I actually find statistically significant increases in daytime drunk-driver-related incidents, which is concerning.

Finding an effect of Lyft and Uber on drunk driving appears to be correlated with the geographical unit of measurement (city vs. county), as this paper and Martin-Buck's (2017 working paper) find reductions in *city-level* drunk driving fatal incidents while Brazil and Kirk (2016) cannot reject the null hypothesis of no effect for *county-level* drunk driving fatal incidents. Neither Dills and Mulholland's paper (2018) nor this one can reject the null of no effect on alcohol-related fatal crashes. Lyft and Uber may be substitutes for drunk driving but not driving after one or two drinks. With respect to the city versus county issue, perhaps Uber and Lyft could be substitutes for drunk driving in the city, but not the county. Uber and Lyft don't necessarily cover the entire county, so they may not be an alternative to drunk driving for somebody who lives in a suburban or rural area.

Future work should include all cities with Lyft or Uber and stratify the sample into smaller and larger cities to determine whether the effects of Uber and Lyft are dependent on city size. Another area for future research would be to expand the definition of alcohol-involved crashes to include passengers, cyclists, and pedestrians. Finally, future work should examine whether the existence of ridesharing companies in a city induces greater alcohol consumption by that city's inhabitants.

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A Additional Tables and Figures

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.

Table A.2: Effect of Uber and Lyft on Log of Alcohol-Related Fatal Motor Vehicle Crashes

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.017	0.008	0.007
	(0.026)	(0.019)	(0.021)
(Uber and/or Lyft)*k	-0.006***	-0.005**	-0.003***
	(0.002)	(0.002)	(0.001)
R^2	0.56	0.51	0.28
N	13,062	13,062	13,062

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

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

Table A.3: Effect of Uber and Lyft on Log of Alcohol-Related Motor Vehicle Fatalities

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.003	-0.002	0.003
	(0.029)	(0.020)	(0.021)
(Uber and/or Lyft)*k	-0.006***	-0.006**	-0.003***
	(0.002)	(0.002)	(0.001)
R^2	0.55	0.50	0.27
N	13,062	13,062	13,062

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

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

Table A.4: Effect of Uber and Lyft on Log of Fatal Motor Vehicle Crashes

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.035	-0.003	0.037
	(0.023)	(0.017)	(0.029)
(Uber and/or Lyft)*k	-0.004**	-0.003*	-0.003**
	(0.001)	(0.001)	(0.001)
R^2	0.77	0.68	0.66
N	13,062	13,062	13,062

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

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

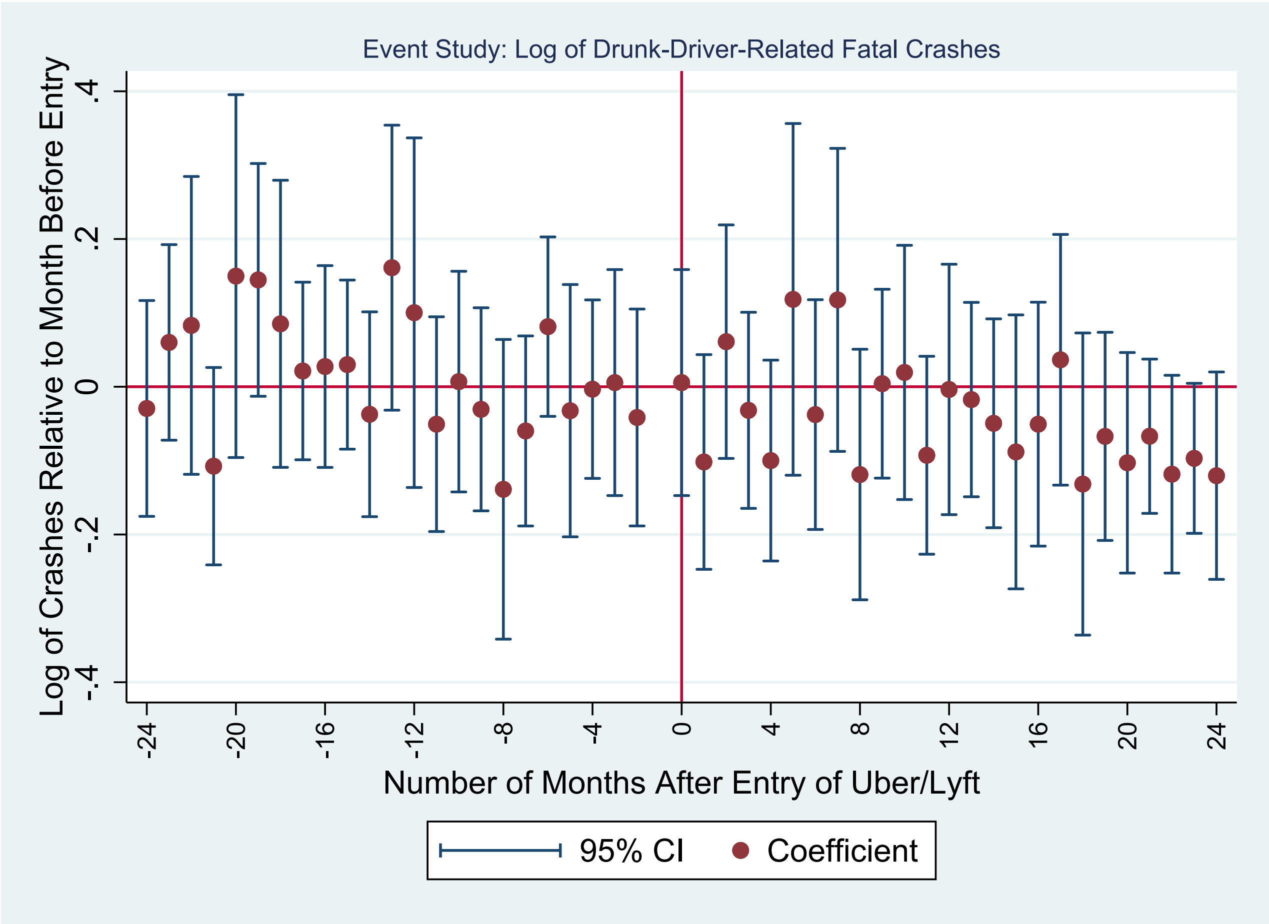
Table A.5: Effect of Uber and Lyft on Log of Motor Vehicle Fatalities

	Overall	Nighttime	Daytime
Uber and/or Lyft	0.036	-0.003	0.035
	(0.024)	(0.017)	(0.028)
(Uber and/or Lyft)*k	-0.004***	-0.003*	-0.004**
	(0.002)	(0.001)	(0.001)
R^2	0.76	0.67	0.65
N	13,062	13,062	13,062

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

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

Figure A.1



Event Study: Log of Drunk-Driver-Related Fatal Crashes
Log of Crashes Relative to Month Before Entry
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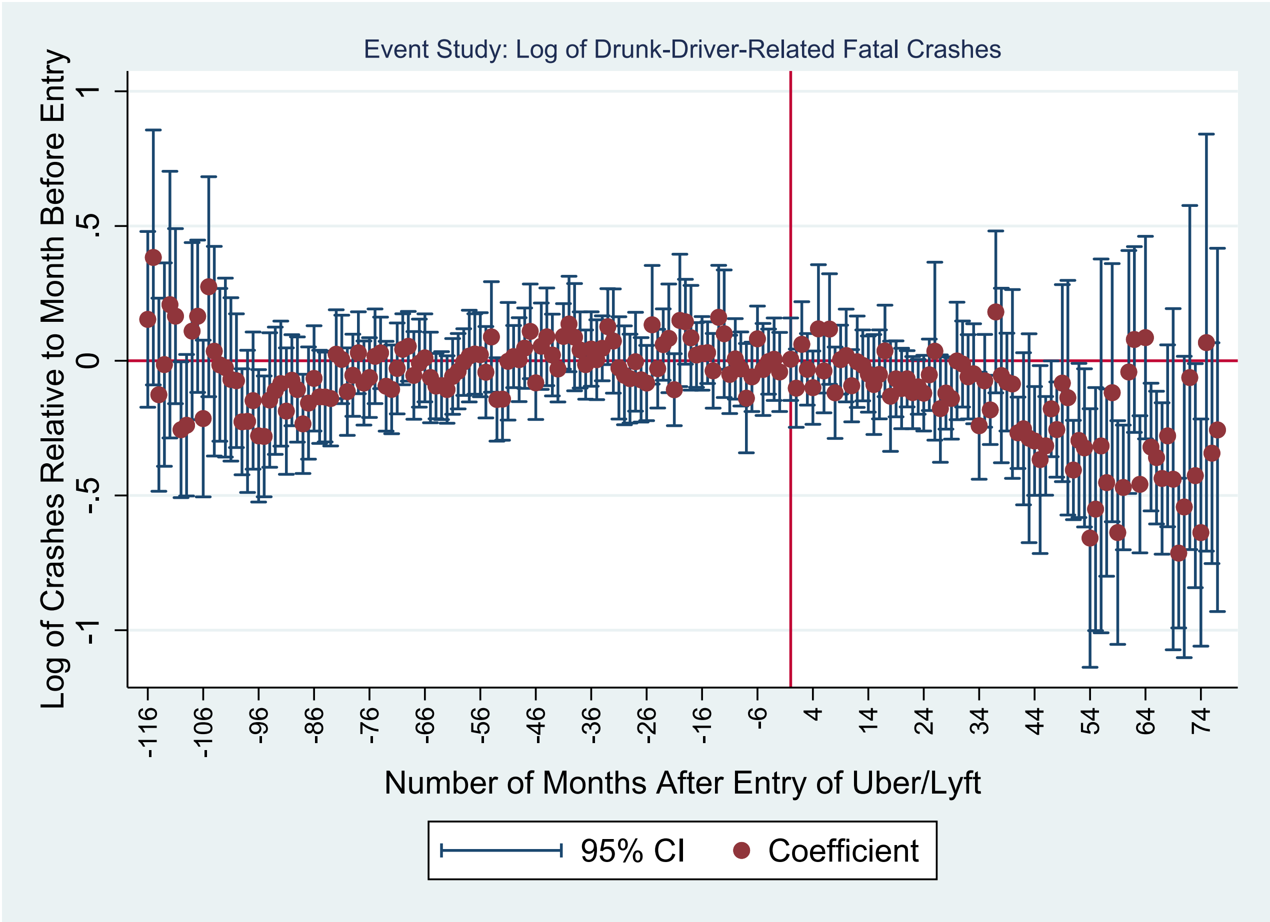
Number of Months After Entry of Uber/Lyft
95% CI
Coefficient

Figure A.2


Event Study: Log of Drunk-Driver-Related Fatal Crashes
Log of Crashes Relative to Month Before Entry
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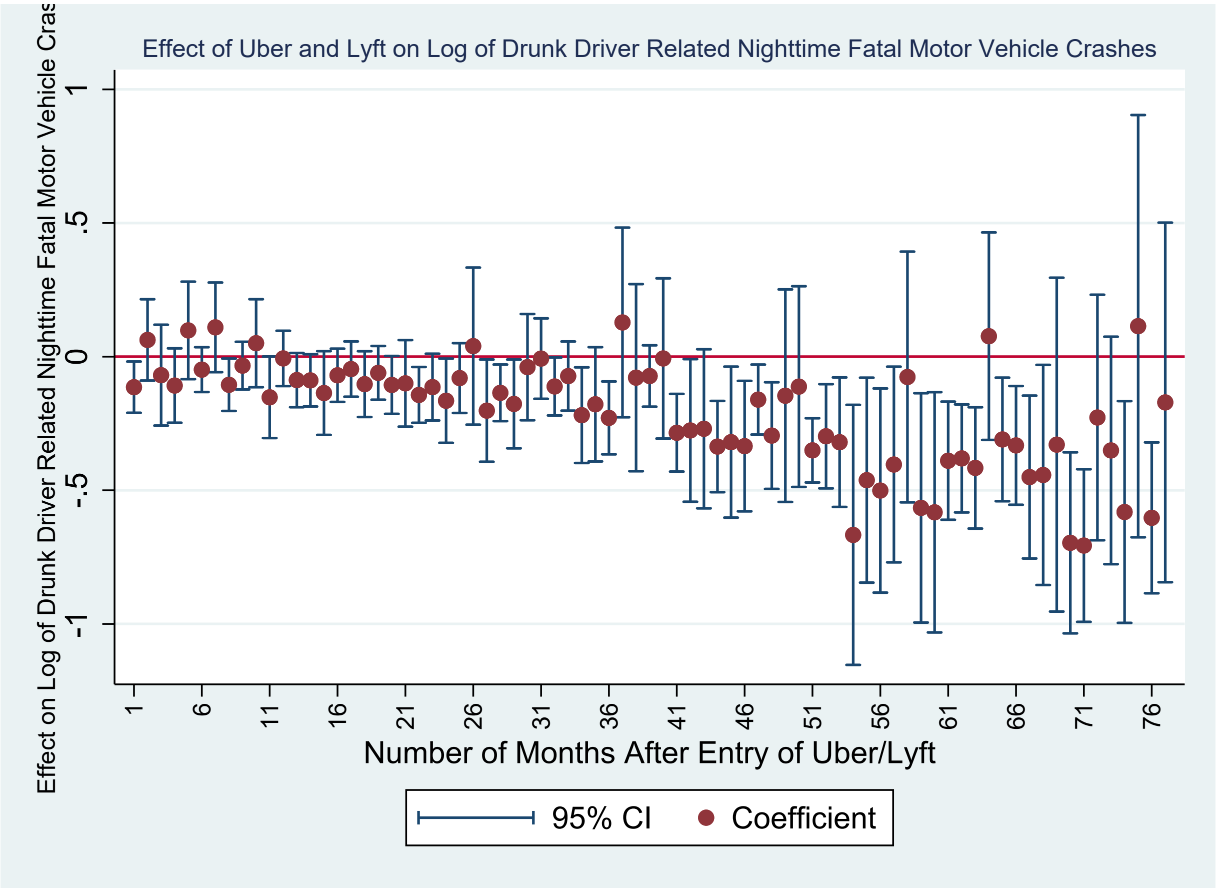
44
54
64
74
Number of Months After Entry of Uber/Lyft
95% CI
Coefficient

Figure A.3



Effect of Uber and Lyft on Log of Drunk Driver Related Nighttime Fatal Motor Vehicle Crashes
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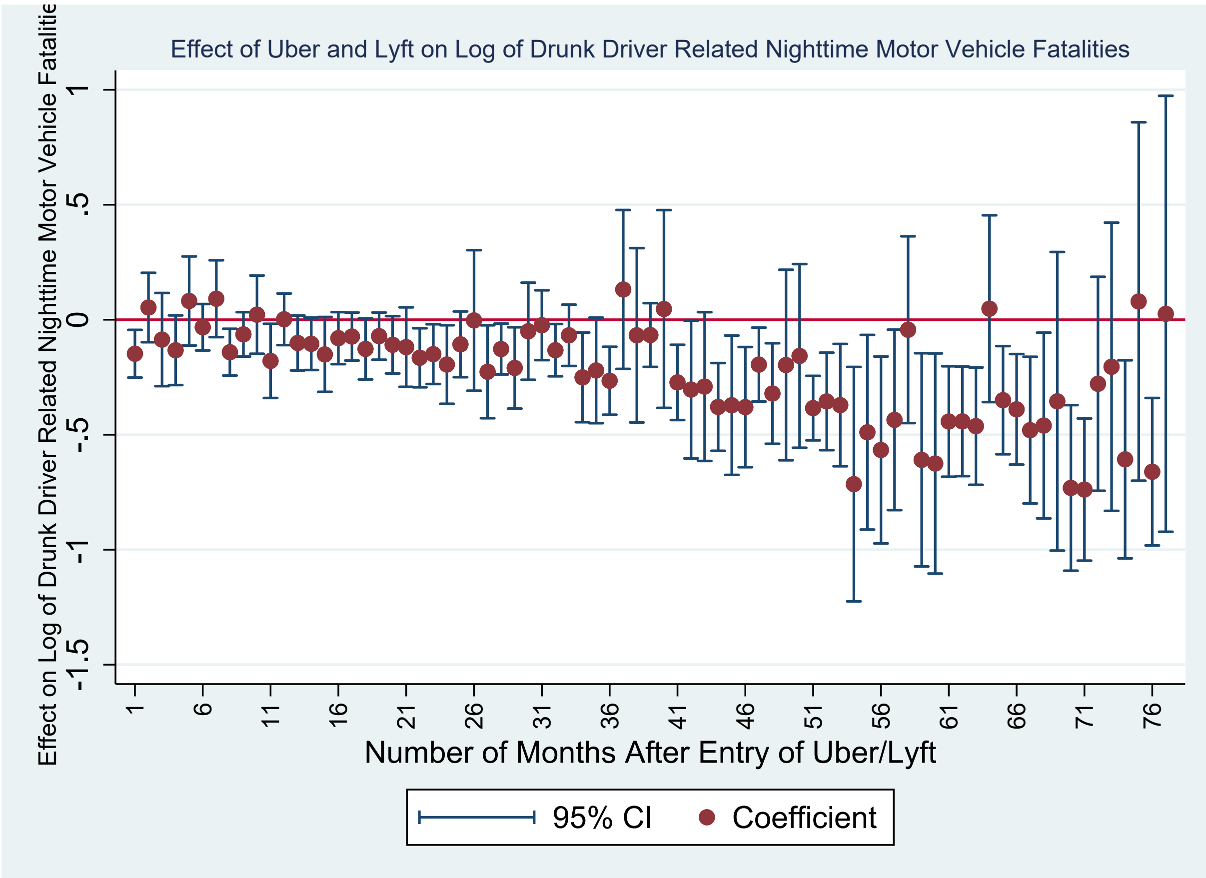
Number of Months After Entry of Uber/Lyft
95% CI
Coefficient

Figure A.4


Effect of Uber and Lyft on Log of Drunk Driver Related Nighttime Motor Vehicle Fatalities
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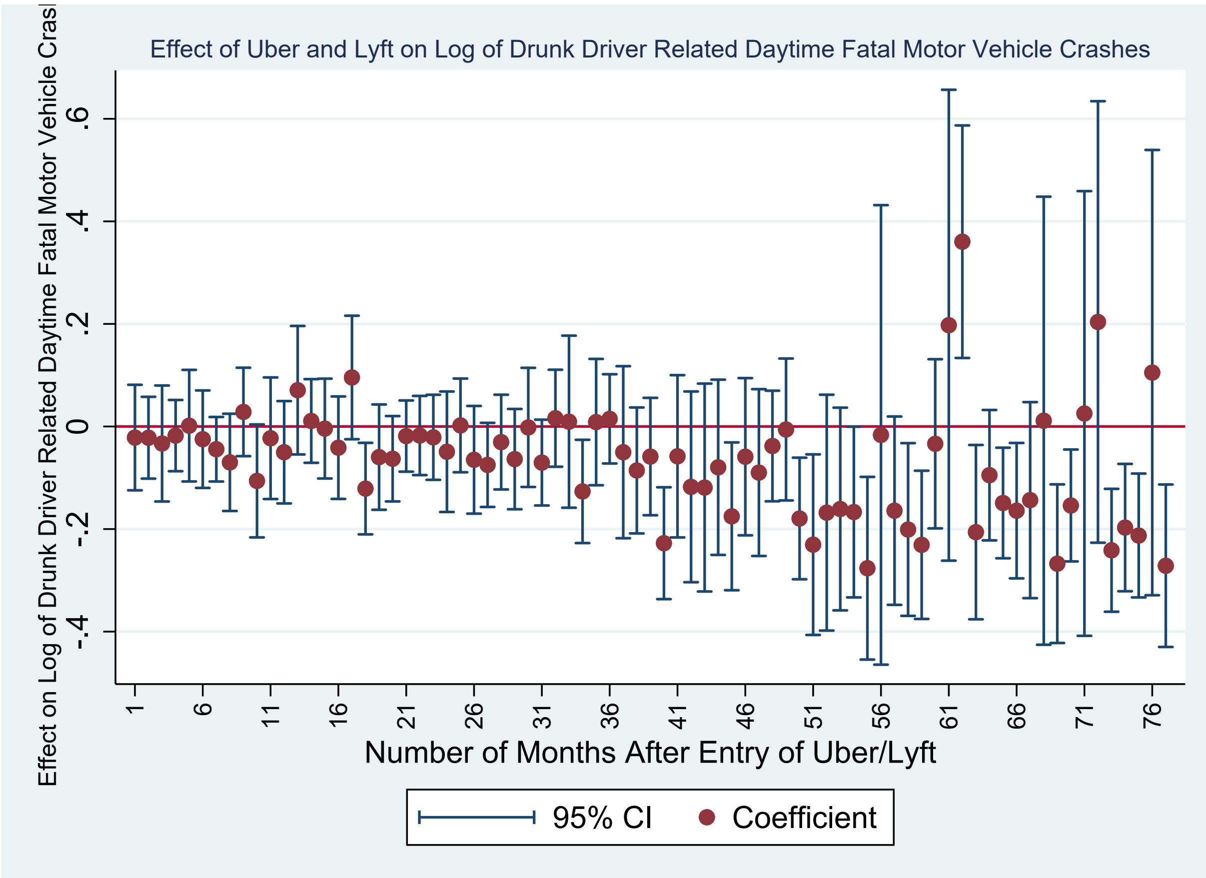
Number of Months After Entry of Uber/Lyft
95% CI
Coefficient

Figure A.5



Effect of Uber and Lyft on Log of Drunk Driver Related Daytime Fatal Motor Vehicle Crashes
Effect on Log of Drunk Driver Related Daytime Fatal Motor Vehicle Crasl
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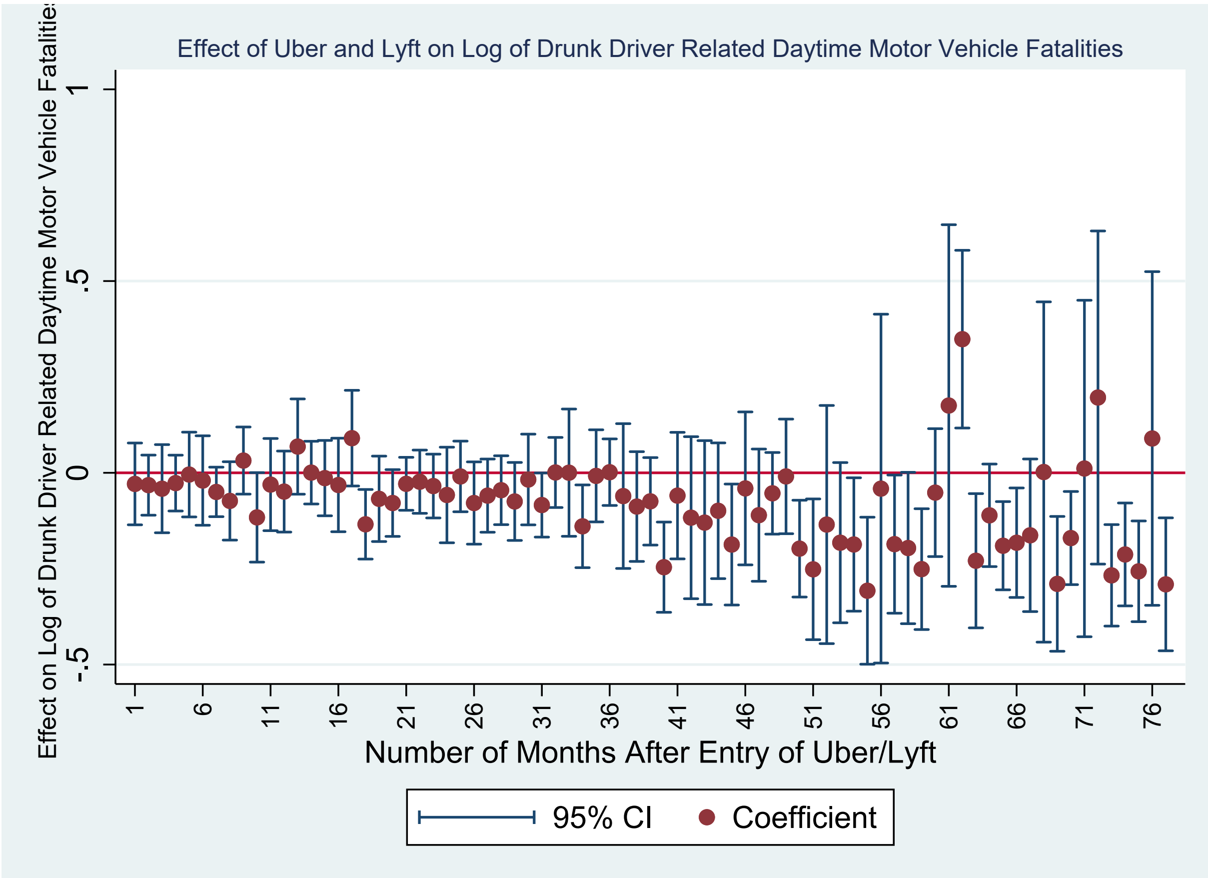
Number of Months After Entry of Uber/Lyft
95% CI
Coefficient

Figure A.6



Effect of Uber and Lyft on Log of Drunk Driver Related Daytime Motor Vehicle Fatalities
Effect on Log of Drunk Driver Related Daytime Motor Vehicle Fatalities
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Number of Months After Entry of Uber/Lyft
95% CI
Coefficient