Driving Safety: An Empirical Analysis of Ridesharing's Impact on Drunk Driving and Alcohol-Related Crime

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Abstract

This paper examines the effect of ridesharing services such as Lyft and Uber on the incidence of drunk driving and other alcohol-related crimes. Ridesharing services are convenient, low-cost alternatives to traditional taxi cabs. I use the gradual expansion of ridesharing to cities across the U.S. to identify the effects of ridesharing on alcohol-related traffic fatalities, DUI/DWI arrests, physical and sexual assault arrests, and arrests for other potentially alcohol-related crimes. Using a large sample of U.S. cities and a fixed-effects difference-in-differences approach I find that ridesharing services significantly reduce fatal alcohol-related auto accidents and, for a large subset of cities, DUI/DWI arrests as well. I further find that ridesharing's introduction leads to a significant decline in arrests for both physical and sexual assault and has no impact on arrests for drunkeness or liquor law violations. I explore the possibility of heterogeneous effects on these outcomes based on the quality of public transit available as well as the duration over which ridesharing has been operating. I find that the effects of ridesharing on fatal accidents and physical and sexual assaults increase over time. For DUI/DWI arrests I find a larger reduction due to ridesharing in cities with lower public transit usage.

1 Introduction

Drunk driving is a significant problem in the United States. In the U.S. in 2010, auto accidents caused by intoxicated drivers killed over 11,000 people and injured 326,000. The National Highway Traffic Safety Administration (NHTSA) estimates the direct economic cost of these accidents at \$44 billion and the total societal costs at \$201 billion.[1] In 2012, over 1.2 million US drivers were arrested for drunk driving[2], with the penalties for conviction including fines and potential prison time. Drunk driving costs the U.S. tens of thousands of lives and billions of dollars in law enforcement, property damage, and lost productivity each year. Accordingly, methods to discourage intoxicated driving are an important issue in public policy at the local, state, and national levels.

Most policies focus on deterrence through fear of punishment. Increasing the penalties for drunk driving convictions through higher fines, more jail time, and/or driver's license confiscation serve to increase the expected cost of a conviction. Increased police patrols, sobriety checkpoints, and no refusal laws increase the probability of being caught should an individual choose to drive drunk. These policies impose substantial costs on society, both through public expenditures on police and courts and lost productivity of convicted offenders. For example, Miller et. al. (1998) estimate the cost of operating a single sobriety checkpoint for one hour at over \$4,000, converted to 2016 dollars. Bouchery et al (2011) estimate the lost productivity due to alcohol-related incarceration to be over \$7.5 billion annually, converted to 2016 dollars.

Another avenue for public policy is to increase the convenience or decrease the cost of alternative forms of transportation, either public or private. One such alternative that has garnered substantial attention recently is ridesharing, as exemplified by companies such as Uber and Lyft. Ridesharing services are private, for-hire transportation that operate similarly to traditional taxis. One of the main advantages they provide over traditional taxis are their real-time app-based dispatch systems, which allow riders to hail drivers from their mobile device and know exactly when they will arrive. A second major advantage is that in most cities ridesharing firms are not subject to the same quantity constraints and licensing requirements as traditional taxis. This means that the introduction of ridesharing can drastically increase the number of for-hire cars available. A third advantage of ridesharing services is that they are typically priced below the rates charged by traditional taxis. For example a 10 minute, 3 mile ride in San Francisco would cost \$9.70 using Uber[3]

[1] National Highway Traffic Safety Administration. "The Economic and Societal Impact Of Motor Vehicle Crashes, 2010." National Highway Traffic Safety Administration, May 2014, DOT HS 812 013. http://www-nrd.nhtsa.dot.gov/Pubs/812013.pdf

[2] Federal Bureau of Investigation, "Crime in the United States: 2013." Web. 26 May 2015.

[3] Uber. "San Francisco." Web. 26 May 2015. https://www.uber.com/cities/san-francisco

but over \$11.75 using a traditional taxi[4]. The fourth advantage of ridesharing services are their variable pricing structures. These allow for higher prices during demand surges to incentivize more drivers to operate at those times. These advantages of ridesharing relative to traditional taxis both increase the convenience and decrease the cost of private for-hire transportation relative to driving a personal car.

In this paper I estimate the impact of ridesharing services on drunk driving and other alcohol-related crimes. In particular I use a difference-in-differences methodology to estimate the effect of introducing ridesharing on the number of fatal alcohol-related auto accidents, the number of DUI arrests, and the number of arrests for crimes such as physical and sexual assault in a city. In a sample consisting of every U.S. city with a population of 100,000 or more, I find large reductions in both measures of drunk driving following ridesharing's launch. Using data from 2000-2014 and controlling for public transportation availability, unemployment rates, and any time-invariant or city-invariant factors I estimate that ridesharing reduces fatal alcohol-related auto accidents by 10% to 11.4%. Separating out these effects by the length of time these services have been operating, I show that the effects manifest quickly after introduction and persist (and in some cases may even increase) over time. For DUI arrests the results reveal heterogeneous effects for cities with different levels of public transit quality. In cities with the highest transit usage ridesharing has little effect on the number of DUI arrests. In cities where transit is less utilized however, ridesharing results in an 8.7% to 9.2% reduction in DUI arrests. These large effects indicate ridesharing services may be a potentially potent tool for reducing the incidence and harm of drunk driving.

A common concern is that any benefits of ridesharing in terms of drunk driving might be mitigated by increased levels of alcohol-related and ridesharing driver-committed crime. The reasoning for the former is that by making drinking outside the home more convenient, ridesharing might induce more people to do so or encourage those who would otherwise have moderated their alcohol intake to drink excessively. A larger number of intoxicated individuals in public could result in an increase in alcohol-related crimes. The concerns about ridesharing driver-committed crimes stem from the vetting process for ridesharing drivers compared to that for traditional taxi drivers. Some have worried that less stringent background checks could result in a risk of sexual assaults perpetrated by drivers against their passengers. In this study I examine the effect ridesharing has had on each of these categories of crimes. I find that contrary to these concerns, ridesharing actually corresponds with a significant reduction in both physical and sexual assaults of 7.9% and 9.3% respectively. I further find no change in arrests for other alcohol-related crimes such as public

[4]San Francisco Municipal Transportation Agency. "Taxi Rates." Web. 26 May 2015. http://www.sfmta.com/getting-around/taxi/taxi-rates

drunkeness and liquor law violations after ridesharing introduction. These results indicate that the benefits of ridesharing availability extend beyond just drunk driving prevention.

The remainder of this paper proceeds as follows. Section 2 provides an overview of ridesharing and the problem of drunk driving in the U.S. Section 3 describes the different data sources used in the analysis. Section 4 presents the conceptual framework for the study. Section 5 describes the empirical methodology. Section 6 presents the results. Section 7 explores the robustness of the estimates. Section 8 discusses the findings in the context of drunk driving prevention strategies and quantifies the estimated benefits. Finally, Section 9 concludes.

2 Background

2.a Existing Evidence on Effects of Drunk Driving Policies in the U.S.

The United States faces a high incidence of drunk driving. In 2012, an estimated 11.2% of all Americans drove under the influence of alcohol at least once.[5] Drunk driving accidents result in tens of thousands of deaths and hundreds of thousands of injuries each year. The estimated economic costs of these accidents range from $44 billion in direct costs to $201 billion in total social costs. The incidence of drunk driving and the attendant costs vary significantly across cities and regions. For example, Dallas experiences around 130 fatal alcohol-related accidents each year while comparably-sized San Diego only has around 70 fatal accidents.

Efforts to reduce drunk driving typically take the form of greater enforcement or stricter punishments. The severity of penalties for drunk driving varies between states. For example, drunk driving in Georgia carries penalties of a one year license suspension and vehicle confiscation in addition to potential fines and jail time. On the other hand, drunk driving in Maryland has no vehicle confiscation or impounding and only a 45 day license suspension, again in addition to potential fines and jail time.[6]

Accordingly, most of the research into drunk driving prevention has focused on policies that affect the severity and enforcement of anti-drunk driving laws or that attempt to reduce alcohol consumption. Shults et al (2001) find that police sobriety checkpoints reduce fatal drunk driving accidents by 18-20%. Eisenberg (2003) finds that lowering the legal BAC limit from 0.10 to 0.08 reduces fatal alcohol-related accidents by 3.1%. Kenkel (1993) examines the effect of both the nationwide standardization of the U.S. drinking age as well

[5]Substance Abuse and Mental Health Services Administration, Results from the 2012 National Survey on Drug Use and Health: Summary of National Findings, NSDUH Series H-46, HHS Publication No. (SMA) 13-4795. Rockville, MD: Substance Abuse and Mental Health Services Administration, 2013.

[6]Governors Highway Safety Association. "Drunk Driving Laws." Web. 26 May 2015. http://www.ghsa.org/html/stateinfo/laws/impaired_laws.html.[18]

as the wave of anti-drunk driving laws enacted during the 1980's and he finds that both policies significantly reduced rates of drunk driving. Villaveces et al (2003) examine other drunk driving prevention policies, finding that administrative license revocation reduces fatal drunk driving accidents by 5% while zero-tolerance laws reduce them by 12%.

Recently a small but growing literature has examined the impact of transportation alternatives, including ridesharing services, on drunk driving outcomes. Jackson and Owens (2011) estimate the effect of extending the operating hours of Washington DC's subway system on DUI arrests, fatal alcohol-related auto accidents, and other alcohol-related arrests. They find little city-wide effect of the extended subway hours on any of the outcomes. However, in neighborhoods with bars in walking distance to subway stations they find a decrease in DUI arrests and an increase in other alcohol-related arrests. These results are consistent with the idea that increased convenience of public transit reduces drunk driving, while also appearing to increase the overall level of alcohol consumption.

Over the past year, a few studies have attempted to measure the effect that ridesharing services like Uber have had on drunk driving and other crimes. Greenwood and Wattal (2015) focus on Uber's launch in different cities across California and find that Uber's low-cost Uber X service reduces alcohol-related vehicle deaths by 3.6% - 5.6%. Dills and Mulholland (2016) expand on this by extending the scope to a national sample of cities. Using data from 2010 through 2013 they find that Uber's launch results in a reduction in DUI arrests, fatal auto accidents, and arrests for assault and disorderly conduct. Unlike Greenwood and Wattal (2015), however, they find no effect on alcohol related fatal accidents. Most recently, Brazil and Kirk (2016) look at the impact of Uber's entry on fatal accidents in the 100 most populous U.S. cities. Unlike the prior two studies, the authors find no evidence of a reduction in fatal accidents, whether alcohol-related or otherwise. These second two papers are limited to observing traffic fatalities at the county rather than municipal level, potentially reducing the precision of their estimates. Given the conflicting findings in the existing literature, this study contributes by expanding the analysis to a larger sample of U.S. cities for a longer time period. In addition, my drunk driving measures are at the city level, where the impact of ridesharing services are more likely to be concentrated. Furthermore, my study will more accurately measure the presence of ridesharing services by incorporating launch dates for other ridesharing firms in addition to Uber. Methodologically, my study expands on the prior literature by testing for heterogeneous effects of ridesharing by duration of operation and public transit availability. These factors should help provide some clarity to the literature on ridesharing and drunk driving.

2.b History of Ridesharing

Prior to the introduction of ridesharing services the forms of private for-hire transportation available were limited to traditional taxis, limousines, and larger vehicles such as bus and van services. Of these, only traditional taxis did not need to be reserved in advance and all came at fairly substantial costs. Furthermore, the for-hire transportation options and number of cars available varied widely from city to city. Most municipalities heavily regulate the traditional taxi industry, placing restrictions on the number of vehicles that can operate, the prices they can charge, and the licensing and insurance requirements for the drivers and cars. These restrictions, particularly on quantity, can lead to shortages of traditional taxis during periods of high demand such as late in the evening on Fridays and Saturdays after bars close or at the end of large events.

In most cities in which they operate, ridesharing firms are not subject to these same restrictions, allowing them to expand supply during periods of high demand and adjust prices to encourage more riders or drivers to participate in the market. Many major ridesharing companies adjust pricing in real time to better match supply and demand, charging higher "Surge Pricing" fares during periods with high demand relative to supply.[7] This serves to encourage more drivers to operate during periods of high demand.

Uber was the first ridesharing firm in the U.S., launching in San Francisco in May 2010. They were followed two years later by Lyft and Sidecar. Uber's initial expansion was gradual, growing to cover nine city markets in the two years between their launch and the launch of their competitors. After the introduction of Lyft and Sidecar, ridesharing expanded rapidly across the U.S. Cities served by ridesharing range from large metropolises like New York and Los Angeles to small college towns like College Station, TX. By the the end of 2014, ridesharing firms operated in about 80% of all U.S. cities with a population of 100,000 or more.[8] In many of these cities, ridesharing services began operations months before city and state officials permitted them to legally operate.[9]

3 Data

Below I describe the data sources I use for ridesharing launch dates, alcohol-related fatal accidents, DUI and other alcohol-related crime arrests, and public transit availability. I collected data for all 273 U.S. cities with populations of 100,000 or greater covering the years

[7]Uber. "What is surge pricing?" Web. 25 Oct. 2016. https://help.uber.com/h/34212e8b-d69a-4d8a-a923-095d3075b487

[8]The cities with at least 100,000 people covered by ridesharing as of December 2014 contained 24.7% of the U.S. population.

[9]This was the case in Milwaukee[23], Tampa Bay[28], Kansas City[10], several cities in Texas[3], and many others.

2000 through 2014.[10] Additionally, I collected city-level unemployment and population data for each of these cities.[11] Table 1 presents summary statistics by year of ridesharing introduction. Ridesharing firms appear to enter earlier in cities with larger populations, larger public transit systems, and lower rates for fatal alcohol-related accidents and DUI arrests. If the impact of ridesharing on drunk driving is positively correlated with preexisting levels of drunk driving or negatively correlated with public transit availability the magnitude of the results in this paper will represent a lower bound for the effect in cities where ridesharing services have yet to launch.

[Table 1 about here.]

3.a Ridesharing Launch Dates

The two largest and longest-operating ridesharing services in the U.S. are Uber and Lyft. Uber officially launched in San Francisco in 2010 followed two years later by Lyft.[12] Uber and Lyft regularly post announcements to their websites when they launch in new cities. I collected all of the launch dates contained in these announcements. Not all city launches are accompanied by announcements so I supplement these data with news articles discussing the launch for any remaining cities.[13] There are a small number of cities for which one or more ridesharing services launched but later suspended service. I collected the date service was suspended for each of these cities. Figure 1 shows how the proportion of U.S. cities served by ridesharing services has increased over time.[14]

[Figure 1 about here.]

3.b Alcohol-Related Traffic Fatalities

Since 1975 the National Highway Traffic Safety Administration (NHTSA) has collected detailed information on all fatal traffic accidents in the U.S. All traffic accidents on publicly accessible roads resulting in at least one fatality in all 50 U.S. states plus the District of Columbia and Puerto Rico are recorded in the NHTSA's Fatality Analysis Reporting System (FARS) database[15]. This database contains detailed information about the time,

[10] Population measured as of 2010 Census. Some of the detailed public transportation network data as well as arrest data are not available for all cities due to voluntary reporting.

[11] Unemployment data are from the Bureau of Labor Statistics' Local Area Unemployment Statistics and the city population data are from the U.S. Census Bureau.

[12] A third service named Sidecar also launched in 2012 but struggled to grow along with Uber and Lyft and has since ceased operations. I gathered launch data data for this company as well.

[13] I supplement this further with data provided by representatives at Uber and Lyft for any cities which did not have clearly published launch dates. This primarily pertains to suburbs of larger cities.

[14] I include all cities with populations of 100,000 or more as of the 2010 Census.

[15] The FARS data can be accessed using the NHTSA's website at http://http://www.nhtsa.gov/FARS/.

location, and other important details regarding the accident and all of the vehicles and persons involved. Importantly, it also includes data on whether any of the drivers involved were under the influence of alcohol. These data are published annually and I have collected them for the years 2000 through 2014, the last year for which data are available. All of the data are at the incident level so I aggregate them to monthly totals for each city. When the data contain geographic coordinates I assign accidents to sample cities if they are within five miles of the city centroid.[16] When this information is not available I use the city and state identifier codes contained in the FARS data to assign observations to the appropriate city. Geographic coordinates are present in 92.2% of observations in the sample period and in 98.2% of observations after 2001.

3.c Drunk Driving and other Crime Arrests

The Federal Bureau of Investigation (FBI) has collected data on arrests for driving under the influence (DUI) and other crime categories from police departments across the United States every year since 1930 under the Uniform Crime Reporting (UCR) program. I gathered monthly arrest reports for all available police agencies from 2000 through 2014, the last year of available data. Although participation in the UCR program is voluntary, over 18,000 law enforcement agencies report arrest data under the program.[17] Of the sample of all U.S. cities with populations of 100,000 or more 45% report DUI arrests for every month between 2000 and 2014, 77% report for at least 80% of these months, and 10% do not report DUI arrests at all. Despite less than universal reporting each month, the UCR data represent the most complete nationwide collection of DUI and other crime arrest data available.[18] To estimate the effect of ridesharing services on arrests I determine the municipal police agency for each of the sample cities that reports arrest data.

3.d Public Transit

To properly estimate the effects of ridesharing on drunk driving outcomes it is important to control for other factors that may affect drunk driving as well. Quality public transportation has the potential to affect drunk driving rates by providing an alternative to driving when consuming alcohol.[19] To account for this I gathered detailed data on the size of each sample city's public transit system from the Federal Transit Administration's National

[16] City centroid data are assembled from National Geospatial-Intelligence Agency data by MaxMind Inc. and are available from http://www.maxmind.com/.

[17] Federal Bureau of Investigation. Uniform Crime Reports. Web. 11 Dec 2015. https://www.fbi.gov/about-us/cjis/ucr/ucr.

[18] Any city-month observations which are not present in the UCR data I treat as missing.

[19] Jackson and Owens (2011) find some evidence of an effect of late night subway service in Washington DC.

Transit Database (NTD).[20] Not every transit agency submits reports to the NTD, these data are only available for 69% of the sample cities. For each city that does report I gather information on the size of both their bus and rail transit networks. I gather data on the number of "directional route miles" for each type of service as well as the number of miles for which buses have designated right-of-way, meaning they do not share the road with other forms of traffic. Directional route miles are the number of miles transit vehicles travel while in revenue service. To ensure I have measures of transit availability for every sample city I also gather data from each city's transit authority on the presence of rail transit services during the 2000-2014 time period. For each city I record whether they have heavy rail and/or light rail services as well as the date these services began operation, if that occurred within the sample period.[21]

4 Conceptual Framework

In order to understand the effect ridesharing has on drunk driving it is important to understand the decision process around alcohol consumption and driving while intoxicated. This process involves multiple layers of choices regarding the amount of alcohol to consume, both the a priori expectation as well as the in-the-moment decision once drinking begins, as well as where to drink and how to get there and back. Drunk driving occurs when a specific combination of decisions are optimal for an individual. The person must optimally choose to consume more alcohol than would allow them to legally drive. Again, this decision may take place prior to beginning drinking or it can occur after the individual has already begun consuming alcohol. Additionally, drunk drivers must select to drink outside their home and to drive themselves to that location. Driving drunk carries with it several risks. There is a chance the driver will be detected by law enforcement and face penalties including jail time, fines, and license suspension. There is also an increased risk of accidents and the injuries and property damage that go along with them.

Much of the public policy and research into drunk driving has focused on policies and actions that affect the risk of being caught driving drunk (Shultz et al (2001), Villaveces et al (2003), Chang et al (2011), Hansen (2015)). Sobriety checkpoints, no-refusal[22], increased police patrols, and similar actions increase the probability of being detected when driving under the influence. Other strategies affect the expected penalty from being caught. Increasing the severity of punishment increases the expected cost of being detected. The

[20] https://www.transit.dot.gov/ntd

[21] Heavy rail transit such as subways and elevated trains have dedicated right-of-way as well as longer and faster trains than light rail.

[22] A no refusal law makes it illegal to refuse an alcohol breath test when suspected by a police officer of driving drunk. In some states this is always in force, while in others it only applies during certain designated time periods.

ultimate goal of such policies is to reduce the attractiveness of driving drunk relative to other options such as reduced alcohol consumption, drinking at home, or taking alternative transportation. These strategies have the potential to be effective, as some of the research into them has shown.

Less focus has been placed on how increasing the attractiveness of alternative transportation may influence drunk driving. By improving the convenience and/or lowering the cost of taking a transportation method other than self-driving, some people who would have optimally chosen to drive drunk before may now choose to take alternative transportation instead. One common concern with alcohol-related decisions is that perceived risks may be different when under the influence of alcohol, potentially reducing the expected cost of driving drunk thus increasing its attractiveness.[23] This concern is mitigated by the observation that the initial transportation decision will often be made prior to the individual becoming intoxicated. Once the person has opted to take alternative transportation **to** the location where they plan to drink alcohol they no longer have the option to drive drunk **from** that location on their return trip. Improving the attractiveness of alternative transportation will, ceteris paribus, weakly reduce the number of individuals who optimally choose to drive drunk. The degree to which drunk driving is reduced depends on how significant the improvement in alternative transportation attractiveness is as well as the number of individuals who are on the margin between driving drunk and taking alternative transport.

Another potential concern is that increasing the appeal of alternative transportation could induce people to optimally consume more alcohol or increase their likelihood of consuming alcohol outside the home. Greenfield (1998) estimates that as much as 35% of violent crimes are committed by individuals who have recently consumed alcohol. Accordingly, an increase in alcohol use, particularly in public might have the potential to increase the incidence of crimes other than drunk driving. Some studies have examined this possibility (Dills and Mullholland (2016), Jackson and Owens (2010)) with each finding differing results depending on the particular crimes and the particular form of alternative transportation. For services like ridesharing the exact effect on crime is unclear a priori. While lowering the cost of drinking excessively outside the home could increase the number of intoxicated people in public, having lower cost and more convenient transportation available could also allow people to return home more quickly and easily after they become intoxicated, reducing the time they are in public and at risk of committing or being the victim of a crime. Separating out these two competing forces is difficult, but it is possible to estimate the net effect of alternative transportation services on the incidence of particular crimes.

[23] Many thanks to the seminar participants at The University of Texas at Austin for this observation.

5 Methodology

To estimate the effects of ridesharing services on drunk driving and alcohol-related crime outcomes I use a fixed effects differences-in-differences methodology. City fixed effects control for any time-invariant differences across cities in the average level of drunk driving and other crimes, while month by year fixed effects control for any time-varying factors that are common across cities. In each specification I also control for differences in city population by including indicators for each population decile within the sample.[24] Additionally, each specification includes controls for the city-level unemployment rate as well as an indicator for the presence of light rail transit.[25] Finally, for some specifications I also include detailed NTD data on the size of each city's bus and rail transit networks.[26]

5.a Overall Effects

I first test for the effect of ridesharing by testing for any overall change in drunk driving and other crime outcomes after the introduction of these services. I use Equation 1 below to perform this estimation.

$$y_{i,t} = \alpha_0 + \beta RS_{i,t} + X_{i,t}\gamma + \delta_i + \phi_t + \epsilon_{i,t} \tag{1}$$

$y_{i,t}$ represents the outcome of interest. $RS_{i,t}$ is an indicator for whether one or more ridesharing services were operating in city i at time t. $X_{i,t}$ represents a vector of covariates about the city such as public transportation availability, population, and unemployment rate. δ_i are the city fixed effects. ϕ_t are the month by year fixed effects. The effect of ridesharing's presence on outcomes in this model is captured by the time-invariant coefficient β.

5.b Time-Varying Effects

It is possible that any effects ridesharing has on drunk driving and other crime rates may change with the duration of time the services have been present. Accordingly, the econometric model I use to test for this allows the coefficient on the treatment variable to vary with the number of months since ridesharing introduction. Equation 2 provides the estimation equation for testing this.

[24] To do this I calculate population deciles for the sample each year and assign each city to its corresponding decile.

[25] I do not include an indicator for the presence of heavy rail transit because there is no variation in heavy rail transit availability over the sample period so any effect of these services will be captured by the city fixed effects.

[26] I do not include these data in every specification because they are only present for 69% of the sample cities.

$$y_{i,t} = \alpha_0 + \sum_{g=1}^{G} \rho_g RS_{i,t,g} + X_{i,t}\gamma + \delta_i + \phi_t + \epsilon_{i,t} \tag{2}$$

Again, $y_{i,t}$ represents the outcome of interest, $X_{i,t}$ is a vector of covariates, δ_i are the city fixed effects, and ϕ_t are the month by year fixed effects. $RS_{i,t,g}$ are a set of dummy variables which equal one if one or more ridesharing services are present in city i at time t and whose duration of operation in that city falls into group g. The operating time groups I use are 0-6 months, 6-12 months, 12-18 months, 18-24 months, and 24 months or more.[27] The coefficients ρ_g represent the the effect of ridesharing for each operating duration group.

5.c Identification

The difference-in-differences will identify the causal effect of ridesharing if the treatment and control groups would have followed parallel trends but for the treatment. The gradual expansion of ridesharing services to cities across the U.S. means the treatment group represents an expanding proportion of U.S. cities, covering 80% of cities with 100,000 people or more by the end of 2014. This also means that the control group changes over time as well, representing all cities where ridesharing services have yet to launch as of each particular month. To assess whether the two groups followed similar trends prior to ridesharing introduction, I focus on cities in which ridesharing launched between 2012 and 2014 and compare them to those in which ridesharing launched in 2015 and later (or have yet to launch). Together, these two groups represent over 94% of the sample cities.[28] Figure 2 presents the average annual fatal alcohol-related auto accidents for each of the two groups from 2000 through 2011, representing all or most of the pre-ridesharing period for each of the cities.[29] The pre-introduction trends for the two groups closely track one another, making plausible the assumption that absent ridesharing's launch they would have continued to do so. Figure 3 presents the same information for DUI arrests. In this graph the two groups differ in the first few years of the sample but then begin moving together. Importantly, the graph shows that DUI arrests in early-adoption cities were not declining relative to later-adoption cities prior to ridesharing's introduction.

[Figure 2 about here.]

[Figure 3 about here.]

[27] Each group is exclusive of the lower bound and inclusive of the upper bound.

[28] Restricting the analysis to this subsample does not change the results.

[29] Fatal accidents in 2000 are significantly lower than in subsequent years. Omitting observations from 2000 does not change the results.

6 Results

6.a Fatal Alcohol-Related Auto Accidents and DUI Arrests

I begin my analysis by examining the effect of ridesharing services on fatal alcohol-related auto accidents and DUI arrests. I first test for an overall effect after ridesharing introduction. It is possible that this overall effect exhibits heterogeneity depending on the length of time these services have been operating, so I also test for differential effects of ridesharing based on duration of operation. The fatal accident and DUI arrest data represent monthly counts of the number of accidents or arrests in each city. Accordingly, any effect of ridesharing is best estimated using a count model such as Poisson or Negative Binomial. In my estimates I use the Negative Binomial model because it relaxes the Poisson model assumption of equal mean and variance of the dependent variable. For reference I include OLS estimates as well.

6.a.1 Overall Drunk Driving Results

To estimate the effect of ridesharing services on fatal alcohol-related auto accidents and DUI arrests I begin by testing for an overall change in the frequency of such outcomes after ridesharing services enter a city. I estimate Equation 1 using drunk driving data for all cities with populations of 100,000 or more covering the period from 2000 through 2014. Specification (1) in Table 2 uses OLS to estimate the effect of ridesharing on the natural log of the monthly number of fatal accidents.[30] The coefficient on ridesharing in specification (1) indicates a significant reduction in fatal accidents following ridesharing introduction.[31] Because fatal accidents are discrete count data, specification (2) in Table 2 estimates Equation 1 using a Negative Binomial count model. Using this specification I estimate that ridesharing reduces fatal alcohol-related auto accidents by 10%.[32] Specification (3) estimates the same model but adds in additional public transportation covariates measuring the size of each city's public transit system. I incorporate data on the directional route miles for all rail transit excluding commuter trains, the directional route miles for all bus transit excluding commuter buses, and the directional route miles for buses with exclusive right-of-way. I exclude commuter trains and buses because they operate primarily during weekday rush

[30] Following Greenwood and Wattal (2015) I add one to each monthly fatal accident observation to account for the fact that observations with zero accidents are undefined in log form absent this modification. DUI arrests do not have this issue. Because the estimated effects on fatal accidents are negative, this modification will understate the true reduction due to ridesharing.

[31] The coefficient of -0.049 indicates that ridesharing reduces the mean of log fatal accidents by 0.049, which corresponds to a percentage reduction of 4.8%. Due to the modification described in the previous footnote, this estimate understates the true magnitude of the reduction in fatal accidents.

[32] The coefficients for Negative Binomial regressions represent the change in the mean of the natural logs for the outcome variable in response to a unit change in the independent variable. Accordingly, the percentage change is calculated as $exp(coefficient) - 1$.

hours and are of limited use outside of commuting to and from work. These additional transit data are not available for every city in the original sample so the number of cities included declines from 273 to 189 for specification (3).[33] The inclusion of these additional control variables does not substantively change the estimated impact of ridesharing, with the magnitude of the estimated effect for specification (3) rising slightly to a 11.4% reduction in fatal accidents. Specifications (4), (5), and (6) perform the same estimations as the first three specifications using DUI arrests as the dependent variable. The first two DUI specifications indicate little effect of ridesharing on the number of DUI arrests. Adding detailed public transit data in specification (6) results in a marginally significant but substantial 6.9% reduction in DUI arrests following ridesharing's introduction. This result is driven largely by the change in sample composition in specification (6) due to the detailed transit data availability. Restricting specifications (4) and (5) to the same sample yields estimated reductions in DUI arrests due to ridesharing of 8.1% to 11.0% ($p<0.05$).

[Table 2 about here.]

The number of fatal alcohol-related accidents increases sharply during weekend evenings.[34] For the cities in my sample, 45% of all fatal alcohol-related accidents occur during these time periods. It is possible that the effect of ridesharing services will differ for these higher-risk time periods, when more people are going out to bars and restaurants. It is unclear, a priori, whether the effect during this time period will be higher or lower than the overall effect. While weekend evenings are when the bulk of drunk driving fatalities occur, they are also more likely to be periods of high demand for ridesharing services, which can trigger surge pricing and reduce the appeal of ridesharing as a transportation alternative.[35] To test how the effect of ridesharing differs I repeat the analysis above using only accidents that occur within this time frame. The results, presented in columns (1)-(3) of Table 3 are similar to those using all alcohol-related fatal accidents. This suggests that there is not a significantly different effect on fatal accidents during weekend evenings. It also mitigates the concern that the potential for surge pricing during high-demand times might reduce ridesharing's impact during these periods.

For DUI arrests it is not possible to focus on certain time periods as I did with the fatal accident data. These data are aggregated to the monthly level and do not include any time-of-day arrest information. These data do, however contain arrest totals by gender and age range. This allows me to hone in on another potentially higher-risk subset of data,

[33] Redoing the estimation in specification (2) with the sample restricted to that in specification (3) yields an estimated coefficient on the ridesharing variable of -.114 ($p<0.001$).

[34] Weekend evenings are between 5pm and 4am on Fridays, Saturdays, and Sundays.

[35] "Surge pricing" is a term coined by Uber. When demand for rides exceeds supply Uber and Lyft increase prices to better equalize demand and supply.

males aged 21 to 44.[36] It is possible that this high risk group would be more affected by ridesharing since they more frequently drive drunk. It is also possible that this group would be less affected by the presence of ridesharing as they already have a higher-than-average preference for drunk driving. Columns (4)-(6) of Table 3 presents the results of estimating the overall effect models on this subset of DUI arrest data. The point estimates are similar to the overall DUI arrest results, implying that ridesharing does not have a substantively different effect on drunk driving arrests for males aged 21-44 than it does for all drivers.

[Table 3 about here.]

6.a.2 Time-Varying Drunk Driving Results

It is possible that ridesharing service in a city requires time to reach full scale after launch. Hall and Krueger (2015) provide evidence for this, showing that the number of active ridesharing drivers increases significantly with the duration of ridesharing's presence, particularly for early-adoption cities. This may mean that during the start up period the effect on drunk driving is lower than it is once the service has established a network of drivers and riders. To test this I estimate Equation 2 using the same OLS and Negative Binomial models as I used to measure the overall ridesharing effect. I separately estimate the effect of ridesharing services for each six-month period following their launch.[37] The results are presented in Table 4. Specifications (2) and (3) provide some support for this hypothesis for fatal alcohol-related accidents. In each the point estimates of the effect of ridesharing are larger after the first year of service availability. The estimated effects of ridesharing in the last half of the second year of operation are significantly larger than those within the first year (p-values 0.025 to 0.061).[38] The time-varying results for ridesharing's effect on DUI arrests follow the overall results. There is some evidence of an effect once detailed transit data are included in column (6) but they are imprecisely estimated. There is little evidence that ridesharing has a substantially larger effect on DUI arrests the longer the services have been operating. Equality between all of the DUI arrest ridesharing coefficients cannot be rejected for any of the specifications.

[Table 4 about here.]

[36] In 2014 Males were responsible for 80.4% of fatal drunk driving accidents and drivers aged 21-44 were responsible for 60% of such accidents. National Highway Traffic Safety Administration. "Traffic Safety Facts, 2014 Data" National Highway Traffic Safety Administration, Dec. 2015, DOT HS 812 231. https://crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/812231.

[37] After two years of ridesharing operation all observations are grouped into a single "$>$ 24" months category.

[38] All other ridesharing coefficients are statistically indistinguishable from one another.

As with the overall results, I repeat the above analysis restricting my attention to fatal alcohol-related accidents which occur on weekend evenings which are high-risk times for drunk driving. Columns (1)-(3) of Table 5 presents the results. The pattern seen in the time-varying results for all fatal alcohol-related accidents is present in the weekend evening results as well. The estimates of the effects of ridesharing are higher after the services have been operating for at least a year than within the first year of operation. All of the coefficients for ridesharing's effect after 18 months of operation are significantly larger than those for the first year (p-values 0.003 to 0.037). As with the overall fatal accident results the estimated effect of ridesharing is somewhat larger for weekend evening crashes than it is for all fatal alcohol-related accidents. I also estimate the time-varying effects of ridesharing on DUI arrests for males aged 21-44. Columns (4)-(6) of Table 5 present these estimates. As with the estimates for all DUI arrests, there is no evidence of a differing effect of ridesharing on DUI arrests for this high-risk group the longer ridesharing services have been operating.

[Table 5 about here.]

6.a.3 Heterogeneity by Public Transit Quality

The previous sections estimated the effects of ridesharing across all cities with populations of 100,000 or greater. It is possible that the effect of introducing a new form of transportation will differ depending on the existing stock of transportation options. The cities in the sample vary widely in their public transit systems, ranging from immense subway systems in cities like New York to simple municipal bus systems. To test whether the effects of ridesharing on drunk driving outcomes differ for cities with extensive transit systems versus those without I separate out the top ten cities in terms of transit usage from the rest of the sample and estimate the effect of ridesharing separately for each.[39] Using alternative measures of high transit usage does not change the results.[40] I estimate the effects of ridesharing separately for the two groups using Equation 3 below.

$$y_{i,t} = \alpha_0 + \beta_1 RS_{i,t} + \beta_2 (RS_{i,t} \times TopTen_i) + \gamma X_{i,t} + \delta_i + \phi_t + \epsilon_{i,t} \tag{3}$$

In this estimation, the coefficient β_1 captures the effect of ridesharing on cities outside the top ten transit cities while $\beta_1 + \beta_2$ represents the effect for cities in the top ten of transit usage. The coefficient β_2 estimates how the effect of ridesharing differs for cities with high transit usage. Table 6 presents the results of this estimation for both fatal alcohol-related

[39] Top ten transit usage cities are in terms of 2011 total unlinked passenger trips as reported in the 2013 American Public Transportation Association Factbook using data from the National Transit Database. These top ten cities represent over 75% of all 2011 unlinked passenger trips reported for 2011.

[40] I find similar results using the top 5, top 20, and top 30 cities in terms of transit usage. I also find the same results using per-capita usage instead of absolute number of trips.

accidents and DUI arrests. Columns (1) and (2) demonstrate that the effect of ridesharing on fatal accidents is consistent across cities regardless of transit quality. Columns (3) and (4), however provide evidence that the effect of ridesharing on DUI arrests does vary for high transit usage cities. When all cities are combined the estimated effect was only marginally significant, and only for some specifications. The results in Table 6 show that there is a significant reduction in DUI arrests for low transit usage cities of between 8.7% and 9.2%. Adding the two ridesharing coefficients β_1 and β_2 gives an estimated effect on DUI arrests for high transit use cities of close to zero. The differing effects of ridesharing on DUI arrests for the two groups of cities compared to the consistent effects for fatal accidents is an interesting result. One potential explanation is that in cities with high transit use, which are also large cities, the number of drunk drivers exceeds the police's capacity to detect and arrest them. If this discrepancy is large enough, even if there is a reduction in the total number of drunk drivers on the road the number of arrests could remain steady as there are still more drunk drivers on the road than the police are able to arrest. Further research into such heterogeneity could provide insights into whether estimates of the effectiveness of drunk driving prevention strategies are accurate when using DUI arrests as their measure of drunk driving prevalence.

[Table 6 about here.]

6.b Ridesharing and Other Crimes

The previous sections have focused on ridesharing's impact on drunk driving. Any effect of ridesharing on drunk driving should be to reduce its incidence, which is what I find. However, drunk driving is not the only outcome that can be affected by the presence of ridesharing services. One significant concern around ridesharing services has been the potential for sexual assaults perpetrated by insufficiently vetted drivers. The common argument is that the lack of the type of fingerprint-based background checks that chauffeurs and taxi drivers receive increases the risk of assault to passengers. Such concerns have led cities such as Austin and Houston to require fingerprint-background checks for ridesharing drivers. In the case of Austin this regulation resulted in Uber and Lyft shutting down service in the city.

In addition to potential assaults committed by drivers, ridesharing has the potential to affect other types of crime by possibly increasing the amount of alcohol consumption or inducing more people to drink outside the home. A high proportion of perpetrators of crimes such as physical and sexual assault had recently consumed alcohol prior to commission of the crime (Greenfeld (1998)). Increasing the number of intoxicated people in public might then result in an increase in assaults and other potentially alcohol-related crimes. Conversely, ridesharing services might make it easier for people to quickly get home after consuming alcohol without the need to rely on potentially more hazardous means such as walking, waiting for public transit, or accepting rides from acquaintances. This could lower their risk of becoming the victim of physical or sexual assault. A priori, the net effect ridesharing will have on these types of crimes is unclear. It is important to estimate these effects to determine whether the benefits of a reduction in drunk driving due to ridesharing is mitigated by increases in other crimes or bolstered by decreases in them.

6.b.1 Overall Other Crime Results

As with the drunk driving analyses I begin by testing for overall effects of ridesharing's introduction. To test this I use Uniform Crime Report data to estimate whether ridesharing had any effect on arrests for these types of crimes. Table 7 presents the results. Rather than increasing the number of physical and sexual assaults, ridesharing is a associated with a 7.9% and 9.3% reduction in each, respectively. Ridesharing has no effect on arrests for drunkenness or liquor law violations, two other measures that could potentially be affected by greater alcohol consumption. As a check to ensure these estimates are not being driven by some unobserved factors affecting arrest rates or crime rates generally[41] I also test for an effect of ridesharing on embezzlement arrests which should be unrelated to both ridesharing's presence and the amount of alcohol consumption. Consistent with the prediction that embezzlement should be unrelated to ridesharing the coefficient is close to zero and not significant.

[Table 7 about here.]

Contrary to prior expectations, these estimates indicate that ridesharing results in significant reductions in arrests for physical and sexual assault and has no effect on arrests for other alcohol-related crimes. A potential explanation is that ridesharing allows individuals consuming alcohol to return home more easily and without needing to rely on friends, acquaintances, or public transportation. This could potentially allow intoxicated people to return home more quickly and reduce the time they are in proximity to others where they could become the victim or perpetrator of these types of crimes. More research could help hone in on the potential mechanisms of this reduction in physical and sexual assaults after ridesharing introduction. These results alleviate concerns that ridesharing might result in higher rates of sexual assault and alcohol-related crime and potentially highlight another social benefit of ridesharing availability.

[41] Which are coincident with the introduction of ridesharing services.

6.b.2 Time-Varying Other Crime Results

Given the significant overall effects of ridesharing on arrests for physical and sexual assaults it can be useful to examine whether these results exhibit any heterogeneity with the duration of ridesharing operation. As with the drunk driving estimates, I test this by estimating Equation 2 using arrests for each of the potentially alcohol-related crimes as the dependent variable. Table 8 presents the results of estimating the effect of ridesharing on each type of crime separately for each six-month period after introduction.

[Table 8 about here.]

The estimates for physical and sexual assaults exhibit the same pattern as fatal alcohol-related auto accidents, with the effect of ridesharing appearing to increase the longer the services have been operating. By the third year of operation, the reductions in both types of assaults are two to three times larger than the initial reduction. This is consistent with the idea that as ridesharing services become more established and expand their base of riders and drivers the impact they have on alcohol-related crime should increase.

7 Robustness

7.a Placebo Test

One method for testing the validity of my estimated effects is to conduct a placebo test. To do so I randomly assign month-city pairs a placebo "treatment" indicator. Each of the 222 true ridesharing launches occur between May 2010 and December 2014 so my random assignment selects 222 cities from the sample and then randomly selects a "treatment" month for each from within this time frame. I repeat this process 1,000 times to create my placebo samples. Using each of these I estimate the effect the "treatment" has on fatal alcohol-related auto accidents. Figure 4 presents a CDF of the estimated coefficients on the ridesharing placebo treatment variable. The red vertical line represents the true estimated coefficient of -0.105 using the actual ridesharing launch dates. Not only is the true coefficient strongly significant ($p<0.001$), it is a full standard deviation larger in magnitude than the largest of the estimated placebo coefficients. This provides support that the estimated effect of ridesharing on fatal alcohol-related accidents is not due to chance.

[Figure 4 about here.]

7.b Non-Alcohol Related Fatal Accidents

If the presence of ridesharing induces some people to use these services rather than drive drunk we should expect to see a drop in fatal alcohol-related accidents after introduction of these services, which is precisely what my results show. One potential concern is that the introduction of ridesharing might be coincident with other unobserved factors affecting traffic safety such as roadway improvements, higher public transit usage, increased preference for safer automobiles, etc. Most of these factors are likely to affect not only the incidence of alcohol-related fatal accidents but non-alcohol related ones as well. To test whether my results are being driven by unobserved factors such as these I estimate whether ridesharing has had any effect on non-alcohol related accidents. The results from these regressions are presented in Table 9. In each model the coefficient on ridesharing is close to zero and highly insignificant. These results provide support that the effects on alcohol-related fatal accidents are not being driven by coincidental unobserved improvements in traffic safety.

[Table 9 about here.]

7.c Low-Cost Ridesharing

Not all ridesharing services are alike. When Uber initially launched in 2010 they only offered higher-end black car services. While possessing the convenience of other app-based ridesharing services, this "Uber Black" service was substantially more costly and typically cost more than a standard taxi. Services like Lyft and Uber's Uber X provide the same convenience but at a substantially lower cost. These services did not begin until 2012. It is possible that the effect of Uber X and Lyft on drunk driving differs from that of the higher-cost Uber Black. My earlier analyses all use the initial date of ridesharing availability as the treatment variable. In 29% of my sample of ridesharing cities, these low-cost options launched after Uber Black was already operating. For these cities, the low-cost option launched on average one year after Uber Black began service.[42] Table 10 presents the overall effect of ridesharing on drunk driving separating out the effect of the higher-cost Uber Black from the lower-cost services. The coefficients on each type of ridesharing service are statistically indistinguishable and are consistent with my earlier estimates of the effect of any ridesharing presence. This consistency suggests that the increased convenience of ridesharing might induce some would-be drunk drivers to substitute to these services even when the cost is higher than the low-cost Lyft and Uber X options.

[Table 10 about here.]

7.d Constant Sample

The completeness of the data for the different outcomes and covariates varies. This causes the sample composition to differ across regression specifications as different outcomes are

[42] This gap varies widely, ranging from 23 days after Uber Black's launch to almost three years after.

used and different covariates are included. To allow for more direct comparison of the effect of ridesharing in the various models I restrict the data to a constant subset which contains all of the variables needed for each model. Table 11 presents the results. The estimated effects of ridesharing on fatal alcohol-related accidents are similar to the results using the full, unrestricted sample. For DUI arrests, I now estimate a significant reduction of 6.9% to 11.0% after ridesharing's introduction. Previously I found a marginally significant reduction in DUIs only in the specification which included the detailed transit data. This reduction appears to be driven by the reduced sample for which these data are available rather than through increased precision gained by controlling for the transit measures. When using this restricted sample without the detailed transit controls I also find large and significant reductions in DUI arrests due to ridesharing's introduction.

[Table 11 about here.]

8 Discussion

In order to interpret the results presented in this study it is important to put them in context of other drunk driving prevention methods. Shults et al (2001) provides a thorough review of the estimated effects of various factors on alcohol-related fatal accidents. They report that laws aimed at curbing alcohol consumption and increasing the legal risk associated with drinking and driving are effective at reducing fatal accidents. Reducing the legal blood alcohol limit (BAC) to 0.08 resulted in a 7% decline in fatal alcohol-related accidents. Raising the legal drinking age from 18 to 21 reduced fatal accidents among 18-20 year olds by 12%. Increasing the risk of detection for drunk drivers by instituting random sobriety checkpoints reduces fatal accidents by 22%. Placing my estimates of the effect of ridesharing in this context, the estimated 10% reduction means introducing ridesharing can be as effective at reducing fatal alcohol-related accidents as lower BAC limits and higher drinking ages. Ridesharing is only half as effective as random sobriety checkpoints, but the reductions due to ridesharing come at little to no public cost whereas the checkpoints require potentially substantial public funds to operate.

It is possible to use the estimated reduction in fatal accidents due to ridesharing to construct counterfactuals for what fatal accidents would have been had ridesharing been more or less prevalent. Figure 5 presents the annual number of fatal accidents in the sample cities under two alternative scenarios. The top line represents the level of fatal accidents absent ridesharing entirely. The central line presents the true number of fatal accidents. The gap between the two grows over time as ridesharing services enter more and more cities. The bottom line presents the opposite extreme. This line shows what fatal accidents would have been had ridesharing been present since 2010 in every sample city. Using these, I estimate that ridesharing's presence has resulted in over 500 fewer fatal accidents since it's introduction in 2010. This corresponds to a monetary benefit of over $4.6 billion over five years.[43] Were ridesharing to be present in every sample city this benefit would grow significantly, reducing fatal accidents by over 450 each year for annual benefit of over $4 billion.

[Figure 5 about here.]

The reduction in fatal accidents due to ridesharing has grown as these services have expanded to more and more cities. Figure 6 shows how the aggregate reduction in the economic cost of drunk driving fatalities increases over time. It is important to note that the accident-reduction effects of ridesharing's presence persist as long as the services continue operating which will also increase the accumulated economic harm reduction over time. However, as I presented earlier, the effect of ridesharing may change the longer the services have been operating. Using the heterogeneous effects I estimated, Figure 7 presents the aggregate reduction in economic harm due to drunk driving fatalities accounting for this non-constant effect of ridesharing. I found that the effect of ridesharing increases with operating duration, accordingly the estimated aggregate harm reduction accounting for this heterogeneity increases to $4.8 billion by the end of 2014. Since many of the sample cities had ridesharing for one year or less by that date this impact will increase more quickly in 2015 and later.

[Figure 6 about here.]

[Figure 7 about here.]

9 Conclusion

Drunk driving is a significant concern in the U.S., resulting in over 11,000 deaths and 360,000 injuries each year. Offering other forms of transportation as alternatives to self-driving may encourage individuals to utilize those options rather than drive drunk. Ridesharing services such as Lyft and Uber offer a more convenient and potentially cheaper alternative to traditional taxis and alleviate the capacity constraints faced by taxis due to municipal licensing regulations. To test whether ridesharing has indeed reduced drunk driving I use a difference-in-differences design to estimate the effect of ridesharing introduction on fatal alcohol-related auto accidents and DUI arrests. Controlling for important co-factors such as unemployment rates and public transportation availability I estimate that ridesharing

[43] This estimate assumes a single fatality per accident uses the Department of Transportation's recommended value of a statistical life of $9.1 million.

reduces fatal alcohol-related auto accidents by 10% to 11.4%. I find that ridesharing reduces DUI arrests by 8.7% to 9.2% in cities with low to moderate transit usage, but has no effect in cities where transit usage is very high. These results provide strong evidence that the presence of ridesharing services induces a large number of people who would otherwise drive drunk to take alternative transportation. Contrary to common concerns regarding ridesharing's potential to increase incidence of other crimes I find that the introduction of ridesharing corresponds to substantial reductions in arrests for sexual and physical assault of 9.3% and 7.9% respectively. These large estimated benefits suggest that facilitating ridesharing services can be a potent tool for reducing drunk driving and its associated costs.

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Figure 1: Growth of Ridesharing Services

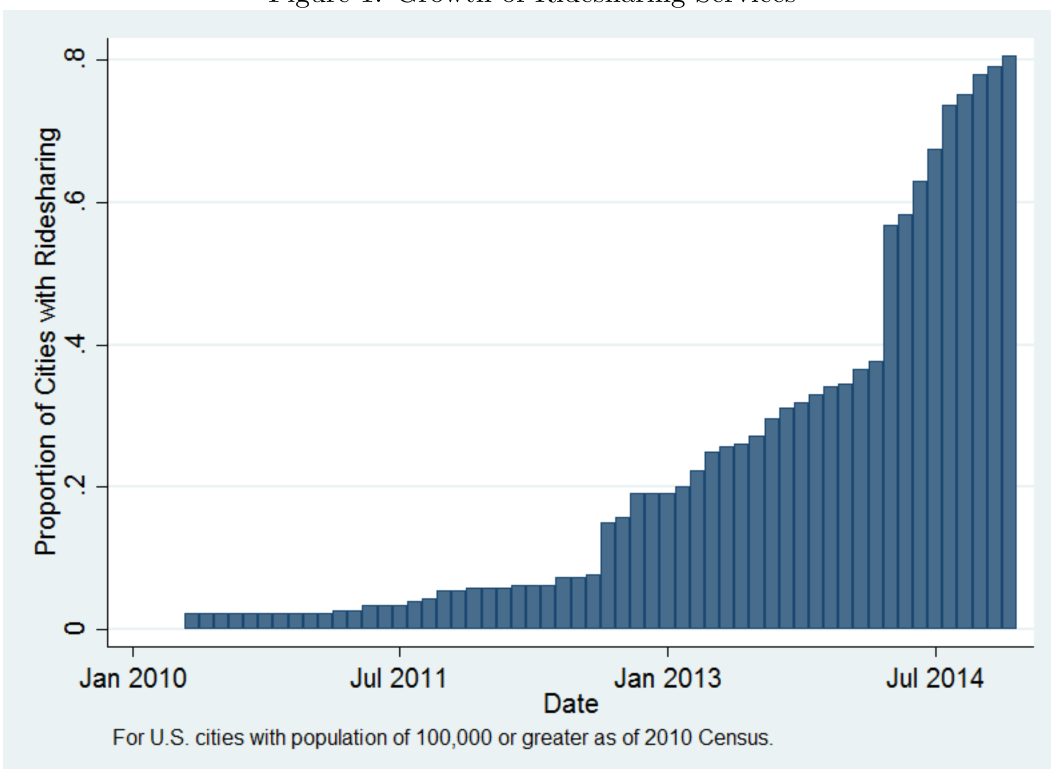

Figure 2: Fatal Accident Pre-Ridesharing Trend Comparison

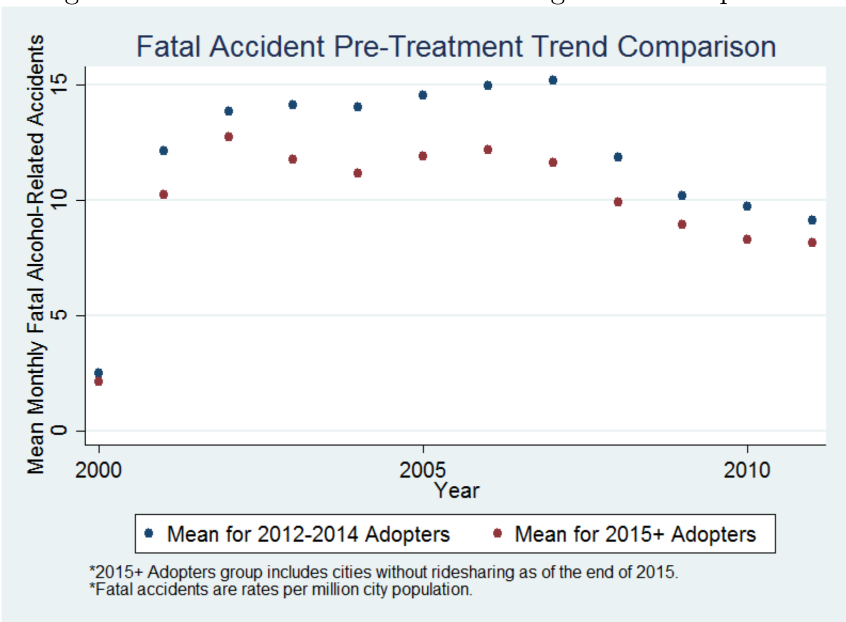

Figure 3: DUI Arrests Pre-Ridesharing Trend Comparison

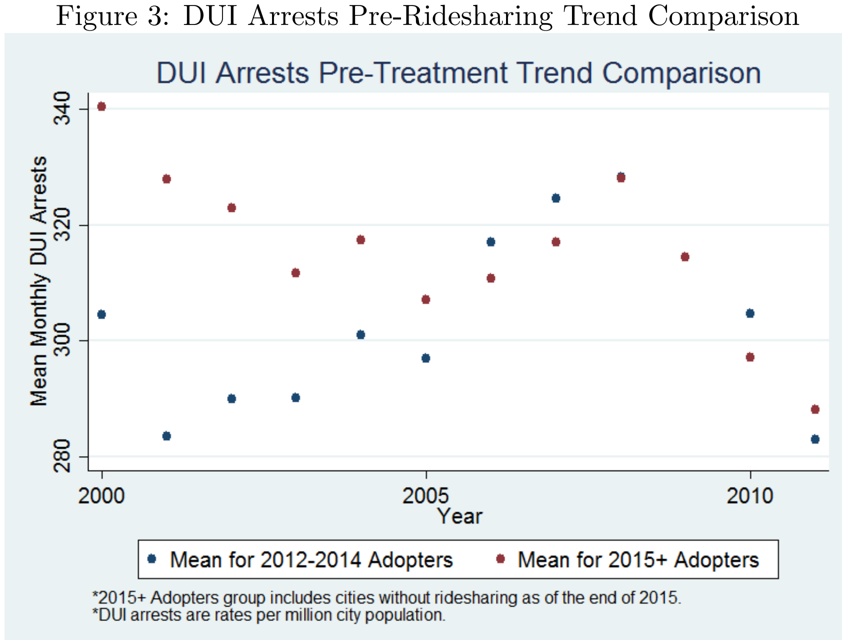

Figure 4: Placebo Test Results

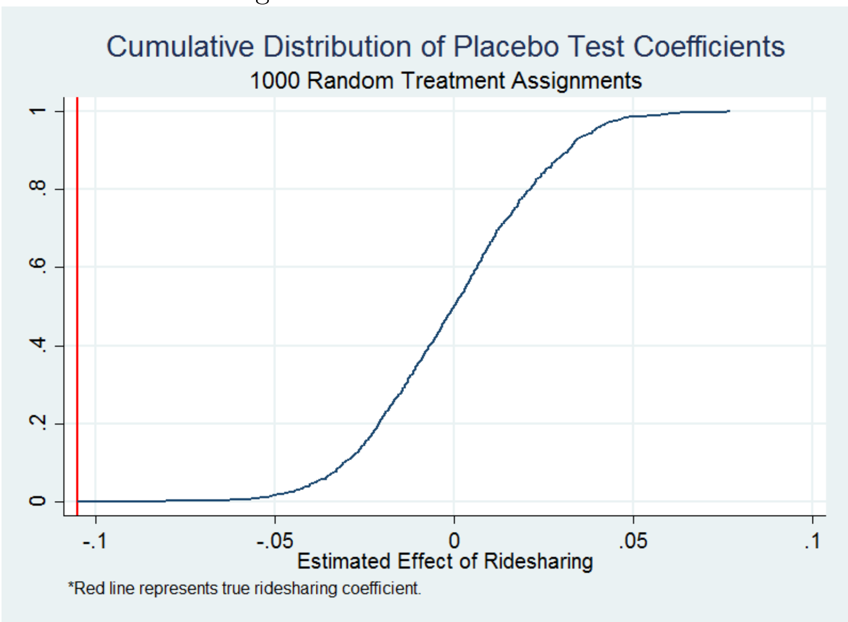

Figure 5: Counter-Factual Fatal Accidents

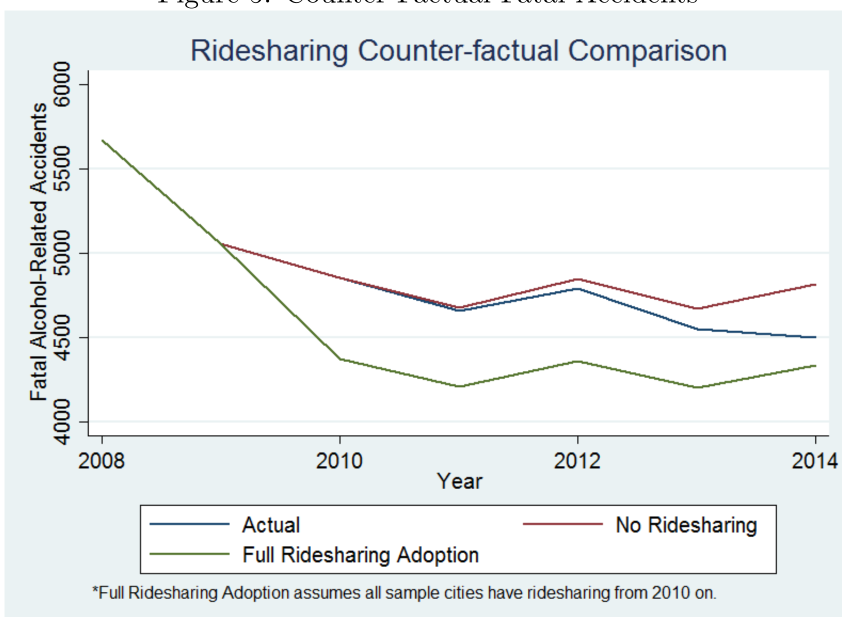

Figure 6: Aggregate Economic Harm Reduction

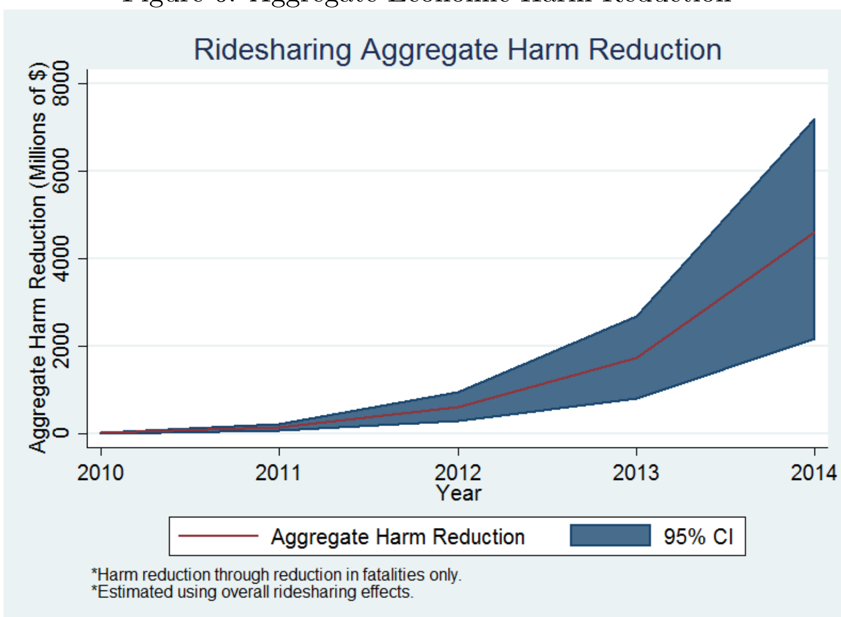

Figure 7: Aggregate Economic Harm Reduction - Heterogeneous Effects

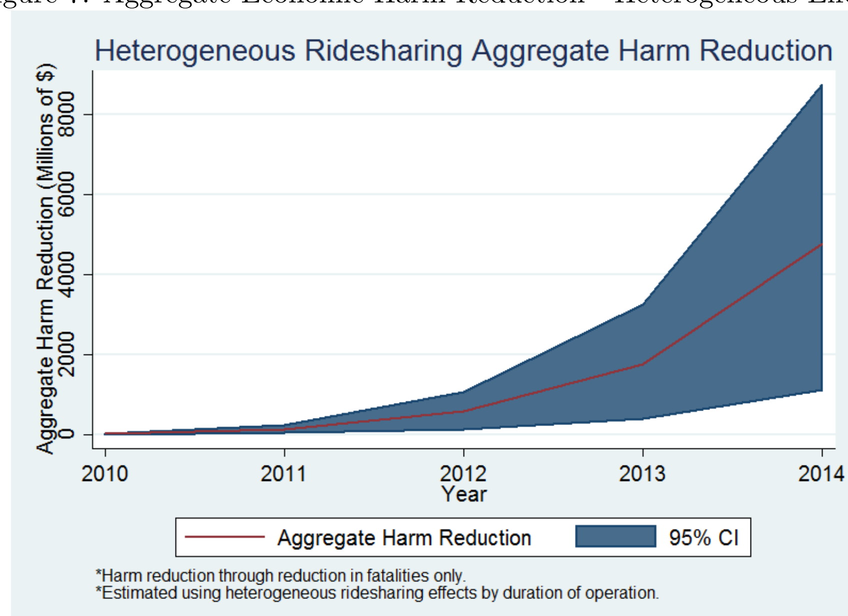

Table 1: Summary Statistics

	2010 Launch	2011 Launch	2012 Launch	2013 Launch	2014 Launch	No Launch By 2014
Fatal Alcohol-Related Crashes						
Mean	0.41	0.65	0.59	0.86	0.99	1.02
Std. Dev.	0.64	0.79	0.76	1.12	1.13	1.21
DUI Arrests						
Mean	18.28	13.13	36.36	25.76	30.26	36.73
Std. Dev.	12.72	17.87	23.36	16.56	19.08	38.49
Population (2010 Census)						
Mean	250,370	1,340,212	454,324	297,388	249,889	168,962
Std. Dev.	256,519	2,431,447	681,539	243,377	241,651	94,208
Unemployment Rate						
Mean	5.77	5.72	5.20	6.42	6.12	6.08
Std. Dev.	2.12	1.97	2.05	3.10	2.61	2.55
Rail Transit Miles						
Mean	42.38	126.66	47.54	24.01	3.46	2.48
Std. Dev.	42.61	140.43	54.80	49.18	12.97	14.61
Bus Transit Miles						
Mean	955.4	2,048.3	2,073.4	1,148.6	663.5	363.2
Std. Dev.	546.8	2,227.3	1,617.6	1,295.3	647.4	366.7
Bus Transit Miles (Excl. ROW)						
Mean	34.11	88.89	70.06	18.17	7.48	1.14
Std. Dev.	45.49	161.67	80.41	42.72	31.61	8.46

Fatal accidents and DUI arrests are monthly per 100,000 population.

Population statistics are based on 2010 Census.

The sample of cities contains all U.S. cities with 100,000 population or greater in 2010.

Transit mileage data are not available for all sample cities in all months.

All figures are monthly averages prior to May 2010, the date of first ridesharing introduction.

Table 2: Overall Effect on Drunk Driving

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Rideshare	-0.049**	-0.105***	-0.121***	-0.021	0.002	-0.071+
	(0.015)	(0.028)	(0.034)	(0.044)	(0.038)	(0.042)
Unemployment Rate	-0.022***	-0.043***	-0.041***	0.012	0.014+	0.010
	(0.003)	(0.005)	(0.006)	(0.009)	(0.008)	(0.009)
Light Rail	-0.059	-0.137*	-0.136*	-0.050	-0.010	0.087
	(0.047)	(0.070)	(0.064)	(0.122)	(0.110)	(0.106)
Rail Miles			0.013			-0.204
			(0.041)			(0.175)
Bus Miles (Total)			-0.005*			0.004
			(0.002)			(0.005)
Bus Miles (Excl. ROW)			-0.141*			0.047
			(0.066)			(0.083)
City FE	✓	✓	✓	✓	✓	✓
Month x Year FE	✓	✓	✓	✓	✓	✓
Population Decile	✓	✓	✓	✓	✓	✓
Estimation	OLS	Neg. Bin.	Neg. Bin.	OLS	Neg. Bin.	Neg. Bin.
N	49130	49130	30666	39372	39372	24774
R^2	0.485	0.172	0.169	0.479	0.204	0.209

Standard errors in parentheses, clustered at the city level.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Rail and bus miles are in units of 100 directional route miles.

Transit mileage and DUI arrest data are not available for all sample cities in all months.

Table 3: Effect on Drunk Driving at High-Risk Times and for High-Risk Groups

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Rideshare	-0.032*	-0.120**	-0.140**	-0.011	0.005	-0.064
	(0.013)	(0.039)	(0.046)	(0.042)	(0.039)	(0.043)
Unemployment Rate	-0.014***	-0.044***	-0.039***	0.007	0.009	0.006
	(0.003)	(0.006)	(0.008)	(0.008)	(0.008)	(0.009)
Light Rail	-0.048	-0.163**	-0.148**	-0.033	0.009	0.087
	(0.032)	(0.058)	(0.055)	(0.114)	(0.108)	(0.106)
Rail Miles			0.034			-0.238
			(0.042)			(0.173)
Bus Miles (Total)			-0.003			0.004
			(0.003)			(0.005)
Bus Miles (Excl. ROW)			-0.139*			0.032
			(0.061)			(0.082)
City FE	✓	✓	✓	✓	✓	✓
Month x Year FE	✓	✓	✓	✓	✓	✓
Population Decile	✓	✓	✓	✓	✓	✓
Estimation	OLS	Neg. Bin.	Neg. Bin.	OLS	Neg. Bin.	Neg. Bin.
N	49130	49130	30666	39372	39372	24774
R^2	0.426	0.147	0.145	0.460	0.217	0.224

Standard errors in parentheses, clustered at the city level.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Rail and bus miles are in units of 100 directional route miles.

Transit mileage data are not available for all sample cities in all months.

High-risk times for fatal accidents are between 5pm and 4am Friday through Sunday.

High-risk groups for DUI arrests are males aged 21-44.

Table 4: Time-Varying Effect on Drunk Driving

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Rideshare Tenure						
0 - 6 Months	-0.049**	-0.091**	-0.090*	-0.048	-0.005	-0.068+
	(0.017)	(0.031)	(0.037)	(0.040)	(0.031)	(0.039)
6 - 12 Months	-0.038+	-0.083*	-0.109*	0.011	0.017	-0.064
	(0.022)	(0.038)	(0.045)	(0.047)	(0.040)	(0.051)
12 - 18 Months	-0.049+	-0.111*	-0.150*	-0.014	-0.022	-0.103+
	(0.025)	(0.050)	(0.067)	(0.055)	(0.048)	(0.062)
18 - 24 Months	-0.101**	-0.204***	-0.240***	0.014	0.032	-0.084
	(0.032)	(0.057)	(0.071)	(0.088)	(0.065)	(0.071)
> 24 Months	-0.034	-0.130*	-0.151*	-0.030	0.005	-0.026
	(0.030)	(0.059)	(0.076)	(0.088)	(0.076)	(0.080)
Unemployment Rate	-0.022***	-0.043***	-0.041***	0.012	0.014+	0.010
	(0.003)	(0.005)	(0.006)	(0.009)	(0.008)	(0.009)
Light Rail	-0.059	-0.135*	-0.136*	-0.052	-0.011	0.089
	(0.047)	(0.069)	(0.063)	(0.122)	(0.110)	(0.106)
City FE	√	√	√	√	√	√
Month x Year FE	√	√	√	√	√	√
Population Decile	√	√	√	√	√	√
Transit Detail			√			√
Estimation	OLS	Neg. Bin.	Neg. Bin.	OLS	Neg. Bin.	Neg. Bin.
N	49130	49130	30666	39372	39372	24774
R^2	0.485	0.172	0.169	0.479	0.204	0.209

Standard errors in parentheses, clustered at the city level.

Rideshare tenure ranges are exclusive of the first number and inclusive of the second.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Rail and bus miles are in units of 100 directional route miles.

Transit mileage and DUI arrest data are not available for all sample cities in all months.

Table 5: Time-Varying Effect on Drunk Driving for High-Risk Times and Groups

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Rideshare Tenure						
0 - 6 Months	-0.031*	-0.098*	-0.089	-0.039	-0.004	-0.059
	(0.015)	(0.046)	(0.055)	(0.037)	(0.032)	(0.038)
6 - 12 Months	-0.011	-0.048	-0.076	0.015	0.020	-0.059
	(0.019)	(0.054)	(0.063)	(0.045)	(0.041)	(0.051)
12 - 18 Months	-0.026	-0.119*	-0.227**	-0.012	-0.021	-0.102
	(0.020)	(0.059)	(0.076)	(0.053)	(0.051)	(0.063)
18 - 24 Months	-0.083***	-0.302***	-0.331***	0.036	0.043	-0.080
	(0.024)	(0.079)	(0.090)	(0.079)	(0.069)	(0.071)
> 24 Months	-0.058*	-0.260***	-0.273**	0.004	0.019	-0.011
	(0.027)	(0.079)	(0.086)	(0.080)	(0.076)	(0.079)
Unemployment Rate	-0.014***	-0.044***	-0.039***	0.007	0.009	0.006
	(0.003)	(0.006)	(0.008)	(0.008)	(0.008)	(0.009)
Light Rail	-0.048	-0.163**	-0.151**	-0.034	0.009	0.089
	(0.032)	(0.058)	(0.053)	(0.114)	(0.108)	(0.106)
City FE	√	√	√	√	√	√
Month x Year FE	√	√	√	√	√	√
Population Decile	√	√	√	√	√	√
Transit Detail			√			√
Estimation	OLS	Neg. Bin.	Neg. Bin.	OLS	Neg. Bin.	Neg. Bin.
N	49130	49130	30666	39372	39372	24774
R^2	0.426	0.148	0.146	0.460	0.217	0.224

Standard errors in parentheses, clustered at the city level.

Rideshare tenure exclusive of the first number and inclusive of the second.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Rail and bus miles are in units of 100 directional route miles.

Transit mileage data are not available for all sample cities in all months.

High-risk times for fatal accidents are between 5pm and 4am Friday through Sunday.

High-risk groups for DUI arrests are males aged 21-44.

Table 6: Overall Ridesharing Effect by Transit Usage

	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests
Rideshare	-0.112**	-0.124***	-0.097*	-0.091*
	(0.036)	(0.038)	(0.045)	(0.046)
Rideshare × High Transit Usage	-0.022	0.009	0.089	0.093
	(0.053)	(0.056)	(0.068)	(0.072)
Unemployment Rate	-0.040***	-0.041***	0.011	0.010
	(0.006)	(0.006)	(0.009)	(0.009)
Light Rail	-0.204**	-0.135*	0.104	0.098
	(0.072)	(0.064)	(0.105)	(0.107)
Rail Miles		0.013		-0.208
		(0.041)		(0.172)
Bus Miles (Total)		-0.005*		0.004
		(0.002)		(0.005)
Bus Miles (Excl. ROW)		-0.142*		0.044
		(0.066)		(0.084)
City FE	√	√	√	√
Month x Year FE	√	√	√	√
Population Decile	√	√	√	√
N	32750	30366	26487	24535
R^2	0.170	0.168	0.207	0.208

Standard errors in parentheses, clustered at the city level.

All models estimated using a negative binomial specification.

Cities in top 10 by 2011 NTD unlinked passenger trips are "High Transit Use".

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Rail and bus miles are in units of 100 directional route miles.

Table 7: Ridesharing's Effect on Other Crimes

	(1) Sexual Assault	(2) Physical Assault	(3) Drunkenness	(4) Liquor Law Violations	(5) Embezzlement
Rideshare	-0.098*	-0.082***	-0.009	0.047	0.011
	(0.046)	(0.025)	(0.067)	(0.070)	(0.053)
Unemployment Rate	-0.026	-0.014*	0.029*	-0.028*	0.017
	(0.019)	(0.007)	(0.012)	(0.013)	(0.018)
City FE	√	√	√	√	√
Month x Year FE	√	√	√	√	√
Population Decile	√	√	√	√	√
Light Rail Indicator	√	√	√	√	√
N	22332	40378	24065	34454	16885
R^2	0.252	0.228	0.193	0.189	0.196

Standard errors in parentheses, clustered at the city level.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

All specifications estimated using the Negative Binomial model.

Arrest data are not available for all sample cities in all months.

Table 8: Time-Varying Effect on Other Crimes

	(1) Sexual Assault	(2) Physical Assault	(3) Drunkenness	(4) Liquor Law Violations	(5) Embezzlement
Rideshare Tenure					
0 - 6 Months	-0.072^{+}	-0.072***	-0.024	0.033	-0.037
	(0.037)	(0.018)	(0.043)	(0.059)	(0.050)
6 - 12 Months	-0.057	-0.069**	-0.065	0.051	0.090
	(0.053)	(0.026)	(0.061)	(0.099)	(0.077)
12 - 18 Months	-0.164*	-0.072*	-0.024	0.087	-0.014
	(0.065)	(0.033)	(0.083)	(0.100)	(0.076)
18 - 24 Months	-0.154*	-0.103**	-0.006	0.037	0.023
	(0.075)	(0.039)	(0.114)	(0.109)	(0.085)
$>$ 24 Months	-0.223*	-0.173**	0.192	0.012	0.018
	(0.102)	(0.056)	(0.234)	(0.154)	(0.091)
Unemployment Rate	-0.026	-0.015*	0.029*	-0.028*	0.017
	(0.019)	(0.007)	(0.012)	(0.013)	(0.018)
City FE	√	√	√	√	√
Month x Year FE	√	√	√	√	√
Population Decile	√	√	√	√	√
Light Rail Indicator	√	√	√	√	√
N	22332	40378	24065	34454	16885
R^2	0.252	0.228	0.193	0.189	0.196

Rideshare tenure ranges are exclusive of the first number and inclusive of the second.

Standard errors in parentheses, clustered at the city level.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

All specifications estimated using the Negative Binomial model.

Arrest data are not available for all sample cities in all months.

Table 9: Ridesharing's Effect on Non-Alcohol Related Crashes

	(1) OLS	(2) Neg. Bin.	(3) Neg. Bin.
Rideshare	-0.005	-0.012	-0.003
	(0.015)	(0.021)	(0.026)
Unemployment Rate	-0.008**	-0.013**	-0.017***
	(0.003)	(0.004)	(0.004)
Light Rail	-0.027	-0.036	-0.023
	(0.031)	(0.043)	(0.043)
Rail Miles			-0.060***
			(0.017)
Bus Miles (Total)			-0.003
			(0.002)
Bus Miles (Excl. ROW)			-0.055
			(0.044)
City FE	√	√	√
Month x Year FE	√	√	√
Population Decile	√	√	√
N	49130	49130	30666
R^2	0.551	0.195	0.192

Standard errors in parentheses, clustered at the city level.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Rail and bus miles are in units of 100 directional route miles.

Transit mileage data are not available for all sample cities in all months.

Table 10: Overall Effect on Drunk Driving - Low-Cost Services Separate

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Low-Cost Rideshare	-0.047**	-0.094**	-0.102*	-0.034	-0.014	-0.090+
	(0.018)	(0.033)	(0.040)	(0.052)	(0.043)	(0.049)
Uber Black Only	-0.052*	-0.125***	-0.152**	-0.002	0.024	-0.033
	(0.021)	(0.039)	(0.051)	(0.048)	(0.041)	(0.045)
Unemployment Rate	-0.022***	-0.043***	-0.041***	0.012	0.014+	0.009
	(0.003)	(0.005)	(0.006)	(0.009)	(0.008)	(0.009)
Light Rail	-0.059	-0.137*	-0.137*	-0.050	-0.010	0.087
	(0.047)	(0.070)	(0.064)	(0.122)	(0.110)	(0.106)
Rail Miles			0.013			-0.207
			(0.041)			(0.175)
Bus Miles (Total)			-0.005*			0.004
			(0.002)			(0.005)
Bus Miles (Excl. ROW)			-0.141*			0.048
			(0.066)			(0.082)
City FE	√	√	√	√	√	√
Month x Year FE	√	√	√	√	√	√
Population Decile	√	√	√	√	√	√
Estimation	OLS	Neg. Bin.	Neg. Bin.	OLS	Neg. Bin.	Neg. Bin.
N	49130	49130	30666	39372	39372	24774
R^2	0.485	0.172	0.169	0.479	0.204	0.209

Standard errors in parentheses, clustered at the city level.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Rail and bus miles are in units of 100 directional route miles.

Transit mileage and DUI arrest data are not available for all sample cities in all months.

Low-cost rideshare options include Lyft, UberX, and Sidecar.

Table 11: Overall Effect on Drunk Driving - Constant Sample

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Rideshare	-0.079***	-0.111**	-0.097**	-0.117*	-0.084*	-0.071+
	(0.023)	(0.041)	(0.040)	(0.052)	(0.043)	(0.042)
Unemployment Rate	-0.024***	-0.046***	-0.046***	0.009	0.010	0.010
	(0.005)	(0.007)	(0.007)	(0.010)	(0.009)	(0.009)
Light Rail	-0.153+	-0.202*	-0.028	0.106	0.098	0.087
	(0.091)	(0.089)	(0.081)	(0.119)	(0.111)	(0.106)
Rail Miles			-0.481*			-0.204
			(0.193)			(0.175)
Bus Miles (Total)			-0.007			0.004
			(0.005)			(0.005)
Bus Miles (Excl. ROW)			-0.152*			0.047
			(0.067)			(0.083)
City FE	√	√	√	√	√	√
Month x Year FE	√	√	√	√	√	√
Population Decile	√	√	√	√	√	√
Estimation	OLS	Neg. Bin.	Neg. Bin.	OLS	Neg. Bin.	Neg. Bin.
N	24774	24774	24774	24774	24774	24774
R^2	0.492	0.167	0.168	0.476	0.208	0.209

Standard errors in parentheses, clustered at the city level.

$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Rail and bus miles are in units of 100 directional route miles.

Sample restricted to city-month pairs present in UCR and NTD data.