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Responsible Options: Empirical Analyses on the Effects of Alternative Transportation on Drunk Driving

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Responsible Options: Empirical Analyses on the Effects of Alternative Transportation on Drunk Driving

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Responsible Options: Empirical Analyses on the Effects of Alternative Transportation on Drunk Driving

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This dissertation examines the impact of different types of alternative transportation options on rates of drunk driving. The first chapter explores the effect of ridesharing services such as Uber and Lyft on fatal alcohol-related auto accidents and DUI/DWI arrests. This study uses a difference-in-differences methodology and the gradual expansion of ridesharing to cities across the U.S. to identify the impact of ridesharing. The second chapter estimates how the development and expansion of public rapid transit systems affects fatal alcohol-related auto accidents and DUI/DWI arrests. Utilizing the development of rapid transit systems since the mid-1970s this study applies difference-in-differences to estimate the causal effects on drunk driving measures. The third and final chapter utilizes unique data on the home addresses of individuals arrested for drunk driving to estimate the effect of late night bus service on drunk driving arrests in Austin, Texas. The causal effects are estimated using the differential availability of late-night bus service based on the day

of the week and a difference-in-differences methodology based on whether or not individuals live within walking distance of late-night routes. These three studies can provide important evidence to policymakers in their efforts to curb drunk driving, a problem which kills over 10,000 people and causes over $50 billion in damage each year in the United States.

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Chapter 1

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

1.1 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.

[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.

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] 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 can 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 increase over time. For DUI arrests the results reveal heterogeneous effects for cities

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

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

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 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.

1.2 Background

1.2.1 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.[6] The incidence of drunk driving and the attendant costs vary

[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] 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

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.[7]

Efforts to reduce drunk driving typically take the form of greater enforcement or stricter punishments. The severity of penalties for drunk driving varies among 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.[8]

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 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

[7]Fatal Accident Reporting System, 2000-2014. National Highway Traffic Safety Administration. Web. 26 Dec 2015.

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

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 measure more accurately 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.

1.2.2 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.[9] 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

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

a population of 100,000 or more.[10] In many of these cities, ridesharing services began operations months before city and state officials permitted them to legally operate.[11]

1.3 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 2000 through 2014.[12] I collected city-level unemployment and population data for each of these cities.[13] 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 could represent a lower bound for the effect in cities where ridesharing services have yet to launch.

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

[11] This was the case in Milwaukee, Tampa Bay, Kansas City, several cities in Texas, and many others.

[12] 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.

[13] 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.

Table 1.1: Summary Statistics

	2010 Launch	2011 Launch	2012 Launch	2013 Launch	2014 Launch	No Launch By 2014
Fatal 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						
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
Excl. Bus Miles						
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.

"Excl." refers to exclusive right-of-way.

1.3.1 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.[14] 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.[15] 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.[16]

1.3.2 Alcohol-Related Traffic Fatalities

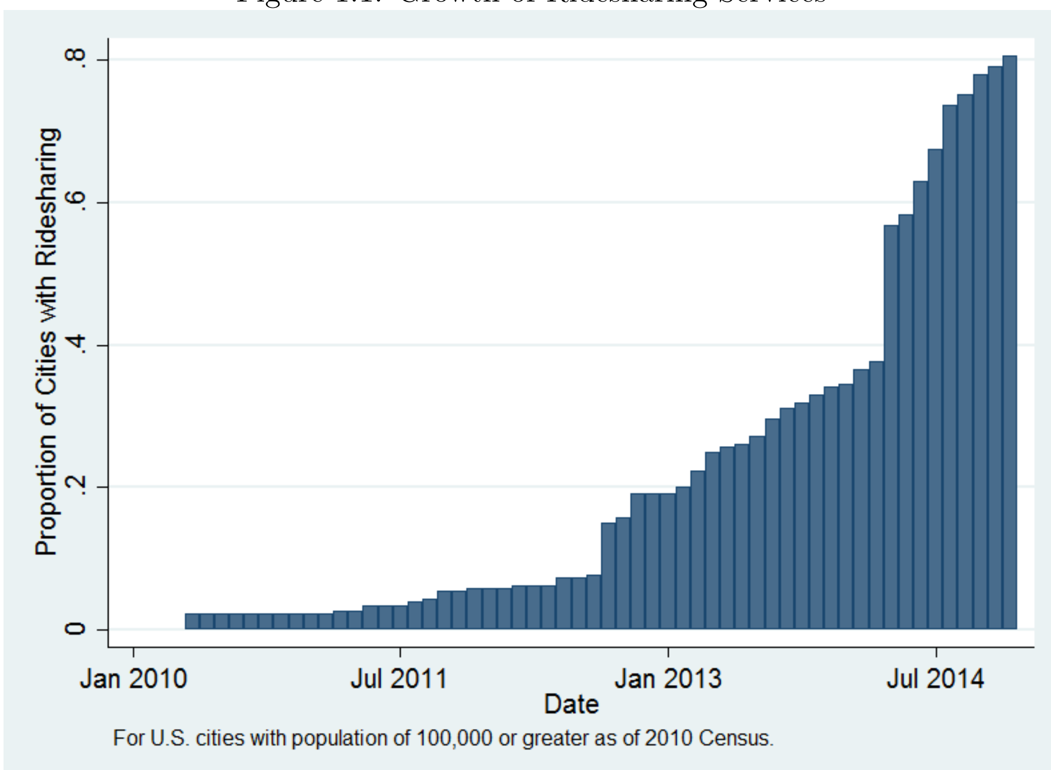
Since 1975 the National Highway Traffic Safety Administration (NHTSA) has collected detailed information on all fatal traffic accidents in the U.S. All traffic accidents on publicly accessible roads resulting in at least one fatality in all 50 U.S. states plus the District of Columbia and Puerto Rico are recorded in

[14] 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.

[15] 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.

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

Figure 1.1: Growth of Ridesharing Services



the NHTSA's Fatality Analysis Reporting System (FARS) database[17]. This database contains detailed information about the time, 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.[18] 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.

1.3.3 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

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

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

data. Although participation in the UCR program is voluntary, over 18,000 law enforcement agencies report arrest data under the program.[19] 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.[20] 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.

1.3.4 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.[21] 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 Transit Database (NTD).[22] Not every transit agency submits reports to the NTD; these data are only available for 69% of the sample cities. For each city that

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

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

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

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

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.[23]

1.4 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.[24] The person must

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

[24] ”Optimal” refers to maximizing the individual's expected utility given their preferences and their perceptions about the risks and benefits associated with each action. It is possible

optimally choose to consume more alcohol than would allow them to drive legally. Again, this decision may take place prior to beginning drinking or it can occur after the individual has already begun consuming alcohol. 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[25], 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 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 alter-

that these risk perceptions differ (perhaps greatly) from the true riskiness of their actions.

[25]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.

native 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.[26] 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

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

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.

1.5 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.[27] Each specification includes controls for the city-level unemployment rate as well as an indicator for the presence of light rail transit.[28] Finally, some specifications include detailed NTD data on the size of each city's bus and rail transit networks.[29]

1.5.1 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.

$$y_{i,t} = \alpha_0 + \beta RS_{i,t} + X_{i,t}\gamma + \delta_i + \phi_t + \epsilon_{i,t} \tag{1.1}$$

$y_{i,t}$ 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.

[27] To do this I calculate population deciles for the sample each year and assign each city to its corresponding decile. Using quadratic population controls instead does not change any of the results.

[28] 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.

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

1.5.2 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.

$$y_{i,t} = \alpha_0 + \sum_{g=1}^{G} \rho_g RS_{i,t,g} + X_{i,t}\gamma + \delta_i + \phi_t + \epsilon_{i,t} \tag{1.2}$$

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.[30] I also include groups for each six month period prior to introduction, excluding the one immediately before ridesharing's launch. These groups are more than 24 months prior, 24-18 months prior, 18-12 months prior, and 12-6 months prior. The coefficients ρ_g represent the the effect of ridesharing for each operating duration group.

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

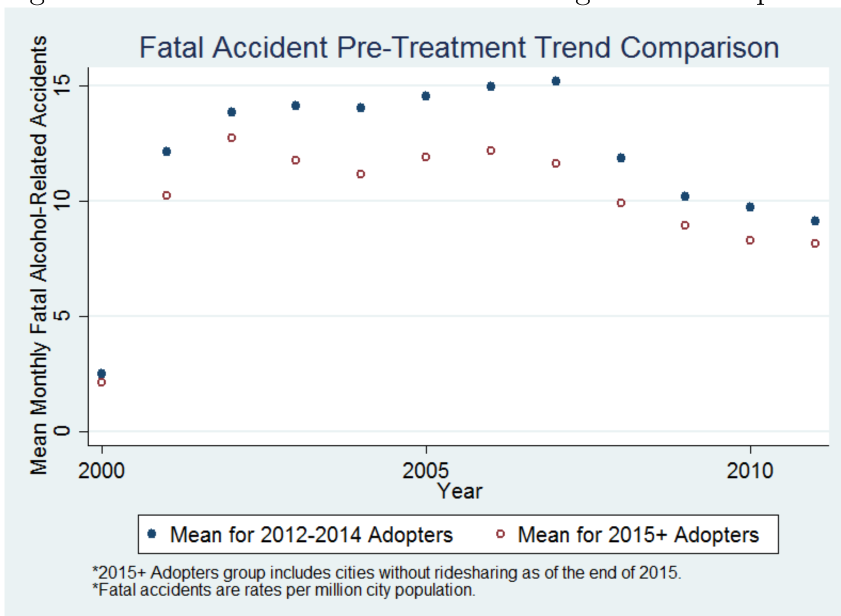
1.5.3 Identification

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.[31] 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.[32] 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

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

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

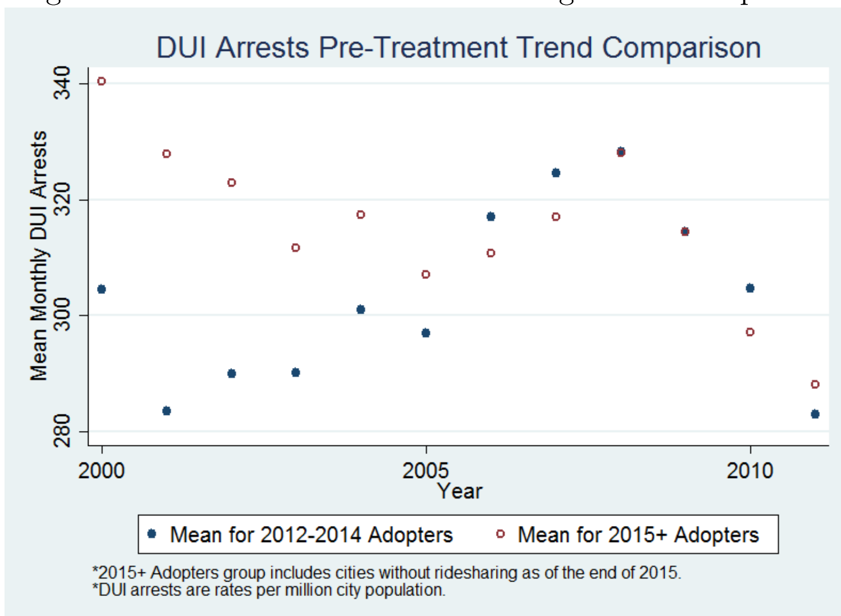
Figure 1.2: Fatal Accident Pre-Ridesharing Trend Comparison



shows that DUI arrests in early-adoption cities were not declining relative to later-adoption cities prior to ridesharing's introduction.

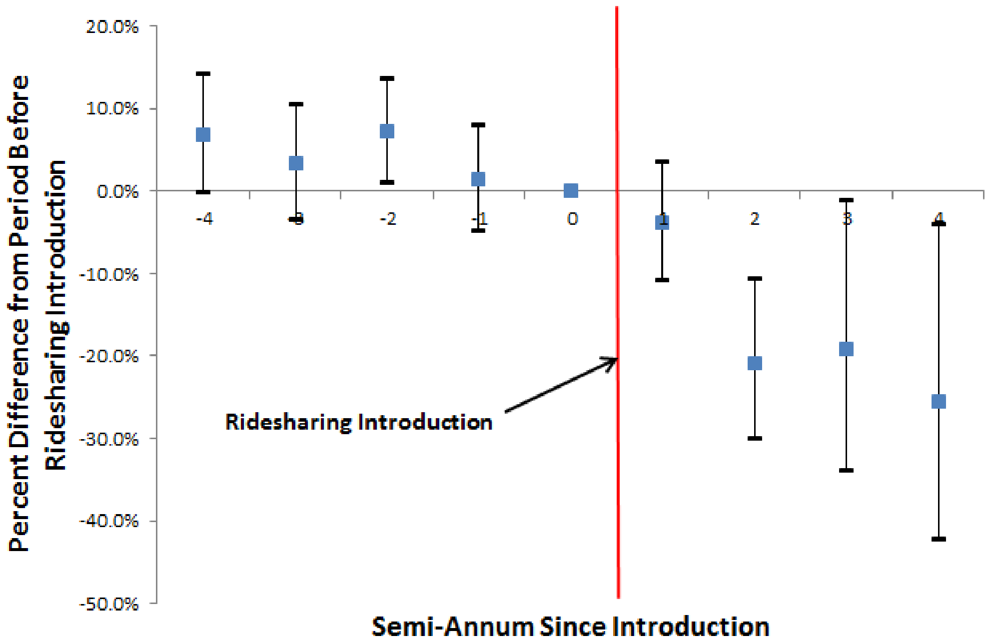
Another method for testing for identification in difference-in-differences settings is to conduct an "event study" analysis. This method is similar to the technique I describe in the previous section for examining the time-varying effects of ridesharing. The key difference is that instead of only testing the effect of ridesharing in each six-month period post-introduction it also tests for effects in each six-month period pre-introduction. If the identification strategy is correct there should be little to no effect of these pre-introduction periods. Figures 4 and 5 present the estimated coefficients and 95% confidence intervals for this event study analysis for fatal alcohol-related auto accidents

Figure 1.3: DUI Arrests Pre-Ridesharing Trend Comparison



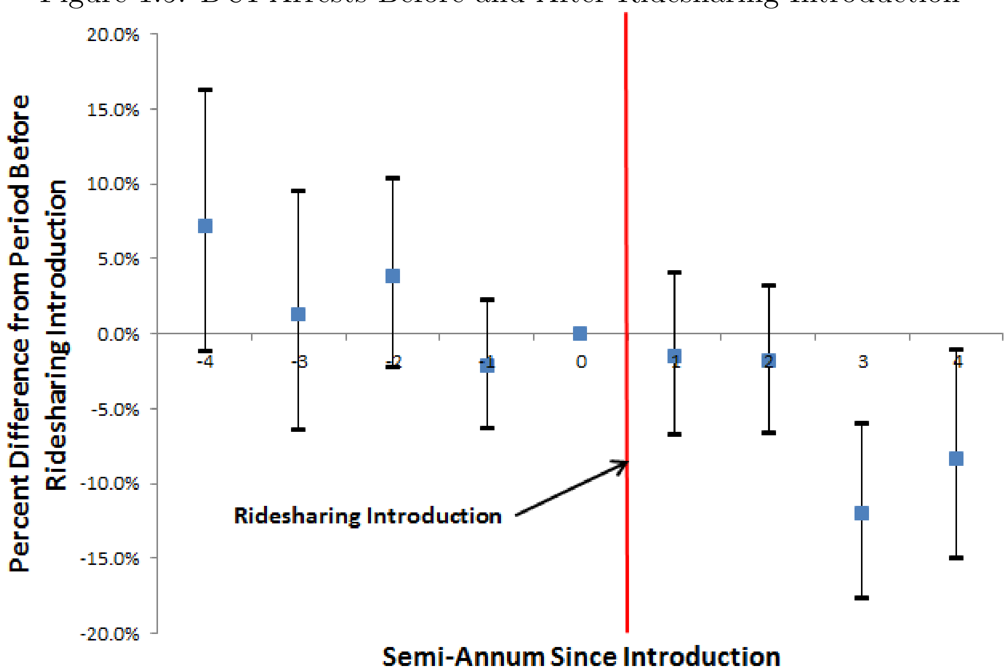
and DUI arrests, respectively. Each shows the estimated effect of ridesharing for each of the four pre-introduction periods prior to the period just before ridesharing introduction (indicated by time period zero) as well as the four six-month periods following introduction. For each drunk driving outcome there is a slight downward trend in the pre-introduction periods followed a substantial reduction in drunk driving outcomes post-introduction. Most of the pre-introduction coefficients are not statistically distinguishable from zero, providing support for my identification of the effects of ridesharing on drunk driving.

Figure 1.4: Fatal Alcohol-Related Accidents Before and After Ridesharing Introduction



*Time period "0" represents the six-month period prior to ridesharing introduction.
*Estimated using Negative Binomial specification.

Figure 1.5: DUI Arrests Before and After Ridesharing Introduction



*Time period "0" represents the six-month period prior to ridesharing introduction.
*Estimated using Negative Binomial specification.

1.6 Results

1.6.1 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.

1.6.1.1 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.[33] The coefficient on ridesharing in specification (1) indicates a significant reduction in fatal accidents following ridesharing introduction.[34] 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%.[35] 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 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).[36] The inclusion of these additional control

[33] 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.

[34] 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.

[35] 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 $exp(coefficient) - 1$.

[36] 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 ($p<0.001$).

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% ($p<0.05$).

The number of fatal alcohol-related accidents increases sharply during nighttime hours on weekends.[37] 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 nighttime hours 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

[37]Weekend nighttime hours are between 5pm and 4am on Fridays, Saturdays, and Sundays.

Table 1.2: 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)
Unemp. 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			-0.005*			0.004
			(0.002)			(0.005)
Excl. Bus Miles			-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.

"Excl." refers to exclusive right-of-way.

of ridesharing as a transportation alternative.[38] 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 nighttime hours. 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, males aged 21 to 44.[39] 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 observed 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

[38]"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.

[39]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.

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.

1.6.1.2 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 prior to and following their launch.[40] 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.[41] 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

[40] After two years of ridesharing operation all observations are grouped into a single "> 24" months category and before 24 months prior are grouped into "< -24".

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

Table 1.3: 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)
Unemp. 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			-0.003			0.004
			(0.003)			(0.005)
Excl. Bus Miles			-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.

"Excl." refers to exclusive right-of-way.

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.

As with the overall results, I repeat the above analysis restricting my attention to fatal alcohol-related accidents which occur on weekend nighttime hours which are high-risk times for drunk driving. Columns (1)-(3) of Table 5 present the results. The pattern seen in the time-varying results for all fatal alcohol-related accidents is present in the weekend nighttime 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. As with the overall fatal accident results the estimated effect of ridesharing is somewhat larger for weekend nighttime 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 1.4: 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						
< -24 Months	0.057^{**}	0.148^{***}	0.151^{**}	0.023	-0.012	0.040
	(0.022)	(0.042)	(0.051)	(0.054)	(0.046)	(0.063)
-24 - -18 Months	-0.012	0.023	-0.090^{*}	-0.001	-0.038	-0.003
	(0.022)	(0.040)	(0.037)	(0.039)	(0.033)	(0.043)
-18 - -12 Months	0.042^{*}	0.085^{**}	0.024	-0.016	-0.032	-0.012
	(0.019)	(0.033)	(0.051)	(0.032)	(0.025)	(0.032)
-12 - -6 Months	-0.004	0.0002	-0.029	-0.004	-0.029	0.0004
	(0.018)	(0.032)	(0.037)	(0.028)	(0.019)	(0.023)
0 - 6 Months	-0.031	-0.045	-0.056	-0.047	-0.024	-0.063^{*}
	(0.019)	(0.036)	(0.042)	(0.029)	(0.032)	(0.029)
6 - 12 Months	-0.023	-0.043	-0.080^{+}	0.011	-0.003	-0.060
	(0.023)	(0.039)	(0.047)	(0.038)	(0.032)	(0.044)
12 - 18 Months	-0.037	-0.077	-0.132^{*}	-0.015	-0.040	-0.102^{+}
	(0.025)	(0.050)	(0.065)	(0.049)	(0.042)	(0.057)
18 - 24 Months	-0.089^{**}	-0.172^{***}	-0.228^{***}	0.013	0.014	-0.083
	(0.032)	(0.058)	(0.071)	(0.083)	(0.059)	(0.066)
$>$ 24 Months	-0.027	-0.106^{+}	-0.146^{+}	-0.034	-0.014	-0.030
	(0.030)	(0.060)	(0.076)	(0.086)	(0.073)	(0.078)
Transit Detail			$\checkmark$			$\checkmark$
Estimation	OLS	Neg. Bin.	Neg. Bin.	OLS	Neg. Bin.	Neg. Bin.
N	49130	49130	30666	39372	39372	24774

All specifications include population decile and city and month by year fixed effects.
All specifications control for light rail and unemployment rate.
Standard errors in parentheses, clustered at the city level.
Rideshare tenure ranges are exclusive of the first number and inclusive of the second.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.5: 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						
< -24 Months	0.036*	0.155**	0.177**	0.033	-0.002	0.038
	(0.056)	(0.042)	(0.070)	(0.051)	(0.047)	(0.063)
-24 - -18 Months	-0.001	0.035	0.056	-0.007	-0.033	-0.002
	(0.018)	(0.057)	(0.071)	(0.036)	(0.034)	(0.043)
-18 - -12 Months	0.025	0.090+	-0.017	-0.048	-0.027	-0.006
	(0.017)	(0.049)	(0.057)	(0.029)	(0.026)	(0.033)
-12 - -6 Months	-0.002	-0.013	-0.062	-0.005	-0.023	0.007
	(0.016)	(0.049)	(0.056)	(0.025)	(0.020)	(0.025)
0 - 6 Months	-0.020	-0.052	-0.059	-0.037	-0.018	-0.052+
	(0.016)	(0.052)	(0.059)	(0.027)	(0.023)	(0.030)
6 - 12 Months	-0.001	-0.008	-0.054	0.016	0.005	-0.053
	(0.021)	(0.059)	(0.069)	(0.036)	(0.033)	(0.044)
12 - 18 Months	-0.019	-0.086	-0.218**	-0.013	-0.035	-0.098+
	(0.021)	(0.064)	(0.080)	(0.048)	(0.045)	(0.058)
18 - 24 Months	-0.075**	-0.272***	-0.332***	0.035	0.029	-0.077
	(0.026)	(0.085)	(0.097)	(0.073)	(0.063)	(0.067)
$>$ 24 Months	-0.054+	-0.238**	-0.278***	-0.001	0.004	-0.012
	(0.028)	(0.081)	(0.088)	(0.078)	(0.073)	(0.077)
Transit Detail			✓			✓
Estimation	OLS	Neg. Bin.	Neg. Bin.	OLS	Neg. Bin.	Neg. Bin.
N	49130	49130	30666	39372	39372	24774

All specifications include population decile and city and month by year fixed effects.

All specifications control for light rail and unemployment rate.

Standard errors in parentheses, clustered at the city level.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

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.

1.6.1.3 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.[42] Using alternative measures of high transit usage does not change the results.[43] I focus on transit usage rather than measures of transit system size to capture differences in city residents' observed preference for public transit. I estimate the effects of ridesharing separately for the two groups using Equation 3 below.

$$y_{i,t} = \alpha_0 + \beta_1 RS_{i,t} + \beta_2(RS_{i,t} \times TopTen_i) + \gamma X_{i,t} + \delta_i + \phi_t + \epsilon_{i,t} \qquad (1.3)$$

In this estimation, the coefficient β_1 captures the effect of ridesharing

[42] 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.

[43] 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.

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 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 1.6: 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	-0.022	0.009	0.089	0.093
	(0.053)	(0.056)	(0.068)	(0.072)
Unemp. 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		-0.005*		0.004
		(0.002)		(0.005)
Excl. Bus Miles		-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.
"Excl." refers to exclusive right-of-way.

1.6.2 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.

1.6.2.1 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[44] 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.

Contrary to the concerns of municipal authorities, these estimates indicate that ridesharing results in significant reductions in arrests for physical

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

Table 1.7: 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)
Unemp. 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 Dummy	√	√	√	√	√
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.

and sexual assault and has no effect on arrests for other alcohol-related 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.

1.6.2.2 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.

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. It should be noted that the coefficients on the pre-ridesharing periods are positive and

Table 1.8: Time-Varying Effect on Other Crimes

	(1) Sexual Assault	(2) Physical Assault	(3) Drunkenness	(4) Liquor Law Violations	(5) Embezzlement
Rideshare Tenure					
< -24 Months	0.129**	0.110***	0.098	0.027	0.058
	(0.048)	(0.027)	(0.064)	(0.079)	(0.090)
-24 - -18 Months	0.072*	0.053**	0.083	0.020	-0.009
	(0.035)	(0.018)	(0.051)	(0.061)	(0.062)
-18 - -12 Months	0.061*	0.048**	0.042	0.023	-0.015
	(0.032)	(0.016)	(0.037)	(0.051)	(0.048)
-12 - -6 Months	0.040	0.034**	0.001	-0.009	0.032
	(0.028)	(0.012)	(0.026)	(0.045)	(0.042)
0 - 6 Months	-0.024	-0.028*	0.013	0.041	-0.025
	(0.034)	(0.013)	(0.032)	(0.053)	(0.048)
6 - 12 Months	-0.013	-0.028	-0.030	0.058	0.101
	(0.050)	(0.021)	(0.055)	(0.098)	(0.075)
12 - 18 Months	-0.126*	-0.037	0.008	0.092	-0.004
	(0.063)	(0.029)	(0.080)	(0.097)	(0.070)
18 - 24 Months	-0.117	-0.068+	0.028	0.042	0.031
	(0.072)	(0.035)	(0.111)	(0.100)	(0.080)
$>$ 24 Months	-0.192+	-0.145**	0.215	0.015	0.023
	(0.099)	(0.052)	(0.232)	(0.147)	(0.085)
N	22332	40378	24065	34454	16885

All specifications include population decile and city and month by year fixed effects.

All specifications control for light rail and unemployment rate.

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.

significant, this could indicate that the estimated reductions after ridesharing's introduction are driven by unaccounted for pre-existing trends.

1.7 Robustness

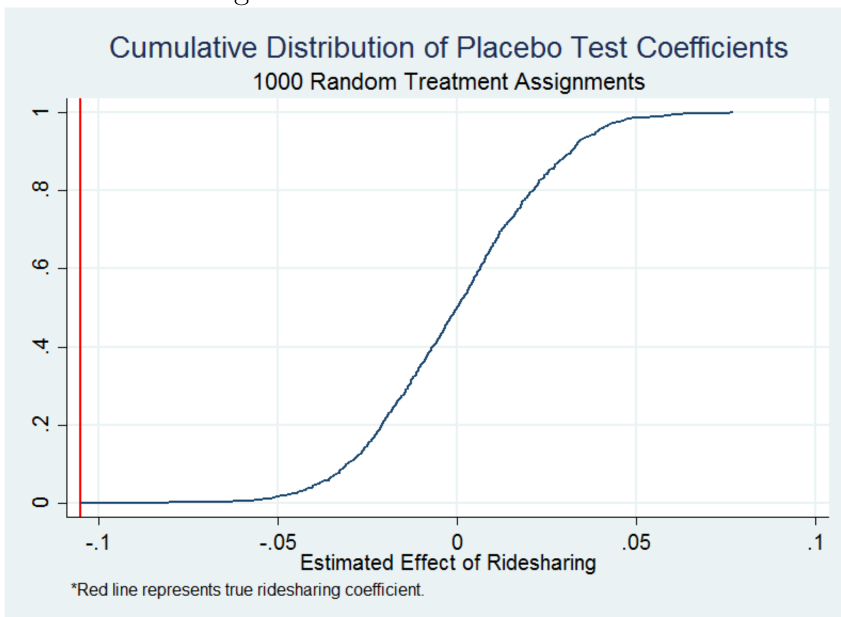
1.7.1 Placebo Test

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 6 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.

1.7.2 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

Figure 1.6: Placebo Test Results



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.

1.7.3 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.[45] 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

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

Table 1.9: Ridesharing's Effect on Non-Alcohol Related Crashes

	(1)	(2)	(3)
	OLS	Neg. Bin.	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.

drunk drivers to substitute to these services even when the cost is higher than the low-cost Lyft and Uber X options.

1.7.4 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 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.

1.8 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

Table 1.10: 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)
Unemp. 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			-0.005*			0.004
			(0.002)			(0.005)
Excl. Bus Miles			-0.141*			0.048
			(0.066)			(0.082)
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

All specifications include population decile and city and month by year fixed effects.
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.
"Excl." refers to exclusive right-of-way.

Table 1.11: Overall Effect on Drunk Driving - Constant Sample

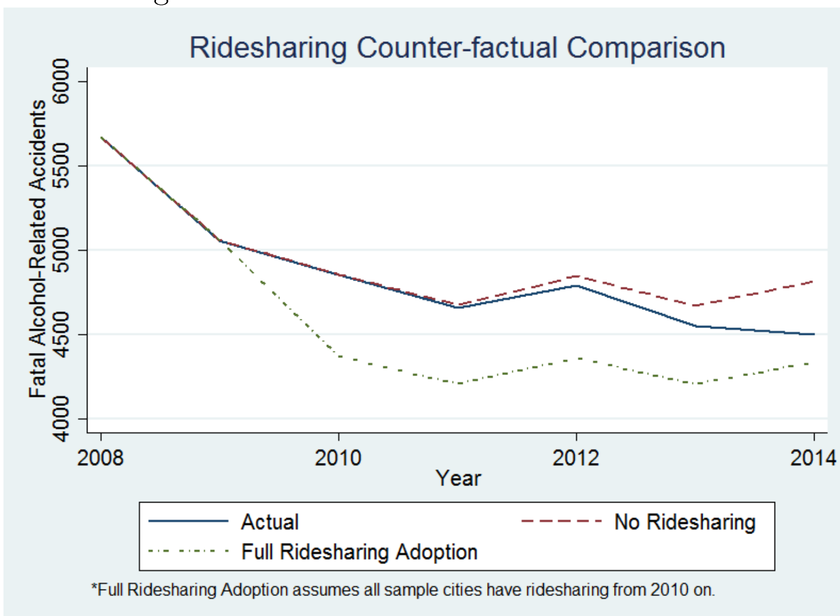
	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Rideshare	-0.079***	-0.111**	-0.097**	-0.117*	-0.084*	-0.071+
	(0.023)	(0.041)	(0.040)	(0.052)	(0.043)	(0.042)
Unemp. Rate	-0.024***	-0.046***	-0.046***	0.009	0.010	0.010
	(0.005)	(0.007)	(0.007)	(0.010)	(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			-0.007			0.004
			(0.005)			(0.005)
Excl. Bus Miles			-0.152*			0.047
			(0.067)			(0.083)
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

All specifications include population decile and city and month by year fixed effects.
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.
"Excl." refers to exclusive right-of-way.

(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 7 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

Figure 1.7: Counter-Factual Fatal Accidents



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.[46] Were ridesharing to be present in every sample city this benefit could grow, reducing fatal accidents by over 450 each year for annual benefit of over \$4 billion. These estimated benefits are solely from reduced fatalities, the total benefit will be higher once property damage and non-fatal injuries are included.

The reduction in fatal accidents due to ridesharing has grown as these services have expanded to more and more cities. Figure 8 shows how the aggre-

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

gate 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 9 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 may increase more quickly in 2015 and later.

1.9 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-

Figure 1.8: Aggregate Economic Harm Reduction

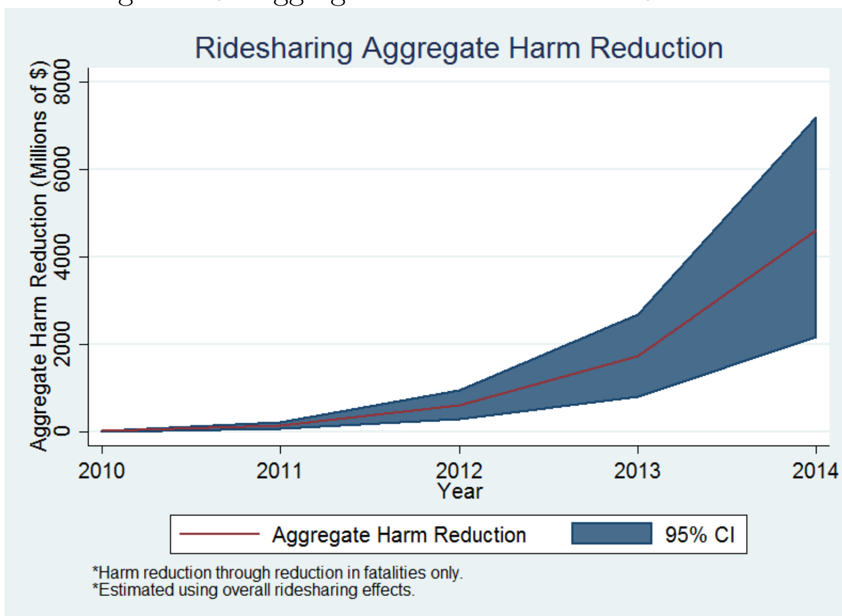
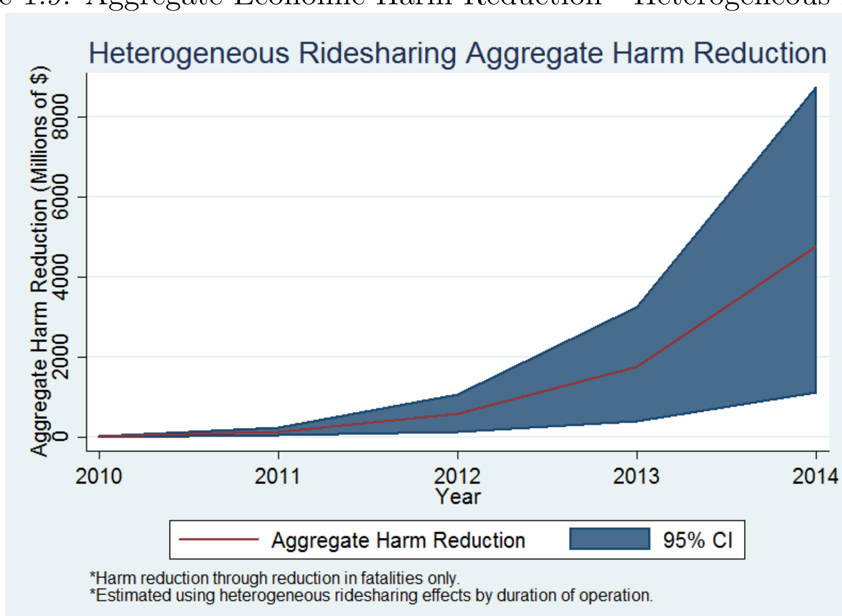


Figure 1.9: Aggregate Economic Harm Reduction - Heterogeneous Effects



ing 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 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. Promotion of these benefits of ridesharing services as well as public subsidization could potentially increase these drunk driving reductions. Further quantitative and qualitative studies can delve into the exact mechanisms by which ridesharing impacts the incidence of drunk driving and assault.

Chapter 2

Quantifying the Effect of Rapid Transit on Drunk Driving

2.1 Introduction

Rapid public transit systems have the potential to alleviate a number of negative outcomes, from road congestion and parking availability to air pollution from automobiles. These systems also have the potential to reduce the incidence of drunk driving if they provide a convenient alternative to self-driving for people who plan to consume alcohol. This last effect is important due to the high prevalence of drunk driving in the United States. Driving under the influence of alcohol causes over 11,000 deaths and 300,000 injuries in the U.S. each year. The monetary cost of these accidents ranges from $49 to $200 billion annually. This high cost makes quantifying the effects of rapid transit on drunk driving rates an essential tool for policymakers tasked with weighing the costs and benefits of developing or expanding rapid transit systems.

Little research exists looking at the effects of building rapid transit systems on drunk driving. Jackson and Owens (2011) estimate the impact of Washington DC extending the operating hours of its subway system in the late 1990s and early 2000s. They find that while there is no measurable effect on drunk driving deaths or arrests city-wide, they do find localized reductions in DUI arrests in areas with both a large number of drinking establishments as well as subway access. In this paper I exploit a more potent source of potential drunk driving reduction, the presence of rapid transit rather than just the operating hours.

Most of the research on drunk driving prevention has focused on either punishment and enforcement or restricting access to alcohol. While these studies find these strategies effective in some cases (Kenkel (1993)) these types of policies can carry significant public costs with little external benefit beyond drunk driving prevention. Rapid transit systems have the potential to reduce drunk driving while simultaneously providing other important benefits to cities such as reduced road congestion and lower traffic pollution.

In this paper I use the gradual build out of rapid transit systems in U.S. cities since the 1970s to identify the causal effect of these systems on fatal alcohol-related auto accidents and DUI/DWI arrests. I gathered data on every line and station opening for sixteen U.S. cities with rapid transit systems (heavy rail, light rail, and/or rapid buses) and combined that with data on all fatal alcohol-related auto accidents and drunk driving arrests from 1975 through 2014.[1] This sample includes all cities with heavy rail (i.e. subways) and a selection of cities with only light rail and rapid bus transit. These non-

[1]The sixteen cities are: Washington DC, Los Angeles, New York, Chicago, Philadelphia, Boston, Atlanta, Miami, Baltimore, Cleveland, San Francisco, Denver, Houston, Buffalo, Charlotte, and Dallas.

heavy rail cities were selected because they developed their systems during the sample period. Using a fixed effects difference-in-differences methodology I find that adding an additional rapid transit line reduces fatal drunk driving accidents by 11.5% to 13.2%. Alternatively, I find that adding an additional station reduces fatal accidents by 1% to 1.2%. For DUI/DWI arrests I find similar reductions of 12.7% to 14.4% in fatal alcohol-related accident for each additional rapid transit line and alternatively a 1.4% to 1.8% reduction per additional rapid transit station. Density and interconnectedness of rapid transit systems, measured by the number of stations per line and the number of connections between lines, have little effect on either drunk driving outcome.

The remainder of the paper proceeds as follows. Section 2 provides background on the use of rapid transit systems in the U.S. as well as the problem of drunk driving. Section 3 provides an analytical framework for the decision process behind drunk driving and the potential effects of rapid transit. Section 4 describes the data sources I use. Section 5 presents the empirical methodology. Section 6 presents the results of my analysis. Section 7 tests the robustness of these estimates. Finally, Section 8 concludes.

2.2 Background

2.2.1 Rapid Transit in the United States

The development of rapid transit systems in the United States varied substantially from city to city. Early adopters such as New York City and Chicago developed their systems in the late 19th and early 20th centuries, with the systems remaining largely fixed since then.[2] Rapid transit systems consist of some combination of heavy rail (subways and elevated trains), light rail (smaller, slower trains which sometimes share roadways with cars), streetcars (similar to light rail but with more frequent shared roadways) and rapid buses. Rapid buses are distinguished from standard buses by their limited stops and dedicated right-of-way for substantial portions of their routes. This allows them to move passengers much more quickly than traditional buses and thus are included as part of the rapid transit system.

Cities across the U.S. have developed rapid transit systems of different types at various times over the last 150 years. Cities like Boston, New York, Chicago, Philadelphia, and Cleveland all built rail transit systems in the late 19th and early 20th centuries and continue to operate them today. Other cities have built their systems much more recently, with places like Washington DC, Los Angeles, Baltimore, Dallas, and Denver developing systems between the 1970s and the present. The scale of each of these systems varies substantially. New York City's rapid transit system contains 27 lines and over 450 individual stations while Atlanta has only four lines with 38 stations. The growth of these systems beginning in the 1970's was also substantial. Figure 1 shows the change in the number of rapid transit lines in each city from 1975 through 2014.[3]

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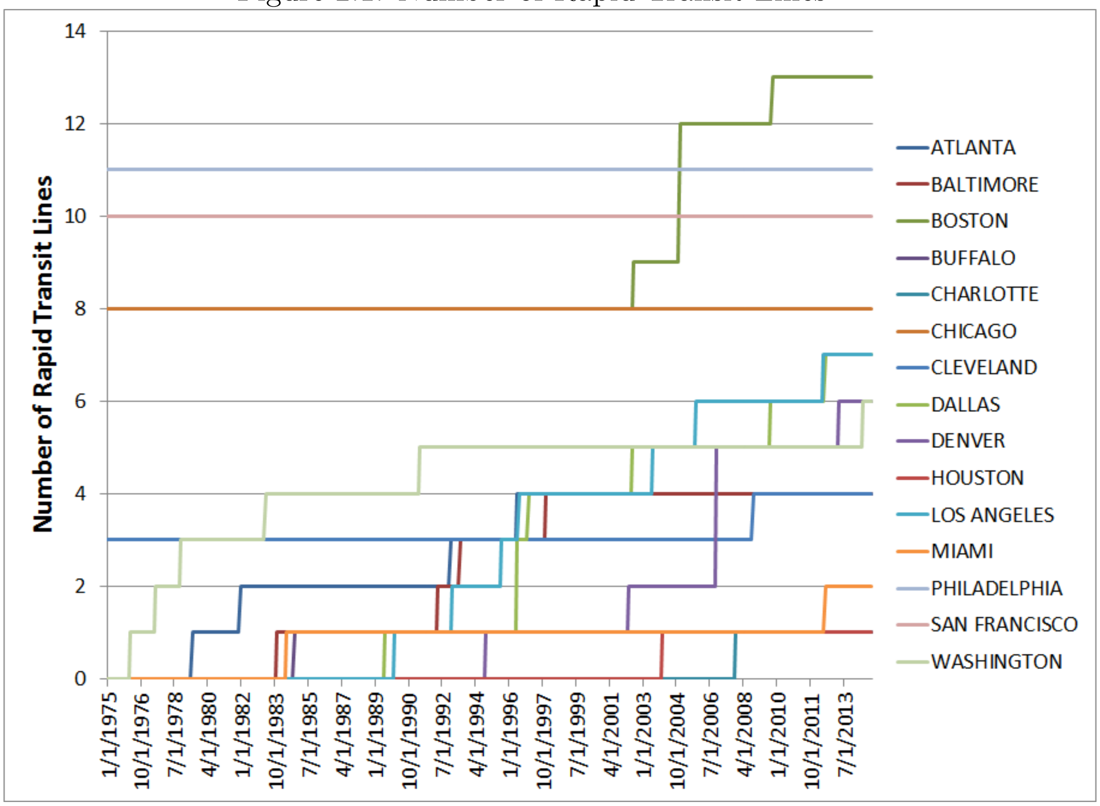
[2]Some early adoption cities such as Boston have since added rapid bus and/or light rail services.

[3]New York City is omitted since it is far higher than any other city. It had a constant 27 rapid transit lines over this period.

Figure 2.1: Number of Rapid Transit Lines



2.2.2 Drunk Driving in the U.S.

Drunk driving is a serious and persistent public safety issue in the U.S.. While there have been significant reductions in the number of drunk driving deaths over the past few decades drunk driving still kills around 10,000 people each year and injures hundreds of thousands. The National Highway Traffic Safety Administration (NHTSA) puts the annual direct economic cost of drunk driving at $49 billion and the total societal cost at almost $200 billion.[4] In 2012 there were approximately 121 million self-reported instances of drunk driving.[5] That same year over 1.2 million drivers were arrested for driving under the influence.[6]

Almost all research into drunk driving has focused on factors affecting the expected cost of driving drunk or policies that affect the amount of alcohol consumption. Deterrence through increasing the probability of detection or increasing the severity of the punishment has been a primary focus. Eisenberg (2003) examines the impact of laws regulating legal blood alcohol content (BAC) when driving as well as other non-enforcement policies such as graduated licensing[7]. Schultz et al (2001) look into more direct enforcement

[4]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

[5]Jewett A, Shults RA, Banerjee T, Bergen G. (August 7, 2015) Alcohol-impaired driving among adults— United States, 2012. Morbidity and Mortality Weekly Report. 64(30):814-17.

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

[7]Graduated licensing is the practice of providing new drivers with restricted licenses which become progressively less restrictive as the driver ages.

mechanisms, finding significant reductions in fatal alcohol-related accidents due to the use of sobriety checkpoints.

Recently, some studies have begun looking at the impact of alternative transportation availability on drunk driving outcomes. Jackson and Owens (2011) find that extending the operating hours of the Washington DC subway resulted in localized reductions in DUI arrests, though little city-wide effect. Greenwood and Wattal (2015) and Dills and Mulholland (2016) find reductions in drunk driving measures after introduction of ridesharing services such as Uber and Lyft. My paper expands on this growing literature by exploring the impacts of not just the operating hours of public transit systems, but their initial introduction. Public transit has many purposes beyond drunk driving prevention, such as relieving traffic congestion, reducing pollution, and easing parking constraints. When deciding to build or expand transit systems policymakers need to have a solid understanding of both the costs and potential benefits, along all dimensions, of these services. This paper helps to quantify the benefit of rapid transit in terms of drunk driving prevention.

2.3 Analytic Framework

In this section I present a simple model of the decision process faced by an individual who plans to consume alcohol. In the model, an individual faces three decisions: whether to consume alcohol inside their home ($H = 1$) or outside their home ($H = 0$), if they go out whether to self-drive ($S = 1$) or take alternative transportation ($S = 0$), and finally how much alcohol to consume (D). The individual will select the values of these variables which solve the utility maximization problem in Equation 1.

$$
\begin{aligned}
\underset{H,S,D}{\text{maximize}} \quad & H \cdot U_H(D, \beta_{i,H}) + (1-H) \cdot S \cdot U_{NH,S}(D, P_S, \delta_{i,S}, \beta_{i,NH}) \\
& +(1-H) \cdot (1-S) \cdot U_{NH,NS}(D, P_{NS}, \delta_{i,NS}, \beta_{i,NH})
\end{aligned}
\tag{2.1}
$$

In this equation, P_S represents the price of self-driving and P_{NS} is the price of taking alternative transportation. These prices take into account the convenience of each mode of transportation as well. β represents idiosyncratic factors affecting the utility of consuming alcohol outside versus inside the home and δ are idiosyncratic factors affecting the utility of self-driving versus taking alternative transportation. $U_{NH,NS}(.)$ represents the utility received from drinking outside the home and taking the individual's most-preferred form of alternative transportation.

Prior to rapid transit introduction, drunk drivers are those individuals who optimally select to drink outside the home ($H = 0$), self-drive ($S = 1$), and consume more alcohol than legally allowed when driving ($D^* > \bar{D}$). Within the utility maximization framework I present here, drunk drivers are those for whom:

$$
\begin{aligned}
& U_{NH,S}(D_i^*, P_S, \delta_{i,S}, \beta_{i,NH}) > U_H(D_i^*, \beta_{i,H}) \\
& U_{NH,S}(D_i^*, P_S, \delta_{i,S}, \beta_{i,NH}) > U_{NH,NS}(D_i^*, P_{NS}, \delta_{i,NS}, \beta_{i,NH}) \\
& D_i^* > \bar{D}
\end{aligned}
\tag{2.2}
$$

The introduction or expansion of rapid transit services affects this decision problem through the price of alternative transportation (P_{NS}). For some individuals, the convenience and cost of rapid transit will now make it their most preferred form of alternative transportation. For a subset of these people, going out using rapid transit will now give them the highest utility. Not all of those who choose to use rapid transit would have driven drunk in its absence, some would have stayed home, others would have used some other form of alternative transportation, and still others would have self-driven but restricted their alcohol consumption to a level at which they could drive legally. The substitution that this study will measure does not include any of these groups. The substitution I measure is for people who would have driven drunk prior to rapid transit introduction (those who satisfy conditions in (2) above) but who now optimally choose to take rapid transit instead. If there is significant substitution of this type it should have an estimable impact on drunk driving outcomes.

2.4 Data

Estimating the effect of rapid transit on drunk driving requires data on both the availability of rapid transit and data on drunk driving outcomes for cities around the U.S.. I have gathered the following data for 16 U.S. cities with rapid transit systems covering 1975 through 2014.

2.4.1 Alcohol-Related Traffic Fatalities

Since 1975 the National Highway Traffic Safety Administration (NHTSA) collects data on every fatal auto accident in the U.S. through their Fatality Analysis Reporting System (FARS). These data include information about the vehicles, their occupants, the time and location of the accident, and importantly whether alcohol was a factor in the accident. I collected these data from the earliest available year, 1975, through 2014 which is the latest year for which data have been published. For the early years of data, before GPS systems became widely available, locations were recorded as state, county, and city identifiers. In the latest 15-20 years of data, geographic locations are recorded via precise coordinates. Whenever available, I use the coordinate data to determine which observations are in each sample city.[8] When these data are unavailable I use the city codes to match accidents to the sample cities. All of the data are incident-level, so each observation represents a separate fatal accident. After matching each observation to the sample cities I aggregate them into total monthly fatal alcohol-related auto accidents for each city.

2.4.2 DUI/DWI Arrests

The Federal Bureau of Investigation (FBI) through their Uniform Crime Reporting (UCR) system collects data on monthly arrests for various crimes from police departments across the U.S.. I collected monthly UCR reports for DUI/DWI arrests for each municipal police agency in my city sample. I

[8]Any accident within 10 miles of the city centroid is considered within that city.

gathered these data for 1980 through 2014. Reporting of DUI/DWI arrests in the UCR program is voluntary, accordingly not every police agency reports these arrest statistics every month. For the cities in my sample, half of them report data for over 93% of the months between 1980 and 2014. All but four report data for at least 78% of months, and all but two report data for over 39% of months. Two cities, Chicago and Washington DC, report for only a small proportion of months. Excluding them from the following analyses does not affect the results.

2.4.3 Rapid Transit System Data

Determining whether rapid transit had any effect on fatal accidents requires data on how the availability of rapid transit has changed over the sample period. For each of the cities in my sample I gathered data for each line and station in their rapid transit system.[9] For each transit line I have the date when it first began operation. For each station I have the opening date of the station, information on which lines stop at the station, the date each line began operating at that station, and an indicator for whether the station is a connection between two or more lines. In many transit systems, two or more lines will run parallel for a section of their route, stopping at all of the same stations over that section. In these cases, I count the stations at which the lines converge or diverge as connection stations while the intermediate

[9] All rapid transit system data were gathered from the corresponding municipal transit authority.

shared stations are not counted as such. The purpose of including data on line connections is to test whether more inter-connected systems are more appealing under the theory that they provide more comprehensive coverage of the city. These data provide several measures for the quality of a city's rapid transit system. For this analysis I calculate the total number of stations, the total number of transit lines, and the total number of line connections in each sample city's rapid transit system in each month between 1975 and 2014.

2.5 Methodology

2.5.1 Econometric Model

To quantify any potential effects rapid transit services had on fatal alcohol-related auto accidents in the sample cities I use a fixed effects difference-in-differences methodology. Between 1975 and 2014, 10 of the sample cities developed their rapid transit systems while the systems in the remaining six cities remained largely static, having been built out during the early 20th century. The cities whose systems were already established by 1975 effectively act as a control group for the treatment cities which built their systems over the late 20th and early 21st centuries. I estimate Equation 3 using Negative Binomial estimation techniques because the outcomes of interest (fatal accidents and DUI arrests) are discrete count variables.

$$y_{i,t} = \alpha_0 + \beta Z_{i,t} + \gamma X_{i,t} + \delta_i + \phi_t + \epsilon_{i,t} \tag{2.3}$$

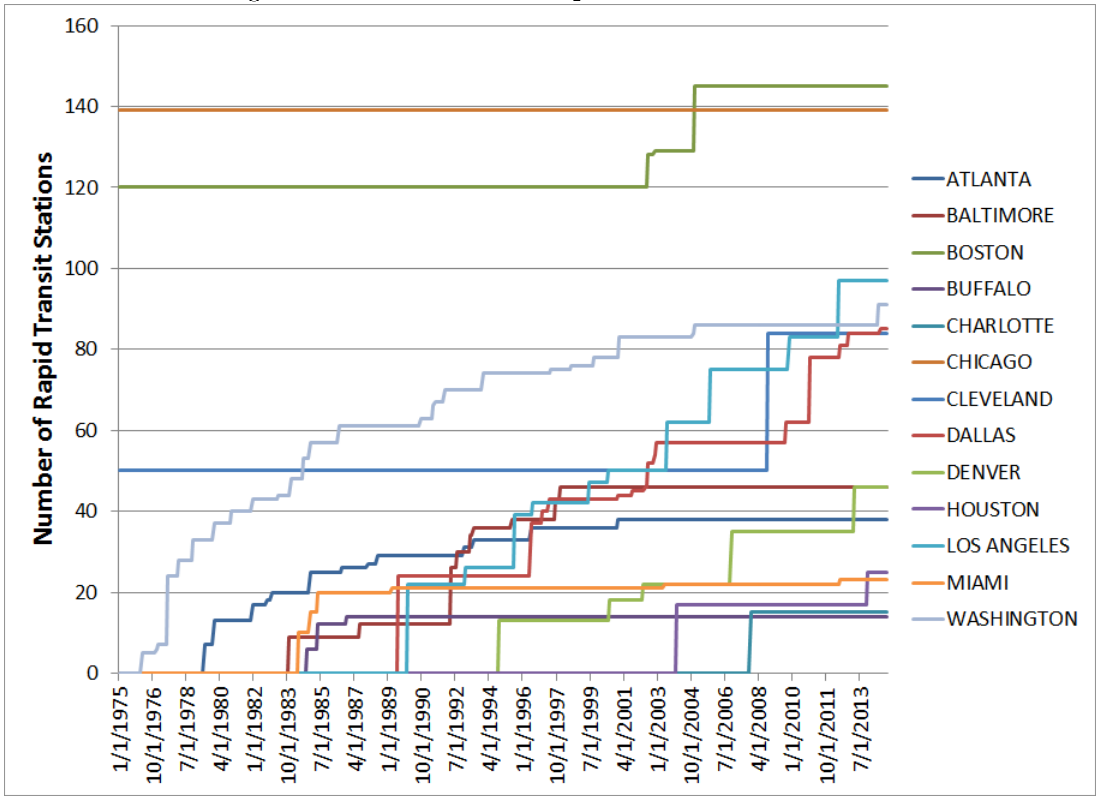
In this equation, $y_{i,t}$ represents the measure of drunk driving prevalence which for this analysis are the numbers of fatal alcohol-related auto accidents and DUI/DWI arrests each month. $Z_{i,t}$ is a vector of potential measures of rapid transit system quality such as number of stations, number of lines, and/or number of line connections for city i at time t. β is the coefficient of interest for this analysis, it captures the effect of these rapid transit measures on drunk driving outcomes. $X_{i,t}$ is a vector of other potentially important covariates such as city population and state unemployment rates. δ_i and ϕ_t are a full set of city and time fixed effects.

2.5.2 Identification

Identification of the causal effect of rapid transit systems comes from their gradual and staggered development. Rapid transit, particularly rail systems are large, expensive projects and typically take years or decades to complete. Systems for the cities in my sample began operation at quite different points in time. Washington DC opened its first heavy rail line in 1976 while Miami's didn't open until 1984 and Los Angeles' until 1990. This gradual roll out of systems across cities allows me to estimate the causal effect of rapid transit service independent of other time-specific factors. Figures 1 and 2 graphically presents the number of lines and stations, respectively, in operation in each sample city from 1975 through 2014.[10] This in conjunction with

[10]New York City is excluded due to its unusually large number of lines and stations. Over the entire period it had a constant 27 lines and 494 stations. For stations, Philadelphia and San Francisco are excluded as well since their street-car lines have a very large number of stations. Each has a constant number of stations over the full sample period.

Figure 2.2: Number of Rapid Transit Stations



the difference-in-differences approach which uses the control cities to remove any trends in drunk driving outcomes as well as any seasonal variation that is common across cities allows me to make causal inference about the effects of rapid transit.

2.6 Results

Table 1 presents the results of estimating Equation 3 using the number of rapid transit lines as the measure of system size. Specifications (1) through

(3) presents Negative Binomial estimates of the effect of adding additional rapid transit lines on monthly fatal alcohol-related auto accidents. Each model progressively adds more control variables. Specification (1) controls linearly for city population. Specification (2) adds quadratic population controls. Finally, specification (3) adds controls for city economic and demographic factors.[11] The results indicate that adding an additional transit line reduces fatal alcohol-related accidents by 11.5% to 13.8%.[12] Specifications (4) through (6) in Table 1 present the same estimations using DUI/DWI arrests as the dependent variable rather than fatal alcohol-related accidents. The effects of adding additional rapid transit lines on DUI/DWI arrests are very similar to the effects on fatal accidents, with reductions of 12.7% to 14.4% from adding additional lines.

The number of lines is not the only potential measure of a rapid transit system's size. A transit line is potentially less useful if it only serves a limited number of stations. Accordingly, I also estimate the effect of adding additional rapid transit stations on drunk driving outcomes. Table 2 presents the results of this estimation. As in Table 1, specifications (1)-(3) present the effects on fatal alcohol-related auto accidents and specifications (4)-(6) give the effects on DUI/DWI arrests. The results indicate that each additional rapid transit station reduces fatal alcohol-related accidents by 1% to 1.2%. The estimated

[11] The additional controls include state-level unemployment rates, poverty rates, the proportions of the population which are children, elderly, male, and males aged 21-44 (each included separately). Unemployment rates and population are annual, other covariates are from decennial census data.

[12] The coefficients reported in the table represent the change in the mean of the natural log of the number of fatal accidents. To convert these into percentage changes I calculate $exp(coefficient) - 1$.

Table 2.1: Effects of Number of Rapid Transit Lines

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Lines	-0.122*	-0.149**	-0.133**	-0.136**	-0.144**	-0.155**
	(0.060)	(0.058)	(0.051)	(0.053)	(0.050)	(0.055)
Population	-0.074*	-0.006	-0.077	0.082	0.149	0.134
	(0.031)	(0.060)	(0.059)	(0.051)	(0.103)	(0.124)
Population^2		-0.001^{+}	-0.0000		-0.001	-0.001
		(0.0004)	(0.0004)		(0.001)	(0.001)
Unemp. Rate			0.018			-0.063^{+}
			(0.035)			(0.036)
Poverty Rate			0.015			-0.057
			(0.040)			(0.039)
Male %			0.372*			0.005
			(0.152)			(0.211)
Male (21-44) %			-0.164			0.144
			(0.133)			(0.147)
Children (0-17) %			-0.107			0.085
			(0.068)			(0.062)
Elderly (65+) %			0.106			0.101
			(0.136)			(0.103)
N	7680	7680	7488	5139	5139	5139

Standard errors in parentheses, clustered at the city level.

Fixed effects for city and month by year are included in the regressions but are not reported.

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

All models are estimated using Negative Binomial specification.

Population measures are in units of 100,000.

Rates and percentages are in percentage points.

impact on DUI/DWI arrests is comparable, with each station reducing arrests by 1.4% to 1.8%.

These results, both for additional transit lines and additional stations indicate that increasing the size of a city's rapid transit system has a significant impact on drunk driving outcomes.[13] Next I try to delve into the effect of the comprehensiveness of a city's rapid transit system. Here I jointly estimate the effects of the number of rapid transit lines, the number of stations per line, and the number of connections between lines. Table 3 presents the results of this estimation.

As in the previous two tables, the first three specifications examine the impact on fatal alcohol-related accidents and the last three look at the effect on DUI/DWI arrests. Estimating each of these three measures jointly shows that the impact of rapid transit systems on drunk driving outcomes seems to be driven by the number of lines each system operates. Of the three measures of system quality only the number of lines has a significant effect. The effects of additional rapid transit lines on drunk driving outcomes are similar to those presented in Table 1. I estimate in Table 3 that each additional line reduces fatal alcohol-related accidents by 13.1% to 18.6%. For DUI/DWI arrests each line corresponds to a 10.5% to 12.0% reduction. All of these results indicate that increasing the size of a city's rapid transit system significantly reduces

[13]The coefficients on city population indicate higher population corresponds with fewer fatal alcohol-related accidents and more DUI/DWI arrests. The negative coefficient for fatal accidents may be due to increased density and traffic congestion in larger cities, leading to slower traffic speeds and fewer fatal crashes.

Table 2.2: Effects of Number of Rapid Transit Stations

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Stations	-0.010^{+}	-0.012*	-0.011**	-0.014***	-0.015***	-0.018***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Population	-0.073*	-0.007	-0.080	0.100*	0.173^{+}	0.132
	(0.030)	(0.060)	(0.055)	(0.051)	(0.101)	(0.116)
Population^2		-0.001	0.0000		-0.001	-0.005
		(0.0004)	(0.0004)		(0.001)	(0.009)
Unemp. Rate			0.017			-0.046
			(0.035)			(0.038)
Poverty Rate			0.019			0.005
			(0.038)			(0.029)
Male %			0.378*			0.044
			(0.158)			(0.191)
Male (21-44) %			-0.171			0.156
			(0.137)			(0.147)
Children (0-17) %			-0.112^{+}			0.121*
			(0.068)			(0.059)
Elderly (65+) %			0.124			0.169
			(0.137)			(0.108)
N	7680	7680	7488	5139	5139	5139

Standard errors in parentheses, clustered at the city level.
Fixed effects for city and month by year are included in the regressions but are not reported.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
All models are estimated using Negative Binomial specification.
Population measures are in units of 100,000.
Rates and percentages are in percentage points.

Table 2.3: Joint Impact of Rapid Transit Measures

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Lines	-0.177**	-0.206***	-0.140**	-0.111*	-0.119*	-0.128+
	(0.056)	(0.053)	(0.041)	(0.050)	(0.048)	(0.067)
Stations per Line	0.009	0.003	0.009	0.009	0.005	0.001
	(0.007)	(0.010)	(0.009)	(0.008)	(0.010)	(0.008)
Line Connections	0.034	0.042	0.004	-0.025	-0.023	-0.023
	(0.028)	(0.029)	(0.031)	(0.028)	(0.029)	(0.034)
Population	-0.074**	-0.007	-0.091	0.069	0.137	0.123
	(0.029)	(0.068)	(0.064)	(0.051)	(0.115)	(0.128)
Population^2		-0.001	0.0001		-0.001	-0.001
		(0.0005)	(0.0004)		(0.001)	(0.001)
Unemp. Rate			0.013			-0.062+
			(0.033)			(0.036)
Poverty Rate			0.012			-0.057
			(0.040)			(0.039)
Demographics			√			√
N	7680	7680	7488	5139	5139	5139

Standard errors in parentheses, clustered at the city level.
Fixed effects for city and month by year are included in the regressions but are not reported.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
All models are estimated using Negative Binomial specification.
Population measures are in units of 100,000.
Rates and percentages are in percentage points.

drunk driving outcomes.

2.6.1 Robustness

One potential concern about the interpretation of these estimates is that the identifying variation might be driven by expansion of rapid bus services rather than more traditional heavy and light rail transit services. While rapid bus systems are much less common than rail services for the cities in my sample all of them were developed during the sample period, potentially increasing their influence on the estimates of rapid transit's effects on drunk driving. The concern is that this might limit the applicability of my estimated effects to rail transit systems. To test this I repeat the estimation in Table 1 excluding rapid buses from the number of transit lines.[14] Table 4 presents these estimates. As each shows, removing rapid buses has no effect on the estimated reduction in fatal alcohol-related accidents or DUI/DWI arrests. This demonstrates that the estimated effects are not driven primarily by rapid bus services but rather apply to rail transit services as well.

Another concern regards statistical inference using clustered standard errors. In all of my analyses I cluster standard errors at the city level. Cameron and Miller (2015) show that clustering can lead to overly small standard errors when the number of clusters is relatively small. Since the number of clusters in my primary analyses is only 16 I re-estimate each specification clustering

[14] I also repeated the analysis in Tables 2 and 3 excluding rapid bus services and found no change in those results.

Table 2.4: Effects of Number of Rapid Transit Lines - Excluding Buses

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Lines	-0.118^{+}	-0.144*	-0.133*	-0.132**	-0.144**	-0.178**
	(0.063)	(0.063)	(0.057)	(0.057)	(0.053)	(0.057)
Population	-0.075*	-0.011	-0.085	0.083	0.156	0.119
	(0.032)	(0.065)	(0.060)	(0.054)	(0.106)	(0.122)
Population^2		-0.001	0.0001		-0.001	-0.001
		(0.0004)	(0.0004)		(0.001)	(0.001)
Unemp. Rate			0.012			-0.068^{+}
			(0.033)			(0.038)
Poverty Rate			0.013			-0.061
			(0.041)			(0.039)
Male %			0.394*			0.109
			(0.157)			(0.200)
Male (21-44) %			-0.154			0.145
			(0.132)			(0.147)
Children (0-17) %			-0.097			0.121*
			(0.073)			(0.061)
Elderly (65+) %			0.132			0.158
			(0.135)			(0.108)
N	7680	7680	7488	5139	5139	5139

Standard errors in parentheses, clustered at the city level.
Fixed effects for city and month by year are included in the regressions but are not reported.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
All models are estimated using Negative Binomial specification.
Population measures are in units of 100,000.
Rates and percentages are in percentage points.

instead at the month level. Doing so increases the number of clusters from 16 to 480, which is sufficiently large for reliable statistical inference. Table 5 presents the results of estimating the effect of the number of rapid transit lines on each of the drunk driving outcomes using date clustering. Contrary to the concern drawn from Cameron and Miller (2015), the standard errors using this larger number of clusters are substantially smaller than when I cluster at the city level.[15] This result provides some support that the standard errors I estimate in my primary results are not significantly underestimated.

2.7 Conclusion

Drunk driving exacts a large cost, both in terms of lives lost and in economic damage. Providing convenient, affordable transportation alternatives represents a potential avenue to reducing this cost. Rapid transit systems have the potential to move people around cities more quickly and efficiently than traditional bus systems. Furthermore, they are typically far cheaper than private transportation options such as taxis. Despite this potential, there is very little research on the effects rapid transit have on drunk driving rates. In this study I use the build out of rapid transit systems across the U.S. over the past 40 years to estimate the causal effect of these systems on fatal alcohol-related auto accidents. Using a fixed effects difference-in-differences methodology I estimate that expanding rapid transit systems significantly reduces both fatal

[15] I likewise find significantly smaller standard errors when re-estimating the specifications in Tables 2 and 3 clustering at the date level.

Table 2.5: Effects of Number of Rapid Transit Lines - Date Clustering

	(1) Fatal Crashes	(2) Fatal Crashes	(3) Fatal Crashes	(4) DUI Arrests	(5) DUI Arrests	(6) DUI Arrests
Lines	-0.122***	-0.149***	-0.133***	-0.136***	-0.144***	-0.155***
	(0.008)	(0.009)	(0.009)	(0.005)	(0.005)	(0.006)
Population	-0.074***	-0.006	-0.077***	0.082***	0.149***	0.134***
	(0.006)	(0.010)	(0.015)	(0.006)	(0.013)	(0.018)
Population^2		-0.001***	-0.0000		-0.001***	-0.001***
		(0.0001)	(0.0001)		(0.0001)	(0.0001)
Unemp. Rate			0.014			-0.057***
			(0.009)			(0.006)
Poverty Rate			0.015$^+$			0.002
			(0.008)			(0.007)
Male %			0.372***			0.005
			(0.039)			(0.031)
Male (21-44) %			-0.164***			0.144***
			(0.029)			(0.023)
Children (0-17) %			-0.107***			0.085***
			(0.017)			(0.016)
Elderly (65+) %			0.106***			0.101***
			(0.025)			(0.018)
N	7680	7680	7488	5139	5139	5139

Standard errors in parentheses, clustered at the date level.

Fixed effects for city and month by year are included in the regressions but are not reported.

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

All models are estimated using Negative Binomial specification.

Population measures are in units of 100,000.

Rates and percentages are in percentage points.

alcohol-related auto accidents and DUI/DWI arrests. I find that adding an additional rapid transit line reduces fatal accidents by 11.5% to 13.8% and DUI/DWI arrests by 12.7% to 14.4%. Alternatively, using number of stations as the measure of system size I find a 1% to 1.2% reduction in fatal accidents and a 1.4% to 1.8% reduction in drunk driving arrests per additional station.

These results provide essential evidence for policymakers when weighing the costs and benefits of building or expanding rapid transit systems in their cities. The estimates above imply that each additional rapid transit line a city builds results in on average four fewer fatal drunk driving accidents each year. At a minimum this corresponds to a reduction of almost $40 million in damage per line each year.[16] These systems operate for many decades after they are developed, meaning these benefits from drunk driving reduction will continue to accumulate year after year.

[16]This assumes only a single death per fatal accident and uses the Department of Transportation's $9.1 million value for a statistical life. It does not include any other costs such as property damage, non-fatal injuries, emergency response costs, medical expenses, or any other likely costs.

Chapter 3

Residential Proximity to Late-Night Bus Routes in Austin: Impact on DWI Arrests

3.1 Introduction

Despite improvements over the past few decades, drunk driving remains a significant problem in the United States. Alcohol-related auto accidents claim over 11,000 lives and result in over 326,000 injuries annually. These accidents impose a substantial cost on society, with estimates of the harm caused ranging from \$44 billion to over \$200 billion each year.[1] In addition to the harm caused by accidents, drunk driving poses a significant criminal justice problem. In 2012, over 1.2 million US drivers were arrested for drunk driving[2], imposing significant cost in terms of enforcement, prosecution, and penalties faced by those convicted. Accordingly, substantial time and resources have been devoted to trying to reduce the incidence of drunk driving around the country.

[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. Web. 6 March 2017.

[2]Federal Bureau of Investigation, "Crime in the United States: 2013." Web. 6 March 2017.

Most drunk driving prevention policies focus on deterrence through increased police enforcement or stricter penalties. Such policies aim to make drunk driving less appealing by increasing the expected cost of doing so, either by increasing the likelihood of detection or increasing the punishments if caught. For this reason, most of the academic research on drunk driving prevention has focused on these deterrence methods as well. However, threat of punishment is not the only potential way to deter drunk driving. Increasing the availability and attractiveness of alternative forms of transportation can potentially induce some people who would otherwise have driven drunk to instead take other transportation.

Late-night public transit is often promoted as a potential tool for reducing drunk driving. To date, limited research exists examining whether such transit services do reduce drunk driving rates. One issue with measuring the effectiveness of late-night transit is that not every person in a city has access to these services. Late-night routes often cover only a subset of city transit routes. Anyone who doesn't live in close proximity to one of them will not be able to use these services as an alternative to driving drunk.

In this paper, I take advantage of a unique dataset containing the home addresses for every person arrested for driving while intoxicated (DWI) by the Austin Police Department to asses how the availability of late-night bus services affects drunk driving outcomes. Using these home addresses along with the fact that Austin's late-night buses do not operate every day of the week I estimate the causal impact of Austin's late-night buses on DWI arrests for people who live within walking distance of one of these routes. This approach uses a difference-in-differences methodology which compares the change in number of DWI arrests for people living close to these routes from days of the week without late-night bus service to days with such services to the corresponding change for people who live farther away from these routes I estimate that Austin's late-night buses reduce DWI arrests by as much as 16.8% for those living within a short walk of a late-night bus stop.

The remainder of this paper proceeds as follows. Section 2 provides background on the problem of drunk driving in the U.S. as well as on Austin, Texas's late-night bus system. Section 3 describes the data sources used in the analyses. Section 4 details the empirical methodology for identifying the causal impact of Austin's late-night buses on DWI arrests. Section 5 presents the results of these estimates. Section 6 examines the robustness of these results. Finally, Section 7 concludes.

3.2 Background

3.2.1 Drunk Driving Prevalence and Prevention

Drunk driving remains a persistent problem across the U.S. Nationally, over 11,000 people are killed each year in alcohol-related accidents. Over 1.2 million are arrested for driving under the influence annually. In Austin, Texas, which is the focus of this study, DWI arrests average around 6,000 each year. The costs imposed by both drunk driving and efforts to discourage it can be substantial. Nationally, the costs imposed by fatal alcohol-related accidents range from \$44 billion to as much as \$200 billion each year. Arrests for DWIs carry costs as well in terms of enforcement, prosecution, and lost productivity of convicted offenders. Bouchery et al (2011) estimate the cost of this lost productivity at \$7.5 billion annually.

Common drunk driving prevention strategies can carry substantial costs as well. Sobriety checkpoints are a common enforcement-based drunk driving prevention method. Miller et al (1998) estimate that operating a single checkpoint costs as much as \$4,000 per hour. Enhanced police patrols will likewise incur additional public costs in terms of personnel and equipment. These strategies can be effective at reducing drunk driving, with Shults et al (2001) showing that sobriety checkpoints can reduce fatal alcohol-related accidents by 18-20%. One question for policymakers is whether these methods are a cost-effective way to achieve reductions in drunk driving.

Enforcement strategies aim to deter drunk driving by increasing the expected cost of doing so, either through increasing the likelihood of detection or increasing the severity of punishment. This is only one possible method for deterring drunk driving, however. When deciding to consume alcohol outside the home individuals face the choice between driving themselves (and potentially driving under the influence on their return trip) or taking some alternative form of transportation. Holding the available transportation options fixed, increasing the expected cost of drunk driving makes the former less appealing relative to the latter. This decision can also be influenced by making alternative transportation more appealing relative to self-driving. This can be accomplished by increasing the availability of alternative transportation methods, increasing their convenience, or decreasing their cost. Greenwood and Wattal (2015) and Dills and Mulholland (2016) have shown that increasing the availability of alternative transportation can reduce rates of drunk driving. Jackson and Owens (2011) look specifically at the effect of late-night public transit in Washington D.C. and find significant, but highly localized effects on drunk driving. They show that extended subway operating hours reduce drunk driving arrests but only in neighborhoods with both subway stations and large numbers of bars.

3.2.2 Late-Night Buses in Austin

There are previous studies which provide evidence that improving the attractiveness of alternative transport relative to self-driving can reduce drunk driving. It is possible that the availability of late-night public transit services could reduce drunk driving as well. Austin, Texas operates two types of late-night bus services. Their "Night Owl" service consists of five routes which cover a wide range of neighborhoods. The other late-night service is the "Entertainment Bus" (E-Bus). This service consists of two routes and primarily connects areas with large amounts of student housing to the downtown entertainment districts. Both operate far later than the typical Austin transit operating hours, running until 3:30 am. Standard buses in Austin typically have final departures between 10:30 pm and 11:30 pm, depending on the route. The Night Owl service operates Mondays through Saturdays while the E-Bus only operates Thursday through Saturday.

3.3 Data

The analyses in this study take advantage of a unique dataset provided by the Austin Police Department. Most drunk driving studies rely on data which at their most detailed only provide location information for the locations where drunk driving accidents or arrests occur. These data are useful but not ideal for measuring the impact of expanding public transit systems, as the key to these systems' usefulness in terms of drunk driving prevention stems from their accessibility to potential drunk drivers' homes. The data I use in this study contain the home addresses for every person arrested for DWI by the Austin police department from January 1, 2014 through November 30, 2015. These data allow me to calculate the number of DWI arrests for people who live near a late-night bus route separately from the number of arrests for people who do not. To determine which addresses are close to late-night bus routes, I utilize the Google Maps Directions API to calculate the walking time from each address to the closest late-night bus stop.[3] This method finds the closest late-night bus route (if applicable) to each address, so if an address is close to both a Night Owl route and an E-Bus route I only count the closer of the two bus services.

To supplement this direct treatment and outcome data I gathered

[3]Many addresses are too far from a late-night route for Google Maps to calculate a walking time, I classify these addresses as not close to a late-night bus route.

Zipcode-level data from the U.S. Census Bureau's American Community Survey (ACS).[4] I gathered data on Zipcode demographics for each of the 52 Zipcodes in Austin for 2015. Table 1 presents summary statistics for the Zipcode-level demographic data.

Table 3.1: Zipcode-Level Demographic Summary Statistics

	Mean	Min	Max
Population	35,234	748	79,067
Age 21-44 Male %	23.1%	10.6%	37.3%
Rental HH %	54.6%	4.7%	88.8%
Unemployment Rate	6.2%	2.4%	15.6%
Median HH Income	$58,606	$12,385	$132,980
Poverty %	19.6%	1.5%	66.4%
Less than HS %	15.4%	0.4%	40.0%
HS Grad %	42.3%	14.2%	58.9%
Post HS Educ. %	42.3%	9.8%	84.2%

These data allow me to explore demographic differences in numbers of DWI arrests in Austin. While these are not causal estimates, correlations between neighborhood demographics and DWI arrest rates can be informative and the data I use in this study is uniquely suited to explore this. For this

[4] ACS data gathered using the U.S. Census Bureau's American FactFinder at https://factfinder.census.gov/faces/nav/jsf/pages/index.xhtml.

estimation I use the same group definitions described above, but separate each group by Zipcode. This means that for each Zipcode in each day there is an observation containing the total number of arrests for people in that Zipcode who fall into each of the three groups determined by their home's proximity to a late-night bus route.[5] Table 2 presents the results of regression analysis on the influence of Zipcode demographics on the number of DWI arrests. All specifications include time fixed effects and cluster standard errors at the Zipcode level.

Areas with higher populations tend to have higher numbers of DWI arrests. Further, I find that the larger the proportion of the population that is made up of young males (a particularly high-risk group for drunk driving), the more observed DWI arrests. Economic factors do not appear to play a large role in DWI arrests in the data. Unemployment rate, median income, and the poverty rate having small and mostly insignificant effects. The only other factors that appear to be associated with different levels of DWI arrests are education and the proportion of households who rent rather than own their home. The proportion of rental households is associated with a significantly higher number of DWI arrests. The proportion of residents who did not finish high school is associated with a small but highly significant increase in arrests. While none of these results imply any causal impact of these factors on DWI arrests, it may be useful to policymakers to understand the neighborhood characteristics that tend to be associated with higher rates of drunk driving.

[5]Some Zipcodes are not served by one or both of the types of late-night bus services.

Table 3.2: Zipcode-Level Demographic Regressions

	(1) DWI Arrests	(2) DWI Arrests
Population	0.801***	0.793***
	(0.107)	(0.098)
Age 21-44 Male %	0.672*	0.620*
	(0.285)	(0.267)
Rental HH %	0.403*	0.451**
	(0.160)	(0.162)
Unemployment Rate	0.001	-0.001
	(0.005)	(0.005)
Median Income	0.002+	0.002+
	(0.001)	(0.001)
Poverty %	-0.001	-0.001
	(0.002)	(0.002)
Less than HS %	0.006***	0.006***
	(0.002)	(0.002)
HS Grad %	-0.001	0.0003
	(0.002)	(0.002)
Day of Week x Month FE	√	
Full Date FE		√
N	3043	3043

Standard errors in parentheses, clustered at the Zipcode level.

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

All specifications use Poisson estimation.

Population is in units of 100,000 people.

Median income is per household in units of $1,000.

To estimate the causal effects of Austin's late-night buses on DWI arrests, comparing the number of DWI arrests for people who live close to late-night bus routes to those who don't is insufficient. Any differences between the two groups could be purely compositional, with different types of people living in areas close to such routes than those who do not. To properly estimate the effect of late-night buses I rely on the fact that these services do not operate every day of the week. Austin's "Night Owl" bus routes only operate Monday through Saturday. The "Entertainment Buses" (E-Bus) only operate Thursdays through Saturdays. This variation allows me to compare the change between days when these services do not operate to days when they do for each group of addresses. I separate the DWI arrests into three groups, those that live close to one of the "Night Owl" routes, those that live close to one of the "Entertainment Bus" (E-Bus) routes, and those that live close to neither. Table 3 presents summary statistics for each group and day of the week. Figure 1 shows graphically how the average number of arrests for each group vary by day of the week. All three groups follow a similar pattern, with DWI arrests increasing later in the week and on the weekend.

3.4 Methodology

3.4.1 Econometric Model

3.4.1.1 City-wide Effects

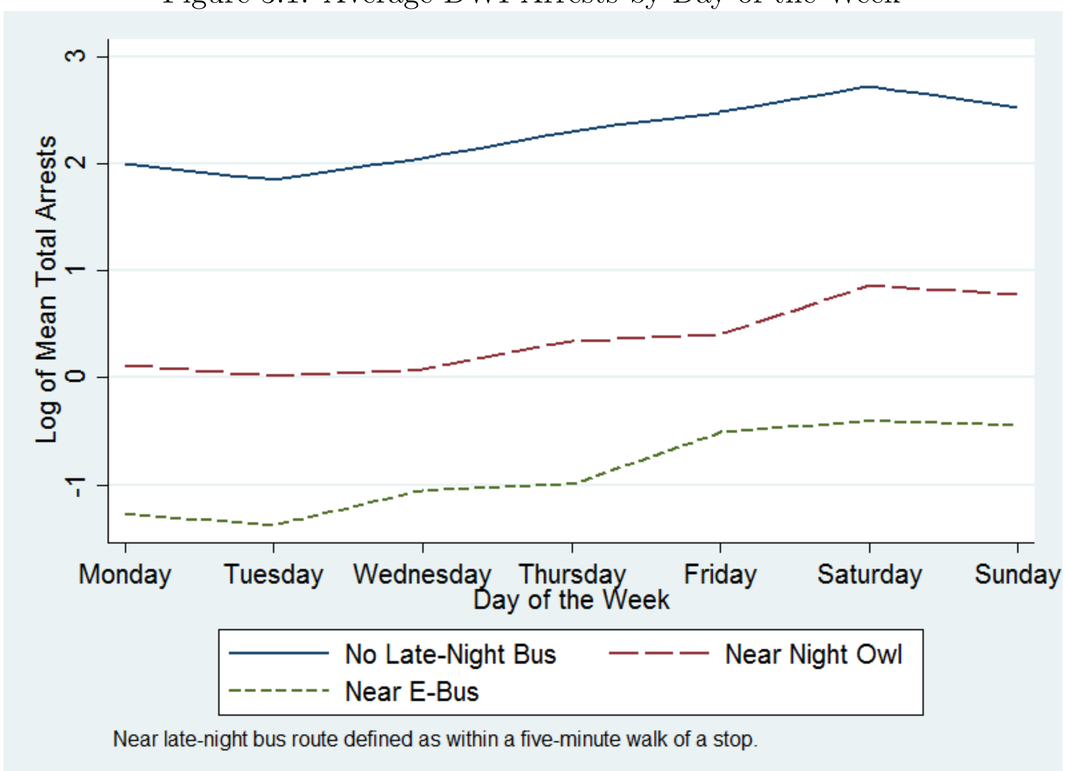
To estimate the potential causal impact of Austin's late-night bus services I use a fixed effects difference-in-differences approach. For each group I

Table 3.3: DWI Arrests Summary Statistics by Late-Night Bus Proximity and Day of the Week

	Control	Night Owl	E-Bus
Sunday			
Mean	12.44	2.18	0.64
Std. Dev.	(3.73)	(1.49)	(0.88)
Monday			
Mean	7.38	1.13	0.28
Std. Dev.	(2.83)	(1.13)	(0.53)
Tuesday			
Mean	6.36	1.02	0.25
Std. Dev.	(2.38)	(0.98)	(0.48)
Wednesday			
Mean	7.80	1.08	0.35
Std. Dev.	(3.56)	(1.04)	(0.58)
Thursday			
Mean	10.04	1.41	0.37
Std. Dev.	(3.36)	(1.11)	(0.61)
Friday			
Mean	11.95	1.50	0.60
Std. Dev.	(3.91)	(1.30)	(0.87)
Saturday			
Mean	15.31	2.37	0.67
Std. Dev.	(4.03)	(1.69)	(0.83)

*Night Owl and E-Bus groups are within a 5-minute walk.

Figure 3.1: Average DWI Arrests by Day of the Week



create an indicator for whether on that day they have access to each of the two types of late-night bus services. For addresses not near either of the late-night bus types these indicators are always equal to zero. For addresses located near one of the routes these indicators equal one on days the services are operating and zero otherwise.

$$y_{i,t} = \alpha_0 + \beta_1 NightOwl_{i,t} + \beta_2 EBus_{i,t} + \delta_i + \phi_t + \epsilon_{i,t} \tag{3.1}$$

In this estimation equation $NightOwl_{i,t}$ is an indicator for whether the Night Owl late-night bus service is available for group i on date t. As described previously, there are three "groups": one for people who live within walking distance of a "Night Owl" route; one for people who live within walking distance of an "E-Bus" route; and one for people who live near neither. $EBus_{i,t}$ is an indicator for whether the Entertainment Bus late-night service is available for group i on date t. For each of these, the indicator will always equal zero if group i does not live near the respective late-night bus route. Each acts as an interaction term between an indicator for whether the service is operating and an indicator for whether group i is the group within walking distance of that late-night service. β_1 and β_2 are the coefficients of interest, representing the effect of late-night bus services on DWI arrests. δ_i and ϕ_t represent group and date fixed effects, respectively. These fixed effects control for any time-specific factors that are common across all Austin residents as well as for any time-invariant differences in the average number of DWI arrests across the three groups.

3.4.1.2 Zipcode-level Effects

It is possible that there is significant variation in the average level of DWI arrests in different areas of the cities, even amongst those with access to the same types of late-night buses. To test for this I expand the group definition described in Section 3 to include Zipcode information. Now there will be a separate group for each of the three original groups (lives close to Night Owl bus, lives close to Entertainment Bus, and lives close to neither) for each Zipcode. Not every Zipcode will have each of the three groups, as some are not served by one (or by either) of the late-night bus routes.[6] The estimation equation for this version is as follows.

$$y_{i,z,t} = \alpha_0 + \beta_1 NightOwl_{i,z,t} + \beta_2 EBus_{i,z,t} + \delta_{i,z} + \phi_t + \epsilon_{i,z,t} \tag{3.2}$$

The only difference between this and the previous estimation equation is that the level of observation is now at the Zipcode-group-date level. This permits a larger set of group fixed effects by Zipcode rather than simply group fixed effects. This additional granularity controls for any differences in the average level of DWI arrests for people across all possible Zipcode and group combinations.

[6]The 52 Zipcodes in Austin along with the 1-3 "groups" per Zipcode results in 76 Zipcode-specific groups.

3.4.2 Identification

Identification in these empirical models comes from the difference-in-differences approach I use. It is possible to compare the change from days without late-night bus service to days with these services, and then contrast this change for people who live near late-night bus lines with the same change for those who do not. This approach allows the estimation of the causal impact Austin's late-night bus services have on DWI arrests. The underlying assumption in this identification strategy is that absent the late-night bus services the change between days when the services don't operate to the days in which they do would follow a similar pattern for each of the groups. While this assumption is not directly testable using the DWI arrest data in this study, there is a very consistent pattern across different cities and states as well as over time in the rate of both drunk driving arrests as well as drunk driving accidents in which they increase significantly during the weekend compared to weekdays. It is not unreasonable to assume that a similar pattern exists among residents in different areas of the same city, though further research into these patterns would be informative.

Since identification in this study relies upon variation in transit availability by day of the week rather than using the initial launch of these late-night services there are some factors which could bias my estimates of the effect these services have on drunk driving. It is possible that people who live near a late-night bus route might shift their drinking to nights of the week on which the services operate. If this happens that would potentially result in fewer DWI arrests on days when late-night services aren't operating for people who live near late-night bus stops. This would bias my estimates towards finding a smaller effect of late-night buses on drunk driving.

Potential drunk drivers aren't the only ones who might have a behavioral response to these bus services. Police looking for drunk drivers might adjust their target areas based on where the late-night buses operate, targeting areas without these services for greater scrutiny on days when the buses operate. This could potentially bias my estimates towards finding a greater effect of late-night buses on drunk driving because it could increase the observed arrests for the control group relative to the treatment group independent of the number of people in each group who actually drive drunk. This may not be a significant concern as it would require drunk drivers who live near late-night bus routes to mostly drive near these routes and conversely for drunk drivers who do not live near them to primarily drive away from the routes. Since the late-night routes serve central entertainment districts and run along major corridors it is unlikely that police officers would significantly shift their drunk driving enforcement away from these areas on days the late-night services run, which are also high-risk days for drunk driving.

Finally, it is important to consider the external validity of these estimates. It is very likely that the days of the week chosen for late-night bus operation were done to maximize their utilization. For the "E-Bus" especially, the Thursday through Saturday operation covers the highest-risk days for drunk driving. This means that my estimated effects of these services may be greater than the effect would be if these services began operating on other, lower-risk days of the week. This study also uses data on DWI arrests and late-night bus service for only a single city, Austin, Texas. The effect of similar services on other cities might differ from the effects in Austin. Preferences for public transit, population density, location of drinking establishments, and many other factors could impact the effect late-night buses have on drunk driving.

3.5 Results

3.5.1 City-wide Results

To estimate the effects of Austin's late night buses on DWI arrests I begin by grouping the home addresses for people arrested for DWI into three groups: those who live within walking distance of a Night Owl bus route; those who live within walking distance of an Entertainment Bus (E-Bus) route; and those who don't live close to either. As I described in Section 3, I use the Google Maps Directions API to calculate the walking time to the closest late night bus stop. To classify each address into one of these three groups I use four different definitions of "walking distance": within 5 minutes, 10 minutes, 15 minutes, and 20 minutes. I estimate the effect of the late night bus services under each of these four definitions.

I use the actual count of DWI arrests for each group for each day in my estimation. Accordingly, I utilize Poisson estimation techniques to account for the distribution of observed daily DWI arrests. I begin by estimating the effects using fixed effects for the "group" as previously described as well as fixed effects for day of the week, calendar month, and year. I cluster all standard errors at the group level. Table 4 presents the results of this estimation for each definition of "walking distance" from a late-night bus route. The first and second row give the estimated effect of each of the two late-night bus services on DWI arrests.

For people who live very close (within a 5-minute walk) of one of the Night Owl bus routes I estimate that the availability of these services reduces the incidence of DWI arrests by 16.8%. For the E-Bus service, I find no effect of their presence on DWI arrests for people living within a 5-minute walk of an E-Bus stop. As I expand the distance from the late-night bus stops the effect of the Night Owl buses decreases but remains a 6.5% to 7.7% reduction in the number of DWI arrests for people who live within a 10-20 minute walking distance of the stops. For the E-Bus service the effect on DWI arrests increases as I expand the walking distance radius. While effects remain imprecisely estimated, the point estimates of the reduction in DWI arrests increase to a 4.8% to 6.6% reduction for a walking radius of 10-20 minutes from the E-Bus stops. The difference in DWI reduction between the two types of late-night bus services may be driven by the different populations each serves. The E-Bus service focuses on areas with large amounts of student housing, while the Night Owl service covers a much broader range of neighborhoods. The coefficient estimates for the group living in close proximity to an E-Bus route shows that this population is substantially less likely to be arrested for DWI

Table 3.4: City-wide Effects of Late-Night Buses on DWI Arrests

	(1) Within 5 Minute Walk	(2) Within 10 Minute Walk	(3) Within 15 Minute Walk	(4) Within 20 Minute Walk
Treat Night Owl	-0.184***	-0.067*	-0.074*	-0.080*
	(0.013)	(0.034)	(0.036)	(0.037)
Treat E-Bus	-0.013	-0.053	-0.068+	-0.049
	(0.009)	(0.033)	(0.037)	(0.052)
Day of Week				
Monday	-0.526***	-0.536***	-0.528***	-0.521***
	(0.018)	(0.035)	(0.041)	(0.055)
Tuesday	-0.666***	-0.676***	-0.668***	-0.661***
	(0.020)	(0.042)	(0.044)	(0.045)
Wednesday	-0.478***	-0.487***	-0.479***	-0.472***
	(0.014)	(0.033)	(0.037)	(0.034)
Thursday	-0.230***	-0.238***	-0.229**	-0.222**
	(0.022)	(0.073)	(0.084)	(0.078)
Friday	-0.058*	-0.065+	-0.056+	-0.050
	(0.026)	(0.035)	(0.030)	(0.060)
Saturday	0.209***	0.201***	0.210***	0.217***
	(0.012)	(0.026)	(0.029)	(0.029)
Near Night Owl	-1.748***	-1.170***	-0.695***	-0.375***
	(0.011)	(0.028)	(0.030)	(0.030)
Near E-Bus	-3.108***	-2.700***	-2.510***	-2.367***
	(0.005)	(0.017)	(0.019)	(0.027)
N	2097	2097	2097	2097

Standard errors in parentheses, clustered at the late-night bus proximity group level.

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

All specifications include calendar month and year fixed effects and use Poisson estimation.

Addresses are "near" a bus route if they are within the specified walking distance.

on average than those living in other areas.

I next estimate the same models but instead of using fixed effects for day of the week, calendar month, and year, I use a full set of date fixed effects. This potentially controls better for variation over time, though at a cost of estimation power due to the large number of fixed effects. Table 5 presents the results of this estimation. The estimates of the effect of each type of late-night bus service do not change with the inclusion of the full date fixed effects. These results can be used to infer that the day of the week, calendar month, and year fixed effects are properly capturing variation over time that is common to all three groups.

3.5.2 Zipcode-level Results

It is possible that there is heterogeneity among residents in different parts of the city, even if they fall into the same group in terms of access to late-night bus transit. To account for residential location differences, I separate the DWI arrest addresses by both late-night bus group and by Zipcode. Table 6 presents the results using day of the week, calendar month, and year fixed effects as well as a full set of Zipcode by group fixed effects. All standard errors are clustered at the Zipcode by group level. Inclusion of the Zipcode-level fixed effects has no impact on the point estimates of the impact of the late-night bus services, though it does substantially increase the standard errors.

As with the city-wide estimates, I also estimate the Zipcode-level effects using a full set of date fixed effects to more precisely control for variation over

Table 3.5: City-wide Effects of Late-Night Buses on DWI Arrests - Full FE

	(1) Within 5 Minute Walk	(2) Within 10 Minute Walk	(3) Within 15 Minute Walk	(4) Within 20 Minute Walk
Treat Night Owl	-0.184***	-0.067*	-0.074*	-0.080*
	(0.013)	(0.034)	(0.036)	(0.037)
Treat E-Bus	-0.013	-0.053	-0.068+	-0.049
	(0.009)	(0.033)	(0.037)	(0.052)
Near Night Owl	-1.748***	-1.169***	-0.695***	-0.375***
	(0.011)	(0.028)	(0.030)	(0.030)
Near E-Bus	-3.108***	-2.700***	-2.510***	-2.367***
	(0.005)	(0.017)	(0.019)	(0.027)
N	2097	2097	2097	2097

Standard errors in parentheses, clustered at the late-night bus proximity group level.

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

All specifications include date fixed effects and use Poisson estimation.

Addresses are "near" a bus route if they are within the specified walking distance.

Table 3.6: Zipcode-Level Effects of Late-Night Buses on DWI Arrests

	(1) Within 5 Minute Walk	(2) Within 10 Minute Walk	(3) Within 15 Minute Walk	(4) Within 20 Minute Walk
Treat Night Owl	-0.184^{+}	-0.067	-0.074	-0.080
	(0.100)	(0.075)	(0.070)	(0.067)
Treat E-Bus	-0.013	-0.053	-0.068	-0.049
	(0.061)	(0.055)	(0.061)	(0.054)
Day of Week				
Monday	-0.526***	-0.536***	-0.528***	-0.521***
	(0.051)	(0.056)	(0.053)	(0.057)
Tuesday	-0.666***	-0.676***	-0.668***	-0.661***
	(0.061)	(0.063)	(0.068)	(0.067)
Wednesday	-0.478***	-0.487***	-0.479***	-0.472***
	(0.063)	(0.059)	(0.065)	(0.067)
Thursday	-0.230***	-0.238***	-0.229***	-0.222***
	(0.056)	(0.058)	(0.061)	(0.065)
Friday	-0.058	-0.065	-0.056	-0.050
	(0.039)	(0.043)	(0.047)	(0.050)
Saturday	0.209***	0.201***	0.210***	0.217***
	(0.037)	(0.038)	(0.042)	(0.048)
N	53124	53124	53124	53124

Standard errors in parentheses, clustered at the late-night bus proximity group level.

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

All specifications include calendar month and year fixed effects and use Poisson estimation.

Addresses are “near” a bus route if they are within the specified walking distance.

time that is common across groups and Zipcodes. The results are presented in Table 7. As in the city-wide results, adding the full date fixed effects has no impact on the estimated effect of either late-night bus service.

Table 3.7: Zipcode-Level Effects on DWI Arrests - Full FE

	(1) Within 5 Minute Walk	(2) Within 10 Minute Walk	(3) Within 15 Minute Walk	(4) Within 20 Minute Walk
Treat Night Owl	-0.184^{+}	-0.067	-0.074	-0.080
	(0.100)	(0.075)	(0.070)	(0.067)
Treat E-Bus	-0.013	-0.053	-0.068	-0.049
	(0.061)	(0.055)	(0.061)	(0.054)
N	53124	53124	53124	53124

Standard errors in parentheses, clustered at the late-night bus proximity group by Zipcode level.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
All specifications include date fixed effects and use Poisson estimation.
Addresses are "near" a bus route if they are within the specified walking distance.

3.5.3 Robustness

A concern when using clustered standard errors is their reliability when the number of clusters is small. Cameron and Miller (2015) demonstrate that small numbers of clusters can lead to artificially small standard errors. My primary analysis clusters at the "group" level, meaning there are only three clusters in the city-wide estimates. To ensure that the standard errors I calculate are not significantly underestimated I repeat the estimation in Table 5 clustering at the date level instead of the group level. This results in 699 clusters instead of the three used in the original analysis. This level of clustering accounts for correlation in the errors across "groups" within a particular day. This is reasonable, as day-specific factors such as weather or special events could affect not only the level of DWI arrests, which are captured by the date fixed effects, but also the variability. The results of this estimation are presented in Table 8. As Cameron and Miller (2015) demonstrate, it appears that the small number of clusters used in Table 5 resulted in significantly underestimated standard errors. Correcting for this by clustering at the date level results in the same 16.8% reduction in DWI arrests for people who live within a five-minute walk of a Night Owl bus stop but no statistically significant effect for those who live farther away or for those living near an E-Bus stop.

3.6 Conclusion

Drunk driving is a persistently common problem in the U.S., causing over 11,000 deaths and 326,000 injuries annually. Around 1.2 million Americans are arrested each year for driving under the influence. Accordingly, discouraging individuals from driving drunk is an important issue for policymakers around the country. Traditional prevention methods tend to focus on discouragement through increased enforcement and enhanced penalties for those caught driving drunk. Most academic research has focused on measuring the effectiveness of these types of strategies.

Increasing the expected cost of drunk driving isn't the only potential method for reducing the incidence of drunk driving. Increasing the availability and attractiveness of alternative forms of transportation can potentially

Table 3.8: City-wide Effects of Late-Night Buses on DWI Arrests - Date Clustering

	(1) Within 5 Minute Walk	(2) Within 10 Minute Walk	(3) Within 15 Minute Walk	(4) Within 20 Minute Walk
Treat Night Owl	-0.184*	-0.067	-0.074	-0.080
	(0.080)	(0.071)	(0.065)	(0.065)
Treat E-Bus	-0.013	-0.053	-0.068	-0.049
	(0.119)	(0.107)	(0.104)	(0.102)
Near Night Owl	-1.748***	-1.170***	-0.695***	-0.375***
	(0.071)	(0.065)	(0.059)	(0.060)
Near E-Bus	-3.108***	-2.700***	-2.510***	-2.367***
	(0.085)	(0.077)	(0.075)	(0.074)
N	2097	2097	2097	2097

Standard errors in parentheses, clustered at the date level.

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

All specifications include date fixed effects and use Poisson estimation.

Addresses are “near” a bus route if they are within the specified walking distance.

induce some people who would otherwise drive drunk to instead take alternative transportation. In Austin, Texas, late-night bus services provide an alternative way for people who live near their routes to get to and from drinking establishments.

In this study I take advantage of the fact that Austin's late-night bus services only operate on some days of the week to use a difference-in-differences approach to identify the impact of these services on DWI arrests. Using a unique dataset containing the home addresses for everyone arrested for DWI by the Austin Police Department I find that Austin's Night Owl late night bus service substantially reduces DWI arrests for people who live near one of the Night Owl bus stops. I estimate that this bus service reduces DWI arrests for people living within a five-minute walk of a Night Owl bus stop by 16.8%. For people who live farther from these lines, within 10-20 minute walk of a bus stop, the reduction in DWI arrests is 6.5-7.7% and is imprecisely estimated. Austin's Entertainment Bus, which serves areas of the city with substantial amounts of student housing has a more limited effect on DWI arrests, with imprecisely-estimated reductions of 1.3-6.6%.

These results add to the small but growing literature on the effects of alternative transportation on drunk driving. Quantifying the impacts of services like late-night buses can help provide policymakers with evidence to better weigh the costs of such services against the potential benefits. Additionally, it allows them to compare the costs and benefits, in terms of drunk driving prevention, of increasing the availability of alternative transportation versus traditional enforcement-based prevention strategies.

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