Crimes on the Move: The Effect of Ridesharing Services on Crime

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This study examines the impact of the introduction of ridesharing services like Uber and Lyft on crime rates across U.S. cities, leveraging a natural experiment created by their staggered rollout. Using a Two-way Fixed Effects (TWFE) Difference-in-Differences (DID) model, I find significant reductions in violent crimes, property crimes, and burglary following the entry of these services, while finding no substantial effects on larceny, motor vehicle theft, or arson. The study also investigates the heterogeneity of these effects across different demographic groups, indicating that ridesharing services may influence crime through mechanisms such as employment and demographic changes. This research adds to the limited literature on the relationship between ridesharing services and crime, providing new insights into the potential mechanisms behind these effects.

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I. Introduction

Since the 2010s, app-based ridesharing services have revolutionized urban transportation, impacting various domains such as transportation modes (Brodeur and Nield, 2018; Hall and Krueger, 2018), mental health (Apouey and Stabile, 2022), alcohol consumption (Teltser, Lennon and Burgdorf, 2021; Zhou, 2020; Burtch, Greenwood and McCullough, 2021), recreation (Park, Kim and Pan, 2021; Park, 2020), mortality (Lennon, Saenz and Teltser, 2024), the labor market (Hall and Krueger, 2016; Omberg, 2024; Tashiro and Choi, 2021; Berger, Chen and Frey, 2018), travel behavior (Alemi et al., 2018), traffic congestion, and traffic accidents (Zhou, 2020; Anderson and Davis, 2023; Greenwood and Wattal, 2015). However, the relationship between ridesharing services like Uber or Lyft and crime remains insufficiently understood, as do the underlying mechanisms driving this relationship. The relationship between crime and transportation is generally ambiguous, given that transportation offers mobility to both potential victims and offenders. This study explores the impact of modern, flexible services like ridesharing on crime, in contrast to traditional modes of transportation such as buses, rail, or bicycles.

Ridesharing services could potentially increase crime rates by enabling greater alcohol consumption, yet they might also reduce crime by improving mobility and enhancing the safety of potential victims. Thus, this issue warrants empirical investigation. Moreover, crime associated with ridesharing services has garnered significant media attention, raising safety concerns. However, these concerns are not always substantiated by empirical evidence. Understanding the direct link between ridesharing services like Uber and Lyft and criminal activities, as well as the mechanisms involved, is crucial for informing policy decisions regarding the regulation of ridesharing services and the prevention of crime.

In this paper, I explore the question: How does the introduction and availability of ridesharing services like Uber and Lyft influence specific types of crime within U.S. cities? My hypothesis is that if ridesharing services lead to increased alcohol consumption, this would manifest in higher crime rates—a pattern that has been documented in the U.S., where a positive relationship between alcohol and crime has been observed (Carpenter, 2005, 2007; Barron et al., 2024; Gyimah-Brempong, 2001; Gyimah-Brempong and Racine, 2006; Markowitz, 2005). Conversely, if Uber and Lyft reduce waiting times for passengers, offer pick-up services directly from homes, or generate sufficient employment opportunities Barrios, Hochberg and Yi (2022); Gorback (2020); Omberg (2024), I would expect a reduction in crime, given the well-established negative relationship between employment and crime (Dix-Carneiro, Soares and Ulyssea, 2018; Britto, Pinotti and Sampaio, 2022; Raphael and Winter-Ebmer, 2001; Dell, Feigenberg and Teshima, 2019; Gould, Weinberg and Mustard, 2002; Lin, 2008; Edmark, 2005).

In this study, I leverage the staggered rollout of ridesharing services like Uber and Lyft across U.S. cities as a natural experiment to identify their causal impact on crime rates. Utilizing a Two-way Fixed Effects (TWFE) Difference-in-Differences (DID) model, I compare crime rates before and after the introduction of these services, controlling for a range of demographic and economic factors. Specifically, I contrast cities where a ridesharing service has already been introduced with not-yet-treated cities, where Uber or Lyft has not yet launched, positioning these not-yet-treated cities as a candidate control group. The analysis incorporates event studies to verify the parallel pre-trends assumption, thereby ensuring the robustness of the findings.

I employ data from the Uniform Crime Report (UCR) as my primary source, covering the years 2005 to 2019, and encompassing 293 U.S. cities where Uber or Lyft were eventually launched. The results indicate that the introduction of ridesharing services leads to significant reductions in violent crimes, property crimes, and burglary, with these effects becoming more pronounced over time. Conditional on fixed effects and covariates, my fully parameterized baseline estimates suggest that the introduction of Uber or Lyft in a city resulted in a 4.7%, 5.65%, and 10.63% reduction in violent crime, property crime, and burglary rates, respectively. However, no significant or robust impact is observed on crimes such as larceny, motor vehicle theft, and arson. These findings imply that ridesharing services may have a more substantial influence on crimes against persons rather than property or societal crimes.

To reinforce these findings, I conduct a series of robustness checks, including alternative estimation methods, different definitions of the dependent variable, the use of incident-based NIBRS data, and Google Trends data as a proxy for Uber and Lyft's market penetration. Additionally, I perform in-time and in-space placebo tests, falsification tests, and county-state level analyses. Given concerns about potential biases in Two-Way Fixed Effects (TWFE) estimators, particularly with heterogeneous treatment effects, I also replicate the results using methods from (Borusyak, Jaravel and Spiess, 2024), (Callaway and Sant'Anna, 2021), and (Wooldridge, 2021). These approaches consistently confirm the robustness of my findings, effectively accounting for heterogeneous treatment effects across cities and over time.

The causal interpretation of these results hinges on the assumption that cities that had already received ridesharing services would have exhibited similar trends in crime rates compared to not-yet-treated cities, in the absence of Uber or Lyft's entry. I provide several pieces of evidence to support this identification assumption, including trend analysis, the inclusion of relevant covariates that could influence both the launch of a ridesharing service and the crime rates in a city, and event study graphs utilizing OLS TWFE and other newly developed heterogeneous-staggered DID estimation methods. Crucially, my event study specification confirms that there is no violation of the parallel trend assumption, reinforcing the validity of the causal interpretation.

This study investigates several potential mechanisms through which the introduction of ridesharing services like Uber and Lyft may contribute to the decline in crime rates. To identify these mechanisms, I analyze data from the Bureau of Labor Statistics and find a statistically significant impact of these services on labor market outcomes, reflected in improved employment rates and reduced unemployment. In examining the heterogeneity of this effect, I demonstrate that Uber and Lyft have a stronger impact on crime reduction in cities with above-average unemployment rates, particularly in relation to money-related crimes. This 'gig economy effect' indicates that improvements in the labor market represent a key pathway through which ridesharing services influence urban crime rates.

To further explore the mechanisms behind the observed decline in crime rates in cities that were early adopters of ridesharing services, I consider factors such as demographic shifts, alcohol consumption, and changes in transportation modes, suggesting that these mechanisms may have played a less significant role. Additionally, I discuss other potential factors, including air pollution, recreation, and mental health, which may also contribute to the observed reductions in crime.

I also examine heterogeneous treatment effects to explore how the impact of Uber and Lyft on crime rates varies across different demographic groups, utilizing data from the American Community Survey (ACS). By interacting the treatment indicator with quartiles based on race, gender, income, and employment, the analysis reveals that ridesharing services have a more pronounced effect in cities with higher female populations and those with a greater proportion of high school graduates, particularly concerning violent crimes. Furthermore, cities with higher unemployment rates experience a reduction in certain property crimes, reinforcing the notion that ridesharing services influence crime through employment-related mechanisms.

Our study contributes to several strands of literature. First, there is a limited number of peer-reviewed papers on the impact of ridesharing services on crime. My primary contribution is providing direct evidence of the relationship between Uber/Lyft and crime. The closest works to mine are studies by Dills and Mulholland (2018) and Martin-Buck (2016), which investigate the impact of Uber's entry on crime in the U.S. Dills and Mulholland (2018) estimates a 0.8% decline in the arrest rate for DUIs and observes increases in motor vehicle thefts over time, as well as decreases in assaults and disorderly conduct. However, for most specifications, the observed declines in arrest rates for other crimes are imprecisely estimated. Martin-Buck (2016), on the other hand, finds that ridesharing's introduction leads to a 7.9% and 9.3% decline in arrests for physical and sexual assault, respectively, with no significant impact on arrests for drunkenness or liquor law violations.

The key difference between my study and these previous works is that while both studies identify an association between Uber's presence and crime, they do not provide direct evidence on the causal pathways. For instance, these studies demonstrate that Uber's entry is associated with a reduction in certain types of crime and traffic fatalities, but they do not fully explore the underlying mechanisms, such as whether the reduction is due to decreased drunk driving, increased mobility, changes in law enforcement practices, or other factors. Furthermore, while these studies rely on arrest data, I utilize offense data, offering a different perspective on crime. In addition, whereas they rely solely on Uniform Crime Reporting (UCR) data, I use both UCR data and National Incident-Based Reporting System (NIBRS) data, which provides further robustness to my findings.

Moreover, I make a methodological contribution by employing several staggered DID models that are more rigorous than those used in the previous studies, which struggle to account for heterogeneous treatment effects and face issues with negative weighting. There have been questions regarding the validity of findings when traditional TWFE models are applied in staggered designs. This paper demonstrates the sensitivity of the results found in these earlier studies, thereby complementing and extending their conclusions.

Second, my findings contribute to the growing body of research on the positive direct or spillover effects of Uber, such as its positive impact on new business formation (Barrios, 2020), the doubled net creation rate of restaurants and improved resident welfare (Gorback, 2020), increased efficiency (Cramer and Krueger, 2016), reduction in unemployment (Omberg, 2024), decreased traffic fatalities (Anderson and Davis, 2023; Greenwood and Wattal, 2015), and improved air quality (Sarmiento and Kim, 2021). Third, this work is more broadly linked to the existing literature on transportation and crime, which itself presents ambiguous findings. Previous research highlights the relationship between crime and various transportation initiatives, including safe ride programs Weber (2014), bus lines (Neiss, 2016), light rail (Liggett, Loukaitou-Sideris and Iseki, 2003; Billings, Leland and Swindell, 2011), university-provided bus services (Heywood and Weber, 2019), late-night metro services (Jackson and Owens, 2011), bus stops (Yu, 2009), metro rail transit (Ridgeway and MacDonald, 2017; Irvin-Erickson and La Vigne, 2015), and the closure of subway stations (Wu and Ridgeway, 2021).

Most of these studies suggest a negative relationship between transportation options and crime, though there are notable exceptions. For instance, Neiss (2016) find that the implementation of a bus line led to an increase in property crime rates in affected areas, with mean property crime rates rising by approximately 1.4% to 2.8% in the tracts served by the bus line. Similarly, Yu (2009) concludes that bus stops and certain types of commercial establishments can create higher crime opportunities in their immediate vicinity, with the effect being context-dependent; for example, food stores are more strongly linked to crime than other types of businesses. Jackson and Owens (2011) observes that metro stations exhibit varying crime patterns depending on their nodal and place-based characteristics, which differ by crime type and time of day, supporting the notion that transit stations can be criminogenic environments with specific risk factors varying across locations and times. My paper distinguishes itself from this body of work by focusing on a modern, flexible, and dynamic mode of transportation that addresses the 'last mile' problem, as opposed to the more static transportation modes examined in these studies.

The remainder of this paper is organized as follows: the second section (II) introduces the brief history and background of ridesharing services in the United States. The third section (III) discusses the data source, description, summary statistics and trend analysis. The fourth section (IV) introduces empirical strategy and explains research design. The fifth section (V) reports empirical results. The sixth section (VI) unveils several possible mechanisms by which ridesharing services may affect criminal incidences. The seventh section (VII) provides several robustness checks of the main results. The eighth section (VIII) reports heterogeneity of the treatment effects. The final section (IX) concludes the study with some policy implications.

II. Background

A. Ridesharing services in the US

According to Bloomberg Second Measure, by 2024, Uber and Lyft collectively account for nearly 100 percent of the U.S. ridesharing or ride-hailing market, with Uber dominating 76 percent of the market share. Since its launch in San Francisco in July 2010, Uber has expanded its services globally, primarily at the city level (Punt et al., 2023). As of 2024, Uber operates in over 10,000 cities across more than 70 countries, facilitating 9.4 billion trips. By Q4 2023, Uber reported having 150 million Monthly Active Platform Consumers (MAPCs) (Uber Technologies Inc., 2024) [1] Conversely, Lyft introduced its peer-to-peer ridesharing service in 2012 and currently operates in more than 65 U.S. cities (Feeney 2015). Like Uber, Lyft was founded in San Francisco on June 9, 2012 (initially as Zimride), and by 2024, Lyft operates exclusively in two countries: the United States and Canada. In 2023, more than 40 million people utilized the Lyft platform to find a driver, bike, or scooter (Lyft Economic Impact Report 2024). [2]

The services of both Uber and Lyft can be ordered via a mobile application available on the three most widely used smartphone operating systems (iOS, Android, and Windows Mobile). After downloading and registering for the app, potential passengers can check the location of the nearest available Uber or Lyft cars. They can also view the driver's ratings and reviews before deciding which car to order. Likewise, drivers can check the profile of the potential passenger and confirm the ride request. Payments are made through the mobile app, meaning there are no cash transactions in the car. After the trip, both the passenger and the driver can rate and review each other, generating additional ratings and reviews (Bakó et al., 2020). The success of these ridesharing companies is largely due to their ability to attract customers who are frustrated with taxicab and public transportation services, or who want to avoid high parking costs or the need to drive home after a night out (Schaller, 2021; Rayle et al., 2016). Uber's user base primarily consists of a young, tech-savvy urban population. By 2019, a Pew Research Center survey found that 70% of urban college graduates had used ride-hailing services like Uber or Lyft (Schaller, 2021;

[1] As of December 31, 2023, The total assets of Uber Technologies, Inc. is $38,699 million with the net income of $1,887 million. Link: https://investor.uber.com/news-events/news/press-release-details/2024/Uber-Announces-Results-for-Fourth-Quarter-and-Full-Year-2023/default.aspx

[2] For the year ended December 31, 2023, Lyft has a net loss of $340.3 million, total assets of US$4.56 billion with the revenue of US$4.40 billion. Active Riders for the fourth quarter of the year 2023 are 22.4 million with the total number of rides 709 million. (Lyft Annual Report 2023)

Jiang, 2019). Both Uber and Lyft utilize variable or dynamic pricing based on demand and supply, meaning that prices can increase during peak times, in high-demand areas like airports, or during inclement weather. This pricing model ensures vehicles remain available late at night or in bad weather, while the automatic payment system eliminates the need for passengers to carry cash or negotiate tips (Flores and Rayle, 2017).

B. The Rollout of Ridesharing Services

For this study, I manually compiled the launch dates of Uber and Lyft for 519 cities, focusing exclusively on cities with populations exceeding 100,000. The details of this data collection are described in the data section. Figures 1, 2, and 3 illustrate the gradual expansion of ridesharing services across U.S. cities from 2010 to 2019, with each point representing the location of a launched city. Each subfigure displays the cumulative launch of ridesharing services up to a particular year. For instance, subfigure (d) of Figure 1 shows the locations of cities where Uber or Lyft had been launched by 2013, in addition to those launched in 2010, 2011, and 2012. Moreover, Figure 1 presents the monthly launch of ridesharing services in a bar graph. As shown in this graph, the number of cities where Uber or Lyft were available increased from 1 in 2010 to 293 in 2014. The graph clearly reveals three distinct waves of Uber launches: the first before 2014, during which 88 cities were treated, including all of the most populous cities; the second between 2014 and 2016, particularly in 2014, when 205 cities saw the introduction of app-based ridesharing services in a single year; and the third after 2016, when 88 cities were introduced to ridesharing services.

A central concern in my identification strategy is that the decision of a ridesharing service to enter a city may be correlated with factors that also affect the crime rate in that city. Notably, the most significant factor influencing the timing of Uber's entry appears to be the population size of a metropolitan area, with larger MSAs experiencing earlier entry. Population size alone can account for approximately 40 percent of the variation in Uber's entry timing in a bivariate regression (Berger, Chen and Frey, 2018). Moreover, Punt et al. (2023) observed that, in the United States, Uber preferred locations with lower levels of employment protection, lower compliance with employment laws, and pro-market institutions. Ride-hailing companies are also less likely to operate in cities where fingerprint-based background checks are mandatory (Beer et al., 2017).

Inspired by Berger, Chen and Frey (2018), I plot a binned scatterplot in Figure 5, illustrating the relationship between the logarithm of city populations and the mean year of ridesharing (Uber/Lyft) entry across different cities. The data are grouped into 10 bins based on the logarithm of city populations, with each bin showing the mean year of ridesharing entry. Each point on the scatterplot represents the mean year of ridesharing entry for cities within a specific population bin. The plot shows a downward trend from left to right, providing visual evidence that larger cities (with higher populations) tended to adopt ridesharing services earlier. While larger cities were the primary market for Uber and Lyft during the initial expansion, these services were sometimes launched earlier in smaller cities, potentially influenced by factors such as proximity to already-launched larger cities or the tendency to launch in cities within California, where these companies were founded.

Next, I will discuss how city-level regulations influence Uber and Lyft's decisions to enter a market. To circumvent regulations, Uber differentiates itself from traditional taxi companies, which are heavily regulated, by branding itself as a technology platform rather than a taxi company. Furthermore, Uber classifies its drivers not as employees but as 'registered partners' (Dudley, Banister and Schwanen, 2017; Collier, Dubal and Carter, 2018).

The regulations imposed on Uber vary widely, ranging from restrictions on the number of ride-hailing vehicles allowed to operate, such as in Atlanta, to laws requiring ridesharing services to provide a list of drivers and share trip data with city officials (Beer et al., 2017). Uber, and similarly Lyft, adopt an "act first, apologize later" approach that prioritizes market entry over compliance with existing regulatory frameworks (Collier, Dubal and Carter, 2018).

In each new U.S. city they enter, ridesourcing companies have replicated this strategy: they enter the market, rapidly expand their user base, and then, when regulators file lawsuits, issue fines, or initiate regulatory proceedings, they mobilize support from passengers and drivers to lobby elected officials. This approach has proven to be a powerful tool for mobilizing customers and drivers to advocate for Uber's stance on regulatory matters, often through simple actions like clicking a link. In addition to this "clicktivism," Uber has extensively used online petitions, which it has publicized to drivers and customers. Ultimately, political authorities, often at the state level, have tended to intervene in their favor. Uber strategically calculates its actions, and in its largest markets, it has ultimately accepted even the regulations it most opposes. For instance, in New York, the largest ride-hailing market, Uber has accepted fingerprinting and other regulations. However, in smaller markets like Austin, Uber followed through on its threat and exited the market. In other cities, Uber's structural power, exercised through threats to leave a market, has been an effective tool for overturning regulations (Flores and Rayle, 2017; Collier, Dubal and Carter, 2018).

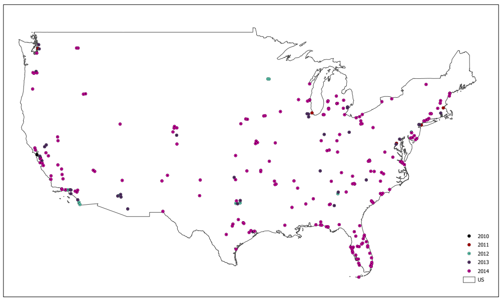
(a) 2010

(b) 2011

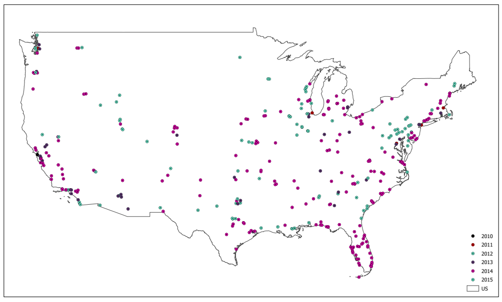
(c) 2012

(d) 2013

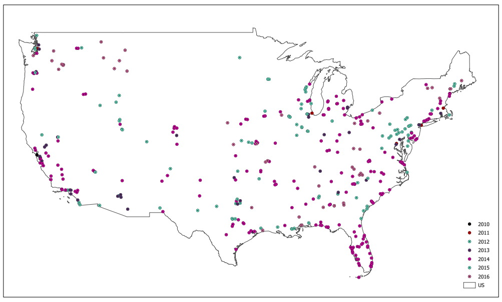
Figure 1. : Launch Timelines of Uber and Lyft During 2010, 2011, 2012, and 2013



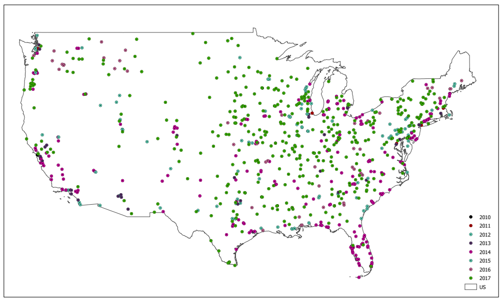
(a) 2014



(b) 2015

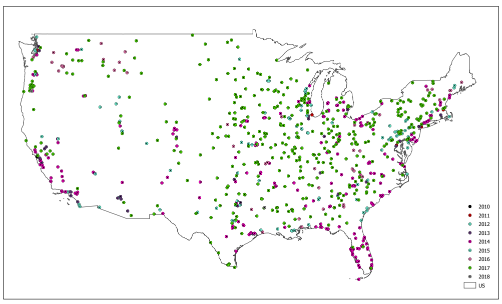


(c) 2016

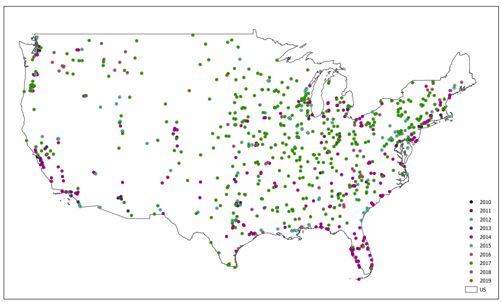


(d) 2017

Figure 2. : Launch Timelines of Uber and Lyft During 2014, 2015, 2016, and 2017



(a) 2018



(b) 2019

Figure 3. : Launch Timelines of Uber and Lyft During 2018, and 2019

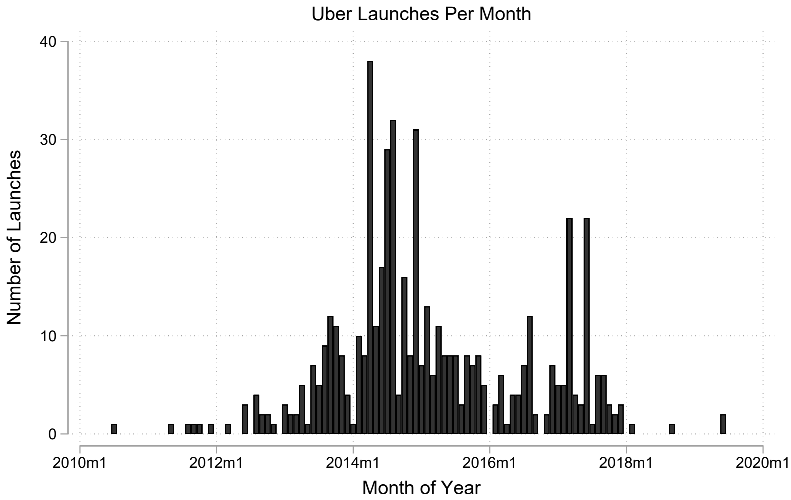


Figure 4. : Monthly Launch of Ridesharing Services

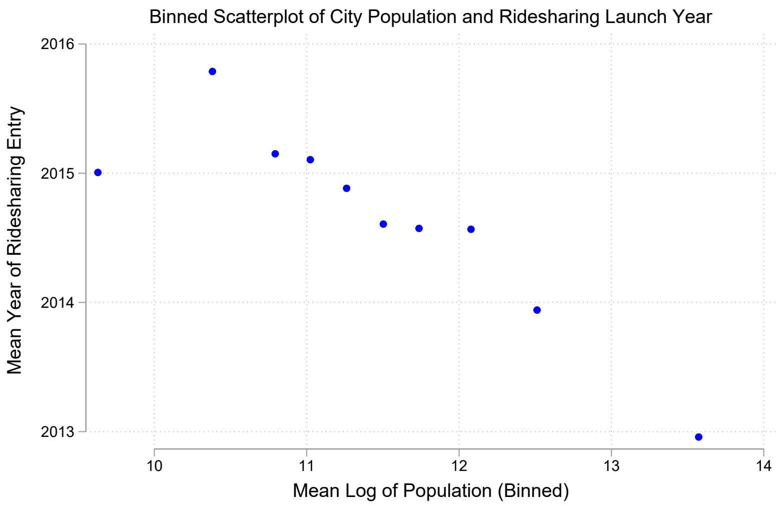


Figure 5. : Binned scatter plot of population and launch of ridesharing services

III. Data

The primary data source for this analysis is the United States Federal Bureau of Investigation (FBI)'s Uniform Crime Reports (UCR), which consists of a panel of annual, city-level offense volume observations from 2005 to 2019. These data were obtained from "Table 8: Offenses Known to Law Enforcement, by State by City, year" in the "Crime in the United States" series, which is published annually by the FBI.[3] The series provides offense volume data collected through the Summary Reporting System. [4] In 2019, the Summary Reporting System (SRS) included data from over 18,000 law enforcement agencies across the United States, typically covering more than 90 percent of the U.S. population. The FBI concluded SRS data collection in 2020, the same year that the COVID-19 pandemic began, prompting me to limit my sample to 2019 (Parker, 2022). On January 1, 2021, the Uniform Crime Reporting (UCR) program transitioned to the National Incident-Based Reporting System (NIBRS), which became the primary national crime data collection program. In the robustness section, I supplement the results derived from UCR data with daily, incident-level crime data from NIBRS, also maintained by the FBI. The UCR data collection began in the 1920s in various forms, while NIBRS was introduced in the 1980s. NIBRS data are more detailed and insightful than summary-based UCR data, offering information about the date and location of crime incidents, as well as details on the characteristics of the crime, perpetrators, and victims. [5]

A key difference between these datasets is that SRS only records the most severe crime in an incident—following the Hierarchy Rule—while NIBRS captures all crimes related to a single incident. For instance, if a murder and robbery occur in the same incident, SRS would only count the murder and aggregate it with other murders, omitting the robbery. In contrast, NIBRS would report both the murder and the robbery, with the capacity to record up to 10 offenses per incident.

Although no federal law mandates participation in NIBRS, many local and state laws do. Consequently, a significant issue with NIBRS data is the alarmingly low level of agency participation. As of 2019, NIBRS covered only 53% of the U.S. population. Figure A1 details the range of NIBRS data across all U.S. states. Notably, NIBRS data were unavailable for any agencies in California, Florida, New York, New Jersey, North Carolina, and Wyoming before 2019—the last year of UCR data in my sample. Additionally, states such as Georgia, Hawaii, Indiana, Maryland, Minnesota, Mississippi, and New Mexico lacked NIBRS data before 2010—the first year ridesharing apps were launched. This limitation likely explains why most crime studies in economics continue to rely on UCR data. The limited coverage of NIBRS presents several challenges for the robustness of my NIBRS results, particularly since major cities in California, New York, and Florida—especially San Francisco—are national leaders in ridesharing app usage. As a result, I primarily use UCR data for the analysis, focusing on 10 different types of crime: violent crime, murder and non-negligent manslaughter, forcible rape, robbery, aggravated assault, property crime, burglary, larceny-theft, motor vehicle theft, and arson. For a brief overview of UCR crime type definitions, please refer to Table A2.

I merge the ridesharing services launch data with the UCR crime data. I manually compiled the launch dates of Uber and Lyft in 519 cities, drawing from Uber and Lyft's websites, social media posts about the launches, local and national news websites, and the UberX entry and exit dates provided in Table C1 by Lennon, Saenz, and Teltser (2024).

[3]Table 6 for the year 2016

[4]https://ucr.fbi.gov/crime-in-the-u.s

[5]NIBRS provides the following information: offense(s), offender(s), victim(s), arrestee(s), and any property involved in an offense. NIBRS reports incident and arrests data for 24 categories of offenses known as Group A offenses and only arrests data for 10 additional categories of offenses known as Group B.

Key variables include the launch date for both Uber and Lyft in each city, along with corresponding county and state information. In 49 cities, Lyft was launched earlier than Uber, with Corpus Christi, TX, experiencing the longest delay, where Uber launched 38 months after Lyft. On average, the time difference between the launches of these companies in such cities is 194 days, or approximately six months. The last city in the sample to receive ridesharing services from Uber or Lyft was Rapid City, South Dakota, on June 20, 2019. For the launch date of a ridesharing service, I compared both Uber and Lyft and selected the earliest launch date. My final sample includes 479 cities with 6,774 city-year observations.

For demographic controls, I use data from the U.S. Census Bureau's American Community Survey (ACS)—a nationally representative, self-reported household survey administered annually to more than three million U.S. households. One-year estimates from 2005 through 2019 are used for the following controls: population, education, race, age, gender, income, and employment.

There are three main reasons for selecting the 2005-2019 sample period. First, the new definitions for ACS data began in January 2005. Second, this period aligns with the UCR data in the sample, which also spans from 2005 to 2019. Third, geographic identifiers at the metropolitan area level were not available until the 2005 survey year; before 2005, the smallest geographic identifier was the state. ACS data are used in this study for three purposes: first, as controls in the main analysis; second, in the mechanism section to investigate whether changes in demographic composition are the primary mechanism through which ridesharing services influence crime; and third, to explore the treatment heterogeneity of the impact of ridesharing services on crime across different groups stratified by age, race, educational attainment, and economic conditions. One limitation of the ACS aggregate data at the MSA level is that the census omits some population totals of interest for certain MSAs in my dataset. The number of missing values for ACS data is provided in Appendix Table X.

I also incorporate All Grades All Formulations U.S. regular gasoline prices (in dollars per gallon) as controls in the main specifications, along with demographic data. These weekly gas prices are sourced from the U.S. Energy Information Administration. However, the gas price data are not available at the city level but are instead provided at the regional level, encompassing regions such as the East Coast, New England, Central Atlantic, Lower Atlantic, Midwest, Gulf Coast, Rocky Mountain, West Coast, and West Coast excluding California. Additionally, gas prices are available for nine populous states and ten major cities. To estimate city-level gas prices, I merge the gas prices for the ten major cities with their respective cities. For other cities within those nine states, I use state-level gas prices, and for the remaining cities, regional-level prices are used as proxies for city-level gas prices. Furthermore, I convert the weekly gas prices to average annual gas prices.

To gauge the intensity of ridesharing app usage in different cities, I use relative Google search data as a proxy. I downloaded the monthly relative Google search data for "Uber" from 2004 to 2024 for 2010 cities from Google Trends.[6] The search results can range from 0 to 100. Following Hall and Krueger (2018), I downloaded the Google search data for San Francisco, where Uber was founded, alongside data for each city. This allows each city's Google search data to be relative to that of San Francisco. I then convert the relative search values into Z-scores for regression and interpretation purposes.

To explore mechanism channels, I also utilize Local Area Unemployment Statistics Data (Smoothed Seasonally Adjusted Metropolitan Area Estimates), sourced from the U.S. Bu-

[6] https://trends.google.com/trends/

reau of Labor Statistics. The key variables of interest include the civilian labor force, total employed, total unemployed, and the unemployment rate.

My final sample comprises 293 cities with 270,475 city-year observations. Table 1 presents the balance test for observable characteristics, comparing cities treated before 2014—where Uber/Lyft launched earlier than 2014 ("Early Adopters")—against cities treated between 2014 and 2016 ("Mid Adopters") and again comparing "Early Adopters" with "Late Adopters," or cities treated after 2016. Columns (1) to (6) display the mean and standard deviation for these six cohorts of cities, while columns (7) and (8) provide the difference and p-value of a two-sided t-test for Early vs. Mid Adopters and Early vs. Late Adopters, respectively. For this balance table, I use data from the year 2010, the year Uber was introduced, thus offering a cross-sectional balance. For balance across all pre-periods, please refer to Table A. Other than crime variables (the outcome variable), no statistically significant differences are observed between these groups. Although there are some differences between earlier and later treated cities, evidence from the literature suggests that the launch of Uber/Lyft in a city is more closely related to the population size rather than crime rates. Additionally, when estimating the effect on crime, I utilize both population-weighted crime variables and raw crime counts. Consequently, I argue that these differences do not necessarily threaten identification. Instead, credible evidence is needed to show that the evolution of city-level crimes, absent the introduction of ridesharing services, serves as a plausible counterfactual for earlier treated cities. As expected, early treated cities have higher incomes, larger populations, and higher crime rates compared to both mid- and late-treated cities.

Before proceeding to the empirical analysis, I first examine crime trends and the evidence of treatment effects without the use of econometric models. Additionally, I review offense rate trends across treated and not-yet-treated cities. Figures A2, A3, and A4 present raw crime rate data per 100,000 population for 10 different types of crime, categorized by three groups of cities: cities treated before 2014 (Early Adopters), cities treated between 2014 and 2016 (Mid Adopters), and cities treated after 2016 (Late Adopters). Each subfigure illustrates the trend for a specific crime type across all three groups of cities. The solid blue line indicates the crime rate for early adopter cities, the red dashed line for mid-adopter cities, and the black long-dashed line for late adopter cities.

Between 2005 and 2010, all crime types, with the exception of rape, exhibit downward trends across the cohorts. For violent crimes, burglary, larceny, robbery, and arson, there is a noticeable and steeper downward slope around 2014, particularly among early and mid-adopters. The murder rate does not display a consistent trend across the cohorts, instead fluctuating over time.

Forcible rape, which was trending downward before 2013, began to trend upward afterward. This shift can likely be traced to the FBI UCR program's redefinition of rape in 2013. The new definition expanded the scope to include both male and female victims and offenders, and broadened the definition to encompass sexual penetration without consent and nonphysical offenses. As a result, this new definition may have led to a reported increase in sex offenses of up to 42.7 percent (Federal Bureau of Investigation, 2013).

Despite differences in the level of crime rates, there are no significant differences in trends among these three cohorts of cities, as they generally move in the same direction. This suggests that later-treated cities serve as appropriate counterfactuals. Moreover, for most crime types, the trends appear nearly parallel across Early, Mid, and Late Adopters, providing visual evidence supporting the parallel trends assumption in my main Difference-in-Differences (DID) specifications.

Table 1—: Balance Table 1

	Early Treated Before 2014		Mid Treated 2014-2016		Newly Treated After 2016		Early vs. Mid	Early vs. New
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Difference (p-value)**	**Difference (p-value)**
Population	11.5	15.1	1.7	2.1	0.8	0.6	9.9*** (0.0)	10.7*** (0.0)
Violent Crime	880.0	407.0	643.9	391.2	532.4	364.9	236.1*** (0.0)	347.6*** (0.0)
Murder	13.6	11.4	9.3	8.0	7.8	6.6	4.3*** (0.0)	5.8*** (0.0)
Forcible Rape	49.0	24.1	56.0	30.4	58.0	36.0	-6.9*** (0.0)	-9.0*** (0.0)
Robbery	340.7	181.5	186.0	152.0	117.9	96.5	154.6*** (0.0)	222.8*** (0.0)
Aggravated Assault	477.9	252.3	398.5	262.6	346.0	271.2	79.3*** (0.0)	131.9*** (0.0)
Property Crime	4279.6	1451.8	4380.9	1801.5	4105.0	1632.9	-101.3 (0.4)	174.6 (0.1)
Burglary	906.0	444.6	942.1	535.2	814.4	506.9	-36.1 (0.3)	91.6** (0.0)
Larceny Theft	2744.9	956.5	3131.6	1282.4	3081.8	1190.3	-386.7*** (0.0)	-336.9*** (0.0)
Motor Vehicle Theft	673.6	326.2	350.3	260.2	234.2	156.4	323.3*** (0.0)	439.4*** (0.0)
Arson	32.3	22.6	30.2	33.0	26.0	20.4	2.0 (0.3)	6.3** (0.0)
High School Grad	19.5	4.9	21.4	5.6	21.9	5.7	-1.9*** (0.0)	-2.4*** (0.0)
Foreign Born	9.4	5.9	3.7	3.1	2.8	2.9	5.7*** (0.0)	6.6*** (0.0)
Hispanic Percentage	46.1	22.9	59.8	29.2	66.0	29.8	-13.7*** (0.0)	-19.9*** (0.0)
Female Percentage	50.8	0.7	50.7	1.0	50.4	1.3	0.1 (0.1)	0.4*** (0.0)
Median Age	24.8	29.0	3.1	4.5	1.3	1.4	21.8*** (0.0)	23.5*** (0.0)
Over 65 Percentage	41.9	15.5	42.6	14.4	42.1	14.7	-0.7 (0.5)	-0.3 (0.8)
Under 5 Percentage	6.4	0.6	6.3	1.0	6.5	1.2	0.1* (0.0)	-0.1 (0.1)
Median Income	70.9	34.0	41.8	22.2	38.8	22.4	29.2*** (0.0)	32.1*** (0.0)
Civilian Employed	21.9	20.3	2.4	2.9	1.1	0.9	19.5*** (0.0)	20.9*** (0.0)
Civilian Labor	23.6	21.7	2.6	3.1	1.1	1.0	21.0*** (0.0)	22.4*** (0.0)
Gasoline Price	2.9	0.5	2.8	0.5	2.8	0.5	0.1* (0.0)	0.1** (0.0)
Observations	219		2270		752		2489	971

Note: This table provides summary statistics segmented by treatment timing for cities that adopted ridesharing services. 'Early Treated' refers to cities that introduced these services before 2014, 'Mid Treated' to those between 2014 and 2016, and 'Newly Treated' to cities starting after 2016. Columns 1, 3, and 5 display the means for each group, while Columns 2, 4, and 6 present the standard deviations. The final two columns show the differences between groups and their respective p-values. Crime variables are expressed as the number of crimes per 100,000 population. The population count, civilian employed, civilian labor force, and median age are all normalized per 100,000 population. Median income figures are presented in thousands of dollars. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

IV. Empirical strategy

To identify the causal impact of the introduction of ridesharing or ride-hailing services on crime rates, I exploit the staggered expansion of services like Uber and Lyft across U.S. cities as a natural experiment. This approach involves comparing crime rates in cities that have not yet been exposed to ridesharing services with those where such services have already been introduced, focusing exclusively on cities where ridesharing services are eventually launched. The staggered rollout of these services enables the application of a Two-way Fixed Effects (TWFE) difference-in-differences (DID) model to estimate the impact of ridesharing on criminal activities:

$$Crime_{j,c,t} = \alpha + \beta_1 Ridesharing_{c,t} + \gamma_i + \phi(t) + \epsilon_{c,t} \quad (1)$$

Here,

- $Crime_{j,c,t}$: Log Crime Index, representing the logarithm of the total number of criminal activities per 100,000 people for crime type j in city c in year t.
- $Ridesharing_{c,t}$: is a dummy variable indicating whether Uber or Lyft is launched for a certain city-year combination
- γ_c : city fixed effects
- $\delta(t)$: year fixed effect
- $\epsilon_{c,t}$: standard errors are clustered at the city level

Here β_1 is the coefficient of interest which shows the marginal effect ridesharing services on crime at the city level. Given the staggered or roll out launch of Uber/Lyft, this overall effect can be interpreted as a weighted average of all possible two-groups-two-periods DiD estimates. Moreover, with only the assumption that the effect of Uber/Lyft is homogeneous for each city and each year can give a causal interpretation. If treatment effects vary across years and crossings, the traditional TWFE settings may assign a negative weight to the β_1. This is attributed to the "forbidden comparison," wherein earlier launched city may be compared with subsequently launched city (Roth et al., 2023). To assess the sensitivity of the results from this baseline specification, I also explore the staggered adoption models with heterogeneous treatment effect in section 6.

The covariate X_{it} includes the percentage of female population (gender), population over 65 years old, foreign-born population (race), Hispanic or Latino population (race), population aged 25 years and over with a high school degree (education), total employed civilian population (labor market outcome), total civilian labor force (labor market outcome), median income (labor market outcome), and regular gasoline prices.

The underlying assumption is that selection into the city is controlled for by the city and year fixed effects. While city and year fixed effects are likely to absorb a wide array of potentially omitted factors that may be correlated with the entry of ridesharing services and crime rates, there is still a concern that crime rates may have evolved differently in cities where Uber and Lyft were introduced due to unobserved time-varying factors. Furthermore, Table 1 documents minor imbalances in select baseline covariates across cities treated before 2014, those treated between 2014-2016, and those treated after 2016. To address this concern, I include a set of time-varying demographic control variables, such as sex, age, education, race, and income. Additionally, I incorporate regular gasoline prices, which may affect the entry of ridesharing services as well as specific types of crime, such as motor vehicle theft.

The identifying assumption behind the difference-in-differences specification is that crime rates would have followed similar yearly trends between cities exposed to ridesharing services early and those treated in later years had it not been for the rollout of Uber and Lyft. A primary concern is that cities exposed early to these services may not have been randomly selected, which could mean that post-launch changes in crime rates are driven by other unobserved factors rather than the ridesharing services themselves. In the robustness sections, I address this issue along with other threats to identification.

To assess the parallel pre-trends between the treatment and control counties, I estimate a specification that incorporates the leads and lags surrounding the launch of ridesharing services, allowing the "impact" of these services to vary annually, starting from at least four years prior to their entry:

$$(2)\quad Crime_{j,c,t} = \alpha + Rideshare_c \left[\sum_{k=-14}^{-2} \eta_{k1}(t - t^* = k) + \sum_{k=0}^{9} \beta_{k1}(t - t^* = k) \right] + \phi_c + \gamma_t + \epsilon_{ct}$$

where $Rideshare_c$ is a binary variable equal to one if city c received the ride-hailing service, with relative yearly dummies. ϕ and λ are city and year fixed effects. Estimation is performed with standard errors ϵ_{ct} clustered at the city level.

The coefficients of interest, denoted as η_{k1} and β_{k1}, capture the dynamic effects of ridesharing services both before and after their rollout. The omitted category is $k = -2$, following Borusyak, Jaravel and Spiess (2024), which includes two periods in the base category to prevent under-identification. This method ensures that all coefficients are estimated relative to these two periods. By employing this dynamic specification, I can test the identification assumption underpinning the DID (Difference-in-Differences) analysis—that is, the assumption that pre-trends in crime rates were parallel between early-treated cities and later-treated cities before the launch of ridesharing services. Furthermore, this approach allows me to observe whether the intensity of the Uber/Lyft treatment changes over time.

Causal identification requires parallel pre-trends between treated and control cities, minimal spillovers onto neighboring cities, and limited anticipation of the launch of a ridesharing service (Callaway and Sant'Anna, 2021). First, I plot the average crime rate for cities introduced to Uber and Lyft before 2014, during 2014-2016, and after 2016 in Figures A2, A3, and A4 showing the raw trend without econometric adjustment. Second, I use a balance table of the crime rate and related covariates in Table 1. Third, I verify the pre-trends visually using the event study plot in Figures 6, 7, and 8. Additionally, I provide event study graphs for alternative and newly developed staggered DID estimators in Figures A8, A9 and A10.

The scope for spillovers between treated and not-yet-treated cities is limited. While one might take an Uber or Lyft ride from a city where these services are available, they cannot take a return trip if the destination city is not yet treated—effectively limiting trips to one-way only. Moreover, few cities in my sample are sufficiently proximate for such trips. I discuss the potential mechanisms through which ridesharing services might influence crime rates, but none appear to operate via spillovers.

A critical identification question is whether the rollout of ridesharing services is exogenous, meaning whether Uber or Lyft launched randomly in cities—an essential feature of a natural experiment. As discussed in Berger, Chen and Frey (2018); Punt et al. (2023), the Uber/Lyft rollout is likely endogenous to certain unobservable city characteristics, particularly population. However, I have already controlled for population in the crime rate analysis. Crime rates or local legal systems do not drive the Uber/Lyft rollout, as explained in the background section. Moreover, given the possibility that city-level demographic, economic, or energy price factors influence a ridesharing service's decision to launch, I have included these as covariates. Based on these considerations, I argue that the launch of ridesharing services in a city is as-good-as-random in this study's context, making it plausible that the crime index is mean-independent of the timing of Uber/Lyft's entry. Additionally, it is plausible that there is limited anticipatory response, given that it takes time for Uber/Lyft to build a consumer base and establish the habit of using ridesharing services, as supported by the event study graphs in Figure A.

V. Results

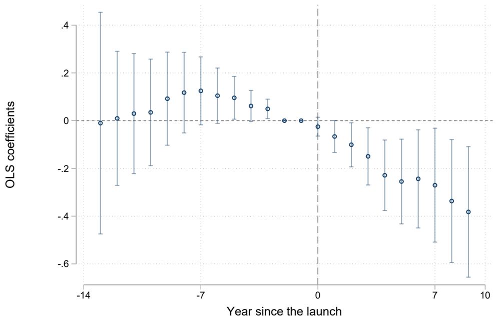
I estimate the baseline model specified in Equation 1 and report the coefficient β in Table 2. Columns (1) to (10) present the results for different types of crime rates (in log form). Additional covariates are included across consecutive panels. In Panel B, I incorporate demographic controls such as age, gender, race, education, and labor market outcomes. In Panel C, I add regular gasoline prices alongside the demographic controls. All regressions control for city and year fixed effects, and cluster-robust standard errors, clustered at the city level, are reported in parentheses.

The results indicate that the signs and magnitudes of the estimates remain consistent across columns (1) to (10) and panels (A) to (C), confirming a reduction in crime following the introduction of Uber and Lyft. Across all panels, violent crimes, property crimes, and burglary show statistically significant decreases due to the arrival of ridesharing services in the city, with magnitudes ranging from 4.5% to 5.5% for violent crimes, 3.2% to 5.5% for property crimes, and 8.1% to 10.1% for burglary. The fully specified model in Panel C suggests that the introduction of ridesharing services is associated with a 4.6%, 5.5%, 10.1%, and 4.7% reduction in the number of violent crimes, property crimes, burglary, and larceny, respectively.

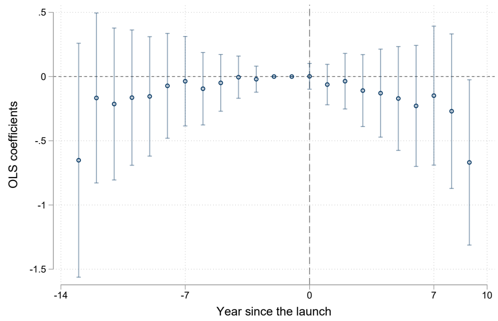
Overall, the introduction of ridesharing services appears to have led to long-term decreases in the total number of criminal occurrences. However, no significant effects were found for certain specific types of crimes, such as larceny, motor vehicle theft, and arson, across different estimations. This suggests that ridesharing services may have a greater impact on crimes against persons rather than crimes against property or society. My findings are consistent with those of Dills and Mulholland (2018) and Martin-Buck (2016), although the magnitude of the effects observed in this study is somewhat higher. It is important to note that these studies used different datasets, such as UCR's arrest data, and different estimation methods compared to mine.

Figure 6 , 7, and 8 graphically present the estimation results for the dynamic effects of ridesharing apps on city-level offense volume per 1,00,000 population. I fail to reject the null hypothesis of no effect on the log of the crime index. While post-treatment estimates are generally negative across all estimators, the coefficients for certain variables, such as var1 and var2, are not statistically significant at the 5 percent level. Although pre-launch coefficients are typically small and insignificant, some crime types exhibit mild signs of decreasing trends in the three or four periods preceding the ridesharing rollout. Moreover, the absence of pre-trends in these new staggered DID methods suggests that the parallel trends assumption holds, making it unlikely that there are differential trends in crime rates prior to the introduction of ridesharing services.

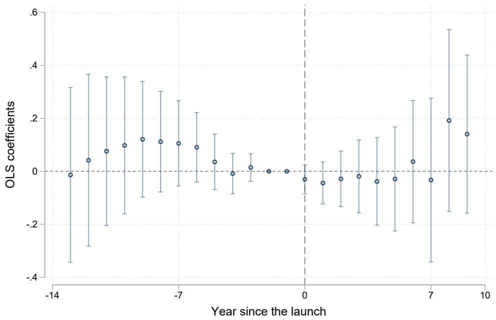
The effects on various types of crime are predominantly medium- to long-term, persisting up to nine years after the launch of Uber and Lyft. Around the time of the ridesharing services' launch (time 0), no sharp discontinuity or immediate change in trend patterns is observed; rather, most effects become evident 4 to 5 years post-launch, indicating that the impact of ridesharing services on crime takes time to materialize. The higher estimates observed in cities exposed early to ridesharing services are reasonable, as these cities tend to be more populous and have higher crime rates compared to smaller cities where ridesharing services were introduced from 2016 onwards.



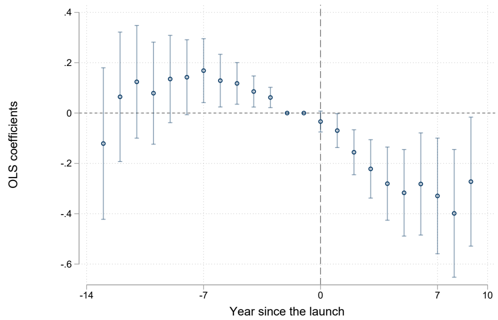
(a) Violent Crime



(b) Murders



(c) Forcible Rape



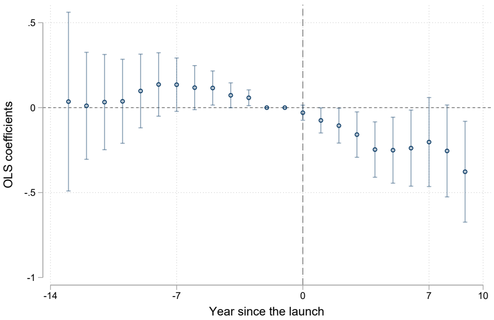
(d) Robbery

Figure 6. : Note: Event study estimates showing the effect of gradual roll out of ridesharing on the following crime types: Violent Crime, Murders, Forcible Rapes and robbery per 100,000 Population. Each dependent variable is a natural log transform of these crimes per 100,000 population.Number along the x-axis indicates years since the entry of Uber/Lyft. This is a City-Year Analysis and years included in this figure are 2005 to 2019. Point estimates come from regressions given by Eq. 2. 95% confidence intervals are displayed and are calculated using robust standard errors clustered at the city level.

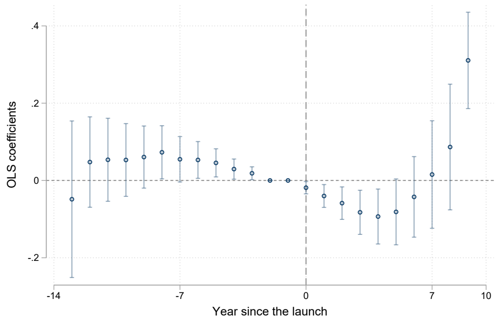
Table 2—: Baseline Results: Impact of Ridesharing Apps on Crime Rate

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Panel A: Without any Controls										
Rideshare	-0.055**	-0.088**	-0.005	-0.068**	-0.063**	-0.032**	-0.081**	-0.018	-0.039	-0.021
	(0.019)	(0.027)	(0.025)	(0.022)	(0.024)	(0.012)	(0.018)	(0.012)	(0.026)	(0.036)
Observations	6655	6771	6661	6773	6768	6292	6765	6756	6767	6078
Panel B: With Demographic Controls										
Rideshare	-0.045*	-0.083*	-0.011	-0.052	-0.038	-0.054***	-0.099***	-0.046**	-0.047	-0.029
	(0.022)	(0.042)	(0.035)	(0.028)	(0.026)	(0.016)	(0.022)	(0.016)	(0.034)	(0.059)
N	2562	2607	2564	2608	2607	2383	2603	2596	2607	2493
Panel C: With Demographic and Gasoline Price Controls										
Rideshare	-0.046*	-0.083	-0.012	-0.053	-0.038	-0.055***	-0.101***	-0.047**	-0.047	-0.029
	(0.022)	(0.042)	(0.035)	(0.028)	(0.026)	(0.015)	(0.022)	(0.016)	(0.033)	(0.059)
Observations	2562	2607	2564	2608	2607	2383	2603	2596	2607	2493
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

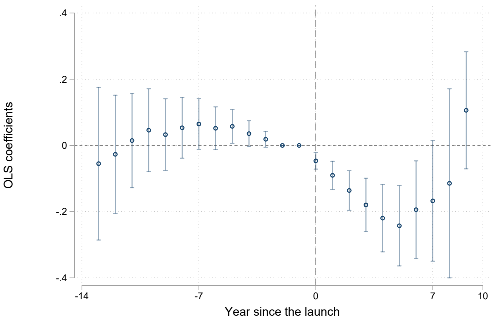
Note: This table presents the impact of ridesharing services on various types of crimes across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the OLS estimation method without any covariates in Panel A, with demographic covariates in Panel B and with both demographic and gasoline price controls in Panel C. The dependent variable is the yearly log of crime index- number of crimes per 100k population. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.



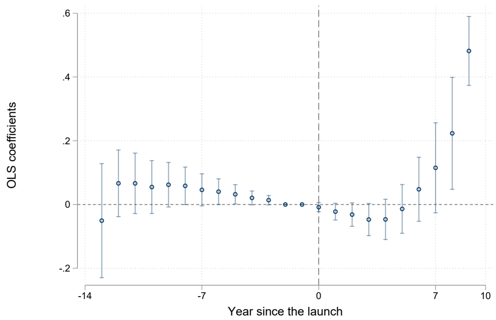
(a) Assaults



(b) Property Crimes

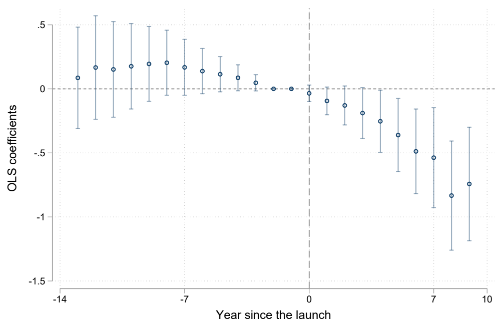


(c) Burglary

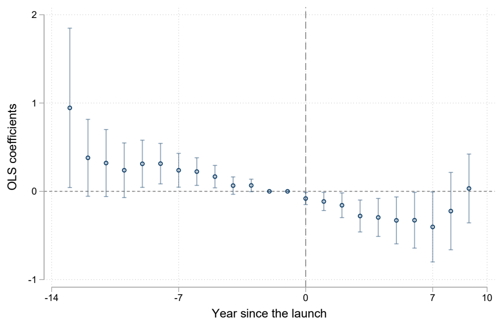


(d) Larceny

Figure 7. : Note: Event study estimates showing the effect of gradual roll out of ridesharing on the following crime types: Assaults, Property Crimes, Burglary and Larceny per 100,000 Population. Each dependent variable is a natural log transform of these crimes per 100,000 population.Number along the x-axis indicates years since the entry of Uber/Lyft. This is a City-Year Analysis and years included in this figure are 2005 to 2019. Point estimates come from regressions given by Eq. 2. 95% confidence intervals are displayed and are calculated using robust standard errors clustered at the city level.



(a) Motor Vehicle Theft



(b) Arson

Figure 8. : Note: Event study estimates showing the effect of gradual roll out of ridesharing on the following crime types: motor vehicle thefts and arsons per 100,000 Population. Each dependent variable is a natural log transform of these crimes per 100,000 population.Number along the x-axis indicates years since the entry of Uber/Lyft. This is a City-Year Analysis and years included in this figure are 2005 to 2019. Point estimates come from regressions given by Eq. 2. 95% confidence intervals are displayed and are calculated using robust standard errors clustered at the city level.

VI. Mechanisms

In this section, I explore potential mechanisms underlying the impact of Uber and Lyft's entry on the decline in crime rates. Specifically, I investigate whether these effects can be attributed to a positive externality of ridesharing services on the economy, which I refer to as the "gig economy effect," by examining the impact of Uber and Lyft entry on labor market outcomes. Additionally, I consider whether the ridesharing effect on crime is driven by changes in the demographic composition of the population. Using ACS data, I estimate Equation 1 with different demographic variables—such as sex, race, and education—as dependent variables to determine whether changes in these factors could explain the effects observed in the baseline analysis.

Another potential explanation for the significant reduction in crime is that Uber and Lyft may alter the preferred modes of transportation in the cities where they operate. To investigate this channel, I use ACS data on the means of transportation to commute to work to assess whether ridesharing services have influenced these modes, which could, in turn, explain the effects on crime. Beyond these mechanisms, I also discuss the potential roles of alcohol consumption, pollution levels, recreational activities, and mental health, as these factors may further illuminate the relationship between ridesharing and crime.

A. *Employment Effects*

To assess the impact of ridesharing on labor market outcomes, I estimate the following Difference-in-Differences (DID) regression model using Ordinary Least Squares (OLS) and the imputation-based approach proposed by (Borusyak, Jaravel and Spiess, 2024):

$$Labor_{c,t} = \alpha + \beta_1 Ridesharing_{c,t} + \gamma_i + \phi(t) + \epsilon_{c,t} \tag{3}$$

In this model, the dependent variables $Laborit_{ct}$ include: (1) civilian labor force in city c in month t, (2) employed population in city c in month t, (3) unemployed population in city c in month t, and (4) unemployment rate in city c in month t.

Table 3 presents the results of regressing the logarithms of the civilian labor force, employed population, unemployed population, and unemployment rate on the ridesharing treatment indicator. Columns 1 through 4 display the coefficients of the treatment effects for each of these dependent variables, respectively. Panel A shows the results using the TWFE model, Panel B uses the estimation method proposed by Borusyak, Jaravel and Spiess (2024), and Panel C reports the results using the approach by (Callaway and Sant'Anna, 2021).

Overall, the findings suggest improved labor market outcomes, as Panel C reveals a statistically significant increase in the civilian labor force and employed population, alongside a decrease in the unemployed population and the unemployment rate. Although Panels B and C show mixed results, the increase in the civilian labor force and employed population remains significant across all models, ranging from a 0.7% to 3.7% increase in the civilian labor force and a 1.6% to 3.9% increase in the employed population, respectively.

These results align with existing evidence from studies such as Barrios, Hochberg and Yi (2022); Gorback (2020); Omberg (2024), which indicate that Uber's arrival in a city is associated with a decline in the unemployment rate by between a fifth and a half of a percentage point. This suggests that Uber provides employment opportunities, particularly for those who are frictionally unemployed. Moreover, these studies suggest that

Uber serves as a complement to traditional employment rather than a substitute, offering flexible income opportunities that can help smooth transitions between jobs. Barrios, Hochberg and Yi (2022) further explores how the introduction and adoption of gig economy platforms, particularly ridesharing services like Uber and Lyft, influence new business formation in the U.S., finding that these platforms are associated with a significant increase in new business registrations. This may be because platforms like Uber and Lyft, by offering a flexible income source, allow potential entrepreneurs to maintain financial stability while launching their businesses.

The positive economic externality generated by ridesharing services like Uber and Lyft is not confined to any single sector and is challenging to attribute to specific industries. For instance, Park (2020) provides empirical evidence of the complementary relationship between Airbnb and Uber, demonstrating that together they can significantly boost local tourism employment. Uber's entry notably amplifies Airbnb's positive impact on employment in the tourism sector. During the initial rollout of Uber and Lyft, there were concerns about their potential impact on the taxicab industry. Berger, Chen and Frey (2018) suggests that while Uber has negatively affected the earnings potential of conventional taxi drivers, there is no significant impact on the labor supply of incumbent drivers. Hall and Krueger (2016) report that Uber driver-partners work fewer hours per week on average yet earn similar or slightly higher hourly wages than traditional taxi drivers, even after accounting for driving expenses. However, Tashiro and Choi (2021) finds that Uber's online platform has a negligible impact on the labor supply (time spent working) and earnings of conventional taxi drivers. This indicates that despite Uber's presence, the work patterns of traditional taxi drivers remain largely unaffected.

Consistent with expectations, cities that were early adopters of ridesharing services exhibit a more pronounced reduction in unemployment rates, which implies a likely decrease in crime rates related to financial incentives, owing to the greater economic opportunities provided by Uber and Lyft. As highlighted by Chalfin and McCrary (2017), improvements in legitimate labor market opportunities, such as increased wages and lower unemployment rates, are associated with reductions in crime. This suggests that "carrots"—in the form of better opportunities—can be more effective than "sticks"—such as harsher punishments—in curbing crime. These mechanisms are further supported by the well-documented relationship between unemployment and crime, where rising unemployment or job loss is linked to increases in violent crime (Dix-Carneiro, Soares and Ulyssea, 2018; Britto, Pinotti and Sampaio, 2022; Raphael and Winter-Ebmer, 2001), drug trafficking and violence (Dell, Feigenberg and Teshima, 2019), property crimes (Gould, Weinberg and Mustard, 2002; Lin, 2008; Raphael and Winter-Ebmer, 2001; Edmark, 2005), and other economically motivated crimes (Britto, Pinotti and Sampaio, 2022). Moreover, when cities with above-median unemployment rates are interacted with the binary ridesharing indicator, the analysis shows a significant decline in burglary, robbery, and motor vehicle theft, relative to cities with below-median unemployment rates. This finding suggests that the reduction in crime may not be solely attributed to the economic impact of ride-hailing services, indicating a more heterogeneous effect.

B. Variations in Population Composition

Next, I investigate the role of variations in population composition as a potential mechanism influencing crime rates. Higher crime rates are often observed when demographic groups with greater involvement in criminal activities constitute a larger share of the population. In Panels B and C of Table 2, I include demographic controls from ACS data to explore whether changes in age, sex, racial, and educational composition at the city level affect the baseline results. However, the inclusion of these controls does not materially alter the baseline results shown in Panel A (without controls) in terms of sign, magnitude, or statistical significance. Moreover, using the same data, I assess the heterogeneous effects of Uber and Lyft on crime based on demographic composition, uncovering noteworthy insights related to sex, education level, and foreign-born population. As anticipated, cities in the fourth quartile of female population exhibit lower crime rates relative to those in the first quartile. Similarly, cities in the fourth quartile for population with a high school degree and foreign-born population show a positive association with crime when interacted with the ridesharing service treatment indicator.

To further explore this mechanism, I estimate Equation 1 with various demographic characteristics as outcome variables, including the percentage of female population, population over 65 years old, foreign-born population, Hispanic or Latino population, and population 25 years and over with a high school degree, using the Uber/Lyft treatment indicator. Although the quartile analysis yields significant results, Table 4 suggests that ridesharing apps do not significantly influence the percentage of female population in a city, which is consistent with the observed female participation in the gig economy. Similarly, the percentage of the population with just a high school degree appears to be insignificant. Surprisingly, the analysis reveals that Uber/Lyft significantly reduces the percentage of foreign-born population at the city level. This finding might be explained by the outsourcing of many jobs in cities with a significant gig economy to developing countries, thereby reducing the need for migrant workers to relocate to the U.S. Prior research Decker, van Gemert and Pyrooz (2009); Moehling and Piehl (2009); Bell, Fasani and Machin (2013) indicates that changes in immigration can impact the level of criminal offenses. Therefore, it is feasible that demographic composition serves as a critical channel through which Uber and Lyft influence crime rates.

C. *Changed Modes of Transportation*

"Do changes in modes of transportation due to the introduction of ride-hailing services contribute to the observed decrease in city-level crime rates?" To address this question, I utilize American community survey (ACS) transportation data regarding how individuals commute from home to work. Specifically, Question 32 of the American Community Survey asks respondents: "How did this person usually get to work LAST WEEK?" with response options including Car, truck, or van; walked; drove alone; bicycle; and public transit. I calculate the percentage of workers aged 16 and over who use each of these transportation modes to commute and use these as dependent variables in Equation 1.

Table 5 presents the results of regressing five outcome variables—the percentage of people using (i) bicycles, (ii) cars/trucks/vans, (iii) driving alone, (iv) public transit, and (v) walking—on the post-ridesharing service launch treatment indicator, which equals 1 if a ridesharing service like Uber or Lyft is available for a city-year combination, and 0 otherwise. Panel A displays the results using the traditional two-way fixed effects model, while Panels B, C, and D present results from the estimation methods proposed by (Borusyak, Jaravel and Spiess, 2024), Callaway and Sant'Anna (2021), and Wooldridge (2021), respectively. The results are largely consistent in sign, magnitude, and significance across these different estimation methods. The introduction of ridesharing services does not significantly affect the use of bicycles, public transit, or walking, as evidenced by the negative but statistically insignificant coefficients for these variables. However, the introduction of Uber and Lyft shows a significant negative effect on the usage of cars/trucks/vans and driving alone to commute to work.

Hall and Krueger (2018) reports similar findings using ACS data on commute times by primary mode of transit, although their study does not use the usage of these modes as a dependent variable. Instead, they conclude that Uber complements the average transit agency, using transit ridership data from the National Transit Database (NTD). Likewise, Ward et al. (2021) Ward (2020) and Cairncross, Hall and Palsson (2022) do not find statistically significant effects of Uber and Lyft's entry on per capita transit trips and public transit ridership, respectively. Had I observed a significant decrease in bicycle usage or walking, I might have argued that the decline in these less safe modes of transportation could make riders more vulnerable to becoming crime victims. However, these results refute the possibility that changes in transportation modes are a mechanism driving the reduction in crime rates.

D. Other Mechanism Channels

The introduction of ridesharing services may prompt some drowsy or intoxicated individuals to opt for a ride instead of driving, yet this ease of access might simultaneously encourage higher alcohol consumption and other risky behaviors. The effect of alcohol consumption on crime is twofold: ridesharing services could increase alcohol consumption Burtch, Greenwood and McCullough (2021); Greenwood and Wattal (2015); Teltser, Lennon and Burgdorf (2021), which is linked to higher crime rates (Carpenter, 2005; Barron et al., 2024; Campbell et al., 2009; Gyimah-Brempong, 2001; Gyimah-Brempong and Racine, 2006; Markowitz, 2005). For instance, Barron et al. (2024) uses a nationwide alcohol sales ban in South Africa as a natural experiment and finds a significant reduction in violent crimes, including 77 fewer homicides, 790 fewer assaults, and 105 fewer reported rape cases per week during the ban period. Similarly, Campbell et al. (2009) reviews the literature and concludes that higher alcohol outlet density is generally associated with increased alcohol consumption and related harms, including violence. Conversely, Uber and Lyft offer a safer alternative for intoxicated passengers, thereby reducing the likelihood of victimization. This perspective aligns with Carpenter (2005), who, by exploiting the variation in the timing of zero-tolerance (ZT) law adoption across states, found no significant impact of ZT laws on violent crime arrests, suggesting that heavy alcohol use is more closely related to property and nuisance crimes rather than violent crimes. Carpenter also highlights that "the intensity of alcohol use—not merely participation—may be the more important determinant in causing crime." Based on the estimates, it appears that the latter mechanism—offering a safer option for intoxicated individuals—is more influential in reducing crime, or at the very least, the former mechanism has a less pronounced effect, indicating that even if ridesharing services increase alcohol consumption, it does not translate into higher overall crime rates.

The increased interaction between passengers and non-government-certified drivers might lead to more incidents of violence, as Uber drivers are not subjected to the same rigorous driver history and criminal background checks as taxicab and limousine drivers. However, such incidents constitute only a negligible proportion of total crimes. For example, only 0.0002% of U.S. Uber trips in 2020 involved a reported critical safety incident. Specifically, there were 11 reported fatal physical assaults and 998 reported sexual assaults, which included incidents of non-consensual kissing, touching, or penetration of a sexual or non-sexual body part (Uber Technologies Inc., 2024).

Ridesharing applications reduce the time passengers spend waiting on the street, especially at night, which could lower the risk of victimization. Furthermore, ridesharing services may alter the availability of potential victims by increasing the frequency with which people go out, thereby possibly raising the probability of becoming a victim. On the other hand, these services also provide safer mobility for "potential victims," thus reducing the likelihood of victimization. Henao (2017) finds that ride-sourcing significantly increases mobility and overall vehicle miles traveled (VMT) by 185%. Similarly, Leard and Xing (2020) observes an increase in total VMT, attributing it to ridesharing replacing trips that would have otherwise been made by walking, biking, or using public transit.

Ridesharing services can facilitate greater social engagement, which may contribute to improved mental health outcomes (Lennon, Saenz and Teltser, 2024). Apouey and Stabile (2019) indicates that employment within the gig economy, including roles such as Uber driving, positively impacts mental health. This beneficial effect is largely due to the flexibility, autonomy, and control that these jobs offer, which in turn enhance overall well-being.

Kim and Sarmiento (2021) utilize data from the Environmental Protection Agency (EPA) to demonstrate that the presence of Uber generally improves air quality, particularly by lowering ozone (O3) levels during the summer, which results in a reduction of the maximum yearly AQI value by 10.69 units. Uber may contribute to pollution reduction by replacing older vehicles and potentially enhancing access to public transportation hubs. Moreover, studies on crime have identified a positive correlation between air pollution and crime rates (Herrnstadt et al., 2021; Burkhardt et al., 2019; Bondy, Roth and Sager, 2020; Lu et al., 2018; Chen and Li, 2020). Consequently, it is plausible that ridesharing services affect crime rates indirectly through their impact on pollution.

Leard and Xing (2020), using data from the National Household Travel Survey (NHTS), finds that 36.87% of social and recreational trips involve ridesharing services as the primary mode of transportation. Park, Kim and Pan (2021) investigate the impact of Uber's introduction on the tourism economy in sub-Saharan African countries, concluding that while Uber enhances tourist mobility and accessibility, resulting in higher tourist satisfaction and expenditures, it does not necessarily attract more tourists to the region. However, Uber's introduction significantly increased tourist spending in regions where it operates, contributing approximately $20 million annually to the tourism economy, which equates to an average increase of $24 per tourist.

Recreational activities are recognized as a crucial factor in crime prevention. Özgen and Balcı (2015) finds that these activities play a significant role in reducing crime rates, especially among youth. They facilitate better socialization, stress relief, and the discovery of hidden talents, thereby reducing the likelihood of engaging in criminal behavior.

I do not take a definitive stance on the exact underlying mechanism; however, my results indicate that ridesharing services may influence criminal behavior and tendencies more directly and significantly through labor market dynamics, particularly employment, compared to other channels considered. On one hand, Uber and Lyft contribute to increased employment and civilian labor force participation while simultaneously reducing the unemployment rate. On the other hand, as demonstrated in Table A from the heterogeneity analysis section, cities with above-median unemployment rates witnessed a relatively greater and statistically significant decline in robbery, burglary, and motor vehicle theft—all of which are crimes related to financial motives.

Table 3—: Mechanism: Impact of Ridesharing Apps on Labor Market Outcomes

	Log of Labor Market Outcomes			
	Civilian Labor Force	Employed	Unemployed	Unemployment Rate
	(1)	(2)	(3)	(4)
Panel A: TWFE Model				
Rideshare	0.034***	0.033***	0.057***	0.022
	(0.009)	(0.009)	(0.014)	(0.012)
Observation	96,840	96,840	96,840	96,840
Panel B: Borusyak, Jaravel and Spiess (2024)				
Rideshare (τ)	0.0366***	0.0397***	-0.0090	-0.0459
	(0.0093)	(0.0088)	(0.0317)	(0.0277)
Observations	94,957	94,957	94,957	94,957
Panel c: Callaway and Sant'Anna (2021)				
Rideshare	0.007**	0.016***	-0.115***	-0.123***
	(0.003)	(0.005)	(0.029)	(0.031)
Observations	7,801	7,801	7,801	7,801
City FE	Yes	Yes	Yes	Yes
Month of Year FE	Yes	Yes	Yes	Yes

Note: This table presents the impact of ridesharing services on various labor market outcomes across the United States from 1990 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (4) show β coefficients estimated from equation 3. Panel A shows the results using the Two-way Fixed Effect Model. To check the robustness of the results, I also use model proposed by Borusyak, Jaravel and Spiess (2021) in Panel B and Callaway and Sant'Anna (2021) in Panel C. The dependent variable is the log of different types of labor market outcomes. Panel A and B shows the monthly level of data whereas Panel C shows results using the yearly data. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 4—: Mechanism: Impact of Ridesharing Apps on Demographic characteristics

	(1) Total Population	(2) Female (Percent)	(3) Highschool (Percent)	(4) Over 65 Years (Percent)	(6) Hispanic (Percent)	(7) Foreign Born (Percent)
Ridership	36419.866***	0.056	0.201	0.190	-2.467*	-45071.757*
	(9198.844)	(0.038)	(0.151)	(0.100)	(1.035)	(21893.360)
N	3564	3075	3075	3075	2893	3075

Note: This table presents the impact of ridesharing services on various types of characteristics across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (5) show β coefficients estimated from equation 3, using the following models: Two-way Fixed Effect Model, model proposed by (Borusyak, Jaravel and Spiess, 2024; Callaway and Sant'Anna, 2021; Wooldridge, 2021). The dependent variable is the yearly log of different types of transportation usage percentage. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 5—: Mechanism: Impact of Ridesharing Apps on Mode of Transportation

	(1) Bicycle	(2) Car\Truck\Van	(3) Drove Alone	(4) Public Transit	(5) Walked
Panel A: TWFE Model					
Rideshare	-0.044	-0.004*	-.005	0.057	0.011
	(0.045)	(0.002)	(0.003)	(0.035)	(0.029)
Observations	3,007	3,260	3,260	3,231	3,260
Panel B: Borusyak, Jaravel and Spiess (2024)					
Rideshare	-0.071	-0.016***	-0.012***	-0.190	0.043
	(0.143)	(0.004)	(0.005)	(0.119)	(0.044)
Observations	2,495	2,695	2,695	2,671	2,695
Panel C: Callaway and Sant'Anna (2021)					
Rideshare	-0.053	-0.017***	-0.016***	-0.107	0.056
	(0.083)	(0.004)	(0.004)	(0.065)	(0.063)
Observations	2,392	2,627	2,627	2,603	2,627
Panel D: Wooldridge (2021)					
Rideshare	0.030	-0.018***	-0.012***	-0.178	0.019
	(0.125)	(0.004)	(0.004)	(0.104)	(0.038)
Observations	2,739	2,965	2,965	2,939	2,965
City FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Note: This table presents the impact of ridesharing services on various types of mode of transportation across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (5) show β coefficients estimated from equation 3, using the following models: Two-way Fixed Effect Model, model proposed by (Borusyak, Jaravel and Spiess, 2024; Callaway and Sant'Anna, 2021; Wooldridge, 2021). The dependent variable is the yearly log of different types of transportation usage percentage. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

VII. Robustness checks

Baseline estimates consistently indicate that the introduction of ridesharing services has led to a significant reduction in violent crime, including murder, robbery, burglary, assault, and property crime. To strengthen the robustness of these findings, this section presents a series of additional checks. These robustness checks encompass a range of alternative estimation techniques, different definitions of the dependent variables, the use of alternative crime data from incident-based NIBRS, and the inclusion of Google Trends data as a proxy for Uber's market penetration, instead of relying solely on Uber's entry date as a binary treatment. Furthermore, I conduct placebo tests both in-time and in-space, as well as falsification tests where the outcome crime variable should theoretically remain unaffected by Uber's presence. Lastly, I include a county- and state-level analysis to ensure the consistency of the results.

A potential concern is that the baseline estimates may be biased if Uber and Lyft's effects on crime vary across cities or evolve over time, as highlighted in recent literature on two-way fixed effect (TWFE) estimators (Borusyak, Jaravel and Spiess, 2024; de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). These studies suggest that, in cases with heterogeneous treatment effects, models estimated using OLS may introduce bias and potentially lead to incorrect conclusions due to negative weights being assigned to some individual treatment effects. To address this concern, I conducted additional robustness checks by replicating the main empirical results using three alternative methods: (Borusyak, Jaravel and Spiess, 2024), (Callaway and Sant'Anna, 2021), and (Wooldridge, 2021). These methods consistently estimate a staggered difference-in-differences (DID) model that allows for heterogeneous treatment effects across cities and over time.

Table 6 presents the estimates using these alternative methods and compares them with the baseline OLS estimates. Panel A displays the coefficients from the OLS TWFE model, followed by results using the estimators proposed by Borusyak, Jaravel and Spiess (2024) in Panel B, Callaway and Sant'Anna (2021) in Panel C, and Wooldridge (2021) in Panel D. The results show that most alternative estimates maintain the same sign and are similar in magnitude to the baseline OLS estimates. Specifically, the coefficients for violent crime, robbery, assault, burglary, and motor vehicle theft are consistently negative and statistically significant. Although the coefficients for rape, larceny, and arson are negative, they are not statistically significant in any of the models. Property crimes are also statistically significant and negative, except in the (Callaway and Sant'Anna, 2021) specification, possibly due to the small subsamples used in their estimation process. Overall, the main findings hold even though these new procedures tend to produce larger standard errors. Moreover, the coefficients are generally larger than those from the standard OLS estimates, suggesting that the baseline specifications may slightly underestimate the effect of Uber and Lyft, providing a lower bound for the true effects. Consequently, I argue that my main results are not biased due to treatment heterogeneity in a staggered DID design.

The key identification assumption in a difference-in-differences design, such as mine, is that crime trends in treated and not-yet-treated cities would have followed parallel paths in the absence of ridesharing services. Figures A8, A9, and A8 tests this parallel pre-trends assumption for the outcome crime variables, presenting dynamic estimates derived from the alternative methods in Table 6 alongside the baseline OLS estimates. This figure shows the coefficients that capture the differences between the treatment and control groups at each year relative to the launch of ridesharing services. The results indicate that, in most specifications, the differences between the treatment and control groups do not exhibit diverging trends before the introduction of Uber and Lyft. Therefore, the parallel pre-trends assumption is satisfied in most cases. Additionally, the figure shows limited anticipation effects of the Uber and Lyft launch on various types of crime.

To further empirically validate my identification strategy, I conduct in-time placebo tests where the treatment time is artificially moved forward by 1 to 10 periods, simulating an Uber or Lyft rollout that occurred one to ten years before the actual event. For instance, if Uber entered Tampa in 2014, the placebo test would adjust the launch year to 2011, assuming a forward shift of three years. If Uber or Lyft had any anticipation effects or if the treatment effect were spurious, these placebo tests would reveal statistically significant effects during these fake treatment periods. However, as shown in Figure M, the treatment effects for nearly all crime types across these fake periods are not statistically different from zero, indicating no anticipation effects or spurious correlations.[7]

Additionally, I conduct in-space placebo tests using 500 randomly selected fake treatment units. Demographic controls from the main analysis are also applied in these placebo tests. Table A presents the two-sided p-values, comparing the estimated effects from the main results to those generated from the 500 placebo treatments. This analysis interprets the probability that the absolute value of the placebo effects is greater than or equal to the absolute value of the estimated treatment effect, providing further confidence in the robustness of the original findings.

As an alternative source of variation in treatment status, I incorporate the relative number of Google searches for the keyword "Uber" per U.S. city as a measure of the intensity of Uber's market penetration. Following the approach of Hall and Krueger (2018), I downloaded the relative search index for the term "Uber" for each city, using San Francisco—the city where Uber originated—as a reference point, covering the period from 2004 to 2024. Consequently, the relative search index for each city is benchmarked against San Francisco. The search index, which ranges from 0 to 100, can reach a value of 100 for one of the two cities, but not both. In all cases, San Francisco in May 2019 received the highest index value of 100.

I re-estimate Model 1, this time replacing the binary post-treatment variable with the continuous search index as the treatment variable. Table 8 demonstrates that the results remain robust regardless of the specific definition of treatment used. With the exception of arson, larceny, and property crimes, all other types of crime show statistically significant and negative coefficients, consistent with my baseline results. Therefore, when substituting the binary treatment of whether Uber or Lyft launched in a particular city with the intensity of Google searches for "Uber" relative to San Francisco from 2004 to 2024, I observe similar effects, reinforcing the robustness of the findings.

In Equation 1, I used the Log of the Crime Index, defined as the logarithm of the total number of criminal activities per 1,000,000 people for each crime type-city-year combination. To test the robustness of this dependent variable, I employed three alternative definitions/forms of the dependent variable and re-estimated Equation 1 using these variations:

- Crime count of crime type j in city i in year t.
- Log of the total number of criminal activities of crime type j in city i in year t.
- Crime Index: the total number of criminal activities per 100,000 people for crime type j in city i in year t.

For the model estimation using the raw count of crime, I applied the Poisson Pseudo Maximum Likelihood (PPML) model. Additionally, I incorporated the Wooldridge (2021)

[7] I use the 'DIDPLACEBO' Stata package of (Chen, Qi and Yan, 2023)

model, as it is the only estimator among the newer methods that accounts for count variables. The results, presented in Table 5, show the outcomes of using the crime count in Panel A, the log of the crime count in Panel B, and the crime index in Panel C. Consistent with the baseline results using the log of the crime index, all crime types, except for rape and larceny, show a statistically significant decrease.

Using the FBI's Uniform Crime Reporting (UCR) data, I find statistically significant declines in various types of criminal activities following the introduction of Uber and Lyft. To further assess the robustness of these findings, I turn to a second dataset: the National Incident-Based Reporting System (NIBRS). For this analysis, I examine seven types of crime: murder, rape, robbery, burglary, motor vehicle theft, assault, and property crime. Although the NIBRS data are available at the city level on a monthly basis, I aggregate the crime counts to the yearly level to maintain comparability with the UCR data. Using city-level population data, I create a crime index similar to the one used in the earlier analysis.

Estimating Equation 1 with this NIBRS data yields results consistent with those presented in Table 14. In Panel A, I report the results using the count of crimes as the dependent variable with a Poisson-Pseudo Maximum Likelihood (PPML) model [cite the paper and explain the reason for using this model]. Panel B presents results using the estimator proposed by BJS (2021), and Panel C shows results using Wooldridge (2021) estimator. The PPML model indicates that all coefficients are negative, in line with the main findings; however, only burglary and assault are statistically significant. In contrast, the BJS and Wooldridge models show statistically significant declines for all crimes except property crimes.Compared to the baseline results in Table 1 and the findings in Table 6 using alternative estimation methods, the magnitudes of the coefficients vary significantly across crime types and models. These differences in magnitude may be attributed to the limited data coverage, as discussed in the data description section.

Table 6—: Robustness Checks: Impact of Ridesharing services on Crime using several estimation methods

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Panel A: TWFE Model										
Rideshare	-0.055**	-0.088**	-0.005	-0.068**	-0.063**	-0.032**	-0.081**	-0.018	-0.039	-0.021
	(0.019)	(0.027)	(0.025)	(0.022)	(0.024)	(0.012)	(0.018)	(0.012)	(0.026)	(0.036)
Observations	6655	6771	6661	6773	6768	6292	6765	6756	6767	6078
Panel B: Borusyak, Jaravel and Spiess (2024)										
Rideshare	-0.205***	-0.010	-0.021	-0.329***	-0.227***	-0.098*	-0.239***	-0.059	-0.180**	-0.057
	(0.049)	(0.054)	(0.039)	(0.080)	(0.065)	(0.040)	(0.031)	(0.050)	(0.067)	(0.067)
Observations	6223	5154	6161	6294	6338	5869	6342	6332	6328	5390
Panel C: Callaway and Sant'Anna (2021)										
Rideshare	-0.090***	0.070	-0.059	-0.177***	-0.072*	-0.045	-0.136***	-0.018	-0.112***	-0.015
	(0.032)	(0.080)	(0.043)	(0.042)	(0.041)	(0.032)	(0.018)	(0.041)	(0.036)	(0.072)
Observations	6,150	4,795	6,071	6,205	6,266	5,779	6,272	6,256	6,261	4,839
Panel D: Wooldridge (2015)										
Rideshare	-0.190***	-0.122***	-0.008	-0.272***	-0.212**	-0.086*	-0.223***	-0.045	-0.174*	0.024
	(0.048)	(0.032)	(0.038)	(0.054)	(0.064)	(0.039)	(0.030)	(0.050)	(0.067)	(0.047)
Observations	6223	6349	6227	6349	6344	5869	6342	6332	6343	5658
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents the impact of ridesharing services on various types of crimes across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the TWFE estimation method in Panel A, Borusyak, Jaravel and Spiess (2024) in Panel B, Callaway and Sant'Anna (2021) in Panel C and Wooldridge (2021) in Panel D. The dependent variable is the yearly log of crime index- number of crimes per 100k population. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 7—: Robustness Checks: Impact of Ridesharing on Different Form of Dependent Variable: Crime (count), Crime (log) and Crime Index

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Panel A: Crime (count)										
Rideshare	-0.003	-0.097***	0.011	-0.029	0.011	-0.033*	-0.047**	-0.014	-0.067*	-0.058*
	(0.039)	(0.027)	(0.027)	(0.031)	(0.048)	(0.014)	(0.016)	(0.016)	(0.033)	(0.028)
Observations	6655	6758	6661	6773	6768	6292	6765	6756	6767	6078
Panel B: Crime (log)										
Rideshare	-0.051**	-0.061	-0.018	-0.061*	-0.058*	-0.029*	-0.079***	-0.015	-0.025	-0.029
	(0.020)	(0.031)	(0.027)	(0.025)	(0.025)	(0.012)	(0.019)	(0.012)	(0.026)	(0.035)
Observations	6655	5504	6588	6715	6762	6292	6765	6756	6751	5794
Panel C: Crime Index										
Rideshare	-24.899	-0.529*	-0.840	-15.530***	-9.337	-88.398	-41.260**	-13.426	-34.446***	-0.277
	(14.360)	(0.263)	(1.271)	(3.818)	(11.342)	(45.584)	(15.745)	(34.379)	(8.432)	(0.920)
Observations	6655	6771	6661	6773	6768	6292	6765	6756	6767	6078
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents the impact of ridesharing services on various types of crimes across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the OLS estimation method. The dependent variable is the yearly count of crimes in Panel A, the yearly log of crime in Panel B, the crime index- number of crimes per 100k population in Panel C. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 8—: Robustness Checks: Impact of Ridesharing services on Crime using NIBRS data

	(1) Murder	(2) Rape	(3) Robbery	(4) Burglary	(5) Motor Vehicle Theft	(6) Assault	(7) Property Crime
Panel A: PPML							
Rideshare	-0.079	-0.036	-0.005	-0.157**	0.051	0.146*	-0.066
	(0.069)	(0.044)	(0.065)	(0.050)	(0.046)	(0.065)	(0.037)
Observations	32389	32526	10194	10254	10254	10254	10254
Panel B: Borusyak, Jaravel and Spiess (2024)							
Rideshare	0.107***	-0.086***	-0.232***	-0.224***	-.232***	-0.109*	-0.073
	(0.040)	(0.032)	(0.059)	(0.046)	(0.075)	(0.058)	(0.053)
Observations	7,332	22,796	23,615	27,699	26,572	26,669	28,134
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month of Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents the impact of ridesharing services on various types of crimes across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the PPML estimation method in Panel A, Borusyak, Jaravel and Spiess (2024) in Panel B. The dependent variable is the yearly count of crime. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 9—: Robustness Checks: Impact of Ridesharing (measured by the Google Search Intensity) on Crime

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Google Search	-0.076***	-0.102***	0.092***	-0.071**	-0.066**	-0.002	-0.062**	0.027	-0.139***	-0.032
	(0.018)	(0.020)	(0.027)	(0.022)	(0.021)	(0.018)	(0.022)	(0.019)	(0.026)	(0.046)
Observations	2384	2429	2386	2430	2429	2250	2428	2417	2427	2137

Note: This table presents the impact of ridesharing services on various types of crimes across the United States from 2005 to 2019. I use relative Google Search of the search term "Uber" as a measure of intensity of Uber penetration in different cities. The unit of analysis is city-year, considering cities with eventual ridesharing services. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the OLS estimation method. The dependent variable is the yearly log of crime index- number of crimes per 100k population. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

VIII. Heterogeneity Analysis

Using ACS data, I perform a heterogeneity analysis to assess the effects of Uber and Lyft across various demographic groups, categorized by race, gender, income, and employment. Each subgroup is divided into quartiles, which are then interacted with the binary treatment indicator (in this case, the post-launch indicator). In line with the baseline regression model, this heterogeneity analysis compares the demographic quartiles of treated cities with those of cities that have not yet been treated. The focus on these specific sources of demographic heterogeneity is driven by the potential for demographic composition in cities to change following the introduction of ridesharing services. The results are presented in three separate tables—Tables 11, 12, and 13—where each panel illustrates the heterogeneous effects of Uber and Lyft on crime for different socio-economic and demographic variables, and each column represents a distinct regression for a different crime type. The first row of each panel outlines the main effects of ridesharing apps, while the second to fourth rows detail the heterogeneous treatment effects for the second to fourth quartiles, with all results relative to the first quartile.

In Panel A of Table 11, the analysis reveals that the effects of ridesharing services on crime are more pronounced in cities within the fourth quartile of female population percentage, particularly concerning violent crime, robbery, rape, and motor vehicle theft. All the coefficients are statistically significant, negative, and closely aligned in magnitude with the main results. Conversely, the second and third quartiles do not exhibit any significant effects of Uber and Lyft on crime relative to the first quartile. These results align with existing studies that suggest an increase in the male-to-female ratio corresponds with higher crime rates (Filser et al., 2021; Edlund et al., 2013). Filser et al. (2021), utilizing longitudinal data from Swedish registers, examines the relationship between sex ratios and violence. While some theories posit that a surplus of men leads to increased violence due to heightened competition for mates, Filser et al. (2021)'s study presents mixed evidence. The positive association at the individual level supports the "more-men-more-violence" hypothesis, particularly concerning male-on-male violence. However, my findings do not differentiate between male and female offending, as South and Messner (1987) found that high sex ratios (a shortage of women) are linked to lower female criminal offending, especially in property crimes like theft. This trend is attributed to traditional gender roles that restrict women's opportunities for criminal behavior in societies with high sex ratios.

Higher educational attainment is significantly associated with reduced crime rates Lochner (2020); Fella and Gallipoli (2014). Lochner (2020) reviews the existing literature and empirical studies examining the relationship between education and crime, exploring the mechanisms through which education reduces criminal activity. Education primarily reduces crime by enhancing future employment opportunities and wages, thereby increasing the opportunity cost of engaging in criminal behavior. Additionally, education may foster greater risk aversion, patience, and social interactions, all of which contribute to lower crime rates.

In Panel B of Table 11, I report the results of a heterogeneity analysis based on the quartiles of the percentage of the population with only a high school degree. Compared to the first quartile, the third and fourth quartiles show a significant increase in crime rates across all crime types. In contrast, the second quartile shows no effect for most crime types. This suggests that cities with a higher proportion of high school degree holders (and fewer Bachelor's or Graduate degree holders) have experienced an increase in crime rates following the entry of Uber and Lyft, relative to cities in the first quartile of educational attainment. This evidence is consistent with Bell, Costa and Machin (2022), who exploits the variation in the timing of compulsory school leaving (CSL) reforms across states as a natural experiment to identify the impact of extending compulsory schooling on crime rates. Bell finds that the primary mechanism through which CSL reforms reduce crime is dynamic incapacitation, rather than merely improving educational outcomes. The crime reduction resulting from these reforms is largely due to the short-term incapacitation effect, which keeps youth in school and off the streets, and a more sustained reduction due to dynamic incapacitation, where continued schooling leads to a long-term decrease in criminal behavior.

According to Steffensmeier et al. (1989), the age distribution of crime varies across different types of offenses. Crimes such as burglary, auto theft, and vandalism tend to peak during adolescence and decline rapidly, whereas other crimes like fraud, embezzlement, and public drunkenness have later peaks and more gradual declines.

In Panel C of Table 11, I present the results of regressing different crime types on the quartiles of the percentage of the population over 65 years old, interacted with the ridesharing treatment indicator. The coefficients are largely insignificant across crime types and quartiles, with the exception of Rape, which shows a significant coefficient for cities in the fourth quartile of population over 65 years, relative to the first quartile. This finding aligns with Farrington (1986), who suggests that the peak in the age-crime curve is more indicative of variations in the prevalence of offending rather than the incidence. In other words, while more individuals tend to engage in crime during their teenage years, the frequency at which offenders commit crimes does not necessarily decrease with age. Another possible explanation for this result is that while age influences the number of crimes, it also affects the usage of ridesharing services, as these apps are particularly popular among younger, internet-savvy individuals.

I investigate whether cities with higher percentages of Hispanic and foreign-born populations are impacted differently by the introduction of Uber and Lyft. Panels A and B of Table 12 illustrate the heterogeneity of Uber and Lyft's effects on crime based on the quartiles of population percentage for Hispanic and foreign-born residents. The interaction coefficients between Hispanic population quartiles and crime types are mostly insignificant, except for rape, which is negative and statistically significant in the third and fourth quartiles. This outcome aligns with Krivo and Peterson (2000), who found that racial differences in the effects of structural conditions—such as concentrated disadvantage, community stability, racial residential segregation, and socioeconomic inequality—on homicide rates are primarily due to the varying levels of disadvantage experienced by Blacks and Whites. In cities where Blacks and Whites face similar levels of disadvantage, the effects of structural factors on homicide rates are likewise similar.

Relative to the first quartile, cities in the fourth quartile of foreign-born population percentage exhibit increased crime rates for violent crime, robbery, assault, property crime, and burglary. The third quartile also shows increases in violent crime, robbery, and assault. This rising interaction mirrors the increasing average baseline crimes across the quartiles, indicating a more substantial effect for higher quartiles. This finding contradicts Moehling and Piehl (2009), who reanalyzed historical data and concluded that immigrants were not more likely than native-born individuals to commit serious crimes. By 1930, immigrants were generally less likely to be imprisoned than natives, especially for violent offenses. One possible explanation for the discrepancy between my findings and this study may lie in the relationship between age distribution and crime, as Moehling and Piehl (2009) highlights the importance of considering age when comparing crime rates between immigrants and natives. Younger immigrants, particularly those aged 18-19, had higher imprisonment rates than their native-born counterparts, but this difference diminished and even reversed as they aged. Another possible explanation is provided by Bell, Fasani and Machin (2013), who examines the relationship between immigration and crime by analyzing two significant waves of immigration to the UK: the late 1990s/early 2000s influx of asylum seekers and the post-2004 inflow from EU accession countries. Bell, Fasani and Machin (2013)'s study suggests that differences in labor market opportunities among immigrant groups are crucial determinants of their potential impact on crime. Better labor market integration can reduce the likelihood of immigrants engaging in criminal activities. While I find evidence that Uber and Lyft improve labor market outcomes, it remains difficult to disentangle these opportunities specifically for the foreign-born population.

Panels A and B of Table 13 present the results for the quartiles of civilian labor force, civilian employment percentage, and median income. The coefficients are predominantly negative but remain insignificant across various crime types and quartiles relative to the first quartile. In addition, I explore whether the effect of ridesharing apps on crime differs between quartiles of median income, as illustrated in Panel C of Table 12. The results in Panel C suggest that cities within the fourth quartile of median income display a negative relationship with most crime types. This finding aligns with the hypothesis that higher income levels are associated with lower crime rates.

Further, I investigate whether the treatment effect exhibits heterogeneity based on the level of unemployment. I utilize unemployment rate data from the Bureau of Labor Statistics for 2010, the year Uber was established. Cities with unemployment rates above the median are identified and interacted with the Uber/Lyft launch indicator. The results, displayed in Table M, reveal that the treatment effect is negative for all crimes, with statistical significance observed only for robbery, burglary, and motor vehicle theft. This indicates that violent crimes, such as murder, rape, and assault, are not significantly impacted in cities with higher unemployment rates. These findings lend further support to the employment mechanism through which ridesharing apps influence crime rates. This result also aligns with Edmark (2005), who found a positive and statistically significant relationship between unemployment and certain property crimes, particularly burglary and car theft, with a 1% increase in the unemployment rate linked to a 0.15% increase in burglary and a 0.16% increase in car theft.

Table 10—: Heteroeneity Analysis of Above Median Unemployment: Impact of Ridesharing apps on Crime

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Post-launch	-0.005	-0.046	-0.005	0.014	-0.019	-0.019	-0.027	-0.023	0.037	-0.048
	(0.025)	(0.040)	(0.035)	(0.032)	(0.032)	(0.019)	(0.026)	(0.020)	(0.045)	(0.054)
Post-launch * Above Median Unemployment	-0.054	-0.063	0.010	-0.134***	0.007	-0.044	-0.077*	-0.015	-0.168***	0.079
	(0.031)	(0.037)	(0.046)	(0.035)	(0.040)	(0.023)	(0.032)	(0.025)	(0.045)	(0.057)
Observations	3597	3660	3599	3661	3660	3397	3655	3648	3659	3251

Note: This table presents the impact of ridesharing services on various types of crimes across the United States from 2005 to 2019. I interact the post-launch indicator with the indicator for above median unemploment rate in the year 2010- the year before the launch of ridesharing service Uber. The unit of analysis is city-year, considering cities with eventual ridesharing services. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the OLS estimation method. The dependent variable is the yearly log of crime index- number of crimes per 100k population. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 11—: Heteroeneity Analysis of Demography: Impact of Ridesharing apps on crime (Part I)

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Panel A: Quartile Analysis of Percentage of Female Population										
Post-launch	-0.019	-0.050	0.045	-0.002	-0.072	-0.052*	-0.073	-0.063*	-0.011	-0.011
	(0.032)	(0.048)	(0.053)	(0.049)	(0.037)	(0.024)	(0.041)	(0.025)	(0.046)	(0.070)
Post-launch * 2nd quartile	-0.006	-0.041	-0.069	-0.027	0.060	0.017	-0.007	0.036	-0.031	0.002
	(0.035)	(0.052)	(0.055)	(0.053)	(0.042)	(0.031)	(0.047)	(0.030)	(0.049)	(0.073)
Post-launch * 3rd quartile	-0.062	-0.060	-0.066	-0.125*	0.032	-0.011	-0.088	0.027	-0.062	-0.067
	(0.040)	(0.058)	(0.063)	(0.054)	(0.051)	(0.032)	(0.049)	(0.035)	(0.056)	(0.075)
Post-launch * 4th quartile	-0.093*	-0.020	-0.147*	-0.185***	0.024	-0.015	-0.055	0.034	-0.119*	-0.064
	(0.037)	(0.058)	(0.059)	(0.054)	(0.046)	(0.034)	(0.050)	(0.033)	(0.056)	(0.096)
Observation	3175	3229	3178	3231	3230	2996	3227	3218	3230	2871
Panel B: Quartile Analysis of Percentage of Population With a High school Degree										
Post-launch	-0.144***	-0.164*	-0.056	-0.162***	-0.128**	-0.098***	-0.178***	-0.072*	-0.210***	-0.178*
	(0.033)	(0.063)	(0.060)	(0.043)	(0.041)	(0.026)	(0.041)	(0.030)	(0.052)	(0.082)
Post-launch * 2nd quartile	0.059	0.106	0.011	0.064	0.052	0.026	0.041	0.015	0.149**	0.098
	(0.035)	(0.070)	(0.067)	(0.046)	(0.043)	(0.029)	(0.044)	(0.031)	(0.054)	(0.079)
Post-launch * 3rd quartile	0.143***	0.082	0.053	0.140**	0.143***	0.089**	0.121**	0.073*	0.219***	0.220**
	(0.033)	(0.062)	(0.064)	(0.046)	(0.040)	(0.031)	(0.044)	(0.033)	(0.054)	(0.082)
Post-launch * 4th quartile	0.148***	0.145*	0.053	0.107*	0.139**	0.062	0.124*	0.048	0.209**	0.229**
	(0.037)	(0.066)	(0.067)	(0.052)	(0.046)	(0.033)	(0.052)	(0.035)	(0.064)	(0.085)
Observations	3175	3229	3178	3231	3230	2996	3227	3218	3230	2871
Panel C: Quartile Analysis of Percentage of Population over 65 years										
Post-launch	-0.040	-0.101	0.020	-0.130*	-0.006	-0.053*	-0.114*	-0.034	-0.113	-0.078
	(0.038)	(0.072)	(0.052)	(0.057)	(0.052)	(0.027)	(0.044)	(0.028)	(0.069)	(0.089)
Post-launch * 2nd quartile	-0.020	0.025	-0.040	0.096	-0.085	0.027	0.050	-0.001	0.158*	0.048
	(0.037)	(0.093)	(0.067)	(0.060)	(0.047)	(0.027)	(0.050)	(0.029)	(0.066)	(0.103)
Post-launch * 3rd quartile	-0.010	0.010	-0.027	0.075	-0.040	0.014	-0.002	0.016	0.069	0.095
	(0.039)	(0.079)	(0.062)	(0.058)	(0.051)	(0.027)	(0.043)	(0.029)	(0.063)	(0.087)
Post-launch * 4th quartile	-0.040	0.054	-0.134*	0.018	-0.033	-0.040	-0.014	-0.036	-0.012	0.010
	(0.039)	(0.080)	(0.065)	(0.059)	(0.051)	(0.026)	(0.045)	(0.028)	(0.070)	(0.102)
Observations	3175	3229	3178	3231	3230	2996	3227	3218	3230	2871

Note: This table presents the heterogenous impact of ridesharing services on various types of crime across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the Two-way Fixed Effect Model. The dependent variable is the yearly log of different types of crime index. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 12—: Heteroeneity Analysis of Demography: Impact of Ridesharing apps on crime (Part II)

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Panel A: Quartile Analysis of Hispanic Population Percent										
Post-launch	-0.045	-0.095	0.043	-0.108*	-0.026	-0.036	-0.116**	-0.018	-0.051	-0.002
	(0.033)	(0.061)	(0.044)	(0.049)	(0.043)	(0.025)	(0.039)	(0.025)	(0.058)	(0.073)
Post-launch * 2nd quartile	-0.022	0.006	-0.064	0.050	-0.034	-0.011	0.006	-0.015	0.037	-0.045
	(0.032)	(0.065)	(0.047)	(0.047)	(0.040)	(0.026)	(0.040)	(0.025)	(0.054)	(0.073)
Post-launch * 3rd quartile	-0.024	0.046	-0.115*	0.010	-0.019	-0.024	0.023	-0.027	-0.020	0.001
	(0.032)	(0.077)	(0.052)	(0.051)	(0.042)	(0.026)	(0.041)	(0.026)	(0.054)	(0.090)
Post-launch * 4th quartile	-0.003	-0.004	-0.139*	0.069	-0.027	-0.061	-0.004	-0.073	-0.225*	-0.144
	(0.057)	(0.128)	(0.060)	(0.076)	(0.077)	(0.037)	(0.056)	(0.037)	(0.108)	(0.121)
Observations	3175	3229	3178	3231	3230	2996	3227	3218	3230	2871
Panel B: Quartile Analysis of Percentage of Foreign Born Population										
Post-launch	-0.059*	-0.095	-0.035	-0.008	-0.094*	-0.050*	-0.091**	-0.045	-0.018	-0.020
	(0.030)	(0.061)	(0.048)	(0.033)	(0.042)	(0.022)	(0.034)	(0.023)	(0.042)	(0.069)
Post-launch * 2nd quartile	0.017	-0.007	-0.024	0.021	0.015	-0.004	0.008	-0.002	-0.013	-0.052
	(0.014)	(0.030)	(0.028)	(0.018)	(0.021)	(0.011)	(0.015)	(0.010)	(0.019)	(0.040)
Post-launch * 3rd quartile	0.054***	0.047	0.022	0.063**	0.054**	0.012	0.028	0.009	0.001	-0.002
	(0.016)	(0.035)	(0.034)	(0.024)	(0.020)	(0.013)	(0.019)	(0.012)	(0.024)	(0.047)
Post-launch * 4th quartile	0.064***	0.038	0.036	0.054*	0.071**	0.032*	0.051*	0.021	0.027	0.028
	(0.018)	(0.034)	(0.038)	(0.026)	(0.025)	(0.014)	(0.021)	(0.013)	(0.028)	(0.047)
Observations	3175	3229	3178	3231	3230	2996	3227	3218	3230	2871

Note: This table presents the heterogenous impact of ridesharing services on various types of crime across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the Two-way Fixed Effect Model. The dependent variable is the yearly log of different types of crime index. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 13—: Heteroeneity Analysis of Demography: Impact of Ridesharing apps on crime (Part III)

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Panel A: Quartile Analysis of Civilian Employed Percent										
Post-launch	-0.004	-0.054	-0.055	-0.034	-0.040	-0.081*	-0.068	-0.087**	-0.067	0.028
	(0.046)	(0.065)	(0.063)	(0.061)	(0.049)	(0.032)	(0.051)	(0.033)	(0.064)	(0.083)
Post-launch * 2nd quartile	-0.058	-0.040	0.008	-0.002	-0.041	0.029	-0.028	0.038	0.101	-0.052
	(0.050)	(0.071)	(0.077)	(0.069)	(0.057)	(0.040)	(0.061)	(0.042)	(0.069)	(0.098)
Post-launch * 3rd quartile	-0.014	0.007	0.036	-0.069	0.069	0.042	-0.052	0.069	0.038	-0.096
	(0.054)	(0.068)	(0.075)	(0.067)	(0.066)	(0.040)	(0.062)	(0.040)	(0.072)	(0.095)
Post-launch * 4th quartile	-0.084	-0.045	0.079	-0.063	-0.019	0.045	-0.050	0.073	-0.053	-0.074
	(0.052)	(0.064)	(0.075)	(0.068)	(0.054)	(0.041)	(0.061)	(0.043)	(0.070)	(0.101)
Observation	3175	3229	3178	3231	3230	2996	3227	3218	3230	2871
Panel B: Quartile Analysis of Median Income										
Post-launch	-0.002	0.380*	-0.213	-0.272**	0.056	-0.001	-0.032	0.034	0.073	0.320**
	(0.052)	(0.180)	(0.167)	(0.096)	(0.052)	(0.029)	(0.064)	(0.030)	(0.057)	(0.115)
Post-launch * 2nd quartile	-0.034	-0.461*	0.173	0.189	-0.073	-0.047	-0.070	-0.055	-0.135*	-0.303**
	(0.051)	(0.194)	(0.168)	(0.100)	(0.054)	(0.034)	(0.070)	(0.033)	(0.068)	(0.116)
Post-launch * 3rd quartile	-0.055	-0.430*	0.129	0.150	-0.082	-0.063*	-0.084	-0.084**	-0.131*	-0.364**
	(0.050)	(0.183)	(0.171)	(0.097)	(0.050)	(0.030)	(0.065)	(0.031)	(0.053)	(0.113)
Post-launch * 4th quartile	-0.074	-0.490**	0.246	0.225*	-0.140***	-0.045	-0.072	-0.069*	-0.165**	-0.352***
	(0.046)	(0.183)	(0.171)	(0.097)	(0.041)	(0.030)	(0.067)	(0.032)	(0.050)	(0.105)
Observation	3175	3229	3178	3231	3230	2996	3227	3218	3230	2871
Panel C: Quartile Analysis of Civilian Labor Force										
Post-launch	-0.015	-0.053	-0.069	-0.034	-0.047	-0.089**	-0.081	-0.095**	-0.086	-0.006
	(0.044)	(0.064)	(0.061)	(0.059)	(0.047)	(0.031)	(0.050)	(0.032)	(0.063)	(0.082)
Post-launch * 2nd quartile	-0.036	-0.035	0.027	0.004	-0.026	0.047	0.003	0.054	0.137*	0.013
	(0.050)	(0.071)	(0.076)	(0.068)	(0.056)	(0.039)	(0.059)	(0.041)	(0.068)	(0.097)
Post-launch * 3rd quartile	-0.007	0.013	0.045	-0.077	0.079	0.046	-0.046	0.072	0.062	-0.062
	(0.054)	(0.066)	(0.074)	(0.065)	(0.065)	(0.039)	(0.061)	(0.039)	(0.070)	(0.092)
Post-launch * 4th quartile	-0.072	-0.054	0.101	-0.057	-0.015	0.054	-0.038	0.084*	-0.042	-0.037
	(0.052)	(0.062)	(0.074)	(0.066)	(0.053)	(0.041)	(0.060)	(0.042)	(0.069)	(0.100)
N	6476	6583	6481	6585	6581	6120	6578	6569	6579	5912
Observations	3175	3229	3178	3231	3230	2996	3227	3218	3230	2871

Note: This table presents the heterogenous impact of ridesharing services on various types of crime across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the Two-way Fixed Effect Model. The dependent variable is the yearly log of different types of crime index. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

Table 14—: Heteroeneity Analysis: Impact of Ridesharing apps on crime by crime data of 2010

	(1) Violent Crime	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) Property Crime	(7) Burglary	(8) Larceny	(9) Motor Vehicle Theft	(10) Arson
Panel A: Quartile Analysis of Various Crime Types										
Post-launch	-0.008	-0.055	-0.022	0.003	-0.077	-0.070**	-0.085**	-0.074**	-0.066	0.128*
	(0.036)	(0.037)	(0.045)	(0.037)	(0.050)	(0.022)	(0.031)	(0.023)	(0.050)	(0.051)
Post-launch * 2nd quartile	-0.048	-0.067	-0.005	-0.041	0.016	0.044	0.016	0.046	0.109*	-0.175**
	(0.043)	(0.050)	(0.054)	(0.044)	(0.057)	(0.027)	(0.040)	(0.029)	(0.055)	(0.059)
Post-launch * 3rd quartile	-0.104*	-0.086	-0.045	-0.120**	-0.011	0.039	-0.021	0.061*	0.081	-0.258***
	(0.042)	(0.045)	(0.055)	(0.039)	(0.057)	(0.027)	(0.038)	(0.027)	(0.054)	(0.060)
Post-launch * 4th quartile	-0.028	-0.010	0.095	-0.099*	0.053	0.060*	0.019	0.100***	-0.033	-0.165**
	(0.040)	(0.036)	(0.051)	(0.040)	(0.053)	(0.027)	(0.036)	(0.029)	(0.054)	(0.061)
Observations	6476	6583	6481	6585	6581	6120	6578	6569	6579	5912
Panel B: Median Analysis of Various Crime Types										
Post-launch	-0.031	-0.075*	-0.022	-0.017	-0.068*	-0.048**	-0.076**	-0.051**	-0.016	0.037
	(0.025)	(0.033)	(0.032)	(0.028)	(0.033)	(0.015)	(0.024)	(0.016)	(0.035)	(0.046)
Post-launch * Above Median	-0.036	-0.023	0.033	-0.089**	0.015	0.028	-0.008	0.059**	-0.031	-0.105*
	(0.026)	(0.031)	(0.036)	(0.027)	(0.035)	(0.018)	(0.026)	(0.019)	(0.035)	(0.043)
Observations	6476	6583	6481	6585	6581	6120	6578	6569	6579	5912

Note: This table presents the heterogenous impact of ridesharing services on various types of crime across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the Two-way Fixed Effect Model. The dependent variable is the yearly log of different types of crime index. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

IX. Conclusion

This study examines the expansion of Uber and Lyft since 2010 as a quasi-experiment to assess the impact of ridesharing services on crime rates. By analyzing crime rates per 100,000 population for 10 different types of crime and employing staggered DID models, an overall decrease in most crime types is observed. The labor market analysis suggests that ridesharing contributes to a reduction in unemployment, which may partly explain the observed decline in crime rates, even in the face of potential increases in alcohol-related activities. Furthermore, the analysis reveals that changes in transportation modes due to the Uber/Lyft rollout do not account for the decline in crimes. Similarly, demographic changes are ruled out as a potential mechanism through which Uber/Lyft affects crime rates. This may be due to the labor market mechanism dominating other potential factors, such as alcohol effects, that link decreased crime with ridesharing services. The current paper contributes to a better understanding of how app-based ridesharing services influence criminal activities and the possible mechanisms through which these effects operate.

My findings complement the existing literature that provides evidence of declining crime rates following the rollout of ridesharing services by exploring possible mechanisms, demonstrating the sensitivity of the results across multiple datasets, and employing more rigorous, newly developed staggered DID estimation methods.

Despite the extensive media attention related to the safety concerns of ridesharing services and their potential harmful effects in terms of higher crime rates, I find no evidence of increased crime rates due to the entry of Uber or Lyft. Furthermore, none of my results support the safety concerns often cited by regulatory agencies, which provides less support for the tighter regulation of ridesharing services.

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1. Appendix A

Table A1—: Balance Table 2

	Ridesharing Not Available yet		Ridesharing is Available		
	Mean	**SD**	**Mean**	**SD**	**Difference (p-value)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Murder	0.686	2.385	1.274	3.566	-0.588 *** (0.000)
Rape	4.704	6.817	6.373	9.696	-1.669 *** (0.000)
Robbery	19.803	52.577	23.742	62.415	-3.940 *** (0.000)
Burglary	91.101	162.199	85.071	158.732	6.031 ** (0.001)
Motor Vehicle Theft	44.497	117.938	62.231	134.780	-17.734 *** (0.000)
Assault	32.297	69.776	50.438	111.917	-18.141 *** (0.000)
Property	657.572	898.957	737.621	1177.649	-80.049 *** (0.000)
Population	94912.907	112991.831	148237.081	217575.900	-53324.174 *** (0.000)
Total Officers	179.413	255.573	301.184	514.074	-121.771 *** (0.000)
Total Civilians	45.068	65.443	76.480	126.912	-31.411 *** (0.000)
Observations	21261		11273		32534

Note: This table presents the impact of ridesharing services on various types of crimes across the United States from 2005 to 2019. The unit of analysis is city-year, considering cities with eventual ridesharing services. The unit of analysis is city-year, considering cities with eventual ridesharing services. Columns (1) to (10) show β coefficients estimated from equation 1, using the OLS estimation method without any covariates in Panel A, with demographic covariates in Panel B and with both demographic and gasoline price controls in Panel C. The dependent variable is the yearly log of crime index- number of crimes per 100k population. Statistical significance levels are denoted as follows: *** for $p < 0.001$, ** for $p < 0.01$, * for $p < 0.05$.

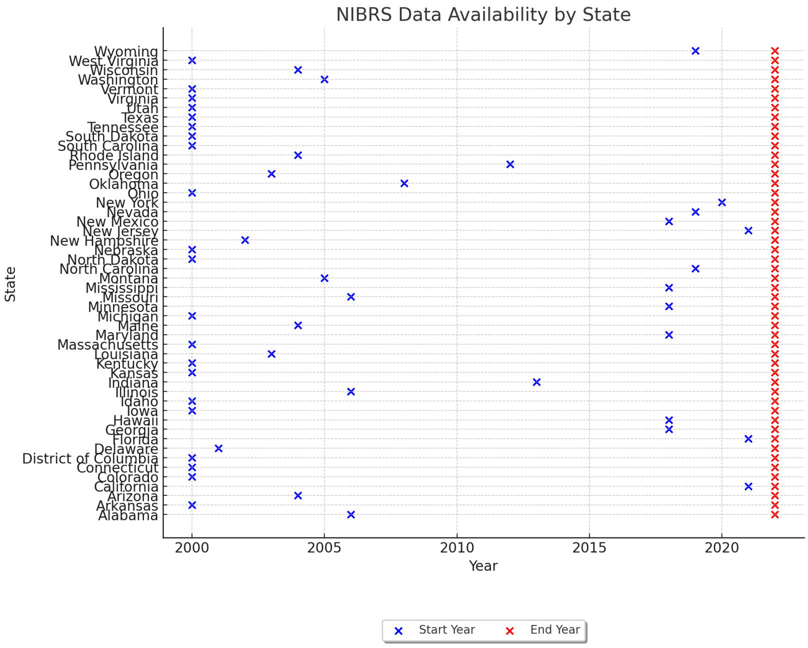
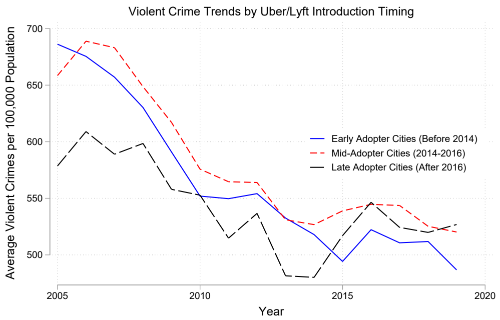
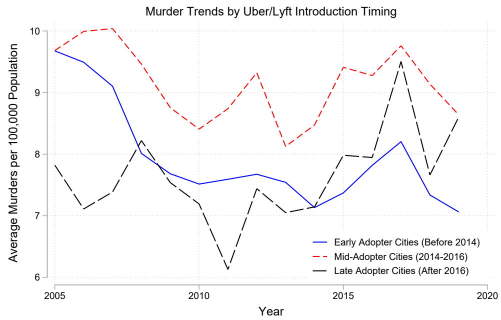


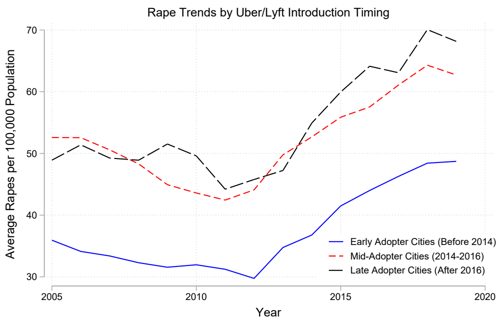
Figure A1. : Availability of NIBRS data by State



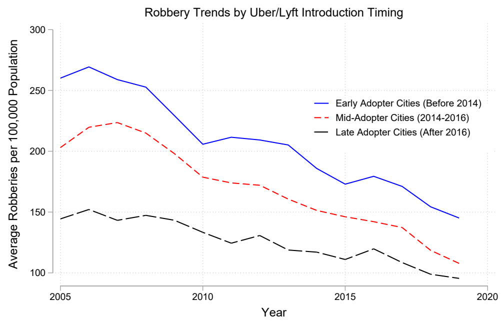
(a) Violent Crime



(b) Murders



(c) Forcible Rape



(d) Robbery

Figure A2. : Crime Trends in Violent Crime, Murders, Forcible Rapes and robbery per 100,000 Population: A City-Year Analysis. Cities are categorized as 'Early Treated' for those with Uber/Lyft entry before 2014, 'Mid-Treated' for introductions from 2014 to 2016, and 'Newly Treated' for entries after 2016.

Table A2—: UCR Crime Definitions

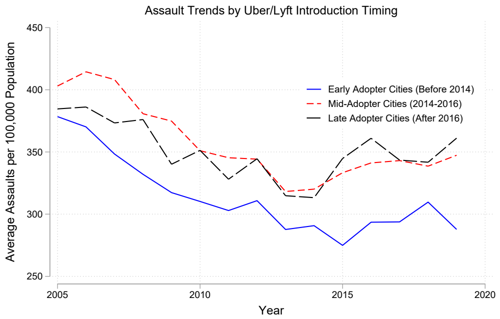
Crime Type	**Definition**
Violent Crime	Includes murder, rape, robbery, and aggravated assault, involving force or threat of force.
Murder	Willful killing of one person by another, excluding negligence, suicides, and justifiable homicides.
Rape	Non-consensual penetration of the vagina, anus, or mouth by any body part or object.
Robbery	Taking or attempting to take valuables by force, threat, or intimidation.
Assault (Aggravated)	Attack to inflict severe injury, often involving a weapon. Simple assaults are excluded.
Burglary	Unlawful entry into a structure to commit theft or felony.
Property Crime	Includes burglary, larceny-theft, motor vehicle theft, and arson. Involves the taking of money or property without force or threat of force against the victims.
Larceny-Theft	Unlawful taking of property (e.g., thefts of bicycles, shoplifting). Excludes fraud and embezzlement.
Motor Vehicle Theft	Theft or attempted theft of a motor vehicle, excluding boats, planes, and farming equipment.
Arson	Willful or malicious burning or attempt to burn property.

Note: This table presents the crime definition of Uniform Crime Report (UCR). for details definitions, see: https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/topic-pages/offense-definitions

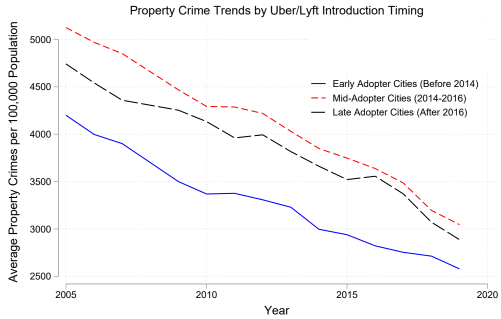
Table A3—: Robustness Checks: Impact of Ridesharing on Crime with In-space Placebo

Crime Type	Coefficient	Two-sided	Left-sided	Right-sided
Violent Crime	-0.045	0.094	0.054	0.946
Murder	-0.083	0.042	0.016	0.984
Rape	-0.011	0.762	0.392	0.608
Robbery	-0.052	0.102	0.050	0.950
Assault	-0.038	0.236	0.142	0.858
Property Crime	-0.054	0.002	0.000	1.000
Burglary	-0.099	0.000	0.000	1.000
Larceny	-0.046	0.006	0.000	1.000
Motor Vehicle Theft	-0.047	0.224	0.096	0.904
Arson	-0.029	0.606	0.314	0.686

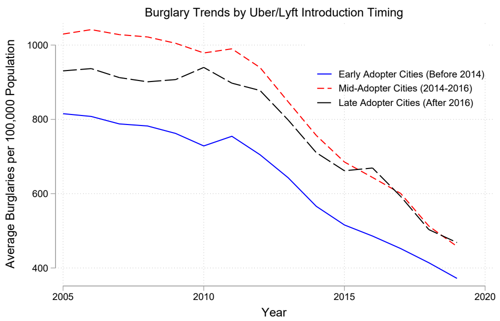
Note: This table displays two-sided p-values from in-space placebo tests involving 500 fake treatment units, comparing these to the main results.The analysis assesses the likelihood that the absolute value of the placebo effects is greater than or equal to the absolute value of the estimated treatment effect. The unit of analysis is city-year, considering cities with eventual ridesharing services. Rows (1) to (10) show the crime types and columns (1) shows β coefficients estimated from equation 1, using the OLS estimation method.



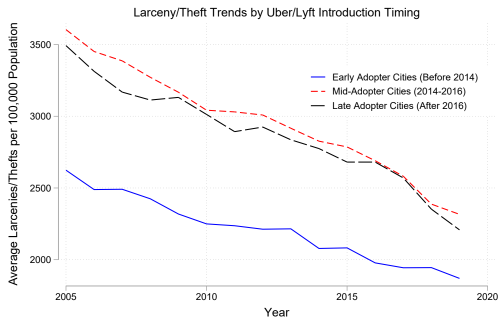
(a) Assault



(b) Property Crime

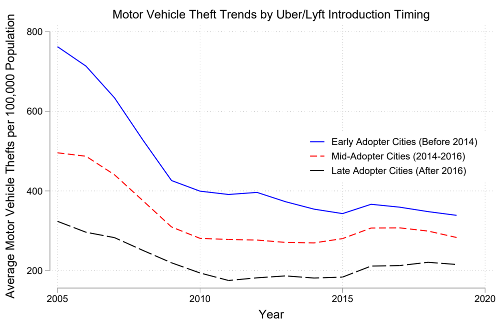


(c) Burglary

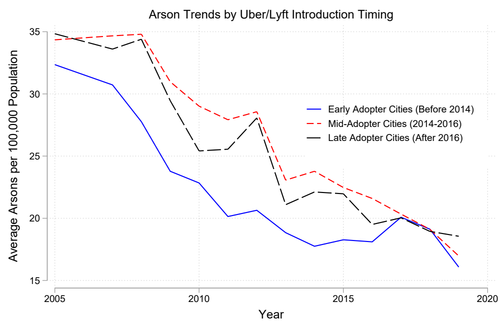


(d) Larceny

Figure A3. : Crime Trends in Assaults, Property Crimes, Burglary and Larceny per 100,000 Population: A City-Year Analysis. Cities are categorized as 'Early Treated' for those with Uber/Lyft entry before 2014, 'Mid-Treated' for introductions from 2014 to 2016, and 'Newly Treated' for entries after 2016.

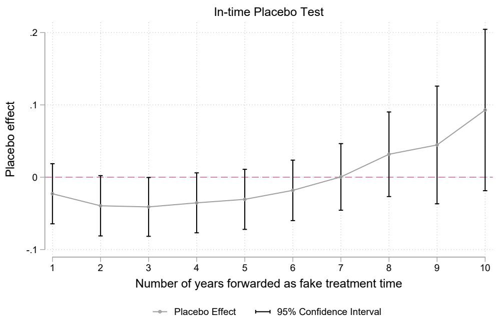


(a) Motor vehicle Theft

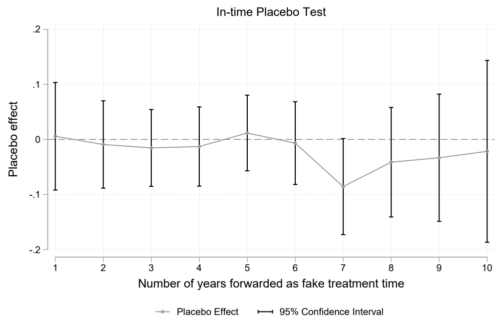


(b) Arson

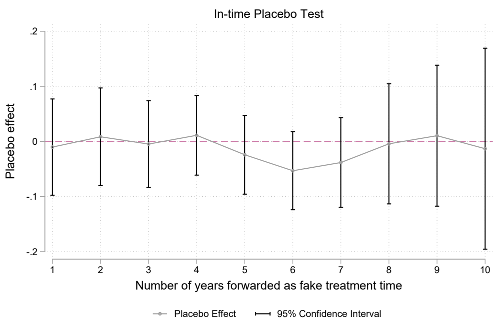
Figure A4. : Crime Trends in Motor Vehicle Theft and Arson per 100,000 Population: A City-Year Analysis. Cities are categorized as 'Early Treated' for those with Uber/Lyft entry before 2014, 'Mid-Treated' for introductions from 2014 to 2016, and 'Newly Treated' for entries after 2016.



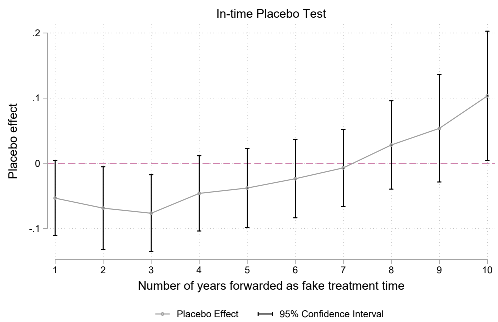
(a) Violent Crime



(b) Murders

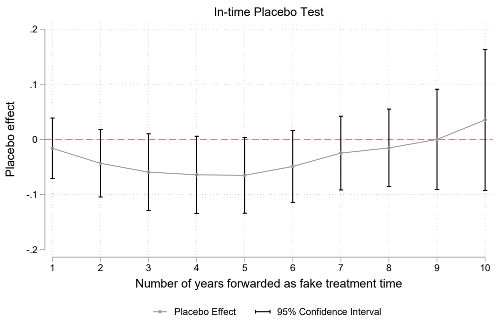


(c) Forcible Rape

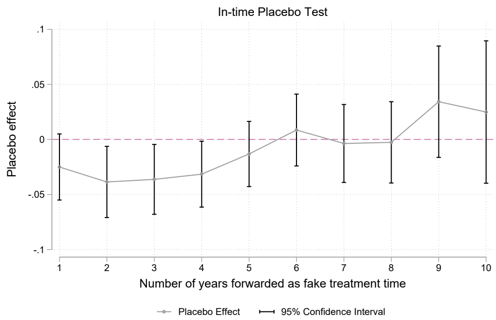


(d) Robbery

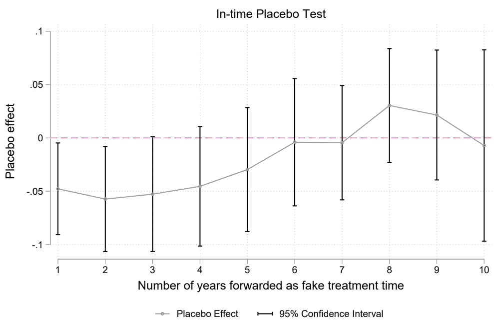
Figure A5. : The figure presents in-time placebo tests for violent crime, murders, forcible rapes and robbery per 100,000 Population in which the treatment timing is artificially advanced by 1 to 10 periods, simulating a hypothetical Uber or Lyft launch occurring one to ten years prior to the actual rollout. The bars indicate the 95-percent confidence intervals, with standard errors clustered at the city level.



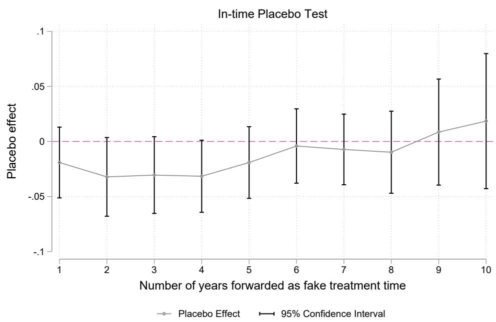
(a) Assaults



(b) Property Crimes

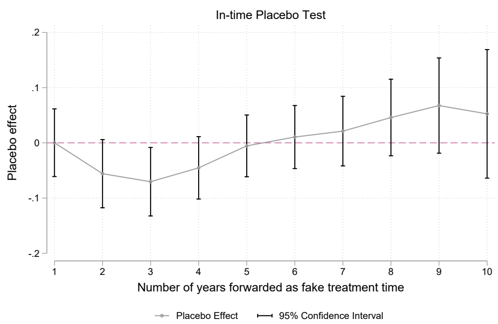


(c) Burglary

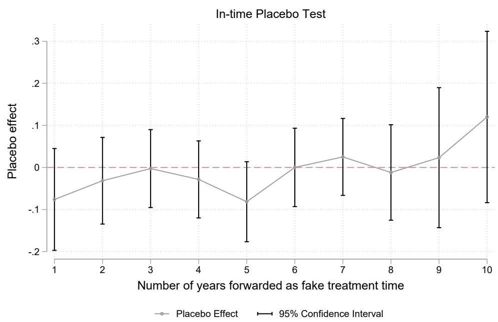


(d) Larceny

Figure A6. : The figure presents in-time placebo tests for Assaults, Property Crimes, Burglary and Larceny per 100,000 Population per 100,000 Population in which the treatment timing is artificially advanced by 1 to 10 periods, simulating a hypothetical Uber or Lyft launch occurring one to ten years prior to the actual rollout. The bars indicate the 95-percent confidence intervals, with standard errors clustered at the city level.

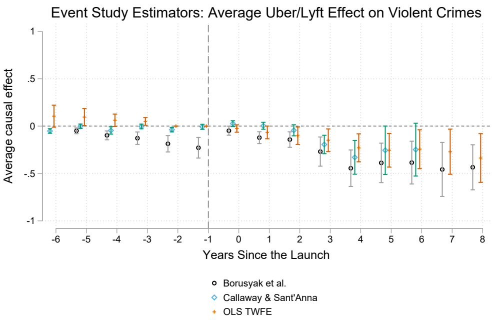


(a) Motor Vehicle Theft

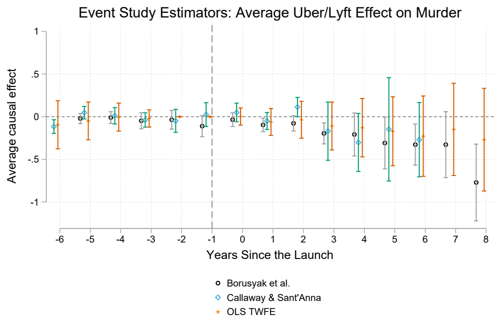


(b) Arson

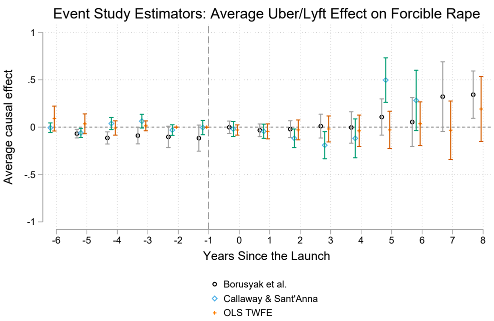
Figure A7. : The figure presents in-time placebo tests for Motor Vehicle Theft and Arson per 100,000 Population in which the treatment timing is artificially advanced by 1 to 10 periods, simulating a hypothetical Uber or Lyft launch occurring one to ten years prior to the actual rollout. The bars indicate the 95-percent confidence intervals, with standard errors clustered at the city level.



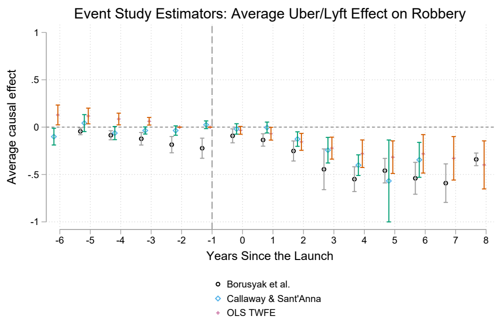
(a) Violent Crime



(b) Murders

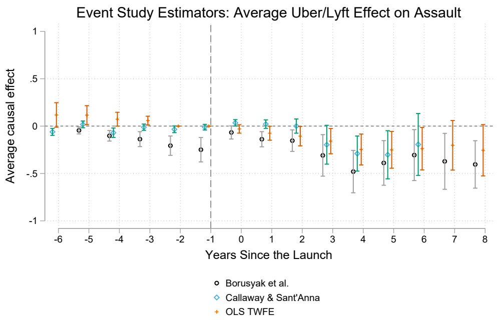


(c) Forcible Rape

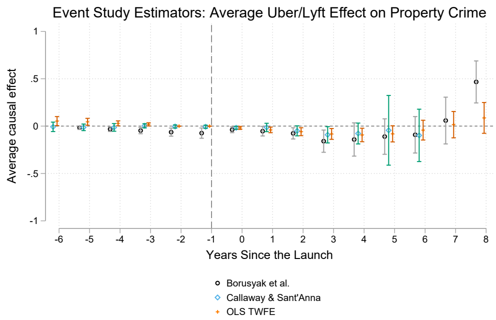


(d) Robbery

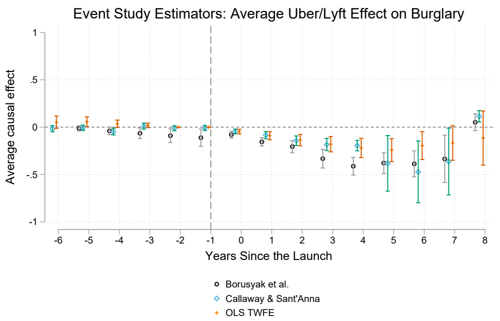
Figure A8. : Note: Event study estimates showing the effect of gradual roll out of ridesharing on the following crime types: Violent Crime, Murders, Forcible Rapes and robbery per 100,000 Population. Each dependent variable is a natural log transform of these crimes per 100,000 population.Number along the x-axis indicates years since the entry of Uber/Lyft. This is a City-Year Analysis and years included in this figure are 2005 to 2019. Point estimates come from regressions given by Eq. 2. Each panel displays event study estimates using the methodologies proposed by Borusyak, Jaravel and Spiess (2024) (shown in black), Callaway and Sant'Anna (2021) (in magenta), and two-way fixed effects models estimated through OLS (in orange). 95% confidence intervals are displayed and are calculated using robust standard errors clustered at the city level.



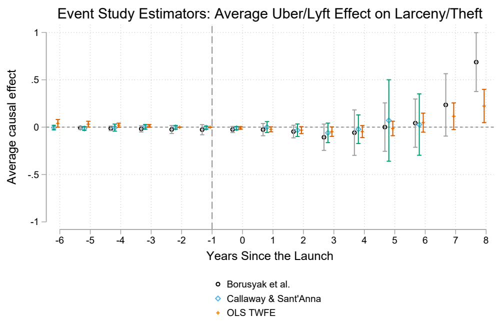
(a) Assaults



(b) Property Crimes

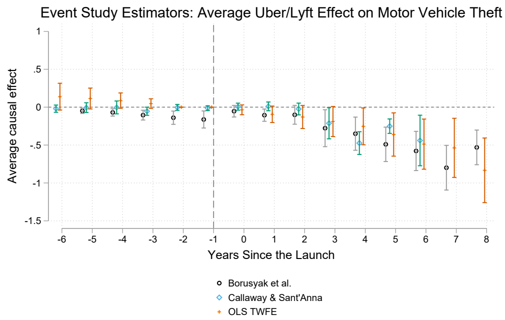


(c) Burglary

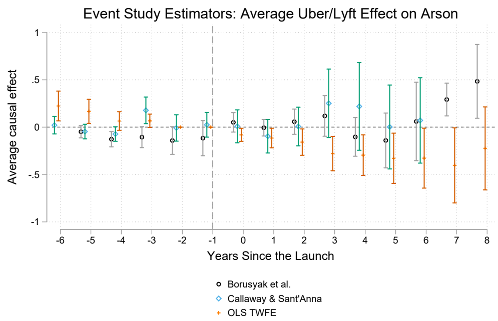


(d) Larceny

Figure A9. : Note: Event study estimates showing the effect of gradual roll out of ridesharing on the following crime types: Assaults, Property Crimes, Burglary and Larceny per 100,000 Population. Each dependent variable is a natural log transform of these crimes per 100,000 population.Number along the x-axis indicates years since the entry of Uber/Lyft. This is a City-Year Analysis and years included in this figure are 2005 to 2019. Point estimates come from regressions given by Eq. 2. Each panel displays event study estimates using the methodologies proposed by Borusyak, Jaravel and Spiess (2024) (shown in black), Callaway and Sant'Anna (2021) (in magenta), and two-way fixed effects models estimated through OLS (in orange). 95% confidence intervals are displayed and are calculated using robust standard errors clustered at the city level.



(a) Motor Vehicle Theft



(b) Arson

Figure A10. : Note: Event study estimates showing the effect of gradual roll out of ridesharing on the following crime types: Violent Crime, Murders, Forcible Rapes and robbery per 100,000 Population. Each dependent variable is a natural log transform of these crimes per 100,000 population.Number along the x-axis indicates years since the entry of Uber/Lyft. This is a City-Year Analysis and years included in this figure are 2005 to 2019. Point estimates come from regressions given by Eq. 2. Each panel displays event study estimates using the methodologies proposed by Borusyak, Jaravel and Spiess (2024) (shown in black), Callaway and Sant'Anna (2021) (in magenta), and two-way fixed effects models estimated through OLS (in orange). 95% confidence intervals are displayed and are calculated using robust standard errors clustered at the city level.