Volume 51, Number 1, March 2026

RIDE-SHARING AND PUBLIC SAFETY: AN ANALYSIS OF UBERX'S EFFECTS ON CRIME IN BRAZIL*

RAFAEL ALFENA ZAGO

Texas A&M International University, U.S.A

This paper investigates the impact of UberX's expansion on crime and traffic safety in São Paulo, Brazil. Using quarterly data from 2012 to 2021 for all 645 municipalities in the state, I exploit variation in the timing of Uber's entry and estimate dynamic treatment effects using a staggered difference-in-differences approach. The results reveal a mixed set of effects. UberX's availability is associated with statistically significant reductions in traffic-related manslaughter, personal injuries, vehicle thefts and robberies, and rape. However, traffic-related homicides increased, and no significant effects were found for certain other crime categories. These effects become more pronounced three to four years after the service becomes available. This study contributes to the literature by providing new evidence on how the availability of technology-based transportation alternatives may influence crime and public safety across a broad range of outcomes.

Keywords: Ride-Sharing; Crime; Transportation; Differences-in-Differences
JEL Classification: L91; R41; K42; O18; O33

1. INTRODUCTION

Crime remains one of the most pressing challenges in Latin America. In 2013, 42 of the 50 most violent cities in the world were in the region (Chioda, 2017). In Brazil, the region's largest country, 44% of the population cite crime as their main concern[1], 62%

* Author would like to appreciate Dr. Joan Hamory for outstanding research advice, the University of Oklahoma, Texas A&M International University, and several Uber drivers who, over the course of several conversations, contributed to this project's creation and collaborated on its development.

[1] Crime is the second most reported concern in Brazil, following unemployment and economic growth (IPSOS, 2024).

report seeing or hearing about vehicle thefts in their neighborhood, and 54% report hearing about violence against women within the last year (IPSOS, 2024). The homicide rate in 2021 reached 21.3 per 100,000 - five times the global average (UNODC, 2023). These high levels of violence undermine public trust, strain institutional capacity, divert resources from social services, and disproportionately affect women, children, and underrepresented groups (Molina Medina, 2024).

A wide range of policies have been used to deter crime. From increased police presence and dry laws to better environmental design, several strategies have shown promising results in both developed and developing settings (Levitt, 2002; McCrary, 2002; Biderman et al., 2009; Barclay et al., 1996). The availability of new transportation alternatives such as ride-sharing services can influence crime through multiple, and sometimes opposing, channels. On one hand, improved access to mobility reduces exposure to risky environments, increases the presence of potential guardians in public spaces, and offers alternatives to driving under the influence, mechanisms associated with crime deterrence and increased safety (Dills and Mulholland, 2018; Barrios et al., 2023; Jackson and Owens, 2011). For instance, ride-hailing may lower incidents of sexual assault or robbery by allowing individuals to avoid walking alone at night or waiting for public transit in isolated areas. On the other hand, platforms like Uber may also introduce new vulnerabilities: they increase the number of people and vehicles circulating at odd hours, which could expand opportunities for certain crimes such as carjackings, thefts, or violence against drivers (Greenwood and Wattal, 2017). Furthermore, by altering police patrol patterns or saturating city areas with vehicles, these services might unintentionally dilute formal surveillance. As such, the net impact of ride-sharing on crime is ultimately an empirical question that may vary across contexts, crime types, and time horizons. Studies in the United States and Europe suggest that ride-sharing may reduce traffic fatalities and DUI arrests (Dills and Mulholland, 2018; Zhou, 2020) and may also decrease other types of crimes, including robberies and assaults (Jackson and Owens, 2011; Barrios et al., 2023). However, the evidence remains mixed and is mostly limited to high-income countries. In developing contexts, where infrastructure gaps are more severe and law enforcement capacity is more constrained, the relationship between ride-sharing and crime remains largely unexplored - especially beyond traffic-related outcomes.

This paper fills this gap by studying the impact of UberX's rollout on crime and traffic safety in São Paulo, Brazil. I leverage the staggered introduction of UberX services across 645 municipalities between 2014 and 2021 to estimate dynamic treatment effects using the Callaway and Sant'Anna (2021) estimator. This approach addresses common limitations of two-way fixed effects models, allowing for heterogeneous treatment timing and flexible treatment dynamics.

The context is ideal for studying ride-sharing and public safety. São Paulo is Brazil's most populous state and its most important economic hub. Despite having lower homicide rates than many other regions, it still faces serious challenges: in 2019 alone, the state recorded over 90,000 vehicle thefts, 46,000 vehicle robberies, 3,000 rape cases, and nearly 75,000 traffic injuries (Secretaria de Segurança Pública do Estado de São Paulo, 2024). Meanwhile, Uber has achieved deep market penetration in the country. Launched in São Paulo in 2014, the platform had reached 238 cities in the state by 2021 (Uber, 2019). Brazil is now Uber's largest market outside the United States, with estimates suggesting over 5 million drivers and more than 11 billion trips since launch (Uber, 2024). This scale offers an opportunity to study how behavioral patterns change as Uber becomes a widespread transportation option.

My results reveal a complex and delayed pattern of effects. The availability of UberX is associated with statistically significant reductions in several categories of crime and traffic incidents - but these effects take time to materialize. In the quarters following entry, per-sonal injuries in traffic decline by up to 28% ($p < 0.01$), traffic manslaughter by 15% ($p < 0.06$), vehicle theft by 24% ($p < 0.01$), vehicle robbery by 27% ($p < 0.01$), and rape by 20% ($p < 0.01$). However, traffic-related homicides increase by 15.6% ($p < 0.01$). Importantly, the most consistent effects emerge 12 to 15 quarters after Uber's entry, highlighting the importance of long-term analysis and cautioning against premature evaluations based on short-run effects.

This paper makes three main contributions. First, it is the first to apply a staggered difference-in-differences estimator in the context of ride-sharing and crime, using the methodology developed by Callaway and Sant'Anna (2021). This estimator corrects for biases present in conventional two-way fixed effects models and allows for flexible treatment timing and dynamic effects - features that are crucial given the gradual adoption of Uber across cities and over time. Second, the paper expands the range of outcomes typically examined in this literature. While much of the existing work has focused narrowly on traffic fatalities or alcohol-related offenses, this study evaluates disaggregated effects on both traffic-related incidents and a variety of violent and property crimes, including homicide, rape, vehicle thefts, and robberies. Third, it provides new and policy-relevant evidence from a large middle-income country, where transportation constraints and institutional weaknesses may shape the effectiveness of interventions differently than in high-income settings.

Beyond these, the paper also documents a delayed response in crime outcomes - a finding that is both novel and important. Most prior studies assume immediate behavioral responses to ride-sharing availability, yet my findings suggest that significant changes may take several years to emerge. This temporal dimension helps explain some of the mixed evidence in previous research and cautions against drawing conclusions based on short-run analyses. Together, these contributions help clarify not only whether Uber affects crime, but how and when such effects unfold - insights that are particularly valuable for policymakers evaluating the broader societal implications of platform-based transportation.

While this paper explores the overall effect of UberX's introduction on various categories of crime, it does not directly test the mechanisms underlying these changes. Nonetheless, I discuss plausible behavioral and contextual explanations for the observed patterns, which may help guide future research. Ultimately, this research speaks to broader questions of how new technologies reshape public safety in the urban environment. Understanding how platforms like Uber affect criminal behavior is vital for policymakers who must weigh the benefits of innovation against its unintended consequences. In contexts where infrastructure is constrained and public safety is fragile, ride-sharing may represent a scalable, if imperfect, lever for social improvement.

The rest of the paper is organized as follows: Section 2 describes the literature on transportation, social outcomes, and crimes and the mechanisms behind how different means of transportation can affect social outcomes. Section 3 presents the data. Section 4 the estimation strategy; Section 5 describes the results. Sections 6 and 7 present the robustness checks and discusses the results, and Section 8 concludes.

2. TRANSPORTATION, RIDE-SHARING AND CRIME

The expansion of ride-sharing platforms has fundamentally altered urban mobility and, in turn, has influenced various social outcomes, including crime. Governments across the globe have struggled to regulate these rapidly growing services, particularly in developing countries where institutional capacity is more limited. As the dominant firm in the industry, Uber has received particular attention from researchers aiming to understand how such platforms reshape transportation systems, labor markets, and public safety.

One major area of inquiry concerns the displacement effects Uber generates across transportation modes. Evidence suggests that Uber complements rail transit but substitutes for bus usage, particularly in cities with low pre-existing transit ridership (Hall et al., 2018). It has also reduced demand for taxis without significantly affecting drivers' wages (Cramer and Krueger, 2016; Berger et al., 2018). These shifts may affect mobility patterns, travel safety, and ultimately exposure to crime. For instance, Nie (2017) find that Uber's entry reduces taxi utilization, changing the distribution of people and vehicles in urban areas at different times of day.

Transportation availability, whether through public or private means, can influence criminal activity through several mechanisms. It can deter crime by reducing the need to walk long distances, enhancing informal surveillance, and limiting isolation, especially at night. Ride-sharing services, in particular, reduce the cost of safe transportation, potentially lowering crimes such as robbery, sexual assault, or DUI-related fatalities (Dills and Mulholland, 2018; Barrios et al., 2023; Jackson and Owens, 2011). Under the rational choice framework, this reflects an increase in the opportunity cost of committing crimes or a reduction in victim vulnerability (Becker, 1968; Fajnzylber et al., 2002).

However, these same platforms may also generate conditions conducive to crime. In-creased alcohol consumption away from home facilitated by easier access to transport, for example, can raise the incidence of disorderly conduct and assault (Jackson and Owens, 2011; Markowitz and Grossman, 2000; Carpenter, 2007). The growth of ride-hailing may also saturate certain areas with vehicles, create new targets (e.g., distracted riders), and alter policing patterns. Some studies document an uptick in minor offenses or ambiguous impacts on DUI and traffic incidents in the short run (Greenwood and Wattal, 2017). In Brazil, where drivers do not need commercial licenses and inspections are lenient, Uber may introduce more unregulated and potentially unsafe drivers and vehicles onto the roads (Rogers, 2015).

The direction of impact may also depend on the type of crime. For instance, vehicle-related crimes could decline if Uber leads to fewer privately owned vehicles being parked unattended in high-theft zones. Empirical studies on car theft prevention emphasize that vehicles left in poorly lit, low-traffic areas near major roads are particularly vulnerable (Barclay et al., 1996; Clarke, 1996). If Uber substitutes for personal driving, the reduction in parked vehicles could in turn lower the incidence of vehicle theft and robbery, even as overall vehicle traffic increases (Tirachini and Gomez-Lobo, 2020).

In parallel, Uber's labor model exemplifies both the promises and perils of the gig economy. While offering income opportunities and service flexibility, it relies heavily on independent contractors lacking basic labor protections such as minimum wage guarantees, paid leave, or health benefits. Scholars have noted that gig workers often face precarious conditions, algorithmic management, and income volatility (Ravenelle, 2019; Schor et al., 2020). In developing countries like Brazil - where formal labor enforcement is weaker - these dynamics may exacerbate inequalities and concentrate risks among already marginalized groups (Dubal, 2017; Vallas and Schor, 2020). Thus, any social benefit from Uber's presence, such as reductions in crime or traffic harms, must be weighed against the broader costs imposed on the workforce that sustains the service.

In short, while platforms like Uber may improve urban safety by increasing access to transportation and reducing specific risks, they may simultaneously introduce new vulnerabilities and exacerbate labor precarity. Given the diversity of mechanisms and their potentially opposing effects, the net impact of ride-sharing on crime is ultimately an empirical question.

3. DATA

Data on when Uber entered each city where it operates in the state of São Paulo was collected on the company's website (Uber, 2019). My concentration is on UberX, as it is the company's most commonly utilized service by users and the sole option available in every city where the company operates. Uber, concerned about advertising its service for every new location, records on its website the day, month, and year that the service became available in each city nationwide. This information is regularly updated for every city in which the service is launched, providing a reliable source of information on the beginning of the company's operations at each of the different locations. In 2014, Uber entered its first city in São Paulo, and in 2021, it operated in 238 cities statewide[2].

[2] It may be the case that a city in the state is covered by the company, but its service was not advertised in Uber's blog. Thus, because a formal starting date of operation cannot be found/confirmed, this city is treated in the research as never receiving UberX.

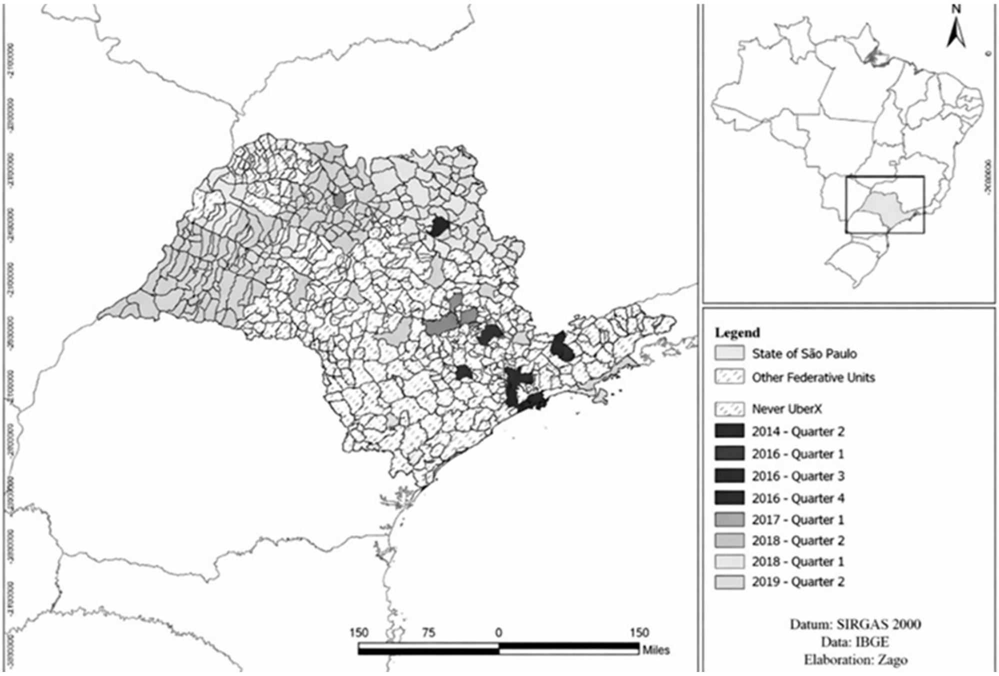

Figure 1. Uber's Entry Date by City in São Paulo State

The difference in the location of cities where Uber is eventually present and when it entered in each city of the state are depicted in Figure 1[3]. The figure shows that, although the first city to receive UberX is São Paulo, which is located in the South-East of the state, the company is more likely to cover cities in the North/North-West of the state. On the other hand, cities in these two regions tend to receive the company's services later in study period[4], with cities in West being first covered by the company.

To estimate the impacts of Uber's entrance on crime, I use data from the website of the Public Safety Secretary for the Government of São Paulo (SSP, 2017) that is recorded and publicly available at a government database (BD, 2021). The government of São Paulo state has reported the number of different offenses registered in each city monthly per year. Among other types of crimes, data is available for murder/manslaughter in traffic[5],

[3] If a city without Uber service borders a city with Uber service, it can be the destination point for rides that start in the city with service, but it cannot be the origination point for new drives.

[4] The darker the color on the map, the later the city received UberX's services.

[5] In the Brazilian legal system, traffic manslaughter refers to cases where a fatality occurs without the driver's intention to kill - typically due to negligence or recklessness - while traffic-related homicide (sometimes classified as intentional or qualified homicide) includes cases where the driver acted with disregard for the likely fatal consequences of their behavior, such as combining DUI with aggressive driving.

personal injury in traffic, rape, and vehicle theft and robbery. These categories were selected based on their theoretical connection to transportation access, public safety concerns, and data availability. In addition, the Departamento Estadual de Trânsito de São Paulo (DETRAN - SP)[6] reports monthly data on the number and type of traffic citations. For this study, I consider DUI the category described as "driving under the influence of alcohol" and aggregate all the other categories of traffic tickets to estimate the total number of tickets in a city at a given month of the year (DETRAN, 2021). To avoid the higher incidence of zeros, which could generate problems in the data distribution and for the estimation, I aggregate data by each quarter of the year. Therefore, the sample includes quarterly observations of the number of crimes and traffic-related tickets registered in all 645 cities in the state from 2012 to 2021.

Finally, annual information on the characteristics of cities, also publicly available, is extracted from Fundação Sistema Estadual de Análise de Dados (SEADE), a state government institution that collects and reports socioeconomic and demographic data for cities and regions within the state of São Paulo. Among several available variables, I follow the literature by using variables that account for the main factors that lead Uber to enter a specific city, which is the market size in terms of population and its income (Greenwood and Wattal, 2017; Dills and Mulholland, 2018; Hall et al., 2018; Barrios et al., 2023; Barreto et al., 2021). Thus, I collect data on population density (pop/km2), and GDP per capita (in R$) (SEADE, 2019). Table 1 provides summary statistics for both the characteristics of the cities in the state of São Paulo, crimes, and traffic-related tickets in 2012, two years before Uber entered the first city in the state.

Columns (1) to (3) of Table 1 present means of quarterly measures of crimes and traffic-related citations for 2012, while showing the year measures of GDP/per capita and population density. Column (4) depicts the mean differences between the cities that never had UberX and those that eventually received UberX during the study period (with standard errors in parentheses). Although cities that are eventually covered by the company's main service are significantly more populous than those that never counted with its services by 244 people per km^2 ($p < 0.01$), the same difference is not observed in terms of GDP/capita, where the mean difference is not statistically significant. In terms of crimes and traffic-related citations, cities without UberX present a significantly lower number of cases registered each quarter on average for all offenses. DUI tickets are 3 times higher ($p < 0.01$) in areas with UberX than in cities without it, while the latest counts with 743 fewer ($p < 0.01$) total tickets registered quarterly than the first. From crimes, it is also important to highlight that the number of people who are murdered in traffic in cities with UberX is almost twice ($p < 0.05$) as much as those in cities without UberX. Besides, 50 more people are injured in traffic ($p < 0.01$), about 3 more are raped ($p < 0.05$), 40 more vehicles ($p < 0.05$) are robbed and 53 are stolen ($p < 0.01$) on average in each quarter of 2012 in cities that will count with UberX at any time in the study period.

6 Similar in function to the Department of Motor Vehicles (DMV) in U.S. states.

Therefore, Table 1 suggests that, although cities that will count with Uber service at some point during the study period are more densely populated than those that are ever covered by the company, they are similar in terms of income per capita. The significantly higher number of crimes and traffic-related tickets registered may reflect the fact that areas with more people per km^2 will most likely present a higher number of reported offenses, what strengthens the importance of taking population density into account when estimating the model.

Table 1. Cities Characteristics, Crimes, and Traffic-Related Tickets, 2012

	(1) Total sample	(2) Eventually UberX	(3) Never UberX	(4) (3)-(2)
GDP/capita (in R$)	23,191	22,884	23,371	487
	(18,175)	(12,586)	(20,759)	(742)
Pop. Density (pop/km^2)	307	154	397	244***
	(1,214)	(566)	(1,458)	(49)
DUI Tickets	5.7	9.8	3.2	-6.6***
	(34)	(56)	(6)	(1.4)
Total Tickets	728	1,196	453	-743***
	(5,674)	(9,236)	(976)	(231)
Victims of Murder in Traffic	2	2.8	1.5	-1.3**
	(16)	(25)	(4.3)	(.64)
Victims of Traffic Manslaughter	1.8	2.3	1.5	-0.81***
	(6.9)	(11)	(2.6)	(.28)
Personal Injury in Traffic	56	87	37	-50***
	(288)	(462)	(77)	(12)
Total Rape	5	7.1	3.7	-3.4**
	(33)	(53)	(8.1)	(1.3)
Vehicle Robbery	34	59	19	-40**
	(442)	(717)	(89)	(18)
Vehicle Theft	42	76	22	-53***
	(434)	(705)	(82)	(18)
No. of Cities-Quarters	2,580	952	1,628	2,580
No. of Cities	645	238	407	645

Notes: Columns (1) through (3) depict the means and standard deviations (in parentheses) for crimes by quarter of 2012 and the same measures for the year 2012 for selected cities' characteristics. Column (4) shows mean differences between columns (3) and (2) with standard errors in parentheses. Superscripts *, ** and *** represent statistical significance at $p<0.1$, $p<0.05$, and $p<0.01$, respectively.

4. ESTIMATION STRATEGY

While the dataset allows for detailed and consistent analysis over a nine-year period, it is important to note that it does not capture individual-level ride-sharing behavior, such as frequency of Uber use or rider demographics. Nevertheless, the broad spatial and temporal variation in Uber's rollout enables a robust empirical assessment of its aggregate effects on public safety and crime. To estimate the effect of Uber on traffic-related tickets and different categories of crimes, I take advantage of the fact that Uber enters different cities in the state in different months/years. Thus, I exploit the variation in the start of treatment and use a staggered differences-in-differences estimation approach proposed by Callaway and Sant'Anna (2021). Because they accommodate varying treatment timings and control for pre-existing trends, these estimators reduce bias found in the canonical differences-in-differences when used in similar research designs by allowing for different baseline levels of outcomes and trends before treatment. In addition, they offer a clear framework for interpreting results by taking into account heterogeneous effects across groups according to their date of treatment, and allow for an analysis of how the treatment effects evolve after treatment rather than just a single average effect. Finally, the Callaway and Sant'Anna (2021) estimator allows me to construct group-time ATT effects by comparing each group of treated municipalities to an appropriate control group composed of never-treated or not-yet-treated units. This ensures that municipalities are only compared to valid counterfactuals at each point in time, avoiding the contamination problems common in two-way fixed effects when treatment timing varies.

In most cities, not all Uber services (such as "Uber Black" and "Uber Pool")[7] are offered, and "UberX" accounts for the highest proportion of ridership. Therefore, the focus of the estimate is on the latter. Estimates are derived from the following model[8]:

$$Y_{cyq} = \beta_0 + \beta_1 UberX_{cyq} + \beta_3 X_{cy} + \theta_c + \theta_q + \theta_y + \xi_{cym}, \tag{1}$$

where the dependent variable is $\ln(outcome + 1)$ in city c, year y, quarter q; $UberX_{cyq}$ is equal to one for quarter q in which the ride-sharing service is available in city c in year y; X_{cy} is a set of control variables that include log of population density and log of GDP/capita to allow for areas with more people per km^2 and higher GDP/capita to differ in terms of the dependent variable; θ_c, θ_q and θ_y are, respectively, city, quarter, and year fixed effects, and ξ_{cym} is the error term. To address issues with serial correlation, robust standard errors are clustered at the city level.

[7] According to the company website, "Uber Black matches riders with drivers driving luxury vehicles for a higher price", while "UberPool gives passengers the option to share a ride for a more affordable price".

[8] The described equation is the main estimation strategy used in this research. Results are consistent even after dropping the set of controls.

To assess the validity of the conditional parallel trends assumption, I estimate event-study specifications with leads and lags relative to the quarter of UberX entry. Pre-treatment coefficients are inspected to ensure that treated and control municipalities followed similar trends prior to treatment. To do so, I created a series of time dummy variables that indicate the distance from the quarter q to the quarter that UberX was implemented in the city c. Thus, by regressing the outcome variable on these dummies, the model measures not only the effect of having UberX available over time but also the pre-entry differences between cities with and without its services. Therefore, if no differences are found in the outcome of interest comparing quarters before Uber becomes available in both treated and control groups, there is no violation in the parallel pre-trend assumption and the use of the control group is valid for the use of the difference-in-difference method. Thus, the following strategy is used:

$$Y_{cyq} = \alpha_0 + \sum_{j=-12}^{24} \alpha_{1,j}\gamma_{t+j} + X_{cy}\beta_3 + \phi_c + \phi_q + \phi_y + \eta_{cyq}, \quad (2)$$

where the dependent variable is $\ln(outcome + 1)$ in city c, year y, quarter q; γ_{t+j} is a series of dummy variables that equal one for each time distance j (in quarters) from Uber entry quarter t in city c. As highlighted above, X_{cy} is a set of control variables that include the log of population density and log of GDP/capita, and ϕ_c, ϕ_q, and ϕ_y are, respectively city, quarter, and year fixed effects; and η_{cyq} is the error term. Because behavior and market penetration evolve gradually, I focus on both the average treatment effects (Simple Weighted ATT) and the dynamic effects at different horizons (Event Study ATT).

While treatment is defined based on Uber's official blog announcements, there are potential concerns regarding treatment misclassification and spillover effects. First, Uber may have started operating in certain municipalities without explicitly reporting it in public records. For instance, cities in the São Paulo metropolitan area - such as Osasco, São Bernardo, and São Caetano - were not mentioned in Uber's rollout blog, yet rides could be requested to or from these locations. Second, even when Uber does not formally operate in a given municipality, it is often possible to request a ride to or from neighboring cities where the service is available. This may result in indirect exposure to UberX even in officially untreated cities. Third, the introduction of Uber in a municipality may attract riders and drivers from nearby cities, altering mobility patterns and nightlife activity across municipal borders. These potential spillovers imply that treatment effects may be attenuated due to contamination of the control group, which suggests that the estimates should be interpreted as conservative lower bounds of the true effects.

5. RESULTS

Before turning to the regression estimates, I present a series of event studies for each crime that resulted from estimating equation (2) to check the pre-treatment parallel trends assumption. Figures 2 to 9 depict graphical representations of these estimators with 95% confidence intervals. Estimates range from $-12 \leq j \leq 24$ (respectively, 12 quarters before and 24 quarters after UberX entry in a city), and the quarter immediately before UberX entered a city is taken as the baseline of comparisons (marked with a red vertical line).

Results are shown requesting the use of not-yet-treated observations as controls, rather than never-treated observations only[9]. This singular feature of the Callaway and Sant'Anna (2021) estimator is crucial in three main aspects. First, it helps to avoid bias in estimating treatment effects, as it ensures that we are comparing treated units with those that have not yet received treatment. Secondly, by conditioning on units that have not yet been treated, the estimation process can better isolate the causal effect of the treatment, reducing the potential for contamination from later treatment effects. Finally, this option allows us to focus on the periods before treatment for units that will eventually receive it. This can help to control for any pre-treatment trends or differences that might affect the estimates.

When taking into account the results in the analysis of pre-trends, it can be noted that, before UberX enters a city, except for Total Tickets (Figure 3), there are no observable significant differences at a 95% CI in trends between treated and untreated cities at that time for any of the crimes. Thus, the parallel pre-trend assumption holds for all of the dependent variables and can be considered to be doubtful only for Total Tickets, where it can be seen that some significant differences in trends emerge about 11 quarters and immediately 2 quarters before UberX enters cities. Therefore, the differences-in-differences model can provide credible results for most crimes.

On the other hand, when we focus on the outcomes of post-UberX entry, results vary by crime. However, one common outcome is that, regardless of crime, it takes time for significant effects to be felt. About 3 quarters are needed so DUI and victims of traffic manslaughter can decrease, while it takes at least 12-15 quarters on average for victims of personal injury, vehicle thefts and robberies to be affected by the presence of UberX with a decrease in cases quarterly. Although effects vary by quarter and crime, clear downward trends can be observed for vehicle thefts and robberies, when cases registered significantly decreased after 14 and 15 quarters respectively. The observed delay in response to UberX is not a puzzle as the same phenomenon is observed in the literature. While Greenwood and Wattal (2017) finds that the effect of UberX on motor vehicle fatalities takes about 9 months (3 quarters) to stabilize, Dills and Mulholland (2018) claims that the longer Uber is present in a city, the higher its effects on traffic fatalities. Besides, Barrios et al. (2023) points out that after 4 years (16 quarters) in US cities, UberX may have increased fatalities and accidents in traffic by 15%. Finally, Barreto et al. (2021) show that fatalities and

9 The models also include the log of population density and log of GDP/capita as control variables.

hospitalizations in major Brazilian cities may take respectively 5 and 3 quarters to be significantly affected by UberX, while Hall et al. (2018) shows that UberX affects public transport ridership only 2 years (16 quarters) after its entry.

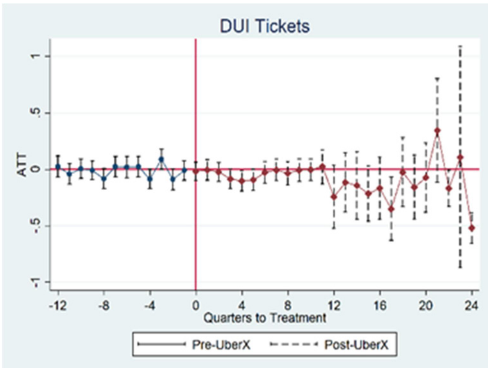

Figure 2. Effect of UberX on DUI Tickets

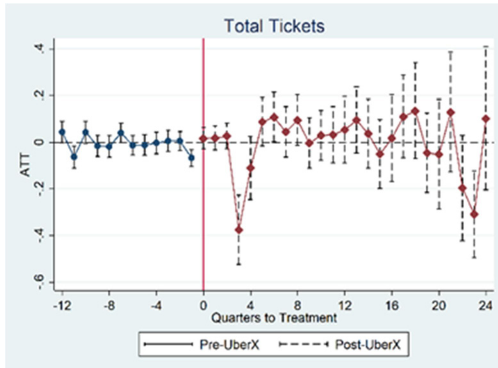

Figure 3. Effect of UberX on Total Tickets

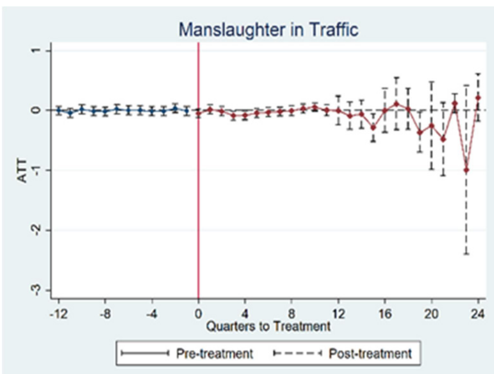

Figure 4. Effect of UberX on Traffic Manslaughter

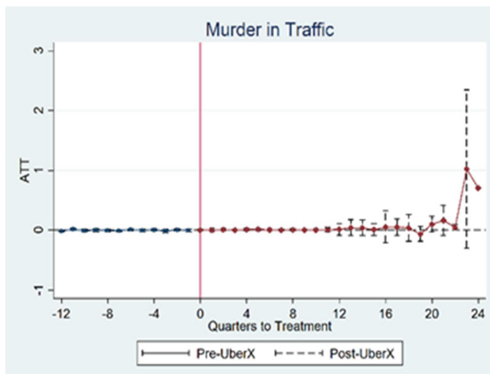

Figure 5. Effect of UberX on Murder in Traffic

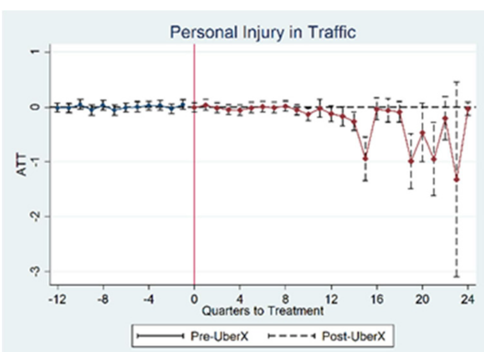

Figure 6. Effect of UberX on Personal Injury

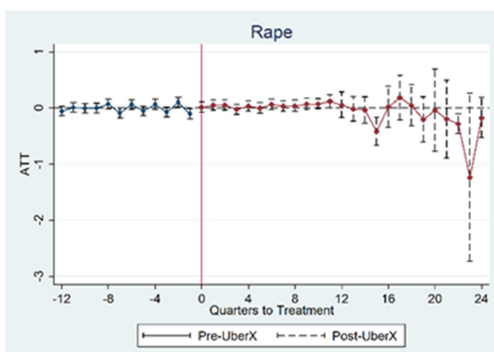

Figure 7. Effect of UberX on Rape

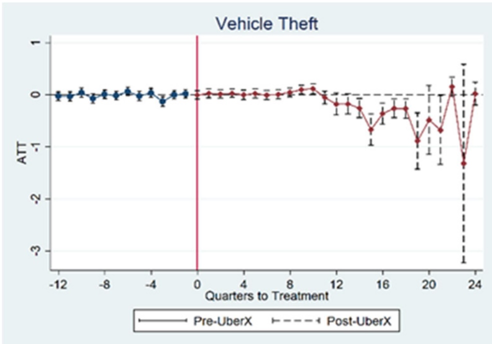

Figure 8. Effect of UberX on Vehicle Theft

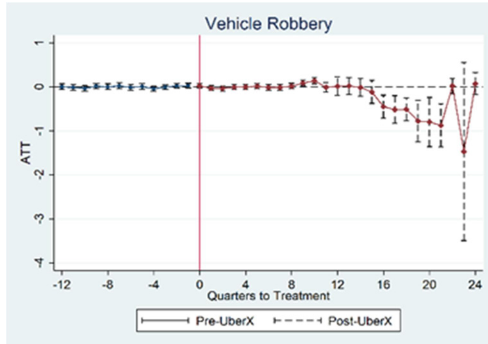

Figure 9. Effect of UberX on Vehicle Robbery

Thus, the next step is to estimate the magnitude of UberX's entry impacts on crimes. However, there are three main reasons to believe that not only the Simple Weighted ATT (Average Treatment on the Treated) but also the Event Study ATT can be informative in this scenario. First, Uber's effects on crime are unlikely to be immediate. The platform's adoption by both drivers and users typically evolves over time, and crime-related behavioral responses, such as shifts in nightlife patterns or mobility habits, may only emerge gradually. Second, the dynamics of treatment effects may differ by crime type; for example, traffic-related incidents might respond faster than interpersonal crimes or thefts. The event-study design allows us to visualize these heterogeneous dynamics across time. Third, post-treatment effects may vary considerably across different horizons, with some crimes responding only after sustained Uber penetration. Relying solely on average treatment effects could obscure these important temporal patterns. The event-study specification, therefore, provides a richer picture of how treatment effects unfold and helps identify the timing and persistence of UberX's influence on public safety.

Table 2 reports the results for the model specified in Equation (1) for the outcome variables. The first row presents the estimates of Simple Weighted ATT, while the second depicts the coefficients for Event-Study ATT. Panel A displays results based on the assumption of unconditional parallel trends, whereas Panel B presents the preferred findings, which are based on conditional parallel trend.[10]

[10] The conditional parallel trends assumption requires that, in the absence of treatment, treated and control units would have experienced the same average change in outcomes over time, conditional on observed covariates. Formally, $E[Y_t(0) - Y_{t-1}(0)|\ G = g, X] = E\ [Y_t(0) - Y_{t-1}(0)|\ G \in C, X]$, for all $t \geq g$, where $Y_t(0)$ is the potential outcome without treatment at time t; G is the period in which a unit first receives treatment; C denotes the set of never-treated or not-yet-treated groups; and X represents observed covariates. Given the observable and possible unobservable differences between control and treated groups that can lead to a failure of the parallel trend hypothesis, I treat the latter as the main specification.

Table 2. Effect of UberX on Traffic-Related Tickets and Crimes

	DUI Tickets		Total Tickets		Murder - Traffic		Manslaughter - Traffic	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Unconditional Parallel Trends								
Simple Weighted ATT	-0.017	-0.043	-0.022	-0.001	0.008	0.009	-0.043	-0.027
	(0.039)	(0.036)	(0.029)	(0.032)	(0.015)	(0.014)	(0.031)	(0.032)
Event Study ATT	-0.258***	-0.022	-0.033	0.075*	0.137***	0.145***	-0.318***	-0.165**
	(0.047)	(0.050)	(0.033)	(0.045)	(0.045)	(0.043)	(0.060)	(0.077)
Calendar ATT	0.040	0.010	-0.026	-0.004	0.499***	0.497***	-0.029	0.033
	(0.030)	(0.034)	(0.017)	(0.020)	(0.063)	(0.061)	(0.053)	(0.066)
Group-Specific ATT	0.002	-0.043	-0.022	-0.001	0.001	0.002	-0.037	-0.027
	(0.038)	(0.036)	(0.030)	(0.034)	(0.005)	(0.004)	(0.029)	(0.029)
Panel B: Conditional Parallel Trends								
Simple Weighted ATT	-0.015	-0.043	-0.022	0.000	0.008	0.009	-0.043	-0.027
	(0.039)	(0.036)	(0.029)	(0.032)	(0.015)	(0.014)	(0.032)	(0.032)
Event Study ATT	-0.257***	-0.022	-0.032	0.076*	0.137***	0.145***	-0.319***	-0.165**
	(0.047)	(0.050)	(0.033)	(0.045)	(0.045)	(0.043)	(0.060)	(0.077)
Calendar ATT	0.057*	0.015	-0.023	-0.003	0.498***	0.496***	-0.029	0.033
	(0.031)	(0.034)	(0.017)	(0.020)	(0.063)	(0.061)	(0.053)	(0.066)
Group-Specific ATT	0.003	-0.042	-0.021	-0.000	0.001	0.002	-0.037	-0.027
	(0.038)	(0.037)	(0.030)	(0.034)	(0.005)	(0.004)	(0.029)	(0.029)
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The level of analysis is the city-quarter level. Where indicated, regressions include controls for the natural log of GDP per capita and the natural log of population density. For both Panels B, doubly robust estimators are shown with standard errors in parenthesis calculated by a wild bootstrap procedure. While Panel A uses only never-treated districts in the set of control groups, Panel B uses both observations never-treated and those not yet treated in the control group. Superscripts *, ** and *** represent significance at 10%, 5% and 1%, respectively.

Table 2. Effect of UberX on Traffic-Related Tickets and Crimes (cont')

	Personal Injury - Traffic		Rape		Vehicle Theft		Vehicle Robbery	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Panel A: Unconditional Parallel Trends								
Simple Weighted ATT	-0.025	-0.051	0.062*	0.030	0.063*	-0.001	0.009	-0.006
	(0.035)	(0.034)	(0.035)	(0.037)	(0.037)	(0.033)	(0.035)	(0.027)
Event Study ATT	-0.581***	-0.329***	-0.300***	-0.221***	-0.590***	-0.273***	-0.953***	-0.314***
	(0.078)	(0.061)	(0.073)	(0.085)	(0.074)	(0.051)	(0.074)	(0.059)
Calendar ATT	-0.043**	-0.053**	0.026	-0.007	0.004	0.016	-0.086***	-0.009
	(0.021)	(0.022)	(0.039)	(0.060)	(0.023)	(0.026)	(0.032)	(0.042)
Group-Specific ATT	0.002	-0.039	0.067**	0.033	0.097***	0.018	0.050*	0.008
	(0.035)	(0.034)	(0.033)	(0.035)	(0.035)	(0.033)	(0.027)	(0.023)
Panel B: Conditional Parallel Trends								
Simple Weighted ATT	-0.026	-0.053	0.062*	0.029	0.063*	-0.001	0.009	-0.006
	(0.035)	(0.034)	(0.036)	(0.037)	(0.037)	(0.034)	(0.035)	(0.027)
Event Study ATT	-0.582***	-0.330***	-0.300***	-0.221***	-0.590***	-0.273***	-0.954***	-0.314***
	(0.077)	(0.061)	(0.074)	(0.085)	(0.073)	(0.051)	(0.073)	(0.059)
Calendar ATT	-0.044**	-0.055**	0.027	-0.010	-0.003	0.019	-0.093***	-0.007
	(0.020)	(0.022)	(0.039)	(0.061)	(0.023)	(0.026)	(0.033)	(0.042)
Group-Specific ATT	0.001	-0.041	0.067**	0.033	0.098***	0.018	0.049*	0.008
	(0.035)	(0.035)	(0.034)	(0.035)	(0.035)	(0.034)	(0.027)	(0.023)
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: The level of analysis is the city-quarter level. Where indicated, regressions include controls for the natural log of GDP per capita and the natural log of population density. For both Panels B, doubly robust estimators are shown with standard errors in parenthesis calculated by a wild bootstrap procedure. While Panel A uses only never-treated districts in the set of control groups, Panel B uses both observations never-treated and those not yet treated in the control group. Superscripts *, ** and *** represent significance at 10%, 5% and 1%, respectively.

The odd-numbered columns show regression results including only population as a control, while the even-numbered columns provide coefficients that include the full set of controls. None of the coefficients for the ATT are statistically significant after controlling for population density and GDP/per capita, and the estimates are very similar in both Panels. The lack of significant results seems to reflect the facts observed in Figures 2 to 9. Because it takes a long period until UberX affects the outcomes of interest, which creates a series of quarters with zero effects, the average effect per quarter is also not statistically different from zero. On the other hand, when taking into account the Event-Study ATT, the results are very different. While DUI is not affected by UberX, total tickets are increasing by about 8% ($p < 0.01$) per quarter after treatment. At the same time, murder in traffic increased by 15.6% ($p < 0.01$) while traffic manslaughter decreased by 15% ($p < 0.05$) and personal injury fell by 28% ($p < 0.01$). In addition, rape dropped by 20% ($p < 0.01$), vehicle thefts by 24% ($p < 0.01$), and vehicle robberies by 27% ($p < 0.01$) statewide per quarter on average after the company's entry in the market.

The third and fourth rows report the average effects across different calendar periods post-treatment and the average effect for units treated in the same period respectively. Panel B indicates that murder in traffic increases on average by about 64% ($p < 0.01$) in each quarter after UberX is available while personal injury decreases by 2.3% ($p < 0.05$), pointing to a sharp increase in fatal accidents and a decrease in cases of accidents that do not result in traffic deaths. Results are not statistically significant for units treated in the same period (fourth row).

6. ROBUSTNESS CHECKS

While the findings in the previous section are both informative and significant, they might overlook some variability in the effects of UberX. One key difference that may not be reflected in the overall ATT and the Event-Study ATTs presented in Table 2 is that the impact of UberX's entry on crime in large cities could differ from its effects in smaller cities. Because cities with higher populations tend to have different criminal behavior than cities less populated, UberX may affect criminal activity in different forms according to the size of the city. To determine whether the results shown in Table 2 are influenced by city size, I categorize the sample based on the population size of the cities throughout the study period and re-run the main regression model presented in Equation (1) separately for each group. Cities that consistently have less than 100,000 people over the 10 years are considered to be "small cities", while those with at least 100,000 are classified as "big cities"[11]. Results are presented in Table 3.

[11] Statewide, about 90% of the cities can be considered "small cities" according to this definition. Only three cities changed classifications (from being a "small city" to a "big city") during the study period (Assis, Itanhaem, and Paulinia). They are dropped from both samples.

Table 3. Effect of UberX on Crimes - by Population Size

	DUI Tickets (1)	Total Tickets (2)	Murder - Traffic (3)	Manslaughter - Traffic (4)	Personal Injury - Traffic (5)	Rape (6)	Vehicle Theft (7)	Vehicle Robbery (8)
Panel A: Cities with Population $\geq$ 100, 000								
Simple Weighted ATT	-0.059	0.014	0.078	0.124	0.034	0.017	-0.051	-0.064
	(0.093)	(0.047)	(0.080)	(0.118)	(0.074)	(0.107)	(0.066)	(0.110)
Event Study ATT	0.015	0.079	0.179***	-0.023	-0.166**	-0.180*	-0.188***	-0.271***
	(0.071)	(0.048)	(0.068)	(0.105)	(0.069)	(0.104)	(0.057)	(0.084)
Calendar ATT	-0.012	0.009	0.508***	0.117	0.010	-0.014	-0.003	-0.033
	(0.064)	(0.034)	(0.072)	(0.100)	(0.048)	(0.090)	(0.046)	(0.081)
Group-Specific ATT	-0.091	-0.000	0.054	0.123	0.061	0.015	-0.033	-0.022
	(0.083)	(0.044)	(0.033)	(0.112)	(0.067)	(0.088)	(0.065)	(0.078)
Panel B: Cities with Population < 100, 000								
Simple Weighted ATT	-0.031	0.001	0.002	-0.037	-0.030	0.043	0.046	0.019
	(0.040)	(0.037)	(0.004)	(0.030)	(0.039)	(0.038)	(0.038)	(0.025)
Event Study ATT	0.025	0.057	-0.002	-0.106**	-0.134**	0.005	0.014	0.074*
	(0.044)	(0.048)	(0.003)	(0.050)	(0.054)	(0.063)	(0.070)	(0.043)
Calendar ATT	0.046	0.003	0.002	-0.106**	-0.087	0.038	-0.006	0.033
	(0.041)	(0.035)	(0.007)	(0.042)	(0.060)	(0.054)	(0.050)	(0.047)
Group-Specific ATT	-0.036	0.000	-0.002	-0.033	-0.027	0.042	0.049	0.018
	(0.040)	(0.037)	(0.004)	(0.030)	(0.039)	(0.038)	(0.037)	(0.024)

Notes: The level of analysis is the city-quarter level. All regressions include controls for the natural log of GDP per capita and the natural log of population density. For both Panels B, doubly robust estimators are shown with standard errors in parenthesis calculated by a wild bootstrap procedure. Panels A and B use observations that were never treated and those that had not yet been treated in the control group. Superscripts $*$, $**$ and $***$ represent significance at 10%, 5% and 1%, respectively.

Table 4. Effect of UberX on Crimes - by Group of Treatment

	DUI Tickets (1)	Total Tickets (2)	Murder - Traffic (3)	Manslaughter - Traffic (4)	Personal Injury - Traffic (5)	Rape (6)	Vehicle Theft (7)	Vehicle Robbery (8)
Q2/2014	0.038	0.059	1.230***	0.033	-0.185***	-0.330**	-0.033	-0.138
	(0.059)	(0.047)	(0.021)	(0.136)	(0.048)	(0.147)	(0.070)	(0.112)
Q1/2016	-0.332***	-0.169***	0.041	0.020	-0.270***	-0.526***	0.063**	0.033
	(0.045)	(0.029)	(0.031)	(0.054)	(0.041)	(0.059)	(0.030)	(0.032)
Q3/2016	0.158	0.021	0.036*	0.145	-0.158*	0.338	-0.141***	-0.459**
	(0.142)	(0.098)	(0.019)	(0.375)	(0.090)	(0.341)	(0.042)	(0.189)
Q4/2016	-0.347*	-0.032	0.102***	-0.183***	-0.139	-0.085	-0.329***	-0.283***
	(0.201)	(0.037)	(0.026)	(0.063)	(0.127)	(0.064)	(0.103)	(0.076)
Q1/2017	-0.235*	0.088**	-0.195	0.024	-0.156	-0.094	-0.387**	-0.365**
	(0.138)	(0.044)	(0.152)	(0.131)	(0.133)	(0.096)	(0.155)	(0.179)
Q1/2018	-0.034	-0.013	0.012*	-0.143	-0.299***	-0.156	-0.202*	0.175*
	(0.114)	(0.062)	(0.007)	(0.105)	(0.065)	(0.104)	(0.106)	(0.092)
Q2/2018	0.049*	0.360***	0.023	-0.633***	0.770***	0.544***	0.215***	0.433***
	(0.027)	(0.018)	(0.014)	(0.026)	(0.024)	(0.025)	(0.027)	(0.022)
Q1/2019	-0.021	-0.027	-0.007	-0.001	0.023	0.129**	0.028	-0.027
	(0.065)	(0.049)	(0.008)	(0.050)	(0.059)	(0.055)	(0.056)	(0.044)
Q2/2019	-0.055	0.015	0.001	-0.032	-0.037	-0.012	0.077	0.050*
	(0.051)	(0.049)	(0.002)	(0.036)	(0.051)	(0.051)	(0.049)	(0.027)

Notes: The level of analysis is the city-quarter level. All regressions include controls for the natural log of GDP per capita and the natural log of population density. For both Panels B, doubly robust estimators are shown with standard errors in parenthesis calculated by a wild bootstrap procedure. All models use observations that were never treated and those that had not yet been treated in the control group. Superscripts *, ** and *** represent significance at 10%, 5% and 1%, respectively.

Table 5. Effect of UberX on Crimes - Excluding Campos do Jordão

	DUI Tickets (1)	Total Tickets (2)	Murder - Traffic (3)	Manslaughter - Traffic (4)	Personal Injury - Traffic (5)	Rape (6)	Vehicle Theft (7)	Vehicle Robbery (8)
Simple Weighted ATT	-0.043	-0.002	0.009	-0.024	-0.057*	0.027	-0.002	-0.008
	(0.036)	(0.032)	(0.015)	(0.032)	(0.034)	(0.037)	(0.034)	(0.027)
Event Study ATT	-0.024	0.074	0.145***	-0.162**	-0.333***	-0.223***	-0.274***	-0.317***
	(0.050)	(0.045)	(0.043)	(0.077)	(0.061)	(0.085)	(0.051)	(0.059)
Calendar ATT	0.015	-0.004	0.496***	0.035	-0.059***	-0.013	0.020	-0.008
	(0.034)	(0.020)	(0.061)	(0.066)	(0.021)	(0.061)	(0.026)	(0.042)
Group-Specific ATT	-0.043	-0.002	0.002	-0.025	-0.044	0.030	0.017	0.007
	(0.037)	(0.034)	(0.004)	(0.029)	(0.035)	(0.035)	(0.034)	(0.023)

Notes: The level of analysis is the city-quarter level. All regressions include controls for the natural log of GDP per capita and the natural log of population density. For both Panels B, doubly robust estimators are shown with standard errors in parenthesis calculated by a wild bootstrap procedure. All models use observations that were never treated and those that had not yet been treated in the control group. Superscripts $*$, $**$ and $***$ represent significance at 10%, 5% and 1%, respectively.

Table 6. Effect of UberX on Crimes - Excluding São Paulo

	DUI Tickets (1)	Total Tickets (2)	Murder - Traffic (3)	Manslaughter - Traffic (4)	Personal Injury - Traffic (5)	Rape (6)	Vehicle Theft (7)	Vehicle Robbery (8)
Simple Weighted ATT	-0.043	-0.001	-0.004	-0.028	-0.052	0.033	-0.000	-0.004
	(0.036)	(0.032)	(0.007)	(0.032)	(0.034)	(0.037)	(0.034)	(0.027)
Event Study ATT	-0.095*	-0.005	-0.006	-0.158**	-0.322***	-0.107	-0.277***	-0.298***
	(0.055)	(0.039)	(0.016)	(0.074)	(0.048)	(0.081)	(0.050)	(0.057)
Calendar ATT	0.001	-0.021	0.003	0.020	-0.069***	0.036	-0.005	-0.013
	(0.039)	(0.022)	(0.018)	(0.073)	(0.026)	(0.060)	(0.027)	(0.039)
Group-Specific ATT	-0.043	-0.001	-0.003	-0.028	-0.040	0.034	0.018	0.009
	(0.037)	(0.034)	(0.004)	(0.029)	(0.035)	(0.035)	(0.034)	(0.023)

Notes: The level of analysis is the city-quarter level. All regressions include controls for the natural log of GDP per capita and the natural log of population density. For both Panels B, doubly robust estimators are shown with standard errors in parenthesis calculated by a wild bootstrap procedure. All models use observations that were never treated and those that had not yet been treated in the control group. Superscripts $*$, $**$ and $***$ represent significance at 10%, 5% and 1%, respectively.

Although the coefficients for the ATT remain statistically not significant for both samples (Panels A and B), the Event-Study ATTs reveal that the effects are more significant and larger in magnitude for big cities. Columns (1) and (2) show that DUIs and Total tickets are not affected by UberX's entry into the market either in big or small cities. On the other hand, while the coefficient for Murder in traffic is negative and statistically not significant for small cities, it is positive and highly significant for larger cities, pointing to an increase of about 19.6% ($p < 0.01$) in victims. However, for traffic manslaughter, the co-efficient is negative and statistically significant for small cities, where victims are decreasing by 10% ($p < 0.05$) per quarter after UberX becomes available in the state, while victims of traffic manslaughter in big cities are unchanged. For personal injury, both groups seem to be highly affected by the availability of the company's service in the region. While cases de-crease by 12.5% ($p < 0.05$) in small cities, they fall by approximately 15% in cities with larger populations, indicating that, in percentage, results are very similar across groups. Finally, the reduction in rape found in Table 2 is led entirely by larger cities, where it decreased by about 16.5% ($p < 0.1$) per quarter, while no significant changes are observed in smaller cities. Finally, estimates for vehicle thefts and robberies point out a significant reduction in cases registered in big cities by 17% ($p < 0.01$) and almost 24% ($p < 0.01$) respectively per quarter as a result of UberX's entry. Simultaneously, vehicle thefts are unchanged in small cities, while vehicle robberies seem to be increasing by approximately 7.7% ($p < 0.01$).

Because big cities normally account for the higher registration of cases of crimes, the coefficients depicted in Table 3 provide the insight that the magnitude of the change in criminal behavior is high. In addition, the way UberX affects cases of crimes in the state is mainly driven by how it affects the outcomes in big cities. Even though offenses in small cities do respond to the entry of the company into the market, as seen especially in victims of personal injury in traffic, generally, the number of crimes remains unchanged.

A second source of heterogeneity may result from the fact that different cities receive the services of UberX in different periods. It may be the case that criminal activity in cities that receive the services earlier/later responds differently due to being first/later treated. For instance, because it may take time until crime significantly responds to UberX entry, it may be the case that cities that were treated earlier present more significant results than those treated later. To investigate this hypothesis, I report the ATT effect for groups of cities that were reported to receive the company's service at the same time. Cities are divided into 10 groups according to the quarter and year they started counting with UberX services, and the results are shown in Table 4.

As expected, cities that received UberX later (Q1/2019 and Q2/2019), were almost not affected by the company's service, except for an increase in cases of rape for those receiving UberX in Q1/2019 by almost 14% ($p < 0.05$) and a slight increase in vehicle robberies by 5% ($p < 0.1$) registered for those treated in the second quarter of 2019. Other than that, coefficients are not statistically different from zero. On the other hand, those treated earlier do show significant results. For DUI tickets, cities receiving UberX in Q1 and Q4 of 2016 and Q1 of 2017 are highly affected with significant decreases of 28% ($p < 0.01$), 29% ($p < 0.1$), and 21% ($p < 0.1$) respectively, while those that counted with the service only in Q2 of 2018 point to a slight increase in DUIs by about 5% ($p < 0.01$). For murder in traffic, results are entirely driven by São Paulo, the first city that received UberX in Q1/2014, where cases increased by about 2.5 times after UberX became available. Significant increases are also observed for cities treated in Q3/2016 (3.6%, $p < 0.1$), Q4/2016 (10.7%, $p < 0.01$) and Q1/2018 (1.2%, $p < 0.1$). At the same time, cities that registered the most significant decreases in victims of traffic manslaughter are those treated in Q4/2016 (17%, $p < 0.01$) and Q2/2018 (47%, $p < 0.01$). Finally, while the group that received UberX in Q2/2014, Q1/2016, Q3/2016, and Q1/2018 reported significant decreases in personal injury in traffic by 17% ($p < 0.01$), 23.6% ($p < 0.01$), 14.6% ($p < 0.1$), and 26% ($p < 0.01$) respectively, the group of Q2/2018 shows an increase in cases of 1.15 times approximately.

Regarding rape, cities that received UberX in Q1/2014 and Q1/2016 reduced cases by 28% ($p < 0.05$) and 51% ($p < 0.01$) respectively by quarter, while those that counted with the service only in Q2/2018 and Q1/2019 observed increases of 72% ($p < 0.01$) and 14% ($p < 0.05$) respectively in the same period. For vehicle thefts the groups of Q1/2016 and Q2/2018 reported increases in cases by 6.5% ($p < 0.05$) and 24% ($p < 0.01$), while cities treated in Q3/2016, Q4/2016, Q1/2017, Q1/2018, and Q2/2018 saw a significant decrease in cases by respectively 13% ($p < 0.01$), 28% ($p < 0.01$), 32% ($p < 0.05$), and 18% ($p < 0.1$). Finally, vehicle robberies decreased in cities receiving UberX in Q3/2016 by 37% ($p < 0.05$), in Q4/2016 by 24.6% ($p < 0.01$), in Q1/2017 by 30.6% ($p < 0.05$) and increased for the group treated in Q2/2018 by 19% ($p < 0.1$) and in Q2/2018 by 54%. Thus, it is possible to observe that the non-significant ATT effects observed in Table 2 mainly hide important heterogeneities of how the entry of UberX affects cities treated in different quarters within the study period.

Although the groups of treated cities present different responses to the company's entry according to the crime being analyzed, the group of Q2/2018 seems to be always positively affected by UberX's services. Except for traffic manslaughter, the coefficients are always positive and significant (except for murder in traffic). These results may reflect a specific phenomenon in this group that pushes crimes upward. If this is the case, this group, as an outlier, could be driving the results found in Table 2. To alleviate these concerns, I re-estimate the main model excluding this group from the sample. Results are presented in Table 5. Coefficients for both Simple Weighted ATT and Event-Study ATT are almost identical to the ones presented in Table 2. Next, I turn attention to the fact that Murder in Traffic in São Paulo (the only city to receive Uber in Q2/2014) surged after UberX. Because of that, Table 6 presents the main estimation results now excluding São Paulo from the sample. Now, although the Simple Weighted ATT remains statistically non-significant across outcome variables, the Event-Study ATT for DUI tickets shows a decrease of 9% ($p < 0.1$) in cases, and the coefficients for murder in traffic and rape turn to not statistically significant (with the magnitude for murder in traffic also close to zero). However, estimates of traffic manslaughter, personal injury, vehicle theft and vehicle robberies remain negative and statistically significant. Thus, although excluding Campos do Jordão does not alter the interpretation of the results, it is possible to see that São Paulo, the largest city of the state, drives the estimates of Murder in Traffic and Rapes. On the other hand, for all the other types of crimes, results are consistent even when excluding outliers.

7. DISCUSSION

It is important to emphasize that the goal of this analysis is not to identify the precise mechanisms through which UberX affects crime rates. The discussion that follows outlines potential pathways - such as changes in routine activities, alcohol consumption patterns, or vehicle exposure - but these remain speculative. This limitation stems primarily from the lack of detailed behavioral, mobility, or enforcement-related data at the municipal level. For instance, I do not observe ride-level data, individual usage intensity, or alcohol sales, which would be essential to rigorously test competing channels. Future research leveraging more granular administrative or proprietary data would be valuable in disentangling these mechanisms and confirming whether the observed effects arise from shifts in opportunity structures, deterrence, or other social dynamics. However, there are some plausible explanations for the findings, which I discuss below.

Thus, the findings above suggest three main important conclusions. First, it takes time for the cities that received the company's services to observe significant changes in criminal behavior. Secondly, crime in cities may respond differently to UberX according to when they receive the company's services. Finally, UberX availability reduces traffic manslaughter and personal injury in traffic, vehicle thefts, and robberies, and increases murder in traffic statewide.

The lagged response of crimes may occur for several reasons. First, it takes time for Uber to increase the supply of drivers in the city. Second, although the company advertises its services in every city it enters, it may be the case that many people do not immediately become aware of Uber's availability and, therefore, do not use the service for the first several months[12]. Third, even after learning about the new service, residents who are risk-averse might delay using the service until they have gained confidence in UberX's safety. Likewise, if transportation is a habit and is therefore

[12] Previous research highlights that, in major Brazilian capitals, Uber's popularity took 3 to 4 quarters to grow since its starting date (Barreto et al., 2021).

difficult to change, it takes time for changes in behaviors to be translated into actions. Finally, if UberX is more expensive than other available transportation options (especially public ones), the service will only be used by the part of the population that can afford it. Therefore, even though the prices of ride-sharing could be lower than those of other private means of transportation, such as taxis, what would happen during its first months of service would be merely a substitution of UberX for taxis (Nie, 2017). Consequently, the company's entry into the market may not affect traffic-related outcomes in the first months of operation[13].

One of the most puzzling findings in this paper is the simultaneous decline in traffic manslaughter and rise in traffic homicides, particularly in the capital. This pattern appears counterintuitive but may reflect underlying behavioral dynamics. Manslaughter typically involves unintentional fatalities, whereas homicides in traffic may involve aggravated or intentional risk-taking behavior. Indeed, prior research has documented that ride-sharing availability may increase alcohol consumption in general, particularly binge drinking, among users. Burgdorf et al. (2020) find that UberX entry is associated with a 3-5% rise in individuals' reported alcohol consumption and drinking days, especially among younger adults and in areas with limited public transit. Meanwhile, studies such as Brazil and Kirk (2016) show that, in some dense metro areas, Uber availability has been linked to increased alcohol-related traffic fatalities - even if overall fatalities remain flat or decline. Taken together, these findings suggest a potential mechanism: while Uber provides a safer travel alternative for many, it may also lead to increased alcohol-related risk among those who continue to drive, particularly during high-intensity drinking events. This could help explain why traffic homicides rise even as traffic manslaughter declines: Uber may divert moderate drinkers away from driving, lowering unintentional fatalities, but heavier drinkers who opt to continue driving may generate more severe or intentional incidents. Though I lack direct data on individual alcohol use, this interpretation aligns with existing behavioral evidence and indicates that ride-sharing's safety benefit may be conditional on changes in drinking behavior.

In addition, different types of crimes respond differently to UberX's entry according to differences in cities' characteristics and time of entry. First, consumers and potential drivers of cities that received UberX later in the study period may not have had enough time to adapt to the use of the ride-sharing alternative, which may explain the lack of significant results for these regions. Secondly, because the reasons why someone commits a certain type of crime differs from crime to crime, a new means of transportation may affect some offenses but not others (Ihlanfeldt, 2003; Weber, 2014, 2019; Dills and Mulholland, 2018). Finally, specific features of some cities or groups of cities may also indicate different responses to adding a new means of transportation. Characteristics such as the availability of other transportation

[13] For more information, see the framework developed by Jackson and Owens (2011) of how public transportation affects DUI rates.

options (Park et al., 2017) and level of income (Ihlanfeldt, 2003) can be important in dictating why crimes are positively or negatively affected by transportation interventions.

UberX influences various types of crime in different ways. Offering a secure and cost-effective transport option, UberX can lower the chances of alcohol consumers opting to drive (Rayle et al., 2014). UberX availability decreases search costs related to finding transportation. Because individuals evaluate the costs associated with drink and drive and of alternative transportation options, the likelihood that people will drive themselves tends to be lower after UberX. Even if the price of riding with the company increases[14], consumers are willing to pay a premium for such service (Jackson and Owens, 2011). As a result, the introduction of UberX could alter the driver demographic on the streets, as many inebriated drivers are substituted by sober ones (Dills and Mulholland, 2018). Thus, the number of DUI citations tends to decrease simply because there are fewer drunk drivers in the cities. In addition, drivers are now more prepared and less likely to engage in risky driving behaviors such as speeding, aggressive driving, running red lights and stop signs, illegal lane changes, not signaling when changing lanes, and failure to use safety equipment, among others. However, while UberX is reducing hazardous conducts in traffic, it might simultaneously be raising the probability that drivers get distracted due to phone use. This occurs because drivers must monitor ongoing and upcoming ride requests. Moreover, there could be an increase in both the number and intensity of vehicles on the roads, with more cars spending more hours operating (Dills and Mulholland, 2018; Rogers, 2015).

Additionally, results also point that UberX is decreasing cases of rape in the state. Because UberX provides services through a cell phone app, users do not have to be outdoors to request a ride and can follow the geolocation of the driver until the car arrives at the pickup point. This simple feature allows users not to be on the streets for long periods of time waiting for a ride as they would be in case of finding a taxi cab and removes potential victims from the streets especially during high-crime hours and in dangerous locations such as bus stops (Weber, 2019). Thus, ride-sharing can reduce the risk of sexual assaults by limiting the likelihood of interaction between potential offenders and victims on the streets Park et al. (2017). At the same time, Uber may approximate vulnerable people and potential offenders, as many cases of sexual assaults by Uber drivers are reported. Results indicate that, mainly in big cities, cases of rape are decreasing, which is mainly led by the estimates for the city of São Paulo.

Finally, a significant factor contributing to "hot spots" for vehicle theft is the presence of unattended vehicles in locations close to major roads, offering convenient

[14] In the United States, it was found that, without including a tip, Uber's prices are less expensive than taxi in 19 of 21 cities studied, while if including a tip to the taxi driver, Uber is cheaper in all of 21 cities (Silverstein, 2014). In Brazil, UberX fares are estimated to be 15% to 50% cheaper than taxis (Higa, 2015).

access to transit systems. (Barclay et al., 1996, *apud* Clarke et al., 1996). If people are now using UberX services more often instead of driving, a lower number of vehicles will be left unattended and parked on the streets, reducing the opportunities for vehicle theft. Moreover, while a greater number of vehicles on the streets raises the opportunity for car theft, the increased sense of surveillance, particularly during times when traffic was formerly light, might deter criminals from committing these crimes. This is because the likelihood of apprehension rises, elevating the risk and cost of engaging in such illegal activities[15]. In addition, because ride-sharing uses GPS so that drivers and users can track and even share the location of the vehicle with others, it may act as a deterrent factor for criminals. Finally, the app allows both drivers and passengers to readily reach out to law enforcement or emergency services if they feel endangered or notice any suspicious behavior, which may help deter theft and robbery.

8. CONCLUSION

This paper estimates the effect of UberX on crimes using staggered differences-in-differences by exploiting the variation in the company's date of entry in different cities of the state of São Paulo, Brazil, from 2012 to 2021. Beyond its empirical findings, the paper contributes to the literature by applying a more robust identification strategy than most prior work, using dynamic treatment effects to uncover temporal patterns that would be masked by average estimates. It also extends the evidence base to a large middle-income setting, where transportation infrastructure and law enforcement capabilities differ markedly from those in the U.S. or Europe. Although pointing to an overall decrease in crimes, results suggest that criminal activity may respond differently in cities with UberX available according to the type of crime analyzed and the time of entry.

Statewide, the introduction of UberX services decreased traffic manslaughter and per-sonal injury in traffic respectively by 15% and 28% per quarter. In addition, vehicle robberies and thefts also dropped by 27% and 24% in the state after UberX's entry into the market. Furthermore, led by São Paulo city, cases of murder in traffic increased by 15.6%, while rape fell by 20% in each quarter. Finally, results indicate that it takes about 12 quarters on average for UberX to produce significant impacts on crimes in cities where it is avail-able, and large cities are the ones most affected by the presence of the ride-hailing company. These results underscore the uneven ways that technology-enabled transportation can influence public safety and the complex

[15] The situational crime prevention theory focuses on deterring crime by changing the environment in which crime can occur. In this scenario, ride-sharing changes the surroundings as it increases the number of potential witnesses on the streets and in neighborhoods in general (Clarke, 1997; Mayhew and Clarke, 1980).

trade-offs associated with platform-based transportation. While UberX may reduce certain categories of crime by offering safer and more convenient mobility, its operation outside traditional regulatory frameworks raises concerns about driver screening, vehicle safety, and labor protections. The observed increase in traffic-related homicides, for example, may be partially linked to impaired driving behaviors among individuals not using ride-sharing services, a phenomenon that is difficult to mitigate without broader enforcement and education campaigns. In this sense, the expansion of gig economy platforms may simultaneously offer public safety benefits and expose institutional weaknesses in regulating informal labor and high-risk behaviors.

Most of the literature on the impacts of ride-hailing companies shows how the introduction of UberX changed the way consumers behave in relation to public and private means of transportation, and how traffic-related outcomes are affected. Given the results presented, there are several mechanisms by which Uber may also influence criminal behavior, and the net direction of change is not obvious. In practice, Uber seems to have changed not only the environment where crimes occur but also prevented some situations that could result in crimes. Its implementation is still recent, and its effects therefore are not entirely understood. This work aims to contribute to the growing literature on the impact of new technological services on social outcomes (Dills and Mulholland, 2018; Jackson and Owens, 2011; Greenwood and Wattal, 2017; Brazil and Kirk, 2016; Cramer and Krueger, 2016) and specifically fill the gap on how the ride-sharing industry affects crime in developing countries. While the paper does not formally test the mechanisms behind these outcomes due to data limitations, it outlines several plausible explanations that may guide future research. The policy implications are significant: ride-sharing services may offer cost-effective public safety gains in some domains, but their expansion also warrants attention to unintended consequences and regulatory gaps, particularly in urban centers.

The present work has shown that having UberX available changes criminal behavior by decreasing the situations where crime can occur, which increases the well-being of residents, creates a higher sense of safety for users and residents in general, saves lives in traffic, prevents traumatic situations, and contributes to cost reductions. In a conservative back-of-the-envelope calculation, savings associated with the decrease in traffic manslaughter may reach approximately R$ 433,300 ($76,000) per year in the state, while those related to personal injury in traffic may reach R$ 531,000 ($93,200)16 in the same period considering only victims with minor injuries and without taking into account the costs associated with repair or replacement of vehicles involved in the accident, which is expected to be from R$ 7,200 (about $1,260 when there are no victims) to almost R$ 20,000 ($6,500 when there are victims) per traffic accident (de Carvalho, 2020). In addition, regarding

[16] These estimates include medical and transportation costs, and an estimated loss of income when deaths occur and are based on the data from 2012 provided by Table 1.

vehicle robberies and thefts, given that the vehicle that is the most frequent target of criminal's costs, on average, R$ 30,000 ($5,300) (Drehmer, 2024; Auto, 2024), savings can reach approximately R$ 2,300,000 ($403,500) per year in the state, just taking the price of the car into account. In total, savings can reach at least R$ 2.5 million ($433,000) that could be redirected to other expenses such as education, health, etc. Finally, given the evidence in the literature that having the ride-sharing company available also increases alcohol consumption (Burgdorf et al., 2020), restaurants and bars may benefit from this alternative not only because UberX allows for higher consumption of alcoholic beverages by those who would drink less because they would have to drive, but also to attract those who would not drink at all.

The study faces three primary limitations. Firstly, the inability to extend the research timeframe and geographical scope is due to insufficient detailed crime data across the country. While São Paulo state offers an intriguing case study, expanding the study area would enhance the generalizability of the findings, particularly in a developing context. Secondly, besides Uber, the study does not account for other ride-sharing firms with smaller market shares that are penetrating the market, potentially impacting social outcomes such as criminal activity in the cities where they launch operations. The interplay between Uber and these emerging ride-hailing companies could present opportunities for future research. Finally, it does not formally test the mechanisms through which UberX affects crime, primarily due to data constraints, such as the absence of ride-level usage, individual behavioral data, or detailed enforcement activity at the municipal level. Future studies could explore how ride-hailing interacts with other interventions such as alcohol control laws, policing, or public transit, and investigate whether effects vary by time of day, user demographics, or driver behavior. As cities continue to integrate digital platforms into everyday life, understanding their full social impact becomes ever more important.

These findings should prompt policymakers to recognize the urgent need to adapt swiftly to the emerging sharing economy. It is essential to view this new industry not just as a transportation method but as an essential element of public policy. Policymakers should recognize that the sharing economy is not just a private-sector innovation but a powerful driver of public outcomes that requires integration into public policy frameworks. Treating ride-sharing as a tool in urban and public policy can open opportunities to achieve social goals, such as reducing crime, improving traffic safety, and enhancing public accessibility. Furthermore, effective regulation and collaboration with the private sector will be crucial to managing the economic and social effects of this industry on cities. In essence, the integration of the sharing economy into urban planning and public policy strategies should be seen as an essential step toward creating safer, more efficient, and more resilient urban environments.

REFERENCES

Auto, M. (2024), “Tabela Fipe: Preço Volkswagen Gol 2015 1.6 VHT Comfortline I-Motion (Flex) 4p,” Accessed October 2024.

Barclay, P., J. Buckley, P.J. Brantingham, P.L. Brantingham and T. Whinn-Yates (1996), “Preventing Auto Theft in Suburban Vancouver Commuter Lots: Effects of a Bike Patrol,” in *Preventing Mass Transit Crime*, Crime Prevention Studies, 6, 133-161.

Barreto, Y., R.d. M.S. Neto and L. Carazza (2021), “Uber and Traffic Safety: Evidence from Brazilian Cities,” *Journal of Urban Economics*, 103347.

Barrios, J.M., Y.V. Hochberg and H. Yi (2023), “The Cost of Convenience: Ridehailing and Traffic Fatalities,” *Journal of Operations Management*, 69(5), 823-855.

BD (2021), “Base dos Dados,” Database, Accessed September 2024.

Becker, G.S. (1968), “Crime and Punishment: An Economic Approach,” in *The Economic Dimensions of Crime*, Springer, 13-68.

Berger, T., C. Chen and C. B. Frey (2018), “Drivers of Disruption? Estimating the Uber Effect,” *European Economic Review*, 110, 197-210.

Biderman, C., J.M. De Mello and A. Schneider (2009), “Dry Laws and Homicides: Evidence from the São Paulo Metropolitan Area,” *Economic Journal*, 120(543), 157-182.

Brazil, N. and D.S. Kirk (2016), “Uber and Metropolitan Traffic Fatalities in the United States,” *American Journal of Epidemiology*, 184(3), 192-198.

Burgdorf, J., C. Lennon and K. Teltser (2020), “Do Ridesharing Services Increase Alcohol Consumption?” Andrew Young School of Policy Studies Research Paper Series, 19-23.

Callaway, B. and P.H. Sant’Anna (2021), “Difference-in-Differences with Multiple Time Periods,” *Journal of Econometrics*, 225(2), 200-230.

Carpenter, C. (2007), “Heavy Alcohol Use and Crime: Evidence from Underage Drunk-Driving Laws,” *Journal of Law and Economics*, 50(3), 539-557.

Chioda, L. (2017), *Stop the Violence in Latin America: A Look at Prevention from Cradle to Adulthood*, World Bank Publications.

Clarke, R.V.G. (1997), *Situational Crime Prevention*, Criminal Justice Press: Monsey, NY.

______ (1996), *Preventing Mass Transit Crime*, Criminal Justice Press: Monsey, NY.

Cramer, J. and B. Krueger (2016), “Disruptive Change in the Taxi Business: The Case of Uber,” *American Economic Review*, 106(5), 177-182.

de Carvalho, C.H.R. (2020), “Custos dos Acidentes de Trânsito no Brasil: Estimativa Simplificada com Base na Atualização das Pesquisas do Ipea Sobre Custos de Acidentes nos Aglomerados Urbanos e Rodovias,” Technical report.

DETRAN (2021), “Estatísticas de Trânsito,” Database, Accessed March 2021.

Dills, A.K. and S.E. Mulholland (2018), “Ride-Sharing, Fatal Crashes, and Crime,” *Southern Economic Journal*, 84(4), 965-991.

Drehmer, V. (2024), “Os 10 Carros Mais Roubados em São Paulo: Veja se o Seu Está na Lista,” Accessed October 2024.

Dubal, V.B. (2017), “The Drive to Precarity: A Political History of Work, Regulation, and Labor Advocacy in San Francisco's Taxi and Uber Economies," *Berkeley Journal of Employment and Labor Law*, 38(1), 73-135.

Fajnzylber, P., D. Lederman and N. Loayza (2002), "What Causes Violent Crime?" *European Economic Review*, 46(7), 1323-1357.

Greenwood, B.N. and S. Wattal (2017), "Show Me the Way to Go Home: An Empirical Investigation of Ride-Sharing and Alcohol-Related Motor Vehicle Fatalities," *MIS Quarterly*, 41(1), 163-187.

Hall, J.D., C. Palsson and J. Price (2018), "Is Uber a Substitute or Complement for Public Transit?" *Journal of Urban Economics*, 108, 36-50.

Higa, P. (2015), "Táxi ou Uber? Um Comparativo dos Preços das Corridas em Cinco Cidades," Article, Tecnoblog, Accessed October 2019.

Ihlanfeldt, K.R. (2003), "Rail Transit and Neighborhood Crime: The Case of Atlanta, Georgia," *Southern Economic Journal*, 70(2), 273-294.

IPSOS (2024), "Pesquisa Ipsos: Atitudes Sobre o Crime e a Aplicação da Lei," IPSOS.

Jackson, C.K. and E.G. Owens (2011), "One for the Road: Public Transportation, Alcohol Consumption, and Intoxicated Driving," *Journal of Public Economics*, 95(1-2), 106-121.

Levitt, S.D. (2002), "Using Electoral Cycles in Police Hiring to Estimate the Effects of Police on Crime: Reply," *American Economic Review*, 92(4), 1244-1250.

Markowitz, S. and M. Grossman (2000), "The Effects of Beer Taxes on Physical Child Abuse," *Journal of Health Economics*, 19(2), 271-282.

Mayhew, P. and R.V.G. Clarke (1980), *Designing Out Crime*, HM Stationery Office.

McCrary, J. (2002), "Using Electoral Cycles in Police Hiring to Estimate the Effects of Police on Crime: Comment," *American Economic Review*, 92(4), 1236-1243.

Molina Medina, V. C. (2024), "Citizen Security in Latin America and the Caribbean," Accessed October 2024.

Nie, Y.M. (2017), "How Can the Taxi Industry Survive the Tide of Ridesourcing? Evidence From Shenzhen, China," *Transportation Research Part C: Emerging Technologies*, 79, 242-256.

Park, J., J. Kim, M.-S. Pang and B. Lee (2017), "Offender or Guardian? An Empirical Analysis of Ride-Sharing and Sexual Assault," KAIST College of Business Working Paper Series (2017-006).

Ravenelle, A.J. (2019), *Hustle and Gig: Struggling and Surviving in the Sharing Economy*, University of California Press: Berkeley.

Rayle, L., S. Shaheen, N. Chan, D. Dai and R. Cervero (2014), "App-Based, On-Demand Ride Services: Comparing Taxi and Ridesourcing Trips and User Characteristics in San Francisco," University of California Transportation Center, 2, 49-52.

Rogers, B. (2015), "The Social Costs of Uber," *University of Chicago Law Review Dialogue*.

Schor, J.B., W. Attwood-Charles, M. Cansoy, I. Ladegaard and R. Wengronowitz (2020), "Dependence and Precarity in the Platform Economy," *Theory and Society*, 49(5), 833-861.

SEADE (2019), "Informações dos Municípios Paulistas," Database, Accessed August 2019.

Secretaria de Segurança Pública do Estado de São Paulo (2024), "Ocorrências Policiais

Registradas por Mês," Database, Accessed October 2024.
Silverstein, S. (2014), "These Animated Charts Tell You Everything About Uber Prices in 21 Cities," *Business Insider*, 16.
SSP (2017), "Portal do Governo," Database, Governo de São Paulo, Accessed August 25, 2018.
Tirachini, A. and A. Gomez-Lobo (2020), "Does Ride-Hailing Increase or Decrease Vehicle Kilometers Traveled (VKT)? A Simulation Approach for Santiago de Chile," *International Journal of Sustainable Transportation*, 14(3), 187-204.
Uber (2019), "Brasil e a Uber: Aonde Ir e o que Fazer," Database, Accessed August 2019.
Uber, E. (2024), "Mais de 120 Milhões de Usuários e 5 Milhões de Parceiros: Uber Revela Dados Inéditos Sobre Seu Impacto no País," Accessed October 2024.
UNODC (2023), "Homicide and Organized Crime in Latin America and the Caribbean," Accessed October 2024.
Vallas, S.P. and J.B. Schor (2020), "What Do Platforms Do? Understanding the Gig Economy," *Annual Review of Sociology*, 46, 273-294.
Weber, B. (2014), "Can Safe Ride Programs Reduce Urban Crime?" *Regional Science and Urban Economics*, 48, 1-11.
Weber, B.S. (2019), "Uber and Urban Crime," *Transportation Research Part A: Policy and Practice*, 130, 496-506.
Zhou, Y. (2020), "Ride-Sharing, Alcohol Consumption, and Drunk Driving," *Regional Science and Urban Economics*, 85, 103594.

Mailing Address: Rafael Alfena Zago, Texas A&M International, A.R. Sanchez School of Business, 5201University Boulevard, Laredo, Texas 78041, United States of America, E-mail: rafael.zago@tamiu.edu.

Received November 07, 2025, Accepted March 08, 2026.