No Need for Speed: Speed Limit Reductions Do not Increase Vehicle Robberies

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Abstract

I analyze the effect of a widespread speed reduction policy implemented in the city of São Paulo on vehicle robberies. Exploring the staggered nature of the policy implementation, I estimate an event study robust to heterogeneity and different adoptions of the treatment, using crime data on reported vehicle robberies from 2014 to 2016, for the months before and after the policy's implementation in 2015. I demonstrate that the overall effect on the treated road segments is either null or, when significant, small and negative, contradicting common concerns that the speed reduction could increase drivers' exposure to crime. I also conduct analyses of heterogeneity for different subgroups by vehicle type and time of day, and the null results remain consistent.

Keywords: Crime, Accidents, Urban economics

1 Introduction

This paper examines a controversial issue: does lowering speed limits on urban roads lead to an increase in vehicle robberies? Reducing speed limits on urban roads is a policy gaining traction globally, with cities in Colombia to France implementing it to improve traffic safety (NGO, 2023). The World Bank and the World Health Organization even recommend lower speed limits to reduce traffic fatalities (World-Bank) and (WHO). However, public opinion often opposes such policies due to concerns about the inconvenience of increased travel and driver's safety. Critics worry that slower speeds make drivers – especially those in vulnerable vehicles like motorcycles – easier targets for robberies and having their vehicles stolen. This concern is especially prevalent in Developing Countries, which have higher crime rates than developed countries and worries might not be unfounded.

Little research exists on the causal connection between speed limits and crime. This study aims to fill that gap by exploring the potential link between Speed Limits and Criminality. To answer this question, I analyze a natural experiment in São Paulo, Brazil, in 2015. To improve traffic safety and reduce the number of accidents, the city government implemented a program called *Programa de Proteção à Vida* (Life Protection Program). As part of this program, speed limits were reduced on most major urban roads and highways throughout the city. This policy change sparked public debate, with even the policy being questioned in courts, arguing "it would make drivers more vulnerable to crime, particularly robberies perpetrated against slow-moving vehicles" (de Minas, 2015).

To verify this claim, using an event study design, I estimate the impact of this policy on a monthly panel of vehicle robberies by road segments. Using the (Sun and Abraham, 2021) event study estimator, which is robust to heterogeneous treatment and staggered adoption, I estimate the model from 2015 to 2016 on a monthly panel of vehicle robberies by road segments, which is constructed using the universe of reported vehicle robberies in the State of Sao Paulo. I use the Sao Paulo State Department of Public Security definition of robbery as a robbery in which the crime perpetrator uses violence against the owner of the vehicle. I consider the roads that received a speed limit reduction from the city government to be part of the treatment group. To get the date of the speed change, I access the Sao Paulo City Government public announcements in its website. I do not find any significant effect on the reported post-treatment coefficient for total reported robberies in the segment. All coefficients are bounded in the interval 0 and -0.013, with only three coefficients for relative periods l after the implementation $1, 4$ and 12 being significant but still negative and very small. Due to the data's higher prevalence of zeros, I estimate the same model for transformations of the outcome variable total robberies. The three other transformations I consider are inverse hyperbolic sine function (IHS), reported total monthly vehicle robberies by kilometer, and a dummy variable that takes a value of one if the road segment reports a strictly positive value of total robberies. I apply the IHS due to the high prevalence of zeros, with approximately 85% of the control segments never reporting a robbery yearly for 2014-2016 and 75% of the treated never reporting for the same period. The Robberies by Kilometer accounts for the differing size of the road segments, and finally, to better capture the extensive margin effects of changing from zero to at least one, I use the dummy of strictly positive reported robbery. Even after I estimate the same model with the different transformations of the outcome variable, most of the estimates are not statistically significant, and when they are, they have a negative sign; that is, the results go in the opposite direction of what critics of the policy argue.

I address potential issues that could undermine my results to ensure more reliable results. One concern is crime displacement: Did robberies simply shift to nearby untreated roads following the speed limit reduction on treated roads? To address this, I restrict the control group to roads at least 1.6 kilometers away from treated segments, minimizing the likelihood of robberies moving to nearby areas. Another concern is the comparability of control groups: were the treated more likely to receive treatment than control? Since the speed limit reductions were implemented to reduce traffic accidents and avoid unnecessary traffic deaths, the city government would implement the speed reduction policy on the most dangerous roads and with the most traffic. To ensure a more balanced comparison, I perform a nearest-neighbor matching. This approach pairs each treated road segment with a control segment that had the most similar number of reported accident victims in the year 2014, which is the year prior to the policy change. As an additional restriction, I perform the same matching procedure as before but restrict the road segments to be also 1.6km away from all treated units. Even making all these sample restrictions, I do not find evidence of an increase in robberies for most of the different outcomes, and when I find some coefficients to be significant, they are very small and have a negative sign.

To further comprehend the results, I performe two heterogeneity analysis exercises based on the reported crime period and by vehicle type. I estimate the model for different window frames of a period of the day: Early Morning from 00:00 AM to 06:00 AM, Morning from 06:00 AM to 12:00 PM, Afternoon from 12:00 PM to 18:00 PM, and Night from 18:00 PM to 00:00 PM. The other heterogeneity analysis uses four vehicle types: cars, trucks, motorcycles, and buses. Excluding motorcycles, all other types of vehicles report no increases after the policy. Motorcycles seem to have pretends, although the post-treatment coefficients are in the interval $[-0.01, 0]$ (the only exception is $t = 11$, which is positive but very small), with it being all negative and most of them insignificant.

The rest of the paper is structured as follows. Section 2 discusses the literature review. Section 4 details the institutional background and implementation of the policy. Section 5 describes the data sources used and the panel's construction and presents some summary statistics. In Section 6, I present the details of the Empirical Strategy and an explanation of the estimator used. Section 7 presents the main results and heterogeneity by period and vehicle type, and Section 8 presents the conclusion.

2 Literature Review

To the best of my knowledge, this paper's main contribution lies in being among the first to explore the relationship between urban road speed limits and crime. It provides evidence that it may not impact a particular driver concern: the risk of exposure to vehicle robbery due to low speed. The findings are significant in a broader context of developing countries with higher crime rates.

On this topic, the most similar papers to mine are Jardim (2017) and Ang et al. (2020), which analyze the effects of speed limit reductions in São Paulo. However, what distinguishes these papers from mine is their focus on accidents, not crime. Jardim (2017) estimates a reduction of 0.32 accidents per month on treated roads, while Ang et al. (2020) estimates a 21.7% reduction in accidents and 1,889 averted accidents. They also estimate that the social benefits are 1.32 times larger than the social costs, with most benefits concentrated at the most vulnerable: lower-income pedestrians and motorcycle drivers.

Regarding road safety and accidents, the literature has identified that lower road speed limits lead to fewer accidents and increased driver safety. For example, Friedman et al. (2009) estimates that the 1995 reversal of speed limits in the United States led to a 3.2% increase in road accidents overall, with a 9.1% increase in rural interstates and a 4.0% increase in urban interstates. A seminal paper by Ashenfelter and Greenstone (2004) uses the 1987 United States Federal Government permission for states to increase rural interstate highway speed limits as a natural experiment, estimating the trade-off between increased speed limits and increased accidents and road fatalities. The authors found that increasing the speed limit from 55 mph to 65 mph raised the average driving speed by 2.5 mph and increased travel fatalities by 35%.

Using the same natural experiment and additional variation coming from the Federal Government giving back to states the right to set up speed limits in 1995, van Benthem (2015), a comprehensive paper on optimal speed limits, discusses the private and public costs and benefits of speed limit reductions. Analyzing data from California, Oregon, and Washington, the study estimates that a 3–4mph increase in travel speed led to a 9–15% increase in accidents and a 34–60 % increase in fatal accidents. Additionally, the same increase in speed limits resulted in a 14–24% increase in carbon monoxide levels, an 8–15% increase in nitrogen oxides, a 1–11% increase in ozone, and a 9% increase in fetal death rates around the affected freeways.

The literature on the impacts of roads and other transportation infrastructures has explored various outcomes. The most common include the effect on health and pollution (Currie and Walker, 2011), van Benthem (2015), the effects on the spatial distribution of crime and disamenities Agnew (2020) and Brinkman and Lin (2022), Wuschke et al. (2021), and the effects on accidents (Ang et al., 2020), (Agnew, 2020), van Benthem (2015), (DeAngelo and Hansen, 2014), (Friedman et al., 2009), (Ashenfelter and Greenstone, 2004).

Moreover, roads and highways can also affect urban decline and crime in nearby neighborhoods. Agnew (2020) examines the effects of road expansion in Irish counties and finds a significant association between the two, arguing it facilitated burglars' escape routes. A more comprehensive study by Brinkman and Lin (2022) analyzes the effect of interstate roadway expansion on urban decline in Chicago, calibrating a quantitative model to the city and estimating an 18% reduction in amenities for residents near the freeway.

As highlighted, the literature has explored the various impacts of roads and, in some cases, their speed limits on topics such as health outcomes (Friedman et al., 2009), infant mortality (Currie and Walker, 2011), urban decay (Brinkman and Lin, 2022), and accidents (van Benthem, 2015), (Ang et al., 2020). However, to the best of my knowledge, I am the first to relate the effect of speed limits to crime.

3 The São Paulo Speed Limit Reduction

This section explains the context of the speed limit reduction program in São Paulo and the policy implementation.

3.1 Context

The city of São Paulo, Brazil, is the largest in Brazil, with a population, according to the Brazilian Census of 2010, of 11.253.503 inhabitants IBGE (2010), and in its metropolitan region with more than 20 million. The city had approximately 8 million motor vehicles registered in the city in 2015 IBGE (2015); consequently, it is prone to accidents being frequent. Concerned about the increasing number of accidents and fatalities, the city government implemented the program *Programa de Proteção à Vida* with a widespread set of policies to decrease the number of victims and road accidents in the city. Among the policies implemented by this program, it reduced the speed limits in the two most important highways, the *Marginal Tietê* and *Marginal Pinheiros*, and other critical arterial roads and streets, with the first cohort of treated roads being in July of 2015 and the last one being in December of 2015. For reference, the *Marginais* highways, the two most important highways in the city, with the Marginal Tietê interconnecting the West, North, and East regions of the city, and Marginal Pinheiros linking the North and South of the city. The policy reduced the speed limits on several highways and important roads in the city. For reference, the first treated roads, *Marginais*, the express lanes' speed limits were reduced from 90 km/h to 70 km/h for light vehicles and from 70 km/h to 60 km/h for heavy vehicles. For both types of vehicles, the speed limit went down on the central lanes from 70 km/h to 60 km/h. On the *Marginais* local lanes, the limit was reduced from 70 km/h to 50 km/h. The speed limit was reduced on other arterial roads from 60 km/h to 50 km/h.

Despite the well-intentioned purpose of the speed limit reduction policy, it faced significant public backlash, and it was widely unpopular among the city's residents, particularly those who frequently used the Marginal Tietê and Marginal Pinheiros. Widespread disapproval arose among the public, with critics claiming the policy not only burdened drivers with increased travel time but also did not increase driver and road safety. A common concern among the general public was that the policy would increase the risk of car theft (Uol, 2015). One notable example to highlight the opposition from the São Paulo Bar Association (*Ordem dos Advogados do Brasil - OAB*), which legally contested the speed limit reductions, advocating for their repeal (G1, 2015), (ConJur, 2015) and citing as arguments the policy would increase the vehicle robberies. This dispute over speed limits echoed through the political landscape, becoming a focal point in the 2016 mayoral race. The candidate and future Mayor of São Paulo, João Doria, successfully campaigned to SPEED UP SÃO PAULO! (*ACELERA SÃO PAULO*). Moreover, he vowed to undo these adjustments. The vow to increase speed limits seemed to echo with voters, and the candidate successfully won the election in the first round. Following his electoral victory, the new Mayor reverted the speed limits to their original values before the treatment in early 2017, nullifying the previous administration's controversial decision.

3.2 Policy Implementation

In order to implement the speed limit reduction policy, the City Government tasked the *Companhia de Engenharia de Tráfego* (CET), the city department of road safety and transportation, to make all the processes and information. The CET is a department of the City of São Paulo responsible for managing and organizing the city's traffic. Before the initial date of implementation of the policy, the CET and the City of São Paulo would announce on both their websites the day of the change and which highways, streets, and roads would have a reduction. In Figure 20, we can see an example of an announcement[1].

On average, the announcement date would occur one or two weeks before the day the speed limit reduction took effect. The CET would use banners on the treated roads to inform drivers of the change. For a complete table of all treated road date announcements and speed limit changes, please go to the last section of the Appendix.

The Speed limit reduction program was staggered in nature, with the policy interventions occurring between July and December of 2015. Figure 3 shows a widespread policy affecting the city of Sao Paulo, with all its major regions having a treated road, with the only exception being the southernmost part of the city, a more rural area with less population. To get an idea of the total treated roads, Figure 4 shows the cumulative kilometers of treated roads by month. Figure 4 shows that at the end of staggered implementation, a total of 953.736 km were affected by this policy.

4 The São Paulo Speed Limit Reduction

This section explains the context of the speed limit reduction program in São Paulo and the policy implementation.

4.1 Characteristics of São Paulo

São Paulo is the largest city in Brazil with a population of 11,253,503 according to the 2010 Brazilian Census (IBGE, 2010), with its metropolitan region housing over 20 million people. The city is the major economic hub in Brazil with the city proper accounting for approximately 10.. In addition to its economic importance, São Paulo is characterized by havoingThe city's infrastructure includes an extensive network of roads and highways, including major thoroughfares such as *Marginal Tietê* and *Marginal Pinheiros*, which are vital for intra-city and inter-city connectivity. However, the high number of registered vehicles—approximately 8 million in 2015 (IBGE, 2015)—contributes to severe traffic congestion and frequent accidents.

4.2 Programa de Proteção à Vida and Speed Limit Reduction

In response to the growing number of traffic accidents and fatalities, the city government, under Mayor Fernando Haddad, launched the *Programa de Proteção à Vida* (Program for the Protection of Life). This initiative aimed to enhance road safety through various measures, with a key component being the reduction of speed limits on major highways and arterial roads.

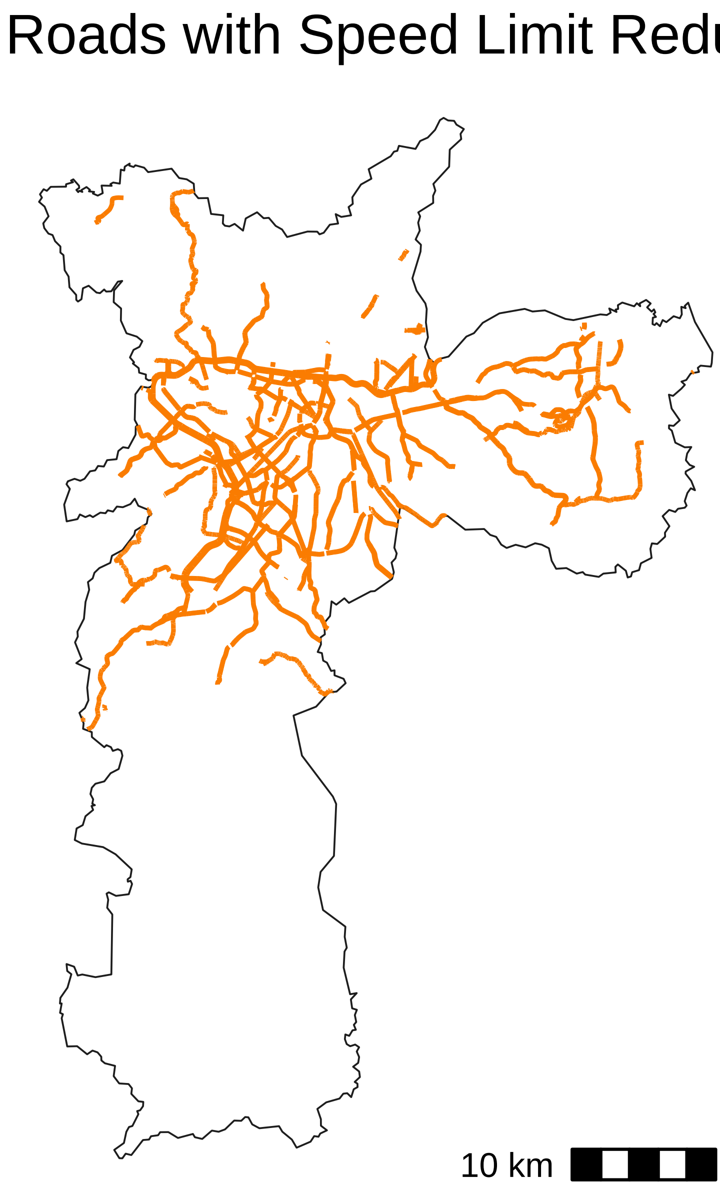
The implementation began in July 2015 and concluded in December 2015. The *Marginais* highways, comprising *Marginal Tietê* and *Marginal Pinheiros*, were prioritized due to their high traffic volumes and accident rates. The policy adjustments included reducing speed limits on express lanes from 90 km/h to 70 km/h for light vehicles and from 70 km/h to 60 km/h for heavy vehicles. The central lanes' speed limits were lowered from 70 km/h to 60 km/h, and the local lanes saw reductions from 70 km/h to 50 km/h. Other arterial roads also experienced speed limit reductions from 60 km/h to 50 km/h.

The *Companhia de Engenharia de Tráfego* (CET), São Paulo's traffic management and road safety department, was tasked with executing the policy. Announcements were made on the CET and City of São Paulo websites one to two weeks before the speed limit changes took effect, and banners were placed on treated roads to inform drivers. These announcements provided specific details about the highways, streets, and roads that would be affected by the speed limit reductions.

The speed limit reduction program was implemented in a staggered manner, affecting various regions of the city at different times. Figures 3 and 4 illustrate the scope and timeline of the policy implementation, showing that by the end of 2015, a total of 953.736 kilometers of roads had been

[1]This specific announcement can be accessed at this link

Figure 1: Map of Roads and Street with Speed Limit Reduction in São Paulo



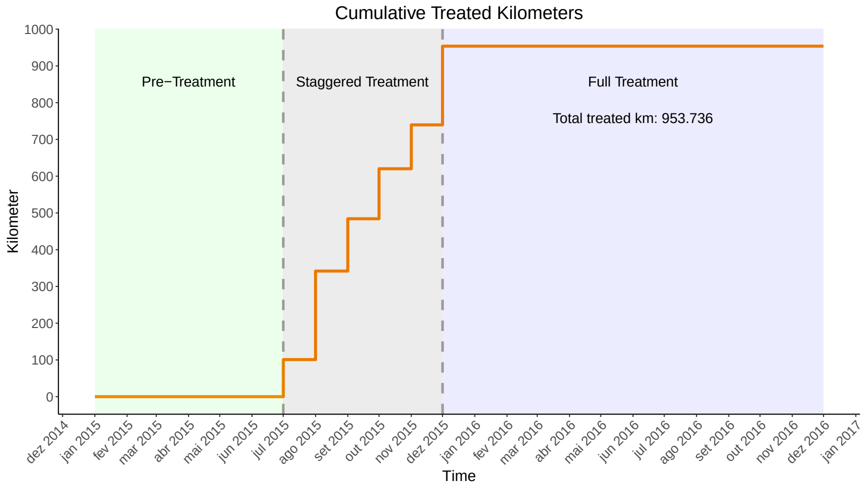
affected. This comprehensive approach aimed to create a safer driving environment across São Paulo, reducing the incidence of traffic accidents and fatalities.

4.3 Public Debate and Controversy

Despite the intended benefits, the speed limit reduction policy faced significant public backlash and was widely unpopular among São Paulo's residents, particularly frequent users of the Marginal Tietê and Marginal Pinheiros. Critics argued that the policy increased travel times and did not substantially improve road safety. Concerns about heightened risks of car theft due to slower speeds also emerged (Uol, 2015).

The São Paulo Bar Association (*Ordem dos Advogados do Brasil - OAB*) legally contested the speed limit reductions, advocating for their repeal and citing increased vehicle robberies as a potential consequence (G1, 2015; ConJur, 2015). This controversy became a focal point in the 2016 mayoral race, with candidate João Doria campaigning under the slogan *ACELERA SÃO PAULO!* (Speed Up São Paulo!). His promise to reverse the speed limit reductions resonated with voters, leading to

Figure 2: Cumulative Monthly Treated Road by Kilometers in São Paulo



his electoral victory. Following his election, the new mayor promptly reinstated the original speed limits in early 2017, effectively nullifying the previous administration's decision.

5 Data

This section presents the data sources I use for the analysis. I describe them, detail the features of the data, and summarize them.

5.1 Data Sources

This subsection introduces the datasets I employed in the analysis, highlighting their principal features.

5.1.1 Crime Data - *Registro Digital de Ocorrências* (RDO)

I use data on vehicle robberies from the *Registro Digital de Ocorrências* (RDO), which is publicly available on the São Paulo State Public Security Department's website[2]. This dataset comprises detailed records from *Boletim de Ocorrência* (*B.O.*), the tool police stations use to document crimes. Each record includes the crime's reported date and time, the location (address, City, coordinates), details about the vehicle (plate, color, model, type), the police station handling the case, and its resolution status. Although I have data from 2000 to 2020, I only use crime data from 2014 to 2016 since the City Goverment reverted the policy at the beggining of 2017. Here, I use the Sao Paulo State Public Security Department's definition of vehicle robbery. It defines a vehicle robbery as forcibly stealing a vehicle from a person. In other words, to qualify as robbery, it is important the crime perpetrator resorted to violence. It does not include cases in which thieves steal unattended parked vehicles or cases without the use of violence.

5.1.2 Road Segments - Shapefile

I obtained São Paulo's road and street segment data from a shapefile provided by da Metrópole (2021), encompassing the City's entire metropolitan area. The shapefile details each segment's name, zip code, coordinates, length, neighborhood, number, address code, City, state, district, and road type (e.g., street, avenue, highway, viaduct, tunnel). The *Centro de Estudos da Metrópole* (*CEM*), an urban studies research institute affiliated with the University of São Paulo, maintains this shapefile, and it is constantly updating the shapefile to include all road segments in the São Paulo Metropolitan Region. The shapefile data has exactly 163,245 individual road segments, which are located within the City of São Paulo limits.

5.1.3 Road Segment Characteristics

For details like traffic accidents, victims, traffic lights, speed monitoring cameras, and speed bumps on road segments, I refer to the *Painel de Mobilidade Segura*, a publicly available dashboard[3]. The City of São Paulo's Secretary of Transportation and Transit maintains the dashboard panel. It includes the locations, activation, and deactivation dates of speed monitoring cameras, speed bumps, traffic lights, and which roads. Additionally, it provides accident statistics, including location, the number and type of vehicles involved, and the number of casualties and injuries. However, unlike the dates for accidents and speed monitoring cameras, the database lacks specific installation and removal dates for speed bumps and traffic lights.

5.1.4 Road Segments - Treatment date

To get the date when a speed limit reduction would occur, I used the City of São Paulo Municipal Government website, the same source in which the City announced the speed limit changes.

[2]You can find the *Registro Digital de Ocorrências* data here: link

[3]The Dashboard Panel *Painel de Mobilidade Segura* can be accessed here: link

5.2 Construction of the Panel

To construct the panel, I collapse the crime data at the *Boletim de Ocorrencia* level so that there was just one observation for each crime. Note that the crime data has multiple instances of the same crime since it would record different stages of investigating a specific crime, from initial reporting by the victim to the final solution. I collapse this dataset so it only keeps distinct observations that have the vehicle plate recorded in it. The dataset of vehicle robberies has the reported crime address and geographical coordinates (latitude and longitude) for most of the observations. Unfortunately, some crimes had a missing value in the address section, missing coordinates, or both.

To handle the missing coordinates in the crime data, I first drop the observations with a missing address since it is not possible to verify the location. For the observations that have the address but missing coordinates, I use the **R** package, *tidygeocoder* with the BING Maps API to recover the geographical coordinates based on the reported address. The process recovered the latitude and longitude of all missing addresses, but some were for locations outside the City of Sao Paulo limit. Again, fortunately, most of the observations recovered were within the city boundaries. The ones outside the city limits were dropped. To better understand the process, Table 1 shows the counts of observations for this process. At the end of this process, I end with 121155 unique reported vehicle robberies for 2014-2016.

To do the spatial match of road segments in the shapefile, I use the open source software *QGis*. I match the reported vehicle robbery to the closest road segments in the shapefile within a radius of 35 meters. I do the same for other road segment characteristics, like the presence of Speed Monitoring Cameras and accident victims, and export it back to **R**.

Finally, I aggregate the number of vehicle robberies, road characteristics, and other outcomes at the month, segment I.D. level, and assembly road I.D. month panel data.

5.3 Descriptive Statistics

In this subsection, I briefly describe some characteristics of the dataset. I report in Table 2 the summary statistics of the dataset. Panel A, titled "Treatment Detail," shows the observational count, distinguishing between the control group, with a substantial 157,245 observations, and the treated group, which comprises a smaller subset of 6,208 observations. The panel shows a small fraction of approximately 3.80 % of road segments being treated.

I report some characteristics fixed in time by treatment status in panel B. Regarding road segment characteristics in Panel B, treated segments have a greater prevalence of various restrictions in treated areas, including those on cars, trucks, and freight, suggesting a more regulated environment. Moreover, treated segments are more likely to have traffic control measures like traffic lights, although control segments exhibit a higher frequency of speed bumps than their Control counterparts. Moving to Panel C, which assesses crime outcomes, the data, presented as the yearly mean value per segment for 2014, 2015, and 2016, reveal a consistent pattern where the treated group exhibits higher mean counts across various vehicle categories compared to the control group. Specifically, the mean count for total vehicles in the control group was 0.251 in 2014, 0.222 in 2015, and 0.218 in 2016, which starkly contrasts with the treated group's higher mean of 0.721 in 2014, 0.593 in 2015, and 0.558 in 2016. Similarly, cars in the control group had mean counts of 0.181 in 2014, 0.168 in 2015, and 0.166 in 2016, while the treated group showed higher means of 0.401 in 2014, 0.344 in 2015, and 0.326 in 2016. This trend extends to motorcycles, microbuses and buses, trucks, and other vehicles, with the treated group consistently demonstrating higher mean counts, which should be expected since these are the most used roads in the City.

Moving to Panel D, which focuses on accident categories, the data again is presented in mean values, illustrating clear differences between the treated and control groups regarding injuries, fatalities, accidents with victims, and incidents involving hitting someone. The control group's mean count for injuries was notably lower at 0.138 in 2014, 0.118 in 2015, and 0.099 in 2016, compared to the treated group's substantially higher means of 0.930 in 2014, 0.776 in 2015, and 0.446 in 2016.

Fatalities followed a similar pattern, with the control group reporting lower mean counts of 0.006 in 2014, 0.005 in 2015, and 0.004 in 2016, against the treated group's higher means of 0.040 in 2014, 0.030 in 2015, and 0.024 in 2016. As before, the higher values of treated is due to the latter having more vehicles using them than other roads.

To better understand the overall distribution of reported vehicle robberies, Figure 21 showcases bar plots illustrating the annual sum of thefts by segment group alongside the treatment status. It becomes apparent that incidents of vehicle theft are relatively uncommon for both the treated and control groups. Annually, approximately 85% of control segments report no vehicle robbery incidents. At the same time, about 75% of treated segments also record zero instances, and only a small minority of vehicle robberies are reported as more than five crimes per year. The previous fact indicates that vehicle robberies are infrequent and control segments report fewer robberies than the treatment group. Moreover, it is observed that, on average, the proportion of treated segments with robbery incidents tends to be higher than that of their control counterparts each year, given it reports a strictly positive value. Figure 21 shows this type of crime is infrequent, with a minority of road segments having a strictly positive value, and the treated group, on average, reports more crimes than controls.

Finally, Figure 5 shows the total vehicle robberies by treatment status. In Figure 5, the Control segments have more total vehicle robberies than the treated roads. Three regions divide the Figure into regions with Pre-Treatment occurring before July 2015, staggered adoption between July 2015 and December 2015, and post-treatment after December 2015. The maximum reported value for the control group was approximately 3500 in January 2014 and a minimum of 2500 in June 2014, while the treated group reports a line plot bounded by 250 and 350. It is possible to perceive a null difference in trends between the two groups, with the two being "parallel". The figure also shows an increase in control over the staggered implementation. The overall message is the trend of these two line plots seems to be parallel.

6 Empirical Strategy

In this section, I describe the proposed empirical estimation using the Difference-in-Difference (DiD) estimator Sun and Abraham (2021), designed to deal with treatment heterogeneity under a staggered adoption and its associated problem.

6.1 Empirical Model

I will use an event study model for the 2015-2016 period to measure the effects of speed limit reductions. This model proposed by Sun and Abraham (2021) recovers a weighted average of the Cohort Average Treatment Effect (CATT) that does not suffer from the pitfalls of the traditional Two-Way Fixed Effects (de Chaisemartin and D'Haultfœuille, 2020). The model is defined as:

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_{e \notin C} \sum_{l \neq -1} \delta_{e,l} \left(\mathbf{1} \{E_i = e\} \cdot D^l_{i,t} \right) + \epsilon_{i,t} \tag{1}$$

$$v_g = \frac{1}{|g|} \sum_{e \notin C} \sum_{e,l} \delta_{e,l} \Pr \left(E_i = e \mid E_i \in [-l, T-l] \right) \tag{2}$$

In this equation 1, $Y_{i,t}$ represents the outcome variable for road segment i in month t. The variable E_i is the treatment date of unit i, and e is a specific treatment date. The variable $D^l_{i,t} := 1\{t - E_i = l\}$ is a binary variable, taking a value of 1 if the road segment i in month t is l periods away from initial treatment date . The set C contains all units whose treatment date e are used to compare control groups. In the case of never-treated units, $C = \{i : E_i = \infty\}$; in other words, they never receive treatment. $1\{E_i = e\}$ is a dummy if unit i received treatment at date e, and $\delta_{e,l}$ is the coefficient of the estimated of the interaction between $1\{E_i = e\} \cdot D^l_{i,t}$, and it will be equal to the Conditional Average Treatment Effect, which is the Average Treatment of units that share the same treatment date, i.e., $E_i = e$. The precise terminology of what it means and more estimation details are explained in subsection 6.2.

The term α_i denotes fixed effects for each road segment. These fixed effects account for all unobserved, time-invariant characteristics of each road segment that could influence the outcome variable, such as fixed characteristics like the steepness of the terrain, its size, traffic lights, speed bumps, and more. Similarly, λ_t is month-year fixed effects, capturing any temporal trends or seasonal patterns.

The core of model 1 lies in the parameter given by equation 2, v_g, which aggregates a weighted average of the CATTs grouped in bin g. In this case, the weighted average in bin g such that unit i belongs to this bin if it has a treatment date E_i inside the interval $[-l, T-l]$ where T is a specific value of time t. It means it groups the units with varying treatment dates that have experienced at least l treatment periods. For reference, if we estimate $\hat{v_1} = 1.0$, it means the treatment increased in 1.0 units the outcome variable after 1 period away from date of initial treatment.

I estimate the model 1 on a sample of reported total robberies between January 2015 and December 2016. Additionally, I employ the same regression framework as in the model (1) but with variations of Y_{it} to better understand the impact. These variations include the total vehicle robberies divided by the length of the road segment (Total Robberies by Km), a binary variable indicating whether any robberies were reported on the segment to capture an extensive margin and lastly, an inverse hyperbolic sine transformation to address the high prevalence of zero counts as observed in Figure 21 of the Appendix and give an interpretation of percentages similar to a *log* transformation, but under the presence of zeros.

Given the staggered nature of the treatment across different road segments and times, I lean on the estimator proposed by Sun and Abraham (2021). This estimator is particularly robust in handling treatment effect heterogeneity and varying treatment timings. In the subsection , I explain the methodology.

In my analysis, I estimate both models using the entire dataset for the monthly period between 2015 and 2016. Then, I estimate an additional approach with three additional sample restrictions to enhance the robustness of my findings. These restrictions include:

1. matching based on the pre-trends of the total number of victims in 2014,
2. selecting control segments that are beyond a threshold distance of 1.6 kilometers,
3. a combined approach that incorporates both matching and distance criteria.

The rationale behind the matching procedure is to create a control group that mirrors the treatment group more closely in terms of their characteristics. Given the treatment group consists of exactly 6,208 road segments out of a total of 163,245, a potential concern that the treatment segments might significantly differ from the control ones, which could compromise the latter's suitability as a comparison group. In the Appendix, Table 5 shows the means of the Total reported vicitims in the segments in 2014.

Implementing a distance threshold aims to account for potential spillover effects resulting from changes in driver behavior. Road segments near treated areas might experience increased traffic as drivers choose these routes to maintain higher average speeds, especially if speed limit enforcement is perceived to be less stringent on these alternative routes. I choose the threshold value of 1.6km to replicate the same value used in the work of Ang et al. (2020) that analyzes the same policy intervention but on road accidents. Lastly, I perform a final sample restriction that simultaneously restricts the control to match and be more than 1.6km away.

6.2 Sun and Abraham (2021)

In this section, I explain in detail the procedure used to estimate models. de Chaisemartin and D'Haultfœuille (2020) and Goodman-Bacon (2021) show the traditional Difference-in-Differences or Two-Way Fixed Effects estimator and its associated event study have pitfalls related to not being robust to treatment heterogeneity in staggered adoption of a policy. Several estimators robust to heterogeneous treatment in a staggered adoption have been proposed. For instance, Borusyak et al. (2021), Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2020) andSun and Abraham (2021), Gardner (2022) propose different solutions to this problem, with varying assumptions to identify the target parameter. In this context, I will use the DiD estimator proposed by Sun and Abraham (2021), from now on, referred to as SA-DiD. The main reasons were its simplicity and interpretability.

Under the hypothesis of Conditional Parallel Trends and no anticipatory behavior, the proposed SA-DiD estimator estimates a weighted average of Conditional Average Treatment Effects (CATT) for specific cohorts and periods. The following three steps can obtain this estimator:

1. Estimate a regression that interacts group dummies (excluding dummies for being in control) with relative time dummies, using a specific control group as a comparison (never treated units or last units to be treated).
2. Estimate the probabilities $\Pr(E = e)$ by the sample shares of each cohort.
3. Form the Interaction-Weighted (IW) estimator by taking the weighted average of CATT estimates from step 1, using weights from step 2.

Now, I formally explain the method of Sun and Abraham (2021), explicitly detailing the procedure.

Define Conditional Average Treatment Effect (CATT) as:

$$CATT_{e,l} = E\left[Y_{i,e+l} - Y^{\infty}_{i,e+l} \mid E_i = e\right]$$

Where i indicates an individual unit, e denotes the cohort sharing the same treatment initiation date, E_i is the treatment date of the unit, l is a relative time indicator that marks the period before or after the treatment relative to e. The term $E[Y_{i,e+l} - Y^{\infty}_{i,e+l} \mid -E_i = e]$ denotes the expected difference in outcomes for an individual unit i conditional on having treatment date E_i equal to e, comparing the observed outcome $Y_{i,e+l}$ at time $e+l$ with the counterfactual outcome $Y^{\infty}_{i,e+l}$ that would have prevailed in the absence of the treatment.

The main identification assumption is Conditional Parallel Trends The underlying Parallel Trends hypothesis is:

$$\mathbb{E}\left[Y^{\infty}_{i,t} - Y^{\infty}_{i,s} \mid E_i = e\right] \quad \forall s \neq t \text{ is the same for all } e \in \operatorname{supp}(E_i)$$

Intuitively, the Parallel Trend says the difference in mean outcomes between two different periods s and t for never being treated should be equal conditional on all units that share common treatment date E_i.

The estimator of effect for the binned group g will be:

$$v_g = \frac{1}{|g|} \sum_{l \in g} \sum_{e} CATT_{e,l} \operatorname{Pr}\left(E_i = e \mid E_i \in [-l, T-l]\right)$$

Where $\operatorname{Pr}\left(E_i = e \mid E_i \in [-l, T-l]\right)$ is the share of units with treatment date e and which treatment date is within the relative period interval $[-l, T-l]$, g is the bin that contains units that have common treatment date at the interval defined. The IW estimator, $\widehat{v}_g$, is thus defined as the sample analog to the above equation:

$$\widehat{v}_g = \frac{1}{|g|} \sum_{l \in g} \sum_{e} \widehat{\delta}_{e,l} \widehat{\operatorname{Pr}}(E_i = e | E_i \in [-l, T-l]) \tag{3}$$

where $\widehat{\delta}_{e,l}$ represents the estimated treatment effects for group e at relative time l.

Note that as Sun and Abraham (2021) points out, their proposed estimator, under comparison with never treated units, is a special case of the Callaway and Sant'Anna (2021) with the main difference being on the computation of the standard errors of the coefficients in this setting[4] . While Callaway and Sant'Anna (2021) prefer a bootstrap approach that calculates simultaneous confidence intervals for multiple coefficients, Sun and Abraham (2021) computes analytically using a plug-in consistent estimator of the asymptotic variance of the estimators[5] Finally, due to the similarity of the estimator but less computational cost of running and that the confidence intervals are pointwise valid, I opted for this method.

[4]This fact does not hold if we do not use never-treated units. Callaway and Sant'Anna (2021) uses as alternative comparison Not Yet Treated while Sun and Abraham (2021) uses the Last Unit Treated.

[5]If interested, the reader can go to Appendix C of Sun and Abraham (2021) for the proof of the Asymptotic Normality of the estimator and its asymptotic variance.

7 Results

In this section, I report the main results from model estimation 1 using four different samples: Full Sample, Matching on Victims in 2014, Controls 1.6 km away, and Matching on Victims in 2014 and Controls 1.6km away.

7.1 Main results

In Table 6, I report the results of estimating the model 1 on the Full Sample, with coefficients being displayed of the relative periods ≥ 0 after the date of initial treatment. The model was estimated to have 12 pre-treatment and 16 post-treatment coefficients. At first glance, the table shows that for every different outcome variable, each individual coefficient is not statically different from zero, with the notable exception of some coefficients that are statically negative but by a small margin for $t = 1, 4, 12$. In order to better understand and visualize clearly the overall pattern, in Figure 6, I have the even study plots for each one of the outcome variables being analyzed and 95 % confidence intervals. The first observable pattern is the absence of pretrends in all plots, with zero being included in the confidence interval in all 12 coefficient pre-treatment for each outcome. After the treatment date, Figure 6 shows the post-treatment coefficients include the zero in its confidence region, and the ones that do not like $t = 1, or 4 or 12$, the upper bounds of the confidence interval are very close to zero.

With that in mind, the Panel (a) for Figure 6 of Total Robberies in magnitude, the coefficients are between -0.01 and 0, meaning that if our estimates were to be significant (remember most of them are not), it would be that the policy would reduce the total robberies in the treated road by most -0.01. Take the example of the coefficients $t = 1$, $t = 4$, and $t = 12$, which are significant. This is indeed the case. So, by a small margin, the total robberies decrease. For panels (b) and (d), which are rescaled of the original total robberies by dividing the outcome variable by the total length of the segment in kilometers or transforming it using the inverse hyperbolic sine function, the same pattern for pretends is presented.

Since the dataset has an excess of zeros, capturing extensive margin effects would be an interesting exercise. Panel (c) helps identify an extensive margin effect of a road segment moving in percentage points from reporting from 0 to 1 total robberies. Again, almost all the coefficients are insignificant, and also, when it is significant, like in $t = 12$, the value is close to -0.075% percentage points from the baseline. So even if the estimates were significant, it would have as a lower bound of a reduction of 1% in the baseline for a farther away period, at most.

In Figures 10, 8 and 12, I graph the results of the coefficients of the event study for the threshold distance of 1.6km away for control, Matching on Victims in 2014 and both Matching and 1.6km, respectively. As a general pattern, the pre-treatment period coefficients show no evidence of pre-trends, with only Panel (b) of Figure 8 having an individual pretend for coefficients $t = -7, t = -4$. For the post-treatment, there is evidence of the coefficients being not statistically significant, with the only exception being $t = 1, 4, 12$ as before.

Related to the event study graph, one notable mention is to explain the pattern observed of increasing confidence regions as the coefficients are closer to the edge. The reason for this fact is that although my Panel is balanced in time, it will unbalanced in relative time. Remember, the setting has a staggered adoption of the treatment, so farther away from the treatment date, fewer units would have been under treatment. For example, the first units to have experienced treatment were in June 2015, and the last one in December 2015. As my Panel ends in December 2016, at this last period, all units would have experienced at least 12 months of treatment for the last ones to be treated, while the first ones would have experienced 16 months.

I perform an aggregation by cohort of treatment ATT on Table 4. The monthly aggregation cohort report values of the coefficients for the dates of treatment: July 2015, August 2015, September 2015, October 2015, November 2015, and December 2015. A noticeable pattern is when doing this procedure, the results seem to be not statically different than zero. with for all cohorts of treatments, with the only exception being the last cohort of treatment in December 2015. For the Columns (1) , (2) and (4) the coefficients are statically significant at the 10 percent level, a reduction coefficients of -0.012 in total robberies for the Full, 1.6km away and - 0.015 for the matching and 1.6km away. The same pattern of statically significant coefficients holds true for columns (9), (10), (12) with an implied reduction of 0.9%, 0.9% and -1.1%, respectively of th

To better access the overall Average Treatment Effect on the Treated (ATT), I aggregate the post-treatment coefficients $\beta_l \forall l \geq 0$ of the SADID estimator as explained in the Sun and Abraham (2021) in equation 3 but now the aggregation bin g is the whole period after treatment. i.e. $l \geq 0$. I do for each one of the different outcomes with each different sample. Table 3 presents the results of the post-treatment ATT aggregation for each outcome variable. Related to the variable of Total Robberies, the ATT coefficients exhibit a slight decrease, ranging from -0.005 to -0.006 between different samples, with one particular model (column 2) of the Sample of Controls 1.6km away from the treatment groups, having a statistically negative effect of -0.006 at the 10% level. Overall, we do not see a statistically significant effect except for the column, which, although significant, is only at 10% and negative. Exploring the results of Robberies per km, Table 3 reports all coefficients for the different samples being significant, with ATT coefficients spanning from -0.078 to -0.088. Although significant, the values are very small and would indicate a modest decrease in the number of vehicle robberies, again contradicting the critics. The extensive margin of the Dummy Robberies reveals ATT coefficients ranging between -0.003 and -0.004, with a significant reduction observed in column 10 at the 10% level. This denotes a slight but significant reduction in the probability of robbery occurrences due to the intervention, as inferred from the negative coefficients. Furthermore, the IHS Total Robberies exhibits the same ATT coefficients of -0.004 across multiple models, with a significance level of 10 % achieved in column 14 for the matching sample. The overall message from Table 3, the post-treatment aggregation for different outcomes, the aggregation shows the intervention had a null effect or, at most, a small and negative reduction in the measures of Vehicle robberies.

In conclusion, the analysis of the model across various samples reveals that speed limit reductions impact on vehicle robberies is largely negligible, with most coefficients for periods following the treatment not being statistically different from zero. Although there are isolated instances of small, statistically significant reductions at certain time points, these effects are minimal and do not suggest a strong or consistent impact of the policy. The event study plots reinforce the absence of pre-treatment trends and indicate that any post-treatment effects are either non-significant or, when significant, very close to zero and thus of limited practical relevance. The aggregation of post-treatment coefficients to assess the Average Treatment Effect on the Treated suggests only a slight potential decrease in robbery incidents, with most outcomes not showing statistically significant effects. Overall, the evidence suggests that the intervention did not have a positive impact on the vehicle robberies.

7.2 Effects on road segments with Speed Monitoring Cameras

For a more comprehensive understanding of the policy's effects and to isolate the impact of enforcement, I conducted a model estimation using a sample limited to road segments with a camera at any point during the pretreatment period. This approach yielded 2933 unique road segments, all in the treated group. This outcome is not surprising, considering the treated roads were the most heavily trafficked and therefore were the most likely to received the treatment. It's worth noting that the model does not include a never-treated group. Instead, the comparison group consists of the last cohort to be treated, as Sun and Abraham (2021) recommended. This distinction is important because it means the coefficient will differ slightly from that of Callaway and Sant'Anna (2021), based on never-treated units. In this specific case, the difference arises because the former uses the last treatment cohort as the comparison group, while the latter uses the never-treated group.

With that in mind, Figure 14 shows the event study for this subsample, which compares conditional on having a camera at any time during the pretreatment period. In panel a), I report the total robberies; in panel b) Robberies per kilometer; panel c) Prob of robbery; and d) the IHS transformation. An analysis of pretends shows for panel a) c) and only of relative time -12 does not include the zero, with the confidence interval failing to include it by a small margin, while for panel b), we can see all pretreatment coefficients are not statically different from zero.

Turning to the post-treatment coefficients, in panel a), all of them are statistically significant, indicating an apparent effect of the treatment on the total number of robberies. The same trend is observed in panel b), where all coefficients are within the interval $[-0.025, 0]$, suggesting a consistent but not significant change in the rate of robberies per kilometer. The only exception is at $t = 16$, which is slightly closer to -0.3, although it is not statistically significant. In the probability of robbery, all post-treatment coefficients are statistically insignificant, with the exception of $t = 9$, which has a coefficient of less than 0.10, which is very high but limited to only this period having an effect.

In conclusion, it is shown that, on average, treating roads did not necessarily lead to an additional risk increase in vehicle robbery, with the only execution being the coefficient of probability of robbery in $t = 9$ being approximately 0.08.

7.3 Heterogeneity

In this subsection, I perform different heterogeneity analysis estimations based on the period of the day a vehicle robbery happens and by type of vehicle. I use the sample from 2015 to 2016 as a reference.

7.3.1 Period of the day

To perform an heterogeneity analysis by period of the day, I estimate the same models as before by four different periods called Early Morning (00:00 AM to - 06:00 AM), Morning (06:00 AM - 12:00 PM), Afternoon (12:00 PM - 18:00 PM) and Night (18:00 PM - 00:00 PM). The outcome is estimated based on total robberies by the period of the day of the full sample. At first glance, a similar pattern in Figure 16 of not having pretrends on all panels except Panel a) of early morning. In this specific case, there is evidence of pretend since coefficients of relative periods $t = -9, -3, -2$ being statically different than zero, but the confidence interval upper bound is very close to zero. Note that the figure shows that after post-treatment, there is no effect, indicating that all periods except early morning (due to pretends) do not seem to have an effect. The main reason for not saying a) has a negative effect is that we are less precise on the effects since 3 out of 12 pre-treatment periods are statically different than zero,

In relation to panel b) for Morning all coefficients include the zero, so the post-treatment period shows to be not statically different than zero. The same holds for panel c) and d), afternoon and night, respectively. These effects seem to indicate the results are null even when restriction by period of the day, with exception of Early morning - although the pretrends are not entirely inexistant and the effects are null, the most conservative approach is take with caution the null results.

7.3.2 Vehicle type

In this more detailed section, I delve into a nuanced analysis of heterogeneity by categorizing vehicles into four distinct types: cars, motorcycles, trucks, and buses. I apply the same event study methodology as delineated in model 1 to assess total robbery incidents across the entire dataset. However, this time, the analysis is meticulously narrowed down to exclusively account for vehicle-related robbery, segmenting the data into the aforementioned four vehicle categories.

The graphical representation in Figure 18, specifically in panel a), elucidates the outcomes of modeling the data on car-related robberies. The findings here reveal that all coefficients are statistically insignificant, suggesting that the inherent security features of cars, such as their enclosed spaces, potentially offer a form of natural defense against theft, posing significant challenges for perpetrators.

Turning our attention to motorcycles, the situation presents more alarming insights. The data unmistakably exhibit several coefficients, specifically $-11, -10, -9, -8, -7, -5$, where the confidence intervals distinctly exclude zero. Despite the presence of some noise in the estimation, adopting a cautious stance refrains us from outright dismissing the impact on motorcycle thefts. It's noteworthy that the majority of these coefficients are negative, hinting at a trend.

On the contrary, the analysis for trucks and buses mirrors the pattern observed in cars, with no significant findings to report. This can be attributed to the relatively smaller sample sizes for these vehicle types, each comprising fewer than 1000 observations, which may constrain the robustness and reliability of the results in these categories.

8 Conclusion

This paper investigates a crucial question for policymakers: Does reducing speed limits increase vehicle robberies? A common concern is that slower traffic could create more opportunities for criminals. However, this paper presents compelling evidence that challenges this assumption.

The analysis utilized various samples and vehicle robbery measurements. The results consistently demonstrated that, in most cases, speed limit reductions did not lead to a statistically significant increase in vehicle robberies. In fact, the data suggests a possible slight decrease in robberies after implementing lower speed limits. This finding contradicts the common perception and highlights the need to re-evaluate the potential trade-off between traffic safety and robbery risk.

This research bears significant policy implications. Policymakers often grapple with the decision to implement lower speed limits, fearing potential spikes in robberies and electoral backlash. However, this paper's findings dispel in part these concerns. The results show that in the largest city of a developing country the speed limit reduction did not increase vehicle robbery. By removing this potential barrier, policymakers can be more confident in prioritizing traffic safety through speed limit reductions, reassured that this action may not adversely affect robbery rates and potential

While the findings are promising, this paper acknowledges its limitations. The results are specific to Sao Paulo and may not be universally applicable. Different cities have varying conditions and policing strategies that could influence the relationship between speed limits and robberies. This opens up new avenues for further research to explore the generalization of these findings in other contexts.

In conclusion, this paper challenges the traditional assumption that slower traffic leads to more robberies. The evidence from Sao Paulo suggests that speed limit reductions can be implemented without a significant increase in vehicle robberies. This finding encourages policymakers to re-evaluate their approach to traffic safety and prioritize lower speed limits that demonstrably save lives. Future research should focus on replicating these findings in other contexts and exploring newer estimators to better understand the complex relationship between traffic safety and crime.

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9 Figures and Tables

9.1 Figures

Figure 3: Map of Roads and Street with Speed Limit Reduction in São Paulo

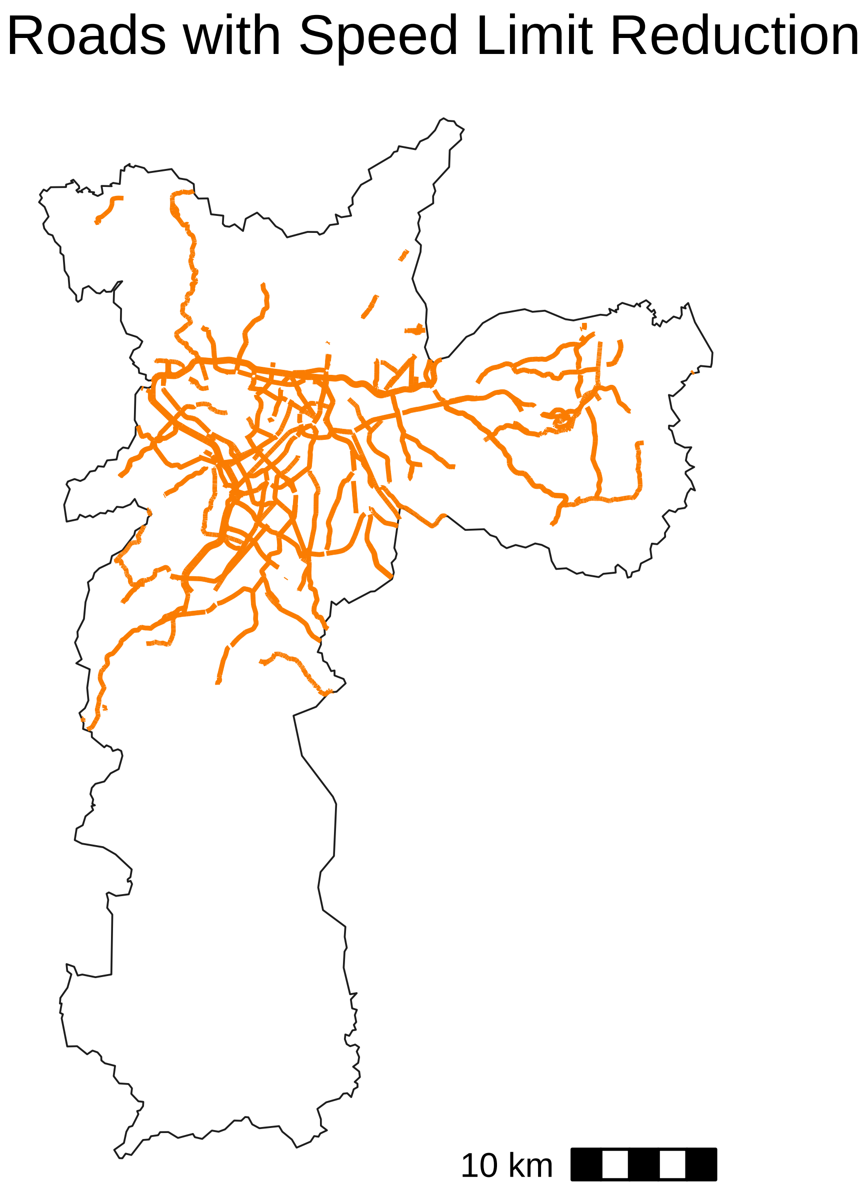


Figure 4: Cumulative Monthly Treated Road by Kilometers in São Paulo

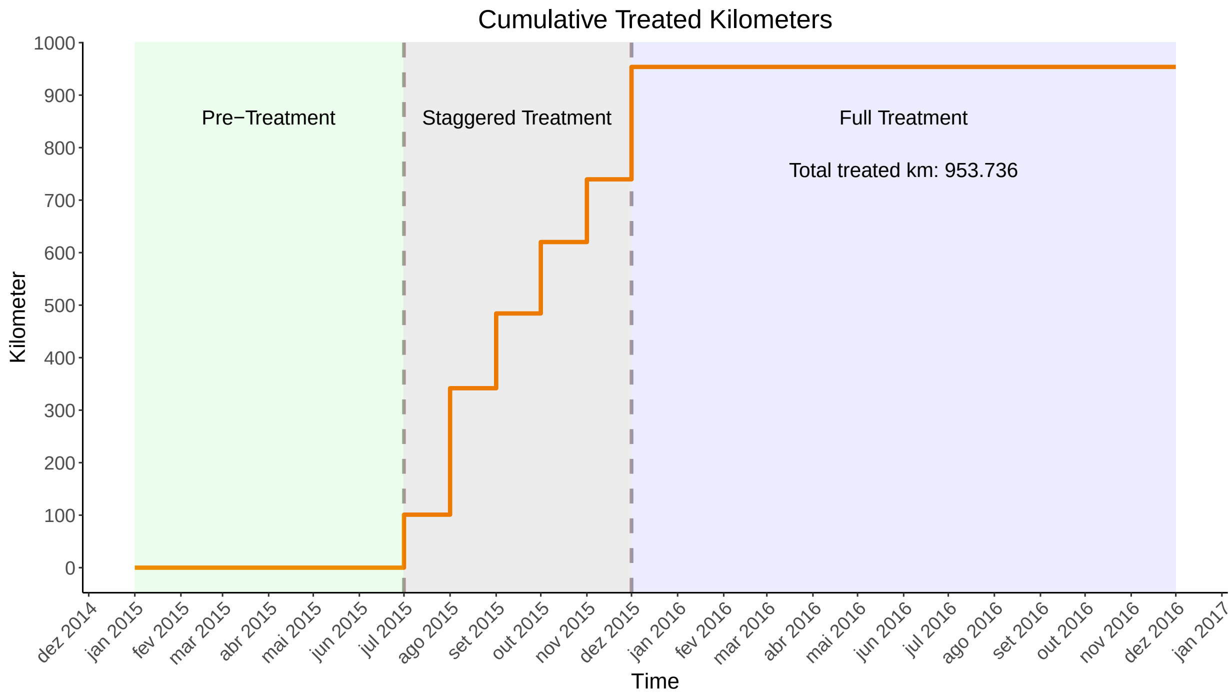


Figure 5: Monthly Total Vehicle Robberies over Time

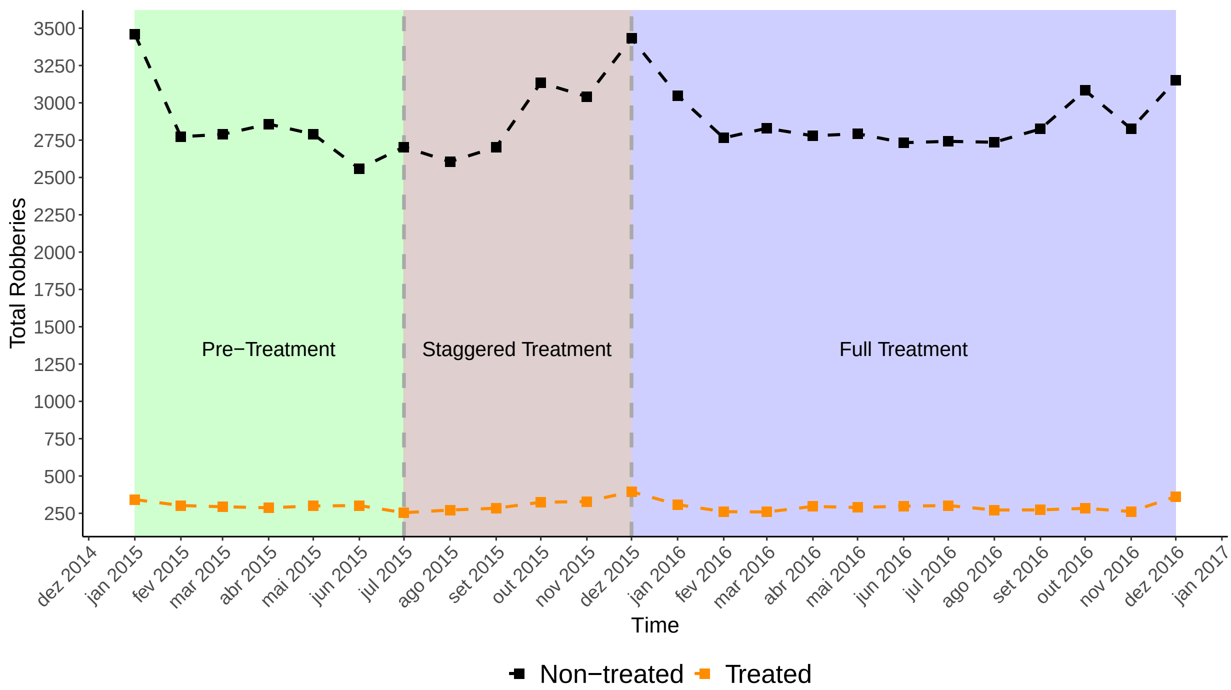
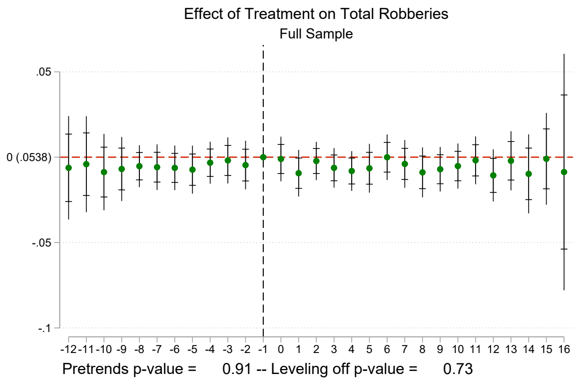
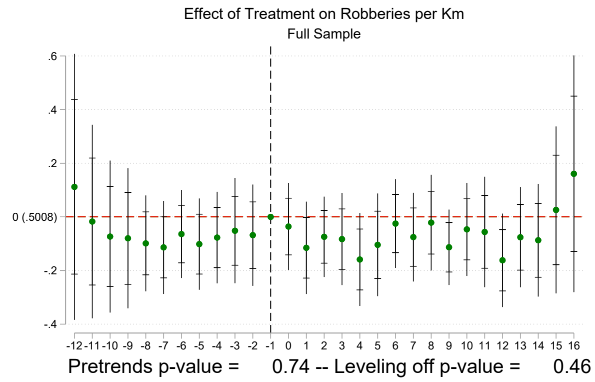


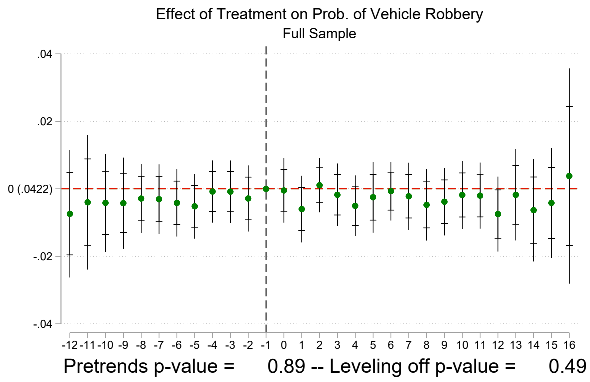
Figure 6: Event Study Plots - Full Sample



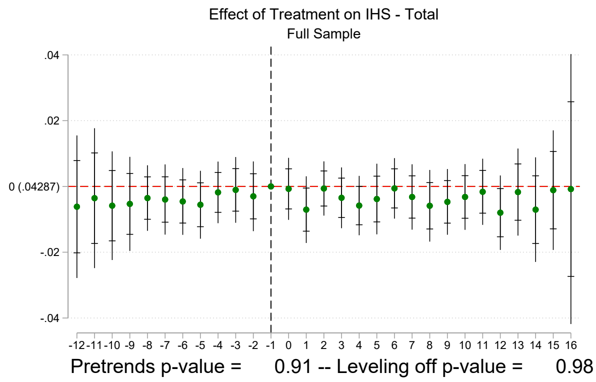
(a) Total Robberies



(b) Robberies per Km



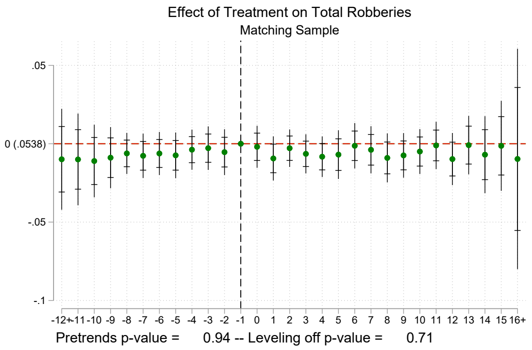
(c) Prob of Robbery



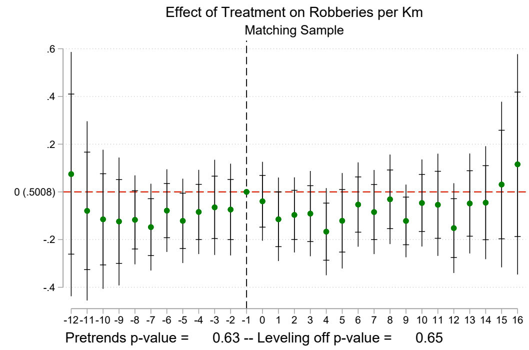
(d) IHS Total Robberies

This figure shows the event study graph on Full sample. Inner bars show the 95% point-wise confidence intervals while the outer bars show 95% uniform confidence intervals using the sup-t as in Montiel Olea and Plagborg-Møller (2018). The value in parenthesis at $y = 0$ represents the sample mean of the outcome variable $y_{i,t}$ one period before treatment, i.e., sample analog of $E(y_{it} \mid l = t - E_i = -1)$. Below it is reported the p-values of a Wald test for the absence of pre-event effects $(H_0 : \beta_k = 0, \quad -12 \leq k < -1)$ and p-values for testing the null that dynamics have leveled off (is not changing) after period 15 $(H_0 : \beta_15 = \beta_{15+k}, \quad 0 < k \leq 1)$. Panel a) reports outcome of Total Robberies, Panel b) reports for outcome of Total Robberies per km, Panel c) reports for outcome of Probability of Robberies and Panel d) reports for outcome of IHS - Total Robberiest.

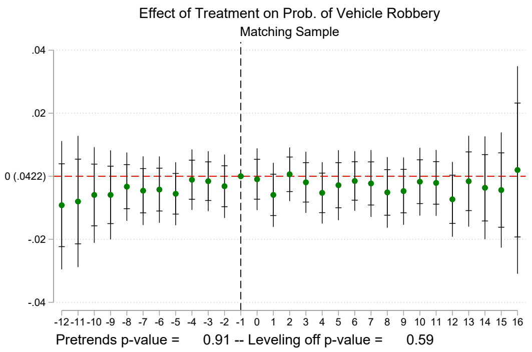
Figure 8: Event Study Plots - Matching



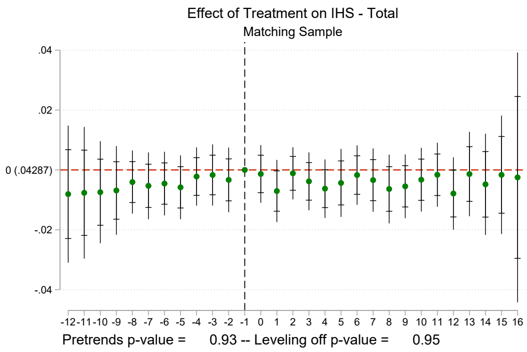
(a) Total Robberies



(b) Robberies per Km



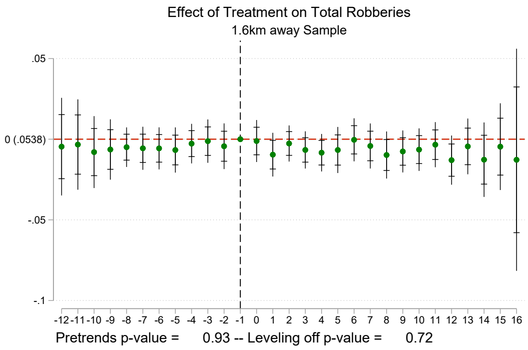
(c) Prob of Robbery



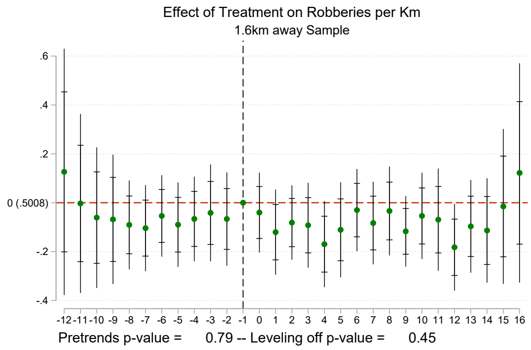
(d) IHS Total Robberies

This figure shows the event study graph on Matching victims in the 2014 sample. Inner bars show the 95% point-wise confidence intervals while the outer bars show 95% uniform confidence intervals using the sup-t as in Montiel Olea and Plagborg-Møller (2018). The value in parenthesis at $y = 0$ represents the sample mean of the outcome variable $y_{i,t}$ one period before treatment, i.e., sample analog of $E(y_{it} \mid l = t - E_i = -1)$. Below it is reported the p-values of a Wald test for the absence of pre-event effects ($H_0 : \beta_k = 0, \quad -12 \leq k < -1$) and p-values for testing the null that dynamics have leveled off (is not changing) after period 15 ($H_0 : \beta_{15} = \beta_{15+k}, \quad 0 < k \leq 1$) Panel a) reports for outcome of Total Robberies, Panel b) reports for outcome of Total Robberies per km, Panel c) reports for the outcome of Probability of Robberies and Panel d) reports for the outcome of IHS - Total Robberies.

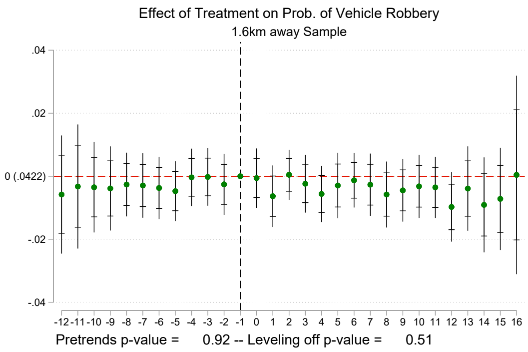
Figure 10: Event Study Plots - ≥ 1.6km away



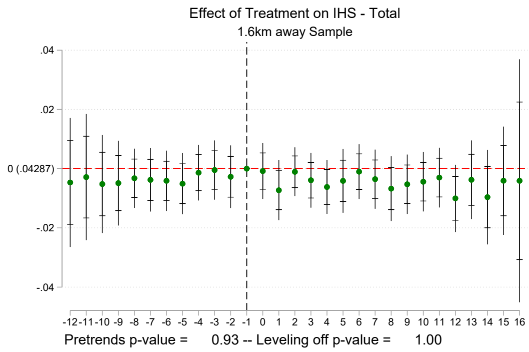
(a) Total Robberies



(b) Robberies per Km



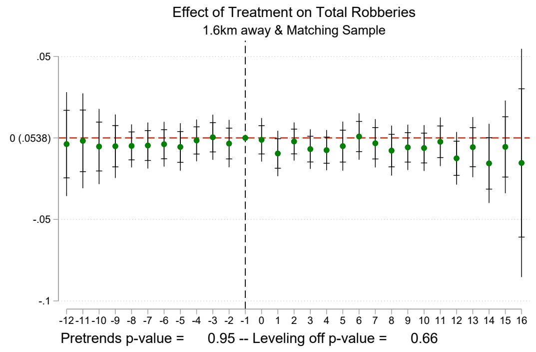
(c) Prob. of Robbery



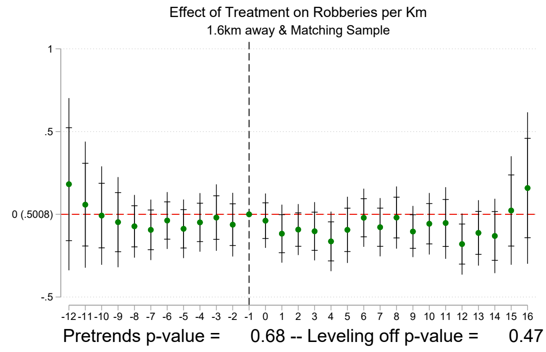
(d) IHS Total Robberies

This figure shows the event study graph on Controls farther than 1.6km away sample. Inner bars show the 95% point-wise confidence intervals while the outer bars show 95% uniform confidence intervals using the sup-t as in Montiel Olea and Plagborg-Møller (2018). The value in parenthesis at $y = 0$ represents the sample mean of the outcome variable $y_{i,t}$ one period before treatment, i.e., sample analog of $E(y_{it} \mid l = t - E_i = -1)$. Below it is reported the p-values of a Wald test for the absence of pre-event effects ($H_0 : \beta_k = 0, \quad -12 \leq k < -1$) and p-values for testing the null that dynamics have leveled off (is not changing) after period 15 ($H_0 : \beta_{15} = \beta_{15+k}, \quad 0 < k \leq 1$). Panel a) reports on the outcome of Total Robberies, Panel b) reports on the outcome of Total Robberies per km, Panel c) reports on the outcome of Probability of Robberies, and Panel d) reports on the outcome of IHS - Total Robberies.

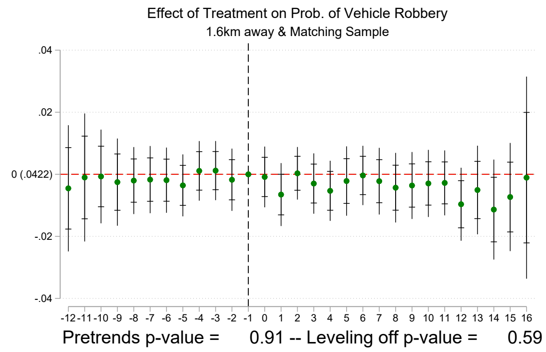
Figure 12: Event Study Plots - Matching and $\geq$ 1.6km away



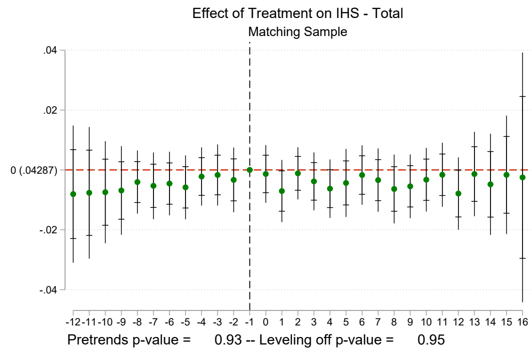
(a) Total Robberies



(b) Robberies per Km



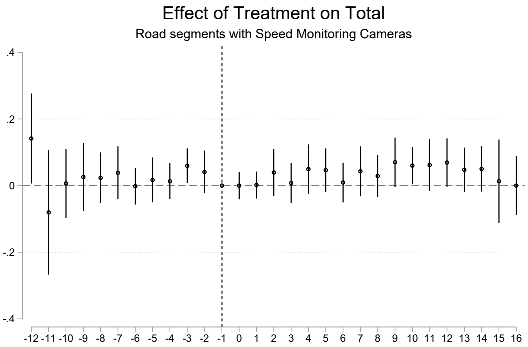
(c) Prob of Robbery



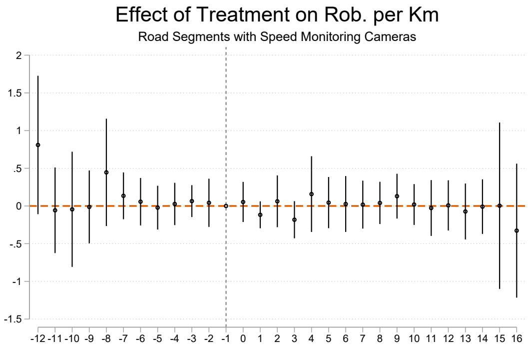
(d) IHS Total Robberies

This figure shows the event study graph on Controls, which is more than 1.6km away, and Matches the victims in the 2014 sample. Inner bars show the 95% point-wise confidence intervals while the outer bars show 95% uniform confidence intervals using the sup-t as in Montiel Olea and Plagborg-Møller (2018). The value in parenthesis at $y = 0$ represents the sample mean of the outcome variable $y_{i,t}$ one period before treatment, i.e., sample analog of $E(y_{it} \mid l = t - E_i = -1)$. Below it is reported the p-values of a Wald test for the absence of pre-event effects ($H_0 : \beta_k = 0, \quad -12 \leq k < -1$) and p-values for testing the null that dynamics have leveled off (is not changing) after period 15 ($H_0 : \beta_{15} = \beta_{15+k}, \quad 0 < k \leq 1$). Panel a) reports on the outcome of Total Robberies, Panel b) reports on the outcome of Total Robberies per km, Panel c) reports on the outcome of the Probability of Robberies, and Panel d) reports on the outcome of IHS - Total Robberies.

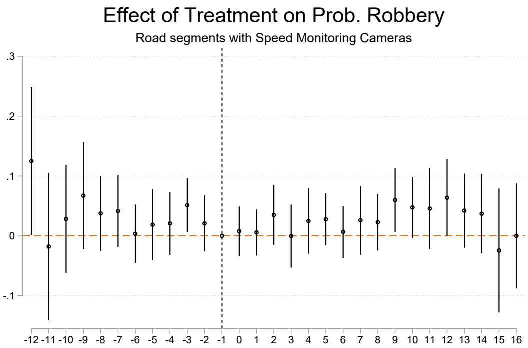
Figure 14: Event Study Plots - Restricted Sample on having Speed Limiting Camera



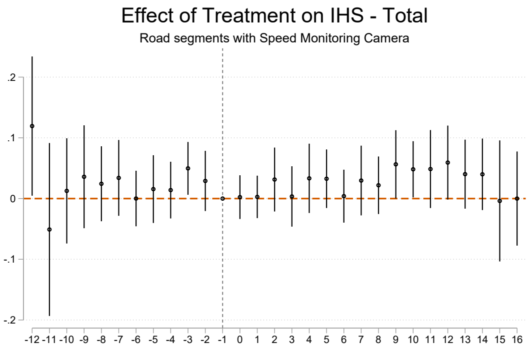
(a) Total Robberies



(b) Robberies per Km



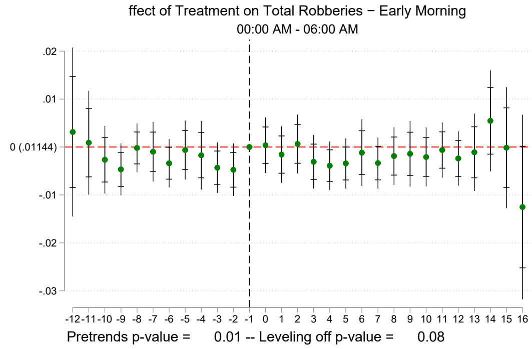
(c) Prob of Robbery



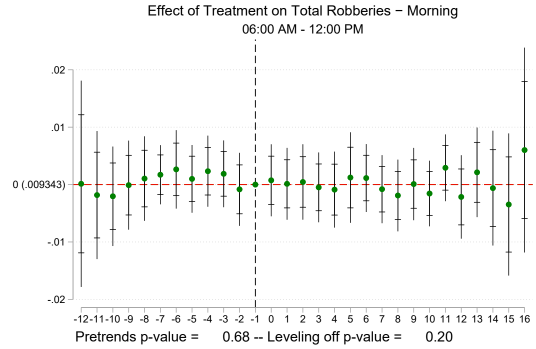
(d) IHS Total Robberies

This figure shows the event study graph on a sample of road segments conditional on having a speed monitoring camera in a period at any time of pre-treatment. Inner bars show the 95% point-wise confidence intervals while the outer bars show 95% uniform confidence intervals using the sup-t as in Montiel Olea and Plagborg-Møller (2018). The value in parenthesis at $y = 0$ represents the sample mean of the outcome variable $y_{i,t}$ one period before treatment, i.e., sample analog of $E(y_{it} \mid l = t - E_i = -1)$. Below it is reported the p-values of a Wald test for the absence of pre-event effects ($H_0 : \beta_k = 0, \quad -12 \leq k < -1$) and p-values for testing the null that dynamics have leveled off (is not changing) after period 15 ($H_0 : \beta_{15} = \beta_{15+k}, \quad 0 < k \leq 1$).. Panel a) reports on the outcome of Total Robberies, Panel b) reports on the outcome of Total Robberies per km, Panel c) reports on the outcome of the Probability of Robberies, and Panel d) reports on the outcome of IHS - Total Robberies.

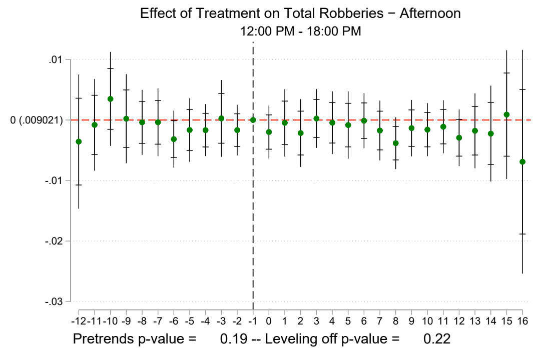
Figure 16: Event Study Plots - Period of the day



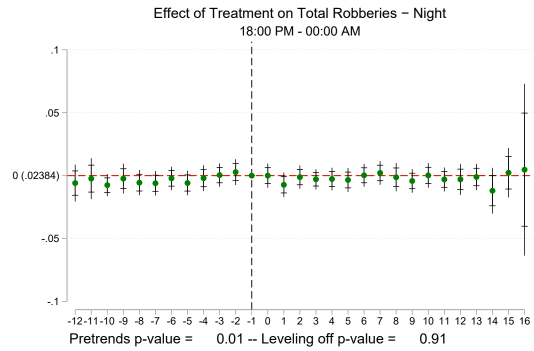
(a) Total Robberies - Early Morning (00:00 AM - 06:00 AM)



(b) Total Robberies - Morning (06:00 AM - 12:00 PM)



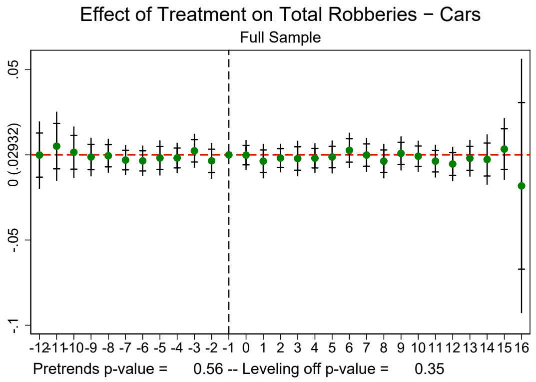
(c) Total Robberies - Afternoon (12:00 PM - 18:00 PM)



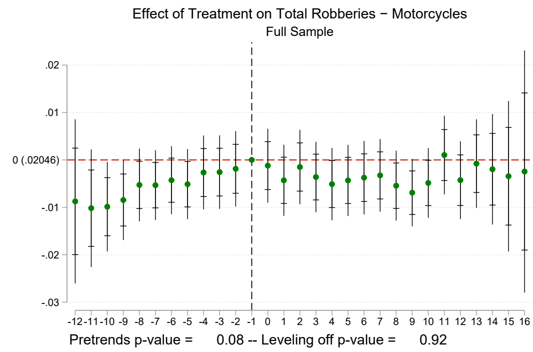
(d) Total Robberies - Night (18:00 PM - 00:00 PM)

This figure shows the event study graph on the full sample by the time of the day. Inner bars show the 95% point-wise confidence intervals while the outer bars show 95% uniform confidence intervals using the sup-t as in Montiel Olea and Plagborg-Møller (2018). The value in parenthesis at $y = 0$ represents the sample mean of the outcome variable $y_{i,t}$ one period before treatment, i.e., sample analog of $E(y_{it} \mid l = t - E_i = -1)$. Below it is reported the p-values of a Wald test for the absence of pre-event effects ($H_0 : \beta_k = 0, \quad -12 \leq k < -1$) and p-values for testing the null that dynamics have leveled off (is not changing) after period 15 ($H_0 : \beta_{15} = \beta_{15+k}, \quad 0 < k \leq 1$). Panel a) reports the outcome restricted for the Early Morning period (00:00 AM - 06:00 AM), Panel b) reports for the Morning period (06:00 AM - 12:00 PM), Panel c) for the Afternoon period (12:00 PM - 18:00 PM) and Panel d) Night period (18:00 PM - 00:00 PM).

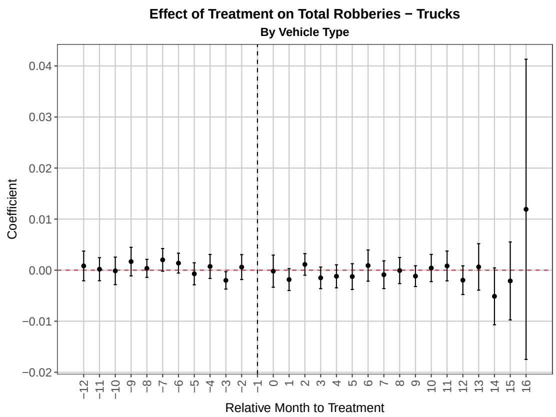
Figure 18: Event Study Plots by Vehicle Type



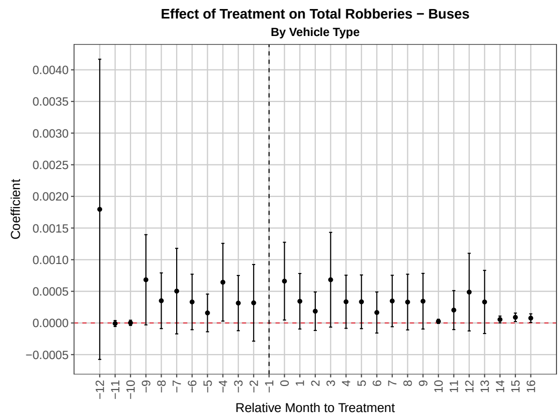
(a) Total Robberies - Cars



(b) Total Robberies - Motorcycles



(c) Total Robberies - Trucks



(d) Total Robberies - Buses

This figure shows the event study graph on full sample by type of vehicle. Inner bars show the 95% point-wise confidence intervals while the outer bars show 95% uniform confidence intervals using the sup-t as in Montiel Olea and Plagborg-Møller (2018). The value in parenthesis at $y = 0$ represents the sample mean of the outcome variable $y_{i,t}$ one period before treatment, i.e., sample analog of $E(y_{it} \mid l = t - E_i = -1)$. Below it is reported the p-values of a Wald test for the absence of pre-event effects ($H_0 : \beta_k = 0, \quad -12 \leq k < -1$) and p-values for testing the null that dynamics have leveled off (is not changing) after period 15 ($H_0 : \beta_{15} = \beta_{15+k}, \quad 0 < k \leq 1$). The outcome variable is total robbery. Panel a) reports total for Cars, Panel b) for motorcycles, Panel c) for Trucks and Panel d) Buses.

9.2 Tables

Table 1: Summary of Missing Observations

Description	Count
Total raw observations	130312
Entirely removed observations without address (unverifiable)	7890
Observations with address but missing coordinates (coordinates retrieved using tidy geocoder)	9422
Total observations after address filtering, coordinate retrieval, and São Paulo boundary filtering	121155

The table describes the total count of individual reported vehicle observations, observations that do not possesses addresses, observations without geographic coordinates (latitude and longitude) and final total crime observations after removing all observations that are not within the Sao Paulo City Limits.

Table 2: Summary statistics

Panel A. Treatment Detail						
Observations	Control		Treated			
	157,245		6,208			
Panel B. Road Segment Characteristics						
Variable	Mean		SD			
	Control	Treated	Control	Treated		
Length	99.61	153.63	114.26	194.03		
Car Restriction	0.14	0.40	0.35	0.49		
Truck Restriction	0.10	0.26	0.30	0.44		
Freight Restriction	0.07	0.24	0.26	0.43		
Traffic Light	0.03	0.14	0.18	0.35		
Speed Bump	0.06	0.02	0.23	0.13		
Panel C. Crime Outcomes						
	2014		2015		2016	
	Control	Treated	Control	Treated	Control	Treated
Total Vehicles	0.251	0.721	0.222	0.593	0.218	0.558
Cars	0.181	0.401	0.168	0.344	0.166	0.326
Motorcycles	0.052	0.241	0.044	0.194	0.043	0.187
Microbuses and Buses	0.001	0.003	0.001	0.004	0.001	0.003
Trucks	0.009	0.061	0.007	0.049	0.007	0.040
Others	0.008	0.016	0.001	0.002	0.001	0.002
Panel D. Accidents Categories						
	2014		2015		2016	
	Control	Treated	Control	Treated	Control	Treated
Injured	0.138	0.930	0.118	0.776	0.099	0.446
Fatalities	0.006	0.040	0.005	0.030	0.004	0.024
Accidents with Victims	0.089	0.618	0.077	0.529	0.066	0.314
Accident hit someone	0.030	0.174	0.026	0.135	0.021	0.072

The summary table shows some descriptive statistics of the Road Segments and treatment status for the 2014-2016 period. Panel A shows the size of treatment and control groups. Panel B Reports characteristics . Panel C and Panel D reports means per segment of crime outcomes and accident categories.

Table 3: Event Study Coefficients - ATT aggregation - All samples

	Total Robberies				Robberies per km				Dummy Robberies				IHS Total Robberies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Variables																
ATT	-0.005	-0.006*	-0.005	-0.005	-0.078*	-0.088**	-0.083*	-0.087**	-0.003	-0.004*	-0.003	-0.004	-0.004	-0.004*	-0.004	-0.004
	(0.003)	(0.003)	(0.004)	(0.003)	(0.042)	(0.042)	(0.044)	(0.044)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample																
	Full	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**
Fit statistics																
Observations	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984
R^2	0.17716	0.20040	0.25841	0.24508	0.11750	0.14805	0.18301	0.18766	0.11905	0.15738	0.18900	0.19548	0.13821	0.18426	0.22337	0.22806
Within R^2	0.00012	0.00039	0.00069	0.00070	6.39×10^{-5}	0.00020	0.00051	0.00052	8.77×10^{-5}	0.00032	0.00064	0.00063	0.00010	0.00036	0.00068	0.00068

Clustered standard-errors at Street Name level in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table presents the estimated aggregated Average Treatment Effect on the Treated (ATT). The ATT is aggregated across the coefficients for periods $l \geq 0$, utilizing the cohort sizes as weights for the aggregation. Columns (1)-(4) display the aggregated ATT for Total Robberies across different samples. Columns (5)-(8) detail the aggregated ATT for Robberies per Km, columns (9)-(12) for the Probability of Robberies, and columns (13)-(16) for the Inverse Hyperbolic Sine Transformation of Total Robberies.

Table 4: Event Study Coefficients - Cohort ATT aggregation - All samples

	Total Robberies				Robberies per km				Dummy Robberies				IHS Total Robberies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
ATT by Cohort of Treatment																
July 2015	0.008	0.006	0.004	0.007	0.075*	0.063	0.059	0.049	0.009	0.007	0.005	0.008	0.009	0.008	0.005	0.008
	(0.017)	(0.017)	(0.017)	(0.017)	(0.045)	(0.046)	(0.053)	(0.052)	(0.011)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
August 2015	-0.007	-0.008	-0.009	-0.007	-0.070	-0.088	-0.084	-0.047	-0.003	-0.005	-0.005	-0.003	-0.004	-0.005	-0.006	-0.004
	(0.006)	(0.006)	(0.007)	(0.007)	(0.065)	(0.066)	(0.070)	(0.073)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
September 2015	-0.003	-0.004	-0.006	0.0003	-0.165	-0.186*	-0.243**	-0.170	-0.003	-0.005	-0.007	-0.002	-0.004	-0.005	-0.007	-0.002
	(0.011)	(0.011)	(0.011)	(0.011)	(0.106)	(0.107)	(0.108)	(0.110)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
October 2015	-0.006	-0.007	-0.006	-0.004	-0.014	-0.030	-0.070	-0.010	-0.002	-0.004	-0.003	-0.002	-0.004	-0.005	-0.004	-0.003
	(0.008)	(0.008)	(0.008)	(0.008)	(0.098)	(0.098)	(0.100)	(0.104)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
November 2015	0.005	0.005	0.008	0.005	-0.066	-0.060	0.011	-0.056	0.005	0.005	0.007	0.004	0.004	0.004	0.006	0.004
	(0.008)	(0.008)	(0.008)	(0.008)	(0.093)	(0.094)	(0.101)	(0.099)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
December 2015	-0.012**	-0.012**	-0.010	-0.015**	-0.119	-0.117	-0.080	-0.173	-0.009**	-0.009**	-0.008	-0.011**	-0.010**	-0.010**	-0.008	-0.012**
	(0.006)	(0.006)	(0.007)	(0.007)	(0.118)	(0.119)	(0.126)	(0.121)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample																
	Full	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**
Fit statistics																
Observations	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984
R^2	0.17716	0.20040	0.25841	0.24508	0.11750	0.14805	0.18301	0.18766	0.11905	0.15738	0.18900	0.19548	0.13821	0.18426	0.22337	0.22806
Within R^2	0.00012	0.00039	0.00069	0.00070	6.39×10^{-5}	0.00020	0.00051	0.00052	8.77×10^{-5}	0.00032	0.00064	0.00063	0.00010	0.00036	0.00068	0.00068

Clustered standard-errors at Street Name level in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

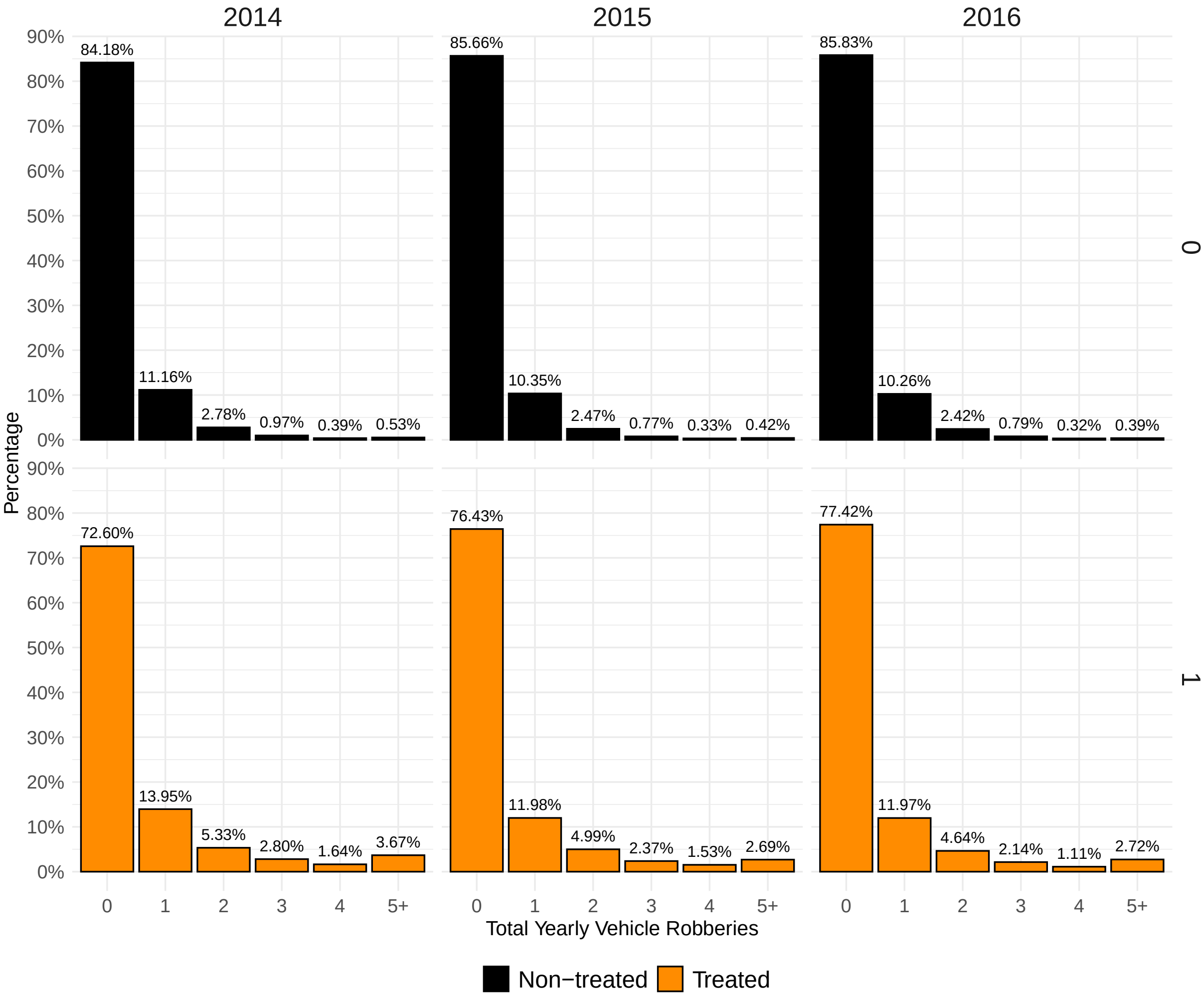
Note: This table presents the estimated aggregated Average Treatment Effect on the Treated (ATT) by cohort of treatment (July 2015 to December 2015). The ATT is aggregated across the coefficients for periods $l \geq 0$ and , utilizing the cohort sizes as weights for the aggregation. Columns (1)-(4) display the cohort aggregated ATT for Total Robberies across different samples. Columns (5)-(8) detail the aggregated cohort ATT for Robberies per Km, columns (9)-(12) for the Probability of Robberies, and columns (13)-(16) for the Inverse Hyperbolic Sine Transformation of Total Robberies.

A Other Figures and Tables

A.1 Figures

Figure 20: Example of São Paulo Municipal Government Announcement

Figure 21: Yearly Reported Vehicle Robberies by Segment



A.2 Tables

Sample	Mean Treated	Mean Control
Matching	0.6178	0.620
Matching with 1.6km	0.6178	0.3558

Table 5: Balance Table - Matching on Number of reported victims

Table 6: Event Study Coefficients Results - Post-Treatment - All samples

	Total Robberies				Robberies per km				Dummy Robberies				IHS Total Robberies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Relative month after Treatment																
0	-0.001	-0.001	-0.002	-0.0009	-0.036	-0.040	-0.040	-0.041	-0.0005	-0.0006	-0.0010	-0.0008	-0.0007	-0.0008	-0.001	-0.0008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.053)	(0.053)	(0.054)	(0.054)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
1	-0.009**	-0.010**	-0.009**	-0.009**	-0.115**	-0.120**	-0.115**	-0.119**	-0.006*	-0.006**	-0.006*	-0.007**	-0.007**	-0.007**	-0.007**	-0.007**
	(0.004)	(0.004)	(0.005)	(0.005)	(0.056)	(0.057)	(0.057)	(0.057)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
2	-0.002	-0.003	-0.003	-0.002	-0.074	-0.081*	-0.096*	-0.094*	0.001	0.0005	0.0006	0.0003	-0.0006	-0.001	-0.001	-0.0008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.049)	(0.049)	(0.051)	(0.050)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
3	-0.006*	-0.007*	-0.006	-0.007*	-0.083	-0.092	-0.091	-0.105*	-0.002	-0.002	-0.002	-0.003	-0.003	-0.004	-0.004	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)	(0.056)	(0.056)	(0.058)	(0.058)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
4	-0.008**	-0.008**	-0.008**	-0.007*	-0.159***	-0.170***	-0.167***	-0.166***	-0.005*	-0.006*	-0.005*	-0.005*	-0.006**	-0.006**	-0.006**	-0.006*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.057)	(0.057)	(0.060)	(0.059)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
5	-0.006	-0.007	-0.007	-0.005	-0.104*	-0.111*	-0.121*	-0.096	-0.002	-0.003	-0.003	-0.002	-0.004	-0.004	-0.004	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.063)	(0.063)	(0.066)	(0.065)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
6	-4.38×10^{-5}	-0.0004	-0.001	0.001	-0.025	-0.030	-0.053	-0.023	-0.0007	-0.001	-0.002	-0.0004	-0.0006	-0.001	-0.002	-2.28×10^{-5}
	(0.004)	(0.004)	(0.005)	(0.005)	(0.054)	(0.055)	(0.058)	(0.058)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
7	-0.004	-0.004	-0.004	-0.003	-0.075	-0.083	-0.085	-0.080	-0.002	-0.003	-0.002	-0.002	-0.003	-0.004	-0.003	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.054)	(0.055)	(0.058)	(0.058)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
8	-0.009*	-0.010**	-0.009*	-0.008	-0.021	-0.034	-0.031	-0.022	-0.005	-0.006*	-0.005	-0.004	-0.006*	-0.007*	-0.006*	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.058)	(0.059)	(0.061)	(0.062)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
9	-0.007*	-0.008*	-0.007	-0.006	-0.113**	-0.117**	-0.122**	-0.106**	-0.004	-0.005	-0.005	-0.004	-0.005	-0.005*	-0.005	-0.004
	(0.004)	(0.004)	(0.005)	(0.004)	(0.046)	(0.047)	(0.050)	(0.050)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
10	-0.005	-0.006	-0.005	-0.006	-0.047	-0.054	-0.046	-0.059	-0.002	-0.003	-0.002	-0.003	-0.003	-0.004	-0.003	-0.004
	(0.004)	(0.004)	(0.005)	(0.005)	(0.057)	(0.058)	(0.060)	(0.061)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
11	-0.002	-0.003	-0.001	-0.002	-0.056	-0.069	-0.054	-0.055	-0.002	-0.004	-0.002	-0.003	-0.002	-0.003	-0.002	-0.002
	(0.005)	(0.005)	(0.005)	(0.005)	(0.068)	(0.068)	(0.070)	(0.071)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
12	-0.011**	-0.013***	-0.010*	-0.012**	-0.162***	-0.182***	-0.152**	-0.183***	-0.007**	-0.010***	-0.007*	-0.010**	-0.008**	-0.010***	-0.008**	-0.010**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.057)	(0.058)	(0.061)	(0.061)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
13	-0.002	-0.005	-0.0009	-0.005	-0.076	-0.097	-0.049	-0.117*	-0.002	-0.004	-0.002	-0.005	-0.002	-0.004	-0.001	-0.005
	(0.006)	(0.006)	(0.006)	(0.006)	(0.061)	(0.062)	(0.068)	(0.065)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)
14	-0.010	-0.013*	-0.007	-0.015*	-0.087	-0.114	-0.045	-0.137*	-0.006	-0.009*	-0.004	-0.011**	-0.007	-0.010*	-0.005	-0.012**
	(0.008)	(0.007)	(0.008)	(0.008)	(0.069)	(0.069)	(0.077)	(0.074)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
15	-0.0010	-0.005	-0.001	-0.005	0.026	-0.015	0.031	0.013	-0.004	-0.007	-0.004	-0.007	-0.001	-0.004	-0.002	-0.004
	(0.009)	(0.009)	(0.009)	(0.009)	(0.102)	(0.103)	(0.114)	(0.107)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
16	-0.009	-0.013	-0.010	-0.015**	0.161	0.122	0.116	0.119	0.004	0.0004	0.002	0.002	-0.0008	-0.004	-0.002	-0.003
	(0.022)	(0.023)	(0.023)	(0.023)	(0.145)	(0.146)	(0.151)	(0.151)	(0.010)	(0.010)	(0.011)	(0.011)	(0.013)	(0.013)	(0.013)	(0.013)
	Full	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**	**Full**	**$\geq$ 1.6km**	**Matching**	**Matching & $\geq$ 1.6km**
Fit statistics																
Observations	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984	3,917,880	1,039,008	297,984	297,984
R^2	0.17716	0.20040	0.25841	0.24508	0.11750	0.14805	0.18301	0.18766	0.11905	0.15738	0.18900	0.19548	0.13821	0.18426	0.22337	0.22806
Within R^2	0.00012	0.00039	0.00069	0.00070	6.39×10^{-5}	0.00020	0.00051	0.00052	8.77×10^{-5}	0.00032	0.00064	0.00063	0.00010	0.00036	0.00068	0.00068

Clustered standard-errors at Street Name level in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

[a] The Inverse Hyperbolic Sine Transformation is given by the formula: $\text{IHS}(x) = \ln(x + \sqrt{x^2 + 1})$.
This table presents a level analysis following the methodology described by Sun and Abraham (2021). Columns (1)-(4) reports total robberies on the segment by differing sample, columns (5)-(8) represent robberies per kilometer per segment, columns (9)-(12) utilize a dummy variable for segments reporting at least one robbery in a month, and columns (12)-(16) apply a IHS transformation to total robberies.
Standard errors are clustered at the Street Name level to account for within-group correlation.

B Dates of Speed Limit Changes

In the next pages are the date of speed limits changes.

Announcement Date	Speed Limit Change Date	Name of Treated Roads, Street or Avenues	Announcement Link
2015-07-08	2015-07-20	AV MARGINAL DO RIO TIETE, AV MARGINAL DO RIO TIETE, AV MARGINAL DO RIO PINHEIROS	Link
2015-07-30	2015-08-03	AV JACU-PESSEGO, AV JACU-PESSEGO, AV ARICANDUVA, VD ENG ALBERTO BADRA, AV S JOAO, AV GAL OLIMPIO DA SILVEIRA, RUA AMARAL GURGEL	Link
2015-08-12	2015-08-17	AV ANGELICA, AV ANGELICA, AV NADIR DIAS DE FIGUEIREDO, RUA MAJ NATANAEL, AV DR ABRAAO RIBEIRO, AV PACAEMBU	Link
2015-08-17	2015-08-20	AV AFRANIO PEIXOTO, AV VALDEMAR FERREIRA, RUA HENRIQUE SCHAUMANN, AV PAULO VI, AV SUMARE, AV ANTARTICA, AV PROF MANUEL JOSE CHAVES, AV CARLOS CALDEIRA FILHO, AV VER JOSE DINIZ, ES DO CAMPO LIMPO	Link
2015-08-20	2015-08-23	RUA DOMINGOS DE MORAIS, AV GUARAPIRANGA, ES M'BOI MIRIM, AV SEN TEOTONIO VILELA, AV ARNOLFO AZEVEDO, RUA ALM PEREIRA GUIMARAES, RUA DOMINGOS DE MORAIS	Link
2015-08-24	2015-08-27	AV PEDROSO DE MORAIS, AV PROF FONSECA RODRIGUES, AV DR GASTAO VIDIGAL	Link

Announcement Date	Speed Limit Change Date	Name of Treated Roads, Street or Avenues	Announcement Link
2015-08-27	2015-08-31	PTE ENG ARY TORRES, AV DOS BANDEIRANTES, AV AFFONSO D'ESCRAGNOLLE TAUNAY, CV MARIA MALUF, AV SANTOS DUMONT, AV TIRADENTES, AV PRESTES MAIA, TN PAPA JOAO PAULO II, AV VINTE E TRES DE MAIO, AV RUBEM BERTA, AV MOREIRA GUIMARAES, AV WASHINGTON LUIS, AV INTERLAGOS, AV WASHINGTON LUIS	Link
2015-09-03	2015-09-09	AV SALIM FARAH MALUF, AV JUNTAS PROVISORIAS, RUA MALVINA FERRARA SAMARONE, AV PRES TANCREDO NEVES	Link
2015-09-04	2015-09-11	AV FRANCISCO MATARAZZO, VD LESTE-OESTE, AV ALCANTARA MACHADO, RUA MELO FREIRE, AV CD DE FRONTIN, AV ANTONIO ESTEVAO DE CARVALHO, RUA DR LUIZ AYRES, RUA ENG SIDNEY APARECIDO DE MORAES, AV JOSE PINHEIRO BORGES	Link
2015-09-14	2015-09-18	RUA CARMOPOLIS DE MINAS, AV BANDEIRANTES DO SUL, RUA CEL GUILHERME ROCHA, RUA CIRO SOARES DE ALMEIDA, AV OLAVO FONTOURA, AV EDUC PAULO FREIRE	Link
2015-09-18	2015-09-23	AV PEDRO ALVARES CABRAL, AV BRASIL, AV JABAQUARA, AV JABAQUARA	Link
2015-09-22	2015-09-25	AV DO ESTADO, AV DO ESTADO, AV ATLANTICA	Link
2015-09-24	2015-09-30	AV VITOR MANZINI, PTE DO SOCORRO	Link

Announcement Date	Speed Limit Change Date	Name of Treated Roads, Street or Avenues	Announcement Link
2015-09-30	2015-10-02	AV DOM PEDRO I, RUA TEREZA CRISTINA, AV NAZARE, AV DR RICARDO JAFET, AV DR RICARDO JAFET, AV PROF ABRAAO DE MORAIS	Link
2015-10-01	2015-10-07	RUA MANUEL DA NOBREGA, AV REPUBLICA DO LIBANO, AV INDIANOPOLIS	Link
2015-10-06	2015-10-09	AV BRIG FARIA LIMA, RUA DOS PINHEIROS, AV HELIO PELEGRINO, RUA INHAMBU, TN SEBASTIAO CAMARGO, AV PRES JUSCELINO KUBITSCHEK, CV TRIBUNAL DE JUSTICA, RUA ANTONIO MOURA ANDRADE, CV AYRTON SENNA	Link
2015-10-09	2015-10-14	AV PRES WILSON, RUA S RAIMUNDO, RUA S RAIMUNDO, RUA MANOEL PEREIRA DA SILVA, RUA MANOEL PEREIRA DA SILVA, AV DR FRANCISCO MESQUITA	Link
2015-10-14	2015-10-16	AV REBOUCAS, AV EUSEBIO MATOSO, TN JORN FERNANDO VIEIRA DE MELO	Link
2015-10-16	2015-10-21	AV PROF FRANCISCO MORATO, AV EMERICO RICHTER	Link
2015-10-20	2015-10-23	AV DR ARNALDO, AV JORN ROBERTO MARINHO, PTE OCTAVIO FRIAS DE OLIVEIRA, AV JOAO SIMAO DE CASTRO	Link
2015-10-22	2015-10-28	AV ROQUE PETRONI JUNIOR, AV PROF VICENTE RAO, AV VER JOAO DE LUCA, RUA JUAN DE LA CRUZ, AV CUPECE	Link
2015-10-26	2015-10-30	AV DR HUGO BEOLCHI, AV ENG ARMANDO DE ARRUDA PEREIRA, AV ENG GEORGE CORBISIER	Link

Announcement Date	Speed Limit Change Date	Name of Treated Roads, Street or Avenues	Announcement Link
2015-10-29	2015-11-04	AV CORIFEU DE AZEVEDO MARQUES, AV VITAL BRASIL, AV DOS TAJURAS, TN PRES JANIO QUADROS, AV LINEU DE PAULA MACHADO	Link
2015-11-04	2015-11-06	PTE ENG ROBERTO ROSSI ZUCCOLO, AV CIDADE JARDIM, TN MAX FEFFER, AV EUROPA, RUA COLOMBIA, RUA AUGUSTA, RUA NOVE DE JULHO	Link
2015-11-16	2015-11-19	AV ELISEU DE ALMEIDA, RUA PIRAJUSSARA, AV INTERCONTINENTAL, AV JAGUARE, AV ESCOLA POLITECNICA, AV ESCOLA POLITECNICA, AV DR ANTONIO MARIA LAET, AV DR ANTONIO MARIA LAET, RUA PARANABI, RUA ARARITAGUABA, RUA ARARITAGUABA, AV DO POETA	Link
2015-11-19	2015-11-25	AV S GABRIEL, AV SANTO AMARO, AV JOAO DIAS, AV ADOLFO PINHEIRO, RUA RHONE, AV ADUTORA DO RIO CLARO	Link
2015-11-23	2015-11-27	AV MIGUEL IGNACIO CURI, RUA CASTELO DO PIAUI, AV RAGUEB CHOHFI, ES IGUATEMI	Link
2015-11-27	2015-12-02	AV PAES DE BARROS, RUA TAQUARI, RUA BRESSER, VD BRESSER, AV BERNARDINO BRITO FONSECA DE CA, AV BERNARDINO BRITO FONSECA DE CA, AV PROF EDGAR SANTOS, AV PROF EDGAR SANTOS, AV ITAQUERA	Link

Announcement Date	Speed Limit Change Date	Name of Treated Roads, Street or Avenues	Announcement Link
2015-12-02	2015-12-04	AV PIRES DO RIO, AV DEP JOSE ARISTODEMO PINOTTI, AV DEP JOSE ARISTODEMO PINOTTI, ES DO IMPERADOR, ES DE MOGI DAS CRUZES, RUA EMBIRA, AV S MIGUEL	Link
2015-12-04	2015-12-09	RUA DR ASSIS RIBEIRO, AV VER ABEL FERREIRA, RUA BRIG GAVIAO PEIXOTO, RUA MONTE PASCAL, VD DOMINGOS DE MORAES, AV GAL EDGAR FACO	Link
2015-12-09	2015-12-11	AV INAJAR DE SOUZA, AV INAJAR DE SOUZA, AV COMEN MARTINELLI, AV ERMANO MARCHETTI, AV MARQ DE SAO VICENTE, RUA SERGIO TOMAS, RUA NORMA PIERUCCINI GIANNOTTI, AV RUDGE, VD ENG ORLANDO MURGEL, AV RIO BRANCO, AV ORDEM E PROGRESSO, PTE JULIO DE MESQUITA NETO, AV NICOLAS BOER, VD POMPEIA, AV ALEXANDRE COLARES, AV MANOEL MONTEIRO DE ARAUJO, AV DOMINGOS DE SOUZA MARQUES, AV ALM DELAMARE, RUA ANCHIETA, RUA FUNCHAL, AV CHEDID JAFET	Link
2015-12-14	2015-12-16	AV SARG MIGUEL DE SOUSA FILHO, AV TTE AMARO FELICISSIMO DA SILVEIRA, AV TTE AMARO FELICISSIMO DA SILVEIRA, AV SERAFIM GONCALVES PEREIRA, AV MORUMBI	Link

Announcement Date	**Speed Limit Change Date**	**Name of Treated Roads, Street or Avenues**	**Announcement Link**
2015-12-16	2015-12-18	RUA MANOEL BARBOSA, AV RAIMUNDO PEREIRA DE MAGAL, RUA PRINCIPAL(PERUS), RUA GUIDO CALOI, AV GIOVANNI GRONCHI, ES DO ALVARENGA, RUA DR JOSE MARIA WHITAKER, RUA ALVINOPOLIS, AV ANTONIO BATUIRA, AV QUEIROZ FILHO, RUA CERRO CORA, RUA CERRO CORA, RUA CERRO CORA, RUA CONS MOREIRA DE BARROS, RUA MAUA, AV DUQ DE CAXIAS	Link
2015-12-30	2015-12-29	AV LUIZ GUSHIKEN	Link