The effectiveness of speed enforcement policies in congested cities

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Abstract

Large, congested cities face a persistent dilemma: how to reduce motor vehicle accidents without worsening traffic. Speed reduction policies such as slow zones, speed bumps, and automated monitoring systems are designed to enforce lower driving speeds, but their spatial effects remain poorly understood. This study proceed an empirical and theoretical estimation of speed policy effects. I estimate the effects of these interventions using a spatial difference-in-differences design, leveraging high-frequency, street-level data on accidents, injuries, and fatalities. The analysis exploits variation in proximity to the street segments where each policy was implemented. The results reveal heterogeneous impacts by policy type. Automated enforcement via speed cameras is associated with increases in accidents and injuries in treated areas, alongside positive spillovers to nearby streets. In contrast, moderate-intensity measures such as slow zones and speed humps show modest, suggestive reductions in accidents and injuries, with benefits extending beyond treated segments. No statistically significant effects are found for fatalities. These findings highlight the importance of tailoring traffic safety interventions to the urban context and accounting for both localized and spillover effects when designing policy.

Keywords: Urban Mobility, Traffic Accidents; Law Enforcement; Safe Traffic.

JEL Classification: RH23, R41, R48.

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1 Introduction

Motor vehicle accidents represent a major issue in urban transportation, generating large economic and social costs. These include not only fatalities and injuries but also productivity losses, insurance costs, congestion delays, and strain on emergency services (Blincoe et al., 2002; Small, 1997). In 2019 alone, crashes across the United States resulted in an estimated \$340 billion in economic damages, 36,500 deaths, and over 4.5 million injuries (Blincoe et al., 2015). Dense and congested cities bear a disproportionate share of these costs. In New York City (NYC), residents spend an average of 140 hours per year in traffic congestion, costing the local economy more than \$11 billion annually (INRIX, 2021). In that same year, the NYPD recorded over 120,000 motor vehicle collisions and 60,000 injuries within city limits.

Traffic accidents, from an economic perspective, represent both a private cost for drivers and a negative externality for society. Drivers choose routes and speeds to maximize utility, implicitly trading off travel time and accident risk (Couture et al., 2018; Vickrey, 1963). But for urban planners and residents, crashes impose substantial external costs. In dense states like California, each additional driver can raise others' annual insurance premiums by thousands of dollars (Edlin and Karaca-Mandic, 2006), underscoring the case for regulatory intervention.

In response to growing safety concerns, NYC adopted its own version of the Vision Zero (VZ) policy in 2014, inspired by successful European initiatives. The program aimed to eliminate all traffic-related deaths and serious injuries by 2024 through a combination of speed limit reductions, infrastructure changes, and enforcement technologies. Core measures include reducing the citywide speed limit from 30 to 25 mph, installing speed humps, implementing Slow-Neighborhood Zones (SNZs) with 20 mph limits, and deploying Automated Speed Enforcement (ASE) cameras in targeted areas. Despite these efforts, traffic accidents in NYC have not declined as expected, and in some years have even increased slightly (see Figure A.8).

This policy-performance gap raises an important question: *how effective are localized speed reduction and enforcement policies in reducing crashes, injuries, and fatalities in congested urban environments*? While prior work documented that speed regulation can reduce accident rates, most studies focus on single policies or lack causal identification strategies. Whether lower speed limits inherently improve traffic outcomes and promote safety is a longstanding debate in transportation economics.

Lave (1985) argued that coordination of traffic flow, rather than speed reduction itself, explains safety gains. Subsequent studies refine this view: Levy and Asch (1989) highlight the role of speed variance, especially among fast drivers; Fowles and Loeb (1989) confirm speed as a significant fatality factor even after controlling for confounders; and Synder (1989) show that dispersion in speed mainly endangers aggressive drivers. Together, these findings suggest that targeted enforcement may be more effective than blanket speed reductions in diverse urban settings.

Recent studies, for example Ang et al. (2020) show that speed limit reductions in São Paulo led to a 21.7% decline in crashes, albeit with increased commute times. Similarly, Aney and Ho (2019) find that China's traffic regulations reduced accident frequency but increased severity due to heterogeneous behavioral responses. Yet, few evaluations exist for the specific suite of interventions implemented under Vision Zero in U.S. cities.

In this paper, I evaluate the effectiveness of three core speed-control policies from NYC's Vision Zero program: speed humps, Slow-Neighborhood Zones, and ASE cameras. Using high-resolution, geocoded accident data from the NYPD and a spatial difference-in-differences (DiD) framework, I estimate the causal impacts of each policy on traffic outcomes at the street-segment-by-day level. The identification strategy exploits variation in proximity to treated street segments, comparing trends in accident outcomes across treated and untreated areas over time.

This work contributes to the economics of urban transportation and accident externalities in three ways. First, it provides a disaggregated evaluation of multiple enforcement tools within a single urban context, extending the literature on the effectiveness of speed regulation (Ashenfelter and Greenstone, 2004; Gallagher and Fisher, 2020; Van Benthem, 2015). Second, it applies a spatial DiD design with high-frequency data, offering methodological improvements over traditional before-and-after or aggregate panel analyses (Gao et al., 2025; Luca, 2015). Third, I conduct a policy-relevant cost-benefit analysis, using estimates from Parry (2004) on the monetary value of accident reductions to assess the fiscal efficiency of each intervention.

This analysis also complements prior studies on Vision Zero in NYC. Mammen et al. (2020) find that speed limit reductions led to significant casualty declines. However, they do not disentangle the roles of specific enforcement types. Likewise, while Tang (2017) and Gao et al. (2025) document localized effects of cameras, they do not assess spillovers or policy heterogeneity. By contrast, this paper examines which policy delivers the largest safety improvements per dollar spent, providing insight into the optimal design of urban traffic enforcement strategies in congested cities.

2 Background

The Vision Zero (VZ) initiative, launched in Sweden in 1997, marked a paradigm shift in road safety policy by setting ambitious targets like eliminating traffic fatalities and injuries. Its success influenced policies across Europe, including in Norway and the Netherlands. Research shows VZ's effectiveness: in Sweden, fatalities fell from 550 to 450 per year over a decade, with median barriers reducing fatalities by 80% (Johansson, 2009). In Norway, the annual decline in traffic fatalities accelerated to 6.1% post-VZ, with fatalities staying below 255 in 14 of 19 years after 2001 (Elvik, 2022). The Netherlands saw a 30% reduction in traffic fatalities from 1998 to 2007, with benefits four times higher than costs, validating these road safety policies (Wegman et al., 2022).

This success and the apparent consensus about the effectiveness of VZ in Europe led to its adoption in the US. NYC implemented a VZ policy in 2014. Despite this adoption, NYC remains far from reaching the VZ targets set for 2024, with a stubbornly high level of injuries and fatalities distributed across all neighborhoods in the city as illustrated in Figure A.1.

Some of the NYC VZ policies focus on speed limit enforcement, as studies tend to show that speed calming measures reduce accidents (Hess, 2004; Hess and Polak, 2003; Hu and McCartt, 2016; Novoa et al., 2010). Speed reductions of up to 5 mph can change accident outcomes and save the lives of those involved. To this end, actions heterogeneous in their intensity, cost and implementation have been employed, such as the creation of slow neighborhood zones, the legal reduction of speed limits, the implementation of speed bumps, and the deployment of traffic monitoring cameras. Figure A.2 shows the actual distribution of speed limits across streets and road segments in NYC.

Since the launch of VZ in 2014, New York City has significantly ramped up the installation of speed bumps as a critical measure to enhance road safety (Figure A.3). Speed bumps are considered a medium level of enforcement: while they do not result in fines for speeding, drivers who ignore them risk damaging their vehicles if they pass over them too fast. The city has recognized that speed bumps are an effective tool to slow down traffic, particularly in residential areas and near schools, thereby reducing the likelihood and severity of accidents involving pedestrians and cyclists.

By the end of 2023, New York City had installed over 1,400 new speed bumps across its five boroughs. This marks a substantial increase from the previous years, demonstrating the city's commitment to using physical traffic calming measures to improve safety. The installations are strategically placed in areas identified through traffic studies as having higher rates of speeding and traffic incidents. For instance, neighborhoods such as

East New York in Brooklyn, the South Bronx, and various parts of Queens have seen a considerable number of these installations due to their historically higher traffic accident rates.

The implementation of Slow Neighborhood Zones (Figure A.4) are a initiative to promoting pedestrian-friendly environments. These zones are strategically designated areas within the city where speed limits are reduced and enforced by traffic signalization. They in general follow a speed limit of 20 to 25 miles per hour on signs. The zones are characterized by a combination of traffic calming measures and infrastructure enhancements designed to encourage safer behaviors and discourage speeding. These measures may include physical alterations to the roadway, such as speed bumps, chicanes, raised crosswalks, and curb extensions. The community is also part on the decision of implementing the zones.

The New York City Automated Speed Enforcement Program (Figure A.5) is the most stringent of the city's speed enforcement measures. Launched in 2013, the program uses speed cameras to automatically detect and ticket vehicles that exceed the posted speed limit by more than 10 miles per hour on a given street. Violations result in a $50 fine, issued as a civil penalty, meaning they do not affect the driver's insurance premiums or add points to their driving record. These cameras are strategically placed in school zones to enhance road safety, particularly in areas with vulnerable populations such as children. By leveraging technology, the program aims to reduce speeding and improve traffic safety outcomes.

Initially, the program began with a limited number of speed cameras deployed across the city, but it has since expanded significantly. As of 2020, there were over 2,000 speed cameras installed in 750 school zones, operating 24 hours a day, seven days a week. This expansion was authorized by New York State legislation in 2019, which allowed the city to increase the number of cameras and extend their operational hours beyond the original restrictions, which limited enforcement to school hours and days.

The New York City Department of Transportation (NYC DOT) has led the implementation of Vision Zero (VZ) initiatives, managing over 90% of the program's funding (Figure A.6). The VZ budget has steadily increased and is projected to continue growing through 2029, with the majority of funds allocated to engineering-based safety measures. These measures include street and intersection redesigns, improvements to pedestrian and bicycle infrastructure, and traffic-calming interventions such as those analyzed in this study.

3 Data

The empirical analysis relies on a comprehensive dataset of police-reported motor vehicle accidents recorded by the NYPD at the time of accident occurrence and obtained from the NYC Open Data portal. The dataset covers the period from 2012 to 2018 and includes detailed geocoded records of accident locations along with the number of injuries and fatalities and some accidents characteristics. Two limitations of the data are worth noting. First, accidents that result in no damage or damages below $1,000 may not be included, even if they were reported. Second, unreported accidents are also excluded from the dataset. Although data for 2019 is available, we exclude it from our analysis due to a change in the injury classification system[1] that could bias results for that outcome variable.

The dataset begins in 2012 at the micro level. To construct our descriptive trend graphs, we supplement the totals from NYC with data from the Fatality Analysis Reporting System (FARS), which extends back to 2009. The data also records fatalities and injuries by specific groups as cyclists, motorists and pedestrians. An important feature of the dataset is that it includes detailed information on contributing factors for each individual accident. These variables allow us to control for potential determinants of crashes that are typically unobserved in similar studies. Contributing factors are categorized into six groups: driver behavior, vehicle issues, environmental conditions, external influences, and medical illness.[2] Including these controls represents a notable advancement, as it helps mitigate omitted variable bias arising from latent or context-specific factors influencing accident outcomes. In addition, data on the geographic coordinates, and other specific features of slow neighborhood zones, speed bumps, and the automated speed enforcement were sourced from the New York City Department of Transportation (NYDOT) and the Vision Zero View.

A descriptive overview of aggregate trends in the proposed analysis suggests a general decline in fatalities following the implementation of Vision Zero (VZ), while the total number of injuries has remained relatively stable (Figure A.7). However, NYC continues to report a high injury rate, with at least one person injured in every two accidents, significantly exceeding the national average of approximately 0.46 injuries per accident.

The trend in total accidents remained stable in New York until 2018 (Figure A.8). Despite

[1]Starting in 2019, the National Highway Traffic Safety Administration (NHTSA) mandated the adoption of Model Minimum Uniform Crash Criteria (MMUCC) 4th Edition guidelines, requiring all jurisdictions to standardize serious injury reporting. This change led the New York State Department of Motor Vehicles (DMV) to redefine severe "A"-type injuries, potentially inflating post-2019 injury counts.

[2]Contributing factors are recorded by police officers responding to the crash scene. While they may not precisely determine the root cause of the accident, they offer valuable insights into potential behaviors or circumstances, often unobservable, that may have contributed to the incident.

these patterns, the budget for the VZ program has continued to grow, with planned increases in the coming years, particularly for the implementation of localized safety-focused infrastructure (Figure A.6).

4 Empirical Strategy

To estimate the effects of localized speed enforcement policies under Vision Zero, I adopt a spatial difference-in-differences (DiD) framework. This design exploits variation in proximity to treated street segments, those receiving interventions such as speed bumps, automated enforcement, or designation as a slow zone, over time. The approach builds on the logic of conventional DiD by leveraging differences across space rather than across administrative units. Specifically, I compare traffic safety outcomes before and after treatment between street segments located near a policy intervention and those farther away, under the assumption that untreated areas provide a valid counterfactual for treated areas at similar distances. This implies that the distance-based specification for the control group helps ensure comparability in traffic conditions and infrastructure patterns. The main identifying assumption is a spatial parallel trends condition: in the absence of treatment, safety outcomes for nearby treated and control street segments would have followed similar trends over time.

The primary specification is given by the following model:

$$Outcome_{st} = \alpha + \delta_1 \text{Post}_t + \delta_2 \text{Within}_s + \delta_3 (\text{Post}_t \times \text{Within}_s) + \mathbf{X}'_{st}\theta + \gamma_t + \lambda_s + \varepsilon_{st} \tag{1}$$

Here, $Outcome_{st}$ denotes the number of accidents, injuries, or fatalities on street segment s on day t. The variable Post_t is an indicator for periods after the implementation date of a given intervention. The variable Within_s indicates whether the street segment lies within a treatment distance threshold d^* of the nearest intervention site. The coefficient δ_3 on the interaction term $\text{Post}_t \times \text{Within}_s$ captures the local average treatment effect of enforcement policies. The vector $\mathbf{X}_{st}$ includes observable accident-level controls such as contributing factors listed as potential influences to the accident[3]. The specification includes fixed effects for year (γ_t), month, and day of the week. (λ_s) is the fixed effect by street segment that rules out meaningful differences in traffic location characteristics, and standard errors are clustered at the street-segment level to account for spatial correlation.

[3] I include a set of dummy variables for each contributing factor category, leaving "other factors" as the omitted reference group. The factors are grouped into seven broad categories, which include mechanical problems, alcohol or drug use, and road- or weather-related conditions, among others.

The definition of the threshold distance d^* is central to the design. For speed bumps, this threshold is derived from reaction distances informed by prior studies on driver response times and visibility (Pau and Angius, 2001; Vadeby and Howard, 2024). These studies suggest that drivers require approximately 1.5 seconds to respond to road features. Based on the equation $d = v \times t$, where v is the vehicle speed in feet per second and $t = 1.5$ seconds, reaction distances were calculated using typical urban speeds up until the maximum speed limit possible for NYC. Given that 1 mile per hour equals 1.467 feet per second and 1 mile equals 5,280 feet, the maximum allowed speed after the policy (25 mph) corresponds to approximately 0.01042 miles. This value defines the treatment distance in my main specification. For comparison, I also present results using 0.01250 miles, which corresponds to the 30 mph limit in place before the policy implementation. Beyond 0.01042 miles, treatment groups are defined in equal increments of 0.00208 miles to enable finer spatial comparisons.

For automated enforcement cameras, I use the limit capacity of cameras to capture speeders, which is a quarter of a mile as the treatment zone. Beyond that, treatment rings are defined in uniform 0.05-mile intervals. This creates concentric zones that represent different levels of policy exposure for nearby street segments. The slow neighborhood zones (SNZ) are already defined as polygonal areas. I estimate a compatible DiD regression including observations inside the SNZs as treated, and I estimate a separate distance measure from each accident location to the nearest SNZ border. In contrast, the distance-to-border measure enables a border regression discontinuity design (RDD), with a post-policy indicator.

The identifying assumption of this border RDD is that any change in the discontinuity of outcomes at the SNZ boundary after the implementation can be causally attributed to the policy. This local design allows us to compare the border effect before and after the intervention, thereby isolating the causal impact of the policy at the threshold. In doing so, the specification captures short-range spillovers that may be attenuated or averaged out in the broader spatial DiD framework.

The border RDD specification with a post-policy indicator is given by:

$$\begin{aligned} Outcome_{st} = \alpha + \tau_{\text{pre}} \cdot \mathbb{1}(BorderDist_s < 0) + \delta\tau \cdot \left[\mathbb{1}(BorderDist_s < 0) \times Post_t\right] \\ + \beta \cdot Post_t + f_0(BorderDist_s) + Post_t \cdot f_1(BorderDist_s) \\ + \mathbf{X}'_{st}\theta + \gamma_t + \varepsilon_{st} \end{aligned} \tag{2}$$

Here, the outcomes are the same, $\mathbb{1}(BorderDist_s < 0)$ is an indicator for whether segment s is located inside the SNZ boundary (i.e., negative distance to border), and $Post_t$

is an indicator for the post-policy period. The term τ_{pre} captures the border discontinuity before the policy, while $\delta\tau$ measures the change in the discontinuity after the policy. The flexible functions $f_0(BorderDist_s)$ and $f_1(BorderDist_s)$ allow for different distance trends before and after implementation. $\mathbf{X}_{st}$ includes crash-level controls, and γ_t are fixed effects for year, month, and day of the week, respectively. The parameter of interest is $\delta\tau$, which identifies the causal effect of the policy on the discontinuity at the boundary.

To examine the dynamic effects of enforcement policies over time, I estimate a time-varying version of the main spatial DiD specification. This dynamic specification replaces the static post-treatment indicator Post_t with a series of interactions between the treatment indicator and post-treatment year dummies, excluding the year of implementation. Let $\mathcal{Z} = \{2015, 2016, 2017, 2018\}$ denote the years after the baseline year 2013.

The dynamic DiD specification is:

$$Outcome_{st} = \alpha + \sum_{z \in \mathcal{Z}} \delta_z \cdot (\mathbb{1}_{\{t=z\}} \times \text{Within}_s) + \mathbf{X}'_{st}\theta + \gamma_t + \lambda_s + \varepsilon_{st} \tag{3}$$

In this formulation, each δ_z captures the treatment effect in post-treatment year Z for street segments within the treatment radius ($\text{Within}_s = 1$). This allows us to trace the evolution of policy effects across time relative to the pre-treatment period, using 2013 as the baseline.

The dynamic structure is especially relevant given the implementation lags typical of speed enforcement policies. Many of these interventions, such as speed bumps, slow zones, and camera systems, require time for infrastructure installation, signage placement, and behavioral adaptation. Therefore, this specification tests the hypothesis that treatment effects might be most pronounced in the years following the launch of the broader Vision Zero program.

4.1 Robustness Tests

To assess the robustness of the main spatial DiD results, I implement some complementary strategies that account for distributional properties of the data and selection concerns.

First, I estimate alternative specifications using Poisson quasi-maximum likelihood (PQML) and Zero-Inflated Poisson (ZIP) estimators. These models address two key features of the outcome variables: their skewed count distribution and the excess zeros in the case of fatalities and injuries. While the baseline linear DiD model remains interpretable and computationally tractable, count models allow for more flexible distributional assumptions, offering reassurance that results are not driven by violations of linearity.

Second, to mitigate concerns that treatment assignment is endogenous to pre-treatment crash intensity, since treated street segments may have been selected for intervention based on high accident rates, I implement a synthetic difference-in-differences (SDiD) estimator as proposed by Arkhangelsky et al. (2021). This method constructs a synthetic control group from untreated street segments located beyond the treatment radius, which better approximates the counterfactual trend for treated segments.

The SDiD estimator is used to estimate the average causal effect of treatment, denoted $\hat{\tau}_{\text{sdid}}$, by assigning unit weights $\hat{\omega}_s$ to untreated street segments and time weights $\hat{\lambda}_t$ to balance pre- and post-treatment trends. These weights are selected to minimize differences in pre-treatment outcomes across groups and time periods. The treatment effect is then estimated using a weighted two-way fixed effects regression:

$$(\hat{\tau}_{\text{sdid}}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \arg\min_{\tau,\mu,\alpha,\beta} \sum_{s=1}^{N} \sum_{t=1}^{T} \left(Outcome_{st} - \mu - \alpha_s - \beta_t - D_{st}\tau\right)^2 \hat{\omega}_s \hat{\lambda}_t \tag{4}$$

Here, $Outcome_{st}$ is the number of crashes, injuries, or fatalities on segment s at time t, and D_{st} is a binary treatment indicator equal to one if the segment lies within the treatment radius d^* after the implementation of a policy (i.e., $D_{st} = \text{Post}_t \times \text{Within}_s$). The terms α_s and β_t represent street-segment and day fixed effects, respectively, and μ is a constant.

To account for observed covariates, I follow the residualization approach recommended by Arkhangelsky et al. (2021). Specifically, I regress the outcome on crash-level covariates and estimate the SDiD model on the residuals:

$$Outcome_{st}^{\text{res}} = Outcome_{st} - \mathbf{X}_{st}'\hat{\gamma} \tag{5}$$

$$(\hat{\tau}_{\text{sdid}}, \hat{\mu}, \hat{\alpha}, \hat{\beta}) = \arg\min_{\tau,\mu,\alpha,\beta} \sum_{s=1}^{N} \sum_{t=1}^{T} \left(Outcome_{st}^{\text{res}} - \mu - \alpha_s - \beta_t - D_{st}\tau\right)^2 \hat{\omega}_s \hat{\lambda}_t \tag{6}$$

This approach isolates the average treatment effect net of observable factors while preserving a robust counterfactual trajectory. Inference is conducted via nonparametric bootstrap, resampling street segments with replacement to construct confidence intervals that account for serial correlation and spatial dependence.

5 Empirical Results

This subsection presents the results of the spatial difference-in-differences (DiD) and the proposed variations of specification models for each policy intervention. The estimations allow for a comparison of traffic outcomes in treated areas versus control locations, across different spatial thresholds. The findings are presented separately for Slow Neighborhood Zones (SLZ), speed bumps, and Automated Speed Enforcement (ASE).

5.0.1 Slow Neighborhood Zones (SLZ)

Table 1 reports the localized effects (Columns 1–3) and spillover effects (Columns 4–6) of Slow Neighborhood Zones (SLZ) on the outcome variables. For accidents, localized effects are small and statistically insignificant across specifications, but spillover effects are negative and statistically significant in both OLS and Poisson models. This suggests that the creation of SLZs may have shifted traffic dynamics such that nearby areas outside the zones experienced modest reductions in accidents. For injuries, localized effects are again statistically insignificant, while spillover effects display consistent negative and statistically significant coefficients, indicating that SLZ implementation is associated with reductions in injuries just outside the designated areas. Fatalities, however, show no significant effects in either localized or spillover estimates across all models.

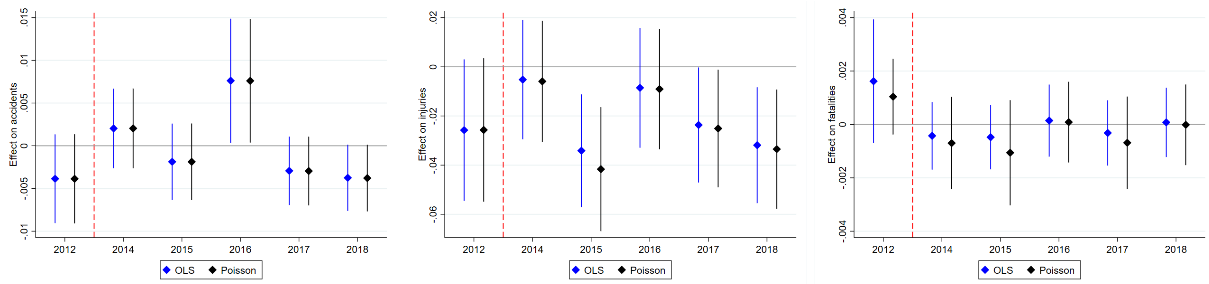
Table 1: Local and spillover effects of Slow Neighborhood Zones (SLZ) on traffic outcomes

	Localized Effects			Spillover Effects		
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Accidents	0.001	0.001		-0.006***	-0.022**	
	(0.001)	(0.001)		(0.001)	(0.005)	
R^2	0.0003	0		0.001	0	
Injuries	-0.013	-0.014	-0.009	-0.0064	-0.045**	-0.041**
	(0.008)	(0.008)	(0.008)	(0.005)	(0.020)	(0.019)
R^2	0.001	0.002		0.001	0.001	0.019
Fatalities	-0.0005	-0.0006	-0.0006	0.00008	0.007	0.006
	(0.0005)	(0.0004)	(0.0004)	(0.0002)	(0.013)	(0.011)
R^2	0.0002	0.0095		0.0001	0.007	0.003
Observations	26,948	26,948	26,948	1,019,373	1,019,373	1,019,373
Estimation	OLS	Poisson	Zero-Inflated	OLS	Poisson	Zero-Inflated

Note: This table presents the spatial DiD and RDD estimations for all main outcome variables: number of accidents, injuries and fatalities for each day in each street segment. Columns 1 to 3 present localized effects estimated with the DiD and columns 4 to 6 present spillover effect estimated with the RDD. The treated street segment in the DiD estimations are accidents inside of the SLZ poligon and in the case of the RDD it uses the distance to the border considering 0.5 miles of distance. The control group for the RDD is drawn from a 1.7–3 mile distance band outside of the poligon area. Preferred estimates are in columns 4 and 5. Fixed effects include year, month, and day-of-week. All robust standard errors in parenthesis. For the Poisson estimators the pseudo R-squared is presented. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1 presents the dynamic estimations, capturing the evolution of policy effects in the years following SLZ implementation. For accidents, both OLS and Poisson estimates fluctuate around zero, with wide confidence intervals and no statistically significant treatment effects in any post-treatment year. Fatalities similarly remain statistically indistinguishable from zero across all years, consistent with the main table results and reflecting the low frequency and limited variation in fatal accident counts. For injuries, the estimates are generally negative throughout the post-treatment period, though the wide confidence intervals prevent precise inference. Despite the lack of significance in most years, the pattern aligns with the spillover estimates, hinting at a modest reduction in injury rates associated with SLZ presence.

Figure 1: Annual treatment effect of speed bumps on traffic outcomes



(a) Accidents (b) Injuries (c) Fatalities

Taken together, the results provide consistent evidence of safety gains from SLZ implementation. While localized effects are generally negligible, the spillover results suggest that these zones may contribute to reducing both accidents and injuries in the surrounding areas. Overall, the evidence indicates that SLZs may have small but meaningful impacts on improving traffic safety beyond their immediate boundaries, though the magnitude of these effects remains modest. The SDiD results confirm the direction and magnitude of the effects. Those zones may cause a better response in the border because is probably where they are better signalized, and in general they advertise about pedestrians and probably the presence of schools, which may make drivers more sensitive to it.

5.0.2 Speed bumps

Table 2 reports the localized effects (Columns 1–3) and spillover effects (Columns 4–6) of speed humps on the outcome variables. For accidents, only the OLS estimate is statistically significant; effect sizes are small and insignificant in the other specifications. Results for injuries follow the same pattern, with the OLS estimate marginally significant at the 10% level. Fatalities show no statistical significance in either the OLS or Poisson models, and the Zero-Inflated model fails to converge even without fixed effects. This non-convergence likely reflects the data's characteristics, very low fatality counts, little variation, and a high proportion of zeros. Such data properties make it difficult for the Zero-Inflated likelihood optimization to separately identify the parameters of the zero-inflation and count processes, leading to non-convergence regardless of model complexity. Spillover effects are also statistically insignificant across all outcomes.

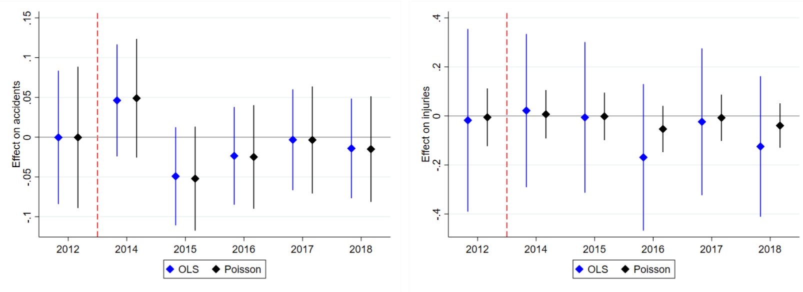
Table 2: Local and spillover effects of speed bumps on traffic outcomes

	Localized Effects			Spillover Effects		
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Accidents	-0.013***	-0.009		-0.006	-0.006**	
	(0.002)	(.023)		(0.003)	(0.003)	
R^2	0.142	0.004		0.043	0.003	
Injuries	-0.027*	-0.018	-0.007	-0.004	-0.004	0.001
	(0.013)	(0.031)	(0.014)	(0.013)	(0.012)	(0.013)
R^2	0.026	0.032		0.007	0.008	
Fatalities	-0.001	-0.0009		-0.0009	-0.0006	-0.0006
	(0.0006)	(0.002)		(0.0006)	(0.0006)	(0.0006)
R^2	0.014	0.075		0.002	0.002	
Observations	26,948	26,948	26,948	1,019,373	1,019,373	1,019,373
Clustered	Yes	No	Yes	Yes	No	Yes
Estimation	OLS	Poisson	Zero-Inflated	OLS	Poisson	Zero-Inflated

Note: This table presents the spatial DiD estimations for all main outcome variables: number of accidents, injuries, and fatalities for each day in each street segment. Columns 1 to 3 present localized effects, and columns 4 to 6 present spillover effect estimations. The treated street segment band covers a distance of 0 to 0.01042 miles, corresponding to the average stopping distance for a driver traveling at 25 mph after detecting a speed bump. The control group is drawn from a 1.7–3-mile distance band. The control group is drawn from a 0.01042- to 1.7-mile distance band. Preferred estimates are in columns 4 and 5. Fixed effects include year, month, street segment, and day of week. Clustered standard errors are at the street-level group; non-clustered estimates use robust standard errors, all presented in parentheses. For the Poisson estimators, the pseudo R-squared is presented. *** p<0.01, ** p<0.05, * p<0.1.

The statistical insignificance of the results for the effects of speed bumps on traffic outcomes doesn't allow us to distinguish the potential localized effects of this policy from zero neither suggests that spillover effects are also different from zero. To further investigate the dynamic nature of this policy I use the dynamic DiD estimation, that captures the effect in post 2014 years for street segments within the treatment radius distance from nearest the speed bump, the results are presented in Figure 2.

Figure 2: Annual treatment effect of speed bumps on traffic outcomes



(a) Accidents (b) Injuries

The results indicate no statistically significant effects of the policy implementation over time on accidents or injuries in the treated street segments. Due to high collinearity and limited variation in the number of fatalities, estimates for this outcome could not be obtained for all periods, and for the periods where estimates were available, the coefficients were not significantly different from zero. Consequently, the fatalities panel is omitted. The Synthetic Difference-in-Differences estimations confirm the same pattern, yielding non-significant coefficients throughout.

Taken together, the results provide only small and limited evidence of a reduction in accidents and injuries following the installation of speed bumps. These effects are not robust across alternative specifications, and in the preferred Poisson estimations their statistical significance disappears, suggesting that any safety gains are likely so small as to be statistically indistinguishable from zero. This finding is consistent with prior transportation studies (e.g., Huang and Cynecki (2000); Pau and Angius (2001); Yeo et al. (2020)), which document that vehicles typically reduce speed only within a short zone, roughly 30 meters around a speed bump, accelerating again immediately after. While such localized deceleration may offer some protection to pedestrians in that narrow range, it is unlikely to meaningfully reduce accidents overall. In the context of a highly congested city, this pattern may even increase interactions among vehicles and drivers, potentially exacerbating traffic congestion.

5.0.3 Automated Speed Enforcement (ASE)

Table 3 reports the localized and spillover effects of ASE implementation on traffic outcomes. Across all specifications, being in a treated street segment with a camera implementation causes a positive statistically significant localized effect in both accidents and injuries compared to similar areas not exposed to treatment. In my preferred specification for accidents (column 2), I observe a rise by 1.499 accidents, and injuries increase by 0.212 on my preferred specification for injuries and fatalities (column 3) on average per street-segment per day, both at the 1% of statistical confidence level.

Table 3: Local and spillover effects of automated speed enforcement on traffic outcomes

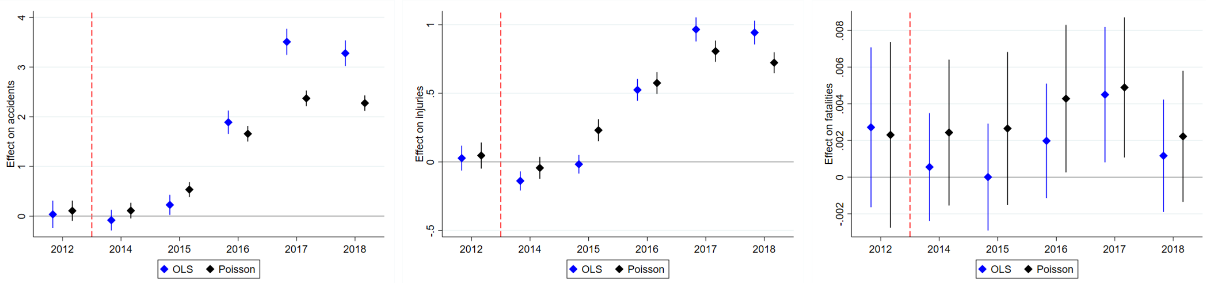
	Localized Effects				Spillover Effects			
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Accidents	1.772***	1.499***		1.974	0.383***	0.487***		0.970***
	(0.144)	(0.054)		(1.313)	(0.088)	(0.020)		(0.208)
R^2	0.294	0.326			0.250	0.144		
Injuries	0.452***	0.494***	0.212***	0.999***	0.105***	0.171***	0.040**	0.271***
	(0.040)	(0.026)	(0.033)	(0.329)	(0.024)	(0.020)	(0.160)	(0.084)
R^2	0.129	0.132			0.058	0.047		
Fatalities	0.0007	0.002	0.001	0.041***	0.0001	0.001	0.0009	0.010***
	(0.001)	(0.034)	(0.001)	(0.003)	(0.0006)	(0.003)	(0.0008)	(0.003)
R^2	0.002	0.052			0.001	0.024		
Observations	126,497	126,497	126,497	78,375	262,086	262,086	262,086	133,000
Clustered	Yes	No	Yes	No	Yes	No	Yes	No
Estimation	OLS	Poisson	Zero-Inflated	SDiD	OLS	Poisson	Zero-Inflated	SDiD

Note: This table presents the spatial DiD estimations for all main outcome variables. The main outcomes are the number of accidents, injuries, and fatalities for each day in each street segment. Columns 1 to 4 present localized effects, and columns 5 to 8 present spillover effect estimations. The treated street segment band ranges from 0 to 0.25 miles, which corresponds to the camera enforcement capture zone. The control group is drawn from a 1.7–3-mile distance band. Preferred estimates are in columns 5 and 6. Fixed effects include year, month, street segment, and day of week. Clustered standard errors are at the street-level group; non-clustered estimates use robust standard errors and bootstrapped standard errors from the synthetic DiD. For the Poisson estimators, the pseudo R-squared is presented. *** p<0.01, ** p<0.05, * p<0.1.

These results suggest that fixed enforcement cameras may lead to localized behavioral adjustments or contribute to additional congestion in an already heavily congested city. Interestingly, the estimates for fatalities show no statistically significant effects. Given that a core goal of the Vision Zero (VZ) initiative is to eliminate traffic-related fatalities, this finding suggests that the policy may have had limited effectiveness in achieving that objective. Another noteworthy finding is the presence of spillover effects. These remain positive and statistically significant for both accidents and injuries, indicating that fixed enforcement cameras not only increase incidents in the directly treated street segments but also contribute to a broader amplification of accidents extending up to 1.6 miles beyond those areas.

Figure 3 presents the results from the dynamic Difference-in-Differences (DiD) estimations, comparing outcomes from both the OLS and Poisson models. These estimates allow us to examine the evolution of policy effects on treated street segments over time. Both models display similar patterns for accidents and injuries, with near-zero effects through 2015, followed by significant and relatively stable positive effects in the subsequent years.

Figure 3: Annual treatment effect of Automated Speed Enforcement (ASE) on traffic outcomes



(a) Accidents (b) Injuries (c) Fatalities

Note: This figure presents the year-by-year coefficients from the dynamic Difference-in-Differences (DiD) estimation. The x-axis represents the years, while the y-axis shows the estimated effect on the outcome variable in count units (e.g., number of accidents, injuries, and fatalities). The estimates reflect the treatment effect for street segments located within the 1.7–3 mile distance band, compared to the control group. Panel A displays results for accidents, Panel B for injuries, and Panel C for fatalities. The point estimates from the Ordinary Least Squares (OLS) model are shown as blue dots, while the corresponding estimates from the Poisson model appear in black. Each estimate is accompanied by its 95% confidence interval. The red vertical line marks 2013, the baseline year used in the estimation. Overall, accidents and injuries show a general increase in treated segments relative to the control group, while fatalities display effects close to zero across all years. These results are consistent with previous findings.

These findings reinforce the results from the previous DiD estimations and reveal an important dynamic: because these enforcement measures require engineering interventions and time for full implementation, their effects only become evident once the policy is more widely deployed across the city. The SDiD estimations further confirm the previous findings. Accidents show a statistically significant average daily increase of 3.8 incidents in treated street segments relative to their synthetic control, significant at the 1% level. For injuries, the estimated effect is an additional 0.71 injuries per day in treated segments compared to the control at 5% level. As with previous models, the results for fatalities remain statistically insignificant.

6 Mechanisms

The estimated effects of Slow Neighborhood Zones (SLZ), speed bumps, and Automated Speed Enforcement (ASE) point to distinct mechanisms through which traffic safety interventions operate in a dense urban environment like New York City. I discuss potential channels separately for each intervention and then contrast them.

The localized increase in accidents and injuries around camera locations is consistent with behavioral responses by drivers. When motorists suddenly perceive that they are entering an enforcement zone, they may engage in abrupt braking or variable acceleration patterns, particularly if they are exceeding the limit just before detection, in order to avoid a fine. Such sharp adjustments in speed can create turbulence in traffic flow, raising the likelihood of rear-end or side collisions. This mechanism is further supported by the finding that injuries, not just accidents, rise, indicating that the incidents are not merely minor fender-benders but involve greater severity.

A second channel relates to congestion. Enforcement cameras are typically installed in areas already prone to heavy traffic and narrower lanes. Compliance with lower posted speed limits may slow overall traffic, intensifying close interactions among vehicles and pedestrians. The broader positive spillover effects on accidents and injuries increasing up to 1.6 miles away, suggest that congestion patterns adjust well beyond the treated street segments, magnifying the risks rather than reducing them.

Taken together, the evidence favors the behavioral-response mechanism as the dominant explanation, with congestion effects amplifying the localized risks into neighboring areas. The absence of an effect on fatalities indicates that while incidents become more frequent and injurious, they may not reach the extreme severity associated with fatal crashes.

For SLZs, the mechanism appears to operate in the opposite direction. The insignificant localized effects but consistent negative spillovers suggest that the main channel is anticipatory behavior. Drivers approaching well-marked SLZ boundaries, often accompanied by signage warning about schools and pedestrians, may adjust their behavior in advance, reducing speed or heightening alertness before entering. This leads to modest safety improvements just outside the designated zones, rather than within them, where speeds are already naturally low due to design and congestion.

Another possible channel is psychological salience: because SLZs are explicitly framed as pedestrian-protection areas, drivers may adopt more cautious driving habits in nearby streets even if they are not physically inside the zone. The reduction in injuries, more than accidents, reinforces the idea that the policy increases driver attentiveness, leading to fewer serious outcomes when incidents occur.

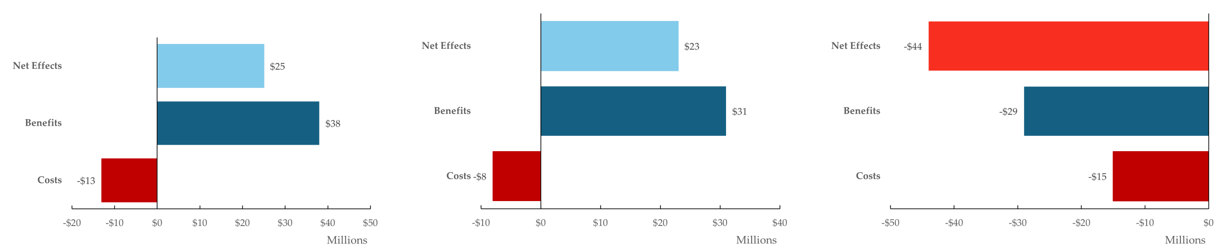
The absence of robust effects for speed bumps aligns with prior evidence that vehicles only slow down immediately at the bump, resuming speed shortly thereafter. The mechanism here is highly localized deceleration, which may benefit pedestrians at the exact crossing point but is unlikely to affect accident rates more broadly. In a congested city, these localized disruptions may even induce abrupt braking and acceleration cycles, increasing variability in vehicle speeds without yielding measurable safety gains. This explains the statistical insignificance of both localized and spillover estimates.

The contrasting results highlight the importance of behavioral and spatial spillover mechanisms. Interventions that trigger abrupt, localized behavioral responses (ASE, speed bumps) risk increasing conflicts in already complex traffic environments. In contrast, interventions that alter broader driving norms or anticipate pedestrian presence (SLZs) appear to diffuse safer behavior into surrounding areas. These dynamics emphasize that not all traffic-calming measures are equal: policies that rely on enforcement and abrupt physical disruptions may backfire in dense urban contexts, while those that rely on signaling and neighborhood-level design can create more sustained improvements in safety.

7 Cost-Benefit Analysis and Policy Implications

In this section I use the results of my main estimations using the spatial DiD and conduct a back to envelop calculation that helps determine the values generated by the potential effects of the policy and then compare them with the potential available costs of implementation. Monetary value estimates of policy effectiveness can be measured through the use components of social average cost of accidents based on systematic surveys of the cost per dollar by consolidated literature such as Small (1997) and Parry (2004) presented in Table A.1.

Figure 4: Implementation costs, benefits, and net effects of traffic safety policies.



(a) Slow Neighborhood Zones

(b) Speed Bumps

(c) Automated Speed Enforcement

Note: Bars represent estimated policy impacts expressed in million USD. Benefits are computed based on significant reductions (or increases) in accidents, injuries, and fatalities.

Figure 4 summarizes the implementation costs, benefits, and net effects of the three main traffic enforcement policies under study: Slow Neighborhood Zones (SLZs), speed humps, and automated cameras. The results provide clear evidence of heterogeneity in the cost-effectiveness of these interventions.

First, both SLZs and speed humps generate net social benefits. For SLZs, the program's relatively low infrastructure and administrative costs are outweighed by the estimated monetary value of accident and injury reductions, yielding net benefits of approximately \$25 million. Speed humps, despite slightly higher per-unit costs, show a comparable magnitude of gains (around \$22 million). These estimates are supported by statistically significant decreases in both accidents and injuries within treated segments, pointing to the effectiveness of localized speed-control measures. Importantly, these policies target driver behavior in a direct, physical manner, ensuring compliance irrespective of enforcement capacity. Their success reinforces prior findings in urban transportation economics that "passive" engineering interventions, once installed, tend to deliver consistent safety benefits at relatively low marginal cost.

By contrast, the camera program demonstrates a markedly different profile. With roughly 2,000 cameras installed at costs ranging between \$60,000 and \$90,000 each, the capital outlay is substantial. Yet, rather than producing measurable safety improvements, the estimates suggest that cameras are associated with an increase in accidents and injuries in their vicinity. As a result, the derived "benefits" of the policy are negative, and when set against the already high fiscal cost of installation, the net effect is strongly adverse, on the order of tens of billions of dollars. These results echo concerns in the literature that automated enforcement can sometimes displace or even exacerbate risks, for example, by inducing abrupt braking, rerouting of drivers into untreated areas, or risk compensation behaviors.

From a policy perspective, the contrast across interventions is instructive. Engineering-based solutions (SLZs and speed humps) appear to deliver consistent and positive returns, with effects concentrated precisely where they are implemented. Conversely, technological enforcement solutions like cameras entail high upfront expenditures but may fail to generate the intended behavioral adjustments, particularly when not embedded within broader safety strategies. Moreover, the negative net effects raise distributional concerns: not only do taxpayers finance costly infrastructure, but road users also bear the external costs of increased crashes and injuries.

Overall, these findings suggest that scaling up low-cost, localized interventions may represent a more efficient strategy for improving traffic safety in congested urban areas, whereas reliance on high-cost surveillance technologies demands careful re-evaluation. Future work could explore whether complementary measures (e.g., driver education, redesign of intersections, or integration with congestion management) might mitigate the unintended consequences of camera deployment.

8 Conclusion

Congested cities face the complex challenge of improving traffic safety without worsening existing congestion. A common approach is to implement traffic calming policies. These measures vary in type and intensity of enforcement, but they share the same objective: to reduce vehicle speeds, prevent accidents, and lessen their severity, thereby reducing injuries and fatalities. However, despite their widespread implementation, there is relatively little well-established evidence on the effectiveness of these policies or the spatial extent of their impacts.

This paper provides a framework to assess the effectiveness of traffic calming policies by leveraging daily, street-level accident data, using New York City as a case study. The granularity of the data allows for precise identification of both localized and spillover effects for each policy. Estimating the effects separately by policy type and enforcement intensity, while holding broader traffic patterns constant, enables a more credible cost–benefit analysis that sheds light on which types of policies may work best in dense urban environments.

The findings reveal spatially counterintuitive effects and heterogeneous outcomes by enforcement type. Stricter measures, such as automated enforcement through speed cameras, appear to increase accidents and injuries in treated areas relative to their counterfactuals. These high-intensity enforcement policies also generate positive spillovers, meaning that accident rates tend to rise in surrounding areas.

In contrast, I find modest, suggestive evidence that lower- to medium-intensity policies, such as slow neighborhood zones and speed humps, may lead to localized reductions in accidents and injuries while also extending these benefits beyond their immediate treatment areas. For fatalities, the results are statistically inconclusive for all policy types.

The evidence suggests that policy design should carefully consider not only the intended localized effects of traffic calming measures but also their unintended spillover impacts. In highly congested cities, strict automated enforcement may inadvertently displace risky driving behaviors to nearby streets, undermining overall safety objectives. Conversely, moderate interventions such as speed humps or slow zones appear to improve safety both within and beyond targeted areas, making them promising candidates for broader application. Policymakers should weigh the trade-offs between enforcement intensity and spillover risks when prioritizing traffic safety investments, particularly in urban environments where road space and driver behavior are tightly interconnected.

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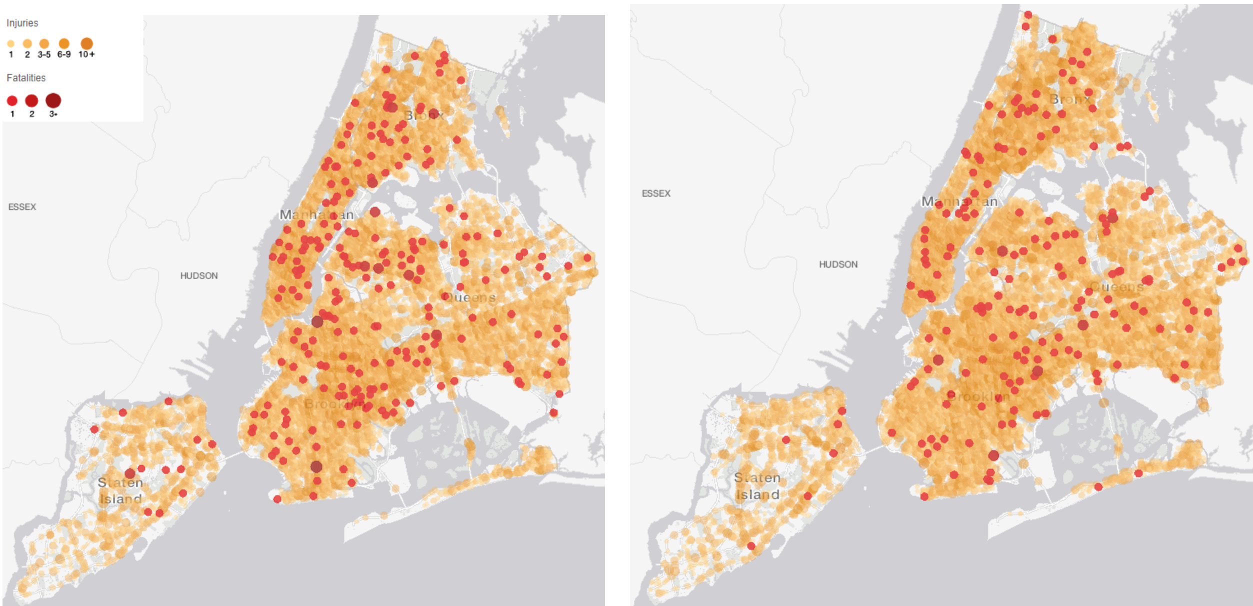
Appendix A - Descriptive Evidence

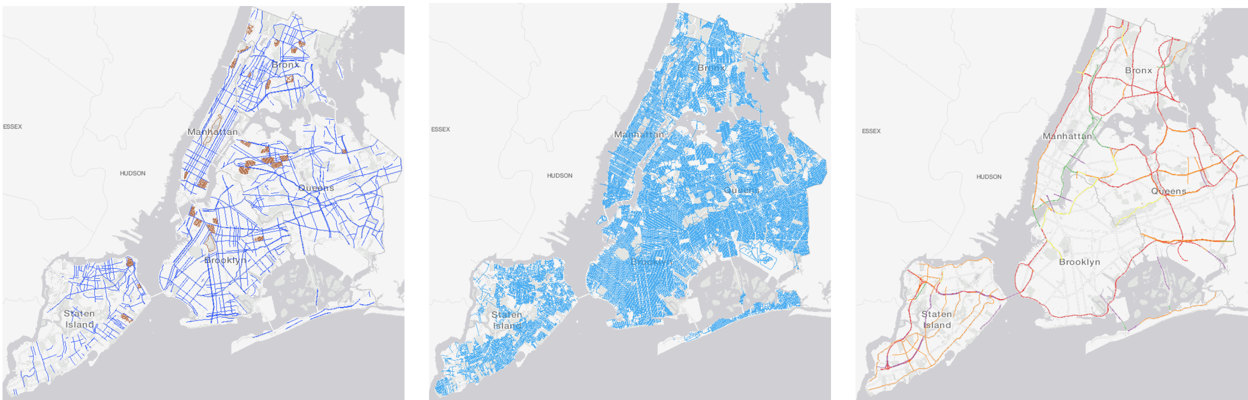
Figure A.1: Total injuries and fatalities before and after the implementation of Vision Zero in New York City



(a) Before VZ implementation (2009-2013). (b) After VZ implementation (2014 - 2018).

Note: This figure illustrates the spatial intensity of motor vehicle accidents resulting in injuries and fatalities in New York City, comparing the periods before and after the implementation of the Vision Zero policy. Orange bubbles represent the intensity of injuries, while red bubbles indicate the intensity of fatalities. Darker and larger bubbles correspond to higher concentrations of incidents at specific locations. *Source:* Vision Zero View.

Figure A.2: Speed limits after the implementation of the Vision Zero program



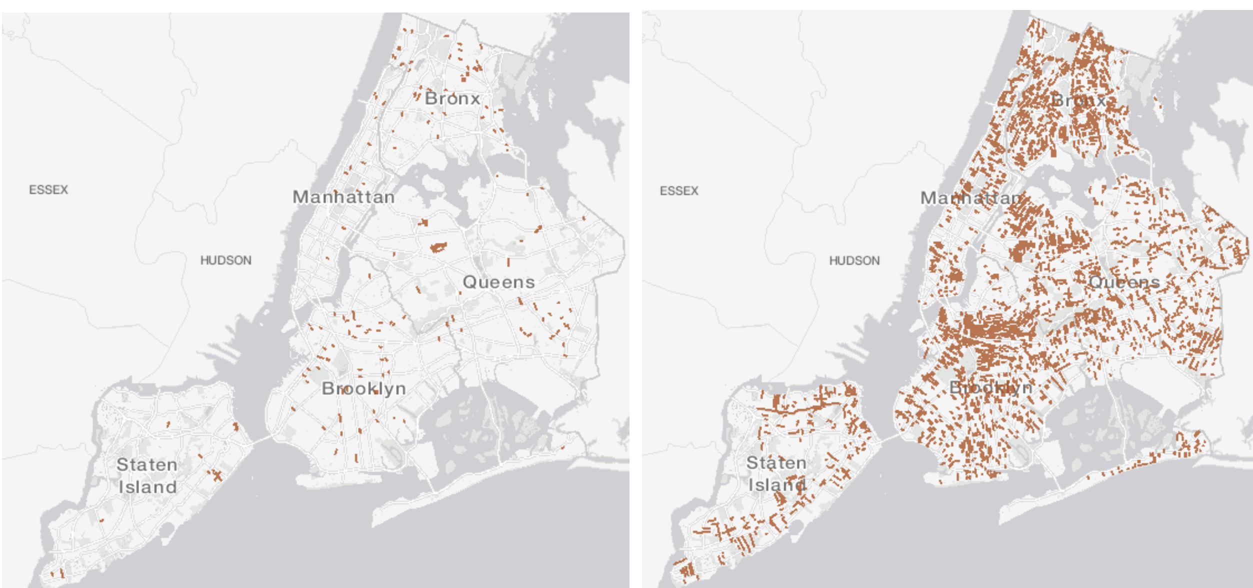
(a) Speed between 20 and 25 miles per hour on signs.

(b) Speed of 25 miles per hour not on signs.

(c) Speed between 30 up to 50 miles per hour on signs.

Note: This figure displays three panels, each highlighting the new speed limits implemented in New York City following the adoption of the Vision Zero (VZ) policy. Prior to the policy, the default speed limit across the city was 30 miles per hour. Panel A shows all signed streets in blue, where speed limits have been reduced to either 20 or 25 miles per hour, depending on the posted signage. In the same panel, brown polygons represent the Slow Neighborhood Zones (SLZ), where the speed limits follow the same reduced pattern. Panel B illustrates the general rule for speed limits in NYC, with most streets shown in blue indicating a citywide limit of 25 miles per hour. Panel C highlights exceptions to this rule, showing major arterial roads and limited-access highways that traverse the city, where speed limits are set higher than the general 25 miles per hour limit. *Source:* Vision Zero View.

Figure A.3: Speed bumps operating before and after the implementation of VZ

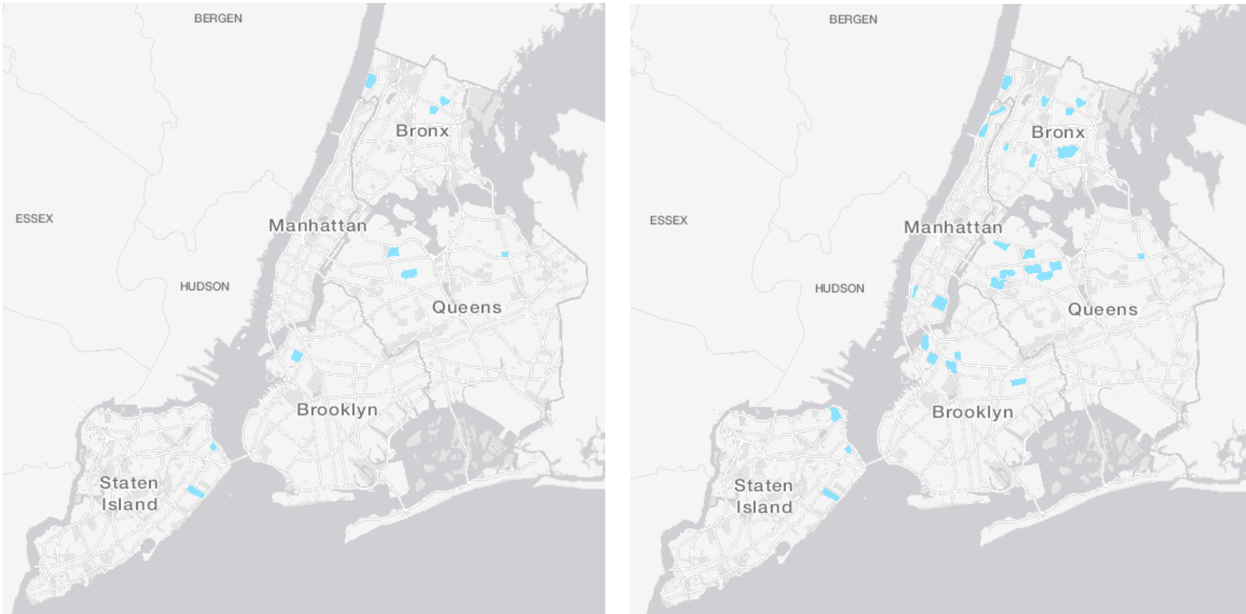


(a) Before VZ implementation (2009-2013).

(b) After VZ implementation (2014 - 2018).

Note: This figure illustrates the significant increase in the number of speed bumps installed across New York City in the years before and after the implementation of the Vision Zero (VZ) initiative. Panel A shows installations from the pre-VZ period (2009–2013), while Panel B reflects the post-VZ period (2014–2018). The brown segments represent the exact locations where speed bumps were implemented. Source: Vision Zero View.

Figure A.4: Slow-Neighborhood Zones (SNZ) operating before and after the implementation of VZ

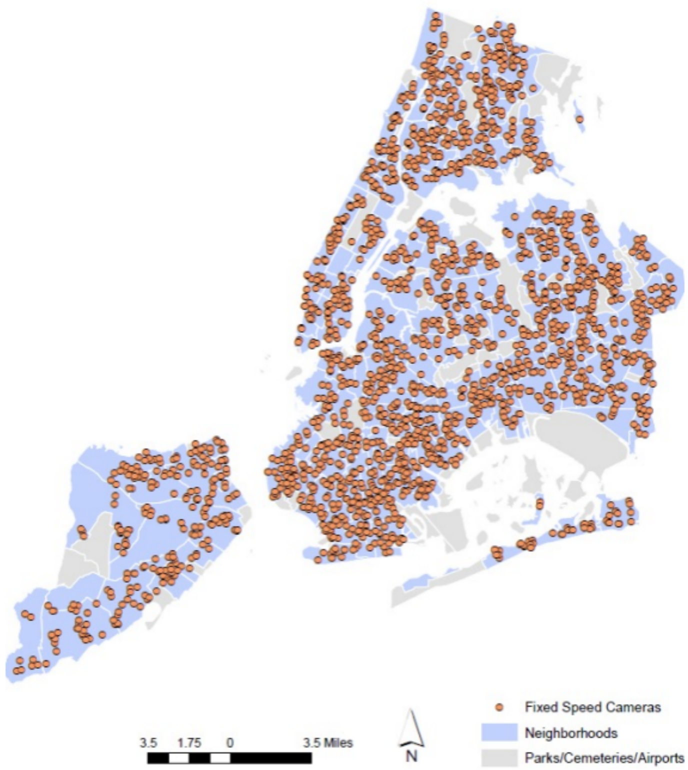


(a) Before VZ implementation (2009-2013).

(b) After VZ implementation (2014 - 2018).

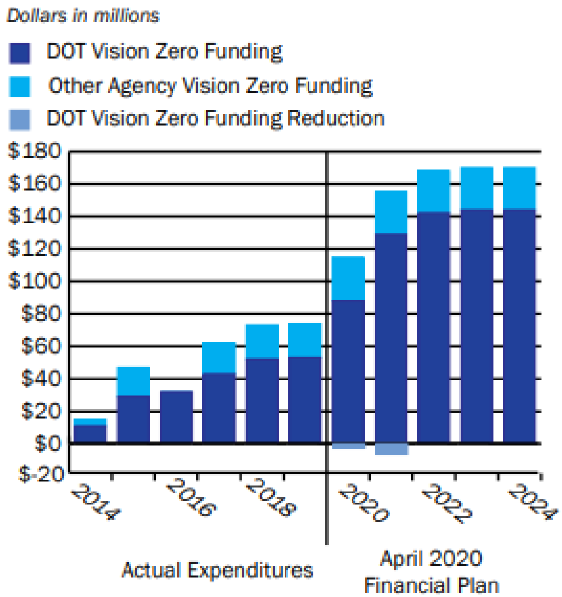
Note: This figure presents the Slow Neighborhood Zones (SNZ) in operation across New York City. The blue polygons in Panel A represent SNZs active before the implementation of Vision Zero (2003–2013), while Panel B shows those established after the policy was introduced (2014–2018). The total number of active zones increased from 9 prior to Vision Zero to approximately 27 by 2018. *Source:* Vision Zero View.

Figure A.5: Location of the cameras operating under the Automated Speed Enforcement (ASE) after the implementation of VZ

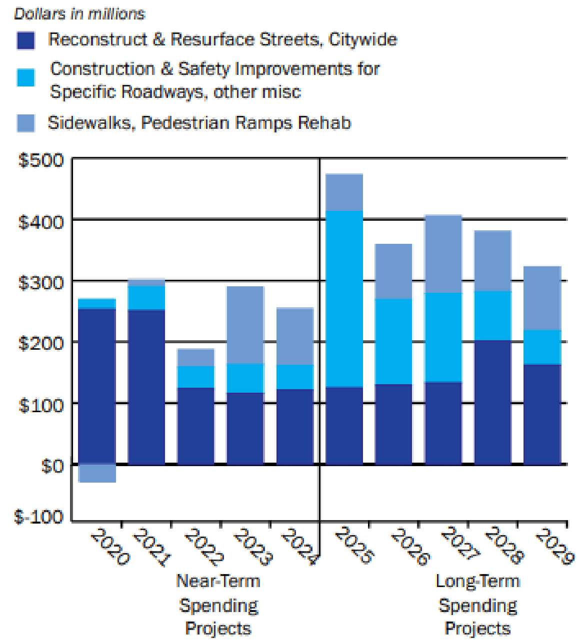


Note: This figure displays the exact locations of all speed cameras operating under the Automated Speed Enforcement (ASE) policy through 2024. Each orange dot represents a camera, while the blue shaded areas indicate neighborhoods, and the grey shaded areas correspond to parks, cemeteries, and airports. The number of cameras expanded significantly, from just 20 used during the 2013 pilot phase to over 2,200 cameras deployed across 750 school zones citywide. *Source:* New York City (2024).

Figure A.6: Total budget for the Vision Zero Program in New York City



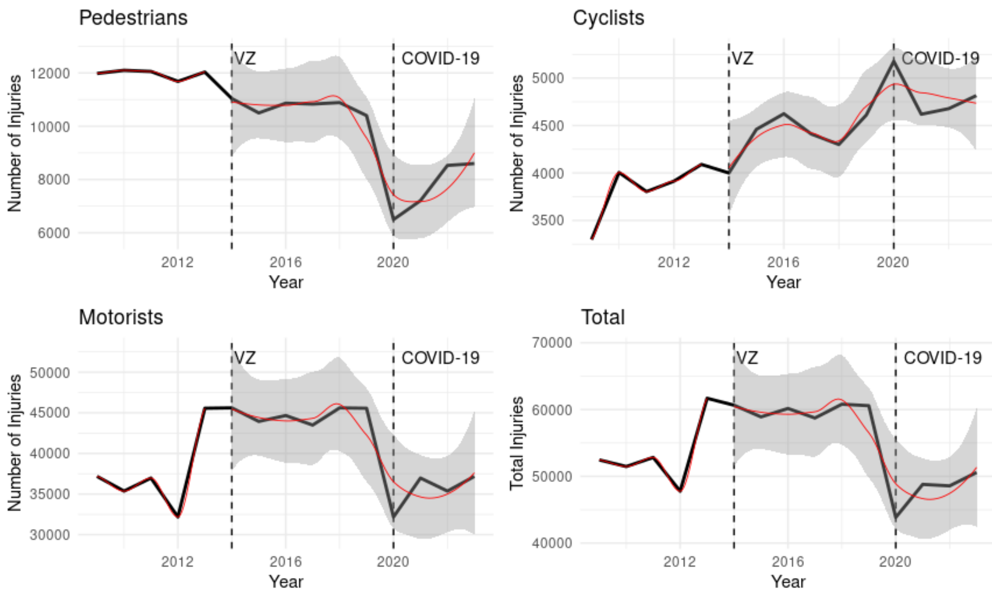
(a) VZ Budget 2014-2024



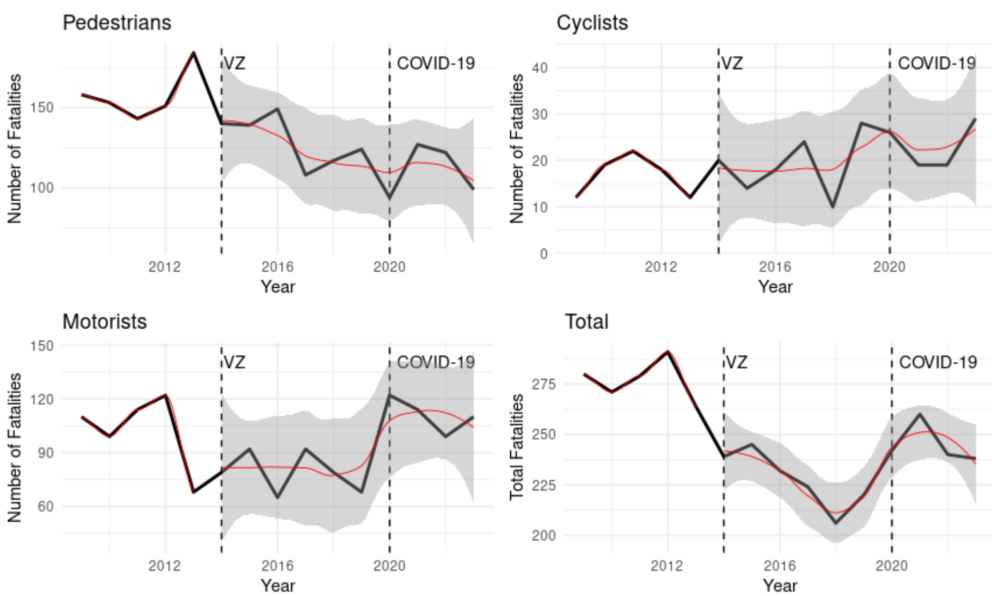
(b) VZ Budget 2024-2029

Note: This figure illustrates the evolution of the executed and projected budget for the Vision Zero (VZ) Program, as well as the agencies responsible for its implementation. The x-axis represents the years, while the y-axis shows budget amounts in millions of dollars. Each bar corresponds to the total executed or projected budget for a given year. Since the report was released in 2020, all figures beyond that year reflect projections available at the time. In Panel A, the dark blue segments indicate the portion of the budget managed by the Department of Transportation (DOT), the lead agency responsible for the implementation and maintenance of Vision Zero. The light blue segments represent the combined budget contributions from other participating agencies. A muted blue shade highlights a slight reduction in DOT's projected budget in 2020 and 2022. Panel B provides a breakdown of the budget by general categories of traffic engineering projects. Dark blue represents street reconstruction and resurfacing, which accounts for the largest share of the budget. Light blue corresponds to construction and safety improvements, while muted blue reflects the portion allocated to sidewalks and pedestrian ramps. *Source:* NYC Independent Budget Office, June 2020 report.

Figure A.7: Comparison of injuries and fatalities before and after the implementation of the VZ



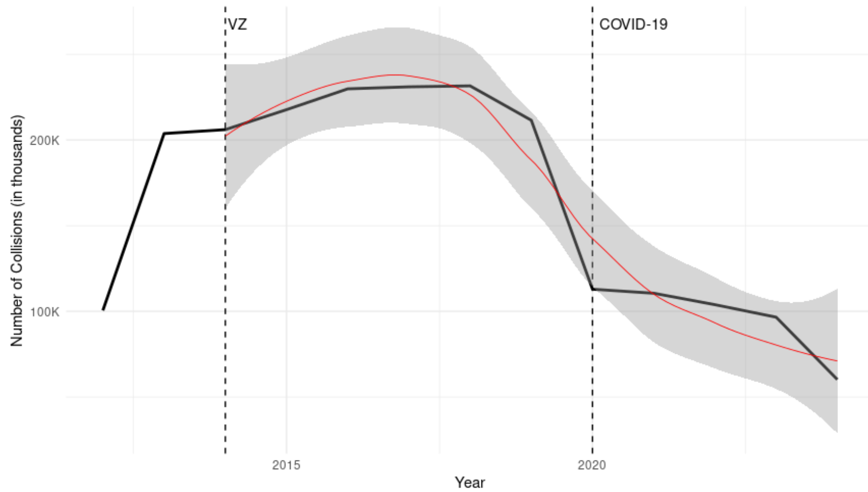
(a) Trends of injuries.



(b) Trends of fatalities.

Note: This figure presents the aggregated trends in raw data for injuries (Panel A) and fatalities (Panel B) from 2009 to 2022. The x-axis in each graph are the years and the y-axis are the count number of injuries and fatalities. The data are drawn from the NYPD motor vehicle accident database, supplemented with totals from the Fatality Analysis Reporting System (FARS) for the years 2009–2012. The black line shows the annual totals based on the raw data, while the red line is a non-parametric fitted trend line that smooths the data before and after the 2014 implementation of the Vision Zero (VZ) policy. The shaded area around the red line represents the 95% confidence interval for the fitted trend. Each panel displays separate trends for pedestrians, motorists, cyclists, and overall totals. A vertical line at 2014 marks the implementation of the VZ policy, while an additional marker denotes the onset of the COVID-19 pandemic. *Source:* NYPD NYC - Motor Vehicle Accidents Database and Fatality Analysis Reporting System (FARS).

Figure A.8: Total number of mother vehicle accidents before and after the implementation of VZ



Note: This figure illustrates the trend in motor vehicle accidents in New York City from 2009 to 2022. The x-axis represents the years, while the y-axis shows the total number of reported accidents. The black line reflects the annual totals from the raw data, and the red line is a non-parametric fitted curve that smooths the trend over time, capturing changes before and after the 2014 implementation of the Vision Zero (VZ) policy. The shaded area around the red line indicates the 95% confidence interval for the fitted trend. A vertical line marks the introduction of the VZ policy in 2014, and an additional marker denotes the onset of the COVID-19 pandemic. *Source:* NYPD NYC - Motor Vehicle Accidents Database and Fatality Analysis Reporting System (FARS).

Table A.1: Components of social average cost of accidents.

By type of cost		**By type of accident**	
Type	(\$/VMT)	Type	(\$/VMT)
WTP of death, injury	0.103	Fatality	0.077
Productivity	0.013	Disabling injury	0.024
Medical expenses	0.008	Other injury	0.033
Property damage	0.007	Property damage only	0.004
Legal, police, fire	0.004	Unknown	0.002
Insurance admin.	0.003	–	–
Traffic delay	0.002	–	–
Total	**0.14**	**Total**	**0.14**

Source: Small and Verhoef (2007), computed from Parry (2004), Tables 1 and 2. Notes: WTP = willingness to pay (for avoidance). All costs are for the U.S. (1998-2000), stated in 2005 prices. Price levels are updated by multiplying the 1998-2000 costs by 1.181, the average between the growth factors of hourly earnings and the Consumer Price Index for all urban consumers (US CEA 2006, Tables B-47, B-60).

Table A.2: Summary Statistics: Treatment vs. Control

	Treatment Mean	Control Mean	Difference
Slow Neighborhood Zones			
Accidents	1.013	1.014	-0.001**
	(0.137)	(0.125)	
Injuries	0.242	0.251	-0.008**
	(0.593)	(0.641)	
Fatalities	0.0008	0.0010	-0.0003*
	(0.030)	(0.035)	
Speed Bumps			
Accidents	1.344	1.021	0.323***
	(0.649)	(0.151)	
Injuries	0.386	0.301	0.085***
	(0.832)	(0.743)	
Fatalities	0.0010	0.0016	-0.0003
	(0.035)	(0.065)	
Automated Speed Enforcement			
Accidents	8.485	1.374	7.110***
	(8.428)	(0.709)	
Injuries	1.885	0.486	1.399***
	(2.634)	(0.962)	
Fatalities	0.0087	0.0016	0.007***
	(0.103)	(0.042)	

Note: This table reports means and standard deviations (in parentheses) of accidents, injuries, and fatalities for treatment and control areas across three traffic calming policies: slow neighborhood zones, speed bumps, and automated speed enforcement. The column "Difference" reports the difference in means between treatment and control groups. Asterisks indicate statistical significance from two-sample t-tests of equal means: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The results indicate that, although many of the differences between treatment and control areas are statistically significant, the magnitudes are generally small and of limited practical importance. The exception is Automated Speed Enforcement, where the treatment areas show a markedly larger difference.