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 collisions without worsening congestion. Speed reduction policies such as slow zones, speed bumps, and automated monitoring systems are designed to enforce lower driving speeds, yet their spatial effects remain poorly understood. This study presents an empirical evaluation of speed calming policy effects on traffic outcomes. I estimate the causal effects of these interventions using New York City as a case study and a spatial difference in differences design, leveraging daily street-level data on motor vehicle collisions, injuries, and fatalities. The analysis exploits spatial and temporal variation in collisions between street segments where each policy was implemented and comparable untreated segments before and after policy implementation. The results reveal heterogeneous impacts across policy types. Stricter enforcement via automated speed cameras is associated with increases in motor vehicle collisions and injuries in treated areas, with spillover effects that also raise collision rates on nearby streets. In contrast, moderate enforcement measures such as slow zones and speed humps show modest, statistically significant reductions in motor vehicle collisions and injuries, with benefits extending beyond treated segments. No statistically significant effects are found for fatalities. Using the estimated coefficients and dollar values of collision costs from the literature, I conduct a cost-benefit analysis showing that slow neighborhood zones and speed bumps tend to be more cost-effective in improving traffic safety than automated speed cameras. These findings highlight the importance of tailoring traffic safety interventions to the urban context and accounting for both localized and spillover effects when designing policies for congested cities.

Keywords: Urban Mobility, Traffic collisions; Law Enforcement; Safe Traffic.
JEL Classification: R41, R48, K42.

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1 Introduction

Motor vehicle collisions represent a major issue in urban transportation, generating large private 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, collisions 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 collisions, 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 collision risk (Couture et al., 2018; Vickrey, 1963). But for urban planners and residents, collisions impose substantial external costs. In traffic-dense states such as California, higher traffic density substantially raises the insurance costs borne by other drivers. Edlin and Karaca-Mandic (2006) estimate external costs of roughly $1,700–$3,200 per driver annually and propose a corrective Pigouvian tax on driving as a policy mechanism to internalize these motor vehicle collision externalities and recover the associated social costs through additional fiscal revenue.

In response to growing safety concerns, many cities across the United States have adopted Vision Zero (VZ) strategies over the past decade, following the success of earlier European implementations. Since Chicago's first commitment in 2012, more than fifty U.S. cities, counties, and metropolitan planning organizations have pledged to eliminate traffic-related fatalities and serious injuries through coordinated Vision Zero programs (Boodlal et al., 2021; Ferenchak, 2023). Early adopters included Austin, Los Angeles, and San Francisco in 2014, with subsequent years seeing rapid diffusion of Vision Zero action plans across medium- and large-sized municipalities. This growing movement underscores the broader relevance of policies aimed at reducing motor vehicle collisions in congested urban settings.

In this context, NYC also adopted its own version of Vision Zero in 2014. 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.

The rationale behind these measures is that, with lower enforced speed limits, traffic outcomes would improve and collisions would be less life threatening and less likely to cause severe injuries. Despite these efforts, motor vehicle collisions in New York City have not declined as expected, and in some years have even increased slightly (see Figure A.4).

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 collision 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) confirms speed as a significant fatality factor even after controlling for confounders; and Synder (1989) shows 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 collision frequency but increased severity due to heterogeneous behavioral responses.

In this paper, I evaluate the effectiveness of three core speed control policies from New York City's Vision Zero program: Speed Bumps, Slow Neighborhood Zones (SNZs), and Automated Speed Enforcement (ASE) cameras. Using high-resolution, geocoded motor vehicle collision data from the New York Police Department (NYPD) and a spatial difference in differences (DiD) framework, I estimate the causal impacts of each policy on traffic outcomes for each street segment and day. The identification strategy exploits variation in the distance of motor vehicle collisions to the nearest implemented policy, where collisions within policy-specific treatment buffers are compared with those located completely outside the reach of any enforcement zone and potential spillover area.

The spatial DiD framework adapts the logic of conventional difference in differences to space rather than administrative units. It compares temporal changes in collision outcomes between street segments exposed to a policy and nearby segments that remain unexposed, under the identifying assumption of spatial parallel trends, that in the absence of treatment, traffic outcomes for treated and nearby untreated street segments would have evolved similarly over time. Treated street segments are defined according to the effective reach of each policy, ensuring that the treatment distance (d^*) reflects actual exposure to enforcement rather than arbitrary buffers. For Speed Bumps, $d^* = 0.014$ miles, corresponding to the braking reaction distance at 25 miles per hour, wich is the maximum speed limit in NYC. For ASE cameras, $d^* = 0.25$ miles, the camera's maximum capture range.

The spatial DiD estimates capture local effects, meaning changes in motor vehicle collisions and injuries within the directly treated zone, and spillover effects, defined as behavioral adjustments in nearby untreated areas (0.015 to 0.025 miles for Speed Bumps, 0.26 to 0.5 miles for ASE, and 0.6 to 1.0 miles for SNZs). Controls comprise street segments located beyond the outer spillover range (0.026 to 0.6 miles for Speed Bumps, 0.6 to 1.5 miles for ASE, and 1.0 to 1.5 miles for SNZs), where no exposure or behavioral influence is expected. For SNZs, whose treated areas are polygonal, I complement the spatial DiD with a border regression discontinuity design (RDD) that compares outcomes just inside and outside the 0.5 mile distance from the polygon border to analyze spillover effects. The identifying assumption for this design is local continuity, meaning that in the absence of the policy, collision outcomes would evolve smoothly at the zone boundary. To avoid contamination, any collision occurring within overlapping treatment of more than one policy is excluded from the estimation sample.

Yet, few evaluations exist for the specific suite of interventions implemented under Vision Zero in U.S. cities. This work fill this gap and contributes to the economics of urban transportation and collision externalities in three ways. First, it provides a disaggregated evaluation of multiple enforcement policies 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 collision 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.

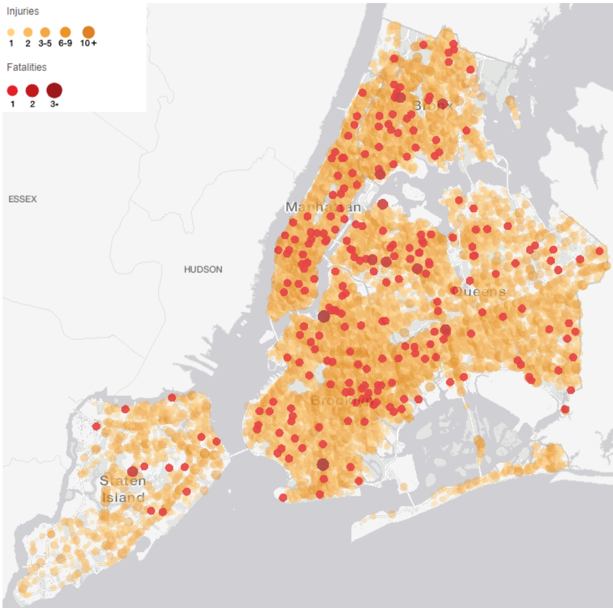
Overall, the results reveal that the effectiveness of speed policies varies considerably across enforcement types. Automated Speed Enforcement (ASE) cameras are associated with a 24.7% increase in motor vehicle collisions and a 29.6% increase in injuries, with spillover effects that appear to worsen safety and congestion in nearby areas. In contrast, physical and awareness-based measures such as Speed Bumps and Slow Neighborhood Zones (SNZs) show modest yet consistent safety improvements: injuries decline by roughly 6–9% near Speed Bumps and by 7.25% at the borders of SNZs, where collisions fall by about 1.1%. The proposed mechanisms behind these patterns are: strict enforcement can trigger abrupt driving responses, such as sudden braking or inconsistent speed adjustments, while awareness-based or design-oriented interventions promote more gradual and sustained behavioral change. Together, these findings highlight the need to tailor enforcement strategies to local conditions and to complement punitive measures with infrastructure and community-based approaches that encourage safer driving behavior. This study thus helps bridge the gap between policy implementation and policy impact, offering evidence-based guidance for allocating enforcement resources to maximize safety and welfare in dense urban environments.

2 Background

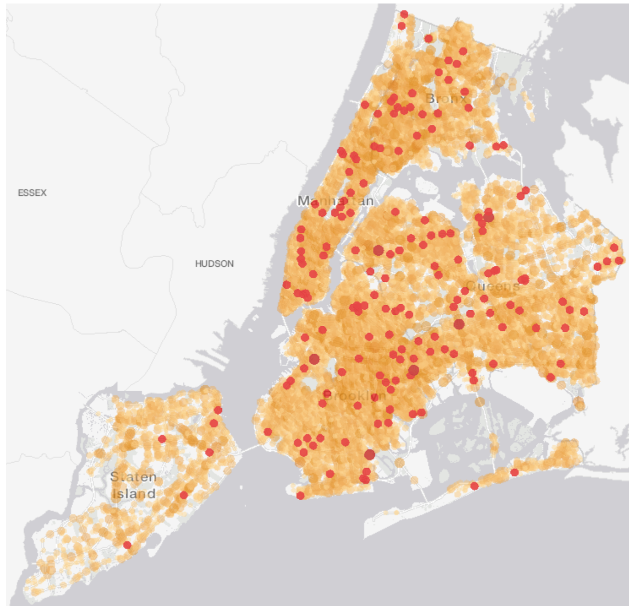
The Vision Zero (VZ) initiative, launched in Sweden in 1997, marked a paradigm shift in road safety policy by setting ambitious targets like eliminating traffic fatalities and injuries. Its success influenced policies across Europe, including in Norway and the Netherlands. Research shows VZ's effectiveness: 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 1.

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



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



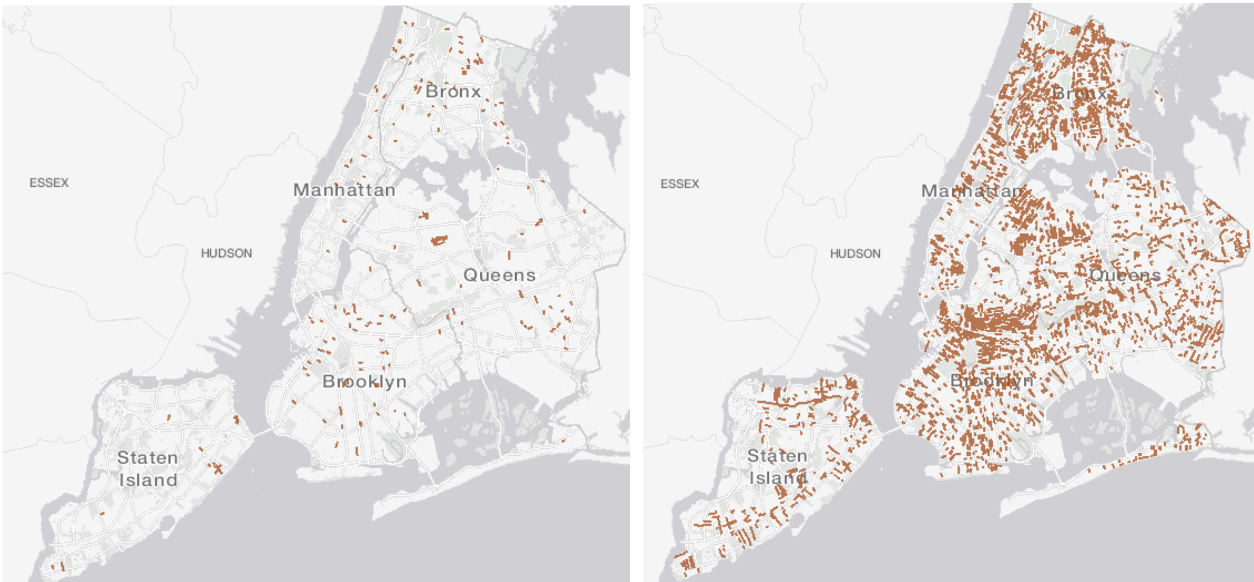
(b) After VZ implementation (2014 - 2018).

Note: This figure illustrates the spatial intensity of motor vehicle collisions 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.

Some of the NYC VZ policies focus on speed limit enforcement, as studies tend to show that speed calming measures reduce collisions (Hess, 2004; Hess and Polak, 2003; Hu and McCartt, 2016; Novoa et al., 2010). Speed reductions of up to 5 mph can change collision 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.1 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 2). 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 collisions involving pedestrians and cyclists.

Figure 2: Speed bumps operating before and after the implementation of VZ



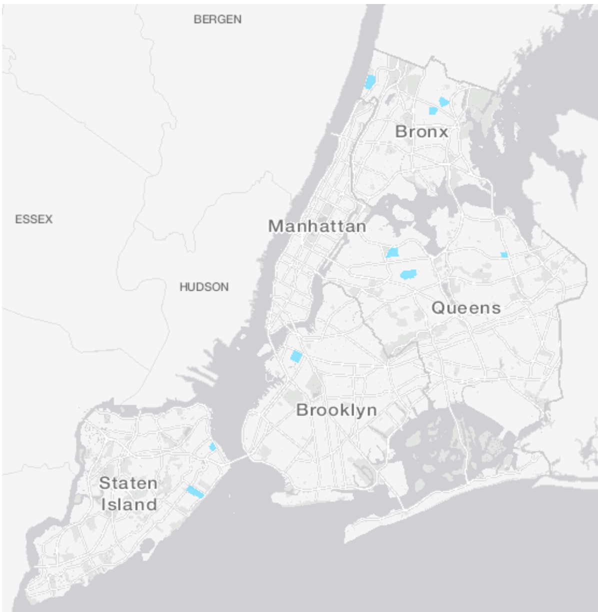
(a) Before VZ implementation (2009-2013). (b) After VZ implementation (2014 - 2018).

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.

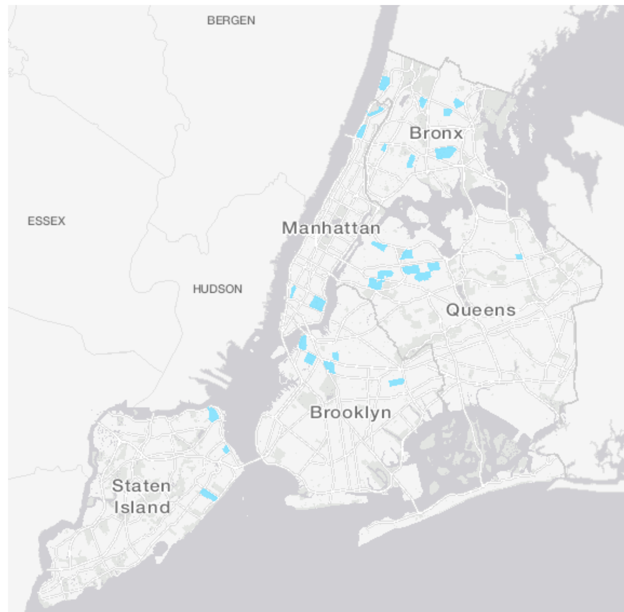
By the end of 2018, New York City had installed approximately 1,678 new speed bumps across its five boroughs. A speed bump is a raised section of pavement, typically about four inches above the road surface, designed to reduce vehicle speed and enhance pedestrian safety. The installations were concentrated in street segments close primarily implemented near schools and senior facilities, where pedestrian vulnerability is greatest. Areas such as East New York in Brooklyn, the South Bronx, and several parts of Queens experienced particularly high numbers of installations. Enforcement associated with speed bumps includes warning signs and roadway markings that alert drivers in advance.

The implementation of Slow Neighborhood Zones (SNZs) (Figure 3) represents an initiative to promote pedestrian-friendly environments across New York City. As of 2018, a total of 26 zones had been established, primarily in residential areas with low traffic volumes and minimal through traffic. Each zone is defined by a posted speed limit of 20 miles per hour and enforced through a combination of gateway signage, pavement markings, and other traffic calming treatments that alert drivers upon entry. These zones are characterized by an integrated set of traffic calming measures and infrastructure enhancements designed to encourage safer driving behavior and discourage speeding. Common interventions include physical modifications to the roadway such as speed bumps, chicanes, raised crosswalks, and curb extensions. Importantly, local community input plays a central role in the selection and design of Slow Neighborhood Zones, ensuring that implementation aligns with neighborhood-specific safety concerns.

Figure 3: Slow-Neighborhood Zones (SNZ) operating before and after the implementation of VZ



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

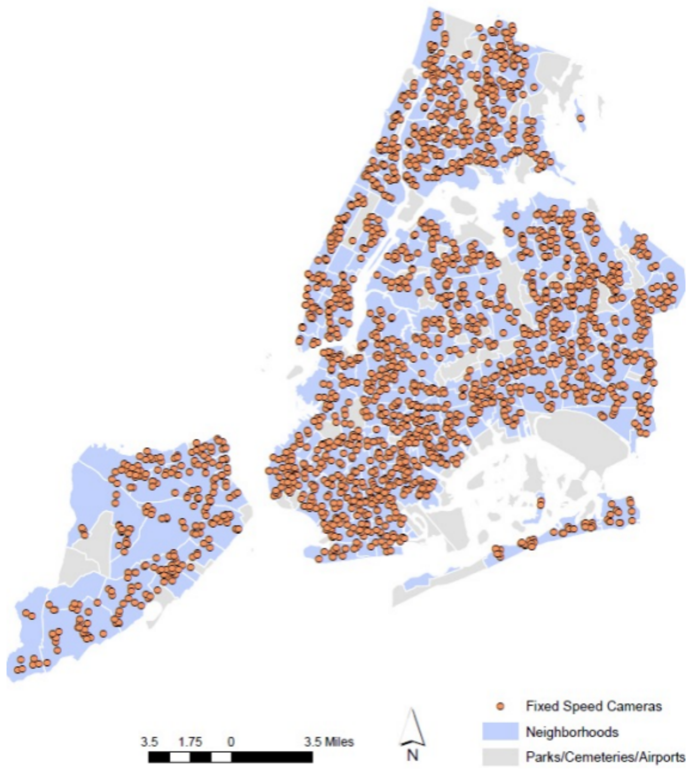


(b) After VZ implementation (2014 - 2018).

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.

The New York City Automated Speed Enforcement Program (Figure 4) 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.

Figure 4: 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).

Initially launched with a limited number of speed cameras, the program grew steadily as its scope and legal authorization expanded. By 2018, approximately 825 cameras had been installed across the city. Following the passage of New York State legislation in 2019, the program underwent a major expansion that increased the number of devices and extended their hours of operation. As of 2020, more than 2,000 cameras were operating in roughly 750 school zones, functioning 24 hours a day and seven days a week. This shift marked a significant departure from the earlier restrictions that had 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.2). 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 collisions recorded by the NYPD at the time of collision occurrence and obtained from the NYC Open Data portal. The dataset covers the period from 2012 to 2018 and includes detailed geocoded records of collision locations along with the number of injuries and fatalities and some collisions characteristics. This dataset is relatively new and has not yet been extensively explored, mainly due to its size, which requires cloud computing resources to extract the full range of available periods through Application Programming Interfaces (APIs). Substantial data cleaning and organization are also necessary before conducting regression analysis, particularly at the daily level, which is uncommon in the existing literature, where temporal aggregation is often applied to reduce data volume.

Two limitations of the data are worth noting. First, collisions that result in no damage or damages below $1,000 may not be included, even if they were reported. Second, unreported collisions are also not included in the dataset. Even with those limitations we have a very large sample with potentially the majority of the collisions in NYC. 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 (Figures A.3 and A.4), 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 collision. These variables allow us to control for potential determinants of collisions 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] These variables are included as dummy indicators for each group, excluding one category in each case to avoid perfect collinearity. Including these controls represents an advancement in general transportation modeling, as it helps mitigate omitted variable bias arising from latent or context-specific factors influencing collision outcomes. In addition, data on the geographic coordinates, and other specific features of slow neighborhood zones, speed bumps, and the automated

[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 collision, they offer valuable insights into potential behaviors or circumstances, often unobservable, that may have contributed to the incident.

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.3). However, NYC continues to report a high injury rate, with at least one person injured in every two collisions, significantly exceeding the national average of approximately 0.46 injuries per collision.

The trend in total collisions remained stable in New York until 2018 (Figure A.4). Despite 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.2).

Table 1 presents descriptive statistics for motor vehicle collisions, injuries, and fatalities before (2012–2013) and after (2014–2018) the implementation of Vision Zero. The data are disaggregated into treated and control street segments according to my definitions of distance, allowing for a comparison of baseline levels and post-policy changes in traffic outcomes. In the pre-policy period, treated areas exhibit higher averages of collisions and injuries than control areas.

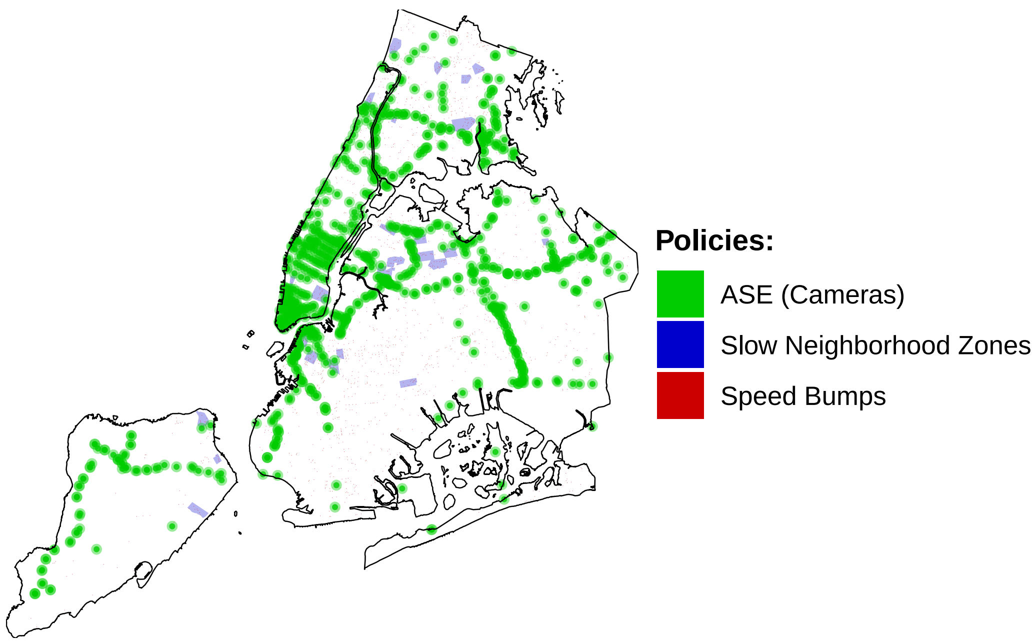
Table 1: Summary Statistics: Collisions, Injuries, and Fatalities Before and After Vision Zero Implementation

	Before (2012 - 2013)			**After (2014 - 2018)**		
	N	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**
Treatment						
collisions	23,155	4.678	5.180	75,641	5.316	5.395
Injuries	23,155	1.058	1.756	75,641	1.170	1.895
Fatalities	23,155	0.006	0.083	75,641	0.005	0.079
Control						
collisions	36,507	1.990	1.585	144,000	1.941	1.421
Injuries	36,507	0.620	1.107	144,000	0.547	1.031
Fatalities	36,507	0.003	0.055	144,000	0.002	0.051
Total						
collisions	59,662	3.033	3.697	220,000	3.101	3.728
Injuries	59,662	0.790	1.411	220,000	0.761	1.421
Fatalities	59,662	0.004	0.067	220,000	0.003	0.062

Note: The table reports descriptive statistics for motor vehicle collisions, injuries, and fatalities before (2012–2013) and after (2014–2018) the implementation of Vision Zero. Observations are at the street segment by day level. Treated segments correspond to areas within the policy-specific treatment buffers of Speed Bumps, Slow Neighborhood Zones, or Automated Speed Enforcement cameras. Control segments are located beyond the maximum spillover distance of any intervention and exclude segments overlapping multiple policy zones.

Following Vision Zero implementation, the number of recorded events increases in both treated and control segments. However, the magnitude of change differs across groups: in treated segments, the mean number of collisions rises slightly from 4.68 to 5.32 per day and injuries from 1.06 to 1.17, while fatalities remain roughly constant. In control segments, average collisions remain stable and injuries decline slightly, suggesting that treatment zones continued to experience relatively higher collision intensity even after policy adoption. These descriptive patterns underscore the spatial heterogeneity that motivates the difference-in-differences strategy employed in the empirical analysis.

Figure 5: Spatial distribution of Vision Zero interventions and treatment buffers in New York City (2012-2018).



Note: The map displays Automated Speed Enforcement (ASE) cameras, Speed Bumps, and Slow Neighborhood Zones (SNZs), along with their respective treatment areas. Collisions occurring in the overlap of treatment buffers are excluded from the analysis to avoid contamination across policies.

Figure 5 provides a spatial overview of the three Vision Zero interventions analyzed. The figure illustrates the spatial concentration of policies across the five boroughs and highlights areas where enforcement zones overlap. Collisions occurring within these overlapping buffers are excluded from the estimation sample to prevent contamination across treatments and to preserve the validity of the counterfactual comparisons. Addressing this overlap is an important empirical challenge, as failing to do so could bias the estimated policy effects upward by attributing the influence of multiple simultaneous interventions to a single policy.

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 the speed calming policies, 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:

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

Here, Y_{st} denotes the number of collisions, 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. 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 collision-level controls such as contributing factors listed as potential influences to the collision[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 unobservable differences in traffic location characteristics, and standard errors are clustered at the street-segment level to account for spatial correlation.

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

[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.

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 collision location to the nearest SNZ border.

To ensure consistency in spatial exposure across enforcement types, I adopt a common structure for defining treatment, spillover, and control areas. Treated segments correspond to the immediate range of policy exposure based on the physical or enforcement reach of each intervention. For Automated Speed Enforcement (ASE) cameras, this radius extends approximately 0.25 miles from each camera. For Speed Bumps, it corresponds to about 0.014 miles, reflecting the braking reaction distance at 25 miles per hour. For Slow Neighborhood Zones (SNZs), the treated area includes all street segments within each designated polygonal zone. Spillover zones are defined as the ranges immediately beyond these treatment boundaries, roughly up to 0.5 miles for ASE cameras, 0.025 miles for Speed Bumps, and 1.0 mile for SNZs, capturing indirect behavioral adjustments. Control areas are located beyond these outer limits, extending up to 1.5 miles, where no policy exposure or spillover influence is expected. This consistent spatial classification allows for direct comparison of local and indirect effects across policies while minimizing contamination between treatment and control groups.

Because Slow Neighborhood Zones (SNZs) are polygonal areas, I estimate a border regression discontinuity design (RDD) to measure the policy's short-range spillover effects at the zone boundaries that would be attenuated or averaged out in the spatial DiD framework. The specification is given by:

$$Y_{zt} = \theta + \tau\, SNZ_{zt} + f(D_z) + \beta \mathbf{X}_{zt} + \alpha_z + \gamma_t + \varepsilon_{zt} \tag{2}$$

where Y_{zt} denotes the number of motor vehicle collisions, injuries, or fatalities for segment z at time t. The variable SNZ_{zt} equals one if a collision occurs inside a Slow Neighborhood Zone after policy implementation. The coefficient τ captures the discontinuity in outcomes at the SNZ boundary after implementation and represents the average treatment effect at the border (ATT). The function $f(D_z)$ is a flexible polynomial of the distance to the SNZ border including collisions within 0.5 miles of distance from the border, allowing for smooth changes in outcomes with distance. The vector $\mathbf{X}_{zt}$ includes observable crash-level covariates, while α_z and γ_t denote segment and time fixed effects, respectively. The key identifying assumption of this design is local continuity: in the absence of the policy, outcomes would evolve smoothly across the SNZ border, such that any discontinuity after implementation can be attributed to the treatment.

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:

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

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 collisions intensity, since treated street segments may have been selected for intervention based on high collision rates in the automated enforcement program case, I implement a synthetic difference-in-differences (SDiD) estimator as proposed by Arkhangelsky et al. (2021). For the other two cases the implementation decision seems to be guided by other factor that are not necessarily associated with high rates of collisions. 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:

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

Here, Y_{st} is the number of collisions, 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:

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

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

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 Automated Speed Enforcement (ASE), speed bumps, and Slow Neighborhood Zones (SNZ). The general interpretation of the result tables is as follows: the main outcome variables appear in the rows of the first column, and the tables contain two groups of results, localized effects which capture the impacts within the treatment areas, and spillover effects which measure the impacts in areas just beyond the treatment boundaries as defined by the distance buffers. The different model specifications are shown in the columns. The baseline estimation is an OLS DiD, while the robustness models include a spatial DiD Poisson regression and a zero inflated model. Specifically for Automated Speed Enforcement, I present a synthetic DiD. For the spillover effects at the border of the Slow Neighborhood Zones, all results in the spillover columns are estimated using the proposed RDD specification.

5.1 Automated Speed Enforcement (ASE)

Table 2 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 collisions and injuries compared to similar areas not exposed to treatment. In my preferred specification for collisions (column 2), I observe a rise by 0.493 collisions, and injuries increase by 0.184 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. The row *Control Mean %* reports the percentage change of the estimated coefficient relative to the pre-treatment control group mean for collisions, injuries, and fatalities.

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

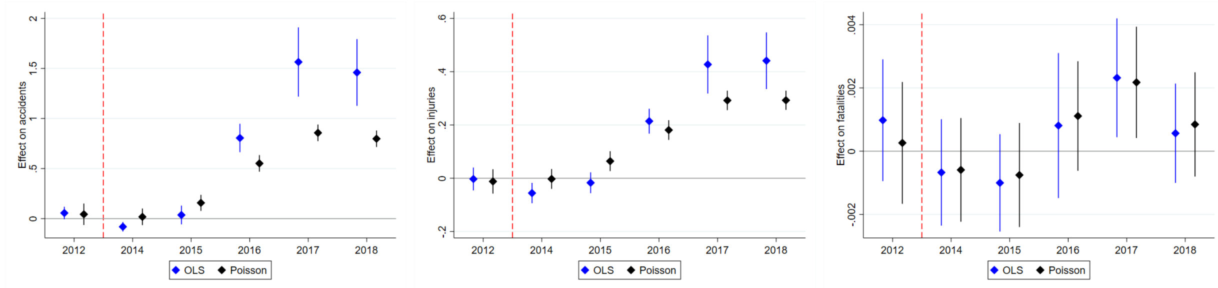
	Localized Effects				Spillover Effects			
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Collisions	0.723***	0.493***		0.617***	0.284***	0.226***		0.243
	(0.069)	(0.028)		(0.103)	(0.048)	(0.021)		(0.157)
R^2	0.194	0.150			0.119	0.054		
Control Mean %	36.3%	24.7%		31%	14.27%	11.3%		12.2%
Injuries	0.198***	0.184***	0.076***	0.196***	0.076***	0.082***	0.022**	0.254***
	(0.021)	(0.012)	(0.017)	(0.066)	(0.012)	(0.011)	(0.007)	(0.082)
R^2	0.138	0.045			0.021	0.018		
Control Mean %	31.9%	29.6%	12.2%	31.6%	12.2%	13.2%	3.5%	40.9%
Fatalities	0.00005	0.0004	-0.00003	0.003***	-0.0007	-0.0002	-0.0003	-0.001
	(0.0007)	(0.0005)	(0.0006)	(0.0004)	(0.0006)	(0.0005)	(0.0004)	(0.008)
R^2	0.0006	0.015			0.0004	0.011		
Control Mean %	-	-	-	100%	-	-	-	-
Observations	279,687	279,687	279,687	144,875	258,320	258,320	258,320	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 collisions, 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 0.6–1.5-mile distance band. Preferred estimates are in columns 2 and 3 and 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 except for the synthetic DiD model. 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 collisions 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 collisions extending up to 0.5 miles beyond those areas.

Figure 6 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 collisions and injuries, with near-zero effects through 2015, followed by significant and relatively stable positive effects in the subsequent years.

Figure 6: Annual treatment effect of Automated Speed Enforcement (ASE) on traffic outcomes



(a) Collisions (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 collisions, injuries, and fatalities). The estimates reflect the treatment effect for street segments located within the 0.6–1.5 mile distance band, compared to the control group. Panel A displays results for collisions, 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.

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. collisions show a statistically significant average daily increase of 0.61 collisions in treated street segments relative to their synthetic control, significant at the 1% level. For injuries, the estimated effect is an additional 0.19 injuries per day in treated segments compared to the control at 1% level. As with previous models, the results for fatalities remain statistically insignificant.

5.2 Speed bumps

Table 3 presents the estimated local and spillover effects of Speed Bumps on traffic outcomes. For collisions, the OLS estimates show a small but statistically significant increase of about 0.026 collisions per segment per day, while the Poisson and Zero Inflated models yield similar magnitudes though not statistically distinct from zero. For injuries, the results indicate a reduction of approximately 0.038 to 0.057 injuries per day in treated areas, which is statistically significant at conventional levels under the OLS and Zero Inflated specifications. Fatalities remain statistically insignificant across all models. The overall magnitude of the coefficients suggests that, although the effects are measurable, they are small in practical terms. Spillover effects for all outcomes are statistically insignificant, except for a modest reduction of collisions of 0.006 less crashes per day indicated for the Poisson estimation. This result is just significant at 10% level, and is 0.30% of decrease compared with the control mean previous the treatment period, indicating that behavioral responses to speed bumps do tend to be very modest to collisions and not extend beyond the immediate treatment distance for injuries and fatalities.

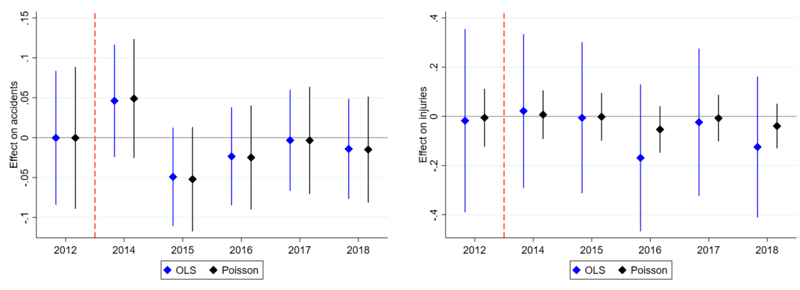
Table 3: Local and spillover effects of speed bumps on traffic outcomes

	Localized Effects			Spillover Effects		
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Collisions	0.026***	0.026***		-0.006	-0.006**	
	(0.002)	(0.006)		(0.003)	(0.003)	
R^2	0.011	0.0001		0.043	0.003	
Control Mean %	1.31%	1.31%		–	-0.30%	
Injuries	-0.038***	-0.041	-0.057***	-0.004	-0.004	0.001
	(0.008)	(0.089)	(0.008)	(0.013)	(0.012)	(0.013)
R^2	0.009	0.012		0.007	0.008	
Control Mean %	-6.13%	–	-9.19%	–	–	–
Fatalities	0.0004	0.0003	0.00003	-0.0009	-0.0006	-0.0006
	(0.0005)	(0.0009)	(0.0005)	(0.0006)	(0.0006)	(0.0006)
R^2	0.003	0.147		0.002	0.002	
Control Mean %	–	–	–	–	–	–
Observations	48,012	48,012	48,012	55,151	55,151	55,151
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 before detecting a speed bump. The control group is drawn from a 0.026–0.6-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.

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 7.

Figure 7: Annual treatment effect of speed bumps on traffic outcomes



(a) Collisions (b) Injuries

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 0.026–0.6 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.

The results indicate no statistically significant effects of the policy implementation over time on collisions 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.

Taken together, these findings suggest that Speed Bumps have, at most, modest and short lived safety effects that are difficult to distinguish from zero once model uncertainty is considered. This pattern aligns with evidence from prior transportation studies (Huang and Cynecki, 2000; Pau and Angius, 2001; Yeo et al., 2020), which find that drivers typically slow down only immediately before the bump, about thirty meters on average, before quickly accelerating again. While such localized deceleration may offer some protection to pedestrians in that narrow range, it is unlikely to meaningfully reduce collisions overall. In dense urban environments like New York City, the overall effect of Speed Bumps appears to be limited to short range behavioral adjustments rather than broader changes in traffic safety conditions.

5.3 Slow Neighborhood Zones (SNZ)

Table 4 reports the localized effects (Columns 1–3) and spillover effects (Columns 4–6) of Slow Neighborhood Zones (SNZ) on the outcome variables. For collisions, 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 SNZs may have shifted traffic dynamics such that nearby areas outside the zones experienced modest reductions in collisions. For injuries, localized effects are again statistically insignificant, while spillover effects display consistent negative and statistically significant coefficients, indicating that SNZ 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.

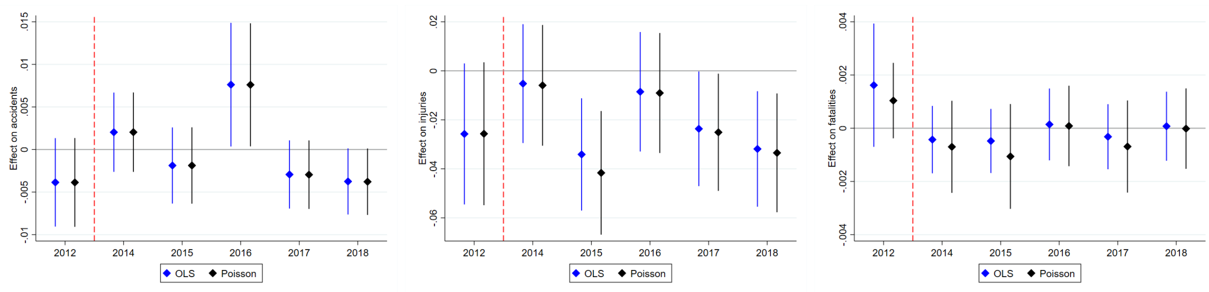
Table 4: Local and spillover effects of Slow Neighborhood Zones (SNZ) on traffic outcomes

	Localized Effects - DiD			Spillover Effects - RDD		
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	
Control Mean %	-	-		0.3%	1.10%	
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
Control Mean %	-	-	-	-	7.25%	6.61%
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
Control Mean %	-	-	-	-	-	-
Observations	376,416	376,416	376,416	261,933	261,933	261,933
Clustered	Yes	No	Yes	No	No	No
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 the spillover effect estimated with the RDD. The treated street segment in the DiD estimations refers to accidents within the SLZ polygon. In the case of the RDD, it uses the distance to the border, considering a 0.5-mile distance. The control group for the RDD is drawn from a 1–1.5 mile distance band outside of the polygon area. Preferred estimates are in columns 4 and 5. Fixed effects include year, month, and day of the week. All robust standard errors in parentheses. For the Poisson estimators, the pseudo R-squared is presented. *** p<0.01, ** p<0.05, * p<0.1.

Figure 8 presents the dynamic estimations, capturing the evolution of policy effects in the years following SNZ implementation. For collisions, 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 collision 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 from 2016 on aligns with the spillover estimates, hinting at a modest and consistent reduction in injury rates at the border of the SNZ.

Figure 8: Annual treatment effect of slow neighborhood zones on traffic outcomes



(a) collisions (b) Injuries (c) Fatalities

The results provide consistent evidence of modest safety gains from SNZ implementation. While localized effects are generally negligible, the spillover results suggest that these zones may contribute to reducing both collisions and injuries in the surrounding areas. Overall, the evidence indicates that SNZs may have small but meaningful impacts on improving traffic safety beyond their immediate boundaries, though the magnitude of these effects remains modest. Those zones may cause a better response at the border because is where they are better signalized, with gateways, and in general they advertise about pedestrians and probably the presence of schools, which may make drivers more sensitive to it. Because these zones are implemented with community involvement, residents who enter and exit them daily are more familiar with their design and purpose, which likely enhances compliance with lower speed limits.

6 Mechanisms

The estimated effects of Automated Speed Enforcement (ASE), speed bumps, and Slow Neighborhood Zones (SNZ) 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 collisions and injuries around camera locations is consistent with behavioral responses by drivers. When motorists suddenly realize that they are entering an enforcement zone, especially one involving monetary penalties, they may brake abruptly or vary their acceleration, particularly if they were exceeding the limit just before detection. This reaction occurs because drivers cannot quickly rationalize the trade-off between the immediate cost of decelerating and the potential fine for speeding. Such sharp adjustments in speed can create turbulence in traffic flow, raising the likelihood of rear-end or side collisions. The increase in injury incidence supports the interpretation that behavioral adjustments near enforcement zones amplify both the likelihood and intensity of collisions, implying that enforcement-induced speed variability contributes to greater crash severity.

A second channel relates to congestion. Enforcement cameras are typically installed in areas already prone to heavy traffic. Compliance with lower posted speed limits may slow overall traffic, intensifying close interactions among vehicles and pedestrians. The broader positive spillover effects on collisions and injuries increasing up to 0.5 miles away, suggest that congestion patterns adjust well beyond the treated street segments, magnifying the risks of collisions 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. Fatality outcomes remain statistically indistinguishable from zero, implying that although collision and injury rates rise near enforcement zones, the intensity of these events does not systematically increase to fatal levels. This result may also reflect the limited variation and low frequency of fatal crashes in the data, which constrains the ability to detect statistically significant effects.

For SNZs, 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 SNZ 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 low due to street design. These zones are typically located in neighborhoods with narrower streets, fewer lanes, and lower traffic volumes, which naturally limit vehicle speed and reduce the potential for severe collisions.

Another possible channel is psychological salience: because SNZs 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 collisions, 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 is consistent with prior studies showing that drivers tend to slow down only at the bump itself and quickly resume their previous speed afterward. The mechanism operates through highly localized deceleration, which may enhance safety for pedestrians at the immediate crossing point but does not substantially alter overall collision patterns. In dense urban environments, these design features appear to produce limited behavioral adjustments rather than broad or sustained improvements in traffic safety, consistent with the statistical insignificance of the 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 (SNZs) 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 preferred estimates from the spatial DiD and RDD models to conduct a back of the envelope calculation that compares the monetary value of the estimated policy effects with the approximate annual costs of implementing each intervention. To convert changes in collisions and injuries into monetary terms, I rely on the social average cost of traffic collisions reported in the consolidated literature, particularly Small (1997) and Parry (2004), summarized in Table A.1. These values provide a benchmark for the cost per collision and the cost per injury.

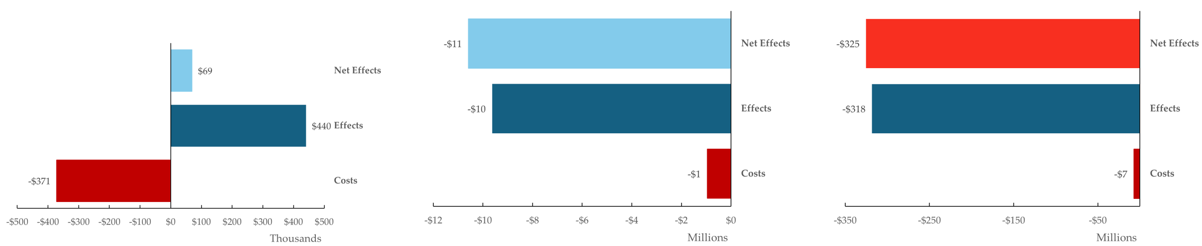
Figure 9 presents the results of this exercise for Slow Neighborhood Zones, speed bumps, and automated camera enforcement. The bars represent annual policy effects expressed in dollars after converting the estimated number of prevented or additional collisions and injuries into monetary units. These are compared directly with the approximate yearly cost of installing each policy.

To construct the annual effects, I take the most parsimonious and statistically reliable coefficients from the preferred estimations for each policy. Using these coefficients, I compute the aggregated number of collisions and injuries that each intervention prevents or generates over the course of one year. Positive values indicate that the policy prevents collisions or injuries. Negative values indicate that the combined localized and spillover effects worsen safety outcomes and generate additional incidents.

To convert these changes into monetary values, I multiply the estimated annual number of collisions and injuries by the corresponding cost per incident. Following Parry (2004) I apply a value of approximately 0.14 dollars per collision and 0.33 dollars per injury, scaled by the average vehicle miles traveled in New York City, which was 10.768 miles in 2018. This produces the annual monetary effects plotted in Figure 9. These values reflect the social cost of additional or prevented collisions and injuries relative to the baseline.

On the cost side, I compare these monetary effects with the estimated yearly cost of implementing each policy. For automated cameras, I use the lowest available installation cost per unit, around $60,000 dollars, recognizing that actual expenditures likely vary across locations. For speed bumps I used the reported monetary value of $4,000.00 dollars per unit installed. For Slow Neighborhood Zones, I construct an approximate implementation cost based on the standard design layout provided in the DOT documentation, resulting in about $100,000.00 dollars. This estimate is subject to uncertainty, because the DOT does not publicly release full itemized construction and installation costs. Speed bump installation costs are taken from publicly available municipal averages and scaled to the number of installations.

Figure 9: 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 thousands or millions USD. Effects are computed based on significant reductions (or increases) in collisions, injuries, and fatalities. Positive monetary values in the effects bar indicate reduction in accidents and injuries and negative values indicate increase.

Figure 9 summarizes the implementation costs, effects, and net effects of the three main traffic enforcement policies under study. The results provide clear evidence of heterogeneity in the cost-effectiveness of these interventions.

First, Slow Neighborhood Zones generate clear net social benefits. Their relatively low implementation costs are more than offset by the estimated reductions in collisions and injuries, resulting in positive net effects. Speed bumps display a markedly different pattern when evaluated through the combined lens of estimated effects and implementation costs. The annual social effects of speed bumps are negative and sizable, with estimated safety impacts amounting to roughly ten million dollars in net social losses. Although implementation costs are relatively modest, on the order of one million dollars per year, the negative safety impacts dominate, producing net effects of approximately negative eleven million dollars. This pattern reflects the empirical findings that speed bumps generate increases in collisions that may be less severe given the reduction in injuries across treated segments, suggesting that the intervention may introduce new risks of collisions that potentially outweigh its benefits given the very small reduction of accidents in the spillover street segments. In this setting, the narrow spatial reach of each bump, combined with abrupt deceleration and acceleration behavior in dense traffic environments, appears insufficient to produce neighborhood level safety gains. Instead, the aggregate effect is that the social costs generated by additional crashes exceed the value of any prevented injuries, leading to a net negative outcome despite the policy's low implementation cost. The findings from slow neighborhood zones align with from earlier transportation studies that document safety gains from passive engineering measures. However the results from speed bumps, departs from this consensus, underscoring that effectiveness may depend heavily on street layout, traffic composition, baseline congestion and specially on how communities are involved in the policy design process.

By contrast, the automated speed enforcement program shows a markedly different pattern. Rather than generating measurable safety improvements, the estimates indicate that camera enforcement is associated with increases in both collisions and injuries within the enforcement zone. When these effects are translated into monetary terms, they produce negative benefits that outweigh the costs of implementation, resulting in large net social losses. This pattern is consistent with concerns raised in the transportation literature that automated enforcement may inadvertently heighten risks by triggering abrupt braking, encouraging rerouting into adjacent untreated streets, or inducing forms of risk compensation among drivers. In this context, the camera program appears not only ineffective but potentially counterproductive, imposing additional social costs rather than delivering safety gains.

It is important to emphasize that this is a rough cost benefit exercise. It does not include operation or maintenance costs, which could vary substantially across interventions, nor does it account for the possibility that collision costs differ across neighborhoods. The cost of a collision is also taken as an average across the literature and may not capture the full distribution of severity in New York City. Nonetheless, this comparison provides a useful benchmark for understanding how the estimated safety effects translate into monetary terms and for highlighting the relative efficiency of each policy.

From a policy perspective, the contrast across interventions is instructive. Engineering-based solutions that involve the communities (SNZs) 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 collisions and injuries.

Overall, these findings suggest that scaling up low-cost, localized interventions were education strategies are part of the engineering intervention and communities are involved may represent a more efficient path 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. Another potential option is to pair existing speed calming measures with stronger financial penalties. Drivers often adjust their behavior based on the perceived cost of noncompliance, and higher fines can shift this calculus in a meaningful way. If the expected penalty is sufficiently large relative to a driver's income, the incentive to reduce speed becomes stronger, potentially enhancing the effectiveness of the engineering interventions already in place.

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 collisions, 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 collision 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 analysis that sheds light on which types of policies may work best in dense urban environments, allowing policy makers to rationalize the cost effectiveness of those policies.

The findings reveal spatially counterintuitive effects and heterogeneous outcomes by enforcement type. Stricter measures, such as automated enforcement through speed cameras, appear to increase collisions and injuries in treated areas relative to their counterfactuals. These high-intensity enforcement policies also generate spillovers, meaning that collision 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 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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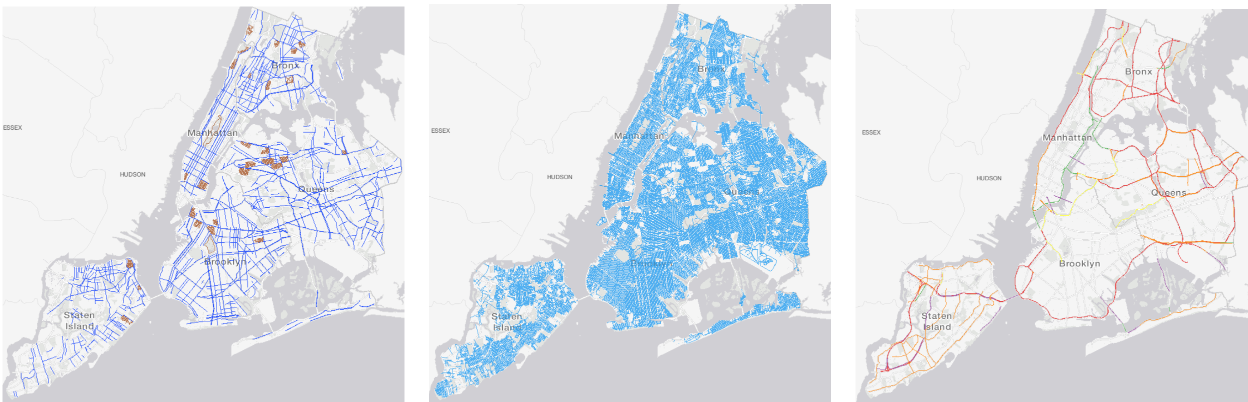
Appendix A - Descriptive Evidence

Figure A.1: Speed limits after the implementation of the Vision Zero program



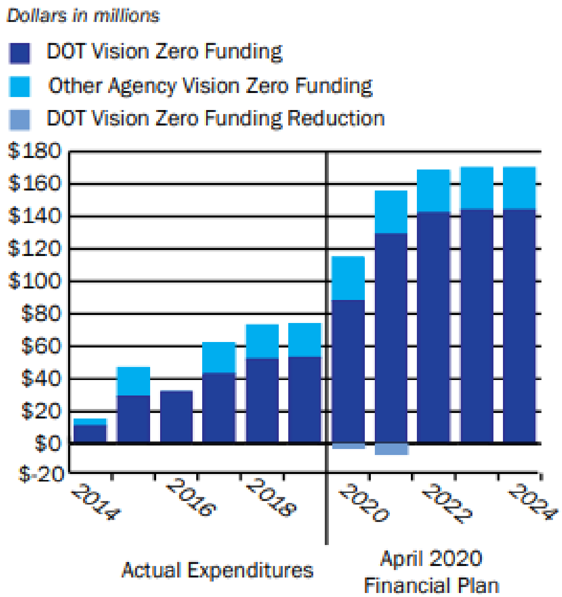
(a) Speed between 20 and 25 miles per hour on signs.

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

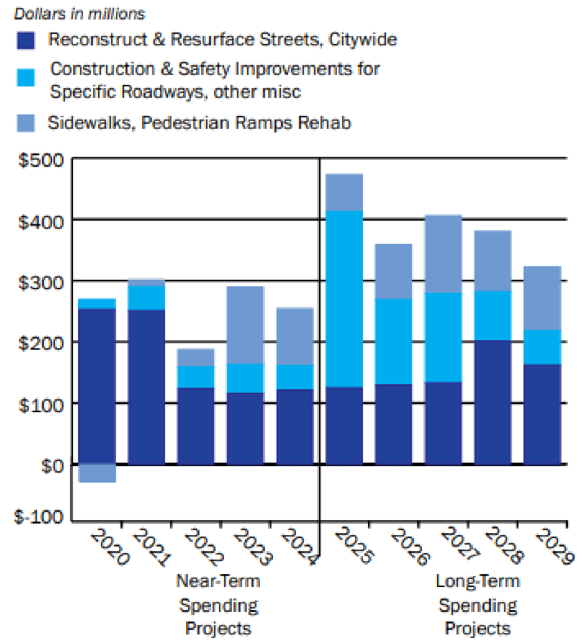
(c) Speed between 30 up to 50 miles per hour on signs.

Note: This figure displays three panels illustrating the new speed limits introduced in New York City after the adoption of the Vision Zero (VZ) policy. Before the policy, the default citywide speed limit was 30 miles per hour. Panel A shows all signed streets in blue, where limits were reduced to 20 or 25 miles per hour depending on signage, and brown polygons indicating Slow Neighborhood Zones (SNZs) following the same reduced pattern. Panel B presents the general rule for New York City, with most streets limited to 25 miles per hour, while Panel C highlights exceptions, major arterials and limited access highways with higher limits. *Source:* Vision Zero View.

Figure A.2: Total budget for the Vision Zero Program in New York City



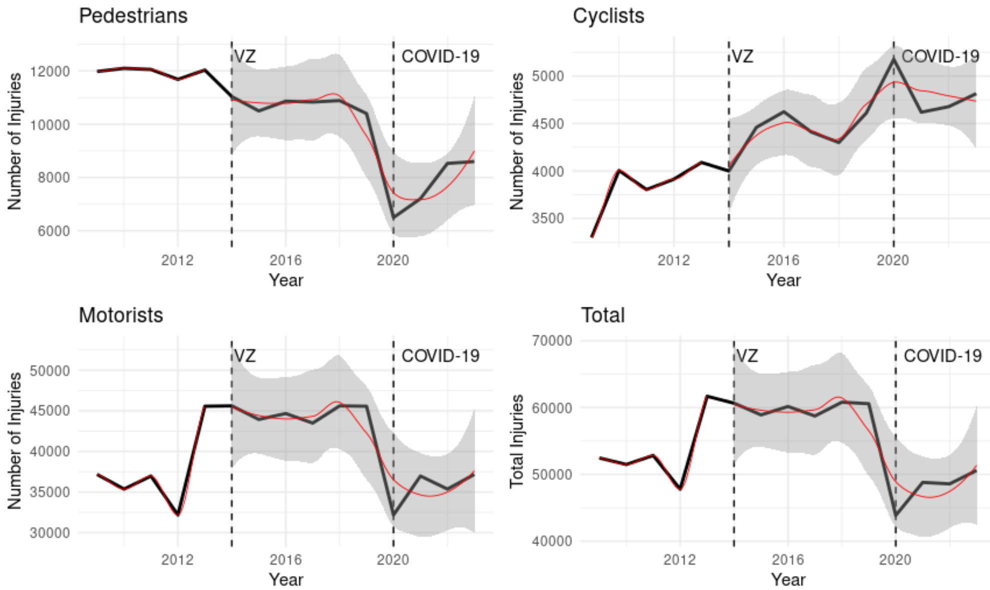
(a) VZ Budget 2014-2024



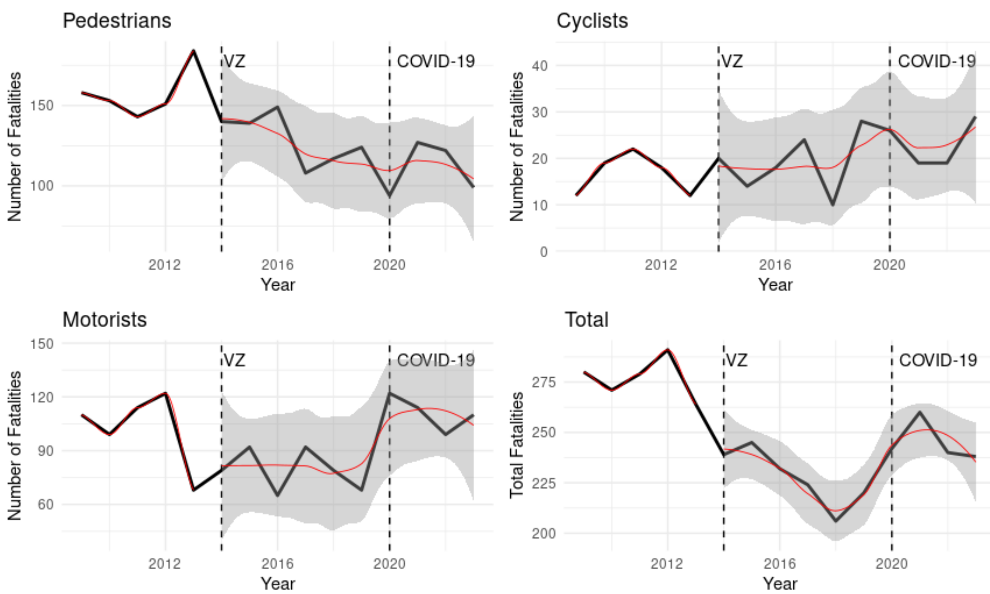
(b) VZ Budget 2024-2029

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.3: Comparison of injuries and fatalities before and after the implementation of the VZ



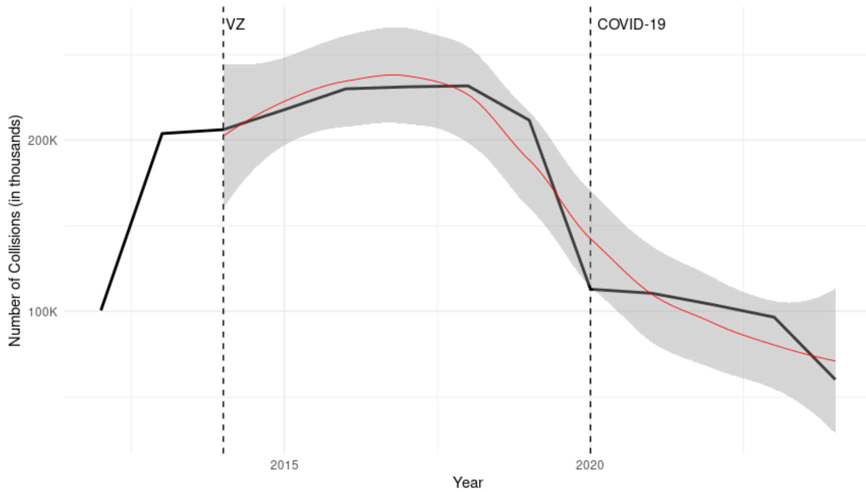
(a) Trends of injuries.



(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 collision 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 collisions Database and Fatality Analysis Reporting System (FARS).

Figure A.4: Total number of mother vehicle collisions before and after the implementation of VZ



Note: This figure illustrates the trend in motor vehicle collisions in New York City from 2009 to 2022. The x-axis represents the years, while the y-axis shows the total number of reported collisions. 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 collisions Database and Fatality Analysis Reporting System (FARS).

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

By type of cost		**By type of collision**	
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).