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URBAN SYMPOSIUM PAPER

Vision Zero: Speed Limit Reduction and Traffic Injury Prevention in New York City

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Abstract
We examine the effect on the incidence of casualties and crashes of a city-wide vehicle speed limit reduction in New York City (NYC) streets. The law change, part of Mayor Bill de Blasio's Vision Zero Action Plan to improve traffic safety, cuts the default speed limit for streets with no speed limit signs from 30 to 25 mph beginning November 7, 2014. We use a monthly panel dataset with crash statistics for the entire population of NYC streets, from July 2012 through March 2019. Several difference-in-differences regressions show a statistically significant and meaningful decline in injuries and crashes.

Keywords Traffic · Safety · Traffic laws

JEL Classification R410 · R480

Introduction

The traffic safety concept known as Vision Zero originated in Sweden in 1996 and became law there in October 1997 (Larsson et al. 2010). The Vision Zero approach seeks to create a road transport system in which crashes never result in fatalities or serious injuries; while crashes resulting from human error cannot be eliminated, the traffic environment must be designed so that the mechanical force of any crash does not exceed the tolerance of the human body (Tingvall and Haworth 1999).[1]

[1] Tingvall and Haworth articulate that crashes with lesser (not long-term disabling) injuries or no injuries are beyond the scope of Vision Zero—reducing those types of crashes is not a goal of the approach. However, in practice, it is plausible that changes in speed made to reduce the severity of injuries from crashes may also reduce the total number of crashes and the number of lesser- or no-injury crashes.

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Vision Zero therefore rejects the traditional approach which placed responsibility for crashes almost entirely on road users, and reallocates it largely to traffic system designers (Tingvall and Haworth 1999), a term which refers broadly to all public and private entities which shape the traffic environment.[2] Sweden now has the world's safest roads, with a road fatality rate of 2.8 per 100,000 persons compared to 12.4 in the US (WHO 2018), and programs similar to Vision Zero, sometimes called Safe Systems approaches, have been implemented in a number of countries, states, and cities around the world (NTSB 2017).

New York City (NYC) has the oldest and most established Vision Zero program among the US cities (Vision Zero Network 2017), spearheaded by Mayor Bill de Blasio since 2014 (McGeehan 2014).[3] The city program naturally differs from the national-level Swedish program due to differences in culture and administrative powers (Flegenheimer 2014), but Vision Zero NYC has reduced annual traffic fatalities from the 2010–2013 average of 321 to 214 in 2017, the lowest number since 1910 when record-keeping began (NYC-MOO 2018, p. 13).

The current investigation evaluates the impact of one of the most sweeping policy changes of Vision Zero NYC, the reduction in the speed limit on unsigned streets from 30 miles per hour to 25 miles per hour (mph) on November 7, 2014 (NYC-MOO 2015, p. 30). Although more than 30 US cities are considered Vision Zero Cities (Vision Zero Network 2019),[4] there has been little study of the effects of the speed limit reduction component of the Vision Zero program in the USA. Our results suggest that the policy has been broadly effective in increasing road safety. We find a 38.7% decline in casualties and a 35.8% reduction in crashes on treated streets relative to the remaining population of streets in New York City. The decline in crashes occurred in the 2 years after the law change, and there were no further effects subsequently. Although we cannot measure traffic speed directly, the results are consistent with drivers reducing their speeds on treated streets throughout the five boroughs of NYC as a result of the speed limit reduction. The decline in casualties was the greatest in the Bronx and Queens, the two most densely populated boroughs after Manhattan, and statistically insignificant in the other three boroughs. In Manhattan, the already low travel speeds due to traffic congestion likely muted the effects

[2] As described in Belin (2016), "'System designer' is a diffuse concept but it refers to any of the public and private agencies responsible for the design and operation of various parts of a transportation system, including roads, vehicles, and public transit services, and those responsible for any support systems, such as laws and regulations, education and public awareness, surveillance, rescue, and care and rehabilitation. State and municipal road-maintenance authorities, vehicle manufacturers, driver education programs and schools, private transportation companies, and healthcare providers are among the other stakeholders included in the definition of system designer."

[3] The previous NYC mayor, Michael Bloomberg had implemented programs and street design changes to increase safety and improve conditions for pedestrians and bicyclists (NYC-DOT 2013).

[4] The Vision Zero Network is a national nonprofit advocating for Vision Zero programs. Its criteria for designating cities as Vision Zero are: "(1) sets clear goal of eliminating traffic fatalities and severe injuries, (2) Mayor has publicly, officially committed to Vision Zero, (3) Vision Zero plan or strategy is in place, or Mayor has committed to doing so in clear time frame, and (4) key city departments (including Police, Transportation, and Public Health) are engaged." (Vision Zero Network 2019).

of the policy, while in the lower density boroughs, Brooklyn and Staten Island, there may have been less scope for reduced speeds to improve traffic safety. The finding of increased traffic safety is especially important given that reducing speed limits is one of the lower-cost policy options available.

Literature Review

The success of Vision Zero Sweden and a general reassessment of road safety management by policymakers and road safety management professionals in the past 30 years have led to the implementation of safe systems approaches in countries, states, and cities across the world (e.g., NTSB 2017, p. 29).[5] For the most part, crash fatalities have been reduced in these jurisdictions, and Vision Zero is widely considered to be the cutting edge in increasing road safety (e.g., Hauer 2010; OECD 2008). For entities continuing with the traditional cost–benefit analysis approach, the choice appears to be a function of political will and the lingering professional practice of weighting mobility more highly than safety (Hauer 2010; Noland 2013; OECD 2008), rather than an argument that Vision Zero is ineffective.[6]

The study of the effectiveness of specific Vision Zero elements has not been systematic (cf. Wegman et al. 2015); but it is important to understand which policies are most effective in order to allocate resources efficiently (Montag 2014, p. 539). Therefore, the current investigation considers the effect of Vision Zero NYC's speed limit reduction. This inquiry is important because vehicle speeds are widely recognized as a crucial factor in traffic safety, and because changing speed limits is one of the lower-cost policy options available (Archer et al. 2008, p. 11). The existing literature has shown that reducing speed limits decreases average operating speeds (e.g., NTSB 2017, p. 27), although the reduction in speed is typically smaller than the reduction in the speed limit (e.g., OECD 2006, p. 100), especially if no other speed counter measures are taken (Finch et al. 1994). Broadly, this implies at least some portions of drivers are influenced by the statutory speed limit. In turn, reduced vehicle speeds decrease the incidence and severity of crashes (e.g., Elvik 2005; NTSB 2017, p. 4).

[5] The program broadly referred to as Toward Zero Deaths (TZD) has been promulgated by 30 US states since 2001. Munnich et al. (2012) find that the four longest-standing programs, in Idaho, Minnesota, Utah and Washington, have been effective in reducing traffic fatalities. They note, "Successful TZD programs have five characteristics: (1) an ambitious goal of eliminating traffic fatalities and serious injuries; (2) high levels of inter-agency cooperation in pursuit of the TZD goal among state departments of transportation, public safety, health, and other relevant agencies; (3) a comprehensive strategy addressing all 4 E's—engineering, enforcement, education, and EMS (emergency medical services) elements of traffic safety; (4) a performance-based, data-driven system of targeting resources and strategies where they will have the greatest impact in reducing traffic fatalities; and (5) policy leadership from relevant entities, including the Governor, the state legislature, and the heads of state agencies."

[6] As one would expect, in low- and middle-income countries, weak legislation and enforcement, and lack of resources are often major impediments to improvements in traffic safety (Peden et al. 2004).

Most studies of speed limit changes in the USA focus on the decrease and increase in maximum highway speed limits of the 1970s and 1990s (e.g., Fowles and Loeb 1989). Relatively few academic studies have examined the effects of speed limit reduction in lower-speed roads (Goodwin et al. 2015, p. 3–14), and these are primarily outside of the US. The studies most similar to the current investigation evaluated default speed limit reductions from 37 to 31 mph in built-up (urban) areas of four Australian states implemented from 1999 to 2003.[7] "Default speed limit" denotes the statutory or legislated speed limit on roads with no posted speed limits (Donnell et al. 2009, p. 4), or in other words, it is the speed limit on unsigned roads, so few or no signage changes are necessary. Using various methodologies, casualty crash measures, and where available, vehicle speed measures, the results show broadly that casualty crashes and speeds declined with the treatment.[8] Three further pertinent studies examined speed limit reductions implemented (in contrast to New York City's) by changing the speed limit signs on sections of roadways at a small number of selected sites in the localities: Bristol, England (Bornioli et al. 2018; 30–20 mph), Edmonton, Canada (Islam et al. 2014; 30–25 mph), and Columbia and Springfield, Missouri (Rossy et al. 2012; 30–25 mph). These studies did not have casualty crash data, but found that average vehicle speeds declined significantly.[9]

The current study contributes to the literature by investigating the first US city-wide reduction in the speed limit of an entire class of roadways (the streets with no posted speed limits). Our dataset comprises the entire population of NYC streets within a long interval before and after the speed limit reduction. We are able to examine many measures of traffic safety including injuries, fatalities, number of crashes, and number of vehicles in crashes that were speeding; and we can disaggregate casualties for motorists, pedestrians, and bicyclists.

[7] The studies are: Hoareau and Newstead (2004) (Western Australia—state-wide—December 2001); Hoareau et al. (2006) (Victoria—state-wide—January 2001); Hoareau et al. (2002) (South East Queensland—March 1999); Kloeden et al. (2006) (Southwestern Australia—March 2003).

[8] A speed limit reduction from 37 to 31 mph in urban areas of New South Wales differed from this study, and the other Australian studies in that participation by local governments was voluntary and reduced speed limits were denoted with extensive signage; crashes and speeds were reduced (New South Wales Road Traffic Authority (NSW RTA) 2000, as cited in Hoareau et al. 2006).

[9] Three additional academic studies are similar to the current investigation in examining the effect on casualty crashes of speed limit changes in urban areas. In Oslo, Norway, Elvik (2013) found a temporary reduction from 50 to 37.5 mph on arterial roads reduced casualty crashes by 25–35%. In Hong Kong, Wong et al. (2005) found speed limit *increases* of 31–43 mph and 44–50 mph increased casualty crashes by 1–36%. In London, England, Grundy et al. (2009) found a 42% reduction in casualty crashes where 20 mph zones were implemented. These studies differ from ours in being implemented at selected sites with signage changes. In addition, the first two studies considered urban highways (NYC's highways were not affected by the NYC speed limit reduction), and the London study considered "zones" where speed limit reduction is accompanied by substantial traffic-calming engineering interventions.

Table 1 Variable definitions. *Source*: Motor Vehicles Collision Data, NYPD

Variable	Definition
Injuries	Sum for a given street–month–year of injuries from traffic crashes subdivided into motorists (drivers or passengers in cars or motorcycles), pedestrians, and bicyclists
Fatalities	Sum for a given street–month–year of fatalities from traffic crashes to motorists, pedestrians, and bicyclists
Crashes	Sum for a given street–month–year of traffic crashes, with or without resulting casualties
Speed attributions	Sum for a given street–month–year of vehicles for which police officers on the scene declared that "speed" was the cause of the vehicle's involvement in a crash
Treatment group	= 1 if street is unsigned
Treatment period	= 1 for dates after November 7, 2014

Data Page: https://data.cityofnewyork.us/Public-Safety/NYPD-Motor-Vehicle-Collisions/h9gi-nx95

Data dictionary: https://data.cityofnewyork.us/api/views/h9gi-nx95/files/b5fd8e71-ca48-4e96-bf63-1b8a7c4cc47b?download=true&filename=Collision_DataDictionary.xlsx

Data and Methodology

Table 1 gives variable definitions for our data. The NYC Police Department (NYPD) provides monthly traffic crash statistics, including injuries, fatalities, casualties (injuries plus fatalities), number of crashes, and casualties disaggregated by victim type—motorists, pedestrians, and bicyclists.[10]

The treatment we investigate is the reduction in New York City's default speed limit from 30 to 25 mph on November 7, 2014 as part of the Vision Zero Action Plan (NYC 2014).[11] All streets in New York City are now governed by this 25 mph speed limit unless otherwise signed. Our main independent variable of interest is the difference-in-differences coefficient on the interaction term of an indicator for observations dated later than November 7, 2014 (the treatment period) and an indicator for unsigned streets (the treatment group). The Vision Zero NYC speed limit reduction was accompanied by publicity to inform drivers of the law change, which seems a necessity to change driver behavior, since there were no speed limit signs on the

[10] The NYPD Motor Vehicles Collision Data compiles information from Police Accident Reports (form MV104-AN) filled out by an officer at the crash scene. Officers are required to report on all crashes where fatalities or injuries of any level of severity occur. If a crash victim has suffered multiple injuries, only the most severe injury is listed in the report. Officers assess injuries either by observation (for example, if the victim is unconscious) or from information reported by the crash victim (for example, if there is no visible injury but the victim reports pain or nausea). (NYS-DMV no date, pages 1 and 26). Therefore, in this paper "casualty" refers to a fatality or any injury observed by, or reported by the victim to, the officer at the crash scene.

[11] Speed limits remained constant throughout the study period on the control streets. The control streets include larger streets, such as limited access highways or major arterial streets, with posted speed limits of 30 mph and above; and some smaller streets, such as those near schools, which are signed for speeds less than 25 mph. (NYC Vision Zero 25-MPH-faq 2014). The majority of NYC streets are unsigned (see Table 2); this is the treatment group. The statutory speed limit on these streets was 30 mph prior to November 7, 2014, and 25 mph thereafter.

affected streets to convey the changed speed limit.[12] However, it appears that the other education, enforcement, and engineering elements of the Vision Zero Action Plan (NYC 2014) were applied to both treated and control streets; for example, these interventions were largely targeted at historically high-crash locations designated Priority Corridors, Intersections, and Areas (NYC–MOO 2019, p. 23), not toward the streets with reduced speed limits. We argue therefore that the difference-in-differences coefficient captures the effect of the speed limit reduction.

Note that the crash data are recorded by location, while the speed limit data are recorded by street. We therefore allocate all crashes to the street within 150 feet of them. For crashes close to two or more streets, we allocate the crash at random to one of the streets within 150 feet. Figure 1 illustrates a hypothetical fatality crash at the circled intersection, for which the fatality would be attributed at random to one of the (bolded) adjacent streets.

Table 2 presents descriptive statistics for the treatment streets (no posted speed limit) and all remaining streets (speed limit posted) before and after the law change. The combined total of signed and unsigned streets represents the total population of NYC streets. These summary statistics show broadly that streets without signs appear safer by many measures. A priori, we attribute the lower level of collisions to both the lower speeds and other fixed features of the road (smaller sizes, less crowded neighborhoods). Later, we will control for fixed effects with the appropriate standard methodology. Despite having fewer crashes per street, these unsigned roads occupy the vast majority of the city and therefore the majority of collisions occur on them.

In addition to measures of traffic safety (casualties, injured, fatalities, count of unique crashes), Table 2 disaggregates the data by type of victim. Motorists are the most frequent casualties of a crash, then pedestrians and lastly cyclists, which seems to match the time and number of each of these groups' time on the road. The last variable, speed attributions, is not a crash of any type, but rather the count of times officers on the scene declared that "speed" was the cause of a vehicle's involvement in a crash. So, for example, in a three-car crash, two could have "speed" attributed as the contributing factor and the third could be "brake failure."

Figure 2a illustrates that most NYC streets are unsigned. The blocks of black-lined signed streets represent densely settled neighborhoods with relatively many signed streets. Figure 2b shows the (red/yellow) concentration of crashes in central Manhattan, though there are a few scattered hotspots of accidents in Brooklyn, the Bronx, and Queens. The accidents generally follow major roadways and seem to match typical traffic patterns.

Figure 3 shows the counts over time of casualties for the treated and untreated groups before and after the speed limit was reduced on treated streets. The treated and untreated streets, when fit with splines before and after the law change, exhibit

[12] Polling results in the 1-year report indicate that in October 2014 prior to the law change, 30% of New York drivers accurately identified the default 30 mph speed limit. The city conducted "25 MPH Outreach" in the fall of 2014, with street-team in-person distribution and mailings of publicity materials, social media postings and radio ads. In December 2014, 62% of New York drivers correctly identified the new 25 mph speed limit (NYC-MOO 2015, p. 46). Campaigns are ongoing regarding the 25 mph speed limit and its safety benefits (NYC-MOO 2019, p. 18, 68).

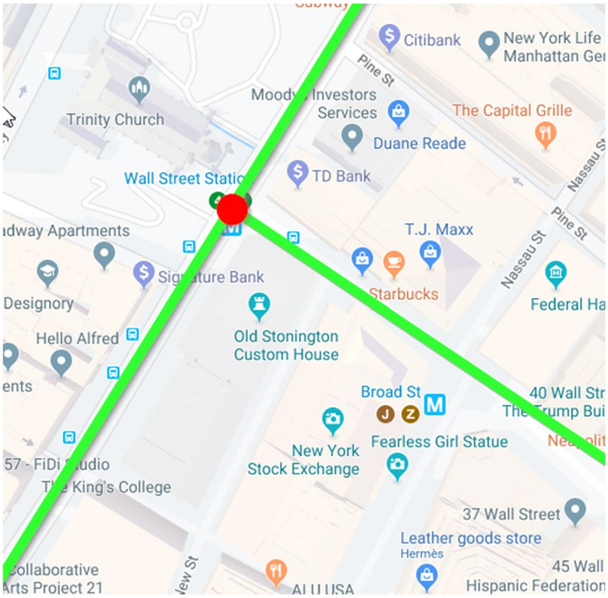
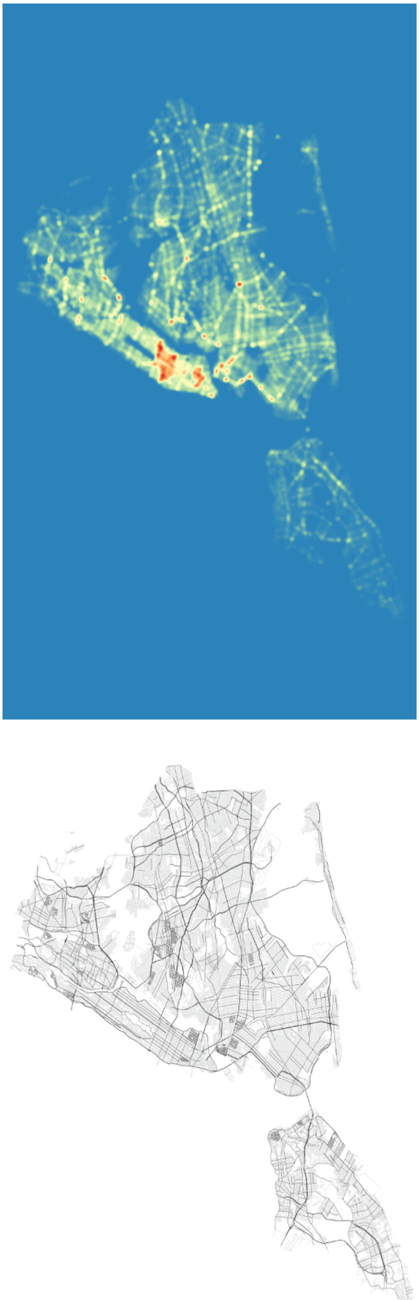


Fig. 1 Attributing casualties to street segments

Table 2 Summary statistics: means by street–month–year

	(1) Treatment streets—no posted speed limit $n=7120$	(2) Control streets—posted speed limit $n=1080$	(3) All streets $n=8200$
Casualties	0.45	1.70	0.65
Injuries	0.45	1.70	0.64
Fatalities	0.00	0.01	0.00
Crashes	1.81	6.13	2.48
Motorist casualties	0.31	1.30	0.46
Pedestrian casualties	0.10	0.29	0.13
Cyclist casualties	0.04	0.11	0.05
Speed attributions	0.01	0.05	0.02
Observations (street–month–year)	439,318	81,297	520,615

parallel casualty trends in both the pre-treatment and post-treatment periods. The treated streets show a modest relative decline in casualties at the treatment date, though this is not a clear-cut change. This illustrates the need for investigation into the policy to see if it is actually associated with overall increased street safety. We next use a



(a) **Signed streets in black; unsigned streets in grey**

(b) **Heat map of NYC crashes**

Fig. 2 NYC streets and crashes. (Color figure online) difference-in-differences methodology to illustrate this point and show that the decline is statistically significant after fixed effects are included. The specification is:

$$Casualties_{it} = \alpha_i + \delta_t + \beta_1 TreatmentPd_t + \beta_2 TreatmentPd * TreatedSt_i + \epsilon_{it}$$

Here, α_i represents a fixed effect for each street (which includes if the street is eventually treated or not) and δ_t, a monthly fixed effect for every month–year of the observation window. The indicator variable $TreatmentPd_t$ takes the value zero before November 2014 and the value one afterward, representing any change that may have happened to streets city-wide once the treatment period began in November of 2014. The coefficient β_2 is the difference-in-differences coefficient representing the change in the treated streets during the treatment period relative to the change in the untreated streets.

Results

Overall, we find the speed limit reduction is associated with improved safety on the treated streets relative to the untreated streets. Table 3 reports the results of several linear difference-in-differences estimations, for casualties (injuries plus fatalities), injuries, and fatalities, respectively, and the count of unique crashes.[13] We use street-level and month–year fixed effects for all estimates.

Column 1 supports the result in Fig. 2. Lower-speed limits are associated with approximately 0.17 fewer casualties per month per treated street relative to the controls, a reduction of 38.7%.[14] Column 2 shows the reduction in casualties is almost entirely composed of a decline in injuries, while Column 3 shows there is no measurable change in fatalities along the treated streets. Column 4 shows the relative count of crashes has declined by about 0.65 crashes per treated street per month, a reduction of 35.8%,[15] suggesting about 27% of these reduced crashes would have had associated injuries, though it is likely many non-injurious crashes go unreported.[16] In Table 4, we decompose crashes by type of victim and the attributed cause of the crash.

Column 1 shows a large and significant decline in injuries and fatalities for motorists, suggesting that the decline in speed limit was associated with a decline of nearly 0.19 motorist casualties per month per treated street. This decline is negated somewhat by the measured, but very small, increase in pedestrian casualties shown in Column 2 of roughly two casualties per every hundred streets. Column 3 considers casualties of cyclists, which have a weakly significant decline of roughly

[13] We perform the same exercises for Fixed-Effect Negative Binomial estimation, and a pooled Zero-Inflated Poisson estimation (since one cannot use fixed effects in ZIP without manually enforcing them), see Weber (2014) for more details. The results remain similar in significance and direction.

[14] This 38.7% is calculated by: (−0.174/0.45)*100, where 0.45 is the average number of casualties on treated streets (Table 2, row 1, column 1).

[15] This 35.8% is calculated by: (−0.648/1.81)*100, where 1.8 is the average number of crashes (Table 2, row 4. column 1).

[16] This 27% is calculated by: (−0.174/−0.648)*100.

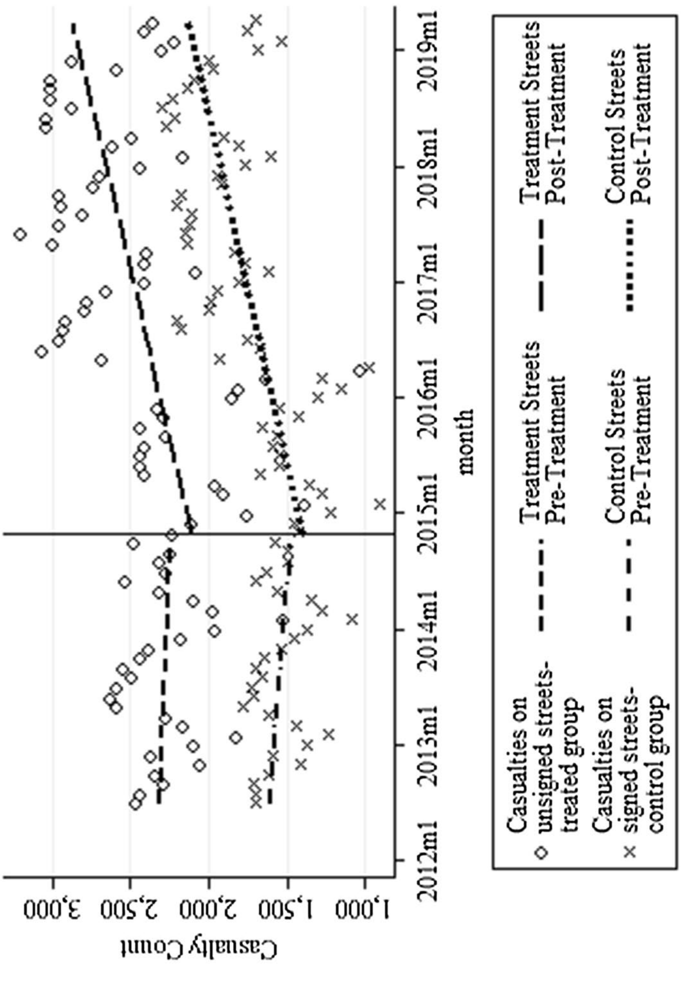


Fig. 3 Count of casualties pre- and post-treatment

Table 3 Road safety outcomes: difference in differences results

Variables	(1) Casualties	(2) Injuries	(3) Fatalities	(4) Crashes
After November 2014	0.105	0.108*	−0.002	0.538***
	(0.065)	(0.065)	(0.003)	(0.162)
D-I-D	−0.174***	−0.174***	0.000	−0.648***
	(0.045)	(0.045)	(0.001)	(0.132)
Constant	0.785***	0.780***	0.005**	2.543***
	(0.028)	(0.028)	(0.002)	(0.045)
Street FE	Yes	Yes	Yes	Yes
Monthly FE	Yes	Yes	Yes	Yes
Observations	520,615	520,615	520,615	520,615
R-squared	0.008	0.008	0.000	0.023
F test	12.60	12.54	1.335	23.40
Prob > *F*	0	0	0.0243	0
Number of streets	8200	8200	8200	8200

Robust standard errors in parentheses

*** $p<0.01$; ** $p<0.05$; * $p<0.1$

Table 4 Heterogeneity in difference-in-differences results

Variables	(1) Motorist casualties	(2) Pedestrian casualties	(3) Cyclist casualties	(4) Speed attributions
After November 2014	0.187***	−0.058***	−0.023**	0.104***
	(0.062)	(0.015)	(0.010)	(0.011)
D-I-D	−0.191***	0.023***	−0.007*	−0.054***
	(0.044)	(0.007)	(0.003)	(0.008)
Constant	0.566***	0.131***	0.088***	−0.010***
	(0.025)	(0.010)	(0.007)	(0.002)
Street FE	Yes	Yes	Yes	Yes
Monthly FE	Yes	Yes	Yes	Yes
Observations	520,615	520,615	520,615	520,615
R-squared	0.007	0.004	0.008	0.018
F test	11.01	9.623	11.04	18.29
Prob > *F*	0	0	0	0
Number of streets	8200	8200	8200	8200

Robust standard errors in parentheses

*** $p<0.01$; ** $p<0.05$; * $p<0.1$

zero. When summed these three columns encompass all possible injuries to return to the original estimate in Table 3, Column 1. The dependent variable in Column 4 is speed attributions. Note that around the time of the policy change, there is a large increase in speed attributions, suggesting that officers became more aware of speed as a contributing factor; this is likely a result of Vision Zero's refocusing of police initiatives (NYC-MOO 2018, p. 34). In spite of the increase in the count of speed attributions, we observe a significant relative reduction of about 0.05 of these attributions per treated street per month, suggesting that speed is less likely to be a contributing factor for crashes on treated streets. Such a decline would be exactly what one would expect to see if traffic on treated streets were indeed slower after the speed limit change.

In Table 5, we consider several timing effects (leads) as robustness checks. In each estimation, we include 6 months of leads prior to the actual speed limit change in 2014.[17] These leads are intended to capture any irregularities prior to the introduction of the treatment. We also implicitly capture any potential announcement effects from the passing of the Vision Zero speed limit change law, which happened the month prior. Reassuringly, the estimated coefficient on our D-I-D is essentially unchanged.[18]

In Column 1, the use of 6 monthly leads leaves the estimated D-I-D coefficient largely unchanged, increasing the measured association between the speed limit change and overall casualties slightly to a significant −0.175 casualties per street. In Column 2, injuries remain the overwhelming contributors of casualties; there are roughly −0.172 fewer injuries in treated streets per month. In Column 3, although the measured association between fatalities and the speed limit change reverses sign to show a decrease after the speed limit change, the association remains broadly insignificant and essentially zero. Column 4 shows the change in overall crash counts; it remains negative, large, and significant at −0.365, or about 1/3 fewer crashes in the treated streets per month after the treatment. Column 5 shows that the reduction in speed attributions on treated streets remains roughly constant and significant at −0.05. Overall, the leads were broadly insignificant, with only two of the individual D-I-D leads obtaining any level of significance (in column 4); and no particular pattern is visible (for example, the significant coefficients are of opposite sign). Jointly, the leads of the D-I-D for change in count of crashes (column 4) were significant at a 1% level, and the leads for change in speed attributions (column 5) were significant at a 10% level, suggesting that there may have been some changes in reporting crashes or changes in how crashes may be attributed to speed.

We also examine potential delayed effects of the treatment, e.g., if it takes time for individuals to adjust to the policy. In Table 6, we use three one-year lags of the D-I-D and treatment period [excluding the first year of treatment (Islam et al. 2004)], in order to check if the impact of the policy was spread over several time periods. We find that the decline in crashes seems to be fully complete by about 2 years after

[17] We omit the decomposition into different types of victims here for brevity, since we show the aggregated totals, but these estimates with leads are available upon request.

[18] In addition to implicitly capturing the announcement effect in the leads, we tried to isolate it exclusively. Creating a separate difference-in-differences for the announcement on October 2014 in the eight estimations from Tables 3 and 4 does not meaningfully change the coefficients on our primary D-I-D, the speed limit change. Furthermore, in the eight specifications tested, the D-I-D for the announcement effect was insignificant in all specifications except for the count of crashes, where it was significantly negative. These results are available upon request.

Table 5 Robustness checks for pre-treatment effects

Variables	(1) Casualties	(2) Injuries	(3) Fatalities	(4) Crashes	(5) Speed attributions
After November 2014	−0.038	−0.042	0.004	−0.479***	0.110***
	(0.104)	(0.103)	(0.005)	(0.178)	(0.011)
D-I-D	−0.175***	−0.172***	−0.002	−0.365**	−0.049***
	−0.038	−0.042	0.004	(0.142)	(0.008)
D-I-D 1-month lead	−0.031	−0.036	0.005	−0.167	−0.001
	(0.078)	(0.078)	(0.004)	(0.105)	(0.004)
D-I-D 2-month lead	−0.015	−0.013	−0.002	−0.092	0.002
	(0.084)	(0.083)	(0.005)	(0.111)	(0.004)
D-I-D 3-month lead	0.145	0.144	0.001	0.003	−0.001
	(0.090)	(0.090)	(0.004)	(0.101)	(0.004)
D-I-D 4-month lead	0.015	0.016	−0.001	0.261**	0.003
	(0.094)	(0.093)	(0.004)	(0.110)	(0.005)
D-I-D 5-month lead	−0.093	−0.096	0.003	−0.051	−0.007
	(0.099)	(0.098)	(0.005)	(0.104)	(0.005)
D-I-D 6-month lead	−0.026	−0.023	−0.003	−0.274**	−0.002
	(0.075)	(0.076)	(0.0036)	(0.108)	(0.003)
Constant	0.788***	0.784***	0.004**	2.553***	−0.008***
	(0.029)	(0.029)	(0.002)	(0.044)	(0.002)
Street FE	Yes	Yes	Yes	Yes	Yes
Monthly FE	Yes	Yes	Yes	Yes	Yes
Observations	520,445	520,445	520,445	520,445	520,445
R-squared	0.008	0.008	0.000	0.023	0.017
F test	11.36	11.29	1.195	11.36	16.12
Prob > *F*	0	0	0.098	0	0
Number of streets	8189	8189	8189	8189	8189

Robust standard errors in parentheses

*** $p<0.01$; ** $p<0.05$; * $p<0.1$

the treatment, and by the third year most of the coefficients have settled to zero. There may be a small but significant increase in the count of fatalities or crashes on these unsigned streets roughly 3 years after implementation, but these coefficients are at least canceled out by the significant reductions observed in the prior year.

We next decompose our primary results by borough, dividing the streets among the districts where necessary. Some collisions were not contained within a borough's land boundaries, instead they were on bridges, for example, and these observations were discarded. In Table 7, we see that the reduction in speed attributions is relatively uniform across all the boroughs regardless of density, and strongly significant. Officers appear to be less likely to claim speed was a factor in collisions on treated streets across all boroughs after the law was changed.

Table 6 Robustness checks for delayed effect of treatment

Variables	(1) Casualties	(2) Injuries	(3) Fatalities	(4) Crashes	(5) Speed attributions
D-I-D (1-year lag)	−0.085**	−0.085**	0.000	−0.218**	−0.045***
	(0.034)	(0.034)	(0.001)	(0.095)	(0.007)
D-I-D (2-year lag)	−0.284***	−0.281***	−0.003**	−0.868***	−0.033***
	(0.048)	(0.047)	(0.001)	(0.130)	(0.007)
D-I-D (3-year lag)	0.020	0.016	0.004**	0.179***	0.005
	(0.030)	(0.030)	(0.001)	(0.055)	(0.005)
Constant	0.788***	0.784***	0.004**	2.553***	−0.008***
	(0.029)	(0.029)	(0.002)	(0.044)	(0.002)
Street FE	Yes	Yes	Yes	Yes	Yes
Monthly FE	Yes	Yes	Yes	Yes	Yes
Observations	520,445	520,445	520,445	520,445	520,445
R-squared	0.010	0.010	0.000	0.026	0.020
F test	13.47	13.40	1.316	23.82	17.50
Prob > F	0	0	0.0271	0	0
Number of streets	8189	8189	8189	8189	8189

Robust standard errors in parentheses

*** $p<0.01$; ** $p<0.05$; * $p<0.1$

Given this relatively uniform change in speed attributions by officers, one might anticipate a similarly uniform reduction in casualties, which we decompose by borough in Table 8. We find the reduction in casualties is not present in Manhattan, but takes effect almost entirely in the other two most densely populated boroughs. Possibly, the policy change has limited scope for effect in Manhattan because congestion often keeps travel speeds lower than the speed limit.[19] The reductions in the other two densest districts, the Bronx and Queens, are strongly significant. This is in contrast to the uniform reduction in speed attributions across the boroughs delivered by police officers.

Discussion

The main results in Table 3 are consistent with a decline in casualties and crashes caused by the speed limit reduction. Here, we briefly discuss possible mechanisms behind this outcome. The decline, on treated streets, of crashes caused by speeding (Table 4 column 4), suggests that average operating speeds decreased on treated streets. Although our study cannot measure vehicle speeds directly, our results concur with the widely found evidence in the literature that a reduction in the speed limit reduces average traffic speed (e.g., Elvik et al. 2004, p. 93).[20] In particular, of

19 NYC-DOT (2019) discusses city-wide congestion and low travel speeds, particularly in Manhattan.

20 Earlier studies of highway traffic sometimes found little effect of speed limit reductions on mean travel speeds (e.g., Parker 1997).

Table 7 Crash-causing speed attributions by borough

Variables	(1) Brooklyn	(2) Bronx	(3) Manhattan	(4) Queens	(5) Staten Island
After November 2014	0.100***	0.108***	0.073***	0.113***	0.074***
	(0.018)	(0.026)	(0.017)	(0.020)	(0.019)
D-I-D	−0.052***	−0.069***	−0.046***	−0.058***	−0.028***
	(0.014)	(0.019)	(0.016)	(0.017)	(0.006)
Constant	−0.007**	−0.010**	0.001	−0.010***	−0.003
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
Street FE	Yes	Yes	Yes	Yes	Yes
Monthly FE	Yes	Yes	Yes	Yes	Yes
Observations	126,385	97,942	69,341	163,609	129,793
R-squared	0.022	0.022	0.015	0.018	0.009
F test	7.162	5.262	3.994	7.920	3.859
Prob > F	0	0	0	0	0
Number of streets	1791	1416	910	2438	2744

Robust standard errors in parentheses

*** $p<0.01$; ** $p<0.05$; * $p<0.1$

Table 8 Casualties by borough

Variables	(1) Bronx	(2) Brooklyn	(3) Manhattan	(4) Queens	(5) Staten Island
After November 2014	0.424**	−0.059	−0.337***	0.259***	0.150
	(0.213)	(0.123)	(0.117)	(0.076)	(0.093)
D-I-D	−0.442***	−0.032	0.117	−0.241***	−0.029
	(0.169)	(0.068)	(0.076)	(0.069)	(0.024)
Constant	0.618***	1.064***	0.986***	0.674***	0.299***
	(0.065)	(0.057)	(0.059)	(0.050)	(0.044)
Street FE	Yes	Yes	Yes	Yes	Yes
Monthly FE	Yes	Yes	Yes	Yes	Yes
Observations	97,942	126,385	69,341	163,609	129,793
R-squared	0.013	0.011	0.009	0.010	0.006
F test	4.133	5.483	3.218	5.390	4.875
Prob > F	0	0	0	0	0
Number of streets	1416	1791	910	2438	2744

Robust standard errors in parentheses

*** $p<0.01$; ** $p<0.05$; * $p<0.1$

the studies cited earlier of similar speed reductions on urban streets, those which had measured speed available as an outcome all found operating speed reductions. So we believe it is reasonable to infer that vehicle speed declined after the treatment.

What caused the slowdown? The question of whether and under what circumstances drivers comply with limits looms large in the literature from both academics and practitioners (e.g., Elvik 2016; NTSB 2017, p. 17). Although drivers weigh a number of factors when choosing operating speeds (e.g., Foster 2010; Nagler 2013; Warner and Åberg 2008), it is widely argued that speed limits do cause at least some portion of drivers to reduce speeds (e.g., Archer et al. 2008, p. 2), along with awareness campaigns, enforcement, and traffic-calming design measures (TRB 1998, Ch. 4 and 5). The latter three interventions (or combinations thereof) are considered important mediators on the effect of speed limits on operating speed, but measuring and quantifying their effects remains a topic of debate (e.g., Elvik et al. 2004, p. 76; Luca 2015, TRB 1998, p. 133). These types of interventions were part of the package of Vision Zero NYC safety initiatives, but they were not limited to the treated streets. We argue therefore that the difference-in-differences coefficient captures the effect of the speed limit reduction in the treated streets relative to untreated streets.

Conclusion

We evaluate one of the primary components of the Vision Zero NYC action plan, a speed limit reduction from 30 to 25 mph applied to the 86% of the population of NYC streets which had no posted speed limit. We examine many measures of traffic safety including injuries, fatalities, number of crashes, and number of crashes blamed on speeding; and we can disaggregate casualties for motorists, pedestrians, and bicyclists. The difference-in-differences results suggest that the speed limit reduction increased traffic safety in New York City. In particular, we find a 38.7% decline in casualties and a 35.8% reduction in crashes on treated streets relative to the remaining population of streets. The implication that speed limit reduction is an effective policy lever in the Vision Zero toolkit is important given that this is one of the lower-cost options available.

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