Braking Habits: The Impact of Leading Pedestrian Intervals*

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This version: September 15, 2026

Abstract:

After introducing traffic safety measures, motorists often adopt riskier driving habits that may offset the intended safety improvement. Exploiting a quasi-natural experiment in Toronto, I investigate how leading pedestrian intervals, advance walk signals that give pedestrians a head start before motorists at a traffic signal, impact pedestrian safety and influence motorist behaviour. I estimate that leading pedestrian intervals decrease the probability of a pedestrian collision at an intersection by 39%, with a 56% decrease in the likelihood of a pedestrian fatality or hospitalization. Rather than a behavioural offset, I determine that leading pedestrian intervals elicit a positive behavioural response from motorists, yielding greater pedestrian safety improvements over time. Although the safety improvements increase pedestrian welfare, motorists bear the costs, as leading pedestrian intervals reduce traffic speeds and marginally increase vehicle collision probability.

Keywords: Pedestrian Safety, Public Health, Traffic Fatalities, Behavioural Responses, Urban Planning

JEL Codes: I1, R41, R42, R48

*I am grateful for the guidance of my supervisor, Jonathan Hall, and for the helpful comments from Nathaniel Baum-Snow, Stephan Heblich, Hugo Cordeau, Rawnak Amin, Alison Cane, Noah Spencer, and many seminar and conference participants. All errors are my own. Declaration of Interest: None.

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1 Introduction

Despite the substantial investment in new road safety initiatives and urban infrastructure, traffic safety remains a worldwide public health issue. From 2000 to 2021, traffic-related fatalities were the 3rd leading cause of death for people under the age of 65 in the United States and the 4th in Canada. As the most vulnerable road users in traffic collisions, pedestrians lie at the forefront of this public health issue and are now the focus of many new traffic safety measures. However, motorists may change their driving behaviour after new safety measures are introduced and offset the intended traffic safety improvement (Peltzman, 1975). Given the risk of this behavioural offset, understanding how traffic policies influence motorist behaviour is imperative to improve pedestrian safety and resolve this public health issue.

In this paper, I investigate how one traffic safety measure, the leading pedestrian interval, impacts pedestrian safety and the behavioural response of motorists. Leading pedestrian intervals provide a 3–7 second head start for pedestrians to cross at an intersection before motorists are permitted to proceed. The advance walk signals are intended to improve pedestrian visibility to motorists and reinforce a pedestrian's right of way at an intersection. To determine how leading pedestrian intervals affect pedestrian safety, I exploit a quasi-natural experiment in Toronto, which rapidly installed leading pedestrian intervals across the city following the launch of the city's Vision Zero Road Safety Plan in 2016. Using the universe of reported traffic collisions in Toronto, I employ a staggered difference-in-differences design and estimate that leading pedestrian intervals decrease the pedestrian collision probability at an intersection by 39%, with a 56% decrease in the probability of a pedestrian collision that results in a pedestrian fatality or hospitalization. Furthermore, the corresponding event study shows that the pedestrian safety benefits become more prominent over time, highlighting a leading pedestrian interval's ability to help resolve this ongoing traffic safety issue. Using a back-of-the-envelope calculation, I estimate that leading pedestrian intervals prevented 104 pedestrian collisions in 2022, including 17 major injuries requiring hospitalization and 4 deaths: a $67.6 million increase in social welfare.

I next evaluate how leading pedestrian intervals influence a motorist's actions during a pedestrian collision to uncover the mechanisms that explain the decrease in collision probability and investigate whether a change in motorist behaviour poses any new collision risk. First, I determine that leading pedestrian intervals work as intended and improve pedestrian visibility to turning motorists, with a 70% decrease in pedestrian collision probability from improper turns. Furthermore, leading pedestrian intervals reinforce the pedestrian's right of way, as the probability of a pedestrian collision caused by a motorist failing to yield decreases by 44%. Although the decrease in collision probability from improper turns is proportionally greater, the pedestrian safety improvement from the motorist behavioural response explains 59% of the total decrease in pedestrian collision probability, as a motorist failing to yield is the most frequent cause of pedestrian collisions in Toronto. Rather than a behavioural offset, motorists improve their yielding behaviour at treated intersections.

Although leading pedestrian intervals significantly improve pedestrian safety, I next investigate whether motorists incur a social cost after leading pedestrian intervals are installed and estimate how leading pedestrian intervals impact vehicle speeds during rush hour. After leading pedestrian intervals are installed, I estimate that rush hour traffic speeds fall by 1.3% along road segments that intersect a leading pedestrian interval. While the decrease in vehicle speed offers another potential mechanism to explain the decrease in pedestrian collision probability, it also indicates that leading pedestrian intervals impose an implicit cost on motorists, who now face longer commutes. To quantify the social cost, I use microdata from the 2016 Canadian Census on commuter mode choice, commute time, and income, alongside the delay estimates, to calculate the total value of travel time lost to delay. If all Toronto vehicle commuters experience a 1.3% decrease in speed, I estimate that the total value of time lost from the delay would be $36.2 million. This cost estimate should be interpreted as an upper bound, as large portions of a worker's commute are along road segments without leading pedestrian intervals, such as freeways, where the speed decreases would not occur.

I next investigate whether the behavioural response results in unintended consequences for other road users and evaluate how leading pedestrian intervals affect cyclist and vehicle

collisions. After the city installs a leading pedestrian interval, I estimate that cyclist safety improves with a 33% decrease in cyclist collision probability while vehicle collision probability marginally increases, an additional cost to motorists. Using a similar back-of-the-envelope calculation, I estimate that the decrease in cyclist collision probability increases social welfare by $11.2 million, while the increase in vehicle collisions increases the total social costs by $15.2 million. Although these costs are substantial, the aggregate social welfare benefits of leading pedestrian intervals from improved pedestrian and cyclist safety far exceed the costs from prolonged commute times and the higher collision risk motorists incur.

Given that the analysis evaluates treatment at a highly localized geographic level, one potential identification concern is that motorists may change their commute patterns to avoid leading pedestrian intervals and affect the collision rates at nearby untreated intersections, violating the stable unit treatment value assumption. To investigate this concern, I evaluate how installing a leading pedestrian interval affects pedestrian collisions at untreated intersections within the same census tract. Under the new specification, I cannot reject the null hypothesis of no spillovers onto nearby intersections.

Finally, I evaluate how the treatment effect varies across intersection features, specifically, whether right turns on red are permitted, or if public transit stops are present. If leading pedestrian intervals are ineffective when certain features are present, motorists will incur the delay cost with no pedestrian safety benefit. When an intersection prohibits right turns on red, I estimate that the pedestrian safety improvement is marginally lower. Turn prohibitions likely mitigate the effects of a leading pedestrian interval because the safety measures share a common mechanism, as both measures protect pedestrians from turning motorists. I then compare the effects at intersections with and without public transit stops and determine that the safety improvements are notably larger at intersections with a public transit stop, which may reflect a larger pedestrian base at these intersections or greater opportunity for visibility improvements where buses and bus shelters obstruct a motorist's views of pedestrians.

I perform several robustness checks to validate the pedestrian safety improvements. First, I exclude all dates between March 2020 and April 2022 to mitigate any confounding factors

during the COVID-19 pandemic. While the point estimate slightly attenuates after I omit the work-from-home period, the treatment effects remain statistically similar. I next supplement the main analysis with mobile device GPS data to evaluate whether the decrease in pedestrian collision rates is robust after controlling for traffic and pedestrian flows. Although the estimated treatment effects after controlling for the number of mobile device pings at an intersection are consistent with the main results, the effects are not statistically significant. This lack of statistical significance reflects limited statistical power, as the mobile device GPS data are available for only 15% of the sample period and collisions are infrequent at a microgeographic scale. Finally, I test whether the increased pedestrian safety benefits over time are explained by selection into earlier treatment cohorts. However, an event study using a fixed treatment cohort also exhibits the downward trend over time, confirming that the safety improvements increase over time.

This paper expands the growing literature on traffic safety following Vickrey (1968) and primarily builds on the strand of the literature that emphasizes pedestrian safety (Tyndall, 2021; Nehiba and Tyndall, 2023; Tyndall, 2025). Within the traffic safety literature, I make two notable contributions. First, I provide the first causal estimates of the effects of leading pedestrian intervals on pedestrian collisions. Recent additions to the literature investigate how new road policies, such as speed cameras (Li et al., 2013; Li and Graham, 2016; Graham et al., 2019), speed limit adjustments (Van Benthem, 2015; Ang et al., 2020; Fridman et al., 2020), and speed limit enforcement (Bauernschuster and Rekers, 2022), affect traffic safety. However, no current analyses estimate the causal effect of leading pedestrian intervals. Several transportation engineering studies analyze video surveillance footage from a sample of intersections with leading pedestrian intervals and show that pedestrian collisions decrease in both frequency and severity after installation (Osama et al., 2015; Guo et al., 2020; Hasanpour and Persaud, 2022; Ali et al., 2023; Arun et al., 2023). While video surveillance analyses alleviate measurement error concerns in the data, the limited sample of intersections over a short time window limits the statistical power and causal identification. Other notable engineering studies in the literature that evaluate leading pedestrian intervals

face similar sample size and empirical limitations, which limit causal inference (Van Houten et al., 2000; King, 2000; Fayish and Gross, 2010). Although the current policy evaluations and before-and-after analyses show that a leading pedestrian interval lowers the frequency of pedestrian collisions, the limited number of treated and comparison intersections results in high variance in the estimates of safety improvements.[1] In this analysis, I provide the first citywide evaluation of leading pedestrian intervals and their effects on pedestrian safety, mitigating the sample size and selection concerns in the existing literature, and apply an empirical strategy to the universe of reported traffic collisions to provide the first causal effects of leading pedestrian intervals. Furthermore, this is the first study to investigate the dynamic treatment effects of leading pedestrian intervals and the mechanisms that explain the improvement in pedestrian safety.

Finally, I contribute to the literature on the unintended consequences of traffic policies, focusing here on the effects on motorists and motorist behaviour (Peltzman, 1975; Currie and Walker, 2011; Kapoor and Magesan, 2014; Hall and Madsen, 2022). Improving our understanding of unintended consequences and how traffic safety policies affect motorist behaviour is necessary to implement effective traffic policies and address the persistent public health issue. In a similar setting, Kapoor and Magesan (2014) determine that motorists in Toronto became more aggressive after the city installed countdown signals at intersections, anticipating the traffic signal changes. Although the positive behavioural response of motorists that I identify contrasts with the behavioural offset commonly found in the literature, both findings indicate that motorist behaviour is an essential factor to consider when implementing new traffic safety policies.

The remainder of the paper is organized as follows. Section 2 provides a brief overview of leading pedestrian intervals in Toronto's Vision Zero Road Safety Plan. Section 3 describes the data. Section 4 explains the empirical strategy and the identification assumptions for causal inference. Section 5 discusses the main results. Finally, Section 6 examines the social welfare effects of leading pedestrian intervals.

[1] Table A1 in the Appendix provides a summary of the notable papers in this literature and shows that the estimates in this paper are consistent with previous policy evaluations.

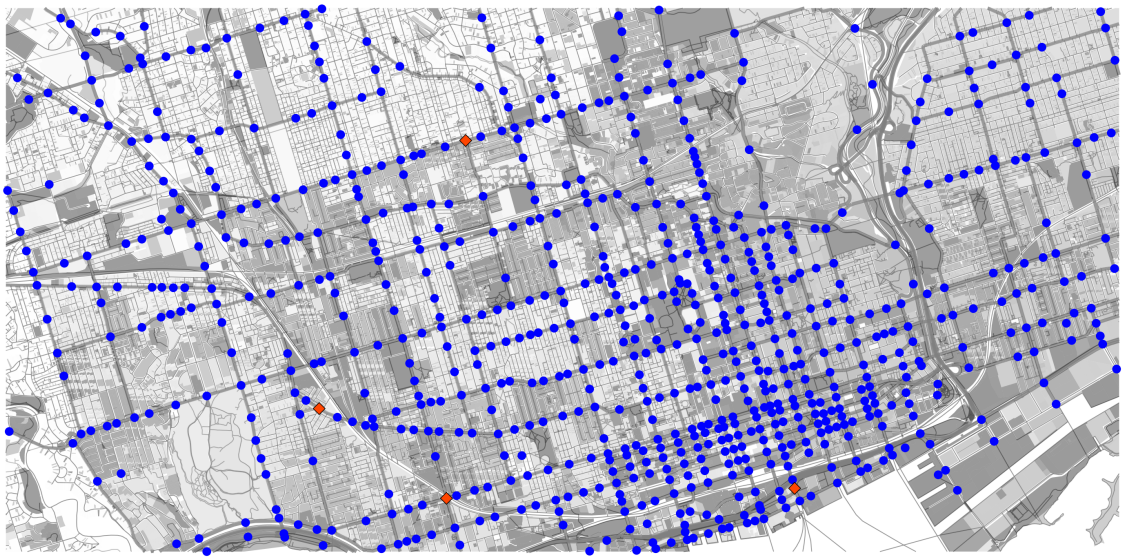
2 Background

In July 2016, the City of Toronto launched its Vision Zero Road Safety Plan to reduce traffic-related fatalities and serious injuries. The comprehensive action plan includes infrastructure investments, such as expanded bicycle lanes, elevated intersections, and automated speed cameras, as well as new road policies, including reduced speed limits and additional designated community safety zones. Although the plan aims to improve traffic safety for all road users, it places a particular emphasis on pedestrian safety, as pedestrians accounted for 55% of traffic-related fatalities in Toronto from 2014 to 2021 compared to 16% across Canada. The City of Toronto's main policy to address the significant public safety issue was to install leading pedestrian intervals at intersections across the city. In Toronto, leading pedestrian intervals provide a 5-second head start for pedestrians to cross at an intersection before parallel travelling or turning motorists are permitted to proceed. The walk advance is intended to improve pedestrian visibility to any turning motorists at the intersection and reinforce the pedestrian's right of way. Furthermore, a leading pedestrian interval's low installation cost makes it an attractive policy option to improve pedestrian safety in high-risk areas.[2] Figure 1 maps the widespread installation of leading pedestrian intervals following Toronto's Vision Zero Road Safety Plan.[3] By the end of 2023, leading pedestrian intervals were installed at 1306 (52%) of the 2515 traffic signals in Toronto compared to 0.4% at the end of 2016.

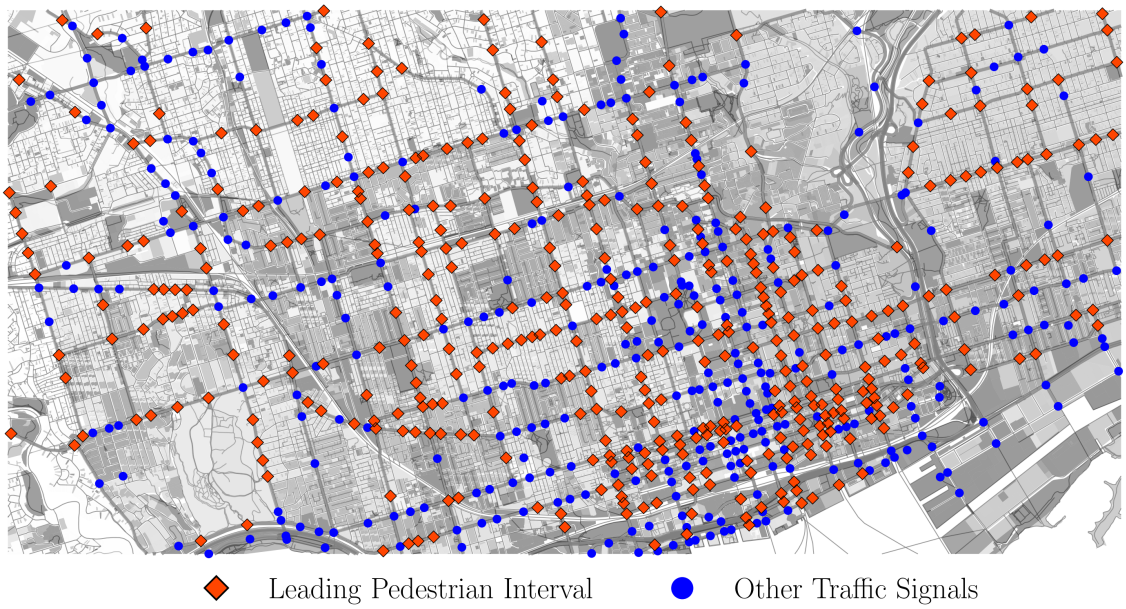
While the City of Toronto's Vision Zero Road Safety Plan initiated the widespread installation of leading pedestrian intervals, the city first introduced the measure in 2004 with a pilot project to evaluate the impact on pedestrian collisions. However, the city abandoned the pilot project after 6 weeks because of increased instances of non-yielding motorist behaviour at the piloted intersections. In the 12 years that followed, only 8 additional leading pedestrian

[2]The City of Toronto reports that installing leading pedestrian intervals costs an average of $120,000 per year between 2017 and 2021, with no associated operating costs after installation. Over this period, the city installed 867 leading pedestrian intervals, at an average cost of $692 per installation. The low installation cost stems from existing capital at Toronto traffic signals, as pedestrian walk signs are already installed at most traffic signals and no additional hardware or software is required; only the timing must be altered.

[3]Figure A1 in the Appendix shows an equivalent figure that shows the rapid installation across all of Toronto.



(a) 2016 Leading Pedestrian Intervals



(b) 2023 Leading Pedestrian Intervals

Figure 1: Leading Pedestrian Intervals Were Installed at 52% of Toronto Traffic Signals by the End of 2023

Notes: Figure 1 shows the rapid installation of leading pedestrian intervals between 2016 and 2023. Orange diamond markers indicate traffic signals equipped with a leading pedestrian interval in at least one traffic direction, whereas blue circle markers indicate all other traffic signals.

intervals were installed across the city. Although the rapid installation under Toronto's Vision Zero Road Safety Plan shows a strong intention to improve pedestrian safety, no causal evidence establishes how leading pedestrian intervals affect pedestrian safety or whether the non-yielding behaviour observed during the pilot offsets their safety benefits.

3 Data

3.1 Data Sources

To investigate how leading pedestrian intervals affect pedestrian collisions, I obtain a dataset containing information on the universe of collision reports from the City of Toronto's Transportation Services and a spatial dataset of Toronto traffic signals. The collision reports contain detailed information on every traffic collision in Toronto from January 2014 to August 2023 reported to the Toronto Police Service or a municipal collision centre. The dataset includes collision-level information, including the date, time, and geographic coordinates recorded by the reporting officer, and individual-level data, including each involved person's role, injury severity, and action during the collision.[4] One potential concern about collisions reported to a municipal collision centre is that the information is self-reported; individuals may misreport vehicle damage, injury severity, or other information. To account for potential reporting error, I exclude self-reported collisions that are neither verified by a city official nor accompanied by a police report.[5]

The dataset on Toronto traffic signals identifies every traffic signal's geographic coordinates, its activation date, whether a leading pedestrian interval is installed in each traffic direction, and the leading pedestrian interval's installation date. While previous traffic safety analyses examine how collisions change at a broader spatial scale, I focus on an intersection's

[4]Injuries are categorized into 5 main categories: Fatal, Major Injury, Minor Injury, Minimal Injury, and No Injury. A major injury is a non-fatal injury that is severe enough to require the injured person to be admitted to a hospital, even if only for observation at the time of the collision. This includes fractures, internal injuries, severe cuts and burns, concussions, and severe general shocks. The distinction between major injuries and minor or minimal injuries is essential to classify severe pedestrian collisions.

[5]This sample restriction drops 0.4% of all pedestrian collisions.

microgeographic area to mitigate the influence of potential confounding factors from other traffic safety policies. For the analysis, I include only collisions at controlled intersections with a traffic signal to ensure all intersections in the control sample are eligible for treatment.

To investigate whether leading pedestrian intervals reduce traffic speeds, I also obtain rush-hour traffic speed data from HERE Technologies via the City of Toronto. HERE Technologies collects vehicle speed data from a mix of vehicle probes across Toronto's major congestion network from January 2017 to August 2023. This dataset provides hourly binned speed data for road segments across Toronto during rush hour, specifically 8:00–8:59 a.m., 9:00–9:59 a.m., 3:00–3:59 p.m., 4:00–4:59 p.m., 5:00–5:59 p.m., and 6:00–6:59 p.m.

Finally, I supplement the collision and traffic speed data with mobile device GPS data from Cuebiq to account for differences in vehicle and pedestrian flows across intersections. Cuebiq Mobility Data is collected from anonymous users who have opted in to provide access to their location data anonymously, through a California Consumer Privacy Act (CCPA) and General Data Protection Regulation (GDPR)-compliant framework. The Cuebiq responsible data sharing framework enables research partners to query anonymized and privacy-enhanced data by providing access to an auditable, on-premise Data Cleanroom environment. All final outputs provided to partners are aggregated to preserve privacy. To maintain privacy, the platform censors sensitive points of interest, such as hospitals and schools, and reprojects each device's imputed home location to the centroid of its associated census block. As such, I exclude mobile device GPS pings from each device's home location because the recorded locations are not the exact coordinates of the mobile device. I then classify a mobile device as a pedestrian if its maximum speed does not exceed 5 metres per second within 30 minutes of its ping.

While the mobile device dataset provides daily estimates of the amount of mobile device traffic at an intersection on a given day, two notable limitations exist. First, mobile device GPS ping locations are often not precise enough to determine whether the mobile device is in an intersection or in an adjacent building. Thus, the data may overstate the number of mobile device GPS pings at intersections near shopping centres and office buildings. To account for

the limitation, I use the number of unique mobile device IDs pinged at an intersection on a given day as my measure of total mobile device traffic, preventing repeated pings from devices in adjacent buildings from inflating the measure. The second limitation is that the mobile device GPS dataset is only available in Toronto from May 2021 to December 2022, approximately 15% of the total sample time period and predominantly during Toronto's work-from-home period. Since this restricted sample has fewer observations and a large proportion of zeros in the pedestrian collision measure, the analysis using mobile device data has limited statistical power. As such, the mobile device dataset is used only as a robustness check.

3.2 Descriptive Statistics

Table 1 presents the annual collision statistics in Toronto from 2014 to 2022.[6] The first panel in Table 1 shows the prevalence of vehicle-only collisions in Toronto, with over 390,000 total collisions and 206 fatal collisions over the 9 observed years.[7] While the first panel presents collision-level statistics, the second and third panels report annual statistics at the individual level for pedestrians and cyclists involved in traffic collisions, with a total of 13,118 pedestrians and 7,671 cyclists over the period. Hereafter, I refer to the individual-level counts as pedestrian and cyclist collisions, respectively.[8] Although vehicle-only collisions are more prevalent than pedestrian collisions in Toronto, only 18% of vehicle-only collisions result in the death or injury of any person involved in the collision, while over 94% of pedestrians involved in collisions are killed or injured. The elevated risk to pedestrians, though unsurprising, illustrates why pedestrians are at the forefront of this public safety issue and why new policies must prioritize pedestrian safety.

Figure 2 presents indexed annual collision trends for vehicle-only collisions and pedes-

[6]Since the collision dataset for the analysis includes all reported collisions in Toronto until August 10, 2023, the annual collision totals for 2023 are incomplete.

[7]The severity classification for vehicle collisions corresponds to the highest level of injury incurred in the collision. The fatality measure corresponds to whether at least 1 fatality occurs in the collision, as opposed to the number of fatalities in vehicle-only collisions.

[8]Since these measures are defined at the individual level, a collision involving multiple pedestrians or cyclists is counted once for each person involved.

Table 1: Toronto Collision Summary Statistics

	2014	2015	2016	2017	2018	2019	2020	2021	2022	**Total**
Vehicle Collisions										
Collisions	44,166	44,966	49,329	51,525	56,105	53,415	26,816	26,780	37,284	**390,386**
Fatal Collisions	16	22	31	22	21	24	15	30	25	**206**
Injury Collisions	8,744	7,905	8,878	9,506	9,533	8,653	4,953	4,560	5,994	**68,726**
Casualty Share	19.8	17.6	18.1	18.5	17	16.2	18.5	17.1	16.1	**17.7**
Pedestrian Collisions										
Pedestrians	1,849	1,876	1,914	1,812	1,743	1,377	878	797	872	**13,118**
Fatalities	32	39	44	37	41	39	21	27	22	**302**
Injuries	1,721	1,734	1,752	1,651	1,576	1,292	820	729	799	**12,074**
Casualty Share	94.8	94.5	93.8	93.2	92.8	96.7	95.8	94.9	94.2	**94.3**
Cyclist Collisions										
Cyclists	1,171	1,152	1,208	1,014	833	584	560	603	546	**7,671**
Fatalities	3	4	1	4	4	1	4	1	2	**24**
Injuries	977	943	1,000	837	700	523	466	513	463	**6,422**
Casualty Share	83.7	82.2	82.9	82.9	84.5	89.7	83.9	85.2	85.2	**84**

Notes: Table 1 presents annual summary statistics for Toronto vehicle collisions, pedestrian collisions, and cyclist collisions. The first panel presents the total number of vehicle-only collisions, fatal vehicle-only collisions, and vehicle-only collisions that result in injury, followed by the percentage of vehicle-only collisions that result in an injury or death (casualty share). While the first panel summarizes total collision counts, the second and third panels present the annual summary statistics at the individual level. The second panel presents the total number of pedestrians involved in collisions, total pedestrian fatalities, and total pedestrian injuries, followed by the percentage of pedestrians involved in collisions who are injured or killed. The final panel presents the analogous results for cyclists.

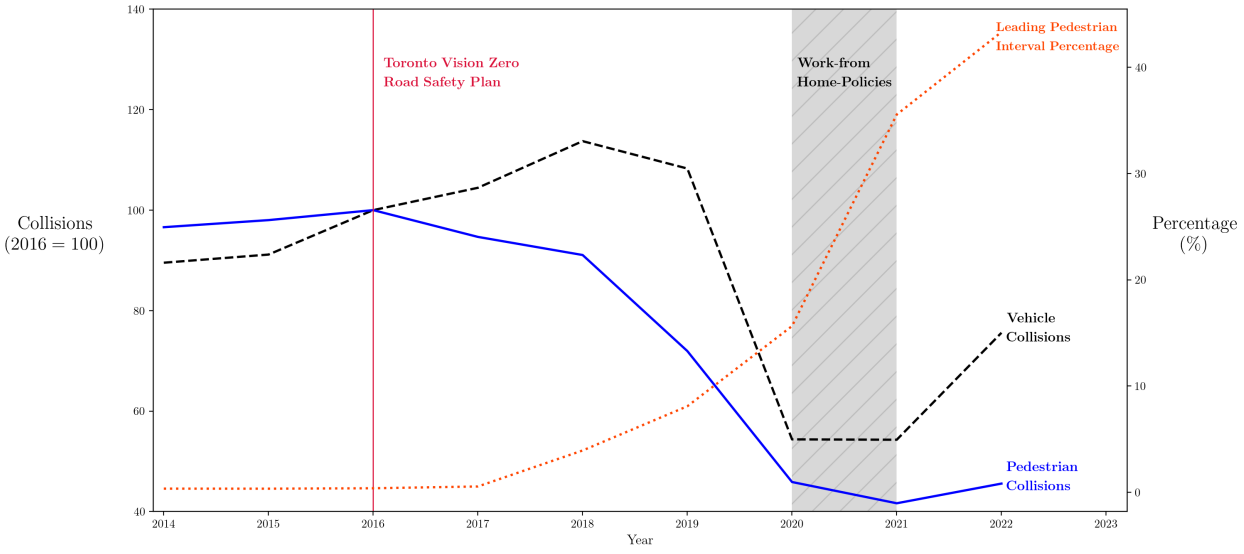


Figure 2: Pedestrian Collisions Remained Low After the Work-From-Home Period as Leading Pedestrian Intervals Expanded

Notes: Figure 2 shows the indexed time trends for both pedestrian and vehicle collisions from 2014 to 2022 on the left vertical axis and the total percentage of leading pedestrian intervals in Toronto on the right vertical axis. The collision index sets both pedestrian and vehicle collisions to 100 in 2016 to align with the launch of Toronto's Vision Zero Road Safety Plan. The solid blue line represents the pedestrian collision index, measured as the number of pedestrians involved in collisions, while the dashed black line represents the vehicle-only collision index. The orange dotted line shows the percentage of Toronto traffic signals equipped with a leading pedestrian interval on the right vertical axis. Finally, the grey-shaded region broadly aligns with Toronto's work-from-home policies in response to the COVID-19 pandemic, when fewer pedestrians and vehicles were on the roads.

trian collisions to provide descriptive evidence for why leading pedestrian intervals might improve pedestrian safety. Following the launch of Toronto's Vision Zero Road Safety Plan in 2016, pedestrian collisions steadily decreased while vehicle collisions continued to increase until 2018. From 2019 to 2021, both vehicle and pedestrian collisions sharply decreased after the government instituted work-from-home policies in response to the COVID-19 pandemic, which led to fewer road users.[9] As the economy re-opened, vehicle collisions increased 39% from 2021 to 2022 while pedestrian collisions only increased 9%.[10] The dotted line in Figure 2 offers a potential mechanism to explain why pedestrian collisions experienced less trend reversion: a rapid 35-percentage-point increase in the share of Toronto traffic signals with leading pedestrian intervals from 2019 to 2022. Figure 2 suggests that the widespread installation of leading pedestrian intervals across the city may explain why pedestrian collisions remained relatively low compared to vehicle collisions following the work-from-home period.

3.3 Intersection-Day Panel

To evaluate the causal relationship between leading pedestrian intervals and pedestrian collisions, I construct an intersection-day panel dataset with a spatial merge of traffic collisions to Toronto intersections. I assign a traffic collision to an intersection if the collision occurs within 50m of the intersection's centre, a sufficiently large radius to capture all traffic collisions within the intersection, at its crosswalks, and on a portion of each intersecting road. Figure 3 shows the pedestrian collision assignment to intersections in part of Toronto's downtown area. One limitation of the 50m radius definition is that the radii around intersections within close proximity may overlap. Although a more compact radius definition would limit the overlapping areas, a smaller radius may not cover the large intersections in the city's periphery and exclude relevant traffic collisions. Instead, I assign collisions within multiple intersection radii to the nearest intersection. The reassignment affects only

[9] Brodeur et al. (2021) shows work-from-home policies caused a significant reduction in vehicle collisions in the United States.

[10] Another way to interpret the difference in trend reversion is that vehicle collisions increased to 76% of the 2016 levels, while total pedestrians involved in collisions increased to 46%.

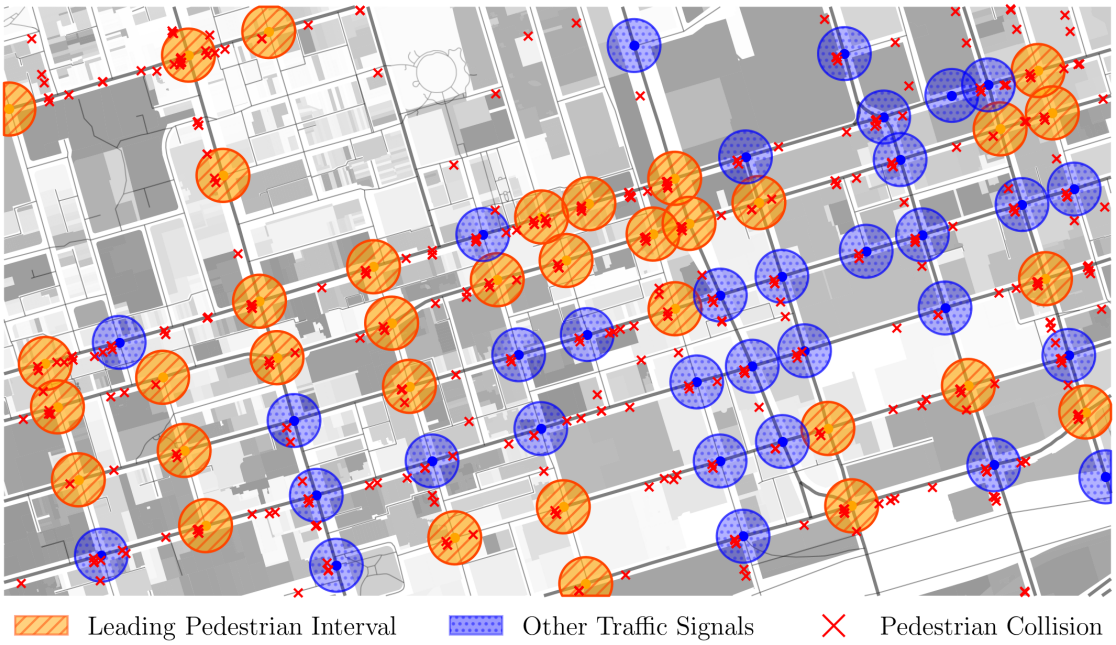


Figure 3: Pedestrian Collisions Are Assigned to Intersections Using 50 m Radii

Notes: Figure 3 presents a map of all traffic signals and pedestrian collisions in a neighbourhood centred around the intersection of Spadina Avenue and Queen Street West, located in Toronto's downtown core. The red 'x' markers represent pedestrian collisions between January 2014 and August 2023. Each intersection is represented by a 50m disc around a traffic signal. Orange-striped discs represent intersections with an installed leading pedestrian interval before August 2023, whereas blue-dotted discs are never-treated intersections. Collisions are assigned to an intersection if the collision occurs within the intersection's 50m disc.

16 pedestrian collisions (0.13%), none of which fall within overlapping radii that differ in treatment status.[11]

4 Empirical Strategy

4.1 Staggered Difference-in-Differences

I exploit the quasi-exogenous variation in a leading pedestrian interval's installation date and use a staggered difference-in-differences design to estimate the causal effect of leading pedestrian intervals on pedestrian collision probability. Equation 1 presents the main specification where PC_{it} is a binary variable for whether a pedestrian collision occurs at intersection i

[11] The percentage is calculated among collisions that occur within the radius of at least one intersection.

on date t, $LPI(t \geq \tau_i)$ is an indicator variable for whether a leading pedestrian interval is present at intersection i on date t where τ_i is the installation date, α_i is an intersection fixed effect, γ_t is a date fixed effect, and $\boldsymbol{X_{it}}$ is a vector of controls. In Equation 1, β is the coefficient of interest, which captures the average change in pedestrian collision probability after the city installs a leading pedestrian interval. Although traffic safety analyses often use Poisson regression models to determine how a safety mechanism affects the total number of traffic collisions, Equation 1 specifies a linear probability model to estimate the change in collision probability after installation. This effect at the extensive margin is the policy-relevant parameter because pedestrian collisions in microgeographic areas are infrequent events; the most relevant impact stems from the likelihood that any pedestrian collision occurs.[12]

$$PC_{it} = \alpha_i + \gamma_t + \beta LPI(t \geq \tau_i) + \boldsymbol{\Gamma X_{it}} + u_{it} \tag{1}$$

In the main specification, $\boldsymbol{\Gamma X_{it}}$ represents neighbourhood-by-date-by-road type fixed effects to control for the unobserved and non-uniform variation in traffic flows across the city over time. During the COVID-19 pandemic, both vehicle and pedestrian traffic flows decreased at different rates across the city, with larger declines in neighbourhoods close to Toronto's downtown core and along major roads. The unobserved variation in traffic volumes directly affects collision probabilities at intersections, as pedestrian collisions are more likely when traffic volumes are higher. To control for the unobserved variation across road types, I first classify an intersection as a 'major intersection' if it includes only arterial roadways as opposed to local roads or collectors.[13] I then interact road type, date, and the neighbourhood containing the traffic signal, using the City of Toronto's definitions of 158 neighbourhoods to control for the unobserved variation across Toronto during the work-from-home period.[14] Together, these fixed effects control for the differences in pedestrian collision probability that result from the non-uniform changes in traffic flows within the city.

[12] In the sample, 0.003% of the intersection-day pairs experience more than one pedestrian collision.

[13] The major intersection classification applies to 19% of all Toronto intersections.

[14] Toronto is composed of 158 social planning neighbourhoods, defined by the City of Toronto to help staff collect data, plan, analyze and forecast services, and help researchers analyze how areas within the city evolve.

Equation 2 then presents the corresponding event study specification to uncover any treatment effect dynamics. The coefficients of interest, $\{\beta_k\}_{k=0}^{899}$, estimate the causal change in the probability that a collision occurs k days after the city installs a leading pedestrian interval. While I restrict the post-treatment period to 900 days to limit the influence of changes in cohort composition on the estimated dynamics, I include an additional indicator variable, $LPI(t > \tau_i + 899)$, to estimate the long-run average treatment effect for all event days $k > 899$, β_{LR}. If the city first installs leading pedestrian intervals at its most dangerous intersections, then selection into the early treatment cohorts, combined with the change in the treated sample's composition over time, may explain any dynamic trends. At 900 days post-treatment, I retain 43% of the treated intersections in the sample. For the main specification, I estimate 900 pre-treatment coefficients to mirror the post-treatment period and compare collision trends at treated and untreated intersections over 10 quarters before treatment to provide evidence for the identification assumptions.

$$PC_{it} = \alpha_i + \gamma_t + \sum_{k=-900}^{899} \beta_k LPI(t = \tau_i + k) + \beta_{LR} LPI(t > \tau_i + 899) + \boldsymbol{\Gamma X_{it}} + \epsilon_{it} \qquad (2)$$

Since the staggered difference-in-differences and event study designs exploit the differential treatment timing at intersections, I follow Borusyak et al. (2024) to allow treatment effects to vary across time and treatment cohorts. The procedure constructs counterfactual outcomes using a first-stage regression on not-yet-treated and never-treated observations to estimate the intersection, date, and control fixed effects. Including the not-yet-treated intersections ensures all intersection fixed effects in the sample are identified for intersections that are not always treated, while including the never-treated intersections allows for every date fixed effect to be identified. For each treated intersection-date observation, the counterfactual pedestrian collision probability is imputed as the sum of its estimated intersection, date, and control fixed effects. The average treatment effect on the treated is then estimated as the average difference between the observed pedestrian collision outcomes and the imputed

counterfactual outcomes for treated intersection-date observations.

For the event-study design, each post-treatment coefficient is calculated as the average difference between observed and imputed collision outcomes among treated intersection-date observations at the corresponding event day. One distinction from the canonical two-way fixed effects regression model is the construction of the pre-treatment coefficients. With the imputation procedure in Borusyak et al. (2024), the pre-treatment coefficients are estimated in a separate event study regression using all untreated observations, $k \in \{-900, \ldots, -1\}$, and measure the differences in collision probability in the k^{th} day before treatment. Rather than using $k = -1$ as the reference period, the pre-treatment coefficients are estimated relative to a pooled reference group comprising untreated observations more than 900 days before treatment and all never-treated observations. Although I estimate the event study daily, I construct pseudo-quarterly treatment effects by taking a simple average of all estimated intersection-day treatment effects within each 90-day bin.[15]

4.2 Identification

Even if the location of a new leading pedestrian interval is chosen endogenously based on the history of pedestrian collisions, a staggered difference-in-differences design will identify the causal effect of treatment if two assumptions hold. First, I assume the timing of a leading pedestrian interval's installation is exogenous. The exogenous treatment-timing assumption requires leading pedestrian intervals not to be installed in response to a surge in pedestrian collisions at an intersection. Second, the framework assumes that, absent treatment, the pedestrian collision probability would evolve similarly at treated and untreated intersections– the parallel trends assumption. For the parallel trends assumption to hold, installation must not occur in response to intersections becoming more dangerous for pedestrians over time. One initial identification concern stems from a Toronto news report where former Mayor John Tory announced that Toronto chose the initial 80 leading pedestrian interval locations

[15]This aggregation mirrors the construction of the average treatment effect on the treated for the entire post-treatment sample, which equally weights all treated intersection-day effects. See Borusyak et al. (2024) for a discussion of how binning treatment periods prior to estimation can induce bias.

based on past pedestrian collisions at the intersection. The announcement, however, does not threaten the identification because the locations were selected based on past collision levels rather than a sudden increase in pedestrian collisions.[16] Furthermore, the announcement does not generate anticipatory effects because it did not identify which intersections would initially receive treatment.

An additional identification threat concerns the stable unit treatment value assumption (SUTVA) and whether leading pedestrian intervals affect the collision probability at nearby intersections in the control group. If leading pedestrian intervals reroute motorists, the collision rate at nearby intersections may increase, a potential SUTVA violation. To investigate the potential spillover effects, I estimate how installing leading pedestrian intervals affects pedestrian collisions at untreated intersections within the same census tract using Equation 3.[17] Equation 3 deviates from Equation 1 in two ways. First, the specification only includes never-treated intersections in the sample to compare untreated intersections in census tracts with a leading pedestrian interval to untreated intersections in census tracts without a leading pedestrian interval. Second, the treatment variable, $LPI(t \geq \tau_{c(i)})$, is an indicator variable for whether a leading pedestrian interval is present in i's census tract $c(i)$ on date t, where $\tau_{c(i)}$ is the date of the first installation in that tract. A statistically significant β under this specification would provide evidence that leading pedestrian intervals affect the collision probability at nearby untreated intersections, indicating a SUTVA violation.

$$PC_{it}^{N} = \alpha_i + \gamma_t + \beta LPI(t \geq \tau_{c(i)}) + \boldsymbol{\Gamma X_{it}} + u_{it} \tag{3}$$

[16] Figure A2 displays the LOWESS-smoothed monthly trend in the average number of pedestrian collisions per intersection for different treatment cohorts to investigate the claim. While the average number of monthly pedestrian collisions at intersections in the early treatment cohort (pre-2018) is significantly greater compared to the other treatment cohorts and the never-treated sample, the treatment timing for each cohort does not appear to be in response to a sudden increase in pedestrian collisions, nor at locations that are becoming increasingly dangerous over time.

[17] The City of Toronto is divided into 583 census tracts, with the median census tract containing 4 traffic signals. Unlike broader geographic definitions, using a census tract definition ensures that only localized spillovers are captured.

5 Results

5.1 Pedestrian Collision Probability

Table 2 reports the change in collision probability after the city installs a leading pedestrian interval, with the change in total pedestrian collision probability in Panel A and the change in major pedestrian collision probability in Panel B. Since the likelihood of any pedestrian collision at an intersection on a given date is low, the percentage point changes are small by construction. To derive a more intuitive estimate, I express the coefficients as the percentage change in collision probability relative to the year before treatment. Columns (1)–(3) present the treatment effect estimates using Borusyak et al. (2024)'s imputation procedure to correct for time and cohort heterogeneity. While Column (1) does not include control variables, Column (2) adds neighbourhood-by-date fixed effects and Column (3) presents the main results with neighbourhood-by-date-by-road type fixed effects. After the city installs a leading pedestrian interval at an intersection, I estimate that pedestrian collision probability decreases by 39% relative to the year before treatment. Furthermore, the likelihood of a major pedestrian collision at an intersection that results in a pedestrian fatality or major injury falls by 56%. The substantial decrease in pedestrian collision probability highlights the potential for leading pedestrian intervals to improve the safety of vulnerable road users and address the ongoing public health issue. [18]

The final columns of Table 2 present alternative estimators to the main specification. In Column (4), I report the treatment effect from a canonical two-way fixed effects (TWFE) regression that does not correct for time and cohort heterogeneity and obtain a near-identical estimate of the decrease in pedestrian collision probability. Although the treatment effect

[18] Table A3 reports additional treatment effects for vulnerable pedestrians and for pedestrian collisions occurring between dusk and dawn, two emphasis areas in Toronto's Vision Zero Road Safety Plan. Following the definitions from Toronto's Vision Zero Road Safety Plan, I classify pedestrians under 19 or over 55 as vulnerable and estimate that leading pedestrian intervals reduce their collision probability by 47%. While the point estimate suggests a 45% decrease in major pedestrian collision probability, the decrease is not statistically significant. Finally, pedestrian and major pedestrian collision probabilities between dusk and dawn decrease by 55% and 68%, respectively, although these estimates are not statistically different from the baseline estimates.

Table 2: Pedestrian and Major Pedestrian Collision Probabilities Decrease Significantly

	Borusyak et al. (2024)			TWFE	PPML
	(1)	(2)	(3)	(4)	(5)
Panel A. Pedestrian Collisions					
ATT	-0.000197***	-0.000160***	-0.000241***	-0.000241***	-0.229***
	(<0.001)	(<0.001)	(<0.001)	(<0.001)	(0.001)
Percentage Change	-31.69%	-25.78%	-38.73%	-38.69%	-20.43%
Panel B. Major Pedestrian Collisions					
ATT	-0.0000495***	-0.0000540***	-0.0000582***	-0.0000448**	-0.676**
	(<0.001)	(<0.001)	(<0.001)	(0.004)	(0.004)
Percentage Change	-47.4%	-51.67%	-55.73%	-42.89%	-49.15%
Fixed Effects					
Intersection	✓	✓	✓	✓	✓
Date	✓	✓	✓	✓	✓
Neighbourhood × Date	–	✓	–	–	–
Neighbourhood × Date × Road Type	–	–	✓	✓	✓
Observations	8,719,865	8,701,697	8,691,154	8,621,613	8,719,865

Notes: Significant at the ***[0.1%] **[1%] *[5%] level with p-values in parentheses. Standard errors are clustered at the intersection level. Panel A presents the average treatment effect on the treated of leading pedestrian intervals on pedestrian collision probability, while Panel B presents the average treatment effect on major pedestrian collisions when a pedestrian is killed or incurs a major injury. Columns (1)–(3) correct for time and cohort heterogeneity following Borusyak et al. (2024), with Column (2) including neighbourhood-by-date fixed effects and Column (3) including neighbourhood-by-date-by-road type fixed effects. Column (4) presents the results from a two-way fixed effects difference-in-differences without correcting for time and cohort heterogeneity. Finally, Column (5) presents the Poisson pseudo-maximum likelihood estimates of the change in the total number of pedestrian collisions. In Columns (1)–(4), the average treatment effect on the treated represents the percentage-point change in collision probability after treatment. The percentage change expresses this estimate relative to the collision probability in the year before treatment.

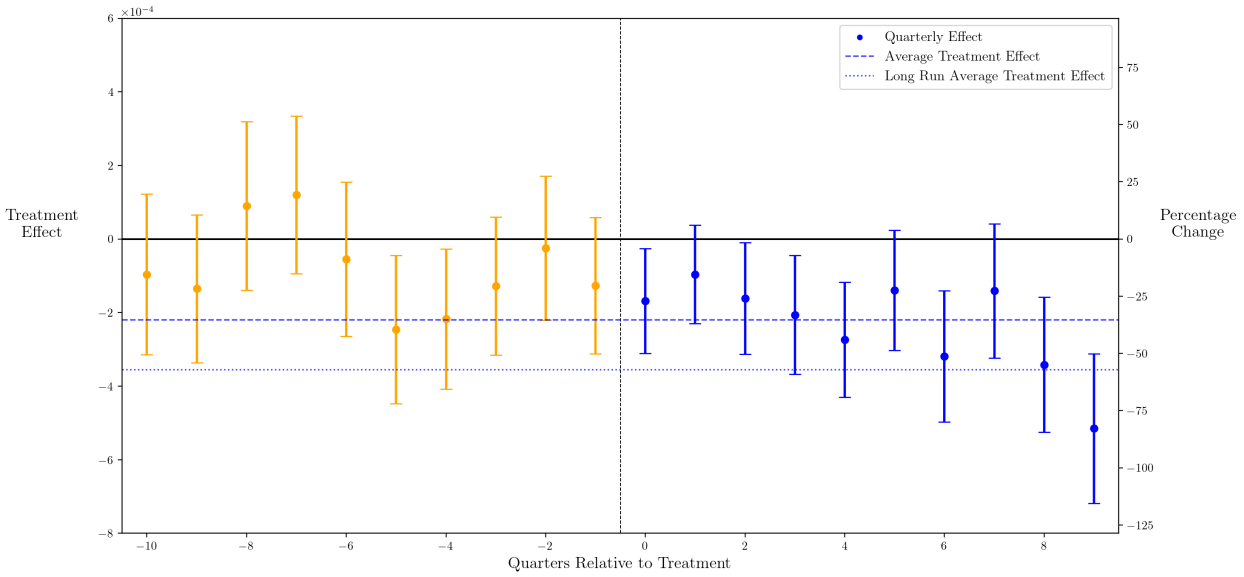


Figure 4: Pedestrian Collision Probability Decreases over Time

Notes: Figure 4 shows the event study results to investigate the dynamic causal effects of leading pedestrian intervals on pedestrian collision probability. Although Equation 2 estimates daily coefficients, the figure above shows the aggregated point estimates at the 90-day level to construct pseudo-quarterly treatment effects. The dashed line shows the average treatment effect from the difference-in-differences specification, while the dotted horizontal line shows the long-run average treatment effect for event days $k > 899$, β_{LR}. The confidence bars show the 95% confidence intervals for the aggregated coefficients. The left vertical axis shows the coefficient estimate, while the right vertical axis shows the percentage change relative to the year prior to treatment.

on major pedestrian collision probability attenuates, the point estimates remain statistically similar. Finally, Column (5) estimates the percentage change in the total number of pedestrian collisions using a Pseudo-Poisson maximum likelihood (PPML) regression. Although analyses using count data often use PPML as the main specification, Borusyak et al. (2024)'s imputation procedure does not extend to Poisson models because the fitted values of Poisson regressions are not unbiased, a requirement to construct the counterfactual outcomes. As such, the PPML results do not correct for time and cohort heterogeneity. The PPML estimates in Column (5) indicate that leading pedestrian intervals decreased total pedestrian collisions by 20% and major pedestrian collisions by 49%, but may be subject to bias from the homogeneous treatment effect assumption. Although the estimates attenuate compared to the main specification, the decrease in total pedestrian collisions supports the result that leading pedestrian intervals improve pedestrian safety.

Figure 4 presents the event study for pedestrian collision probability to assess the iden-

tifying assumptions and investigate any dynamic treatment effects. Alongside the event study coefficients, the dashed line shows the average treatment effect from the difference-in-differences specification, while the dotted line shows the long-run average treatment effect, β_{LR}. During the pre-treatment period, collision probabilities at intersections in the treated sample appear statistically similar to those at untreated intersections, as some quarters show slightly higher collision rates while others exhibit lower rates. Although two pre-treatment coefficients are statistically less than zero, the remaining estimates fluctuate around zero. The lack of a consistent trend in the pre-treatment period suggests the average treatment effect on the treated is unlikely to reflect anticipatory behaviour or a violation of parallel trends. In the post-treatment period, the event study coefficients show that pedestrian collision probability decreases after the city installs a leading pedestrian interval and that the safety improvement becomes more prominent over time.[19] This downward trend suggests that some motorists may require an adjustment period to learn how to adhere to the new traffic safety measure, and that the long-run treatment effects exceed the short-run effects immediately following treatment.

Finally, Figure 5 presents the event study results for major pedestrian collision probability, where the insignificant pre-treatment coefficients show no evidence of a violation of the parallel trends and exogenous treatment time assumptions. However, most event study coefficients in the post-treatment period are not statistically significant, as the infrequency of major pedestrian collisions lowers the statistical power for each coefficient. Although the dynamic trends of major pedestrian collisions do not reveal a clear trend, the significant average treatment effect alongside the valid empirical design suggests that leading pedestrian intervals reduce major pedestrian collision probability and improve pedestrian safety.

[19] I separately estimate trends in the pre- and post-treatment periods, following an approach similar to Freyaldenhoven et al. (2021). I estimate that the slope is -9.79×10^{-6} (p-value $= 0.499$) in the pre-treatment period, and is -2.99×10^{-4} (p-value $= 0.021$) in the post-treatment period. The statistically insignificant pre-treatment slope provides no evidence of a linear pre-trend, while the post-treatment slope provides evidence that the safety improvements become more prominent over time. Although both slopes are negative, they are statistically different at the 10% level.

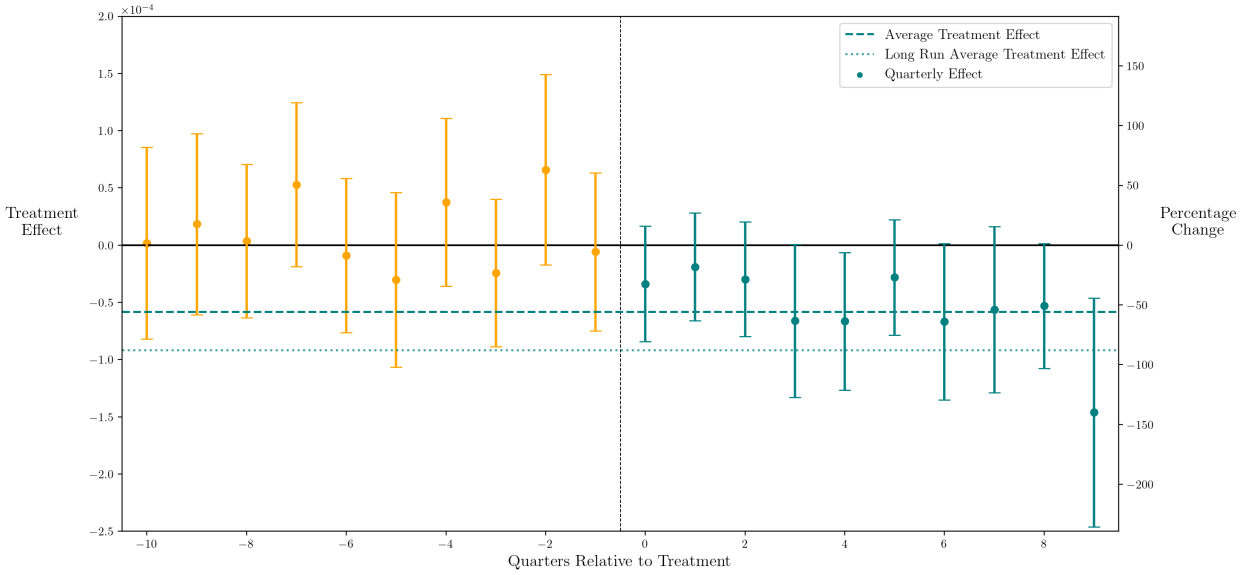


Figure 5: Major Pedestrian Collision Probability Shows No Dynamic Trend

Notes: Figure 5 shows the event study results to investigate the dynamic causal effects of leading pedestrian intervals on major pedestrian collision probability. The figure above shows the aggregated point estimates at the 90-day level to construct pseudo-quarterly treatment effects. The confidence bars show the 95% confidence intervals for the aggregated coefficients. The dashed line shows the average treatment effect from the difference-in-differences specification, while the dotted horizontal line shows the long-run average treatment effect for event days $k > 899$, β_{LR}. The left vertical axis shows the coefficient estimate, while the right vertical axis shows the percentage change relative to the year prior to treatment.

5.2 Behavioural Responses of Motorists

To understand the mechanisms behind the treatment effect and uncover whether a motorist behavioural response exists, I evaluate how the motorist actions that cause pedestrian collisions change after the city installs a leading pedestrian interval. I re-estimate Equation 1 using separate binary dependent variables for whether a pedestrian collision attributed to each motorist action occurs at an intersection on each date. I focus on four classifications of motorist actions: improper turns, failing to yield the right of way, disobeying traffic controls, and all other causes.[20] While failing to yield and improper turns are the two most common causes of pedestrian collisions, I estimate a separate treatment effect for motorists who disobey traffic controls to verify whether the change in the reported motorist action is a mechanical result of a change in reporting classifications. For instance, a collision pre-

[20]The analysis is underpowered to estimate an effect for all motorist actions in the data because many motorist actions infrequently cause pedestrian collisions. I use the "All Other Causes" classification to estimate the combined change in collisions from following too close, exceeding the speed limit, driving too fast for conditions, driving too slow, losing control of the vehicle, improper passing, and no improper actions.

Table 3: Decreases in Failure-to-Yield and Improper-Turn Collisions

	Motorist Action			
	(1) Failure to Yield	(2) Improper Turn	(3) Disobeyed Traffic Control	(4) All Other Causes
ATT	-0.000143***	-0.0000513***	-0.0000163*	-0.0000304
	(<0.001)	(<0.001)	(0.014)	(0.192)
% Change	-43.57%	-69.6%	-98.2%	-14.7%
Share of Total ATT	59.3%	21.3%	6.8%	12.6%
Share of Pedestrian Collisions	54%	9.7%	3.2%	33.1%
Fixed Effects				
Intersection	✓	✓	✓	✓
Date	✓	✓	✓	✓
Neighbourhood × Date × Road Type	✓	✓	✓	✓
Observations	8,691,154	8,691,154	8,691,154	8,691,154

Notes: Significant at the ***[0.1%] **[1%] *[5%] level with p-values in parentheses. Standard errors are clustered at the intersection level. The average treatment effect on the treated shows the percentage point change in collision probability that corrects for time and cohort heterogeneity following Borusyak et al. (2024), while the percentage change expresses this estimate relative to the collision probability in the year before treatment. All columns follow the main specification and include neighbourhood-by-date-by-road type fixed effects. The conditional frequency reports the percentage of pedestrian collisions attributed to each respective collision type in the not-yet-treated sample in the year before treatment.

viously reported as resulting from an improper turn may instead be classified as resulting from a motorist disobeying a traffic control after a leading pedestrian interval is installed. If pedestrian collisions attributed to motorists disobeying traffic controls increase following treatment, decreases in the other causes may reflect reclassification rather than changes in driving behaviour. Table 3 presents the estimates for the change in pedestrian collision probability from each motorist action and decomposes the total treatment effect into the contribution of each collision type.[21]

Column (1) shows that leading pedestrian intervals decrease the probability of a pedestrian collision caused by a motorist failing to yield by 44%, consistent with the treatment effect on all pedestrian collisions. Since failure-to-yield collisions directly reflect a violation of a pedestrian's right-of-way, the decrease in collision probability shows that leading pedestrian intervals work as intended to reinforce a pedestrian's right-of-way at an intersection. As a motorist failing to yield is the most frequent cause of pedestrian collisions in Toronto, the improved yielding behaviour explains 59% of the total decrease in pedestrian collision probability. Rather than a behavioural offset that attenuates the safety improvement, the

[21]Figure A3 presents the event study estimates to provide evidence for the exogenous treatment time and parallel trends assumptions, and show the dynamic treatment effects for each motorist action.

improved yielding behaviour of motorists indicates that leading pedestrian intervals elicit a positive behavioural response from motorists that reduces the pedestrian collision probability.

Although the change in motorist behaviour is the primary mechanism to explain the pedestrian safety improvement, leading pedestrian intervals are also designed to improve pedestrian visibility to motorists. I evaluate this mechanism using pedestrian collisions from improper turns. Since turning vehicles are especially likely to conflict with pedestrians at intersections, improved visibility should be reflected in fewer collisions from improper turns. Column (2) presents the results and shows that leading pedestrian intervals decrease the pedestrian collision probability from improper turns by a substantial 70%. While only 9.7% of pedestrian collisions are from improper turns, the substantial decrease in collision probability explains 21% of the total decrease in pedestrian collision probability. The decrease in collisions from improper turns demonstrates that leading pedestrian intervals work as intended and improve pedestrian visibility to motorists.

Finally, Column (3) indicates that the pedestrian collision probability from disobeying traffic controls decreases and Column (4) shows that no significant change in collisions from other motorist actions occurs. Although the decrease from disobeying traffic controls is only marginally significant, the negative estimate provides evidence that the decreases in collision probability from improper turns and failing to yield are not a mechanical result of reclassifying the cause of the collision as a disobeyed traffic control. While the pedestrian collision probability from all other causes does not significantly change, significant decreases in the two most prominent causes of pedestrian collisions indicate that leading pedestrian intervals improve motorist behaviour and pedestrian visibility.

5.3 Traffic Speed

One potential unintended consequence of leading pedestrian intervals is a decrease in vehicle speed at intersections from the extended vehicle wait times. To investigate this, I estimate how leading pedestrian intervals affect traffic speeds during rush hour in Toronto using

Table 4: Leading Pedestrian Intervals Reduce Rush-Hour Vehicle Speeds

	Rush Hour Vehicle Speed			Morning Rush Hour	Evening Rush Hour
	(1)	(2)	(3)	(4)	(5)
ATT	-0.128***	-0.103***	-0.120***	-0.107***	-0.125***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Percentage Change	-1.40%	-1.13%	-1.30%	-1.16%	-1.37%
Fixed Effects					
Road Segment	✓	✓	✓	✓	✓
Date	✓	✓	✓	✓	✓
Hour	✓	✓	✓	✓	✓
Neighbourhood × Date	–	✓	–	–	–
Neighbourhood × Date × Road Type	–	–	✓	✓	✓
Observations	60,201,573	58,393,160	53,425,744	17,576,667	35,839,586

Notes: Significant at the ***[0.1%] **[1%] *[5%] level with p-values in parentheses. Standard errors are clustered at the road segment level. Table 4 reports the average treatment effect of a leading pedestrian interval on vehicle speed along each road segment. The average treatment effect on the treated represents the absolute change in vehicle speed along a treated road segment after installation, while the percentage change expresses this estimate relative to the segment's average speed in the year before treatment. All results correct for time and cohort heterogeneity following Borusyak et al. (2024), with Column (2) adding neighbourhood-by-date fixed effects, and Column (3) adding neighbourhood-by-date-by-road type fixed effects to follow the main specification. Columns (4) and (5) report the effects during the morning rush hour and evening rush hour, respectively.

Equation 4, a staggered difference-in-differences equation at the road segment-hour-date level. In Equation 4, $Speed_{jht}$ is the traffic speed on each road segment j during hour h on date t, $LPI(t \geq \tau_{j(i)})$ is the treatment variable for whether road segment $j(i)$ crosses through intersection i with a leading pedestrian interval, $\boldsymbol{\Gamma X_{jht}}$ is a vector of controls, and ϕ_j, ξ_h, γ_t are road segment, hour, and date fixed effects, respectively. As in the main specification, $\boldsymbol{\Gamma X_{jht}}$ remains neighbourhood-by-date-by-road type fixed effects. However, the road type variable is now defined based on whether each road segment is an arterial road rather than the previous intersection-based definition. Table 4 reports the results from Equation 4.

$$Speed_{jht} = \phi_j + \xi_h + \gamma_t + \beta LPI(t \geq \tau_{j(i)}) + \boldsymbol{\Gamma X_{jht}} + \nu_{jht} \tag{4}$$

After controlling for neighbourhood-by-date-road type fixed effects, I estimate that average rush-hour vehicle speeds fall by 0.13 km/h along road segments that intersect a leading pedestrian interval, a 1.3% decrease relative to the year before treatment. Columns (4) and (5) present the estimates for morning and evening rush hour traffic speeds independently and show that the decreases in traffic speed at different times of day are statistically similar. While the reduction in traffic speeds offers another potential mechanism to explain the increase in pedestrian safety, it also shows that leading pedestrian intervals increase motorist

commute times. Even though the capital installation costs of leading pedestrian intervals are low, the measures impose a high implicit cost on motorists. I discuss and quantify the total cost based on the total value of travel time lost from the delay in Section 6.1.

5.4 Other Road Users

I next investigate whether leading pedestrian intervals affect the collision probabilities of cyclists and motorists. Although neither cyclists nor motorists are permitted to proceed through the intersection during the advance walk signal, the behavioural response and the lower speeds of motorists may unintentionally improve traffic safety for other road users. Table 5 presents the results from the staggered difference-in-differences specifications analogous to Equation 1 for each collision type, while Figure 6 shows the event study results following Equation 2. After the city installs a leading pedestrian interval, cyclist collision probability decreases by 33%. The statistically significant average treatment effect, alongside the near-zero pre-treatment coefficients in Figure 6a that do not exhibit a trend, suggests that leading pedestrian intervals improve cyclist safety.[22] However, the event study shows considerable noise in the dynamic treatment effects, as cyclist collisions are even less frequent at intersections compared to pedestrian collisions.

While leading pedestrian intervals decrease the cyclist collision probability, I estimate that leading pedestrian intervals increase the vehicle collision probability at an intersection by 7.6%. This increase is attributable to minor vehicle collisions, as the probability of a vehicle collision that results in a fatality or major injury does not significantly change. One possible explanation for the increase is that motorists incorrectly anticipate signal changes after the city installs leading pedestrian intervals. For instance, motorists accustomed to the standard 2–5 second period during which all traffic signals at the intersection remain red may anticipate the traffic signal turning green and accelerate. Alternatively, motorists may

[22] Although the event study coefficient for period $t = -10$ is significantly less than zero, there does not appear to be any pretrend, as remaining coefficients show fluctuations in the cyclist collision probability across intersection types, with some quarters exhibiting higher collision rates at intersections with leading pedestrian intervals and others exhibiting lower rates.

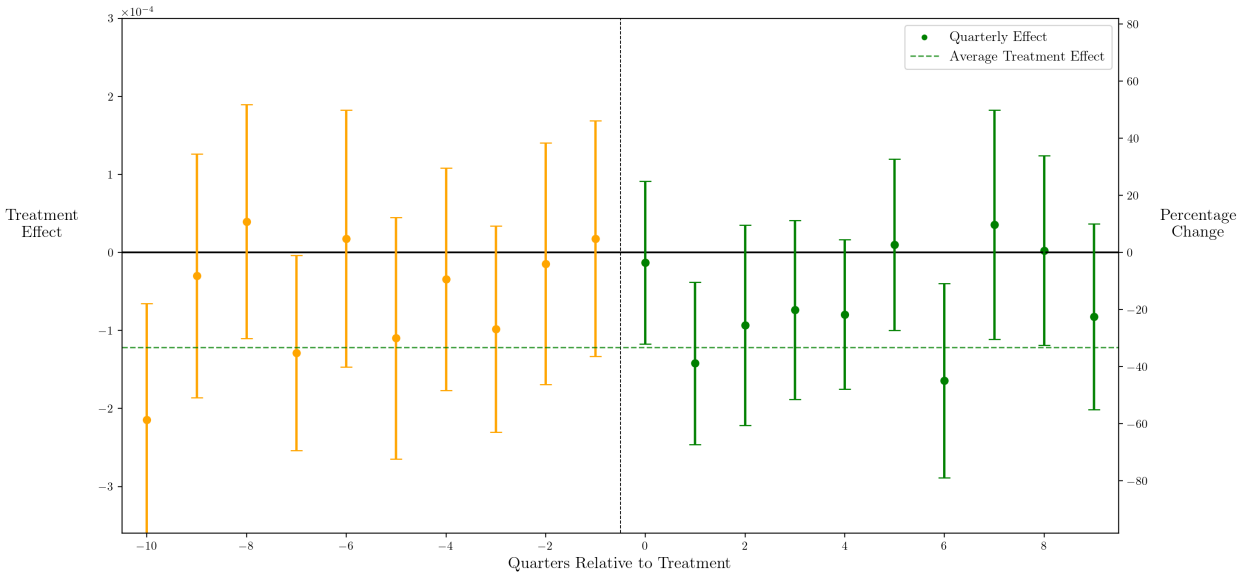
Table 5: Cyclist Collisions Decrease, While Only Minor Vehicle Collisions Increase

	Cyclist Collisions		Vehicle Collisions	
	(1) Collisions	(2) Major Collisions	(3) Collisions	(4) Major Collisions
ATT	-0.000122***	-0.00000737	0.00121***	0.00000641
	(0.000)	(0.199)	(0.000)	(0.567)
% Change	-33.42%	-25.88%	7.59%	10.79%
Fixed Effects				
Intersection	✓	✓	✓	✓
Date	✓	✓	✓	✓
Neighbourhood × Date × Road Type	✓	✓	✓	✓
Observations	8,691,154	8,691,154	8,691,154	8,691,154

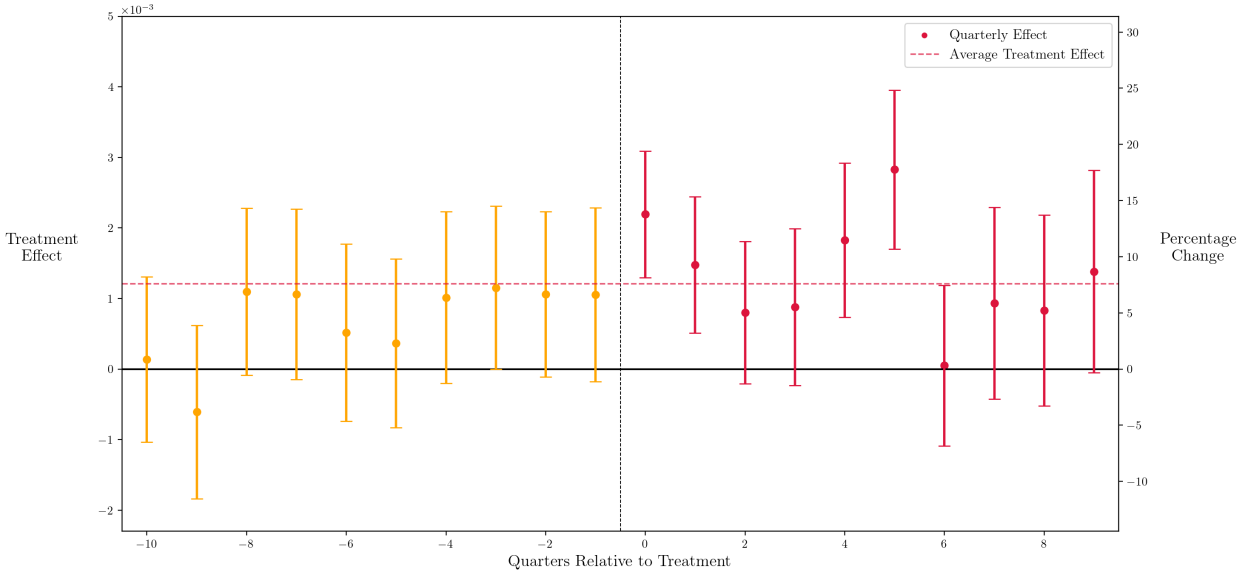
Notes: Significant at the ***[0.1%] **[1%] *[5%] level with p-values in parentheses. Standard errors are clustered at the intersection level. Columns (1) and (2) present the average treatment effect on the treated from leading pedestrian intervals on cyclist collision probability and major cyclist collision probability, while Columns (3) and (4) present the average treatment effect on the treated from leading pedestrian intervals on vehicle collision probability and major vehicle collision probability. The average treatment effect on the treated represents the percentage-point change in collision probability after treatment, while the percentage change expresses this estimate relative to the collision probability in the year before treatment. All results correct for time and cohort heterogeneity following Borusyak et al. (2024).

see the green walk sign illuminate and accelerate into the intersection, expecting the traffic signal to turn green. In both cases, rear-end collisions from motorists following too close may increase if vehicles farther back in the queue accelerate forward while those ahead remain stopped.[23] Table A2 presents the average treatment effects on vehicle collision probability separately by motorist action. Consistent with this narrative, Table A2 shows that only the probability of a vehicle collision attributed to a motorist following too close increases after treatment. While the results indicate a statistically significant increase in vehicle collisions, the magnitudes should be interpreted with caution. As shown in Figure 6b, the event-study estimates are most prominent in the initial quarters following treatment, including a sharp increase during the first quarter. In the pre-treatment period, however, Figure 6b shows that vehicle collision probability at eventually treated intersections is 4.3% higher in the year preceding treatment than in the pooled pre-treatment reference period, suggesting the estimated average treatment effect may overstate the true treatment effect.

[23] In a similar setting, Kapoor and Magesan (2014) find that pedestrian countdown signals increase collisions involving tailgating in Toronto, which they interpret as evidence that drivers who know exactly when traffic lights will change behave more aggressively.



(a) Cyclist Collision Probability



(b) Vehicle Collision Probability

Figure 6: Cyclist Safety Improves, but Vehicle Estimates Require Caution

Notes: Figure 6 shows the event study results to investigate the dynamic causal effects of leading pedestrian intervals on (a) cyclist collision probability and (b) vehicle collision probability. Figure 6 above presents the aggregated point estimates and their 95% confidence intervals at the 90-day level to construct pseudo-quarterly treatment effects. The left vertical axis shows the coefficient estimate, while the right vertical axis shows the percentage change in the collision probability relative to the year before treatment.

Table 6: No Evidence of Spillovers onto Nearby Untreated Intersections

	Pedestrian Collisions		Major Pedestrian Collisions	
	(1)	(2)	(3)	(4)
ATT	-0.0000123	0.0000353	-0.0000112	-0.0000161
	(0.832)	(0.565)	(0.504)	(0.432)
% Change	-1.61%	4.61%	-10.27%	-14.82%
Fixed Effects				
Intersection	✓	✓	✓	✓
Date	✓	✓	✓	✓
Neighbourhood × Date × Road Type	–	✓	–	✓
Observations	4,603,808	4,083,265	4,603,808	4,083,265

Notes: Significant at the ***[0.1%] **[1%] *[5%] level with p-values in parentheses. Standard errors are clustered at the intersection level. The average treatment effect on the treated shows the percentage-point change in collision probability at never-treated intersections after a leading pedestrian interval is installed in the same census tract. Columns (1) and (3) present the estimates with only intersection and date fixed effects, while Columns (2) and (4) include neighbourhood-by-date-by-road type fixed effects to follow the main specification. All specifications correct for time and cohort heterogeneity following Borusyak et al. (2024). Finally, the percentage change expresses this estimate relative to the collision probability in the year before treatment.

5.5 Spillovers

Since treatment occurs at a microgeographic level, one potential threat to identification is that leading pedestrian intervals may reroute motorists to nearby intersections and increase the collision probability in the untreated sample. If leading pedestrian intervals increase collision rates at intersections in the untreated sample, the specification would violate SUTVA and overestimate the treatment effect. To assess this potential SUTVA violation, I investigate whether the effects of a leading pedestrian interval spill over onto nearby intersections within the same census tract. If the estimated spillover is positive, then never-treated intersections close to leading pedestrian intervals become more dangerous relative to untreated intersections in census tracts without leading pedestrian intervals. Table 6 shows the estimated treatment effects on never-treated intersections from Equation 3.[24]

After the city installs a leading pedestrian interval in a census tract, the pedestrian collision probability at untreated intersections within the same census tract does not notably change, with a statistically insignificant 4.7% increase in pedestrian collision probability and

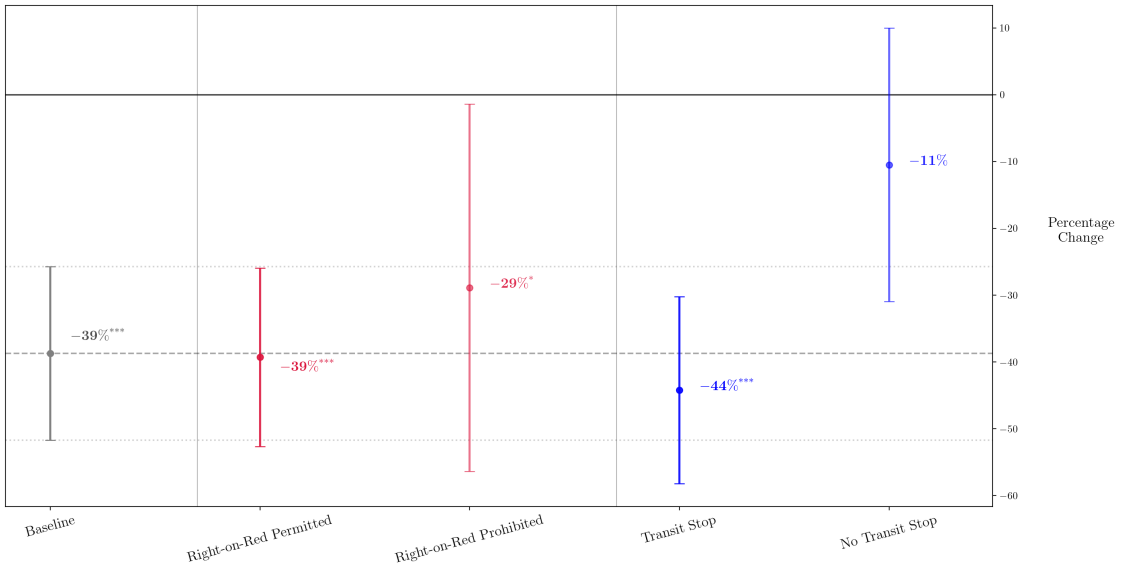
[24]Figure A4 in the appendix shows the corresponding event study results.

a 14% decrease in major pedestrian collision probability. Since both estimated treatment effects are statistically insignificant, I cannot reject the null hypothesis that no spillovers exist from leading pedestrian intervals on intersections within the same census tract, and I do not uncover evidence against SUTVA. The localized effect from leading pedestrian intervals indicates that even though the safety mechanisms elicit improved yielding behaviour from motorists, the change in driving habits does not extend to untreated intersections.

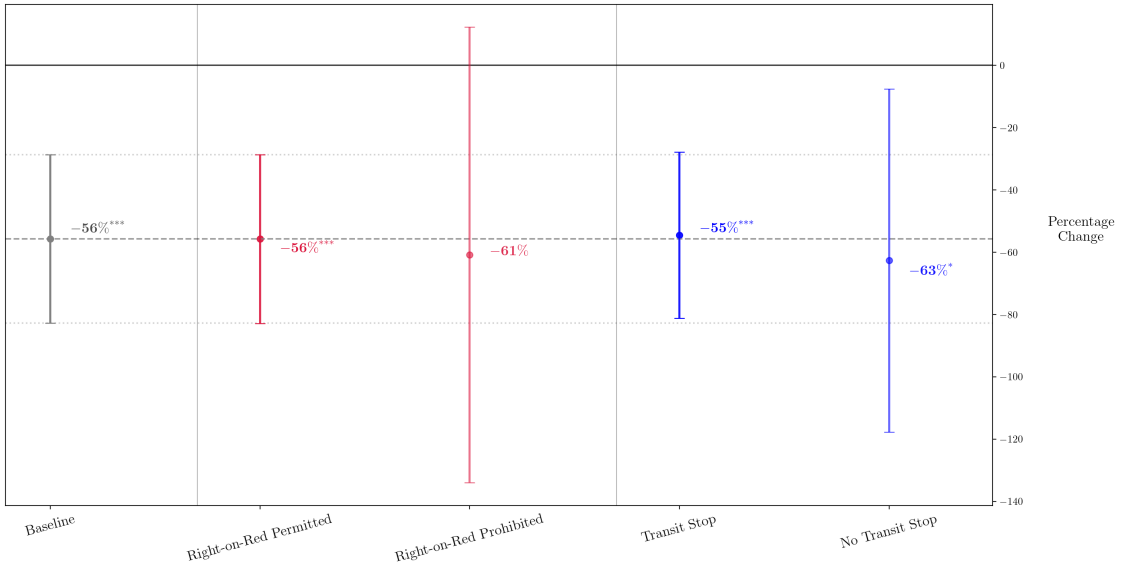
5.6 Heterogeneous Treatment Effects

I next evaluate how various intersection features influence the efficacy of leading pedestrian intervals. If a leading pedestrian interval is ineffective when certain intersection features are present, it will prolong a motorist's commute without any pedestrian safety improvement. I focus on two intersection features: whether right turns on red traffic signals are prohibited and whether public transit stops are present. Figure 7 presents the heterogeneous treatment effects for each intersection trait on both pedestrian collision probability and major pedestrian collision probability.

Figure 7a shows that the decrease in pedestrian collision probability at intersections that permit right turns on red traffic signals is statistically similar to the baseline estimate. However, the safety improvement attenuates and is only marginally significant at intersections that prohibit right turns on red traffic signals. Similarly, the treatment effect on major pedestrian collision probability at intersections that prohibit right turns on red traffic signals is not statistically different from 0. The turn restriction likely attenuates the treatment effect because both safety measures target turning motorists. At intersections where right turns on red are permitted, pedestrians face higher collision risks because vehicles may enter the intersection and pass through the crosswalk during either a red or green signal. With the turn prohibition, the pedestrian collision risk decreases because only vehicles with a green traffic signal may pass through the crosswalk. Since both measures protect pedestrians from turning vehicles, the treatment mechanisms overlap and leading pedestrian intervals generate smaller pedestrian safety improvements at intersections that prohibit right turns on red



(a) Pedestrian Collision Probability



(b) Major Pedestrian Collision Probability

Figure 7: Safety Improvements Vary by Intersection Traits

Notes: Figure 7 shows the heterogeneous average treatment effects of leading pedestrian intervals with various intersection features. Figure 7a presents the estimates for total pedestrian collision probability, while Figure 7b shows the results for major pedestrian collision probability. Each panel presents the estimates and their 95% confidence intervals for (1) the baseline model, (2) intersections where right turns on red are permitted, and (3) intersections with a transit stop. The dashed and dotted grey lines indicate the baseline model's point estimate and confidence bounds. The right vertical axis shows the change in pedestrian collision probability relative to the year before treatment. All estimates correct for time and cohort heterogeneity following Borusyak et al. (2024) and include neighbourhood-by-date-by-road type fixed effects to follow the main specification.

traffic signals.

The third pane presents the treatment effects at intersections with and without public transit stops.[25] Transit stops may influence the efficacy of leading pedestrian intervals for two reasons. First, intersections with transit stops may have higher pedestrian frequencies relative to those without. Second, physical shelters and public transit vehicles at intersections with transit stops may obstruct visibility between motorists and pedestrians. While the decrease at an intersection with a public transit stop is marginally larger than the baseline estimates, the decrease in pedestrian collision probability at intersections without a public transit stop is statistically insignificant. The difference across intersection types suggests that leading pedestrian intervals are most effective at intersections that pedestrians frequent. However, the significant 63% decrease in major pedestrian collision probability at intersections without public transit stops indicates that pedestrian safety benefits exist even at less frequented intersections.

5.7 Robustness Checks

I perform several robustness checks to assess the decrease in pedestrian collision probability. First, the fewer road users during the COVID-19 pandemic might skew the estimated decrease in collision probability, as a large portion of the installations occurred during the work-from-home period. Although the neighbourhood-by-date-by-road type fixed effects in the main specification control for the unobserved variation, as a robustness check, I omit all observations between March 2020 and April 2022 when Toronto's work-from-home policies were most stringent. Column (1) in Table 7 presents the estimates following the main specification using Borusyak et al. (2024)'s imputation procedure. Although the treatment effect slightly attenuates and shows a 26% decrease in pedestrian collision probability, the point estimate remains statistically similar to results from the main specification.

Next, I integrate mobile device GPS data into the main specification to control for pedes-

[25]For public transit stops, I include each bus stop, streetcar stop, and subway train entrance from all Toronto Transit Commission (TTC) routes.

Table 7: Pedestrian Collision Probability Decreases Across Robustness Checks

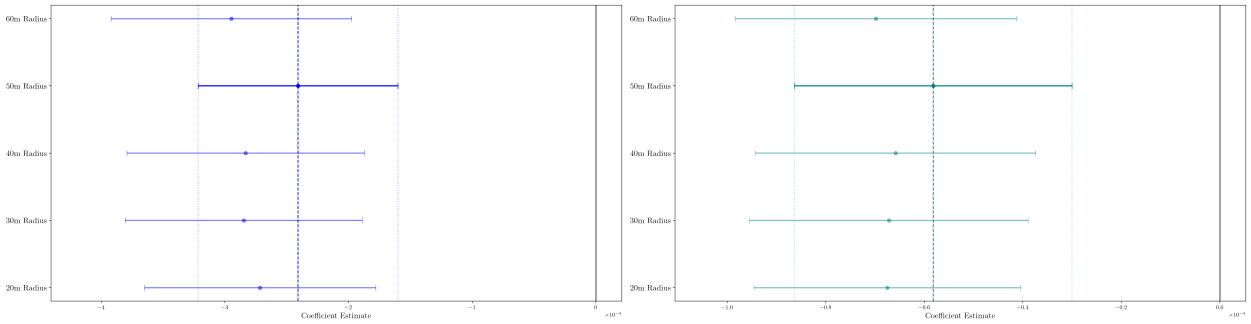
	(1) Pre-COVID	(2) Collisions Per Pedestrian	(3) Pedestrian Weights	(4) PPML w Pedestrian Weights
ATT	-0.000221***	-0.00339	-0.000281	-0.279
	(0.000)	(0.403)	(0.176)	(0.338)
% Change	-26.24%	-55.04%	-51.04%	-24.35%
Fixed Effects				
Intersection	✓	✓	✓	✓
Date	✓	✓	✓	✓
Neighbourhood × Date × Road Type	✓	✓	✓	✓
Observations	6,807,085	1,105,606	1,105,606	1,475,090

Notes: Significant at the ***[0.1%] **[1%] *[5%] level with p-values in parentheses. Standard errors are clustered at the intersection level. All results correct for time and cohort heterogeneity following Borusyak et al. (2024) and include neighbourhood-by-date-by-road type fixed effects following the main specification. Column (1) excludes observations from March 2020 through April 2022 to account for changes in collision probability during the COVID-19 pandemic. Column (2) corrects for differences in pedestrian volumes using pedestrian collisions per pedestrian mobile device ping as the dependent variable. Finally, Columns (3) and (4) weight each observation from the main specification by the number of pedestrian mobile device pings at the intersection on the given date. For Columns (2)–(4), the sample dates are restricted to May 2021–December 2022, the period when the mobile device data are available.

trian volumes and investigate the changes in pedestrian collision rates between May 2021 and December 2022, the only period for which mobile device GPS data are available in Toronto. Although the neighbourhood-by-date-by-road type fixed effects in the main specification control for the variation in traffic volumes over the sample period, the mobile device GPS data offer an explicit, though imperfect, measure of traffic volumes.[26] I modify the staggered difference-in-differences specification in Equation 1 and estimate the change in pedestrian collisions per 1,000 mobile devices classified as pedestrians. Column (2) of Table 7 reports the average treatment effect on the treated estimated using Borusyak et al. (2024)'s imputation procedure, while Columns (3) and (4) present alternative estimates where the dependent variable is the total number of pedestrian collisions, with each observation weighted by the number of unique mobile devices classified as pedestrians.[27] The distinction between Columns (3) and (4) is that Column (3) follows Borusyak et al. (2024)'s imputation procedure while Column (4) uses a PPML specification. Although the coefficients in Columns (2)–(4) indicate that pedestrian collisions decrease, the estimates are not statistically significant. The limited period for which mobile device GPS data are available, combined with the large share of zero observations in the dependent variables, limits the statistical power

[26] While an intersection-level dataset on pedestrian volumes from the City of Toronto is available, the dataset is not suited for this analysis because there are multi-year gaps between an intersection's measurements.

[27] Table A4 shows no statistically significant change in the total number of mobile devices or devices classified as pedestrians after a leading pedestrian interval is installed. Devices classified as vehicles increase by approximately 2%, a small but statistically significant change.



(a) Pedestrian Collision Probability (b) Major Pedestrian Collision Probability

Figure 8: Decreases in Collision Probability Are Robust to the Intersection Radius Definition

Notes: Figure 8 shows how the treatment effects of leading pedestrian intervals change with different intersection radius definitions for various collision types. The horizontal axis measures the estimated average treatment effect, while the vertical axis identifies the intersection definition: 20m, 30m, 40m, 50m, or 60m. The darkest horizontal line represents the 50m radius definition used in the main specification. Finally, the dashed vertical line shows the main specification's point estimate, and the solid vertical line marks 0.

of these analyses. However, the negative point estimates are consistent with the broader results from the main specification.

Another potential consideration is whether the 50m radius definition may overestimate an intersection's size and include collisions unrelated to the intersection. As a robustness check, I re-estimate the staggered difference-in-differences specifications with multiple radius definitions to determine if the treatment effects are sensitive to the intersection size definition. Figure 8 presents the average treatment effects on pedestrian collision probability and major pedestrian collision probability using alternative 20m, 30m, 40m, and 60m radius definitions alongside the baseline 50m definition. Each subfigure presents the results for a different collision type, where the dashed vertical line displays the point estimate from the baseline results. For all collision types and radius definitions, the treatment effects of leading pedestrian intervals are not statistically different from the baseline estimate. Furthermore, the signs and statistical significance of the estimates do not change with the new radius definitions. The treatment effect's stability confirms that the results are not reliant on the baseline intersection definition.

As a final robustness check, I evaluate whether the downward trend in pedestrian collision probability is a mechanical result of the changing composition of the treatment group. If the City of Toronto first installed leading pedestrian intervals at its most dangerous intersections

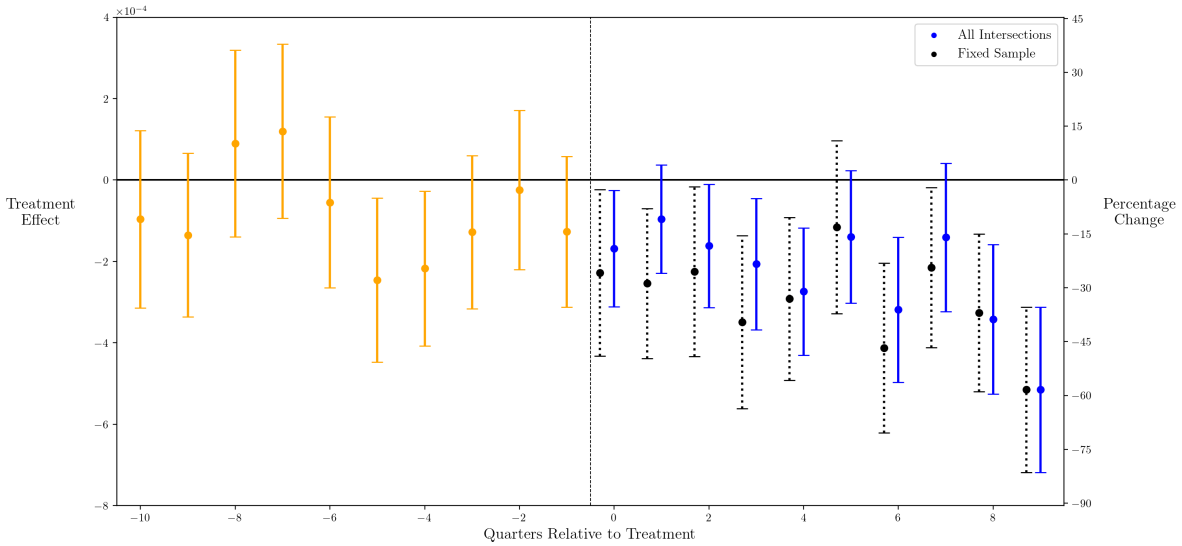


Figure 9: Pedestrian Collision Probability Decreases over Time in a Fixed Treated Sample

Notes: Figure 9 compares the event study results from the main specification to the fixed treatment sample, with the blue markers showing the identical results to Figure 4, and the black-dotted markers showing the estimated treatment effects for the first 608 treated intersections. The left vertical axis shows the coefficient estimate, while the right vertical axis shows the percentage change relative to the year prior to treatment.

to yield the highest safety benefits, the decreases in pedestrian collision probability over time may result from selection into early treatment cohorts, as intersections with a lesser decrease in pedestrian collision probability are not observed in the later post-treatment periods. To investigate the composition effects, I re-estimate Equation 2 with a fixed treatment sample that only includes intersections with at least 810 post-treatment dates.[28] Figure 9 presents the results for the fixed treatment sample alongside the event study that uses the entire treatment sample. For the fixed treatment sample, the coefficient estimates are more negative than those in the full sample in almost every post-treatment quarter, indicating that cohort composition partially explains the treatment effect dynamics. However, the coefficients for the fixed cohort also decrease over time. Since cohort composition is fixed, the downward trend must result from dynamic treatment effects rather than selection into early cohorts. Thus, the safety improvements from leading pedestrian intervals become more prominent over time.

[28] I construct the fixed treated sample using intersections observed for at least 810 post-treatment days, which are the intersections that contribute to the tenth post-treatment quarter in the main specification. Thus, the tenth-quarter estimate is identical across the two samples.

6 Discussion

6.1 Social Welfare

Leading pedestrian intervals increase social welfare through the significant decrease in pedestrian collisions. To quantify the social welfare benefits, I use both the estimated decreases in pedestrian collision probability and major pedestrian collision probability to determine how many additional pedestrian collisions would have occurred at intersections in 2022 if no leading pedestrian intervals were present. Using a back-of-the-envelope calculation, I estimate that leading pedestrian intervals prevented approximately 104 pedestrian collisions at Toronto intersections in 2022, including 21 major pedestrian collisions.[29] The decrease in major pedestrian collisions corresponds to 17 fewer injuries requiring hospitalization and 4 fewer pedestrian deaths, assuming the ratio of major injuries to fatalities for pedestrian collisions is constant post-treatment. Finally, I use Blincoe et al. (2023)'s estimates of the fraction of a statistical life lost to injury and Treasury Board of Canada Secretariat (2023)'s value of a statistical life to calculate that leading pedestrian intervals increased pedestrian social welfare by $67.6 million in 2022.[30] After incorporating the social welfare benefits from the decrease in cyclist collisions (+$11.2 million), and the social costs from the increase in vehicle collisions from the additional injuries and fatalities (-$10.3 million) and greater property damage (-$5.1 million), the net annual social welfare improvement from leading pedestrian intervals marginally attenuates to +$63.4 million.[31]

To estimate the implicit costs of leading pedestrian intervals, I use the speed reduction

[29] The 104 fewer pedestrian collisions in 2022 correspond to an 11% reduction in total pedestrian collisions across Toronto.

[30] Blincoe et al. (2023)'s analysis estimates the fraction of the value of statistical life lost to injury in terms of a collision's maximum abbreviated injury score (MAIS). I follow Wang (2023) to convert the fractional values from the MAIS scale to the KABCO scale to align the fractional values with the injury classifications in the dataset.

[31] I estimate the total property damage from vehicle collisions following Blincoe et al. (2023), who reports the average property damage per collision based on the collision's MAIS. I again convert the fractional values from the MAIS scale to the KABCO scale and use these estimates alongside the imputed estimates of the number of additional vehicle collisions because of leading pedestrian intervals to calculate the total increase in property damage. Finally, I convert the estimate to Canadian dollars based on the average 2018 conversion rate, as Blincoe et al. (2023) reports the results in 2018 U.S. dollars. As a simplification, I assume there is no property damage from cyclist and pedestrian collisions.

estimates to calculate the total value of travel time lost (VTL) caused by the prolonged commute times. I obtain public-use microdata from the 2016 Canadian Census to determine how many people in Toronto commute by vehicle, their average total commute time, and the annual income of each commuter.[32] One limitation is that the geographic identifier available in the public-use microdata defines Toronto at the Census Metropolitan Area (CMA) level, a significantly larger definition than the Toronto Census Division (CD) definition used throughout the analysis.[33] To estimate the total social cost in the Toronto CD, I take $B = 10,000$ bootstrapped samples of 632,430 vehicle commuters to match the publicly reported number in the Toronto CD by Statistics Canada. Although the bootstrapped calculation is necessary, two potential sources of bias arise from the different geographic definitions. First, income within Toronto's CD definition is higher on average compared to the CMA; the sample will underestimate the average commuter's opportunity cost of time and induce a downward bias. Second, the average commute time in each bootstrapped sample will be greater than the average commute time among Toronto CD commuters because those outside the CD are more likely to have longer commutes. This will overstate the estimated delay and bias the cost estimate upward.

Following Equation 5, I estimate the VTL as the expected total value of time lost from the increased commute time across all b bootstrapped samples. Each commuter's VTL is calculated as the product of her pre-tax hourly wage w_k, the expected delay t_k^d, the number of commutes per year ($2 \cdot 5 \cdot 52$), and the value-of-time parameter, α. Since the data on one's average total commute time is reported in 15-minute bins, I use the midpoint of each bin to impute each person's commute time and estimate t_k^d as the change in a worker's commute time after a 1.3% delay. Finally, I set $\alpha = 0.5$, then sum over all vehicle commuters to determine each sample's VTL.[34]

[32] Although the most recent Canadian Census data is from 2021, the data from the 2016 Canadian Census has two advantages. First, the 2021 Canadian Census was conducted during the COVID-19 pandemic, which may make its sample of vehicle commuters and reported commute times unrepresentative of other years. Second, the data in the 2016 Canadian Census was collected before most leading pedestrian intervals were installed; the reported commute times are unlikely to be affected by leading pedestrian intervals.

[33] In 2016, the Toronto CMA's population measured 5.9 million compared to 2.7 million for the Toronto CD.

[34] Setting $\alpha = 0.5$, so that a commuter's value of time equals 50% of her pre-tax wage, follows Small and

$$VTL = \frac{1}{B} \sum_{b \in B} \sum_{k \in b} \alpha w_k t_k^d \cdot 2 \cdot 5 \cdot 52 \tag{5}$$

If all Toronto vehicle commuters experience a 1.3% delay, I estimate that leading pedestrian intervals delay the average commuter by 113 minutes annually, with a total expected VTL of $36.2 million ($p$-value < 0.001).[35] Though substantial, the VTL estimate should be interpreted as an upper bound because leading pedestrian intervals are not on all roads and are absent on freeways. Since significant portions of a worker's commute do not incur a delay, the total time increase would be less than 1.3%; the calculation overstates the total delay. Even with the upper-bound cost estimate, the annual social welfare benefits from leading pedestrian intervals far exceed the $36.2 million cost estimate incurred by motorists, showing the welfare gains from leading pedestrian intervals.

6.2 Behavioural Responses to Traffic Safety Policies

While the net social welfare increase from the reduction in pedestrian collisions highlights the benefits of leading pedestrian intervals, the positive behavioural response from motorists offers critical insights for other traffic safety policies. Specifically, minimal safety improvements in the initial periods after treatment should not deter policymakers. With leading pedestrian intervals, the City of Toronto abandoned its pilot project after non-yielding motorist behaviour became more prevalent at the pilot intersections. However, this analysis shows that leading pedestrian intervals elicit a positive behavioural response from motorists, who improve their yielding behaviour, which generates pedestrian safety benefits that grow over time. Thus, the short-run effects of traffic safety policies may differ from the long-term social benefits as motorists adjust their driving habits. Although the finding that motorists adjust their behaviour aligns with Peltzman (1975), the positive direction of the response

Verhoef (2007) and is broadly consistent with Goldszmidt et al. (2020), who estimate the value of time at 75% of the average post-tax wage.

[35] As a robustness check, I use the upper bound of each commute time bin and perform the same calculations. Using these time bins, the total social costs increase to $43 million, remaining below the total social benefit from traffic safety.

contrasts with the notion that motorists increase risky driving and offset the intended safety benefits after traffic safety devices are installed. Despite the apparent conflict, both results indicate that a motorist's behavioural response is a crucial factor for all traffic safety mechanisms, either for the benefit or the detriment of traffic safety.

7 Conclusion

Understanding the effectiveness of new road safety initiatives and whether changes in driving habits will offset the intended improvements is critical to address the ongoing public safety issue. In this paper, I use the universe of reported pedestrian collisions in Toronto to determine how leading pedestrian intervals affect pedestrian safety and influence motorist behaviour. Using a staggered difference-in-differences design, I estimate that leading pedestrian intervals significantly reduce the pedestrian collision probability and the major pedestrian collision probability at an intersection. A mechanism analysis of motorist actions during collisions suggests that, rather than a behavioural offset, leading pedestrian intervals elicit a positive behavioural response. The pedestrian safety improvement also becomes more prominent over time, consistent with motorists learning to adhere to the new safety measure. While traffic speeds along road segments that intersect leading pedestrian intervals significantly decrease, the social cost of the delay does not outweigh the social benefits from the reduction in pedestrian fatalities and injuries. As pedestrian safety remains a worldwide public health issue, the insights from this analysis show that leading pedestrian intervals are an effective measure to prevent pedestrian collisions and protect vulnerable road users.

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Appendix

Additional Tables and Figures

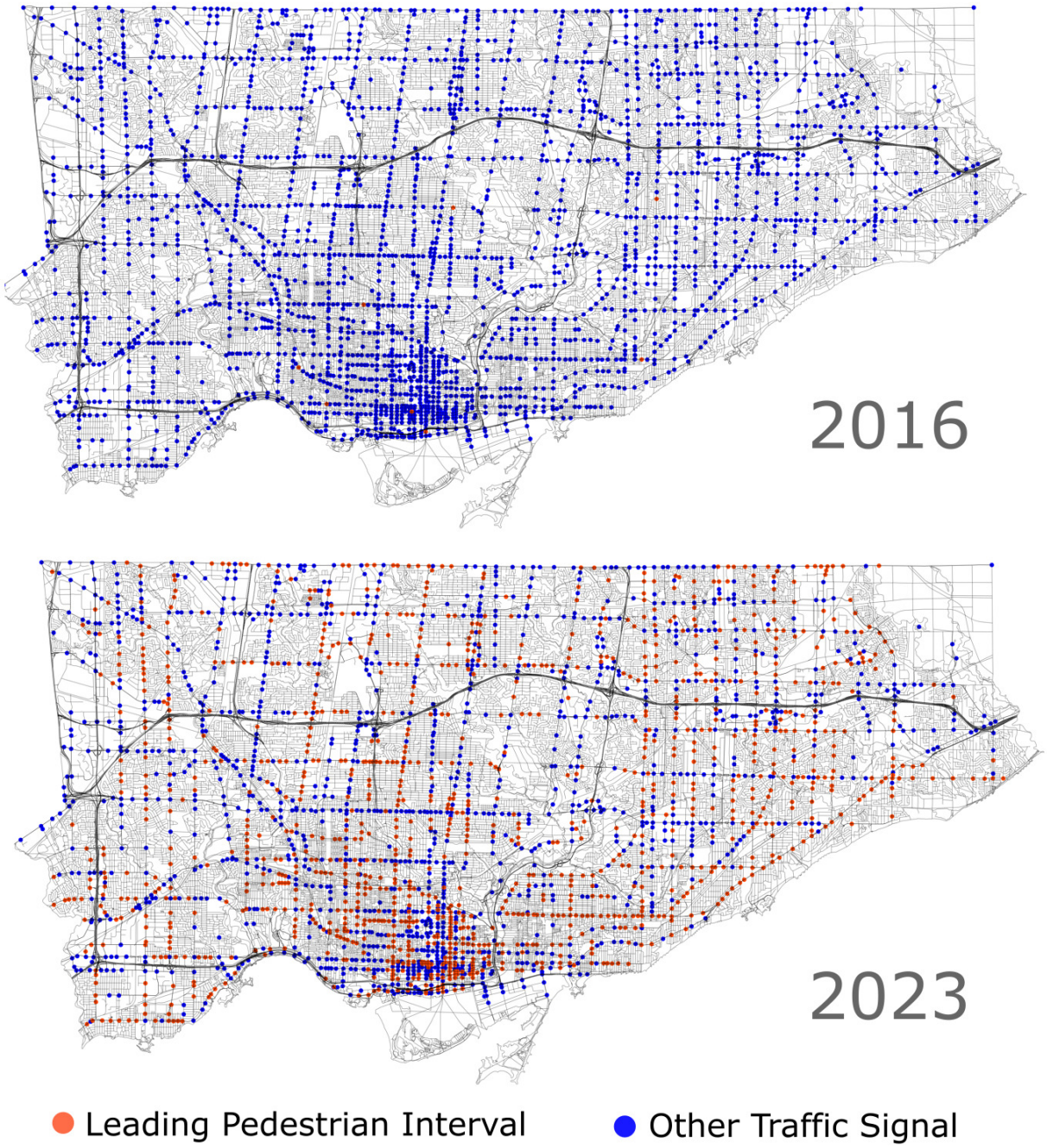


Figure A1: Leading Pedestrian Intervals Were Installed at 52% of Toronto Traffic Signals by the End of 2023

Notes: Figure A1 maps all Toronto traffic signals in 2016 and 2023. Orange markers indicate traffic signals equipped with a leading pedestrian interval in at least one traffic direction, whereas blue markers indicate all other traffic signals within the city limits.

Table A1: Current Literature on Leading Pedestrian Intervals

Article	Setting	Methodology	Treatment Effect
Van Houten et al. (2000)	St. Petersburg, Florida	Before-and-After observational study with logistic regressions over 3 intersections	–97% change in probability of vehicle-pedestrian conflict
King (2000)	New York City, New York	Before-and-After observational study over 26 intersections, comparing the change in collisions relative to control sites.	–28% change in vehicle-pedestrian collision rate from turning vehicles
City of Toronto (2005)	Toronto, Ontario	Case study over 2 Toronto intersections	–34% in non-yeilding at University-Adelaide. Significant increase in non-yielding behaviour at St. George-Harbord.
Pécheux et al. (2009)	San Francisco, California Miami, Florida	Before-after comparison of the yielding behaviour of drivers at 6 total intersections (4 in San Francisco and 2 in Miami)	9% and 18% increases in the percentage of left turners yielding during the walk sign in 2 Miami –1.4%, 1.6%, and –4.6% changes in non-yielding turners in San Francisco (the final estimate is not reported).
Fayish and Gross (2010)	State College, Pennsylvania	Before-and-After study with comparison groups over 10 treated intersections and 63 control intersections	–58.7% change in vehicle-pedestrian collisions
Osama et al. (2015)	Surrey, British Columbia	Before-and-After study of automated traffic camera footage to evaluate changes in vehicle-pedestrian conflicts at 2 intersections; one treatment and one control	–21% change in pedestrian-vehicle conflicts –29% change in conflict severity
Guo et al. (2020)	Vancouver, British Columbia	Before-and-After study of traffic camera footage using a hierarchical Bayesian peak over threshold approach to analyze pedestrian-vehicle conflicts over 2 intersections.	–18.1% - –20.9% change in severe vehicle-pedestrian conflicts
Goughnour et al. (2021)	Chicago, Illinois New York City, New York Charlotte, North Carolina	Empirical Bayes before-and-after study of 56 Chicago, 42 New York City, and 7 Charlotte intersections, using sites within 0.5 miles as controls.	–13% change in pedestrian collisions –14% change in total injury pedestrian collisions
Hasanpour and Persaud (2022)	Toronto, Ontario	Generalized Linear Model with microsimulation over 15 intersections	-53% change in severe vehicle-pedestrian conflicts -48% change in extremely severe vehicle-pedestrian conflicts
Arun et al. (2023)	Bellevue, Washington	Before-and-After evaluation of traffic camera footage using quantile regressions and Bayesian hierarchical extreme value analysis over 3 intersections	–42% change in extreme vehicle-pedestrian conflicts

Notes: Table A1 provides an overview of the current literature on leading pedestrian intervals, primarily from the transportation engineering literature. Saneinejad and Lo (2015) also provide a table that summarizes much of the current literature that investigates leading pedestrian intervals and various outcomes.

Table A2: Following-Too-Close Collisions Drive the Increase in Vehicle Collision Probability

Motorist Action	Coefficient (ATE)	Percentage Change
Following Too Close	0.00033*	41.6%*
	(0.012)	
All Other Causes	0.00009	12.1%
	(0.056)	
Failure to Yield	0.00006	10.8%
	(0.356)	
Speed Too Fast for Conditions	0.00004	24.9%
	(0.160)	
Lost Control	0.00002	3.4%
	(0.635)	
Exceeding Speed Limit	0.00001	10.5%
	(0.536)	
Wrong Way on One Way Road	0.00001	54.3%
	(0.177)	
Speed Too Slow	0.00001	15.9%
	(0.307)	
Improper Passing	0.00000	-3.4%
	(0.846)	
Improper Turn	-0.00006	-12.5%
	(0.210)	
Disobeyed Traffic Control	-0.00004	-11.3%
	(0.329)	
Improper Lane Change	-0.00001	-4.0%
	(0.919)	

Notes: Significant at the ***[0.1%], **[1%], and *[5%] levels, with p-values in parentheses. The average treatment effect on the treated reports the percentage-point change in the probability of a vehicle collision attributable to each motorist action, correcting for time and cohort heterogeneity following Borusyak et al. (2024). Percentage change expresses this estimate relative to the collision probability in the year before treatment. All estimates follow the main specification and include neighbourhood-by-date-by-road-type interacted fixed effects.

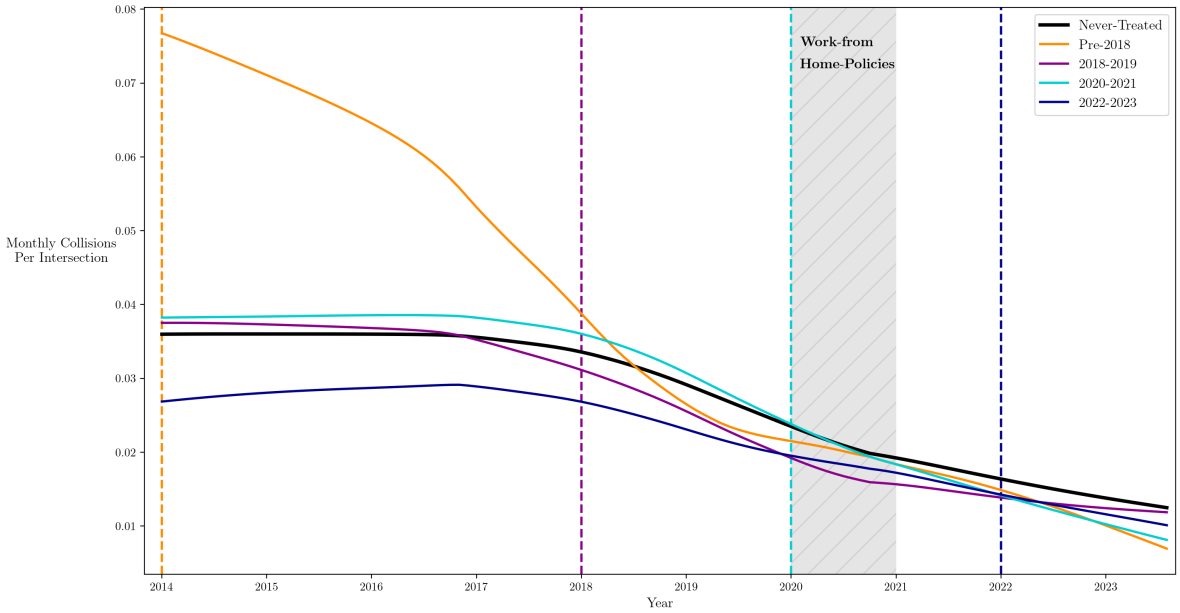
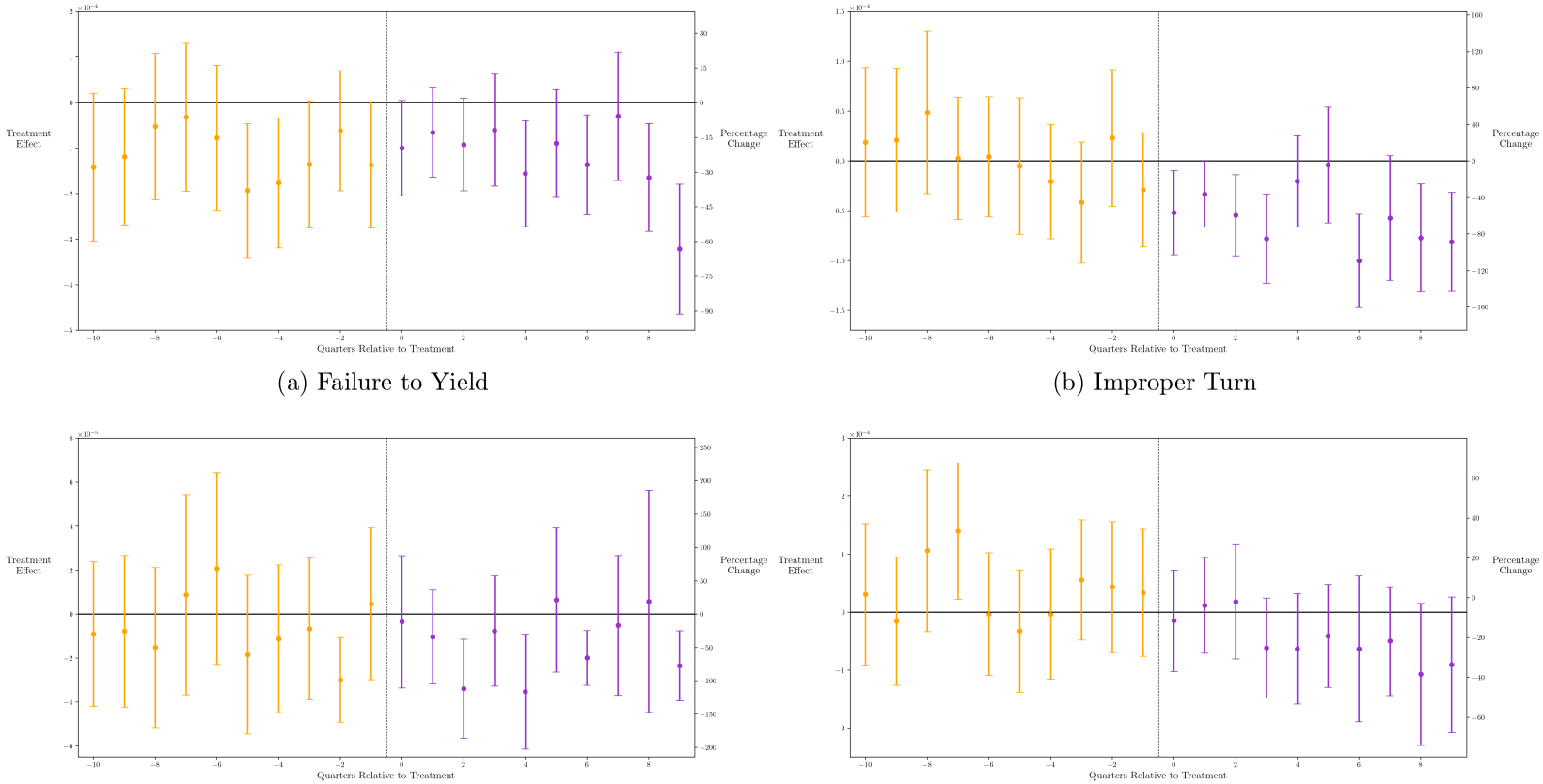


Figure A2: Pedestrian Collision Rates Do Not Rise Before Treatment Across Cohorts

Notes: Figure A2 shows the smoothed monthly pedestrian collision rate by treatment cohort. The solid trend lines show the average number of pedestrian collisions per intersection in each treatment cohort with LOWESS smoothing using a smoothing fraction of 0.6, while the dashed vertical line with a matching colour marks the year when the cohort begins to be treated, except for the pre-2018 cohort, for which the dashed vertical line marks the year 2014 when the collision data begins. The solid black line shows the average number of pedestrian collisions at intersections in the never-treated sample. Finally, the grey-shaded region broadly aligns with Toronto's work-from-home policies in response to the COVID-19 pandemic, when fewer pedestrians and vehicles were on the roads.



(a) Failure to Yield

(b) Improper Turn

(c) Disobeyed Traffic Control

(d) All Other Causes

Figure A3: Leading Pedestrian Intervals Reduce Pedestrian Collisions from Failure to Yield and Improper Turns

Notes: Figure A3 shows the event study results to investigate the dynamic causal effects of leading pedestrian intervals on the major causes of pedestrian collisions in Toronto, and presents 90-day aggregated point estimates and their 95% confidence intervals. The left vertical axis shows the coefficient estimate, while the right vertical axis shows the percentage change relative to the year before treatment.

Table A3: Safety Improvements for Vulnerable Pedestrians and During Low-Visibility Hours

	Vulnerable Pedestrians		Dusk-Dawn Collisions	
	(1) Collisions	(2) Major Collisions	(3) Collisions	(4) Major Collisions
ATT	-0.000125***	-0.00000634	-0.000127***	-0.0000306***
	(<0.001)	(0.214)	(<0.001)	(0.001)
% Change	-47.48%	-44.55%	-55.32%	-67.94%
Fixed Effects				
Intersection	✓	✓	✓	✓
Date	✓	✓	✓	✓
Neighbourhood × Date × Road Type	✓	✓	✓	✓
Observations	8,691,154	8,691,154	8,691,154	8,691,154

Notes: Significant at the ***[0.1%] **[1%] *[5%] level with p-values in parentheses. Standard errors are clustered at the intersection level. Columns (1) and (2) present the average treatment effect on the treated from leading pedestrian intervals on pedestrian collision probability and major pedestrian collision probability for vulnerable pedestrians as outlined in Toronto's Vision Zero plan: pedestrians under 19 and over 55 years of age. Columns (3) and (4) present the analogous results for collisions that occur between dusk and dawn when pedestrian visibility is poor. The average treatment effect on the treated is interpreted as the percentage point change in intersection collisions after treatment, while the percentage change expresses this estimate relative to the collision probability in the year before treatment. All results correct for time and cohort heterogeneity following Borusyak et al. (2024).

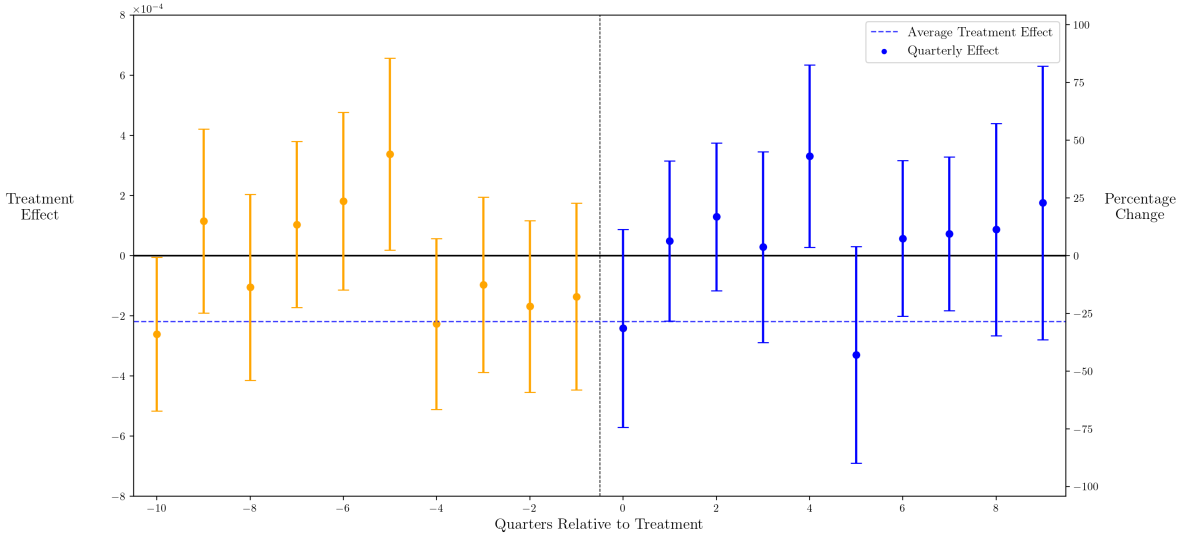


Figure A4: No Evidence of Spillovers onto Nearby Untreated Intersections

Notes: Figure A4 shows the event study results to investigate whether the safety improvements of leading pedestrian intervals spill over onto never-treated intersections in the same census tract. Although I estimate the corresponding event study daily, the figure presents the point estimates aggregated to the 90-day level to construct pseudo-quarterly treatment effects and their 95% confidence intervals. The left vertical axis shows the coefficient estimate, while the right vertical axis shows the percentage change relative to the year prior to treatment.

Table A4: Leading Pedestrian Intervals Do Not Significantly Change Pedestrian Flows

	(1) Total Cellphones	(2) Pedestrian Cellphones	(3) Vehicle Cellphones
ATT	1.298	-0.193	1.744*
	(0.150)	(0.278)	(0.020)
% Change	1.37%	-3.45%	1.98%
Fixed Effects			
Intersection	✓	✓	✓
Date	✓	✓	✓
Neighbourhood × Date × Road Type	✓	✓	✓
Observations	1,105,606	1,105,606	1,105,606

Notes: Significant at the ***[0.1%] **[1%] *[5%] level with p-values in parentheses. Standard errors are clustered at the intersection level. Column (1) presents the average treatment effect on the treated from leading pedestrian intervals on total unique mobile device pings. Column (2) presents the analogous results for mobile devices identified as potential pedestrians, while Column (3) presents the results for vehicle pings. The average treatment effect on the treated is interpreted as the total change in daily mobile device pings after treatment, while the percentage change expresses this estimate relative to the collision probability in the year before treatment. All results correct for time and cohort heterogeneity following Borusyak et al. (2024). All columns follow the main specification and include neighbourhood-by-date-by-road type fixed effects.