Did Terminating Red-Light Camera Enforcement Increase Fatal-Intersection Crashes? Evidence from U.S. Cities

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Abstract

We study whether ending red-light camera enforcement affected fatal-intersection crash outcomes. Red-light cameras enforce violations at signaled intersections, but many jurisdictions ended these programs by means of repeal or program expiration. We use Fatality Analysis Reporting System data from 2000 through 2024 and examine 13 treated U.S. cities that ended red-light camera programs between 2005 and 2014. The primary outcome is fatal-intersection crashes because the policy directly targets intersection behavior. The empirical design uses synthetic difference-in-differences, matching each treated city to donor units from untreated city-year and state-year panels. The design compares post-repeal outcomes with matched counterfactual trends over the post-repeal period. The primary estimates provide limited evidence of immediate post-repeal increases, with mixed effects across cities. The strongest evidence appears over longer windows, with delayed increases among zero pre-period cities and positive effects in Los Angeles. In the broader time horizon specification, zero-pre-period cities show an average increase of 6.23 fatal-intersection crashes per treated city-year relative to their matched synthetic doppelgängers.

Keywords: traffic enforcement; traffic safety; fatal crashes; intersection crashes; synthetic control; synthetic difference-in-differences; program repeal; roadway fatalities; urban policy; public safety; transportation economics

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1 Introduction

Red-light cameras enforce violations at intersections with traffic signals. Cities use these programs to record vehicles entering the intersection area after the traffic light has turned red making any vehicle advancement into the zone illegal. Supporters of red-light intersection surveillance programs argue that cameras reduce crashes and deaths while the critics argue that cameras impose costs and their intent is misguided. Many cities (and states) later terminated these programs because of court rulings, voter opposition, council votes, costs, and revenue disputes. Some voters opposed cameras because they viewed them as revenue tools rather than safety programs for vehicle operators and pedestrians. Moreover, there were concerns about the fairness and equity insofar the tickets went to vehicle owners rather than proven drivers.

The staggered termination (via repeal and expiration) of these programs created variation for a strategic policy evaluation. We study whether repeals changed fatal-intersection crash outcomes. Our analysis focuses on United States cities. We also study New Jersey's 2014 program expiration. This specific setting links enforcement removal to roadway safety. We use Fatality Analysis Reporting System data (National Highway Traffic Safety Administration, 2024). These data cover fatal motor-vehicle crashes. We build a city-year panel from 2000 through 2024. Outcomes include fatal crashes, fatalities, and fatal-intersection crashes. We focus on fatal-intersection crashes. Cameras target violations at intersections with traffic signals. This outcome matches the policy mechanism. The city analysis includes eleven repeal cases. These include Houston, Los Angeles, and San Diego. They also include Baltimore, Kansas City, and Oakland. Additional cases include Lubbock, Minneapolis, Charlotte, Greensboro, and Fresno. The New Jersey analysis uses a state-year panel. New Jersey ended its camera pilot in 2014. We supplement national data with state crash sources. California Crash Reporting System files support California cases (California State Transportation Agency, 2024). Maryland crash files support the Baltimore case (Maryland Open Data, 2024). Texas Crash Records Information System data support Texas cases (Texas Department of Transportation, 2024). New Jersey Department of Transportation data support New Jersey (New Jersey Department of Transportation, 2026). Missouri Statewide Traffic Accident Records System data support Kansas City (Missouri State Highway Patrol, 2024). These sources help assess crashes beyond fatal outcomes.

Our empirical strategy uses a synthetic difference-in-differences design. Each treated unit receives a donor pool. Donors come from untreated city-year panels. The New Jersey case uses state-year donors. The design matches crash levels before repeal. It also matches crash trends before repeal. We then compare outcomes after repeal. The main window covers five post-repeal years. This design addresses two concerns. Treated cities may differ from other cities as crash outcomes may also follow different trends. Synthetic comparison units fixes this as our donor weights reproduce pre-repeal outcomes while the post-repeal gaps estimate treatment effects. Our research project contributes to evidence on automated enforcement and using repeals/expiry tests whether safety effects persist over time. They also reveal outcomes after enforcement ends. The results inform debates over traffic enforcement, pedestrian safety and intersection safety policy.

2 Literature Review

Red-light camera research studies enforcement and outcomes at signalized intersections. This literature examines violations, crashes, injuries, and fatalities. Early studies such as Retting et al. (2003) find fewer red-light running violations after installation. They also find changes in crash composition (Retting et al., 2003). Interestingly, right-angle crashes often decline after camera installation while rear-end crashes often increase after camera installation. This tradeoff is central to the literature. Scrutiny of the federal level evidence reaches similar conclusions. The Federal Highway Administration finds fewer right-angle crashes and finds more rear-end crashes (Council et al., 2005). The safety implications depend on crash severity. Right-angle crashes often cause more severe injuries. Rear-end crashes often cause less severe injuries. This distinction motivates injury v. fatality outcomes which have cost implications. Systematic reviews also find mixed crash effects. Aeron-Thomas et al. (2005) finds a reduction in casualty crashes (Aeron-Thomas and Hess, 2005). Cohn et al. (2020) finds evidence of reductions in injury crashes and reports fewer right-angle injury crashes (Cohn et al., 2020) but also reported more rear-end crashes. These results suggest enforcement changes crash types. Several studies examine citywide fatal crash outcomes. Insurance Institute for Highway Safety research studies large cities and it finds lower fatal red-light-running crash rates (Hu et al., 2011). It also finds lower signalized-intersection fatal crash rates. These studies buttress the idea of broader deterrence effects as drivers may change behavior beyond cam-

era intersections. To the contrary some studies emphasize program design and focus on factors such as yellow-light timing and enforcement rules. Revenue concerns also affect public acceptance. Critics argue programs may prioritize citations over actual safety. These concerns have shaped the arguments of the proponents on the repeal side of the debate. Most evidence studies adoption or active programs while less evidence studies what happens after repeal which is the angle of our current research. While adoption studies may capture implementation and site selection. Repeal studies capture outcomes after enforcement removal and test whether safety gains persist.

Our paper contributes to the literature in three ways. First, we study repeal effects rather than adoption effects. Second, we focus on fatal-intersection crashes, which match the policy mechanism. Third, we compare multiple repeal cases across jurisdictions. We also show whether repeal effects vary across jurisdictions or remain relatively constant. Our Difference-in-differencesd design compares treated and untreated units over time. In our specified synthetic control method we construct weighted comparison units (via donors of untreated units). Synthetic difference-in-differences combines these approaches (Arkhangelsky et al., 2021). This method helps match pre-repeal crash patterns while also using untreated units as counterfactual cases for comparison.

3 Data

Our analysis employs the Fatality Analysis Reporting System (FARS). This reporting system is administered and maintained by the National Highway Traffic Safety Administration (NHTSA) and tracks and records fatal motor-vehicle crashes in the United States. We aggregate crash records to city-year units and tabulate our outcome variable: fatal-intersection crashes. Secondary outcomes are fatal crashes and fatalities. Our sample includes cities that terminated red-light camera surveillance programs. Cities which repealed or failed to renew (allowed expiration) their red-light surveillance programs include: Baltimore, MD; Charlotte, NC; Fresno, CA; Greensboro, NC; Houston, TX; Kansas City, MO; Los Angeles, CA; Lubbock, TX; Minneapolis, MN; Oakland, CA; San Diego, CA; Newark, NJ; and Jersey City, NJ. Newark and Jersey City are used as urban areas within a statewide New Jersey repeal. We supplement crash outcomes with pre-repeal matching characteristics. These include county population, land area, population density, crash rates, crash levels, and pre-repeal crash trends. Population and land area are measured at the county level.

Crash rates are measured per 100,000 residents. We also report urbanized-area vehicle miles traveled as an exposure proxy. This measure comes from the Federal Highway Administration Highway Performance Monitoring System (FHAHPMS). It is measured at the adjusted urbanized-area level, not through municipal boundaries.

3.1 Summary Statistics

Table 1 presents some descriptive statistics of our collected data. The pre-period covers five years before repeal. The post-period covers six years after repeal. Greensboro, Charlotte, Fresno, Minneapolis, and Lubbock have zero treated pre-period fatal-intersection crashes in every pre-repeal year. Newark and Jersey City utilize the statewide New Jersey case in the city-level specification. Donor means use eligible untreated city units from each case-specific donor pool. The zero pre-period values reflect the underlying Fatality Analysis Reporting System panel. Greensboro, Charlotte, Fresno, Minneapolis, and Lubbock record no treated fatal-intersection crashes in the five pre-repeal years.

Table 1: Fatal-Intersection Crashes by Repeal Case

Case	Repeal/expiry	Treated pre	Treated post	Donor pre	Donor post
Greensboro, NC	2005	0.00	4.00	0.00	1.49
Charlotte, NC	2006	0.00	16.33	0.00	5.34
Fresno, CA	2006	0.00	10.33	0.00	4.83
Minneapolis, MN	2006	0.00	5.67	0.00	2.26
Lubbock, TX	2008	0.00	14.00	0.00	3.71
Houston, TX	2011	40.00	199.00	4.21	22.98
Los Angeles, CA	2012	86.00	252.17	11.29	20.89
Baltimore, MD	2013	17.20	33.67	10.53	17.95
Kansas City, MO	2013	27.60	52.00	10.93	19.72
San Diego, CA	2013	39.60	83.00	12.36	23.20
Newark, NJ	2014	19.40	21.83	6.94	9.75
Jersey City, NJ	2014	7.20	8.83	6.94	9.75
Oakland, CA	2014	20.20	28.00	11.77	16.30

Notes: Values are changes in annual mean fatal-intersection crashes.

Table 2 reports the case structure. Each case uses five pre-repeal years and six post-repeal years. Most city cases use 500 donor units. Newark and Jersey City use 2,418 eligible donor units

after excluding New Jersey cities. For Newark and Jersey City, the donor pool excludes other New Jersey cities to avoid contamination from the statewide program expiration. Contamination occurs when comparison cities are affected by the same policy change as the treated city. Because New Jersey's red-light camera program expired statewide in 2014, other New Jersey cities may also experience enforcement changes after the same date. Including those cities as donors would make the comparison group partly treated and could attenuate (bias estimated effects towards zero) the estimated repeal effect.

Table 2: Case Structure and Donor Pools

Case	Repeal/expiry year	Pre years	Post years	Donors
Greensboro, NC	2005	5	6	500
Charlotte, NC	2006	5	6	500
Fresno, CA	2006	5	6	500
Minneapolis, MN	2006	5	6	500
Lubbock, TX	2008	5	6	500
Houston, TX	2011	5	6	500
Los Angeles, CA	2012	5	6	500
Baltimore, MD	2013	5	6	500
Kansas City, MO	2013	5	6	500
San Diego, CA	2013	5	6	500
Newark, NJ	2014	5	6	500
Jersey City, NJ	2014	5	6	500
Oakland, CA	2014	5	6	500

Notes: Donors are selected from eligible untreated units.

Table 3 reports pre/post changes for fatal-intersection crashes. The treated change is compared with the donor change. And Table 4 presents pre-repeal matching characteristics used to screen donor units. These characteristics include population, land area, density, fatal crash rates, fatal-intersection crash rates, baseline crash levels, and pre-repeal crash trends. Population is county population. Land area is measured in square miles and density is county residents per square mile. Crash rates are measured as annual crashes per 100,000 residents. Crash levels are measured as annual pre-repeal means. Our donor pool is restricted using these pre-repeal characteristics before estimation. This reduces comparisons between treated cities and donor units with dissimilar baseline conditions. Synthetic difference-in-differences then weights the remaining donors using outcome histories. This combines covariate screening with outcome-based matching. The treated

change equals the treated post-repeal mean minus the treated pre-repeal mean. In Table 4 the donor change equals the donor post-repeal mean minus the donor pre-repeal mean. The difference equals the treated change minus the donor change.

Table 3: Pre/Post Changes in Fatal-Intersection Crashes

Case	Treated change	Donor change	Difference
Greensboro, NC	4.00	1.49	2.51
Charlotte, NC	16.33	5.34	10.99
Fresno, CA	10.33	4.83	5.50
Minneapolis, MN	5.67	2.26	3.40
Lubbock, TX	14.00	3.71	10.29
Houston, TX	159.00	18.76	140.24
Los Angeles, CA	166.17	9.60	156.56
Baltimore, MD	16.47	7.42	9.05
Kansas City, MO	24.40	8.78	15.62
San Diego, CA	43.40	10.85	32.55
Newark, NJ	2.43	2.81	-0.38
Jersey City, NJ	1.63	2.81	-1.18
Oakland, CA	7.80	4.53	3.27

Notes: Values are changes in annual mean fatal-intersection crashes.

Table 4: Pre-Repeal Matching Characteristics

Case	Population	Land area	Density	Fatal crash rate	Intersection rate	Fatal crashes	Intersection crashes	Crash trend	Intersection trend
Greensboro, NC	431,939	646.0	668.7	5.05	0.00	21.80	0.00	-2.200	0.000
Charlotte, NC	756,447	523.6	1,444.7	6.87	0.00	52.00	0.00	3.700	0.000
Fresno, CA	844,696	5,958.4	141.8	5.28	0.00	44.60	0.00	0.700	0.000
Minneapolis, MN	1,119,867	554.0	2,021.4	1.68	0.00	18.80	0.00	-0.500	0.000
Lubbock, TX	258,762	895.6	288.9	7.19	0.00	18.60	0.00	-3.300	0.000
Houston, TX	3,950,353	1,707.0	2,314.2	5.14	1.01	203.20	40.00	-3.300	40.000
Los Angeles, CA	9,783,651	4,060.2	2,409.6	2.42	0.88	236.80	86.00	-14.800	63.800
Baltimore, MD	621,033	80.9	7,672.2	5.51	2.77	34.20	17.20	-5.600	7.500
Kansas City, MO	672,852	604.5	1,113.0	6.75	4.10	45.40	27.60	0.600	14.000
San Diego, CA	3,099,793	4,210.5	736.2	2.26	1.28	70.20	39.60	-3.200	20.100
Newark, NJ	784,663	126.1	6,223.4	2.88	2.47	22.60	19.40	1.000	4.200
Jersey City, NJ	643,870	46.2	13,938.5	1.34	1.12	8.60	7.20	-1.200	0.200
Oakland, CA	1,535,133	737.5	2,081.7	1.58	1.32	24.20	20.20	1.900	5.900

Notes: Newark and Jersey City employ the statewide New Jersey case in the city-level specification. Their county covariates are based on Essex County and Hudson County.

Table 5 presents additional pre-repeal matching characteristics. Baseline fatalities are treated-unit annual pre-repeal means from the Fatality Analysis Reporting System. Region follows Census region groupings. Urban status is coded using the county-level setting for each treated city.

Table 5: Additional Matching Characteristics

Case	State	Region	Baseline fatalities	Urban status
Greensboro, NC	North Carolina	South	23.00	Urban county
Charlotte, NC	North Carolina	South	55.00	Urban county
Fresno, CA	California	West	47.40	Urban county
Minneapolis, MN	Minnesota	Midwest	19.80	Urban county
Lubbock, TX	Texas	South	19.40	Urban county
Houston, TX	Texas	South	218.20	Urban county
Los Angeles, CA	California	West	251.00	Urban county
Baltimore, MD	Maryland	South	35.80	Urban county
Kansas City, MO	Missouri	Midwest	49.40	Urban county
San Diego, CA	California	West	73.80	Urban county
Newark, NJ	New Jersey	Northeast	23.40	Urban county
Jersey City, NJ	New Jersey	Northeast	9.00	Urban county
Oakland, CA	California	West	26.60	Urban county

Notes: Baseline fatalities are treated-unit annual pre-repeal means from the Fatality Analysis Reporting System (FARS).

To more accurately construct synthetic donor units we also add an urbanized-area VMT exposure measure. Table 6 presents these statistics. Treated cases are matched to adjusted urbanized areas using Census urban-area codes. Daily VMT is converted to annual VMT by multiplying by 365. This measure captures local-road VMT within the adjusted urbanized area. It is an exposure proxy and not a municipal-bound VMT measure.

Table 6: Urbanized-Area Vehicle Miles Traveled Exposure

Case	Daily VMT, millions	Annual VMT, billions	Daily VMT per capita
Greensboro, NC	1.6	0.6	4.23
Charlotte, NC	11.8	4.3	7.83
Fresno, CA	0.9	0.3	1.22
Minneapolis, MN	8.5	3.1	0.00
Lubbock, TX	0.7	0.3	2.99
Houston, TX	8.5	3.1	1.71
Los Angeles, CA	10.2	3.7	0.84
Baltimore, MD	4.1	1.5	1.79
Kansas City, MO	8.4	3.1	0.01
San Diego, CA	2.5	0.9	0.78
Newark, NJ	37.9	13.8	0.00
Jersey City, NJ	37.9	13.8	0.00
Oakland, CA	2.5	0.9	0.74

Notes: Vehicle miles traveled (VMT) is sourced from the Federal Highway Administration Highway Performance Monitoring System.

4 Model

We estimate repeal effects using city-level panel models. Y_{it} denotes an outcome for city i in year t. Outcomes include fatal-intersection crashes, fatal crashes, and fatalities. We allow Treat_i to equal one for cities that repealed red-light camera programs. Post_{it} equal one for years after repeal. The baseline difference-in-differences model is

$$Y_{it} = \alpha_i + \lambda_t + \beta(\text{Treat}_i \times \text{Post}_{it}) + \varepsilon_{it}. \tag{1}$$

In our specification; α_i are city fixed effects which take into account time-invariant city differences. Year fixed effects, are denoted as λ_t. Our coefficient β estimates the post-repeal change in treated cities relative to donor cities. Positive values indicate higher fatal crash outcomes after repeal. Negative values indicate lower fatal crash outcomes after repeal. We also estimate synthetic difference-in-differences models. For each treated city, we construct a donor pool from untreated city-year units. Donor pools are screened using pre-repeal characteristics. These include population, density, crash rates, crash levels, and crash trends. Donor pools are capped at 500 screened units for comparability. The estimator then assigns weights to donor cities and pre-repeal

years. These weights improve pre-repeal fit before estimating post-repeal gaps. The synthetic difference-in-differences estimand is calculated as:

$$\hat{\tau}^{SDID} = \sum_{i=1}^{N}\sum_{t=1}^{T}\left(\hat{\omega}_i^{tr} - \hat{\omega}_i^{co}\right)\left(\hat{\lambda}_t^{post} - \hat{\lambda}_t^{pre}\right)Y_{it}, \tag{2}$$

where $\hat{\omega}_i^{tr}$ and $\hat{\omega}_i^{co}$ are treated and control unit weights. The terms $\hat{\lambda}_t^{post}$ and $\hat{\lambda}_t^{pre}$ are post-period and pre-period time weights. This estimator compares treated cities to weighted donor cities. It also compares post-repeal years to weighted pre-repeal years.

4.1 Two-way fixed effects difference-in-differences model

We report three additional models for as robustness checks. First, we estimate a two-way fixed effects (TWFE) difference-in-differences model. This model compares treated cities before and after repeal against donor cities before and after the same years, using city and year fixed effects. Relative to our benchmark synthetic difference-in-differences model which differs by weighting donor cities and time periods to improve pre-repeal fit the the fixed-effects model treats donors more uniformly after controls. This fixed-effects model is useful for transparency. We specify this model as:

$$Y_{it} = \alpha_i + \lambda_t + \beta(\text{Treat}_i \times \text{Post}_{it}) + \varepsilon_{it}, \tag{3}$$

where Y_{it} is the crash outcome for city i in year t. The term Treat_i equals one for treated cities. The term Post_{it} equals one for years after repeal. City fixed effects, α_i, account for time-invariant city differences. Year fixed effects are denoted as λ_t. The coefficient β measures the post-repeal change in treated cities relative to the synthetic doppelgangers.

4.2 Event-study difference-in-differences model

Next we estimate an event-study difference-in-differences model. This model examines whether treated cities display differential trends before repeal and calculates how crash outcomes evolve after repeal. The specification is

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k\neq -1}\beta_k \mathbf{1}\{t - T_i = k\} + \varepsilon_{it}, \tag{4}$$

where T_i is the repeal year for treated city i. The coefficient β_k measures the difference between treated and donor cities k years from repeal. The omitted period is $k = -1$, the year before repeal. Pre-repeal coefficients provide a check on differential pre-trends. Post-repeal coefficients summarize the timing of changes after repeal.

4.3 Poisson fixed effects model

We then estimate a Poisson fixed effects model. Crash outcomes are nonnegative counts, so a count-data specification is a useful robustness check. This specification is set up as:

$$E[Y_{it} \mid \alpha_i, \lambda_t] = \exp\left(\alpha_i + \lambda_t + \beta(\text{Treat}_i \times \text{Post}_{it})\right). \tag{5}$$

This specification keeps the same treatment definition as our baseline model. This allows the conditional mean to be nonlinear and avoids predicting negative crash counts. The coefficient β is interpreted as a proportional change in the expected crash count after repeal. Each robustness model retain the full treated-city sample.

4.4 Matched Difference-in-Differences for Isolated Zero Pre-Period Cases

For treated cities with zero pre-period fatality at intersection crashes in every pre-repeal year, we construct a zero-pre donor comparison. The donor pool is restricted to untreated city units that also record zero fatal-intersection crashes throughout the same pre-period. This makes treated and donor pre-period means equal to zero by construction. The resulting estimate compares post-repeal fatal-intersection crashes in the treated city with post-repeal fatal-intersection crashes among zero-pre donor units. We report this as a matched difference-in-differences robustness check rather than a synthetic difference-in-differences estimate.

$$\hat{\tau}^0 = \bar{Y}^T_{post} - \bar{Y}^{D0}_{post}, \tag{6}$$

where $\bar{Y}^T_{post}$ is the treated city's post-repeal mean and $\bar{Y}^{D0}_{post}$ is the post-repeal mean among zero-pre donor units. This estimate is reported as a robustness check. It is not a synthetic difference-in-differences estimate because all matched donors have identical pre-period fatal-intersection outcomes.

5 Results

We begin with pooled case-level estimates. These estimates average across repeal cases rather than city-year observations. This specification uses a five-year pre-repeal window and a six-year post-repeal window for each treated city. The pre-period ends in the year before repeal. The post-period begins in the repeal year and runs for six years. We employ a covariate-weighted zero pre-period fatality-intersection case difference-in-differences for zero pre-period fatality-intersection cases and synthetic difference-in-differences for the remaining cases. The Simple DID column reports the corresponding pooled benchmark. Table 7 shows a positive pooled effect. Across all repeal cases our estimate is 2.33 fatal-intersection crashes (p-value of 0.057) in that cities that had terminated or allowed their red-light programs to expire had about 2.33 more fatal-intersection on average crashes per year relative to their matched or synthetic comparison cities across repeal cases. The pooled effect is concentrated in the zero pre-period fatality-intersection cases. These cases report an estimate of 6.23 (p-value below 0.01). The simple DID estimates are much larger. The all-case Simple DID estimate is 29.78, and the SDID-case estimate is 44.30. This distinction suggests that post-repeal increases are large, but a large proportion of that magnitude is not unique to treated cities after applying our synthetic weighting based on out characteristic variables.

Table 7: Case-Level Pooled Main Estimates, Five Pre-Years and Six Post-Years

Sample	Cases	Main estimate	p-value	Simple DiD	Simple DiD p-value
All repeal cases	13	2.33*	0.057	29.78***	0.000
Zero-pre cases	5	6.23***	0.000	6.54***	0.000
SDID cases	8	-0.10	0.962	44.30***	0.000

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

We also estimate a shorter post-period specification using five pre-repeal years and three post-repeal years. This specification keeps the comparison closer to the repeal event and reduces the possible influence of later changes in traffic conditions, enforcement, roadway design, or local crash trends. The six-year window captures more persistent effects, while the three-year window focuses on the shorter-run effects after the termination/expiry. Over six years (post-repeal), other factors can change: enforcement, awareness, traffic volume and patterns, road design, local economic growth, or broader crash trends. A three-year post window limits the influence of these possible

other factors. In these results the zero pre-period fatality cases remain positive in the shorter window. Their pooled estimate is 1.01 (p-value of 0.012). The SDID cases again show no pooled increase. The Simple DID estimates remain large in the shorter window. The all-case Simple DID estimate is 24.57, and the SDID-case estimate is 39.24. Each is statistically significant.

Table 8: Case-Level Pooled Main Estimates, Five Pre-Years and Three Post-Years

Sample	Cases	Main estimate	p-value	Simple DiD	Simple DiD p-value
All repeal cases	13	0.15	0.898	24.57***	0.000
Zero-pre cases	5	1.01**	0.012	1.10**	0.012
SDID cases	8	-0.38	0.828	39.24***	0.000

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

Lastly, we estimate a shorter post-period specification using five pre-repeal years and one post-repeal year. The one-year post-period specification shows a positive short-run effect among the synthetic difference-in-differences cases. The all-case pooled main estimate is 3.53 fatal-intersection crashes, with a p-value of 0.009. This effect is driven entirely by the SDID cases, where the pooled estimate is 5.73 and the p-value is 0.008. The zero pre-period fatality-intersection cases show no one-year post-repeal effect. Their pooled estimate is 0.00 because these cities record no fatal-intersection crashes in the repeal year. This result was omitted from the table.

Table 9: Case-Level Pooled Main Estimates, Five Pre-Years and One Post-Year

Sample	Cases	Main estimate	p-value	Simple DiD	Simple DiD p-value
All repeal cases	13	3.53***	0.009	24.91***	0.000
SDID cases	8	5.73***	0.008	40.48***	0.000

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

The one-year window reports a positive pooled effect. It is driven by SDID cases, especially Los Angeles. The zero-pre cities record no fatal-intersection crashes in the repeal year. The three-year window shows no all-case pooled effect. The zero cases pre-period group becomes positive, but the estimate remains small. The SDID group remains near zero. The six-year window shows a modest all-case effect. That effect is driven by the zero pre-period cities. Greensboro, Charlotte, Fresno, Minneapolis, and Lubbock show delayed increases by the six-year window. Los Angeles shows the clearest immediate increase. Houston turns negative under the weighted SDID model. Baltimore,

Kansas City, Oakland, Newark, and Jersey City show little consistent positive evidence.

Table 10: City-Level Main Estimates by Post-Period Window

Case	Repeal year	1-year estimate	p-value	3-year estimate	p-value	6-year estimate	p-value
Greensboro, NC	2005	N/A	N/A	N/A	N/A	2.41**	0.010
Charlotte, NC	2006	N/A	N/A	N/A	N/A	10.80**	0.010
Fresno, CA	2006	N/A	N/A	N/A	N/A	4.88*	0.091
Minneapolis, MN	2006	N/A	N/A	N/A	N/A	3.44**	0.010
Lubbock, TX	2008	N/A	N/A	5.03**	0.011	9.62**	0.010
Houston, TX	2011	8.08	0.140	-17.92**	0.020	-18.27***	0.000
Los Angeles, CA	2012	32.13***	0.000	14.64*	0.057	18.64*	0.069
Baltimore, MD	2013	-0.37	0.970	2.59	0.510	2.78	0.530
Kansas City, MO	2013	-2.65	0.590	-5.56	0.150	-0.73	0.840
San Diego, CA	2013	9.64	0.120	8.80	0.100	3.87	0.420
Oakland, CA	2014	7.12	0.140	0.86	0.800	-1.04	0.730
Newark, NJ	2014	-8.90*	0.090	-5.51	0.200	-5.88	0.150
Jersey City, NJ	2014	0.82	0.760	-0.97	0.770	-0.16	0.900

Notes: All specifications use five pre-repeal years. N/A indicates zero treated fatal-intersection crashes in the post-period for zero-pre cases. ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

5.1 TWFE Difference-in-Differences Results

Table 11 reports pooled two-way fixed effects difference-in-differences estimates. The fatal-intersection estimate is positive and statistically significant at the five-percent level. The estimate reports an increase of 28.574 annual fatal-intersection crashes after repeal relative to donor cities. The fatal-crash and fatality estimates are also positive. These estimates are statistically significant at the ten-percent level. The pooled results support the main fatal-intersection findings. The largest and clearest effect appears for fatal-intersection crashes, which is the outcome most directly linked to red-light camera enforcement. The estimates for broader fatal crashes and fatalities are smaller. This pattern is consistent with a policy mechanism concentrated at signalized intersections rather than across all fatal crashes.

Table 11: Two-Way Fixed Effects Difference-in-Differences Estimates

Outcome	Estimate	Std. error	p-value	Observations
Fatal-intersection crashes	28.574**	13.631	0.036	61,327
Fatal crashes	3.046*	1.789	0.089	61,327
Fatalities	3.320*	1.866	0.075	61,327

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

Tables 12, 13 and 14 report city-specific two-way fixed effects estimates for fatal-intersection crashes conditional on post-years. The two-way fixed effects (TWFE) model compares treated and donor units before and after repeal, while accounting for unit and year fixed effects. The TWFE estimates are very large for Houston, Los Angeles, and San Diego. But the main weighted estimates are much smaller, and Houston turns negative under the previous synthetic difference-in-differences. TWFE model may not be isolating the repeal effect as well as our main specification. In the six-year window, TWFE and the main estimates are close for Greensboro, Charlotte, Fresno, Minneapolis, and Lubbock. In the one-year and three-year windows, several zero pre-period fatality-intersection cases have no treated post-period fatal-intersection crashes.

Table 12: City-Level Estimates, Five Pre-Years and Six Post-Years

Case	Repeal year	Main estimate	Main p-value	TWFE estimate	TWFE p-value
Greensboro, NC	2005	2.41**	0.010	2.39**	0.043
Charlotte, NC	2006	10.80**	0.010	10.57***	0.002
Fresno, CA	2006	4.88*	0.091	5.49**	0.033
Minneapolis, MN	2006	3.44**	0.010	3.37**	0.020
Lubbock, TX	2008	9.62**	0.010	9.28***	0.000
Houston, TX	2011	-18.27***	0.000	140.19***	0.000
Los Angeles, CA	2012	18.64*	0.069	144.85***	0.000
Baltimore, MD	2013	2.78	0.530	7.43*	0.061
Kansas City, MO	2013	-0.73	0.840	15.63***	0.000
San Diego, CA	2013	3.87	0.420	32.31***	0.000
Oakland, CA	2014	-1.04	0.730	3.12	0.337
Newark, NJ	2014	-5.88	0.150	-0.38	0.910
Jersey City, NJ	2014	-0.16	0.900	-1.18	0.727

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

Table 13: City-Level Estimates, Five Pre-Years and Three Post-Years

Case	Repeal year	Main estimate	Main p-value	TWFE estimate	TWFE p-value
Greensboro, NC	2005	N/A	N/A	0.00	N/A
Charlotte, NC	2006	N/A	N/A	0.00	N/A
Fresno, CA	2006	N/A	N/A	0.00	N/A
Minneapolis, MN	2006	N/A	N/A	0.00	N/A
Lubbock, TX	2008	5.03**	0.011	5.06***	0.008
Houston, TX	2011	-17.92**	0.020	126.16***	0.000
Los Angeles, CA	2012	14.64*	0.057	127.03***	0.000
Baltimore, MD	2013	2.59	0.510	6.77	0.141
Kansas City, MO	2013	-5.56	0.150	7.60	0.129
San Diego, CA	2013	8.80	0.100	32.97***	0.000
Oakland, CA	2014	0.86	0.800	4.35	0.250
Newark, NJ	2014	-5.51	0.200	-0.96	0.822
Jersey City, NJ	2014	-0.97	0.770	-1.76	0.679

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

Table 14: City-Level Estimates, Five Pre-Years and One Post-Year

Case	Repeal year	Main estimate	Main p-value	TWFE estimate	TWFE p-value
Greensboro, NC	2005	N/A	N/A	0.00	N/A
Charlotte, NC	2006	N/A	N/A	0.00	N/A
Fresno, CA	2006	N/A	N/A	0.00	N/A
Minneapolis, MN	2006	N/A	N/A	0.00	N/A
Lubbock, TX	2008	N/A	N/A	0.00	N/A
Houston, TX	2011	8.08	0.140	136.90***	0.000
Los Angeles, CA	2012	32.13***	0.000	128.70***	0.000
Baltimore, MD	2013	-0.37	0.970	2.89	0.669
Kansas City, MO	2013	-2.65	0.590	8.66	0.250
San Diego, CA	2013	9.64	0.120	31.94***	0.005
Oakland, CA	2014	7.12	0.140	9.14*	0.090
Newark, NJ	2014	-8.90*	0.090	-6.23	0.341
Jersey City, NJ	2014	0.82	0.760	-0.03	0.997

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

5.2 Event-Study by City Results

Table 15 reports event-study difference-in-differences estimates by repeal case. Event year -1 is the omitted reference year. Each coefficient measures the treated city's change in fatal-intersection crashes relative to the year before repeal and relative to the donor comparison. The event-study

results show evidence of immediate post-repeal increases. The zero pre-period fatality-intersection cases show larger effects later in the post-period. Greensboro shows a positive and statistically significant estimate in event year 5. Charlotte shows positive estimates in event years 4 and 5. Fresno and Minneapolis also show positive estimates in event year 5, while Lubbock shows positive estimates beginning in event year 2. These results suggest that increases among zero pre-period fatality-intersection cases emerge with a delay rather than immediately after repeal. The non-zero pre-period fatality-intersection cases show more mixed patterns. Los Angeles has large positive estimates in event years 4 and 5. Kansas City has a positive estimate in event year 4. Houston, Baltimore, San Diego, Oakland, Newark, and Jersey City do not show consistent statistically significant post-repeal increases. These results are consistent with the main estimates, which show stronger evidence for delayed effects in selected cases rather than uniform increases across treated cities. This evidence suggests time is a significant factor. Repeal is not associated with an immediate increase in fatal-intersection crashes for most cities. The clearest evidence of increases appear later in the post-period era.

Table 15: Event-Study Difference-in-Differences Estimates by City

Case	Repeal year	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5
Greensboro, NC	2005	-0.05	-0.07	-0.07	-0.05	-0.06	13.28***
Charlotte, NC	2006	-0.08	-0.12	-0.10	-0.08	22.27**	36.05***
Fresno, CA	2006	-0.00	0.00	0.00	0.00	10.69	18.70***
Minneapolis, MN	2006	0.02	0.05	0.04	0.07	7.54**	11.53***
Lubbock, TX	2008	-0.46	-0.43	14.61***	14.45***	12.41**	12.67**
Houston, TX	2011	-6.15	-23.79	-20.78	14.10	-10.30	29.61
Los Angeles, CA	2012	27.72	21.38	27.72	20.29	101.30***	65.92***
Baltimore, MD	2013	0.81	2.70	10.57	12.46	7.63	-1.59
Kansas City, MO	2013	-2.03	-8.05	0.83	4.69	22.67**	11.55
San Diego, CA	2013	10.76	5.48	19.13	13.45	-2.39	20.47
Oakland, CA	2014	3.36	-3.28	-4.36	-5.30	-5.30	-1.03
Newark, NJ	2014	-9.10	-3.84	1.46	-0.46	-4.37	-3.19
Jersey City, NJ	2014	4.90	4.16	0.46	6.54	2.63	3.81

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

Table 16 reports pre-trend event-study estimates. Our coefficients test whether treated cities were already moving differently from their donor comparisons before repeal. Event year -1 is the omitted reference year, which is the year immediately before repeal. Each coefficient compares the

treated city's change from event year −1 to an earlier pre-repeal year against the corresponding donor-group change. For example, the Houston coefficient in event year −5 is −178.92. This means Houston had about 178.92 fewer fatal-intersection crashes five years before repeal, relative to its own year before repeal, compared with the donor group's same change. This is not a repeal effect. It indicates substantial pre-repeal imbalance (a trend already occurring). The zero pre-period cases show little evidence of pre-repeal divergence. We prefer the covariate-weighted zero-pre comparison for those cases. Several non-zero pre-period cases show weaker pre-trend performance. These estimates suggest that treated cities and donor comparisons were not following parallel pre-repeal paths in the event-study difference-in-differences specification.

Table 16: Event-Study Difference-in-Differences Pre-Trend Estimates by City

Case	Repeal year	Year -5	Year -4	Year -3	Year -2
Greensboro, NC	2005	-0.01	-0.01	-0.01	-0.03
Charlotte, NC	2006	0.18	0.08	0.08	0.07
Fresno, CA	2006	0.00	-0.00	-0.00	0.00
Minneapolis, MN	2006	0.02	0.03	0.01	0.01
Lubbock, TX	2008	0.23	0.60	0.49	-0.02
Houston, TX	2011	-178.92***	-178.78***	-178.85***	-178.84***
Los Angeles, CA	2012	-174.25***	-172.09***	-171.56***	13.52
Baltimore, MD	2013	-8.32	-8.20	10.19	-3.68
Kansas City, MO	2013	-27.35***	-27.35***	-1.16	2.44
San Diego, CA	2013	-48.31***	-48.42***	-3.24	-5.83
Oakland, CA	2014	-14.17*	-0.72	-10.52	-3.46
Newark, NJ	2014	-15.33*	2.01	3.11	-4.13
Jersey City, NJ	2014	4.67	10.01	6.11	3.87

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

5.3 City-Level Poisson Fixed-Effects Estimates

Tables 17, 18, and 19 report city-level Poisson fixed-effects estimates. The estimates are shown as incidence-rate ratios (IRRs). The IRR is calculated by exponentiating the treatment coefficient. If $\hat{\beta}$ is the estimated coefficient on the post-repeal treatment indicator, then the IRR equals $\exp(\hat{\beta})$. An IRR greater than one indicates a higher expected fatal-intersection crash count after repeal. An IRR less than one indicates a lower expected count. The percentage change is calculated as

$100 \times (\text{IRR} - 1)$. An IRR of 1.20 means the expected count is 20 percent higher while an IRR of 0.80 means the expected count is 20 percent lower.

Table 17: City-Level Poisson Fixed-Effects Estimates, Five Pre-Years and One Post-Year

Case	Repeal year	Main estimate	Main p-value	Poisson FE IRR	Poisson p-value
Greensboro, NC	2005	N/A	N/A	N/A	N/A
Charlotte, NC	2006	N/A	N/A	N/A	N/A
Fresno, CA	2006	N/A	N/A	N/A	N/A
Minneapolis, MN	2006	N/A	N/A	N/A	N/A
Lubbock, TX	2008	N/A	N/A	N/A	N/A
Houston, TX	2011	8.08	0.140	0.96	0.694
Los Angeles, CA	2012	32.13***	0.000	1.03	0.685
Baltimore, MD	2013	-0.37	0.970	0.95	0.816
Kansas City, MO	2013	-2.65	0.590	0.96	0.832
San Diego, CA	2013	9.64	0.120	1.20	0.171
Oakland, CA	2014	7.12	0.140	1.29	0.206
Newark, NJ	2014	-8.90*	0.090	0.61*	0.077
Jersey City, NJ	2014	0.82	0.760	0.99	0.977

Notes: N/A indicates zero treated fatal-intersection crashes in the post-period for zero pre-period cases. ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

Table 18: City-Level Poisson Fixed-Effects Estimates, Five Pre-Years and Three Post-Years

Case	Repeal year	Main estimate	Main p-value	Poisson FE IRR	Poisson p-value
Greensboro, NC	2005	N/A	N/A	N/A	N/A
Charlotte, NC	2006	N/A	N/A	N/A	N/A
Fresno, CA	2006	N/A	N/A	N/A	N/A
Minneapolis, MN	2006	N/A	N/A	N/A	N/A
Lubbock, TX	2008	5.03**	0.011	3.43	1.000
Houston, TX	2011	-17.92**	0.020	0.89	0.170
Los Angeles, CA	2012	14.64*	0.057	1.01	0.884
Baltimore, MD	2013	2.59	0.510	1.06	0.686
Kansas City, MO	2013	-5.56	0.150	0.92	0.490
San Diego, CA	2013	8.80	0.100	1.17*	0.094
Oakland, CA	2014	0.86	0.800	1.05	0.733
Newark, NJ	2014	-5.51	0.200	0.79	0.148
Jersey City, NJ	2014	-0.97	0.770	0.81	0.429

Notes: N/A indicates zero treated fatal-intersection crashes in the post-period for zero pre-period cases. ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

Table 19: City-Level Poisson Fixed-Effects Estimates, Five Pre-Years and Six Post-Years

Case	Repeal year	Main estimate	Main p-value	Poisson FE IRR	Poisson p-value
Greensboro, NC	2005	2.41**	0.010	2.79	1.000
Charlotte, NC	2006	10.80**	0.010	3.49	1.000
Fresno, CA	2006	4.88*	0.091	2.57	1.000
Minneapolis, MN	2006	3.44**	0.010	3.28	1.000
Lubbock, TX	2008	9.62**	0.010	4.04	1.000
Houston, TX	2011	-18.27***	0.000	0.91	0.232
Los Angeles, CA	2012	18.64*	0.069	1.02	0.778
Baltimore, MD	2013	2.78	0.530	1.05	0.690
Kansas City, MO	2013	-0.73	0.840	1.05	0.665
San Diego, CA	2013	3.87	0.420	1.10	0.237
Oakland, CA	2014	-1.04	0.730	0.99	0.949
Newark, NJ	2014	-5.88	0.150	0.80*	0.097
Jersey City, NJ	2014	-0.16	0.900	0.87	0.529

Notes: ***, **, and * denote $p < 0.01$, $p < 0.05$, and $p < 0.10$.

6 Conclusion

Our analysis estimates the effects of red-light camera repeals on fatal-intersection crashes. The results provide limited evidence of immediate post-repeal increases. The one-year and three-year specifications show mixed effects across cities. Our strongest evidence appears over longer windows, with delayed increases among zero pre-period cities and positive effects in Los Angeles. In the broader time horizon specification, zero-pre-period cities show an average increase of 6.23 fatal-intersection crashes per treated city-year relative to their matched synthetic doppelgängers. These estimates show delayed increases among zero pre-period fatal-intersection cases. These effects are concentrated in Greensboro, Charlotte, Fresno, Minneapolis, and Lubbock. Los Angeles also shows positive effects in several specifications. The two-way fixed effects estimates are often much larger than the weighted estimates. Event-study estimates show pre-trend problems for several large cities which likely implies that the weighted estimates are using more comparable counterfactual constructs. Lastly, our poisson fixed-effects estimates provide limited evidence for broad proportional increases.

The findings suggest that repeal effects are not uniform in non-zero pre-period cities. Red-light camera termination/expiry may increase fatal-intersection crashes in some cities, but the timing

and magnitude vary across cases. The evidence is strongest for delayed increases in cities with zero treated pre-period fatal-intersection crashes.

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