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Effects of Speed Cameras on Intersection Accidents: Evidence from Dayton*

Kevin Willardsen[a]

[a]*Department of Economics, Wright State University, USA*

Abstract: Over 30,000 people die annually in automobile accidents in the United States, which equates to about 10 in every 100,000 deaths. Empirical evidence on the effectiveness of speed cameras is mixed primarily due to the endogeneity of the timing and placement of the cameras. I am able to circumvent these issues by leveraging multiple court cases and political infighting that turn the cameras off, on, and off again on two separate data sets. Using a before-after and a difference-in-differences estimator over a twenty-year period, I find all three exogenous shocks suggest roughly the same effect. Speed cameras are weakly effective at preventing the total number of monthly accidents (0.3), certain types of "Angle" accidents (0.15), and most importantly, the severity of those accidents (0.14), which equate to about an 18.5%, 20%, and 41% decrease respectively.

Keywords: speed camera, intersection, accident, injury
JEL Codes: R41, R42, R48, R40

1. INTRODUCTION

The effect of red light and speed cameras on the total number, composition, and severity of accidents is still debated. Much of the debate is driven by the inherent endogeneity related to both the placement and timing of activation. The typical story involves an abnormally high number of accidents at an intersection which prompts the installation of a camera. Because all-time series have a tendency to revert to their mean following an extreme observation, a failure to adjust will overstate the effectiveness of the camera. Alternatively, there may be something about the intersection that is particularly dangerous: blind corner, high-speed limit, multiple lanes, etc. In this case, it is more the placement of the camera than the timing of activation that is the source of endogeneity.

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E-mail: kevin.willardsen@wright.edu

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This investigation is able to circumvent the endogeneity issues by taking advantage of multiple exogenous shocks to policy. The city of Dayton, Ohio, has been engaged in a legal battle with the State of Ohio over their use of red light and speed cameras. Three speed and two red-light cameras were installed at various four-way intersections throughout the city. Those cameras were initially activated in January of 2003. They remained active and in place until July of 2015. The city of Toledo was sued over their use of the cameras, and the case was appealed all the way to the Ohio State Supreme Court, which ruled (4-3) in December of 2014 that municipalities have home-rule effectively allowing them to continue using the cameras.[1] In response to that ruling, the Ohio state legislature passed Senate Bill (SB) 342, which was designed to undercut the use of the cameras.[2] The bill required that there be an officer stationed alongside each camera in use. Not just that an officer had to review every citation, but they must physically be there, sitting next to the camera. Unable to get an injunction, Dayton deactivated its cameras in July of 2015. The cameras remained decommissioned until 2018. Dayton challenged the constitutionality of part of SB 342, and in July of 2017, the State Supreme Court again ruled in favor of municipalities (5-2), declaring parts of SB 342 unconstitutional.[3] After that ruling, Dayton slowly reintroduced the cameras in 2018. In response, the Ohio legislature passed House Bill (HB) 62, which forced municipalities to report all citation revenue from speed or red-light cameras to the state, so an equal amount of funds could be removed from any state transfers to the municipality. This made the cameras net losers because all of the revenue from citations indirectly flows to the state, but the city still has the cost of operating them.[4] Toledo and Dayton were successful at getting a temporary injunction against HB 62, which lasted from July to the end of October of 2019. Upon the expiration of the injunction, Dayton was confronted with losing that revenue and quietly turned off the cameras in September of 2019.

This course of events provides the perfect set of natural experiments that allow for testing the efficacy of these cameras from not one but three exogenous shocks; when the cameras are turned off, then on, and back off again. There are four shocks in total if we include the introduction of the cameras. The initial activation still suffers from all of the traditional endogeneity concerns, but each of the remaining shocks has nothing to do with traffic conditions or accidents because they are caused by court cases or political infighting. The history of these cameras stretches back nearly 20 years and is not encompassed in a single data set. The longer of the two data sets runs from January 2000 to the end of 2017 but only contains intersections with a camera. This limits investigation to a before-after analysis. The second data set is much shorter, July 2013 through 2020, but it contains control intersections allowing for a difference-in-differences estimator. A preview of the results suggests that speed cameras are estimated to reduce the number of total intersection accidents by about 0.3 accidents a month. Point estimates are consistent across shocks, data, and methodology. Results are also robust to multiple sets of control intersections for the differences-in-differences estimator. I perform a test for spillovers and find some evidence of their existence, but the effects are small. The rest of this work is organized as follows:

[1] *Walker v. Toledo*, 143 Ohio St.3d 420, 2014-Ohio-5461.

[2] https://www.cleveland.com/open/2015/12/ohios_traffic_cameras_down_but.html

[3] Dayton v. State, 151 Ohio St.3d 168, 2017-Ohio-6909.

[4] https:www.daytondailynews.comnewslocaltraffic-cameras-trotwood-suspends-program-dayton-doesn-budge8TwHiAedW7tT6znh6AIPPO.

Section 2 covers the existing literature on the subject, with Section 3 covering the history of the camera program in Dayton. Section 4 theoretically connects the cameras to a reduction in accidents with Section 5 briefly describing both data sets and Section 6 explaining the empirical strategy for each. Section 7 covers all of the results and robustness checks with concluding remarks in Section 8.

2. LITERATURE

There are two reasons municipalities might want to use speed and red-light enforcement cameras. The first is well-intentioned. Local governments seek to reduce the number of accidents and protect their constituents. However, the second is that the cameras generate considerable revenue. Critics, like those in Ohio's State Legislature, have adhered to this second motivation as the "real" reason for using enforcement cameras. Removing the revenue incentive is the primary logic behind HB 62. Critics are emboldened by the fact that it is not entirely clear if these cameras even work to reduce accidents. Municipalities have been shown to use traffic citations as a revenue booster, Garrett and Wagner (2009), and the utilization of cameras serves only to make this process easier. In at least one case, speeding citations increased thirty-fold, Bourne and Cooke (1993).

The endogeneity makes it incredibly difficult to determine if these cameras work. In response to this problem, several strategies have been used to identify their effectiveness. The transportation safety literature tends to rely on statistical techniques to control for camera placement and reversion to the mean (RTM). Nearly all works rely on some variant of an "Empirical Bayes" (EB) strategy outlined in Hauer (2008). A limitation of EB strategies is that the estimated effects of the camera are usually sensitive to assumptions and the calibration of parameters from control intersections. The general methodology is to estimate the relationship between accidents and intersection characteristics using out-of-sample intersections; this is one way to control for the placement of the camera. It models the special characteristics of the intersection. Then apply those estimates on the treated intersection to get a prediction for those treated intersections that are then used to control for mean reversion. In this stage, they model the reversion that *would* have happened absent the camera. Thus, any deviation of that estimate is the effect of the camera. The issue is that any misspecification in the first stage, or the second, can bias the estimate on the effect of the camera. There may also be unobserved characteristics of the treated intersection, so calibrating on out of sample intersections may be building in sample selection issues. Because EB estimates are not identified via exogenous variation, they are not truly causal. EB methods have been shown to overstate the effectiveness of the camera in Monte Carlo simulations, Gallagher and Fisher (2020).

Additional complications in estimation stem from spillovers and halos. Spillovers occur when a camera affects the behavior of drivers at other intersections, or the camera may divert traffic to surrounding intersections, increasing the likelihood of accidents there. In either case, careful consideration must be given to control group selection to prevent the bias toward no effect in estimation. Halos occur when cameras turn off, and some proportion of drivers act as though the camera is still in operation either out of habit or ignorance. This, too, stunts the ability to find an effect.

Red-light cameras have consistently been shown to increase the number of rear-end accidents while reducing the number of "right-angle" or "angle" accidents, (Council et al. (2005), Erke (2009), Retting et al. (2003)). The intuition is that drivers avoid entering the intersection by heavier braking, which results in more rear-end accidents but fewer angle accidents because those vehicles never enter the intersection in those crucial moments where the probability of an angle accident is high, right before the light turns red. The effect on the total number of accidents, however, is more ambiguous, ranging anywhere from a decrease Retting and Kyrychenko (2002), to no effect (Erke (2009),Gallagher and Fisher (2020)), to a positive effect (Wong (2014), Burkey and Obeng (2004)). There are three influential literature review/meta-studies on the subject of red light cameras: Erke (2009),Høye (2013), and Llau and Ahmed (2014). All elaborate on the issues that arise from mean reversion and spillovers, and all three admit that the entire literature suffers from methodological limitations. How reversion to the mean and spillovers are dealt with can have dramatic effects on estimates. Economists have been surprisingly silent on the subject. One of the better works on the topic is relatively recent, Gallagher and Fisher (2020). They utilize a voter referendum to end the use of red-light cameras in Houston and find no effect on the total number of accidents or the number of injury accidents using a difference-in-differences estimator. Further, their results suggest that standard "Empirical Bayes" strategies for dealing with reversion to the mean may still overstate the effectiveness of red-light cameras.

Red-light cameras, by definition, focus on intersections, but the effects of speed cameras on intersections have not, to my knowledge, been considered. A few works investigate speed camera effectiveness by taking a city/region-wide approach, (Tay (2010), Carnis and Blais (2013)) while the vast majority of the remaining literature focuses on particular stretches of highways in both urban (Chen et al. (2002), Shin et al. (2009), De Pauw et al. (2014), Skubic et al. (2013)) and rural (Jones et al. (2008), Mountain et al. (2004)) settings. While early survey analysis showed promising results for speed cameras in self-reported speed and awareness, Corbett and Simon (1999), empirical work on their effectiveness has shown ranges from a 0% to -55% change in accidents depending on the context, Graham et al. (2019). The estimated effects of speed cameras are a little more stable than that of red-light cameras, but they still suffer from all of the same identification problems. Most studies rely on some form of simple EB to deal with RTM, with a few exceptions. Graham et al. (2019) use an approximate Bayesian doubly-robust inference method that places fewer assumptions on estimation relative to simple EB. They find an average of about 15% fall in accidents on treated roads. Borsati et al. (2019) deals with the endogeneity using IV estimation and estimates a much smaller effect of 3.9% fall in accidents given a 10% increase in coverage. Two reviews on the subject (Pilkington and Kinra (2005), Wilson et al. (2010)) ultimately conclude that there is likely a reduction in speeding, accidents, and injury accidents from the implementation of speed cameras, but both admit some issues. That the "level of evidence is relatively poor," and "More studies of a scientifically rigorous and homogeneous nature are necessary, to provide the answer to the magnitude of effect." A randomized controlled trial is generally preferred, but the sequence of events in Dayton should provide a very close second best.

3. THE DAYTON PHOTO ENFORCEMENT PROGRAM

The photo enforcement program of the Dayton City Police Department (DPD) consists of five fixed cameras, seven portable trailers, and six handheld devices used by individual officers.[5] Figure 1 shows the placement of both types of fixed cameras and speed trailers throughout Dayton. The trailer locations (circles) are not aimed at any intersection and are used as additional school zone enforcement. All of them are located near or in front of an elementary or middle school, and only one appears on the same road as a speed camera. However, they pose a potential confounding factor; if anything, they should bias the results toward zero. The speed cameras point in the North and South directions in all cases; and are usually within a block or two (500ft) from the intersection. The exception is North Gettysburg and Hoover, which sits further away, but is still closer than the next intersection. The cameras themselves are all visible from the road and are accompanied by signs informing drivers. An example can be seen in Panel (a) of Figure 2. When the cameras were installed in 2003, they were accompanied by "community notification," and warnings were distributed to motorists for the first month of their activation.[6] These warnings were mailed to drivers, informing them of the date of the end of the grace period and the amount of a formal citation. There are generic signs placed all throughout Dayton indicating that the city uses photo enforcement, Panel (b) of Figure 2.[7] Every ticket is verified by an officer and has a fee of $85 with a $25 late fee. On average, there are a little over 100 citations a day, grossing about 3.3 million annually. Failure to pay the "civil citation" will result in being sent to collections, and three citations (without resolution) will result in vehicle impoundment if parked anywhere in Dayton.

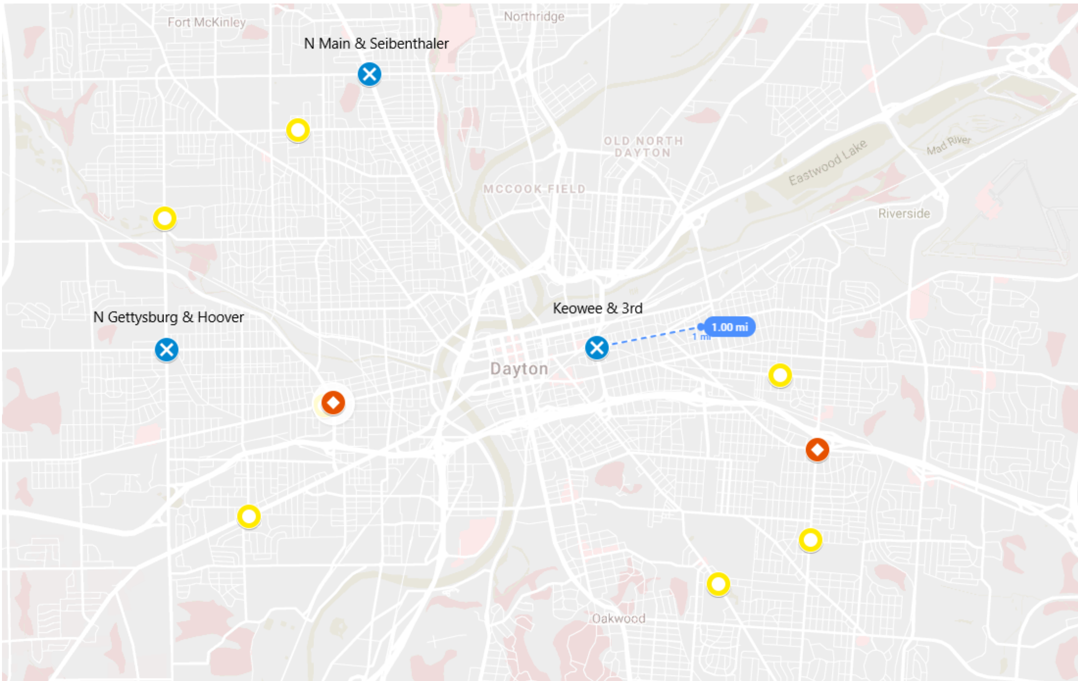
The shocks in 2015 (off) and 2018 (on) were covered extensively by the Dayton Daily News, the area's largest newspaper, and WHIO7 news.[8] Interest in the subject was amplified by the back and forth between city and state representatives and the fact that the issue made it to the state Supreme Court. Current operators of the program have no information on the justification to why those particular intersections were chosen but state that those are high accident areas.[9] The evidence strongly suggests a reversion to the mean story. This is covered in greater detail in Section 7. For the remainder of this investigation, we will be focusing on the speed camera intersections. Red-light cameras have already been investigated using a very similar identification strategy employed here, Gallagher and Fisher (2020), and I find no effect of red light cameras on any of the outcome variables of interest.

4. THEORY

The intuition for the effectiveness of speed cameras is relatively straightforward. Higher speeds increase both the damage and likelihood of an accident. The average force of a

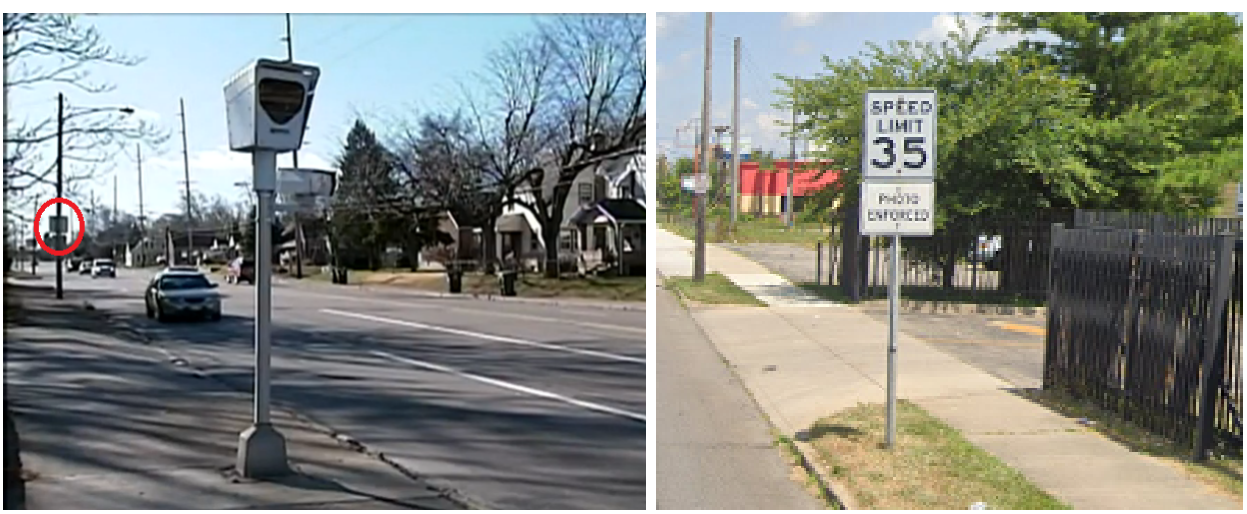
[5]The handheld cameras can go anywhere, and no records are kept as to their location.
[6]It is not clear what community notification means given the age of the program and lack of records.
[7]They vary in size and shape, but all say "Photo Enforced" or "Photo Enforcement."
[8]In 2018, motorists were given warnings for one month at each intersection.
[9]Lt. Mullens of the DPD.

Figure 1: Camera Placement



Speed (blue X), red light (red diamond), trailer (yellow circle) placement.

Figure 2: Camera Characteristics



(a) (b)

Sign for the camera circled mid-level left hand side.

horizontal impact is non-linear in the speed (s) of an object,

$$Horizontal\ Force = m\frac{s^2}{2d}.^{10} \tag{1}$$

A reduction in speed reduces force, which in turn reduces the probability of an accident and the severity of injuries. A reduction of 10 km/h from 50 km/h to 40 km/h will reduce the stopping distance for the typical vehicle by nearly 35%.[11] Reductions in speed also increase reaction window times for both drivers and pedestrians. The cameras reduce speeds by increasing the cost of speeding through fines. Let Equation (2) model the speed (s) **over the speed limit** chosen by a speeder as a function of time saved $T(s)$, cost of an accident $A(s)$, anticipated fine $F(P(s))$, which is a function of the probability of being caught $P(s)$, which is also a function of speed; and other characteristics like willingness to violate rules ϵ.[12] [13] Drivers will choose the speed which maximizes utility defined as,

$$\max_s U(T(s), F(P(s)), A(s), \epsilon), \tag{2}$$

where $\frac{\partial U}{\partial T} \geq 0$, $\frac{\partial^2 U}{\partial T^2} < 0$, $\frac{\partial U}{\partial F} < 0$, $\frac{\partial U}{\partial A} < 0$, and $\frac{\partial^2 A}{\partial s^2} > 0 \; \forall \; s$. Assuming a Cobb-Douglas utility function and applying a log transformation which will preserve the underlying preferences, from the first order conditions we know that the optimal speed must satisfy,

$$U_T\frac{\partial T}{\partial s} = U_F F_P \frac{\partial P}{\partial s} + U_A \frac{\partial A}{\partial s}. \tag{3}$$

Cameras increase the probability of getting caught to nearly 100%, making a fine almost certain for any $s > 0$. If $\mid U_T\frac{\partial T}{\partial s} \mid \leq \mid U_{\bar{F}} \mid \forall \; s$ then the only optimal solution is $s = 0$, i.e. not to speed. This is most likely the case because the utility of time saved is increasing but at a decreasing rate. If $\mid U_T\frac{\partial T}{\partial s} \mid > \mid U_{\bar{F}} \mid \forall \; s$ then the only limiting factor is that the utility of an accident is falling at an increasing rate through crash severity. Cameras reduce speed by changing the probability of confronting a fine. The reduction in speed should reduce the likelihood of all types of accidents, but especially those where speed plays an integral role: loss of control, angle accidents (right of way left turn), and accident severity.

5. DATA

This investigation takes advantage of two data sets, each with its own unique strengths. The first data set was compiled by the DPD.[14] It contains every accident at the five treated intersections from January 2000 to February 2018. Accidents are organized by type according to the legend in Figure 3.

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[10] Assuming direction is irrelevant and where m and d are mass and distance traveled during collision.

[11] https://www.who.int/violence_injury_prevention/publications/road_traffic/world_report/speed_en.pdf

[12] Let the cost of an accident be some positive linear function of horizontal force.

[13] Ignoring drivers whose optimal speed over the speed limit is always zero. We would not expect the cameras to have any effect on them.

[14] Detective Eric Brown of DPD.

Figure 3: Accident Type Composition for DPD Data Set

TRAFFIC ACCIDENT LEGEND

(Use with Accident Printouts beginning with Jan. 1, 2001)

Column Heading	
INTERSECTION	Intersection Accident-Intersecting streets listed i.e.
	Alaska Leo
	Mid-block Accident -address shown i.e.
	434 Alaska Leo
	Accident was at 434 Alaska near Leo.
DATE	Month / Day / Year
TIME	Military time 0 - 2400 hours
DAY	As shown
INJURY	F = Fatal I = Incapacitating V = Visible (non-incapac.) C = Claimed (possible) N = No injury

Column Heading

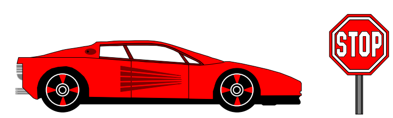
CODE

1) Rear end (ACD)
2) Changing lanes
3) Parked car (same direction)
4) Driving over curb, center divider or median
5) Improper backing
6) Right turn from wrong lane (at alley or private drive)
7) Red light
8) Non-stop
9) Right of way left turn
10) Right of way after stop
11) Right of way from curb
12) Right of way uncontrolled intersection (T intersection)
13) Right of way from private drive
14) Right of way from alley
15) Right of way to pedestrian
16) D.W.I.
17) Passing within 100' of an intersection
18) Jay walking (age 14 and above)
19) Playing in roadway (under age 14)
20) Improper passing
21) Right turn to wrong side of street
22) Left turn to wrong side of street
23) Right of way on green light
24) Right turn from wrong lane (at intersection)
25) Left turn from wrong lane (at intersection)
26) Excessive provable speed
27) Flashing red traffic light
28) Flashing red light at RR crossing
29) a -Wrong side of street (oncoming car)

Column Heading

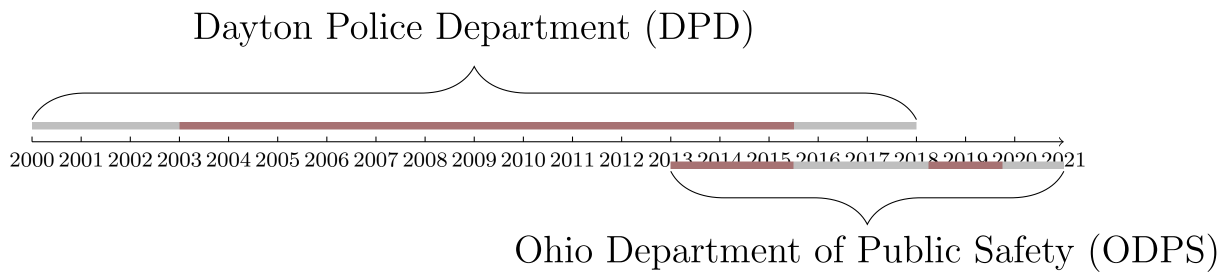
CODE

29) b-Parked car (opposite direction)
29) c-Right of way left turn (to alley/private drive)
30) U-turn
31) Wrong way on ONE WAY street
32) No headlights
33) Mechanical failures
34) Defective brakes
35) Opened door in traffic
36) Failing to signal for slow, stop, or turn
37) Wrong way on ONE WAY alley
38) Miscellaneous
39) Right of way to public safety vehicle (police, fire, etc.)
40) Right of way to Yield sign
41) Drove thru in marked turn lane
42) Ran off road
43) Opposite direction (2-way left turn lane)



1

Figure 4: Timing of Lights and Data Sets



Note: Red bars indicate when cameras are in use.

A diagram of the timing of the DPD data set can be found in Figure 4. Note that the data can be organized into three time periods: an untreated pre-period, a treated period, and an untreated post-period. It will be useful to consider these pieces for the coming discussion. The data is censored by the department and contains only the date, accident type, and the travel direction of the vehicles involved. There are over forty different code designations, but nearly 82% of the observations fall into Rear-Ends, Red Light, or Right of Way Left Turn. We will focus on these accident types along with Total Accidents. It is also common in the literature to consider "Angle" accidents, although the definition of angle varies. In this case, the angle will be defined as the sum of Red Light and Right of Way Left Turn. Red Light refers to anything red-light-related. Means and standard deviations by accident can be found in Table 1. Means for the Pre and Post-treatment times are disaggregated for comparison.

Table 1: DPD: Average Monthly Accidents by Type

Accident Type	Untreated		Treated
	Pre	Post	
All	1.518	1.258	0.968
	(1.211)	(1.169)	(0.952)
Angle	0.712	0.387	0.408
	(0.875)	(0.676)	(0.661)
Rear-End	0.509	0.365	0.315
	(0.754)	(0.585)	(0.560)
Red Light Related	0.490	0.215	0.275
	(0.779)	(0.507)	(0.550)
Right of Way Left Turn	0.222	0.172	0.133
	(0.480)	(0.432)	(0.359)

The second data set is continually updated by the Ohio Department of Public Safety (ODPS) pursuant to a state statute.[15][16] Compliance to the statute depends on county, but comprehensive data for Dayton's parent county, Montgomery County, begins in June of 2013.

[15]Data accessed from https://ohtrafficdata.dps.ohio.gov/crashstatistics/home
[16]RC §5502.01

This data set contains every single accident throughout the county. It also includes a wide variety of other information, including accident severity, location, and vehicle or property damage. There are varying degrees of severity, listed in the first column of Table 2, but there are not enough fatalities or serious injuries to warrant estimating them independently. They are combined with suspected minor injuries into a single variable (Injury). Accident types suffer from a similar problem. Angle and Rear-end accidents are the only types with sufficient observations warranting individual estimation; however, both of the sideswipe (SS) type accidents are combined into a single variable called "Sideswipe." The ODPS uses a different categorization standard for accident type than the DPS. The nine different accident types are collapsed to form four accident types: All Accidents, Angle, Rear-End, and Sideswipe.[17]

Table 2: ODPS Accident Type and Severity Designation

Severity	Accident Type		Treated	Control	Logit Cont.
Fatality	Angle	All	1.610	0.512	0.812
Serious Inj. Susp.	Backing		(1.365)	(0.866)	(1.029)
Minor Inj. Susp.	Head-On	Angle	0.700	0.214	0.372
Inj. Possible	Not Collision		(0.901)	(0.505)	(0.684)
Prop. Dmg Only	Unknown	Rear-End	0.526	0.159	0.236
	Rear-End		(0.726)	(0.444)	(0.499)
	Rear-to-Rear	Sideswipe	0.167	0.060	0.075
	SS; opp. dir.		(0.405)	(0.247)	(0.291)
	SS; same dir.	Injury	0.335	0.092	0.154
			(0.636)	(0.316)	(0.410)

6. EMPIRICAL STRATEGY

The empirical strategy is broken into two parts depending on the data set under investigation. The characteristics of each present their own unique challenges and opportunities. The empirical strategy for each is covered in the next two subsections, starting with the simpler DPD data set.

6.1. DPD Dataset

The structure of the DPD data limits the investigation to a before-after type analysis involving only the treated intersections observed before, during, and after treatment. The cameras first turning on suffering from all of the typical endogeneity problems explained in the introduction, but the cameras are turned off due to requirements of SB 342. One strategy to deal with this is to split the data in half and take advantage of the exogeneity of the camera turning off and compare that to the camera turning on. If both point estimates are the same, then we can plausibly treat the cameras' turning on as exogenous. The data

[17]Note that "Angle" accidents are not similar across data sets.

is split into two overlapping time periods running from 01/2000 to 05/2015, call this the Pre-to-Treatment, and from 01/2003 to 01/2018, referred to as Treatment-to-Post. A simple before-after analysis is executed on,

$$y_{it} = \beta\, treat_{it} + \gamma_i + t + \epsilon_{it}, \tag{4}$$

where y_{it} is the number of monthly (t) accidents at each intersection (i) for the accident types listed in Table 1. The variable of Interest is $treat_{it}$ which is a dichotomous variable equal to one when the camera is on and zero otherwise. γ_i are intersection specific intercepts to control for unobserved fixed characteristics, and t is a time trend.[18] Equation 4 is estimated using Fixed Effect Ordinary Least Squares (FEOLS) and Negative Binomial (NB) estimation. It was common in the traffic safety literature to estimate using Poisson because the number of accidents is bound at zero. However, the Poisson distribution forces the mean equal to the variance. This is a problem if accidents are overdispersed, a situation where the variance can be greater than the mean. From the mean and variance estimates in Table 2, this may indeed be the case. Under these conditions, the variance of the coefficients can be underestimated, Yan et al. (2005). In response to this problem, the literature transitioned to NB estimation. The two distributions are not unrelated. As overdispersion approaches zero, the NB collapses to the Poisson, Llau et al. (2015).[19] Standard errors for each estimation are bootstrapped because the data set suffers from a long $T = 217$ short $N = 3$ problem making a cluster inappropriate. A causal interpretation of β in Equation 4 requires that the untreated pre and post-periods be equivalent. This can be demonstrated by estimating the following equation,

$$y_{it} = \sum_{t \neq 2002} \theta_t \mathbf{1}\{t\} + \alpha_i + \upsilon_{it}. \tag{5}$$

In this case, $\mathbf{1}\{t\}$ is an indicator function for each month where 2002 is omitted to avoid perfect collinearity and to serve as the reference year.

6.1.1. Identification

Recall that there are two stories of endogeneity with respect to speed cameras. The first is that there is an abnormally high level of accidents at an intersection which is why it gets a camera. Second, there is some fixed characteristic about the intersection that makes it dangerous. That characteristic is likely fixed because if it were not, why bother putting a fixed camera there. If it is a characteristic of the intersection, then that should be captured in the fixed effect. As long as the timing of the camera is exogenous, then β will capture the effect of the camera, conditional on fixed intersection characteristics. Non-fixed characteristics like traffic flow should at least be approximated by trends or other dummies. One limitation to before-after analysis is that, due to the lack of a control group, we cannot rule out other changes that happened to have taken place around the time of the cameras turning on/off. This is where the two shocks come into play. If it is the case that estimating them independently gives similar magnitudes, then it must be the case that the unobserved change happened twice, at those exact moments, in opposite directions.

[18] Month dummies are also considered.

[19] A more comprehensive conversation of various methodologies and their strengths can be found in Lord and Mannering (2010).

6.2. The ODPS Dataset

The second portion of the empirical strategy involves estimating a difference-in-differences model using the ODPS data because it contains potential control intersections. The first step is to determine sets of control intersections. Each of the three treated intersections is a four-way stop. The broadest set of control intersections are any lighted intersection spanning two miles in all four directions of a treated intersection, including the red light camera intersections.[20] This leaves 78 control intersections. Mountain et al. (2004) find spillovers on 30 mph roads in the United Kingdom up to 1km away. Similarly, Gallagher and Fisher (2020) only consider intersections at least one-half mile away in Houston. Dayton is less densely populated, so as a first pass, I consider intersections at least three-quarters of a mile away, which leaves 42 remaining control intersections.[21] Gallagher and Fisher (2020) consider control intersections chosen by logit using intersection characteristics. Following them, a final control group is built in the same manner and used as a robustness check in Section 7.5. The standard difference-in-differences estimator is defined as follows,

$$y_{it} = \delta_0 + \delta_1 T_i + \delta_2 O_t + \beta(T_i * O_t) + \psi_i + \eta_t + \xi_{it}, \tag{6}$$

where y_{it} is one of the accident types (All, Angle, Rear-End, Sideswipe, Injury), for intersection i in month t. T_i is an indicator variable equal to one if the intersection is treated with a camera. O_t is also an indicator variable equal to one when the camera is on. The coefficient of Interest is β, which captures the interaction of the treated intersections while the cameras are on. ψ_i and η_t represent intersection and time fixed effects.[22] We will also consider an "event study" type analysis isomorphic to Equation 5. The introduction of control intersections and shorter panels implies that the appropriate standard error is clustered at the intersection.

6.2.1. Complications

Unlike the DPD data, there are no clean breaks in treatment for the time period covered by this data set. The cameras turned off in unison in July of 2015, but the cameras are staggered back on starting in 2018. The first camera is turned on in February, while the last two are not active until April of 2018. An additional complication presents itself when the cameras are turned back off in September of 2019. The city of Dayton deactivated the cameras but made no attempt to notify the public. The information was not widely circulated until March of 2020. The title of the article in the *Dayton Daily News* was "Dayton quietly unplugged red light, speed cameras in fight with state."[23] From September 2019 to March of 2020 it is

[20] Recall that Dayton implemented three-speed **and** two red-light cameras. Potential control intersections can stem from those red light-treated intersections as well. Even though I find no effect of those cameras, they were installed at the same time. The roads they are on receive a camera and are, at least on the surface, decent controls. Results are robust to this selection and dealt with more in Section 7.5.

[21] Three-quarters of a mile is approximately 4000ft. Results are robust to this choice, and this is covered in greater detail in Section 7.6.

[22] Trend and Month FE (Jan, Feb, etc...) are also tested. Time FE is not used in NB estimation because there is likely an incidental parameters problem, and most estimations do not converge.

[23] https:www.daytondailynews.com/newslocaldayton-quietly-unplugged-red-light-speed-cameras-fight-with-statet4OletRJD6MuLv2wwMhpIL

unlikely that anyone knew the cameras were off. This probably continued through the whole of 2020 as the news cycle was dominated by other things. This is problematic because it is tantamount to having treatment bleed into control. This actually provides an interesting opportunity to test the halo effect because the city was deliberately quiet about turning off the cameras. For the initial results, we can avoid the problem altogether by truncating the data set prior to September of 2019. We will, however, address this issue in Section 7.4.

7. RESULTS

7.1. DPD - Total Accidents

We begin our discussion of the results by first examining the DPD results as outlined in Section 6.1. Recall that the first step was to cut the DPD data set into two pieces and measure the effectiveness of the cameras turning on independently of them turning off. Ideally, both estimations should yield the same result. Columns (1)-(6) of Table 3 contain these estimates with various time controls. We will focus our discussion on FEOLS, but the NB results are very similar. The first point to note is that the estimates for the Pre-to-Treated are both larger in magnitude and are statistically stronger than the Treated-to-Post. The next important observation is that the Pre-to-Treated period is sensitive to a trend specification, comparing Columns (1) and (2) vs. that of (3). This indicates that the intersections may indeed be suffering from mean reversion, as evidenced by the coefficient shrinking relative to Columns (1) and (2). Properly controlling for the trend brings the estimated effects of the camera in the Pre-to-Treated and Treated-to-Post time periods into congruence for both estimation strategies. Comparing estimates in Columns (3) and (6) and those of Columns (9) and (12) demonstrates that we are getting roughly similar results. Finally, all of the data is put back together, and Equation 4 is estimated, but an additional specification allows for a split trend where the first and second halves of the data are allowed to have their own trend. Results are in the bottom panel of Table 3. Based on these OLS estimates, the speed cameras appear to reduce the number of accidents by a little under one wreck every two months. Using this specification, we now move forward to investigating individual accident types.

7.2. DPD - Accident Types

Table 4 presents the estimates for each of the accident types. Nearly every specification for the OLS estimates is negative and significant at some level of significance. The NB estimation fails to find a significant effect for the Right of Way Left Turns and most of the Red Light Related specifications, although all point estimates are negative. Larger effects are seen in Rear-End and Angle accidents, where OLS estimates imply that the speed cameras reduce the average number of monthly wrecks by about 0.17 and 0.38, respectively. Causal interpretation of these estimates requires estimation and visualization of Equation 5. Panel (a) of Figure 5 maps the monthly coefficient estimates for total accidents.

The red vertical lines at"Jan 03" and just to the right of "Jan 15" indicate when the cameras are turned on and then off. Although the image in Panel (a) is not dis-confirming, it

Table 3: Pre-to-Treatment and Treatment-to-Post Estimation on DPD Data Set

	Fixed Effects OLS						Negative Binomial					
	Pre-to-Treated			Treated-to-Post			Pre-to-Treated			Treated-to-Post		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β	-0.55***	-0.55***	-0.44***	-0.29	-0.29	-0.37**	-0.45***	-0.45***	-0.33***	-0.26	-0.26	-0.35*
	(0.106)	(0.109)	(0.103)	(0.244)	(0.241)	(0.184)	(0.038)	(0.036)	(0.047)	(0.178)	(0.185)	(0.188)
M.FE		x			x			x			x	
Trend			x			x			x			x
Obs.	558	558	558	543	543	543	558	558	558	543	543	543
R-sq.	0.047	0.078	0.049	0.012	0.042	0.013						
Int.	3	3	3	3	3	3	3	3	3	3	3	3

	Full Time Period				Full Time Period			
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(10)
β	-0.43***	-0.43***	-0.42***	-0.41***	-0.37***	-0.36***	-0.36***	-0.35***
	(0.074)	(0.059)	(0.085)	(0.087)	(0.077)	(0.083)	(0.089)	(0.106)
M.FE		x				x		
Trend			x				x	
Split				x				x
Obs.	651	651	651	651	651	651	651	651
R-sq.	0.036	0.059	0.042	0.043				
Int.	3	3	3	3	3	3	3	3

[a] M.FE - Jan, Feb, Mar, etc.
[b] Bootstrap standard errors in parentheses.
[c] *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

is also uninformative; there is too much noise. A clearer picture of events is found by aggregating up to yearly accidents and re-estimating, results are in Panel (b).[24] One disadvantage of this strategy is that 2015 is a partially treated year. The first half of 2015 is treated where the latter half is not. *Prima facie*, the yearly estimates do satisfy the conditions necessary for a causal interpretation. Coefficient and standard error estimates on both sides of the red lines encompass zero, and most of the observations in the middle are statistically less than zero. However, it does appear that there is a slight trend in point estimates for total accidents from 2000 to 2001. This is likely caused by Rear-End accidents. In Panel (c) of Figure 5, we see that Rear-End accidents do not satisfy the requirements for a causal interpretation. Rear-End accidents are likely the source of the trend seen in Total accidents. This implies that much of the strength of the results in Columns (1)-(3) of Table 4 are due to Rear-End accidents in 2000 and 2001, which is consistent with an RTM story. All of the other Panels (d)-(f) have point estimates either very close to zero or standard errors that contain them.

Another suspect finding is in Panel (d) for Red Light Related accidents. It does not appear that speed cameras have much of an effect until 2008. Obviously, there is something else at work in this result. Interestingly enough, this may actually be explained by the timing of yellow lights. In 2008 Governor Strickland signed House Bill 30, which requires an increase in the length of yellow lights.[25] It appears that the city of Dayton increased the

[24] Quarterly estimates were also tried and suffer from the same problems as the monthly estimates.
[25] https://www.thenewspaper.com/news/45/4527.asp

Table 4: DPD Accident Breakdown by Type

	OLS											
	Rear-End			Red Light Related			Right of Way Left Turn			Angle		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β	-0.12***	-0.12***	-0.12**	-0.08*	-0.07*	-0.07*	-0.07**	-0.06**	-0.05	-0.15***	-0.14***	-0.13**
	(0.042)	(0.035)	(0.051)	(0.045)	(0.042)	(0.042)	(0.027)	(0.027)	(0.039)	(0.043)	(0.046)	(0.052)
M. FE	X			X			X			X		
Trend		X			X			X			X	
Split			X			X			X			X
Obs.	651	651	651	651	651	651	651	651	651	651	651	651
R-sq.	0.021	0.011	0.011	0.018	0.031	0.031	0.034	0.007	0.015	0.029	0.033	0.036
Int.	3	3	3	3	3	3	3	3	3	3	3	3

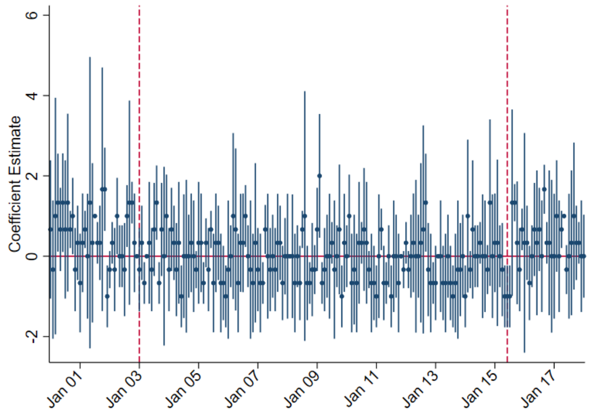
	NB											
	Rear-End			Red Light Related			Right of Way Left Turn			Angle		
	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
β	-0.32***	-0.31**	-0.31**	-0.26*	-0.16	-0.16	-0.41	-0.39	-0.35	-0.32***	-0.25*	-0.22*
	(0.115)	(0.138)	(0.135)	(0.134)	(0.117)	(0.0972)	(0.269)	(0.246)	(0.264)	(0.0676)	(0.131)	(0.118)
M. FE	X			X			X			X		
Trend		X			X			X			X	
Split			X			X			X			X
Obs.	651	651	651	651	651	651	651	651	651	651	651	651
Int.	3	3	3	3	3	3	3	3	3	3	3	3

[a] Month FE - Jan, Feb, Mar, etc.
[b] Bootstrap standard errors in parentheses.
[c] *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

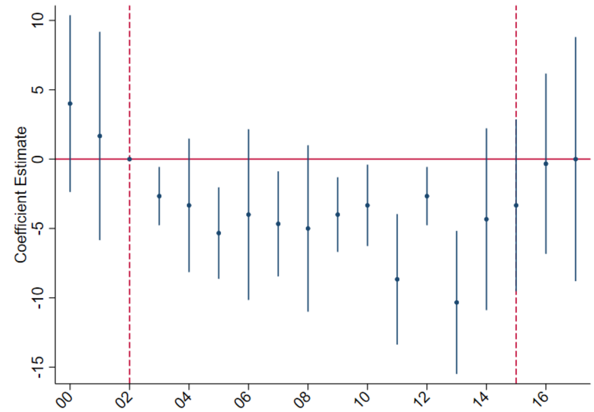
timing of yellow lights by 0.6 seconds which is about a 15% increase. This is a potential explanation for the reduction in Red Light Related accidents. Unfortunately, this does lead us to rule out speed cameras as a cause. These event studies imply that we should only have confidence in Total accidents and Right of Way Left Turns. The event studies for Angle accidents look satisfactory but recall that Angle is defined as the sum of Right of Way Left Turn and Red Light Related.

Mean reversion in Rear-End accidents does call into question the results of Table 3, especially those in the Pre-to-Treated period. The effects of the cameras must be overstated. There are two potential ways to deal with this problem: one, remove Rear-End accidents from Total, or two, drop 2000-2001 and re-estimate. OLS results for both are reported in the top and bottom panels of Table 5, respectively. Comparing Table 3 and Table 5 illustrates that the point estimates in the Pre-to-Treated period in Table 3 are exaggerated. Dropping Rear-end accidents or the first two years of the data dramatically shrinks the discrepancy between the point estimates of Pre-to-Treated, Treated-to-post, and the Full-Time period. The Treated-to-Post period is often insignificant not because the point estimates are radically different from the other time periods but because the standard errors are larger. We would also expect these results to be slightly weaker due to halo effects. The results in Table 5 are smaller and weaker than those in Table 3, but they are likely much closer to the truth, and they imply speed cameras reduce Total accidents by approximately 0.3 accidents a month. This evidence indicates that the most likely scenario is that there was an abnormally high

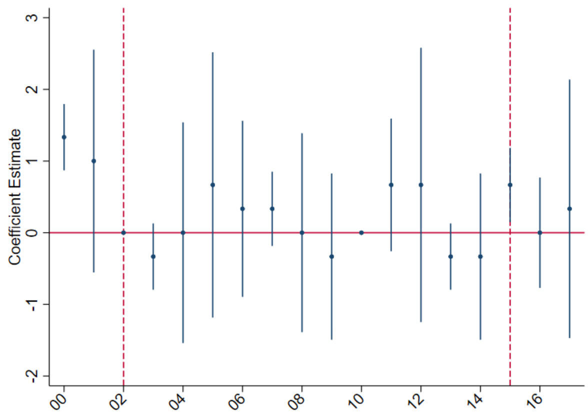
Figure 5: DPD Event Study by Accident Type



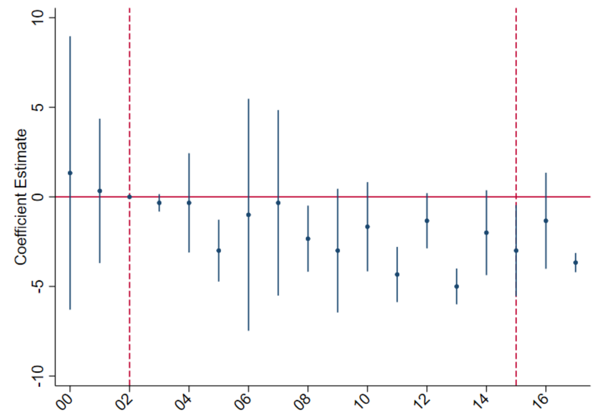
(a) Monthly



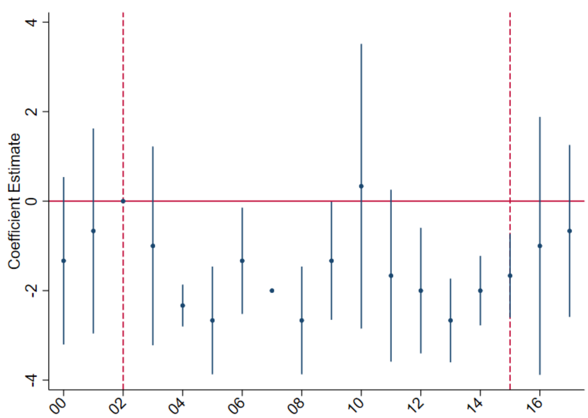
(b) Yearly



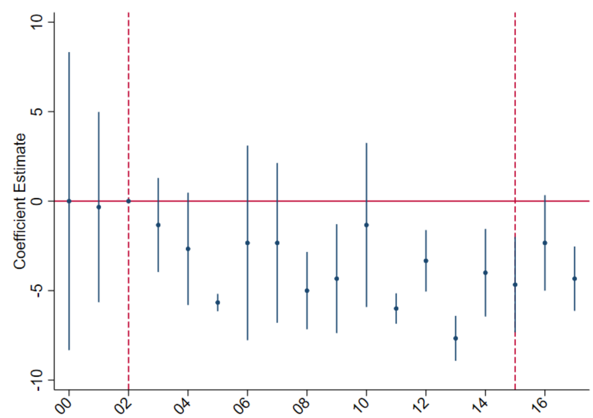
(c) Rear-Ends



(d) Red Light Related



(e) Right of Way Left Turn



(f) Angle

number of accidents around the years 2000-2001. This prompted the city government to consider putting in cameras. At the speed of local government, they were finally installed

Table 5: Robustness Check to Mitigate Effects of Reversion to the Mean - FEOLS Estimates

Drop Rear-End										
	Pre-to-Treated			Treated-to-Post			Full Time Period			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
β	-0.356***	-0.355***	-0.237***	-0.239	-0.234	-0.333	-0.302***	-0.302***	-0.295***	-0.288***
	(0.0823)	(0.0894)	(0.0836)	(0.275)	(0.215)	(0.255)	(0.0922)	(0.0819)	(0.0858)	(0.0869)
M. FE		x			x			x		
Trend			x			x			x	
Split										x
Obs.	558	558	558	543	543	543	651	651	651	651
R-sq.	0.030	0.053	0.034	0.012	0.034	0.014	0.026	0.043	0.030	0.031
Int.	3	3	3	3	3	3	3	3	3	3

Drop 2000-2001										
	Pre-to-Treated			Treated-to-Post			Full Time Period			
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
β	-0.392***	-0.391***	-0.304***	-0.289	-0.285	-0.372**	-0.318*	-0.313*	-0.350**	-0.349***
	(0.116)	(0.132)	(0.0953)	(0.262)	(0.237)	(0.179)	(0.169)	(0.167)	(0.139)	(0.119)
M. FE		x			x			x		
Trend			x			x			x	
Split										x
Obs.	486	486	486	543	543	543	579	579	579	579
R-sq.	0.012	0.044	0.014	0.012	0.042	0.013	0.018	0.044	0.019	0.021
Int.	3	3	3	3	3	3	3	3	3	3

[a] M. FE - Jan, Feb, Mar, etc.
[b] Bootstrap standard errors in parentheses.
[c] *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

in 2003 after the time series had already mean reverted in 2002. There is still one lingering concern. We can be reasonably confident that the β we have found is the effect of the cameras on accidents *at the treated intersections*; this is because the source of variation is exogenous to accidents. What we cannot say is that the same cameras will have similar effects at other intersections. For this, we need to take the analysis a step further and look at the difference-in-differences estimator.

7.3. ODPS

One of the benefits of the DPD data set was its length; however, the analysis was limited to a before-after design. The ODPS data set accommodates a more sophisticated analysis. Table 6 presents the results from estimating Equation 6 using the broadest set of control intersections from the beginning of the data set to September of 2019. These results are consistent with what has already been found using the DPD data set. There is a statistically significant fall in total accidents with a point estimate remarkably close to -0.3 for the OLS estimates. Point estimates for "Angle" accidents in Columns (6) and (7) of Table 6 are also incredibly close to those in Columns (10)-(12) of Table 4. The strongest result is in Injury accidents, which is the only category significant in both OLS and NB estimations. For these to be useful estimates, they must also satisfy the pre-trend conditions. Like the previous data set, the monthly event study estimates are too noisy to really be informative. Aggregating up to yearly data stabilizes the estimates but also has the drawback of a partially treated year. Monthly and yearly event study pictures for Total Accidents are presented in Figure 6. In this case, half of 2015 and the first quarter of 2018 are treated. The first fully untreated year is 2016, which is forced to zero. We are primarily interested in All (Panel (b)), Angle (Panel (d)), and Injury (Panel (f)). In all cases, the years 2016 and 2017 are statistically not different from zero and most of the other years are statistically negative or very close to it. These event studies provide weak support for interpreting results in Table 6 causally. There is still considerable variation even when considering yearly estimates.

Table 6: Difference-in-Difference Estimator on ODPS by Accident Type - Censored Post September 2019

OLS

	All Accidents			Rear-Ends		Angle		Sideswipe		Injury	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
β	-0.291*	-0.295*	-0.293*	-0.0690	-0.0659	-0.158*	-0.158*	-0.0101	-0.0106	-0.140**	-0.139**
	(0.171)	(0.170)	(0.173)	(0.0550)	(0.0553)	(0.0801)	(0.0821)	(0.0547)	(0.0558)	(0.0520)	(0.0526)
M. FE	x										
Trend		x		x		x		x		x	
Time FE			x		x		x		x		x
Obs.	3,375	3,375	3,375	3,375	3,375	3,375	3,375	3,375	3,375	3,375	3,375
R-sq.	0.014	0.025	0.045	0.008	0.028	0.013	0.032	0.005	0.024	0.006	0.029
Int.	45	45	45	45	45	45	45	45	45	45	45

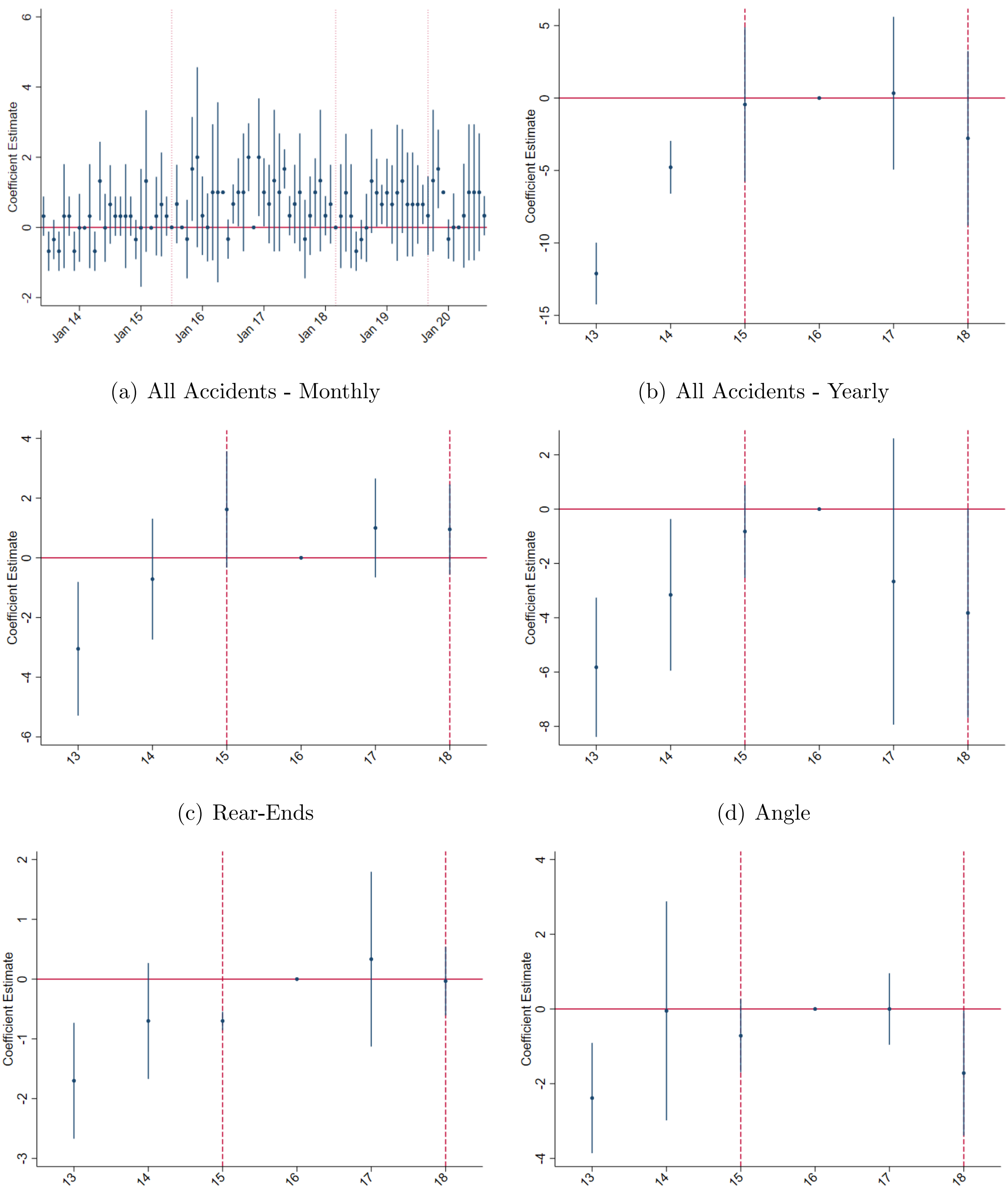
NB

	All Accidents			Rear-Ends		Angle		Side Swipe		Injury	
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
β	-0.0946	-0.0952	-0.103	-0.0882	-0.103	-0.101	-0.109	0.176	0.174	-0.756**	-0.765**
	(0.179)	(0.181)	(0.183)	(0.192)	(0.198)	(0.193)	(0.169)	(0.448)	(0.571)	(0.352)	(0.304)
M. FE		x		x		x		x		x	
Trend			x		x		x		x		x
Obs.	3,375	3,375	3,375	3,300	3,300	3,300	3,300	3,000	3,000	3,075	3,075
Int.	45	45	45	44	44	44	44	40	40	41	41

[a] OLS clustered standard errors and NB bootstrap standard errors in parentheses.
[b] *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Fortunately, event studies like those in Figure 6 are not the only way to test for valid pre-trends. Resampling methods can be used to ensure the treatment and control groups are similar during the untreated time periods. To test, I focus on the period of July 2015 to February 2018 of the ODPS data set. During this time period, the cameras were off. I keep the treatment and control intersections the same and randomly choose treatment times, and estimate Equation 6 twenty-thousand times. Those stored estimates are used to build the empirical distribution of beta under the null hypothesis. Because the treated group is not actually being treated in this time window, the distribution of beta should be centered at zero. From there, we can use a simple t-test to see if the stored estimates equal zero. Distribution characteristics and t-test results can be found in Figure 7. I am unable to reject any null on any distribution, even when considering one-sided tests. Mean and median

Figure 6: ODPS Event Study by Accident Type



(a) All Accidents - Monthly (b) All Accidents - Yearly

(c) Rear-Ends (d) Angle

(e) Sideswipe (f) Injury

estimates for all accident types are effectively zero. This further suggests that the difference between treatment and control groups is zero in expectation while the speed cameras are off.

7.4. Cameras on or off?

As outlined in Section 6.2.1, there is some ambiguity as to the functionality of the cameras post-September 2019. This scenario provides an interesting opportunity to demonstrate that the effect of the camera can persist insofar as the camera is still perceived to be on. To demonstrate, consider the top panel of Table 7. These estimates are the result of estimating Equation 6 on the entirety of the data and coding the cameras as off when they actually turned off in 2019. Call this the "Actual." Notice that only Injury accidents are still significant, and point estimates fall close to 30% for Total accidents and approximately 50% and 15% for Angle and Injury accidents, respectively. The bottom panel of Table 7 is the same estimation, but instead of coding the camera as being off, it assumes that most people still think the camera is on, calling this the "Ignorant." The Ignorant estimates look almost identical to those found in Table 6, which were obtained by dropping the post-September 2019 data to avoid this complication altogether. One concern in the literature with the finding of a null result is that it may just be the case that residents are not aware of the cameras. This evidence does not necessarily get to the heart of that issue but does add credence to that criticism if it can be shown that people's belief that a camera is still on affects their behavior in predictable ways even when the camera is indeed off. It is reasonable to infer two things, one; we need to be careful about proclamations from a null finding, and, two, it is not just the camera. If the intent is to prevent accidents, as opposed to merely generating revenue, then roll-outs of these programs need to be accompanied by information campaigns. No change in the information set implies no change in behavior.

7.5. Robustness: Logit Control

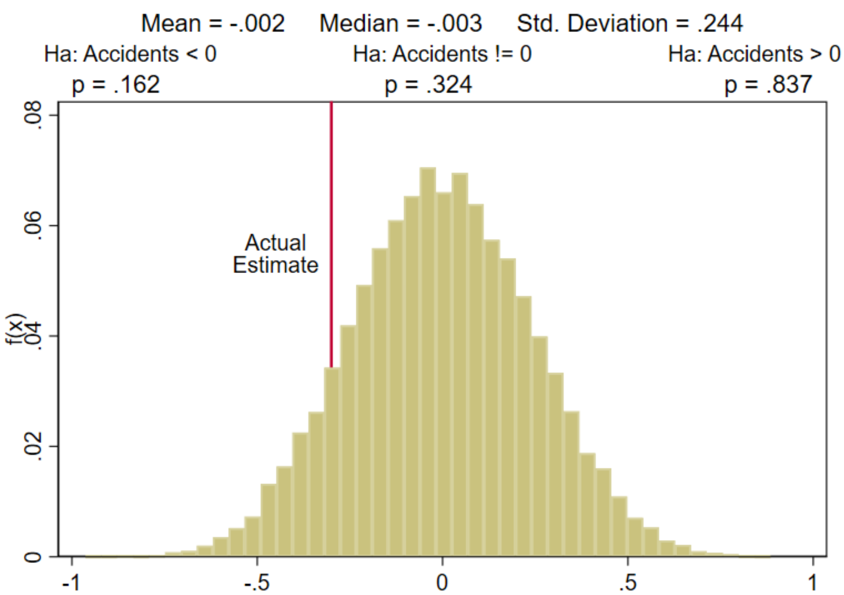
As noted in the introduction, Gallagher and Fisher (2020) use a logit to first identify potential control intersections. Following their lead, a logit model is estimated to determine which of the remaining 42 intersections would also be chosen to receive a speed camera based on observable characteristics.[26] A simple logit of the form,

$$y_{it} = \alpha + X_{it}\beta + v_{it} \tag{7}$$

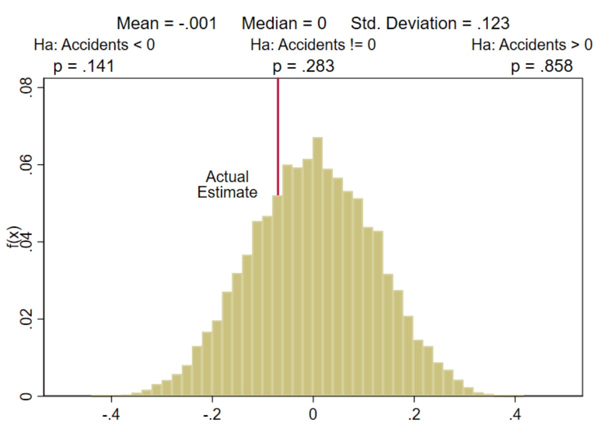
is estimated where X_{it} is a vector of characteristic information including minimum/maximum number of lanes, right turn lane on/off, left-turn lane on/off, speed limit, accidents, accidents squared, and a time trend. Unfortunately, there is not enough variation and all but: max number of lanes, accidents, accidents squared, and the time trend are omitted due to predicting failure perfectly. This investigation would also contain traffic flow data under ideal conditions. The Miami Valley Regional Planning Commission does keep some information on traffic flow, but recordings are too scarce to be useful. Two of the three treated intersections have only been measured once from 2008-2019, and the program did not exist prior to 2008.[27] This reduces the number of control intersections considerably, leaving only five re-

[26]Probit estimation yields the exact same intersections.
[27]https://www.mvrpc.org/transportation/traffic-count-program/mvrpc-traffic-count-viewer.

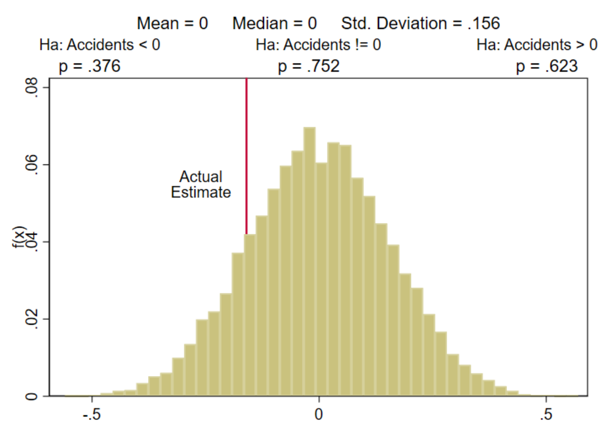
Figure 7: Distribution of β



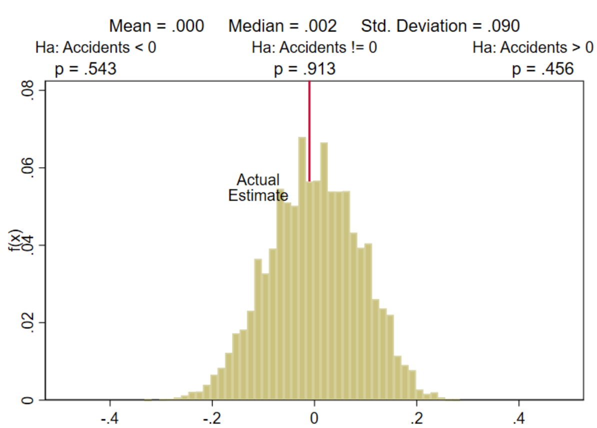
(a) All Accidents



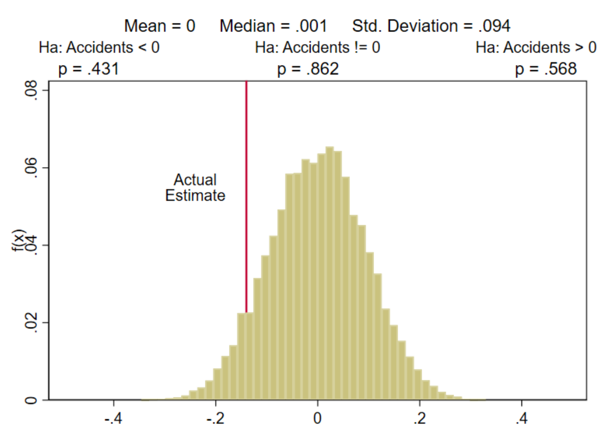
(b) Rear-Ends



(c) Angle



(d) Sideswipe



(e) Injury

Table 7: Difference-in-Difference Estimator on ODPS by Accident Type - Full Time Period

	Actual										
	All Accidents			Rear-Ends		Angle		Sideswipe		Injury	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
β	-0.208	-0.211	-0.210	-0.0485	-0.0471	-0.0866	-0.0865	-0.0458	-0.0462	-0.118***	-0.117***
	(0.171)	(0.169)	(0.172)	(0.0395)	(0.0402)	(0.0765)	(0.0786)	(0.0462)	(0.0471)	(0.0346)	(0.0353)
M. FE	x										
Trend		x		x		x		x		x	
Time FE			x		x		x		x		x
Obs.	4,185	4,185	4,185	4,185	4,185	4,185	4,185	4,185	4,185	4,185	4,185
R-sq.	0.013	0.018	0.046	0.004	0.028	0.010	0.033	0.005	0.030	0.008	0.032
Int.	45	45	45	45	45	45	45	45	45	45	45

	Ignorant										
	All Accidents			Rear-Ends		Angle		Side Swipe		Injury	
	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)
β	-0.269*	-0.272*	-0.271*	-0.0630	-0.0607	-0.169**	-0.168**	0.0232	0.0226	-0.117**	-0.117**
	(0.136)	(0.136)	(0.137)	(0.0571)	(0.0574)	(0.0746)	(0.0758)	(0.0462)	(0.0471)	(0.0520)	(0.0526)
M. FE	x										
Trend		x		x		x		x		x	
Time FE			x		x		x		x		x
Obs.	4,185	4,185	4,185	4,185	4,185	4,185	4,185	4,185	4,185	4,185	4,185
R-sq.	0.008	0.021	0.047	0.005	0.028	0.012	0.034	0.004	0.029	0.007	0.031
Int.	45	45	45	45	45	45	45	45	45	45	45

[a] OLS clustered standard errors and NB bootstrap standard errors in parentheses.
[b] *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

mainings. Even with the dramatic reduction of degrees of freedom, the results of estimating Equation 6 are remarkably stable. Standard results for the estimation up to September of 2019 and for the "Ignorant" specification are presented in Table 8. Results are statistically weaker, but magnitudes increase slightly.

7.6. Robustness: Spillovers

The choice of a three-quarter-mile spillover range is informed by Mountain et al. (2004), but it is imperative that results are not too sensitive to it. Large deviations in restricting control intersections to being a half-mile away or a full mile away will call these findings into question. This also offers an opportunity to measure the spillover gradient. By varying the exclusion distance for control intersections, we can estimate the extent of the spillovers by observing the change in the point estimates of β. For example, imagine that the *true* effect of the spillover is indeed three-quarters of a mile exactly. If we do not restrict the control intersections at all, then the existence of the spillovers will bias β downward because it is a difference-in-differences estimate. Failing to exclude close intersections is adding treated intersections into the control group, making it more difficult to find a difference. As the spillover exclusion is extended outward, the estimate on β should grow as we exclude *spillover treated* intersections from the control group. The point estimate should grow up until three-

Table 8: Robustness Check Using Logit Control Group

OLS

	Ignorant					Drop				
	All	RE	Angle	S.S.	Inj.	All	RE	Angle	S.S.	Inj.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
β	-0.37*	-0.09	-0.16	0.00	-0.12*	-0.41*	-0.12	-0.14	-0.03	-0.14*
	(0.162)	(0.070)	(0.100)	(0.050)	(0.0634)	(0.199)	(0.068)	(0.114)	(0.060)	(0.067)
Obs.	744	744	744	744	744	600	600	600	600	600
R-sq.	0.128	0.104	0.127	0.171	0.111	0.136	0.102	0.130	0.159	0.113
Int.	8	8	8	8	8	8	8	8	8	8

NB

	Ignorant					Drop				
	All	RE	Angle	S.S.	Inj.	All	RE	Angle	S.S.	Inj.
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
β	-0.32*	-0.32	-0.22	0.03	-0.69**	-0.31	-0.39	-0.04	-0.03	-0.77**
	(0.185)	(0.287)	(0.173)	(0.642)	(0.329)	(0.223)	(0.281)	(0.226)	(3.354)	(0.386)
Obs.	744	744	744	744	744	600	600	600	600	600
Int.	8	8	8	8	8	8	8	8	8	8

[a] OLS clustered standard errors and NB bootstrap standard errors in parentheses.
[b] *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

quarters of a mile away. Further extending out the exclusion radius should have no effect on the estimate because the spillover affected intersections have already been removed. To test for spillovers, Equation 6 is estimated using FEOLS with the removal of control intersections in 500ft increments for the three significant accident types: All, Angle, and Injury.[28] Results are stable to the exclusion of control intersections by distance and can be found in Table **??**. There is a small increase in point estimates from 500 to 1000 feet in All and Injury accidents, and they appear to peak at a little over a mile away, but not one of the point estimates is statistically different from one another. If spillovers do exist, I find little of them here.

8. CONCLUSION

Nearly anyone who has spent any meaningful amount of time in a vehicle has been in some form of a fender bender. Even if an accident does not result in bodily injury, it can still be incredibly costly in terms of hassle and time to say nothing of the property damage involved. Speed enforcement cameras are just one of many tools that local governments have to reduce these costs. There is good reason to believe that these cameras would be effective at reducing accidents in general, but the magnitude of those estimates varies wildly, and their effect on intersections has never been investigated directly. This investigation

[28] No control intersections are removed under 500 ft.

took advantage of two data sets over a twenty-year period. Using two different estimation strategies over two different data sets using multiple control groups, we find point estimates that are surprisingly consistent. In both cases, OLS estimates suggest a somewhere between a 0.3-0.4 reduction in monthly accidents with more confidence toward the bottom of the interval. This translates to close to one accident every three to four months or an 18-25% reduction in total accidents. Evidence suggests that they also reduce the number of "Angle" accidents, but we can be far more confident that speed cameras reduce the number of injury accidents. Although there was no significant effect on severe or fatal accidents as individual categories, this is likely due to the small sample size. As with every empirical work, there are limitations. This investigation was able to span a considerable amount of time, but it has few treated groups. There were only three intersections, and each of those intersections was at most, two lanes. It is likely inappropriate to compare these results to intersections with considerable traffic flow and speeds in excess of 40mph. More empirical work is always needed to confirm any result.

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