Criminal Deterrence in a Setting with Multiple Risks: Traffic Cameras, Vehicular Accidents, and Public Safety

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Abstract

In US cities the most likely location for a vehicular accident is at a traffic intersection. Numerous cities have enacted red light camera (RLC) programs under the rationale that higher expected fines for running a red light will induce drivers to stop and lead to fewer crossroad collisions. This view ignores the likelihood that RLC programs lead drivers to accept a greater accident risk from stopping. We evaluate the exogenous removal of RLCs via a voter referendum using 12 years of geocoded police accident data and find no evidence that the RLC program led to fewer total accidents or injuries.

JEL Classification: H27, H71, K32, R28, R41

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1 Introduction

In 2014, 36,675 people died in traffic accidents in the US. 2.3 million were injured in US traffic accidents in 2013 (Economist [2015]). In urban areas, by far the most likely location for an accident is at a traffic intersection. Figure 1 shows yearly accident rates for the city of Houston from 2003-2014 by 100 foot distance intervals from an intersection. There are approximately 8 times as many accidents within 100 feet of an intersection than at any distance beyond 200 feet.

Recognizing the danger that urban street intersections pose, over 438 communities in 23 states, including 36 of the 50 most populous US cities, have enacted red light and speed camera programs as an effort to enforce traffic laws at intersections and to reduce accidents (IIH [2016]). The public safety rationale for the programs is straightforward. Drivers who run a red light at a red light camera (RLC) intersection will receive a ticket with near certainty. The higher probability of getting caught will lead to a reduction in the number of vehicles running a red light and thereby fewer collisions involving vehicles entering the intersection at the same time from different roadways.

Law enforcement personnel are, overall, strongly in favor of red light camera programs and adamant about their effectiveness. For example, the executive director of the Governors Highway Safety Association (GHSA) recently articulated the GHSA's position as "strongly support[ing] the use of automated traffic enforcement technology, including red light cameras, to improve safety for all road users. [...] It is mind-boggling that these proven safety tools are being removed despite numerous research studies validating their safety benefit" (GHSA [2016]).[1]

However, red light camera programs differ from many other crime policies in that crime prevention is not an end of itself, but rather viewed as a mechanism to accomplish a broader policy goal. The assumption is that by

[1] According to their website, the "GHSA provides leadership and representation for the states and territories to improve traffic safety, influence national policy, enhance program management and promote best practices." http://www.ghsa.org/resources/state-highway-safety-group-supports-red-light-cameras

incentivizing fewer drivers to run red lights that there will be a reduction in the total number of accidents. Despite clear evidence that installing a RLC reduces the number of vehicles running a red light, the predicted relationship between the number of vehicles running red lights and the total number of accidents is ambiguous.

A simple economic model shows that a RLC program has two opposite traffic safety effects. A RLC program provides an incentive to reduce red light running which potentially decreases the number of accidents caused by vehicles *not stopping* at a red light. At the same time, a RLC program increases the incentive to *stop* at a red light even when doing so may involve a rapid and potentially dangerous deceleration of the vehicle. Vehicles suddenly attempting to stop could increase the number of accidents. In fact, the model predicts that under the RLC program some drivers will choose to stop and accept a higher accident risk from attempting to stop at the intersection so as to avoid the expected fine from continuing to drive through the intersection. Thus, the overall effect of a RLC program on vehicle accidents is ambiguous a priori.

In this paper we test Becker's deterrence hypothesis that an increase in the probability of being caught breaking the law reduces vehicular crime and improves the public safety (e.g. Becker [1968]; DeAngelo and Hansen [2014]; Hansen [2015]).[2] In particular, we test whether red light cameras, a popular policing tool, are effective at reducing accidents and improving public safety.

There is almost no existing economics literature examining red light traffic cameras as a policing tool (Chen and Warburton [2006] and Wong [2014] are exceptions). However, there is a lengthy literature in transportation and safety journals (e.g. Erke [2009] and Høye [2013] provide reviews). Most of the existing studies either compare city-level accident data between cities with and without RLC programs, or focus on a small number of intersections (often a single intersection before and after the installation of a camera).[3] The

[2]Interestingly, traffic crimes, while not a common setting to study Becker's deterrence predictions, is a specific crime highlighted in Becker [1968], p2.

[3]The most common estimation approach is what the literature calls "Empirical Bayes", whereby the number of accidents during a time period before a RLC is installed is used to project the expected number of future accidents at the same intersection after the RLC is

majority of these studies conclude that RLC programs have a statistically and economically significant effect on reducing traffic accidents, injuries, and deaths. One frequently cited recent study examines vehicular deaths at the city-level for cities with and without RLC programs and concludes that vehicular deaths increase by 30% in the absence of red light camera programs (Hu and Cicchino [2016]).

The main challenge that all existing red light camera studies struggle with is how to account for the endogenous start time and location of the cameras. This challenge is an example of the now well-known endogeneity problem which undermined many early tests of Becker's deterrence prediction. For example, early empirical studies that tested whether an increase in policing intensity reduced crime often failed to detect any effect (e.g. Levitt and Miles [2006] and Chalfin and McCrary [2017] provide reviews). The change in the likelihood of being caught is often endogenous to the level of crime which leads to a bias of finding no correlation (e.g. Levitt [2007]).

In the context of a red light camera program, the endogeneity problem likely leads to over-estimates of the program's effectiveness at reducing traffic accidents and injuries. Intersections chosen for red light cameras are not selected randomly. Intersections with unusually high accident levels may be more likely to receive cameras. These same intersections are also more likely to revert, regardless of intervention, to lower accident levels. Moreover, intersections assigned red light cameras are often more dangerous (e.g. poor traffic flow, high traffic volume) than other nearby intersections. We avoid concerns about the endogenous selection of intersections to install RLCs by looking at the impact of an exogenous removal of RLCs by a voter referendum.

A second key challenge in using policy changes to estimate the deterrence relationship is that the effect of the policy change on the probability of being caught may be unknown to the target population (e.g. Waldo and Chiricos [1972]; Apel [2013]; Chalfin and McCrary [2017]). That is, the perception of being caught might not reflect the actual probability of being caught among

installed. The effect of the RLC program is defined as the difference between the projected number of accidents and the realized number of accidents (Hauer [1997]).

potential offenders. An advantage of studying the deterrence effect in the context of RLC programs is that we can confirm a change of perception among drivers after a RLC is installed. For example, Martinez and Porter [2006] and Porter et al. [2013] use direct observations of driving behavior and conclude that the incidence of red light running falls dramatically immediately after the installation of RLCs, and then returns to pre-camera violation levels within a year of the removal of the cameras. Further evidence comes from the number of tickets issued. A common pattern observed in city RLC programs is that the number of tickets issued peaks in the first year of the program and then is much lower in subsequent years as drivers learn of the camera location and adjust their behavior.

The main empirical setting of our study is Houston, TX. Houston established a red light camera program in 2006 that grew to cover a total of 66 intersections. Houston residents narrowly passed a voter referendum in November 2010 that banned the cameras. The Houston police department and the mayor's office were both opposed to the ban (e.g. Oaklander [2011]). After the referendum, the city immediately removed the cameras.

We estimate a difference-in-differences model using Poisson regression and the complete police record of geocoded accident data for a 12 year period (2003-2014). We estimate models that separately examine the effect of the RLC program on right angle accidents, non-right angle accidents, total accidents, and injuries. Right angle accidents comprise about a third of the total number of accidents at a typical intersection and are the primary target of RLC programs (Retting and Kyrychenko [2002]). If a RLC program is successful at improving traffic safety then we would expect that the removal of the cameras would lead to an increase in the number of accidents (in particular, right angle) and/or injuries at RLC intersections relative to control intersections not subject to the referendum.[4]

We consider several different control groups of intersections, although we

[4] We do not separately estimate the number of deaths because there are far too few to provide reliable estimates, although we do include deaths when we estimate the effect on injuries. We also consider bounds on changes in the number of deaths when evaluating the overall welfare effect of the RLC program.

focus on three main comparisons. The first control group is comprised of non-RLC intersections in Houston with similar accident characteristics and trends as the Houston RLC intersections during the 3 years before the start of the program (2003-2005). While our focus is what happens to accidents after the referendum, in order to avoid concerns regarding the endogenous start time for each RLC intersection, the 12 year panel also allows us to estimate the effect of starting the RLC program.

The 2nd and 3rd control groups examine the removal of the cameras as compared to the years just prior to the referendum while the RLC program was in effect. The 2nd control group again focuses on Houston intersections that never had a RLC, while the 3rd control group is comprised of Dallas RLC intersections that were unaffected by the referendum. The Dallas RLC control group has two main advantages. First, the accident-related characteristics (e.g. total accidents) at Dallas RLC intersections are more similar–as a group–to the Houston RLC intersections than are the non-RLC intersections in Houston. Second, using an out of city control group avoids concerns that the RLC program affects driving behavior at other non-RLC intersections in the city (e.g. Høye [2013]; Shin and Washington [2007] Wong [2014]). In fact, we find suggestive evidence that the Houston RLC program shifted traffic patterns in the city using average daily traffic data. Average daily traffic data are available for both of the shorter panels, but not for the longer panel.

We select the Houston control intersections by estimating the propensity to have a Houston RLC using a logit model that includes pre-referendum accident-related characteristics that have been cited as important criteria in selecting RLC intersections (Department [2016]; Chi [2016]; Stein et al. [2006]). The two Houston samples and the Houston-Dallas sample are mostly well-balanced when comparing the means of important accident-related characteristics (e.g. accident levels, average daily traffic) between the treatment group of Houston RLC intersections and the control groups. More importantly, we show that the treatment and control groups in each sample display similar trends in these same characteristics before the referendum.

We find no evidence that red light cameras reduce the frequency of vehicular accidents. In our preferred model we estimate a positive point estimate for angle crashes (12%) and a negative point estimate for all other types of accidents (-14%) when the cameras are removed after the referendum. The net effect is an estimate of -5%. None of the estimates are statistically significant.

We estimate a negative, statistically insignificant change in the number of injury accidents (and the number of total people injured) after the RLC program ends. Estimates for the change in average daily traffic at the RLC intersections suggest that, if anything, that the model estimates for number of accidents and for the number of injury accidents are upwardly biased. Finally, our study highlights the challenge of using policy tools to deter crime in situations where potential offenders face multiple risks.

2 Model of RLC Program Driver Behavior

This section outlines a simple model of the effect that a red light camera has on driver behavior and the number of traffic accidents. Becker's model of crime predicts that the fraction of drivers breaking the law and running a red light will decrease when the expected penalty for running a red light increases (Becker [1968]). Driver i approaches intersection j at time t as the signal light turns from green to yellow. The driver decides whether to attempt to stop or to continue and proceed through the intersection. A driver will choose to (potentially) run a red light if the expected utility from continuing exceeds the expected utility of stopping. Equations (1) and (2) model the utility from continuing to drive and attempting to stop, respectively.

$$C_{i,j,t} = u(T_{i,j,t}, F_{i,j,t}, A_{i,j,t}, \xi_{i,j,t}; D_{i,j,t}) \tag{1}$$

$$S_{i,j,t} = u(A_{i,j,t}, \psi_{i,j,t}; D_{i,j,t}) \tag{2}$$

The benefit of continuing is assumed to largely be due to $T_{i,j,t}$, the travel time savings of not having to wait at a red light, which can vary by driver (e.g.

hourly salary), intersection (e.g. length of red light phase of traffic signal), and time of day (e.g. whether the driver is commuting to work). The anticipated fine, $F_{i,j,t}$, depends on the likelihood that the driver's vehicle passes through the intersection before the light turns from yellow to red, the probability of receiving a ticket if the vehicle is in the intersection after the light turns red, and the size of the fine. We assume that $F_{i,j,t}$ only appears in Equation (1). Of course, a driver could receive a fine when attempting to stop (e.g. if the vehicle skids into the intersection). Nevertheless, the key point is that the anticipated fine is larger if a driver deliberately continues through the intersection.

$A_{i,j,t}$ is the cost of an accident and enters both utility functions. $A_{i,j,t}$ depends on the probability of being in an accident and the monetized vehicle damage and injury costs conditional on being in an accident. Finally, $\xi_{i,j,t}$ and $\psi_{i,j,t}$ represent all other factors that would affect a driver's utility of continuing (e.g. scaring or annoying other drivers, which might be expressed as other drivers honking the horn) and stopping (e.g. willingness to break the law). All of the factors discussed above are conditional on the distance, $D_{i,j,t}$, that the driver is from the intersection when the light turns yellow. The utility of continuing to drive through the intersection is decreasing in the cost of an accident, $\frac{\partial C_{i,j,t}}{\partial A_{i,j,t}} < 0$, decreasing in the cost of a fine, $\frac{\partial C_{i,j,t}}{\partial F_{i,j,t}} < 0$, and increasing in travel time savings, $\frac{\partial C_{i,j,t}}{\partial T_{i,j,t}} > 0$. The utility of stopping is also decreasing in the cost of an accident, $\frac{\partial S_{i,j,t}}{\partial A_{i,j,t}} < 0$.

A RLC program decreases the utility of continuing through the intersection after the light turns yellow by increasing $F_{i,j,t}$ via a dramatic increase in the probability of receiving a ticket. The probability of receiving a ticket for running a red light at an intersection without a red light camera is very low as it requires a police officer located near the intersection observing the infraction. The probability of receiving a ticket when there is a RLC at the intersection is close to 100%. We expect that an increase in $F_{i,j,t}$ would decrease the number of vehicles running a red light.

Previous studies confirm that the number of vehicles running a red light at an intersection declines after a RLC is installed (e.g. Martinez and Porter [2006]; Porter et al. [2013]; Erke [2009]; Retting et al. [2003]). For example, Martinez and Porter [2006] use direct observations of driving behavior at eight city intersections and conclude that the incidence of red light running fell by 67% during the eight months immediately after the installation of RLCs. In a follow-up study, Porter et al. [2013] estimate that the incidence of red light running begins to return to the pre-camera levels immediately after the removal of a RLC and that a year after removal the rate of running a red light is similar to before the installation of a RLC.

Tickets issued at intersections that install RLCs also support the prediction that the number of vehicles running a red light decreases after the cameras are installed. In general, the number of tickets issued for running a red light at RLC intersections peak immediately after the installation of the cameras and then begins to decline as drivers learn of the camera and adjust their behavior. Figure 2 plots the average yearly number of citations per intersection by year of operation for the 54 Dallas RLC intersections. In the first year of operation there are, on average, more than 3,000 citations at a RLC monitored intersection. The number of citations drops by almost 50% by the 4th year before appearing to level off.[5]

While there is clear evidence that installing a RLC reduces the number of vehicles running a red light, the predicted relationship between the number of vehicles running red lights and the total number of accidents is ambiguous. The RLC program is likely to decrease some types of accidents while simultaneously increasing others.

We base our discussion of the relationship between the number of intersection accidents and vehicles running a red light on the traffic model of Gazis et al. [1960]. Gazis et al. [1960] model the distance required for a vehicle approaching a traffic intersection to safely decelerate and stop. This distance depends on vehicle (e.g. weight, breaks) and roadway (e.g. surface conditions) engineering characteristics, driver response time, and travel speed. For a given travel speed and set of engineering characteristics, one can determine the minimum distance that the typical driver will need in order to stop before entering

[5]We are unable to produce a similar figure for Houston because we are only able to access intersection level citation reports for the last two years of Houston's RLC program.

the intersection.

The minimum distance to stop does not depend on the length of the yellow phase of the traffic light. The engineering rationale for the yellow phase is that vehicles that are already closer to the intersection than the minimum stopping distance would not be able to safely stop before reaching the intersection. The yellow phase of the traffic safety light can be made arbitrarily long so that all vehicles past this minimum distance have time to pass through the intersection before the light turns red.[6] However, in practice, there is often a "Dilemma Zone" (Gazis et al. [1960]). The Dilemma Zone is the area proximate to an intersection where a driver can neither safely stop, nor pass through the intersection (without accelerating) before the light turns red.

Certain types of accidents are likely to *decrease* when there are cameras. Some drivers who ran a red light before the RLC program will choose to stop at the intersection and fewer vehicles will be in the intersection when the light turns green for the cross street. This is likely to decrease accidents such as right angle crashes between two vehicles. In fact, a reduction in the number of right angle crashes is the primary public safety goal of most RLC programs (Erke [2009]). The size of this reduction will depend on the timing of when vehicles that choose to stop would have been in the intersection. There is some evidence that the vast majority of red light violators occur just after a light turns red and before cross street traffic would have entered the intersection (Yang and Najm [2007]). If this is the case, a RLC program may have only a limited effect on reducing cross street accident collisions.

At the same time, other types of accidents are likely to *increase* under a RLC program. There are at least four reasons why the number of accidents could increase. First, all drivers will now accept a higher accident-related cost from attempting to stop. For example, the marginal driver who was willing to continue through the intersection before the RLC program, will now choose to stop provided that $\frac{\partial S_{i,j,t}}{\partial A_{i,j,t}} < \frac{\partial C_{i,j,t}}{\partial F_{i,j,t}}$. That is, the marginal driver will choose to stop and accept higher expected accident costs provided that these costs are

[6] The Federal Highway Administration recommends that the yellow light interval be between three and six seconds (Administration [2009]).

less than the expected fine. Second, a lengthy transportation and engineering literature documents the role that changes in speed (rather than speed levels) have on accident rates (e.g. Gazis et al. [1960]; Hurwitz et al. [2011]). Even if the driver changing speed can do so safely, other drivers may not be able to react in time to avoid an accident. Notably, neither of these reasons depend on imperfect information or 'non-rational' responses by the driver.

Third, if there is uncertainty over the stopping distance (e.g. poor weather conditions, driver unfamiliarity with the intersection) then the increase in the fine under the RLC program may incentivize drivers to attempt to stop when it would be safer to continue. Fourth, drivers may simply make a calculation mistake. The decision to stop or continue is a split-second decision. For example, knowledge of the cameras (perhaps cued by the posted signs), could lead some drivers' first impulse be to stop even when it would be less dangerous to continue (Kapoor and Magesan [2014]).[7]

The overall effect of the RLC program on the total number of accidents will depend on the relative magnitudes of those accident types that are likely to decrease and those that are likely to increase. One advantage of the accident data discussed in the next section is that all accidents are categorized into a detailed list of accident types. We are able to estimate the effect of a RLC program on total accidents, as well as the effect on specific accident types.

3 Background and Data Sources

3.1 Houston and Dallas RLC Programs

All RLC programs share several common characteristics. A camera is installed at or near to an intersection in a location where it can take photos (or video) of vehicles as they pass through the intersection. The camera is positioned such that photos will show the vehicle passing under the traffic signal as well as the license plate of the vehicle. Photos of all vehicles captured passing

[7] Kapoor and Magesan [2014] show that the introduction of pedestrian crosswalk countdown signals that are also visible to drivers have the unintended effect of greater vehicle accidents.

through the intersection are reviewed by city employees and/or a contractor to verify that the light is red and that the license plate is clearly visible. Traffic tickets are then sent to the home address of the individual who registered the vehicle. The main characteristics on which RLC programs differ are: the type of signage (if any) that advises drivers of a camera at the intersection, whether the cameras are permanently placed at an intersection or are mobile units, and if the cameras also monitor vehicle speed and issue speeding tickets.

Houston first approved the installation of red light cameras in 2004 and installed 20 cameras in 2006 and 46 in 2007 (Hassan [2006]). Approximately 800,000 $75 tickets were issued from 2006 to 2010 for a total of about $44 million collected (Olson [2010]).[8] The first 33 Dallas cameras were installed in 2007, along with 22 more between 2008-2011. The Dallas RLC program remained in place throughout our panel. The Houston and Dallas programs were alike in that both programs posted signs advising drivers of the cameras, had permanently placed cameras, and only issued tickets for red light infractions and not for speeding.

Houston residents voted 53% to 47% in favor of a referendum to remove the cameras on November 2, 2010. The referendum was organized by citizens who opposed the RLC program on the grounds that the program was mainly a revenue-raising policy. At the time of the referendum, the majority of members on the Houston City Council approved of the RLC program, as did the Houston Police Department (Board [2010]; Olson [2010]; Oaklander [2011]). After the voter referendum, Houston immediately shut off the cameras and began legal proceedings with the private sub-contractor that administered the program (Jensen [2010]). In July 2011, a judge ruled that Houston had breached its contract (which was set to run through 2014) and the cameras were briefly turned back on. One month later, the Houston City Council voted to repeal the original law that authorized the usage of the cameras (Garrett [2011]). All lawsuits related to the removal of the red light cameras were settled by January 2012 (Houston Mayor's Office [2012]).

[8] Approximately a quarter of fines were not paid.

3.2 Data Sources

3.2.1 Intersection Information

We use information on RLC intersections from TxDOT's annual red light camera enforced intersection reports (Texas Department of Transportation [2009-16]). The earliest available reports are from 2009. These reports are compiled and published by the state of Texas using information submitted by municipalities. Each municipality with a red light camera program is required to submit annual information on each camera, including: the date of installation, intersection speed limits, total tickets issued, and an estimate for the average daily traffic (ADT).

We also collect ADT information from two other sources that provide traffic counts in Houston and Dallas at numerous street locations (City of Houston [2017] and North Central Texas Council of Goverments [2016]). The rationale for using this 2nd source is so we will have a consistent ADT measure for both RLC and non-RLC intersections in each city. Notably, the traffic count information is not always collected at an intersection. Intersections are assigned ADT values using GIS software by summing the ADT values for all roads at the intersection.[9] Importantly, we are able to compare the intersection ADT data using this measure with the ADT data reported by TxDOT for RLC intersections. The ADT means are very similar though there is some variation for individual intersections.

Finally, we collect information on a number of structural intersection characteristics including whether one or more of the streets at the intersection is a divided road (i.e. has a median separating traffic), the speed limit, and number of lanes, and whether the intersection includes a frontage road. A frontage road is defined as a road running parallel to a highway that is often used as an access point to the highway.[10]

[9] Please refer to the Appendix for details.

[10] Intersection characteristics were collected using Google Maps and Google Mapmaker from June-July 2016. The dates of the images used to collect the data roughly match the end of our panel period.

3.2.2 Vehicle Accidents

The accident data cover 2003-2014 and are from the Texas Department of Transportation's (TxDOT) Crash Records Information System (CRIS) which includes all reported motor vehicle traffic crashes in Texas that incurred at least $1,000 in estimated damage (Texas Department of Transportation [2004-16]).[11] The accident data retained in CRIS are from crash reports filled out by law enforcement personnel. CRIS includes information on the location of each accident (latitude and longitude coordinates), type of accident (e.g. right angle crash), driver demographic information (e.g. zip code of vehicle registration), driver behavioral information (e.g. drugs or alcohol detected, whether the driver ran a red light), accident injury information, and the weather at the time of the accident. The 2010-2014 CRIS data include the month and year of the accident, while the earlier data include only the year.

We use GIS software to identify accidents that occur within 200 feet of all Houston intersections and all Dallas RLC intersections (and are on one of the roads that cross at the intersection). Recall that Figure 1 shows much higher accident rates within 200 feet of an intersection. We further restrict our sample to those accidents where law enforcement personnel indicated that the accident was *in or related to* an intersection. We define these accidents as "intersection accidents". We only include intersection accidents in our main estimation panels.

Figure 3 plots the average total number of vehicle accidents per intersection by year from 2003-2014. Panel A plots accident levels for RLC intersections in our study by year and city of installation, as well as Houston intersections with ADT data and at least one accident during our panel that did not have a RLC. Panel B plots accident levels for two groups of intersections in San Antonio, a city without a RLC program. We separately plot the 66 most dangerous intersections from 2003, along with all other San Antonio intersections with

[11] The 2010-2014 data were downloaded via the TxDOT online database. CRIS data prior to 2010 are no longer retained by TxDOT. CRIS data for the years 2003 and 2009 were obtained via a Freedom of Information Act request from The University of Texas at Austin Center for Transportation Research.

ADT data and at least one accident during our panel. The most dangerous intersections are determined by assigning each intersection a risk score based on the weighted average of the number of deaths, incapacitating injuries, non-incapacitating injuries, and non-injury accidents from 2003.[12] We normalize the accident levels in the figure around zero for each group by subtracting the group mean from each yearly point. This is equivalent to plotting the residuals from an OLS regression that includes a vector of intersection fixed effects as the only independent variables.

Panel A provides initial evidence on whether the introduction of RLCs in Houston and Dallas, and the subsequent removal of the Houston RLCs, had any effect on the number of total accidents. If the RLC programs are effective at reducing accidents, then we would expect to see a reduction in the number of accidents beginning in the year after cameras are installed (and perhaps during the year of installation). The figure shows no clear trend break at the time of the camera installation for any of the three RLC groups. The average number of intersection accidents peaks in 2003 for both Houston RLC groups, and then decreases at roughly a constant rate from 2005-2008. Overall, the Dallas RLC and Houston No RLC groups exhibit a small downward trend during the beginning of the time period followed by a similarly sized upward trend during the last five years of the panel. There is also no clear evidence that ending the RLC program in 2010 led to an increase in the number of accidents. The timing of the increase for the two Houston RLC groups does not match the end date of the program. Moreover, the overall increase towards the end of the panel is similar in magnitude to that of the Dallas RLC group (where the RLC program continued to operate).

Panel A also shows two other facts regarding the Houston RLC intersections. First, on average, the Houston RLC intersections are more dangerous than the Houston non-RLC intersections. The average number of total accidents during this period is about four times larger at Houston RLC intersections. Second, the Houston RLC locations appear to have been chosen based

[12]This weighting scheme is the same as that used to evaluate intersections by Stein et al. [2006], except that it is applied only to accidents from one year. See Appendix for details.

on an unusually large number of accidents in the years prior to the RLC program, and in particular, the number of accidents in 2003. This conclusion is supported by a memo to the then Chief of Police in early 2006 in which Stein et al. [2006] advise against using the "Houston Police Department 2003 database" to select RLC intersections, as a "longer time period will provide more reliable information on collision causes" (p1).[13]

Panel B shows that the most dangerous intersections in San Antonio from 2003 display a very similar accident pattern as the Houston RLCs despite the fact that San Antonio never had a RLC program. Together Figure 3 Panels A and B highlight the challenge in evaluating the effect of RLC programs when RLC intersections are positively selected on the number of accidents. A simple difference-in-differences model based around the start of the Houston RLC program would over-estimate its effectiveness at reducing accidents relative to the Houston No RLC group.[14] For this reason, our focus is on the unexpected removal of the cameras. We are also careful to construct a control group of intersections to use as a counterfactual comparison in our difference-in-difference model.

4 Selecting the Samples

We run two main empirical models. The first model estimates the likelihood that an intersection receives a Houston red light camera. Below we discuss how we use the red light camera propensity score estimates from the first model to select our treatment and control groups. The second model, as discussed in Section 5.1, is a difference-in-differences model that exploits the timing of the referendum that shut off the Houston cameras to estimate the causal effect of red light cameras on traffic accidents, injury accidents, and traffic

[13]Stein et al. [2006] were asked by the Houston Police Department to recommend potential intersections for RLCs, and provided a list of 100 intersections based on three years of accident data. Only six of these intersections were selected.

[14]An "Empirical Bayes" research design, the estimation approach preferred in the transportation and engineering literature (see Footnote 3), would also over-estimate the effectiveness of the RLC program.

patterns. Our preferred difference-in-difference estimates use a poisson model and reweight the sample using the propensity scores.

The intersections considered for our estimating sample in our differences-in-differences model are summarized in Table 1. Our treatment group is comprised of all Houston RLC intersections. We use three control groups. The first two control groups use Houston intersections that never had a RLC and which meet our screening criteria. The difference between the two Houston control groups is the time period used to select the final samples: pre-referendum years 2008-2010 (Panel A) versus pre-RLC program years 2003-2005 (Panel C). Hereafter we sometimes refer to these two groups as part of the "Houston sample" and the "Houston 2003-2014 sample", respectively. RLC intersections in the city of Dallas make up the third control group (Panel B). The Dallas RLC intersections are not subject to the referendum (hereafter referred to as the "Houston-Dallas sample").

The screening criteria for the within Houston control groups are as follows. First, the control intersection must have at least one intersection-related accident from 2003-2014. We condition on having at least one accident so as to rule out minor, infrequently traveled intersections. This restriction may also exclude intersections that, for whatever reason, appear to be extremely safe and are thus not comparable to RLC intersections. Second, the control intersection can not be within half a mile of a RLC intersection. Previous research has suggested that driving behavioral responses to a RLC intersection could affect driving behavior at other intersections that are within close proximity (Høye [2013]; Shin and Washington [2007] Wong [2014]). We further require that the intersection have non-missing ADT data for each direction at the intersection for the 2008-2010 based Houston sample and the Houston-Dallas sample (ADT data are not available during 2003-2005). ADT data allows us to control for vehicle traffic levels. We also use the ADT data to test whether traffic patterns at RLC intersections change after the installation of a camera.

Next we run a logit model to estimate the likelihood that an intersection would be assigned a Houston RLC. As described in further detail below, we use the propensity score estimates from the logit model to determine our final treatment and control samples. We specify our preferred logit model as

$$y_i = \alpha + A_{i,t}\gamma + u_i, \tag{3}$$

where the dependent variable $y_i \in (0, 1)$ is the estimated probability that intersection i is a Houston RLC intersection. $A_{i,t}$ is a vector of pre-referendum intersection traffic accident information, α is an intercept, and u_i is an error term which is assumed to have a standard logistic distribution. The pre-referendum years are 2003-2005 and 2008-2010 for the two Houston samples, and 2008-2010 for the Houston-Dallas sample. The variables included in the vector $A_{i,t}$ are motivated by the previous literature and by documents that outline the RLC intersection selection process (Chi [2016]; Stein et al. [2006]). $A_{i,t}$ includes the yearly accident rate at the intersection for each the pre-referendum year t, for: right angle, not right angle, and injury accidents. $A_{i,t}$ also includes a variable for red light related accidents for each pre-referendum year for the Houston sample, and one ADT observation for the 2008-2014 Houston sample.[15] $\hat{y}_i$ corresponds to each intersection's estimated likelihood, or propensity score, of being a Houston RLC intersection (Rosenbaum and Rubin [1983]). The propensity score for the Houston-Dallas sample represents the probability that a intersection with those characteristics would be in Houston as opposed to Dallas.

We use the propensity score to trim the treatment and control groups in each of our samples. We follow Imbens and Wooldridge [2007] and use a simple 0.1 rule to drop observations from our sample if the propensity score is outside of the interval [0.1, 0.9]. Figure 4 shows the distribution of propensity scores in the Houston sample (top panel), the Houston-Dallas sample (middle panel), and the Houston 2003-2014 sample (bottom panel). Each panel plots

[15]The 2010 data do not include accidents from November and December (and thus only include accidents before the referendum). ADT is not included in $A_{i,t}$ when running the 2003-2014 sample because no observations are available for the 2003-2005 time period. We use a more parsimonious logit model for the Houston-Dallas sample that excludes the ADT and red light running variables, since the two samples are relatively balanced before trimming and there are already fewer Dallas RLC intersections than Houston RLC intersections. Our difference-in-difference model estimates are similar when we use other logit specifications to select the estimation samples (although the sample sizes are smaller).

the fraction of observations in the control (black bar) and treatment (grey bar) groups that fall within 5 percentage point propensity score bins. The leftmost bin is for observations with propensity scores ranging from 0.10 to 0.15, while the rightmost bin is for observations with scores from 0.85 to 0.90. The overlap in the propensity scores for the treatment and control intersections is best in Panel A, which is one reason why the Houston sample is our preferred sample.

Table 1 shows how intersection accident and traffic characteristics vary between our control and treatment groups before and after the sample is trimmed using the propensity score. The top panel displays intersection characteristics for the Houston sample, the middle panel for the Houston-Dallas sample, and the bottom panel for the Houston sample. Columns (1) and (2) show the mean intersection characteristic values for the pre-trimmed treatment and control samples, respectively. Column (3) shows the difference in means between treatment and control groups, normalized by the standard deviation of the characteristic. This approach to evaluating the differences in means between the control and treatment groups allows for a comparison that is not affected by the sample size of the groups (Imbens and Wooldridge [2007]). We follow Imbens and Wooldridge [2007] and consider the sample to be well-balanced for a characteristic if the difference is less than 0.25 standard deviations. Columns (4)-(6) repeat the same format as the first three columns for the propensity score trimmed samples.

The Houston sample is not well-balanced in any of the accident characteristics before trimming. The non-trimmed Houston-Dallas sample that already limits the analysis to RLC intersections is much better balanced than the non-trimmed Houston sample, although still significantly differs on 5 of the 6 accident characteristics. After trimming with the propensity score, four of the six accident characteristics are well-balanced in the Houston sample. The worst-balanced sample for accident characteristics, both before and after trimming, is the Houston 2003-2014 sample. The Appendix shows that the two trimmed Houston samples also have reasonable geographic balance in the location of the treatment and control intersections.

There are greater differences in the intersection engineering characteristics. Recall that the engineering characteristics are not measured in the pre-referendum period and not included in the propensity score matching model. Nevertheless, the magnitude difference for the engineering characteristics between control and treatment intersections is generally not large in absolute terms (e.g. the speed limit is about 3 miles per hour greater for the treatment group). The one exception is whether an intersection is a frontage road. 82% of the red light camera intersections in Houston are on frontage roads. In robustness analysis (Section 5.5) we consider a sample that only evaluates intersections on frontage roads.

Figure 5 shows intersection level accident trends for the treatment and control groups for both the Houston (left column) and Houston-Dallas (right column) samples. The accident data plotted are the residuals from an OLS regression that includes a vector of intersection fixed effects as the only independent variables. The figures can be read as the mean accident rate for each year. Row 1 plots angle accidents, row 2 plots non-angle accidents, and row 3 plots injury accidents. For example, the upper left figure plots the average number of yearly angle accidents for a Houston RLC intersection (squares) and a Houston control intersection (triangles) that can not be explained by characteristics at each intersection that are fixed over time during our sample (e.g. speed limit, ADT, visibility, etc.). The number of angle accidents are slightly higher fo the non-RLC intersections than for the RLC intersections and trend the same for the two groups for the three years before the referendum.

5 The Effect of Introducing and Removing Red Light Cameras

5.1 Difference-in-Differences Model

We specify our baseline model as

$$y_{i,t} = \beta_0 + \beta_1 T_i + \beta_2 R_t + \beta_3 C_t + \delta_1 T_i * R_t + \delta_2 T_i * C_t + \alpha_i + v_t + \varepsilon_{i,t}, \qquad (4)$$

where $y_{i,t}$ is a particular outcome for intersection i in year t. The outcomes we focus on in the paper are total accidents, type of accident (right angle, non-right angle), whether the accident results in an injury, and ADT at the intersection. T_i is an indicator variable that equals one if the intersection is in Houston and receives a red light camera. R_t is a post-referendum indicator variable that equals one if the panel observation is from 2011-2014. C_t is an indicator for when the cameras are active (2006-2010). The model allows for two different treatment effects: when the cameras are shut off, δ_1, and when the cameras are turned on, δ_2. The model controls for intersection fixed effects α_i and year fixed effects v_t. Standard errors are robust to heteroskedasticity and are clustered at the intersection level.

Recall that we estimate Equation 4 on three main samples. In the Houston and Houston-Dallas samples we only estimate the effect of the referendum (the terms with C_t are dropped from the model). These samples consider the pre-referendum period while the Houston RLCs are operating as pre-treatment and select the control groups using intersection accident characteristics from 2008-2010. In the Houston 2003-2014 sample, we are able to estimate both treatment effects, and the control group of intersections is selected using accident characteristics from the pre-RLC period (2003-2005).

Table 1 shows that, overall, the accident characteristics are well-balanced in both of the trimmed Houston-Dallas and Houston estimation samples. Nevertheless, there are some differences in the means between treatment and control intersections. For this reason, we also estimate versions of Equation (4) for each outcome where we weight the regression by the inverse of the propensity score (Hirano et al. [2003]). If the propensity score correctly predicts the probability of treatment (defined as a Houston intersection with a RLC), then weighting the regression will balance the composition of the covariates that determine treatment.

The key identifying assumption (for δ_1) is that the post-referendum trend for the dependent variable (e.g. angle accidents) for the control intersections is a valid counterfactual for what would have occurred at Houston RLC intersections had there been no referendum. The similar pre-referendum trends shown in Figure 5 provide support for this assumption.

A specific concern regarding the identifying assumption is that having a RLC program could alter driving behavior in the city at non-RLC intersections. Economic theory predicts that some drivers will engage in averting behavior. For example, the longer expected travel times on roads with RLCs, along with the higher likelihood of a fine, may lead some drivers to avoid traveling through the RLC intersections. If this is the case, then the shift in traffic would likely lead to more accidents at non-RLC intersections. The estimated effect of the RLC program would be biased towards finding that the program is successful (i.e. a larger reduction in accidents when the RLCs are turned on, and a larger increase in accidents when they are removed). In fact, we find suggestive evidence that average daily traffic at RLC intersections decreases relative to non-RLC intersections during the program (Section 5.4). As such, our estimates from our Houston samples should be viewed as an upper bound on the number of accidents prevented during the RLC program.

Finally, as we report below in Section 5.2, our difference-in-differences estimation results are similar regardless of whether we use a control group of intersections inside the city of Houston or outside the city (in Dallas). The similarity of findings lends support to the identifying assumption, since the Dallas RLC intersections are not subject to the same concerns as the Houston non-RLC intersections.

5.2 Traffic Accidents

Table 2 shows the difference-in-difference coefficient of interest for the effect of ending the RLC program on accident levels (δ_1) for 12 separate regressions. There are four panels in the table. The top two panels estimate the difference-in-differences model (Panel A) and the propensity score weighted difference-in-differences model (Panel B). The bottom two panels estimate the same models for the Houston-Dallas sample. We estimate each model separately for angle (column 1), non-angle (column 2), and total accidents (column 3).

There is no evidence that the RLCs reduced the total number of accidents.

If the RLC program had been effective at reducing the number of accidents, then when the program ended we would expect to observe an *increase* in the number of accidents. In our preferred model (Panel B) we estimate a positive point estimate for angle crashes (12%) and a negative point estimate for all other types of accidents (-14%). The net effect is an estimate of -5%. The point estimates for the effect on total accidents in the four models range from -19% to -5%. All of the estimates are statistically insignificant.

5.3 Injury Accidents

Injuries are classified by TxDOT into five categories: unknown, possible injury, non-incapacitating, incapacitating, and death. We define an "injury accident" as an accident with at least one reported injury or death (i.e. excluding the unknown and possible categories). We define a "minor injury" accident as one where there is at least one reported non-incapacitating injury and no incapacitating injuries or deaths. We separate differing types of injuries to account for the large difference in the economic costs caused by different levels of injury (e.g. Shin and Washington [2007]).

Table 3 shows estimation results for the effect of ending the RLC program on the number of accident related injuries. The top two panels estimate Equation 4 on the main Houston sample, while the bottom two panels estimate the same models on the Houston-Dallas sample. Column (1) considers injury accidents, while column (2) considers minor injury accidents. These accident measures do not reflect the fact that accidents with multiple people injured are more harmful than accidents where only one person is injured. Columns (3) and (4) change the dependent variable of the model to be the number of monthly reported accident-related injuries for each intersection. Column (3) considers all injury types, while column (4) only counts non-incapacitating injuries.

We estimate that the change in the number of people injured and the change in the number of injury accidents are negative in both samples after weighting by the propensity score. None of the estimates are statistically significant. While the estimated percent change is economically large in some models, the overall change in the number of injury accidents is modest. For example, a decline of 16 percent in injury accidents (Panel B, Column 1) corresponds to a decrease of approximately 15 injury accidents per year across all RLC intersections in Houston after the RLC program ends. The upper bound of the 95% confidence interval would imply that ending the RLC program led to 22 more injury accidents per year.[16]

5.4 Average Daily Traffic

The installation of cameras could lead drivers to change where they drive in addition to how they drive. Drivers may choose to alter their driving routes to avoid intersections with RLCs as a means to save time or to avoid fines. Table 4 provides some evidence on how average daily traffic at an intersection changes after the referendum banning RLCs.

We estimate a simple difference-in-differences model (Equation 4 without the fixed effects) using an OLS model for the subset of intersections in our Houston sample that have both a pre-referendum and post-referendum ADT observation.[17] We use the same intersection propensity score weights as those used in the accident analysis. All four point estimates are positive and those for the Houston model are statistically significant. The estimates from the Houston propensity score weighted regression implies an approximate increase of 39%. The model estimates are positive, somewhat smaller, and less precise when intersection fixed effects are included in the model (not shown).

We interpret these estimates as suggestive evidence that there was a shift in driving patterns. Practically speaking, an increase in traffic at treatment intersections after the referendum would imply an upward bias on the accident

[16]We calculate the change in the implied number of accidents by taking the product of the point estimate in Panel B, Column 1 (or the 95% upper bound), the yearly mean for all the treated intersections in the sample (1.33), and the number of RLC intersections (66). Note that we apply our estimates from the subset of RLCs in our sample to the entire RLC program.

[17]We only have one ADT observation before and after the referendum. The year of a pre-referendum (2007-2010) and post-referendum (2011-2014) observation is not always the same across intersections.

estimates in Section 5.2 (the positive accident point estimates would overestimate the true effect, while the negative point estimates would be an underestimate and biased towards zero). However, there are a number of caveats to the ADT estimates including: ADT is not measured in the same years for all intersections, the data are only available for a subsample of intersections in Houston, and there is no way to observe whether the ADT trends are similar between treatment and control intersections prior to the referendum.

5.5 Robustness Analysis

5.5.1 The Effect on Accidents and Injuries from Starting the RLC Program

Our focus is on estimating the effect of the exogenous removal of the RLCs so as to avoid endogeneity concerns over the start time of the intersections selected for RLCs. Nevertheless, we can also use our modeling framework to evaluate the effect on traffic accidents and injuries when the RLCs are first installed. The model in Section 2 predicts that angle accidents will decrease and non-angle accidents increase after the cameras are installed and have the opposite effect when the cameras are removed. The estimates in Table 5 confirm this pattern.

We use the Houston 2003-2014 sample which allows us to examine the effect of both the installation and removal of the cameras. Table 5 panels A and B show the estimated installation treatment effect (δ_2) from Equation 4, while panels C and D show the removal treatment effect (δ_1). The pre-trimmed Houston intersections are the same as in our main Houston sample. However, the final control and treatment intersections are selected based on pre-program accident characteristics from 2003-2005. We emphasize the propensity score weighted results since the accident characteristics and propensity score overlap (Panel C of Table 1 and Figure 5) are not as well-balanced as in our main Houston sample that matches on pre-referendum characteristics from 2008-2010. Finally, the estimates in Table 5 are from an unbalanced panel that drops data from the year of installation for RLC intersections.[18] The results are similar to those that use balanced panels (Table 6 panels E and F).

We estimate that installing the cameras leads to 6% fewer angle accidents and 21% more non-angle accidents, while removing the cameras increases angle accidents by 19% and reduces non-angle accidents by 5%. However, the point estimates are imprecise and only the increase in non-angle accidents when cameras are installed is statistically significant. The estimated net effect is a (statistically insignificant) 12% increase in total accidents when cameras are installed. The injury accident point estimate is less intuitive and suggests a decrease in injury accidents after the camera installation. One possibility is that angle accidents are more dangerous and even an increase in the total number of accidents may be welfare improving provided that there are fewer angle accidents. We explore the potential welfare implications in Section **??**.

5.5.2 Alternative Specifications

Table 6 shows six robustness specifications. Each panel in the table shows coefficient estimates from estimating Equation 4 using our propensity score weighted model.

The relevant comparison for the first four panels is the removal estimate for our main Houston sample (Table 2, panel B). Panel A shows OLS estimates that imply percentage change effects very similar to those using the Poisson model. Panel B drops 2011 accidents from our analysis. The Houston RLCs were temporarily turned back on for one month in 2011 in response to a court ruling that Houston had breached its contract with a private RLC service company by turning off the cameras. The results are similar regardless of whether

[18] The RLC intersections were installed in 2006 and 2007. There is no way to correctly assign the observation as pre- or post-treatment, as the exact installation dates in each year are unknown (and the accident data before 2010 are aggregated by year). Thus, we estimate δ_2 from an unbalanced panel. The RLCs installed in 2006 have three pre-installation years (2003-2005) and four post-installation years (2007-2010), while those installed in 2007 have four pre-installation years (2003-2006) and three post-installation years (2008-2010). Further, all non-RLC control intersections have one additional observation. Similarly, δ_1 is also estimated from an unbalanced panel. The 2006 installed cameras include pre-removal data from 2007-2010, while the 2007 installed cameras include pre-removal data from 2008-2010.

we include 2011 data in our post-referendum period. Panel C estimates the model on a trimmed sample of frontage road intersections. Recall that whether an intersection is located on a frontage road is the characteristic that differed most between RLC and non-RLC intersections in our main sample (Table 1, panel A). The frontage road estimates are close to zero for both angle and non-angles accidents, but very imprecise due to a much smaller sample. Panel D explores what happens if we ignore whether an accident was determined by police to be "in or related" to an intersection and instead only use the criteria that the accident is within 200 feet of an intersection. The estimated point estimate for angle accidents is similar, but the effect for non-angle accidents appears to be biased upwards.

Table 6 panels E and F estimate the effect of installing and removing the red light cameras using a balanced sample. In Table 5 panels B and D we estimate the same model on an unbalanced sample. The Houston cameras are installed in both 2006 and 2007. In the balanced panel, we drop observations from 2006 and 2007 for all intersections (both RLC and non-RLC), and estimate the installation treatment effect from a balanced panel with 2003-2005 as pre-installation period and 2008-2010 as post-installation. The removal treatment effect is estimated from a balanced panel with 2008-2010 as the pre-removal period and 2011-2014 as post-removal. The accident and injury estimates are similar in sign and magnitude in both the balanced and unbalanced panels.

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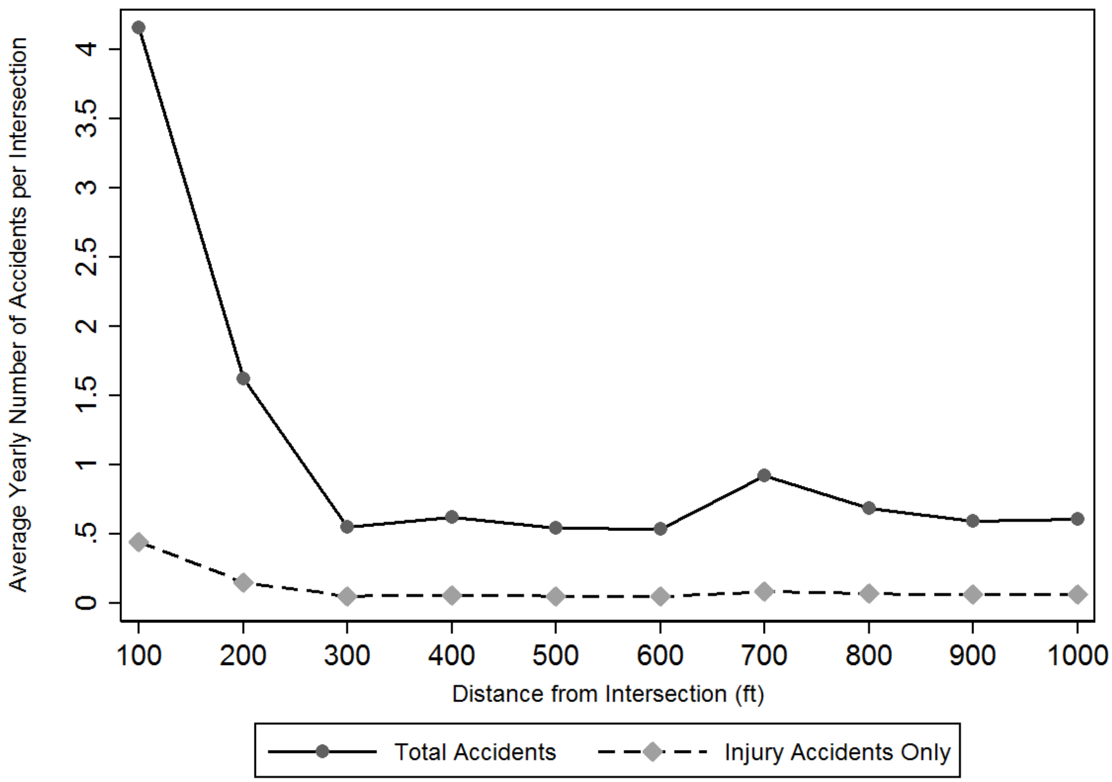
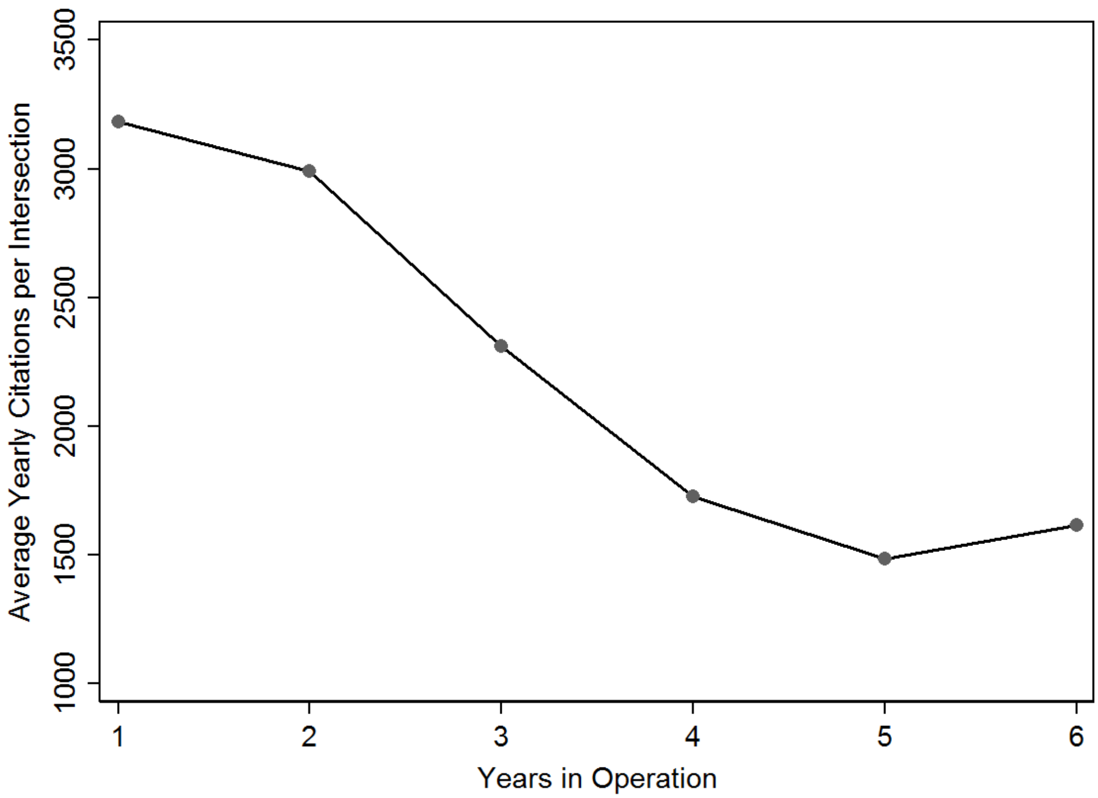
7 Figures and Tables

Figure 1: Yearly Total Accident and Injury Accident Rates at Different Distances from an Urban Street Intersection



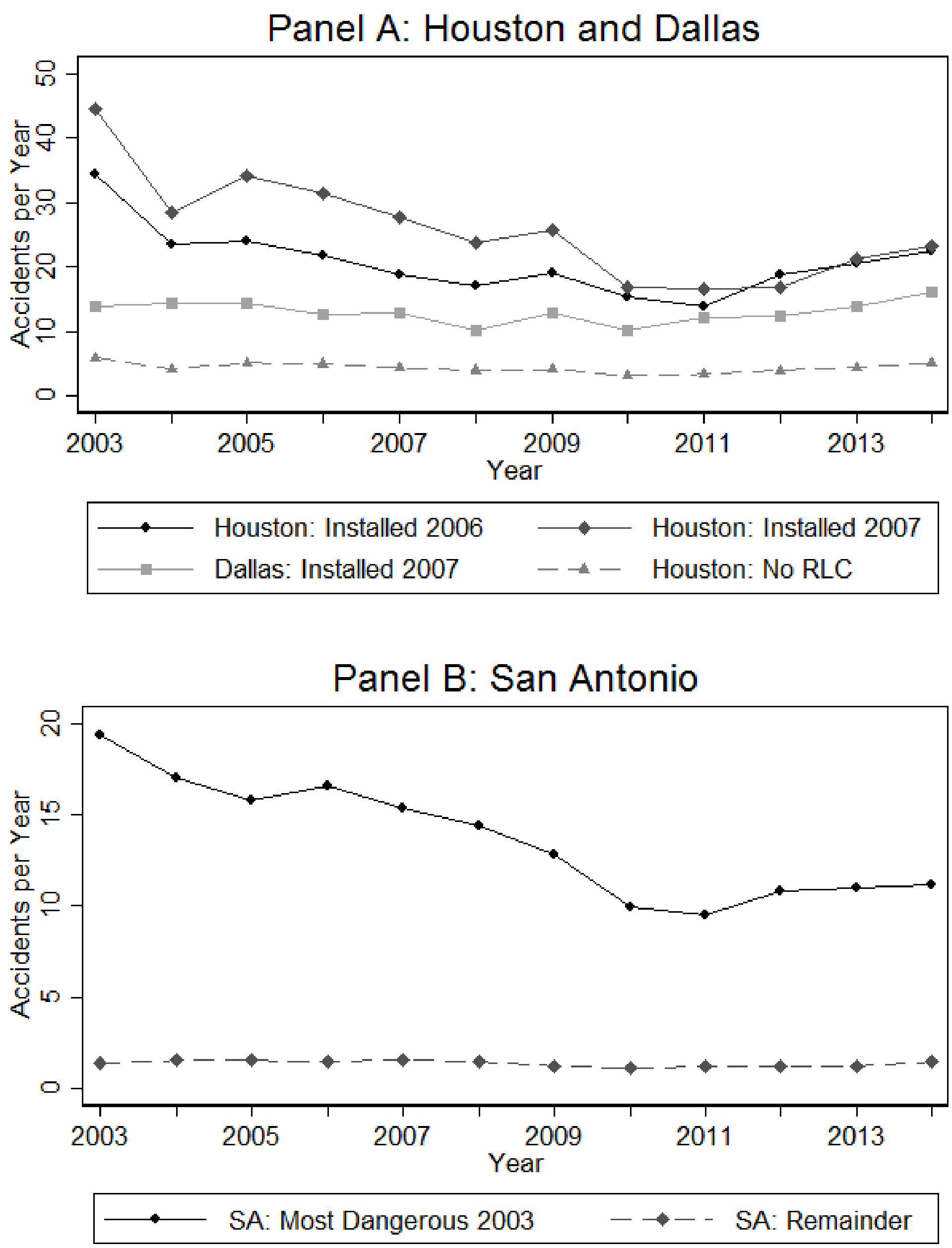
The figure plots average yearly total accidents and injury accidents by distance from a Houston intersection in 100 foot bins for the years 2003-2014. The data include all accidents classified as "in or related" to the intersection by the police who recorded the accident. An injury accident is an accident with at least one non-incapacitating (minor) injury, incapacitating injury, or death. Data source: Texas Department of Transportation.

Figure 2: Red Light Camera Yearly Citation Rate
by Year of Camera Operation



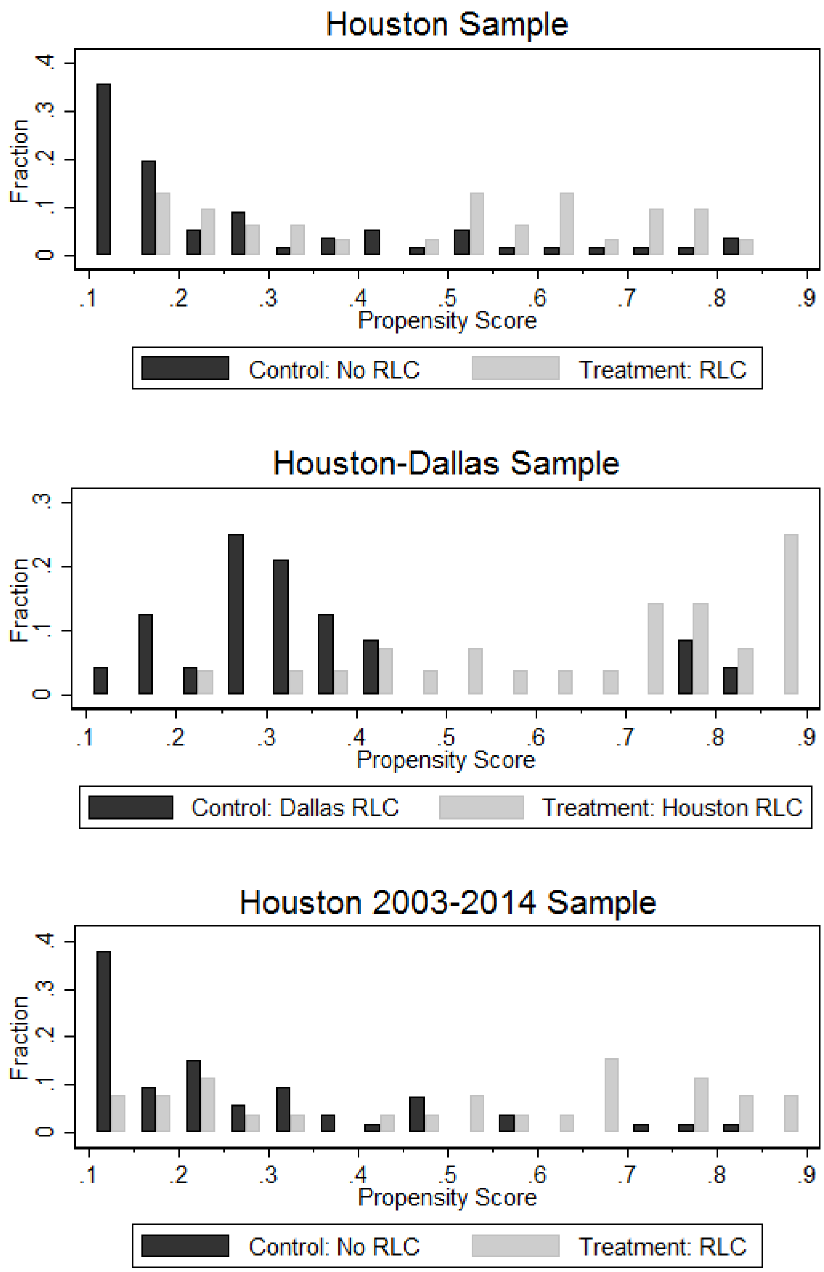
This figure shows the yearly rate of red light camera tickets issued for Dallas red light cameras by the duration since camera installation. The data cover the years 2009-2014. Data source: City of Dallas.

Figure 3: Intersection Vehicle Accident Trends by Date of Red Light Camera Installation and City



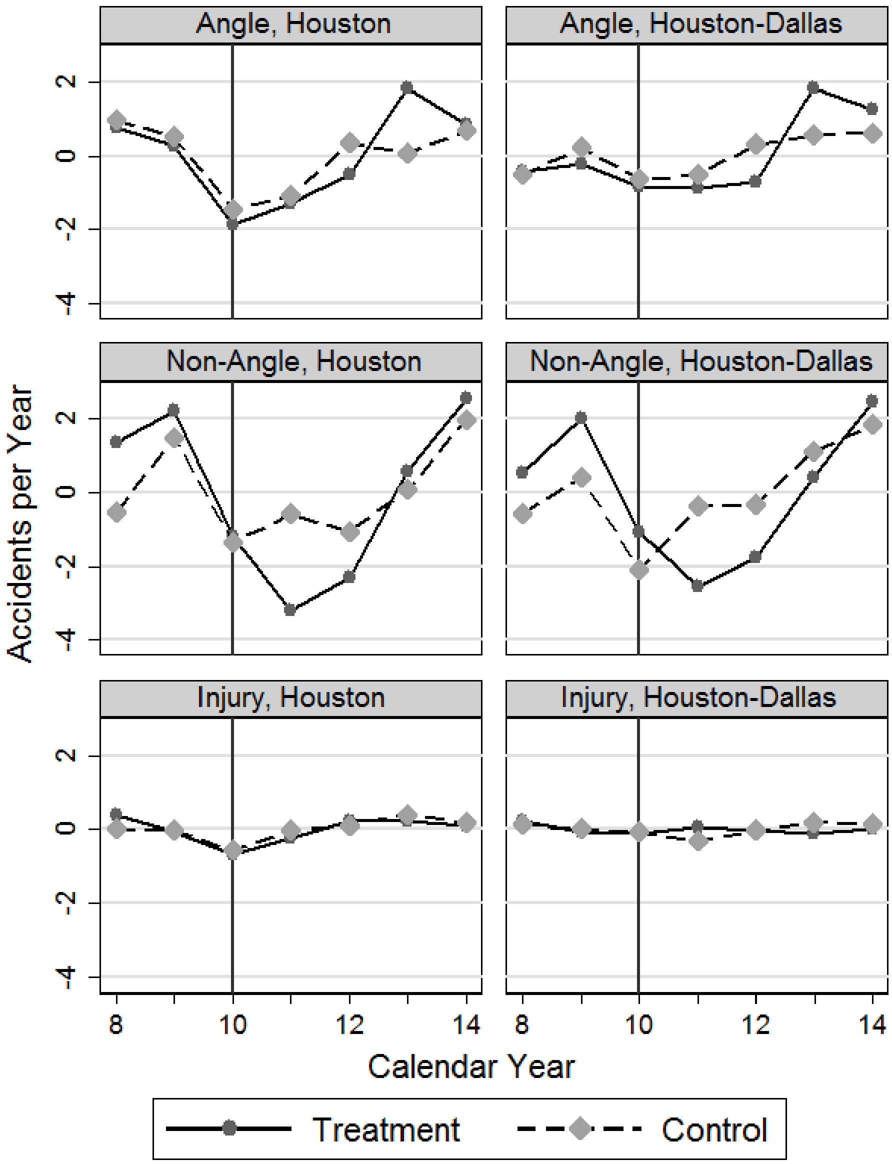
Panel A shows the trends in yearly intersection traffic accidents from 2003 to 2014 in Houston and Dallas for four groups of intersections based on the year of RLC installation and city. Panel B shows accident rates separately for the 66 most dangerous San Antonio intersections (equal to the number of Houston RLC intersections from 2006 and 2007) and all other intersections. The most dangerous intersections are determined by assigning each San Antonio intersection a risk score based on the weighted average of the number of deaths, incapacitating injuries, non-incapacitating injuries, and non-injury accidents from 2003. The data include all accidents within 200 feet from one of the intersections that are classified as "in or related" to the intersection by the police who recorded the accident. Data Source: Texas Department of Transportation.

Figure 4: Distribution of Trimmed Propensity Scores



The figure shows the distribution of propensity scores in the Houston 2008-2014 sample (top panel), the Houston-Dallas 2008-2014 sample (middle panel), and the Houston 2003-2014 sample (bottom panel). The propensity scores are estimated by logistic regression using Equation 3 (see text for details). Each panel plots the fraction of observations in the control (black bar) and treatment (grey bar) groups that fall within 5 percentage point propensity score bins. The leftmost bin is for observations with propensity scores ranging from 0.10 to 0.15, while the rightmost bin is for observations with scores from 0.85 to 0.90.

Figure 5: Yearly Accident Trends for Angle, Non-Angle, and Injury Accidents by Treatment and Control Intersections



The figure plots yearly accident residuals from an OLS regression of yearly angle (1st row), non-angle (2nd row), and injury (3rd row) accidents on a vector of intersection fixed effects. The residuals are plotted separately for the control and treatment intersections. Treatment and control intersections in the Houston sample (left column) are Houston red light camera (RLC) and propensity score matched non-RLC intersections, respectively. Treatment and control intersections in the Houston-Dallas sample (right column) are Houston RLC intersections and Dallas RLC intersections. Data source: Texas Department of Transportation.

Table 1: Sample Accident Intersection Characteristics

	All Intersections			All Intersections, Trimmed		
	(1)	(2)	(3)	(4)	(5)	(6)
	Treatment	Control	Difference/SD	Treatment	Control	Difference/SD
Panel A: Houston Control (2008-2010)						
Accident Characteristics						
Total Accidents	20.64	3.74	2.32	16.40	13.13	0.47
Angle Accidents	7.75	1.33	1.92	4.89	4.70	0.05
Non-Angle Accidents	12.89	2.41	2.22	11.51	8.43	0.54
Injury Accidents	1.89	0.40	1.43	1.33	1.21	0.09
Red Light Running Accidents	5.88	0.82	1.89	3.38	3.34	0.01
Average Daily Traffic	58,540	31,647	1.41	60,153	53,566	0.22
Intersection Characteristics						
Frontage Road	0.82	0.04	2.65	0.77	0.13	1.35
Lanes	7.36	5.54	1.07	7.16	7.29	-0.09
Speed Limit	39.93	34.17	0.96	39.94	37.46	0.41
Divided	0.92	0.71	0.46	1.00	0.95	0.29
Number of Intersections	66	859		31	56	
Panel B: Houston-Dallas Control (2008-2010)						
Accident Characteristics						
Total Accidents	20.64	11.07	0.69	13.11	10.21	0.21
Angle Accidents	7.75	2.90	0.67	4.20	2.75	0.20
Non-Angle Accidents	12.89	8.17	0.55	8.90	7.46	0.17
Injury Accidents	1.89	1.43	0.23	1.21	1.18	0.02
Red Light Running Accidents	5.88	2.58	0.55	2.98	2.42	0.09
Average Daily Traffic	58,540	43,881	0.54	60,759	42,175	0.69
Intersection Characteristics						
Frontage Road	0.82	0.36	0.96	0.75	0.38	0.79
Lanes	7.36	7.48	-0.08	7.21	7.42	-0.13
Speed Limit	39.93	34.98	0.85	39.75	34.48	0.90
Divided	0.92	0.84	0.27	0.89	0.83	0.22
Number of Intersections	66	33		28	24	
Panel C: Houston Control (2003-2005)						
Accident Characteristics						
Total Accidents	33.12	5.06	2.60	26.64	20.12	0.68
Angle Accidents	14.86	2.06	2.35	9.95	8.47	0.25
Non-Angle Accidents	18.26	3.00	2.40	16.69	11.65	0.63
Injury Accidents	3.25	0.51	2.03	2.78	2.28	0.30
Red Light Running Accidents	11.39	1.39	2.35	7.36	6.30	0.24
Intersection Characteristics						
Average Daily Traffic	58,540	31,647	1.41	50,932	47,737	0.16
Frontage Road	0.82	0.04	2.65	0.62	0.23	0.81
Lanes	7.36	5.54	1.07	7.04	7.38	-0.22
Speed Limit	39.93	34.17	0.96	38.87	37.00	0.33
Divided	0.92	0.71	0.46	0.88	0.94	-0.22
Number of Intersections	66	859		26	53	

The table shows the means for accident and intersection characteristics for three samples. Houston RLC intersections are the treatment group for all three samples. The control groups are: Houston non-RLC intersections (Panels A and C) and Dallas RLC intersections (Panel B). The means are taken over the years indicated for each sample. Columns (1)-(3) describe all observations with no propensity score trimming. Columns (4)-(6) show the comparison for our trimmed samples. Data sources: City of Houston, Google maps, North Central Texas Council of Governments, Texas Department of Transportation.

Table 2: The Effect on Accidents from Ending the Red Light Camera Program

Dependent Variable:	(1) Angle	(2) Non-Angle	(3) Total
Houston Sample			
Panel A: Poisson Model			
*After Removal * Treated*	.100	-.157	-.073
	(.152)	(.098)	(.101)
Panel B: Weighted Poisson Model			
*After Removal * Treated*	.118	-.136	-.048
	(.143)	(.125)	(.118)
Treatment Intersections	31	31	31
Control Intersections	56	56	56
Houston-Dallas Sample			
Panel C: Poisson Model			
*After Removal * Treated*	.007	-.264**	-.166
	(.178)	(.134)	(.121)
Panel D: Weighted Poisson Model			
*After Removal * Treated*	-.046	-.280*	-.194
	(.169)	(.148)	(.127)
Treatment Intersections	28	28	28
Control Intersections	24	24	24

This table shows the difference-in-differences coefficient of interest for the *removal* of the Houston red light cameras from estimating Equation 4 using a poisson model. The dependent variable is the average number of angle (column 1), non-angle (column 2), and total accidents (column 3) per year. All panels estimate propensity score trimmed samples. Panels B and D re-weight the sample using inverse propensity score weighting. The Houston sample (2008-2014) uses Houston non-RLC intersections as the control group. The Houston-Dallas sample (2008-2014) uses Dallas RLC intersections as the control group. Both samples include all police-reported, "intersection-related" accidents within 200 feet of an intersection for the indicated years (Source: Texas Department of Transportation). Standard errors are robust to heteroskedasticity and clustered by intersection, * < 0.10, ** < 0.05, *** < 0.01.

Table 3: The Effect on Traffic Injuries from Ending the Red Light Camera Program

	(1)	(2)	(3)	(4)
Dependent Variable:	Injury Accidents		People Injured	
Injury Classification:	All	Minor	All	Minor
Houston Sample				
Panel A: Poisson Model				
*After Removal * Treated*	-.035	-.067	-.140	-.154
	(.21)	(.217)	(.162)	(.184)
Panel B: Weighted Poisson Model				
*After Removal * Treated*	-.158	-.202	-.241	-.288
	(.221)	(.220)	(.186)	(.198)
Treatment Intersections	31	31	31	31
Control Intersections	56	56	56	56
Houston-Dallas Sample				
Panel C: Poisson Model				
*After Removal * Treated*	.019	.111	.003	.067
	(.255)	(.263)	(.216)	(.235)
Panel D: Weighted Poisson Model				
*After Removal * Treated*	-.104	-.024	-.083	-.027
	(.254)	(.262)	(.217)	(.23)
Treatment Intersections	28	28	28	28
Control Intersections	24	24	24	24

This table shows the difference-in-differences coefficient of interest from estimating Equation 4 using a poisson model on the Houston and Houston-Dallas samples. The dependent variable in columns (1) and (2) is an injury accident. Column (1) defines an injury accident as an accident with at least one reported injury, while column (2) defines a minor injury accident as one where there is at least one reported non-incapacitating injury and no incapacitating injuries or deaths. The accident measures in columns (1) and (2) do not account for the possibility of multiple injuries related to the same accident. Column (3) considers the total number of monthly reported accident-related injuries for each intersection. Column (4) considers the total number of minor monthly reported accident-related injuries. Standard errors are robust to heteroskedasticity and clustered by intersection, * <0.10, ** <0.05, *** <0.01.

Table 4: The Effect on Average Daily Traffic from Ending the Red Light Camera Program

	(1) Houston Sample	(2) Houston-Dallas Sample
Panel A: OLS Model		
After Removal * Treated	15,073**	7,271
	(5,648)	(8,205)
Percent Change	42	18
Panel B: Weighted OLS Model		
After Removal * Treated	13,972*	2,604
	(8,185)	(8,626)
Percent Change	39	6
Treatment	15	22
Control	24	19

This table shows the difference-in-differences coefficient of interest from estimating Equation 4 using OLS on the Houston and Houston-Dallas samples. The dependent variable is the average daily traffic (ADT) at each intersection. Intersection ADT values are not available for each year, nor for every intersection. The intersections included in the models have one observation before (measured between 2008 and 2010), and one observation after (measured between 2011 and 2014) the RLC referendum vote. The intersections included in the analysis are a subset of those intersections in our complete Houston and Houston-Dallas samples. Standard errors are robust to heteroskedasticity and clustered by intersection, * < 0.10, ** < 0.05, *** < 0.01.

Table 5: The Effect on Accidents and Injuries from Installing and Removing Red Light Cameras

Dependent Variable:	(1) Angle	(2) Non-Angle	(3) Total	(4) Injury
Installation Estimates				
Panel A: Poisson Model				
*Cameras On * Treated*	.005	.027	.028	-.140
	(.121)	(.096)	(.075)	(.150)
Panel B: Weighted Poisson Model				
*Cameras On * Treated*	-.057	.205**	.119	-.169
	(.169)	(.095)	(.088)	(.169)
Treatment Intersections	26	26	26	26
Control Intersections	53	53	53	53
Removal Estimates				
Panel C: Poisson Model				
*After Removal * Treated*	-.088	-.167	-.137	-.020
	(.155)	(.104)	(.109)	(.194)
Panel D: Weighted Poisson Model				
*After Removal * Treated*	.186	-.047	.036	.219
	(.184)	(.141)	(.151)	(.214)
Treatment Intersections	26	26	26	26
Control Intersections	53	53	53	53

This table shows the difference-in-differences coefficient of interest for the *installation* and *removal* of the Houston red light cameras from estimating Equation 4 using a poisson model and the 2003-2014 Houston Sample. The dependent variable is the average number of angle (column 1), non-angle (column 2), and total accidents (column 3) per year. All panels estimate propensity score trimmed samples. Panels B and D re-weight the sample using inverse propensity score weighting. The sample uses Houston RLC intersections as the treatment group and Houston non-RLC intersections as the control group (selected based on 2003-2005 accident characteristics). The data include all police-reported, "intersection-related" accidents within 200 feet of an intersection from 2003-2014 (Source: Texas Department of Transportation). Standard errors are robust to heteroskedasticity and clustered by intersection, * < 0.10, ** < 0.05, *** < 0.01.

Table 6: The Effect on Accidents from Ending the Red Light Camera Program - Robustness Specifications

Dependent Variable:	(1) Angle	(2) Non-Angle	(3) Total	(4) Injury
Panel A: OLS				
*After Removal * Treated*	.591	-1.280	-.696	-.375
	(.801)	(1.260)	(1.819)	(.293)
Percentage Change	12.1	-11.1	-4.1	-28.2
Treatment Intersections	31	31	31	31
Control Intersections	56	56	56	56
Panel B: Drop 2011				
*After Removal * Treated*	.147	-.089	-.008	-.233
	(.154)	(.127)	(.121)	(.198)
Treatment Intersections	31	31	31	31
Control Intersections	56	56	56	56
Panel C: Frontage Only				
*After Removal * Treated*	.011	.050	.028	-.676**
	(.260)	(.197)	(.188)	(.329)
Treatment Intersections	11	11	11	11
Control Intersections	12	12	12	12
Panel D: Not "In Intersection"				
*After Removal * Treated*	.119	.112	.114	.042
	(.128)	(.115)	(.109)	(.151)
Treatment Intersections	31	31	31	31
Control Intersections	56	56	56	56
Panel E: Balanced Installation				
*Camera On * Treated*	-.028	.189*	.119	-.159
	(.180)	(.105)	(.092)	(.167)
Treatment Intersections	26	26	26	26
Control Intersections	53	53	53	53
Panel F: Balanced Removal				
*After Removal * Treated*	.130	-.051	.010	.195
	(.184)	(.122)	(.138)	(.194)
Treatment Intersections	26	26	26	26
Control Intersections	53	53	53	53

The panel shows the difference-in-differences coefficient of interest for estimating Equation 4 with propensity score weighting. The first four panels estimate our Houston 2008-2014 sample and are comparable to Table 2 panel B. Panel A estimates the model using OLS, panel B drops all accidents from 2011, panel C only considers a trimmed subset of cameras located on frontage roads, and panel D ignores the data restriction that the accident must be coded by police as "in intersection". Panels E and F estimate the installation and removal effects using a balanced Houston 2003-2014 sample and are comparable to Table 5 panels B and D.