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

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Abstract

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 public safety. 2.3 million people were injured in US traffic accidents in 2013. In urban areas, by far the most likely location for an accident is at a traffic intersection. Recognizing this danger, 36 of the 50 most populous US cities have enacted red light camera (RLC) programs as an effort to enforce traffic laws and to reduce accidents. Law enforcement personnel are generally in strong favor of RLC programs and adamant about their effectiveness.

While there is almost no economics literature on this question, there is a lengthy literature in transportation and safety journals. Most of these studies find that RLC programs have an economically large and statistically significant effect on reducing the number of accidents. However, all existing RLC camera studies struggle with how to account for the endogenous start time and location of the RLCs. This challenge is an example of the now well-known endogeneity problem which undermined many early tests of Becker's deterrence prediction. In the

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context of the RLC program, the endogeneity problem likely leads to over-estimates of the program's effectiveness.

We test whether red light cameras reduce accidents using 7 years of geocoded police accident data from Houston and Dallas. We avoid the endogenous selection concerns by looking at an exogenous removal of RLCs by a voter referendum in Houston (the Dallas RLC program was unaffected). Using a difference-in-differences model which includes intersection fixed effects, we find that ending the RLC program leads to an economically small and statistically insignificant change in both the number of accidents and injuries. The upper bound of the 95
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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 monthly accident rates for the city of Houston from 2008-2014 based on the distance from an intersection. There are more than 10 times as many accidents within 100 feet of an intersection than at any other intersection distance in the city.

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 and is based on a key prediction of Becker's seminal work on the deterrence of crime (Becker [1968]). Drivers who run a red light at a red light camera (RLC) intersection will receive a ticket with near certainty (i.e. a dramatic increase in the probability of being caught relative to no camera). 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]

[1]The GHSA is, according to their website, a "nonprofit association representing the highway safety office of states, territories, the District of Columbia and Puerto Rico. GHSA provides leadership and representation for the states and territories to improve traffic safety, influence national policy, enhance pro-

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/after the installation of a camera).[2] The majority of these studies conclude that red light traffic camera programs have a statistically and economically significant effect on reducing traffic accidents, injuries, and deaths. One frequently cited recent study concludes that vehicular deaths increase by 30% in the absence of red light camera programs (Hu and Cicchino [2016]).[3]

The main challenge that all existing red light camera studies struggle with is how to account for the endogenous start time and location of RLCs. 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 police (or policing intensity) reduced crime often failed to detect any effect (e.g. Levitt and Miles [2006] and Chalfin and McCrary [2017] provide reviews). A key challenge faced in testing the deterrence prediction is that a change in the likelihood of being caught is often endogenous to the level of crime (e.g. Levitt [2007]). In the context of an increase in police, the endogeneity problem likely leads to a bias of finding no correlation.

In the context of a red light camera program, the endogeneity problem likely leads to over-estimates of the program's effectiveness at reducing traf-

gram management and promote best practices." http://www.ghsa.org/resources/state-highway-safety-group-supports-red-light-cameras

[2]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 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]).

[3]Hu and Cicchino [2016] examines vehicular deaths at the city-level by year for cities with and without RLC programs.

fic 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.

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 potential offenders.

One 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 red light camera tickets. 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 falls precipitously in subsequent years.

An unusual feature of red light camera programs that differs from many other crime policies is 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 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 at an intersection 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. 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. DeAngelo and Hansen [2014]; Hansen [2015]).[4] In particular, we test whether red light cameras, a popular policing tool, are effective at reducing accidents and improving public safety. We avoid the endogenous selection concerns highlighted above by looking at the impact of an exogenous removal of red light cameras by a voter referendum in 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 (Oaklander [2011]). After the referendum, the city immediately removed the cameras and began legal proceedings with the private sub-contractor that administered the program over payment for breach of contract.

We estimate a difference-in-differences model using Poisson regression and a seven year panel of yearly intersection crash data. The key identifying assumption for the model is that the post-referendum trend in intersection accidents for the control group of intersections represents a valid counterfactual for the trend in accidents that would have occurred at Houston RLC intersec-

[4] Interestingly, traffic crimes, while not a common setting to study Becker's deterrence predictions, is a specific crime highlighted in Becker [1968]: "Although the word "crime" is used in the title to minimize terminological innovations, the analysis is intended to be sufficiently general to cover all violations, not just felonies–like murder, robbery, and assault, which receive so much newspaper coverage–but also tax evasion, the so-called white-collar crimes, and traffic and other violations." (p2)

tions had the referendum not occurred. If the RLC program was 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 and/or injuries at intersections that had a RLC in Houston relative to control intersections that were not subject to the referendum.

We consider two different control groups of intersections. The first control group is comprised of non-RLC intersections in Houston with similar accident characteristics and trends as the Houston RLC intersections. We select the Houston control intersections by estimating the propensity to have a Houston RLC using a logit model that includes accident-related characteristics that have been cited as important criteria in selecting RLC intersections (Department [2016]; Chi [2016]; Stein et al. [2006]). We follow Imbens and Wooldridge [2007] and use the 90-10 rule to select our final Houston sample by dropping those intersections with an estimated propensity score less than 0.10 or greater than 0.90. Our "Houston" sample includes 35 RLC intersections and 53 non-RLC intersections.

Our second control group is comprised of RLC intersections in Dallas, TX. The Dallas RLCs remained in operation throughout our sample period. We select our "Houston-Dallas" sample by again estimating a logit model and dropping intersections using the 90-10 rule. The Houston-Dallas sample includes 37 Houston RLC intersections and 36 Dallas RLC intersections.

The Houston and Houston-Dallas samples are (mostly) well-balanced when comparing the means of important accident-related characteristics between the treatment group of Houston RLC intersections and the control groups (Houston non-RLC intersections and Dallas RLC intersections, respectively). More importantly, we show that the treatment and control groups display similar trends in these same characteristics before the referendum.

We estimate models that separately examine the effect of the RLC program on right angle accidents, non-right angle accidents, and total accidents. 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]).

We find no evidence that red light cameras reduce the frequency of vehicular accidents. If the RLC program is effective at reducing the number of accidents then when the program ended we should observe an increase in the number of accidents. In our preferred model we estimate a positive point estimate for angle crashes (17%) and a negative point estimate for all other types of accidents (-10%). The net effect is an estimate of 0.5%. 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 injuried) after the RLC program ends. The upper bound of the 95% confidence interval implies that the RLC program in Houston prevented just 4 injury accidents per year. 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. We view our study as a corollary to the point made by Chalfin and McCrary [2017] that a focus on policy changes can be a blunt proxy for a careful analysis of underlying fundamentals of the crime deterrence setting.

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 the 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 monthly number of citations per year of operation for the 54 Dallas RLC intersections. In the first year of operation there are, on average, 265 monthly citations at a RLC monitored intersection. The number of citations drops to 144 in 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.

[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.

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 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

[6]The Federal Highway Administration recommends that the yellow light interval be between 3 and 6 seconds (Administration [2009]).

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 under the RLC program (relative to before the program). For example, the marginal driver who was just 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 willingly accept higher expected accident costs provided that these costs are 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 (and costly) to continue.

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. Thus, 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 programmed to take photos of vehicles that enter the intersection from a cross street when the signal is red for traffic on that street. 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 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 began installing its cameras in 2006 (Hassan [2006]). The program grew to cover a total of 66 intersections at the program's peak in 2010. Approximately 800,000 $75 tickets were issued from 2006 to 2010 for a total of about $44 million collected (Olson [2010]).[7] Dallas began its RLC program in 2006, with the installation of initial sites. In 2008 the Dallas program was put under the control of the Automated Red Light Enforcement Commission. The Dallas RLC program remained in place throughout our panel and continues today. 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

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

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]). 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 were settled in January 2012 (Hou [2012]).

3.2 Data Sources

3.2.1 Vehicle Accident Information

The accident data cover 2008-2014 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.[8] 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

[8]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 2008 and 2009 were obtained via a Freedom of Information Act request from The University of Texas at Austin Center for Transportation Research.

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.

3.2.2 RLC Intersection Information

We collect information on RLC intersections from TxDOT's annual red light camera enforced intersection reports. 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).

3.2.3 Intersection Engineering Characteristics

We collect a 2nd source of average daily traffic counts for all Houston intersections and for Dallas RLC intersections. The source of the Houston data is Houston's open data portal. The source of the Dallas data is the Council of Governments. Both of these sources of ADT data are stored as a shapefile of points where traffic levels were monitored. We attach the ADT data to Census Tiger/Line shapefiles for roads in Houston to produce an ADT road map which we then use to assign to intersections on the roadways. The data are from 2007 to 2014 and cover most major roads in Houston. Importantly, we are able to compare the intersection ADT data we derive using the open portal data with the ADT data reported by TxDOT for RLC intersections. The means of ADT when both methods are used are very similar though there is some variation for individual intersections. We only use the shapefile-derived ADT data in regressions where we explore changes in ADT over time and use ADT as the dependent variable.

We also 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. The structural characteristics are used as controls in some model specifications to provide a robustness check to our main results.[9]

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 standard difference-in-differences model that exploits the timing of the referendum that shutoff the Houston cameras to estimate the causal effect of red light cameras on traffic accidents, injury accidents, and traffic patterns. Our preferred difference-in-difference estimates use a poisson model and reweight the sample using the propensity scores.

4.1 Logit Model to Estimate the Likelihood of a RLC at an Intersection

The first step is to define the intersections that will be considered for our estimating sample in our differences-in-differences model. These are the intersections summarized in Table 1. Our treatment group is comprised of all Houston RLC intersections. We use two control groups. The first control group are those intersections within Houston that never have a RLC during the 2008-2014 panel and which meet three screening criteria. The second control group

[9]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.

are RLC intersections in the city of Dallas. The Dallas RLC intersections are not subject to the referendum and always have a camera during our panel.

The screening criteria for the within Houston control group are as follows. First, the control intersection must have at least one intersection-related accident during our panel period. 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]). Third, the intersection must have non-missing ADT data for each direction at the intersection. Requiring 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.

The second step is to run a logit model to estimate the likelihood that an intersection would be assigned a Houston red light camera. 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

$$y_{i,t} = \alpha + A_{i,t}\gamma + u_{i,t}, \tag{3}$$

where the dependent variable $y_{i,t} \in (0, 1)$ 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 $u_{i,t}$ is an error term which is assumed to have a standard logistic distribution. 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 pre-referendum ADT at the intersection, as well as, the monthly accident rate at the intersection separately for each of the 3 pre-referendum years for: right angle, not right angle, injury, and red light related accidents.[10]

The logit model is estimated on two different samples. The first sample is comprised of the Houston RLC intersections and the within Houston control intersections that do not have red light cameras (hereafter referred to as the "Houston sample"). The second sample is comprised of the Houston RLC intersections and the Dallas RLC intersections (hereafter referred to as the "Houston-Dallas sample"). $\hat{y}_i$ 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.

4.2 Trimming the Samples using the Propensity Score

We use the propensity score to trim the treatment and control groups in each of our two 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]. The Houston sample includes all 66 Houston RLC intersections and 854 control intersections before trimming with the propensity score. After trimming there are 28 Houston RLC intersections and 61 control intersections. The Houston-Dallas sample includes all 66 Houston and all 48 Dallas RLC intersections before trimming, and 31 Houston RLC intersections and 35 Dallas RLC intersections after trimming.

Figure 3 shows the location of the Houston RLC and control intersections for the trimmed Houston sample. Both the treated and control portions tend to be southeast of the city. There are a few more treatment intersections to

[10] Our difference-in-difference model estimates are similar when we use other logit specifications to select the estimation samples. In particular, we consider a logit model that also includes intersection engineering characteristics (speed limit, number of lanes, and separated road). We prefer the more parsimonious model as it leads to larger samples, while still providing common pre-referendum trends for the dependent variables between the treatment and control groups.

the north and east of the city than control intersections, though some controls are present. In general there is a geographically similar observation for most treated and control intersections.

Figure 4 shows the distribution of propensity scores in the Houston sample (top panel) and the Houston-Dallas sample (bottom panel). 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. Overall, there is considerable overlap in the propensity scores for the treatment and control intersections in each 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 our Houston sample, while the bottom panel shows the characteristics for our Houston-Dallas 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) and (7)-(9) repeat the same format as the first three columns except for two different propensity score trimmed samples. Columns (7)-(9) limit the samples to intersections that are on frontage streets. A large proportion of the RLC intersections in both cities are on frontage streets. Frontage streets are primary surface roads adjacent to highways which also tend to be the roads that offer greatest access to the highways.

The Houston-Dallas sample is not well-balanced in all six 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 each sample. The maximum standardized difference for the Houston sample is 0.44, while for the Houston-Dallas sample it is 0.29.

There are greater differences in the intersection engineering characteristics. Recall that the engineering characteristics are 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. The final 3 columns of the table show sample characteristics for the same sample trimming procedure after first conditioning on whether an intersection is on a frontage road. Only considering intersections that are on frontage roads leads to a somewhat better match, but much smaller samples.

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 from 2008-2014. The accident data plotted are the residuals from an OLS regression that includes intersection fixed effects as the independent variable. The figures can be read as the mean monthly accident rate for each year. Row 1 plots angle accidents, row 2 plots non-angle accidents, and row 3 plots injury accidents. There are three pre-referendum observations for the Houston sample, but only two pre-referendum observations for the Houston-Dallas sample due to the timing of the installation of the RLCs.

5 The Effect of RLC Removal

5.1 Difference-in-Differences Model

We estimate the effect of red light camera removal on vehicle accidents, casualties, and ADT using a panel data differences-in-differences regression model. We specify our baseline model as

$$y_{i,t} = \beta_0 + \beta_1 T_i + \beta_2 R_t + \delta T_i * R_t + \alpha_i + v_t + \varepsilon_{i,t}, \tag{4}$$

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 has a red light camera before the referendum. R_t is a post-referendum indicator variable that equals one if the monthly panel observation is from November 2010 or later. δ is the coefficient of interest and measures the pre-referendum to post-referendum change in mean for the outcome variable for a Houston intersection with a RLC operating during the pre-referendum period, relative to the change for a control intersection.

Recall that we estimate two different samples. The Houston sample uses a within Houston control group of intersections without a RLC pre-referendum. The Houston-Dallas sample uses RLC intersections in Dallas as the control group. The Dallas intersections have a RLC throughout the entire panel sample period. 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.

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. 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 key covariates that determine treatment.

The key identifying assumption of Equation (4) is that the post-referendum trend for the dependent variable (e.g. total accidents) for the control group of intersections is a valid counterfactual for what would have occurred at Houston RLC intersections had there been no referendum. Figure 5 is one piece of evidence that can be used to evaluate the viability of this assumption and shows similar pre-referendum trends.

A specific concern regarding the identifying assumption is that having a RLC program could alter driving behavior in the city even after the cameras are shut off. If this were the case, then the estimates for the Houston sample would be biased towards not finding an effect on accidents after the cameras are shut off. In other words, the trend in accidents at non-RLC intersections would not correctly reflect the trend in what we would expect to have occurred at RLC intersections had there never been a RLC program. As we report below in Section 5.2, our difference-in-differences estimation results are very 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 between the two samples lends support to the key identifying assumption. The existing literature suggests that red light running violation levels return to pre-RLC program levels after the cameras are removed (Porter et al. [2013]).

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 for 12 separate regressions using our Houston sample. There are four panels in the table. The top two panels estimate our main sample using the difference-in-differences model (Equation 4) and the propensity score weighted difference-in-differences model. The bottom two panels estimate the same models except for a sample limited to frontage road intersections. We estimate each model separately for angle accidents (column 1), non-angle accidents (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 (17%) and a negative point estimate for all other types of accidents (-10%). The net effect is an estimate of 0.5%. The point estimates for the overall effect on accidents in the four models range from -9% to 0.5% (Column 3). All of the estimates are statistically insignificant.

Table 3 shows estimation results for the same models and using the same dependent variables as in Table 3 except for the Houston-Dallas sample. The results are similar to those from the Houston sample. We estimate a small, statistically insignificant overall effect for total accidents. The point estimates for angle accidents are positive (column 1) and the point estimates for non-angle accidents are (mostly) negative, but none are close to being statistically significant.

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 regardless of its severity. 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. For example, Shin and Washington [2007] conclude that a rear end accident causing a minor injury has one third of the economic cost as an incapacitating injury rear end accident. In addition, one would expect injuries from angle crashes to be more severe and costlier than from rear end accidents. Therefore, to ensure accurate policy evaluation we conduct analysis for both pooled and separate injury counts.

Table 4 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 our 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 injury accidents after the RLC ends is negative in all specifications. Likewise, we estimate that the change in the number of people injured is negative. Only estimates from the Houston sample with the unweighted model 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 31.4 percent in injury accidents (Panel A, Column 1) corresponds to a combined decrease of 15 injury accidents per year across all intersections in Houston that had cameras in our sample. The upper bound of the 95% confidence interval would imply that there are approximately 4 fewer injury accidents per year.[11]

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 5 provides some evidence on how average daily traffic at an intersection

[11] We don't specifically study the number of deaths (although they are included in incapacitating injuries). The reason is that despite the large aggregate number of vehicular deaths nationwide, deaths at any particular intersection in Houston (or any other city) are rare. The are approximately 5 deaths per year across all camera intersections in Houston.

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.[12] We use the same intersection propensity score weights as those used in the accident analysis. All four point estimates are positive. The estimates from the propensity score weighted regressions in panel B imply an approximate increase of 38% to 40%. The model estimates are somewhat smaller and less precise from a model that includes intersection fixed effects (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 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.

[12]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.

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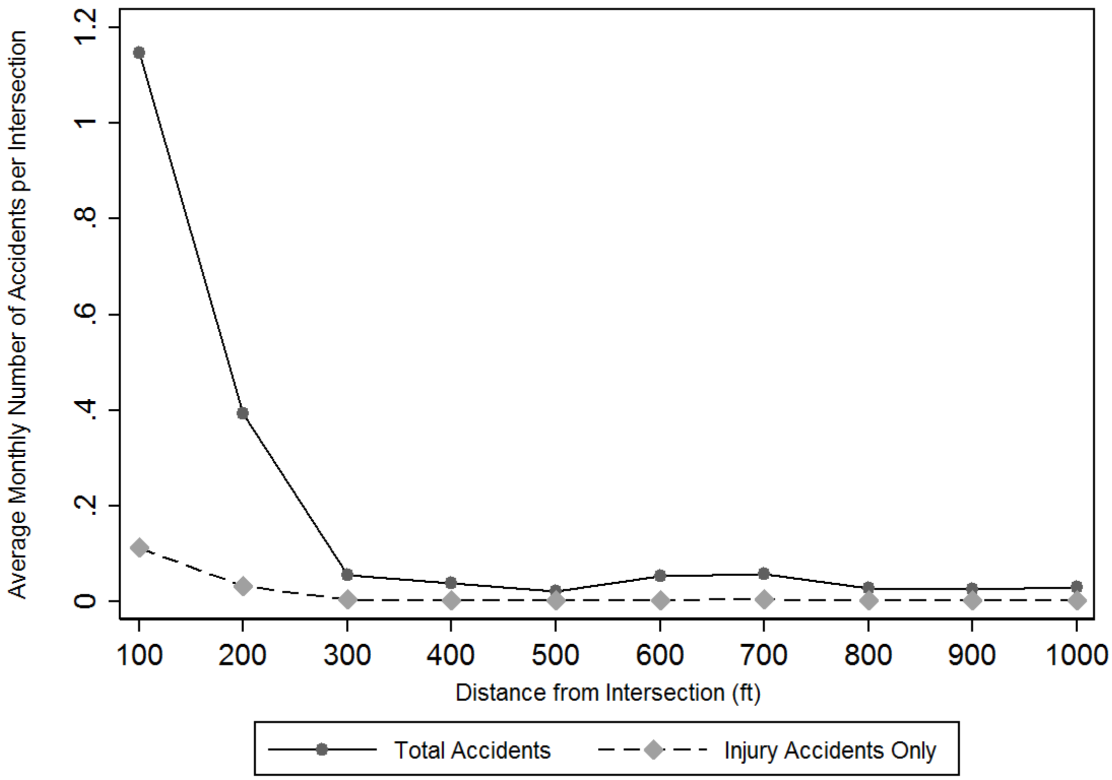
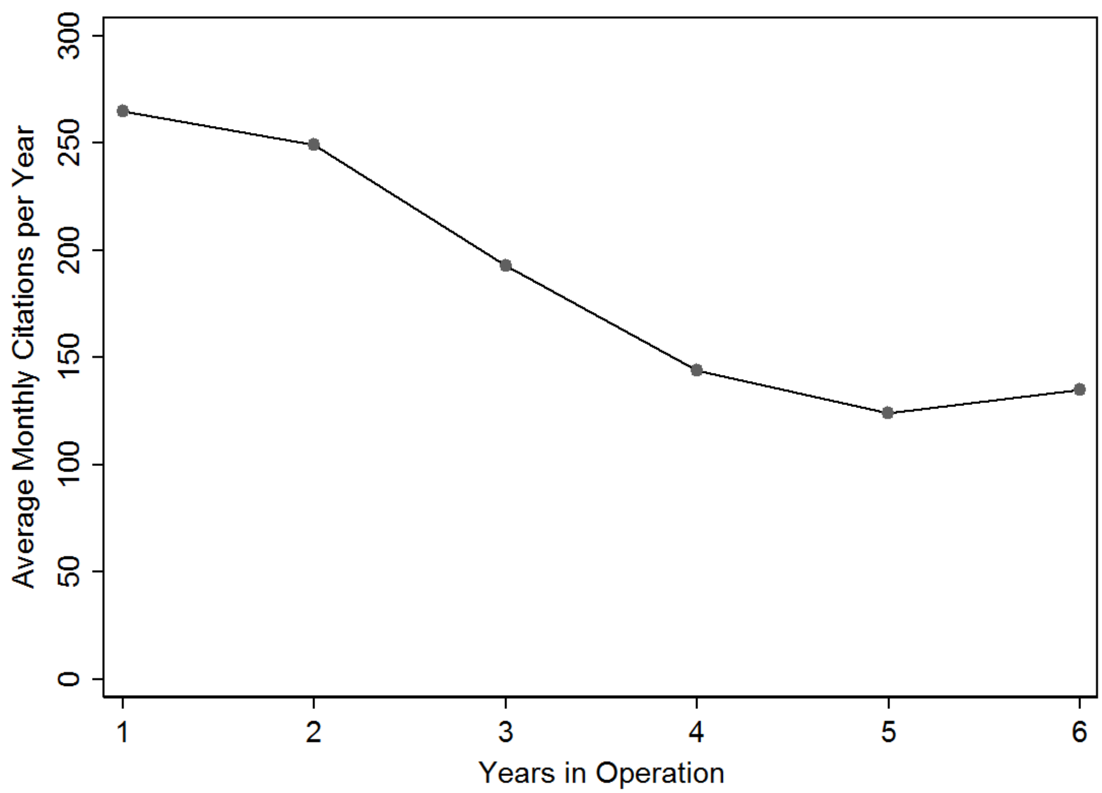
7 Figures and Tables

Figure 1: Accident Rates at Different Distances from an Intersection



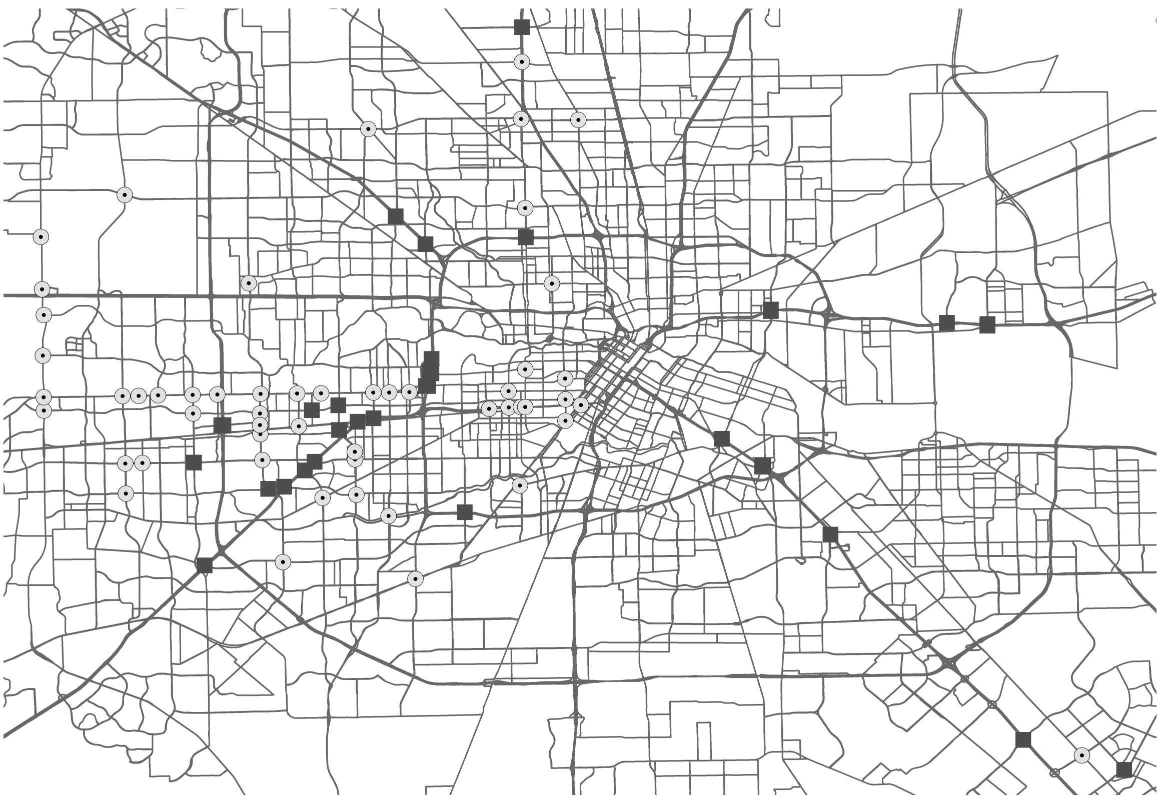
The figure plots total accidents and total injury accidents by distance from an intersection in 100 foot bins. Total accidents count all accidents classified as "in or related" to the intersection by the Texas Department of Transportation (TxDOT). An injury accident is an accident with at least one non-incapacitating (minor) injury, incapacitating injury, or death. Data source: TxDOT.

Figure 2: Citation Rates at Dallas Red Light Cameras



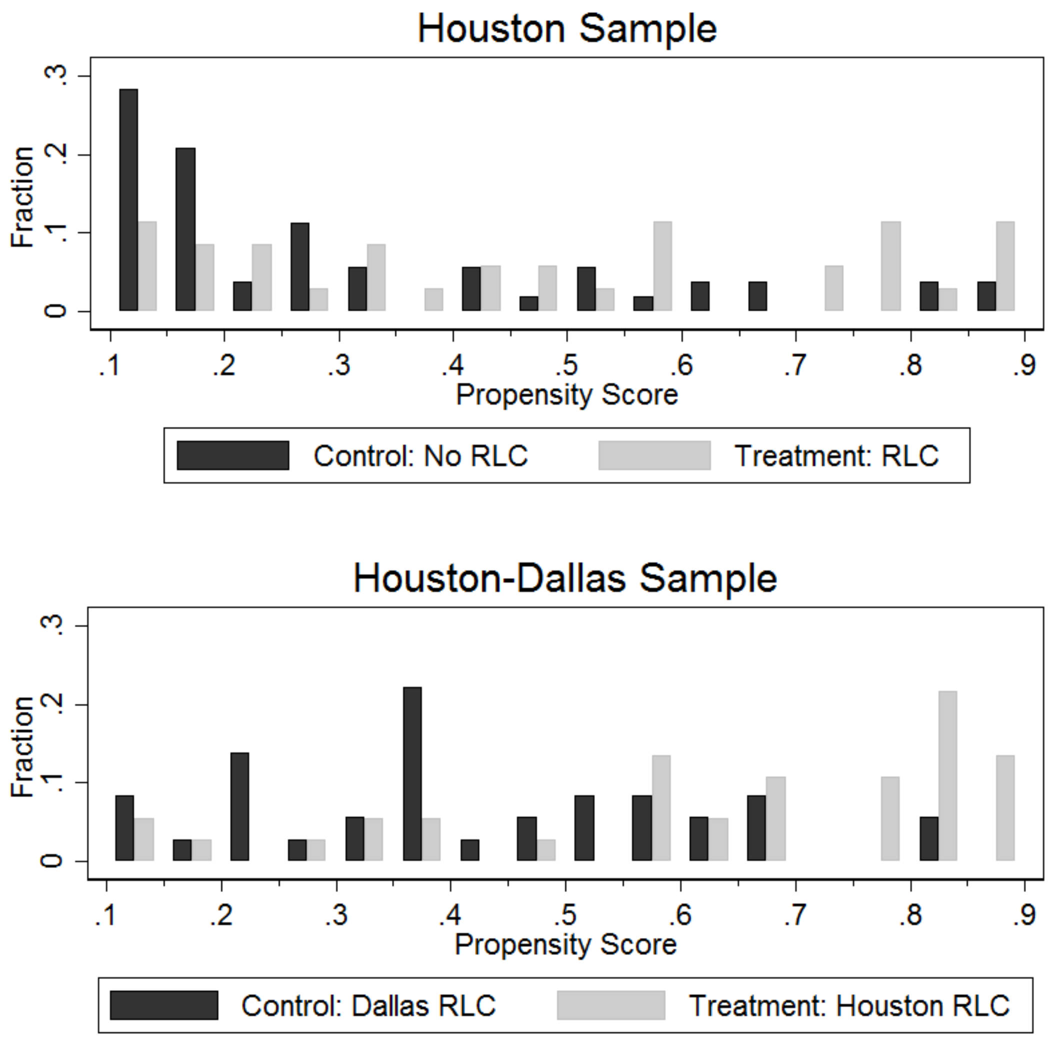
This figure shows the monthly rate of red light camera tickets for Dallas red light cameras over the first 6 years of the program. Data source: annual reports submitted by Dallas to the Texas Department of Transportation.

Figure 3: Location of Houston Treatment and Control Intersections



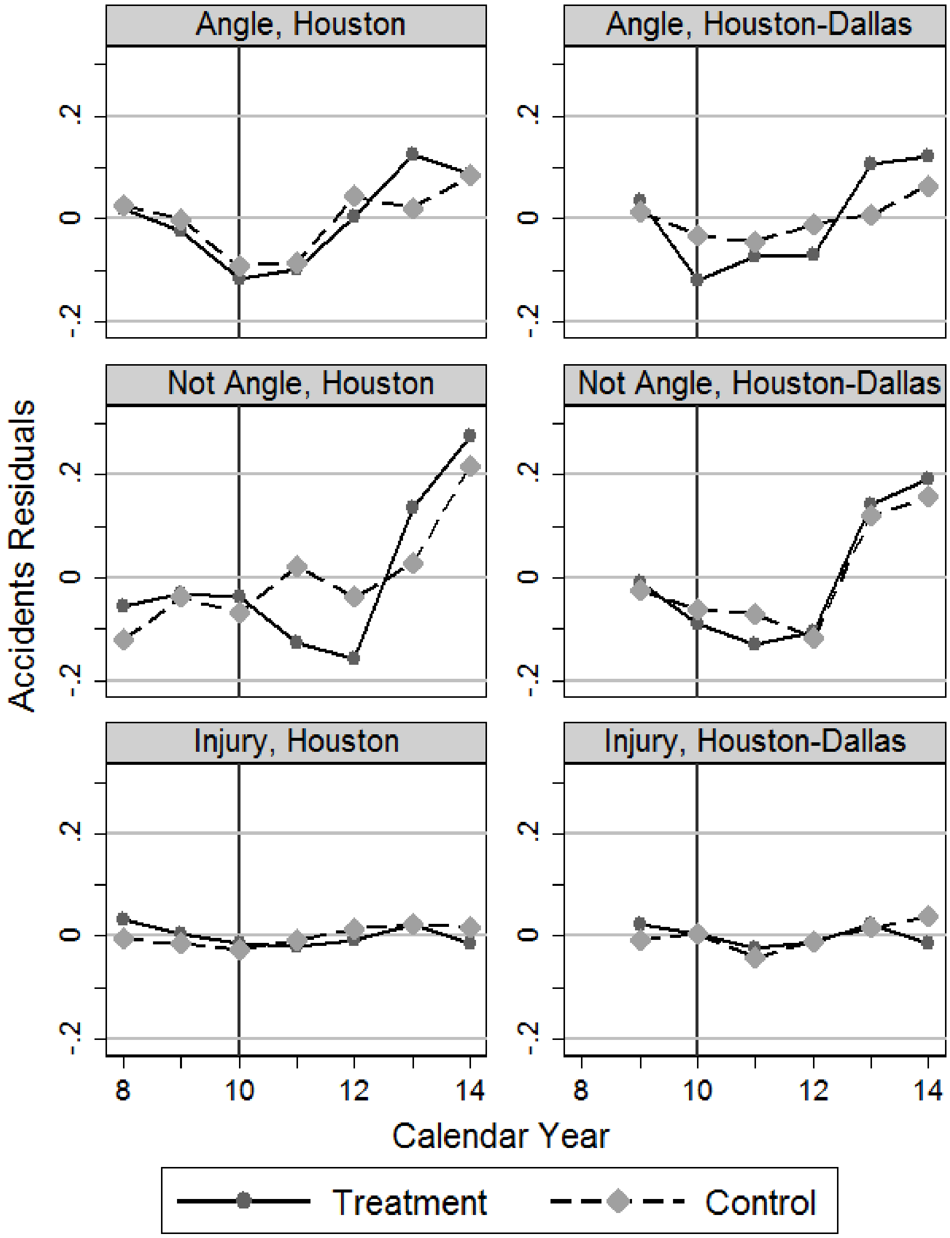
Data source: TxDOT.

Figure 4: Distribution of Trimmed Propensity Scores



The figure shows the distribution of propensity scores for the intersections in our trimmed samples for the Houston sample (top panel) and the Houston-Dallas sample (bottom panel). 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: Trends in Annual Angle, Not Angle, and Injury Accident Levels



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 the set 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.

Table 1: Sample Accident Intersection Characteristics

	All Intersections			All Intersections, Trimmed			Frontage Only, Trimmed		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Treatment	Control	Difference/SD	Treatment	Control	Difference/SD	Treatment	Control	Difference/SD
Panel A: Houston Control									
Accident Characteristics									
Total Accidents	1.44	0.26	2.25	1.19	0.96	0.43	1.01	0.81	0.39
Angle Accidents	0.62	0.10	1.89	0.44	0.38	0.18	0.44	0.34	0.15
Not Angle Accidents	0.82	0.16	2.11	0.75	0.58	0.44	0.57	0.47	0.50
Injury Accidents	0.15	0.03	1.42	0.12	0.10	0.14	0.12	0.09	0.12
Red Light Running Accidents	0.47	0.06	1.90	0.32	0.27	0.19	0.32	0.25	0.16
Average Daily Traffic	58,540	31,633	1.41	57,318	56,928	0.01	44,239	42,738	0.03
Intersection Characteristics									
Frontage Street	0.82	0.04	2.65	0.83	0.13	1.41	1.00	1.00	.
Lanes	7.36	5.55	1.07	7.14	7.57	-0.29	7.33	7.79	-0.32
Speed Limit	39.93	34.17	0.96	39.73	37.29	0.40	41.67	38.57	0.40
Divided	0.92	0.71	0.46	1.00	0.92	0.36	1.00	1.00	.
Number of Intersections	66	858		35	53		12	14	
Panel B: Houston-Dallas Control									
Accident Characteristics									
Total Accidents	1.43	0.89	0.52	0.99	0.84	0.23	1.25	1.32	-0.09
Angle Accidents	0.60	0.26	0.55	0.36	0.26	0.29	0.49	0.50	-0.03
Not Angle Accidents	0.83	0.63	0.35	0.63	0.58	0.11	0.76	0.81	-0.12
Injury Accidents	0.14	0.12	0.09	0.10	0.11	-0.06	0.15	0.17	-0.13
Red Light Running Accidents	0.43	0.22	0.46	0.27	0.21	0.21	0.39	0.41	-0.04
Average Daily Traffic	58,540	44,629	0.52	49,633	44,990	0.26	41,830	40,794	0.11
Intersection Characteristics									
Frontage Street	0.82	0.33	1.01	0.78	0.31	0.96	1.00	1.00	.
Lanes	7.36	7.44	-0.05	7.41	7.44	-0.02	7.00	8.20	-0.79
Speed Limit	39.93	35.24	0.81	39.51	35.21	0.69	42.14	37.13	1.20
Divided	0.92	0.87	0.19	0.89	0.86	0.10	1.00	1.00	.
Number of Intersections	66	39		37	36		14	10	

This table shows the means of intersection characteristics by whether an intersection is a Houston intersection with a RLC (treatment), a Dallas intersection with a RLC (control panel B), or a Houston intersection without a RLC (control panel A). The accident characteristic means are monthly means calculated by summing the total monthly accidents by type for the entire calendar year and dividing by 12 (except for 2010, as we only use data from the pre-referendum months (Jan-Oct) and divide by 10). Columns (1)-(3) describe all observations with no propensity score trimming. Columns (4)-(6) show the comparison for our trimmed sample, and columns (7)-(9) show the comparison for our trimmed sample after first conditioning on whether the intersection is on a frontage street.

Table 2: The Effect on Accidents of Ending the Red Light Camera Program
Houston Sample

	(1)	(2)	(3)
Dependent Variable:	Angle	Not Angle	Total
Panel A: Poisson Model			
After Removal * Treated	.052	-.114	-.050
	(.141)	(.098)	(.100)
Panel B: Weighted Poisson Model			
After Removal * Treated	.201	-.017	.067
	(.132)	(.119)	(.105)
Treatment Intersections	35	35	35
Control Intersections	53	53	53
Panel C: Poisson Model - Frontage Only			
After Removal * Treated	-.199	-.008	-.088
	(.259)	(.183)	(.186)
Panel D: Weighted Poisson Model - Frontage Only			
After Removal * Treated	-.106	.006	-.041
	(.26)	(.188)	(.195)
Treatment Intersections	12	12	12
Control Intersections	14	14	14

This table shows the difference-in-difference coefficient of interest from estimating Equation 4 using a poisson model and the Houston sample. The dependent variable is the average monthly accident rate at each intersection (calculated separately for each calendar year). Column (1) considers only angle accidents, column (2) non-angle accidents, and column (3) all accidents. All panels estimate propensity score trimmed samples. Panels C and D only consider intersections that are on a frontage road. Panels B and D re-weight the sample using inverse propensity score weighting. Standard errors are robust to heteroskedasticity and clustered by intersection, * < 0.10, ** < 0.05, *** < 0.01.

Table 3: The Effect on Accidents of Ending the Red Light Camera Program
Houston-Dallas Sample

Dependent Variable:	(1) Angle	(2) Not Angle	(3) Total
Panel A: Poisson Model			
After Removal * Treated	.106	.005	.039
	(.176)	(.105)	(.103)
Panel B: Weighted Poisson Model			
After Removal * Treated	.148	-.002	.049
	(.236)	(.110)	(.121)
Treatment Intersections	37	37	37
Control Intersections	36	36	36
Panel C: Poisson Model - Frontage Only			
After Removal * Treated	.035	.012	.019
	(.271)	(.198)	(.181)
Panel D: Weighted Poisson Model - Frontage Only			
After Removal * Treated	.161	-.109	-.012
	(.348)	(.169)	(.186)
Treatment Intersections	14	14	14
Control Intersections	10	10	10

This table shows the difference-in-difference coefficient of interest from estimating Equation 4 using a poisson model and the Houston-Dallas sample. The dependent variable is the average monthly accident rate at each intersection (calculated separately for each calendar year). Column (1) considers only angle accidents, column (2) non-angle accidents, and column (3) all accidents. All panels estimate propensity score trimmed samples. Panels C and D only consider intersections that are on a frontage road. Panels B and D re-weight the sample using inverse propensity score weighting. Standard errors are robust to heteroskedasticity and clustered by intersection, * < 0.10, ** < 0.05, *** < 0.01.

Table 4: The Effect on Traffic Injuries of 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	-0.314	-.380*	-.355**	-.375**
	(.206)	(.216)	(.165)	(.189)
Panel B: Weighted Poisson Model				
After Removal * Treated	-.263	-.312	-.343*	-.373*
	(.245)	(.248)	(.196)	(.213)
Treatment Intersections	35	35	35	35
Control Intersections	53	53	53	53
Houston-Dallas Sample				
Panel C: Poisson Model				
After Removal * Treated	-.189	-.172	-.234	-.218
	(.204)	(.206)	(.188)	(.207)
Panel D: Weighted Poisson Model				
After Removal * Treated	-.074	.001	-.142	-.078
	(.228)	(.241)	(.222)	(.253)
Treatment Intersections	37	37	37	37
Control Intersections	36	36	36	36

This table shows the difference-in-difference 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 5: The Effect on Average Daily Traffic of Ending the Red Light Camera Program

	(1)	(2)	(3)	(4)
	Houston Sample		Houston-Dallas Sample	
Sample:	Plain	Fixed Effects	Plain	Fixed Effects
Panel A: OLS Model				
After Removal * Treated	9,182	9,182	11,757*	11,757
	(6,667)	(9,428)	(6,472)	(9,152)
Percent Change	21	21	30	30
Panel B: Weighted OLS Model				
After Removal * Treated	9,660	9,660	13,870*	13,870
	(8,624)	(12,196)	(6,975)	(9,864)
Percent Change	22	22	35	35
Treatment	11	11	27	27
Control	26	26	31	31

This table shows the difference-in-difference coefficient of interest from estimating Equation 4 using OLS on the Houston sample. 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.