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

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Abstract

Numerous cities have enacted electronic monitoring programs at urban intersections in an effort to reduce the high number of traffic accidents. The rationale is that the higher expected fines for running a red light will induce drivers to stop and lead to fewer crossroad collisions. However, the cameras also incentivize drivers to accept a greater accident risk from stopping. We evaluate the termination of a monitoring program via a voter referendum using 12 years of geocoded police accident data. We find that the cameras changed the composition of accidents, but no evidence of a reduction in total accidents or injuries.

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

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1 Introduction

The automobile is a killer. In the United States, 36,675 people died in traffic accidents in 2014. The year before, 2.3 million people were injured in traffic accidents (Economist [2015]). In urban areas, the most likely location for an accident is at a traffic intersection. Figure 1 shows annual accident rates for the city of Houston from 2003-2014 by 100-foot intervals from an intersection. Roughly three times as many accidents happen within 200 feet of an intersection than at any other distance.[1]

Over 438 communities in 23 states, including 36 of the 50 most populous US cities, have employed electronic monitoring programs in order to enforce traffic laws at intersections and to reduce the number of accidents (IIH [2016]). Red light camera programs specifically target drivers that run red lights. The assumption is that by incentivizing fewer drivers to run red lights via a dramatically higher probability of being caught, the total number of accidents will decline.

As a rule, law enforcement officials favor of red light camera programs and testify to their effectiveness. For example, the executive director of the Governors Highway Safety Association (GHSA) recently endorsed "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]).[2]

Red light camera programs (hereafter "camera programs") are distinct from other crime-reduction methods, for crime prevention is not an end in itself, but serves as a mechanism to accomplish a broader policy goal. There is clear evidence that installing a camera reduces the number of vehicles running

[1]The actual difference is likely much greater, as the figure does not control for the fact that many of the accidents outside of 200 feet of the reference intersection may be within 200 feet of another intersection.

[2]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.

a red light. Still, the predicted relationship between the number of vehicles running red lights and the total number of accidents remains ambiguous.

A simple economic model shows that electronic monitoring via a red-light camera has contradictory effects, in terms of traffic safety. First, some drivers who would have otherwise continued to proceed through the intersection when the light is yellow or red will now attempt to stop. The number of accidents caused by vehicles not stopping at a red light will likely decrease (e.g., angle accidents from cross-road collisions). Second, the number of accidents from stopping at a red light is likely to increase (e.g., rear-end accidents). The model predicts that driver awareness of the cameras will lead some drivers to attempt to stop and accept a higher accident risk from stopping at the intersection, in order to avoid the expected fine from continuing to drive through the intersection. Thus, the overall effect of the electronic monitoring on vehicle accidents and injuries depends on the net composition of the two effects. Overall driver safety could increase or decrease.

Traffic cameras as a policing tool is a key topic in transportation and safety journals (e.g., Erke [2009] and Høye [2013] provide reviews), but the economics literature on this topic is scant, at best (Chen and Warburton [2006] and Wong [2014] are exceptions). Most of the existing studies either compare city-level accident data between cities with and without cameras, or focus on a small number of intersections (e.g., analysis of a single intersection before and after the installation of a camera).[3] The majority of these studies conclude that camera 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 camera programs and concludes that deaths increase by 30% when there are no cameras (Hu and Cicchino [2016]).

The main challenge for existing red-light camera studies is how to account

[3]The most common estimation approach is what the literature calls "Empirical Bayes," whereby the number of accidents during a time period before a camera is installed is used to project the expected number of future accidents at the same intersection after a camera is installed. The effect of the camera program is defined as the difference between the projected number of accidents and the realized number of accidents (Hauer [1997]).

for the endogenous start time and location of the cameras. This challenge is an example of the now well-known problem that undermined many early tests of Beckers deterrence hypothesis regarding the probability of being caught and the reduction of crime (Becker [1968]).[4] 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 camera program, the endogeneity problem likely leads to over-estimates of the programs effectiveness. Intersections chosen for cameras are not selected randomly. Intersections assigned cameras are often more dangerous (e.g., poor traffic flow, high traffic volume) than other intersections. Moreover, intersections with unusually high accident levels in the year just prior to the start of the program may be more likely to receive cameras. These same intersections are, in turn, regardless of intervention, more likely to revert to lower accident levels. We avoid concerns about the endogenous selection of intersections by examining the impact of the exogenous removal of cameras via 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 potential offenders. An advantage of studying the deterrence effect in the context of red light camera programs is that we can confirm a change of perception among drivers after a camera is installed using citation data. The number of tickets issued at camera monitored intersections peaks in the first year after installation and is much lower in subsequent years as drivers learn of the camera location and adjust their behavior.

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

A second key challenge entails driver awareness of the deterrent (e.g., Waldo and Chiricos [1972]; Apel [2013]; Chalfin and McCrary [2017]). Among potential offenders, the perception of being caught might not reflect the probability of being caught. An advantage of studying the deterrence effect in the context of red light camera programs is that we can confirm a change of perception among drivers after a camera is installed using citation data. The number of tickets issued at camera-monitored intersections peaks in the first year after installation, and declines as awareness rises and drivers adjust their behavior.

We analyze whether electronic monitoring via red-light cameras is effective at reducing accidents and improving public safety in Houston, TX. We chose Houston as the empirical setting of our study, for it is a large US city that had a large camera program unexpectedly shut down due to a voter referendum. Houston established a camera program in 2006 that grew to include 66 intersections. Houston residents narrowly passed a voter referendum in November 2010 that banned the cameras. The Houston police department and the mayors office were both opposed to the ban (e.g., Oaklander [2011]). After the referendum, the city immediately shut off 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 camera program four types of accidents: angle, non-angle, total, and injury accidents. Angle accidents comprise about a third of the total number of accidents at a typical intersection and are the primary target of the program (Retting and Kyrychenko [2002]). If electronic monitoring in Houston is successful at improving traffic safety, then we expect that the removal of the cameras would lead to an increase in the number of total accidents and injury accidents at camera intersections, relative to control intersections not subject to the referendum.

The estimates for angle and non-angle accidents support the predictions of the economic model. Our preferred econometric model uses a within Houston control group of intersections without cameras. We select the Houston control intersections by estimating the propensity to have a Houston camera using a logit model that includes pre-referendum accident-related characteristics that have been cited as important criteria in selecting camera intersections (Department [2016]; Chi [2016]; Stein et al. [2006]). We estimate that, once the cameras are removed, angle accidents increased by 26% and all other types of accidents decreased by 18%. We can statistically reject that the coefficients are equal.

Overall, we find no evidence that cameras reduce the total number of accidents. We estimate a statistically insignificant reduction in total accidents (-3%) after the cameras are turned off.

We estimate a negative, statistically insignificant change in the number of injury accidents after the camera program ends. We adapt the model of Chalfin and McCrary [Forthcoming] to interpret how electronic monitoring at traffic intersections affects social welfare. Using our estimates for changes in the types of injuries incurred in traffic accidents (fatalities, incapacitating, non-incapacitating, possible, no injury), the model suggests that the camera program led to a decrease in social welfare.

One potential identification concern for our econometric model is that cameras could affect driving behavior at non-camera intersections in the city (e.g., Høye [2013]; Shin and Washington [2007] Wong [2014]). For example, drivers may alter their routes to avoid camera intersections. If this were the case, traffic volume at the non-camera intersections would increase and thereby bias our model estimates towards finding larger beneficial effects of the program. We test for a change in average daily traffic measured at the intersections in a subset of our main sample and find suggestive evidence of a small increase in traffic at non-camera intersections. We also consider a second, out-of-city control group, the camera intersections of Dallas, which were not subject to the referendum. Model estimates using the Dallas control group confirm our main results.

We conclude that the traffic safety benefit of camera programs is much smaller than the consensus view in the existing transportation and engineering literatures. In the case of Houston, our preferred estimates suggest that the change in social welfare from implementing the camera program was negative.

More generally, our study highlights the challenge of using policy tools to deter crime in situations where potential offenders face multiple, offsetting risks.

2 Driver Behavioral Model

This section outlines our model for understanding the impact of electronic monitoring on driver behavior and the number of traffic accidents. We show that the effect on total accidents and injuries from installing a camera at an intersection is ambiguous. Our model predicts that electronic monitoring will decrease certain types of accidents (e.g., right angle), and increase other types of accidents (e.g., rear end).

Beckers 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 (potentially) to 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 upon the likelihood that the drivers 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., 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 drivers 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 red-light camera 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 camera remains low, for it requires a police officer to witness the infraction. The probability of receiving a ticket when there is a camera 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.

In the first year after the end of electronic monitoring, the number of tickets issued citywide (17,282) barely exceeded the number of red-light-running tickets issued at a single intersection (17,055) in the final fiscal year of Houstons camera program. Figure 2 panel A plots the average number of red-light-running tickets per fiscal year for Houston camera intersections. The number of tickets issued dropped by 99.91% in the year after electronic monitoring ended.

Previous studies confirm that the number of vehicles running a red light at an intersection declines after a camera is installed (e.g., Martinez and Porter

[2006]; Porter et al. [2013]; Erke [2009]; Retting et al. [2003]). Using direct observations of driving behavior at eight city intersections, Martinez and Porter [2006] conclude that the incidence of red-light running fell by 67% during the eight months immediately after the camera installation. 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 the cameras, and that a year after removal the rate of running a red light is similar as to before the camera was installed.

The total number of tickets issued at camera intersections also supports the prediction of a decrease in red-light running after a camera is installed. In general, the number of tickets issued for running a red light at a camera intersection peaks immediately after the installation of the camera and then begins to decline as drivers learn about the camera and adjust their behavior. Figure 2 panel B plots the average yearly number of citations per intersection by year of operation for Dallas camera intersections. On average, in the first year of a camera-monitored intersection, more than 6,000 citations are issued. By the fourth year, the number of citations has declined close to 75%, and then levels off.[5]

While there is clear evidence that installing a camera 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. Following Gazis et al. [1960], our model tracks the distance required for a vehicle approaching a traffic intersection to safely decelerate and stop. This distance depends on the engineering characteristics of the vehicle (e.g., weight, brakes) and the roadway (e.g., surface conditions), 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

[5] We are unable to produce a similar figure for Houston because we are only able to access intersection level citation reports for two years of Houstons program (2008-9 and 2009-10).

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 Federal Highway Administration recommends that the yellow light interval be between three and six seconds (Administration [2009]). The yellow phase of the traffic safety light can be made arbitrarily long in order to allow all vehicles beyond this minimum distance to pass through the intersection before the light turns red. In practice, though, drivers enter the "dilemma zone" (Gazis et al. [1960], p5): 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.

With the introduction of electronic monitoring, certain types of accidents are likely to decrease. Some drivers who typically accelerate through the dilemma zone will choose to stop at the intersection and, in turn, fewer vehicles will be in the intersection when the cross-road light turns green. Right-angle crashes between two vehicles are likely to decrease which is, in fact, the primary public safety goal of most camera programs (Erke [2009]). The size of this reduction depends upon the timing of when vehicles that choose to stop would have otherwise been in the intersection. There is evidence that the vast majority of red-light violations 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 red light camera program may have only a limited effect on reducing cross-street collisions.

At the same time, electronic monitoring is likely to increase other types of accidents. Here we consider four reasons. First, all drivers will now accept a higher accident-related cost from attempting to stop. The marginal driver who was willing to continue through the pre-camera intersection will now choose to stop, provided that $\frac{\partial S_{i,j,t}}{\partial A_{i,j,t}} < \frac{\partial C_{i,j,t}}{\partial F_{i,j,t}}$. Our deterrence model predicts that the marginal driver will choose to stop and accept the greater risk of a non-angle accident, along with the associated 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 the first two reasons depend on imperfect information or calculation errors 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 a camera program may incentivize drivers to attempt to stop when it would be safer to continue. Fourth, drivers may simply miscalculate. 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]).[6]

The overall effect of a red light camera 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 red light camera program on total accidents, as well as the effect on specific accident types.

3 Background and Data Sources

3.1 Houston and Dallas Red Light Camera Programs

All camera programs share several characteristics. A camera is installed in a location where it can take photos (or video) of vehicles as they pass through the intersection. The camera is positioned so that photos include the vehicle in the intersection and its license plate. Photos of all vehicles captured passing through the intersection are to be reviewed by city employees, a contractor, or both, in order to verify that the light is red and that the license plate is clearly

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

visible. Traffic tickets are then sent to the home address of the individual who registered the vehicle. The main characteristics on which camera programs differ include whether signage identifies the camera program of the intersection, whether the cameras are permanent fixtures at an intersection or are mobile units, and whether 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]). The first 33 Dallas cameras were installed in 2007, along with 22 more between 2008-2011. The Dallas program also issued $75 fines, and in fiscal year 2008-9 gave out 129,000 tickets. In Houston and Dallas, programs included posted signs advising drivers of the cameras, permanently placed cameras, and issued tickets for red-light infractions, but not for speeding. The Dallas camera program remained in place throughout our panel.

In November 2010, Houston residents voted 53% to 47% in favor of a referendum to remove the cameras. The referendum was organized by citizens who opposed the camera program on the grounds that the cameras were mainly a revenue-raising policy. At the time of the referendum, a majority of members on the Houston City Council approved of the 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 cameras (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 cameras were settled by January 2012 (Houston Mayor's Office [2012]).

3.2 Data Sources

3.2.1 Intersection Information

We use information on camera intersections from the annual (fiscal year) camera intersection reports of the Texas Department of Transportation (TxDOT) (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). Unfortunately, the Houston report for 2010-11 (covering the last four months of the camera program) was not published. Another data limitation is that ADT is measured only once at most of the camera intersections and not updated annually.

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 Governments [2016]). The use of street-based (rather than intersection-based) ADT information allows us to have a consistent ADT measure for camera and non-camera intersections in each city. Intersections are assigned ADT values using GIS software by summing the ADT values for all roads at the intersection. The appendix includes details regarding the ADT calculation. We compare the intersection ADT data using this measure with the ADT data reported by TxDOT for camera intersections. The ADT means are similar, with 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 has a median separating traffic, the speed limit, the number of lanes, and whether the intersection includes a frontage road. A frontage road runs parallel to a highway and often provides an access point to the highway.[7]

[7]Intersection characteristics were collected using Google Maps, Google Mapmaker, and Waze 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 2003-2014 accident data from the TxDOT Crash Records Information System (CRIS) includes all reported motor vehicle traffic accidents in the state (TxDOT [2004-16]).[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 camera intersections (and are on one of the roads that cross at the intersection). Recall that figure 1 indicates much higher accident rates within 200 feet of an intersection. We further restrict our sample to those accidents where law enforcement personnel determined that the accident was "in or related to" an intersection, rather than an adjacent parking lot, for example. We define these accidents as "intersection accidents." We include intersection accidents only in our main estimation panels.

Table 1 shows average yearly accident statistics for Houston for the three years before the start of the camera program (2003-2005). Panel A displays statistics for all accidents, while panel B only displays statistics for intersection accidents in our main Houston panel. We calculate each statistic separately for all accidents, angle accidents, and non-angle accidents. We define "angle accident" as an accident type listed in CRIS that includes the word "angle." There are 49 accident types listed in CRIS, of which ten include the word "angle." The appendix includes a complete list of accident types.[9]

[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 2003 and 2009 were obtained via an open records request under the Texas Public Information Act from The University of Texas at Austin Center for Transportation Research.

[9] Each of the ten angle accident types include a more precise description. The most common angle accident is "Angle: Both Going Straight," which involves 78% of angle accidents.

Of the 77,000 accidents per year in Houston, 34% are in or related to an intersection. The proportion of angle accidents is larger among intersection accidents than for all Houston accidents (32% versus 21%). On average, there are 231 fatalities per year. The likelihood of being killed in an intersection accident is the same for angle and non-angle accidents conditional on each accident type.

The CRIS database includes six accident injury designations: fatality, incapacitating, non-incapacitating, possible, unknown, and none. The categories are mutually exclusive. If multiple individuals are injured in an accident, and the severity of the injuries span two or more of the injury categories, then the accident designation corresponds to the most severe injury. For example, if the accident includes a fatality and an incapacitating injury, then the accident is to be designated as a fatality accident.

The probability of incurring a non-fatal injury is greater for individuals involved in angle accidents at an intersection than for non-angle accidents at an intersection. There are approximately twice as many incapacitating angle accidents than non-angle accidents at an intersection (0.02 versus 0.01). Moreover, the fraction of non-incapacitating injury accidents among angle accidents is 0.11, whereas for non-angle accidents it is 0.06. With intersection angle accidents determined to be more dangerous than intersection non-angle accidents, a change in the composition of the types of accidents could have important welfare implications, even if there were no effect on the total number of accidents.

Figure 3 plots the average total number of vehicle accidents per intersection by year from 2003-2014. Panel A plots accident levels for camera 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 camera. Panel B plots accident levels for two groups of intersections in San Antonio, a city without a red light camera program. We separately plot the 66 most dangerous intersections from 2003, along with all other San Antonio intersections with 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.[10]

Panel A provides initial evidence that the introduction of cameras in Houston and Dallas, and the subsequent removal of the Houston cameras, had no discernible effect on the number of total accidents. If the camera 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 camera groups. The average number of intersection accidents peaks in 2003 for both Houston camera groups, and then decreases at roughly a constant rate from 2005-2008. There is also no clear evidence that ending the program in 2010 led to an increase in the number of accidents. The timing of the increase for the two Houston camera groups does not match the end date of the program. Moreover, the overall increase for the Houston camera groups towards the end of the panel is similar in magnitude to that of the Dallas camera group where the monitoring program continued to operate.

Panel A also shows two other facts regarding the Houston camera intersections. First, on average, the Houston camera intersections are more dangerous than the Houston non-camera intersections. The average number of total accidents during this period is about five times larger at Houston camera intersections. Second, the Houston camera locations appear to have been chosen based on an unusually large number of accidents in the years prior to the 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 camera intersections, as a "longer time period will provide more reliable information on collision causes" (p1).

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

Panel B shows that the most dangerous intersections in San Antonio from 2003 display a similar accident pattern as the Houston camera intersections, even though San Antonio never had a camera program. There is approximately a 50% reduction in the number of accidents in both cities. In figure 3, Panels A and B highlight the challenge in evaluating the effect of electronic monitoring when camera intersections are positively selected on the number of accidents. A simple difference-in-differences model based around the start of the Houston program would over-estimate its effectiveness at reducing accidents relative to the Houston no camera group. 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 a Houston intersection receives a red-light camera. Below we discuss how we use 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 electronic monitoring on traffic accidents, injury accidents, and traffic patterns.

The intersections considered for our estimating sample in our differences-in-differences model are summarized in table 2. Our treatment group is comprised of all Houston red-light camera intersections. We use three control groups. The first two control groups use Houston intersections that never had a camera and 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-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. Camera intersections in Dallas make up the third control group (Panel B). The Dallas camera 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 in order to rule out infrequently traveled intersections. This restriction may also exclude intersections that, for whatever reason, appear to be extremely safe and are thus not comparable to camera intersections. Second, the control intersection cannot be within one-half mile of a camera intersection. Previous research suggests that driving behavioral responses to a camera intersection could affect driving behavior at other intersections in 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 Houston and the Houston-Dallas samples (ADT data are not available for the pre-period (2003-2005) for our longer Houston sample). ADT data allow us to control for vehicle traffic levels. We also use the ADT data to test whether traffic patterns at camera 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 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

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

where the dependent variable $y_i \in (0, 1)$ is the estimated probability that intersection i is a Houston camera 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 red light camera intersection selection process (Department [2016];

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, non-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 samples, and one ADT observation for the 2008-2014 Houston sample.[11] Variable $\hat{y}_i$ corresponds to each intersection's estimated likelihood, or propensity score, of being a Houston camera intersection (Rosenbaum and Rubin [1983]). The propensity score for the Houston-Dallas sample represents the probability that an intersection with those characteristics would be located in Houston.

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]. Appendix figure 1 shows the distribution of propensity scores in our three main samples. The overlap in the propensity scores for the treatment and control intersections is best for the 2008-2014 Houston sample, which is one reason why it is our preferred sample. The availability of the ADT information, and the opportunity to also provide out-of-city control group estimates, are two additional reasons why we prefer the 2008-2014 Houston sample to the longer Houston panel.

Table 2 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 2003-2014 Houston sample. Column 3 shows the difference in mean intersection characteristics between the pre-trimmed treatment group (column 1) and control group (column 2), normalized by the stan-

[11] 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, for 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 fewer Dallas camera intersections than Houston camera 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).

dard deviation. This approach to evaluating the differences in means 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 camera intersections is much better balanced than the non-trimmed Houston sample, although still significantly differs on five of the six accident characteristics. After trimming with the propensity score, the accident characteristics are much more similar between treatment and control groups in each sample. Appendix figures 2 and 3 show 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 engineering characteristics. 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. For example, 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 camera intersections in Houston are on frontage roads. In robustness analysis we consider a sample that only evaluates intersections on frontage roads.

In the engineering characteristics, greater differences arise. These characteristics are not measured in the pre-referendum period and 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. For example, the speed limit is about three miles per hour greater for the treatment group. The one exception is whether an intersection is a frontage road. In Houston, 82% of the camera intersections are on frontage roads. In robustness analysis, we consider a sample that only evaluates intersections on frontage roads.

Figure 4 shows intersection level accident trends for the treatment and control groups for the Houston (left column) and Houston-Dallas (right column) samples. Figure 5 shows the same trends for the Houston 2003-2014 sample. The figures plot the residuals from an OLS regression that includes a vector of intersection fixed effects as the only independent variables. The figures indicate 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 panel of figure 4 plots the average number of yearly angle accidents for a Houston camera intersection (squares) and a Houston control intersection (triangles) that cannot 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 for the non-camera intersections than for the camera 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

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

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.

The accident information are count data. As such, we estimate the model using a Poisson regression and maximum likelihood estimation. The estimated coefficients can be interpreted as semi-elasticities. We also estimate the model using OLS, which provides very similar (percent change) results. An assumption of the Poisson model is the equivalence between the conditional mean and conditional variance. However, the use of robust standard errors relaxes this assumption (DeAngelo and Hansen [2014]).

Recall that we estimate equation 4 on three main samples. In the Houston and Houston-Dallas samples, we estimate only the effect of the referendum (i.e., terms with C_t are dropped from the model). These samples consider the pre-referendum period while the Houston cameras 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-camera period (2003-2005).

Table 2 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, as a robustness check we also estimate a model that weights the regression by the inverse of the propensity score as a robustness specification (Manski and Lerman [1977]; Hirano et al. [2003]). If the propensity score correctly predicts the probability of treatment (i.e., a Houston intersection with a camera), 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 camera intersections had there been no referendum. The similar pre-referendum trends shown in figures 4 and 5 support this assumption.

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

5.2 Traffic Accidents

Table 3 shows the coefficient of interest for the effect of ending the red light camera program on accident levels (δ_1) for six separate regressions using the difference-in-differences model. Panels A and B show estimates for the Houston and Houston-Dallas samples, respectively. We estimate each model separately for angle accidents (column 1), non-angle accidents (column 2), and total accidents (column 3).

We find support for the three main predictions of the behavior model in section 2. First, the model predicts differing treatment effects for the two types of accidents. We can reject equivalence between the coefficient estimates for angle and non-angle accidents. In the Houston sample, the probability value for a null hypothesis that the angle and non-angle accidents are equal is 0.000.

Second, the model predicts that electronic monitoring will lead to an increase in non-angle accidents. Non-angle accidents will increase when there are cameras as drivers will trade off a higher accident risk from stopping with the higher expected fine from continuing through the intersection. When the camera program ends, we estimate a statistically significant decrease in non-angle accidents of 18% in the Houston sample and 28% in the Houston-Dallas sample.

Third, the model predicts that the reduction in red-light running under the camera program will lead to fewer angle accidents. The size of the reduction in angle accidents will depend on the accident risk of the vehicles that had been running a red light. Previous studies find that, without electronic monitoring, the majority of vehicles running a light do so just after the light turns red, when there is a low accident risk (e.g., Yang and Najm [2007]).

We find modest evidence that the camera program reduced angle accidents. If the electronic monitoring program had been effective at reducing the number of accidents, then we would expect to observe an increase in the number of angle accidents after the program ended. The coefficient estimate of 26% is economically and statistically significant in the Houston sample, but is nearly zero in the Houston-Dallas sample.

Finally, there is no evidence that electronic monitoring decreased the number of total accidents. The model in section 2 shows that that the predicted effect on total accidents is ambiguous and depends on the offsetting effects of the two accident types. We estimate negative and statistically insignificant coefficients for the change in total accidents in our Houston (-3%) and Houston-Dallas (-17%) samples. There are close to twice as many non-angle accidents at an intersection (table 1 panel B). Thus, a change in the percentage of non-angle accidents has a larger impact on the overall change in total accidents.

5.3 Injury Accidents

We do not find any evidence that electronic monitoring led to a reduction in total accidents. However, it is possible that the change in the composition of accidents under the camera program could result in more injury accidents. Table 1 shows that the typical angle accident is more dangerous than the typical non-angle accident. Moreover, estimating the effect on injuries is important for understanding the overall welfare effect of the camera program.

Table 4 shows estimation results for the effect of ending the camera program on the number of accident related injuries using our difference-in-differences model. An "injury accident" includes one or more reported injuries or deaths (i.e., excluding the unknown and possible injury categories). We separately estimate the effect for injury accidents, incapacitating injury accidents, and non-incapacitating accidents. Columns (4)-(6) use the number of annual reported accident-related injuries for each intersection as the dependent variable. These specifications reflect the fact that accidents with multiple people injured are more harmful than accidents where only one person is injured. We separately analyze different types of injuries to account for the large difference in the economic costs associated with the severity of an injury (e.g., Shin and Washington [2007]; Blincoe et al. [2015]).

There is no evidence that the electronic monitoring led to fewer accident-related injuries. Estimates from the Houston sample suggest that the camera program may have increased injuries. The point estimates are all negative after the program ends, and are marginally statistically significant for a reduction in injury accidents. Estimates from the Houston-Dallas sample imply that the overall change in injuries is close to zero.

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 30 percent in injury accidents (Panel A, column 1) corresponds to a decrease of approximately 26 injury accidents per year across all camera intersections in Houston after the camera program ends, or about one fewer injury accident per 55 million vehicles passing through an electronically monitored intersection.[12] The estimates from a model that is weighted by the propensity score are similar in statistical significance and suggest a somewhat larger decrease in injuries after the end of electronic monitoring.

[12] We calculate the change in the implied number of accidents by taking the product of the point estimate (-.295), the yearly mean for all the treated intersections in the sample from table 1 (1.33), and the number of camera intersections (66). We calculate the reduction in the accident rate as the total amount of annual vehicle traffic at camera intersections divided by the number of avoided injury accidents: $(59,223 * 365 * 66)/26$.

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 cameras as a means to save time or to avoid fines. Appendix table 2 provides some evidence on how average daily traffic at an intersection changes after electronic monitoring ends.

We estimate a simple OLS difference-in-differences model (equation 4 without the fixed effects) for the subset of intersections in our Houston sample that have one pre-referendum and one post-referendum ADT observation. We estimate the model with and without propensity score weights.[13] The four estimates imply increases in traffic at Houston camera intersections after electronic monitoring ended of between 0 and 18 percent. None of the estimates is statistically significant.

We interpret these estimates as suggestive evidence that there may have been a small shift in driving patterns. 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 estimates would be an underestimate and biased towards zero. However, there are a number of caveats to the ADT estimates: ADT is not measured in the same years for all intersections; the data are only available for a subsample of intersections in Houston; and, prior to electronic monitoring, there is no way to observe whether the ADT trends are similar between treatment and control intersections.[14] Finally, if there is measurement error in the interpolation procedure used to assign the ADT data to intersections (see Appendix for details), then the ADT estimates are likely to be attenuated towards zero.

[13]We use the same propensity score weights as those used in the accident analysis.

[14]Pre-referendum ADT values are measured between 2007-2010, while post-referendum values are measured between 2011-2014.

5.5 Robustness Analysis

5.5.1 The Effect on Accidents and Injuries from Starting the Red Light Camera Program

Our focus is on estimating the effect of the exogenous removal of the cameras so as to avoid endogeneity concerns over the camera start time for the selected intersections. Nevertheless, we can also use our modeling framework to evaluate the effect on traffic accidents and injuries when the cameras are first installed. The model in section 2 predicts that, with electronic monitoring in place, angle accidents will decrease and non-angle accidents will increase; and, once the cameras are removed, the opposite trend will prevail. The estimates in table 5 mostly confirm this pattern.

The Houston 2003-2014 sample allows us to examine the effect of both the installation and removal of the cameras. Table 5 panel A shows the estimated installation treatment effect (δ_2) from equation 4, while panel B shows 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. The estimates in table 5 are from a balanced panel that drops data from the years of camera installation.[15]

We estimate that installing the cameras leads to 11% fewer angle accidents and 9% fewer non-angle accidents, while removing the cameras increases angle accidents by 10% and decreases non-angle accidents by 9%. The signs of three of the four point estimates are in the expected direction, but they are imprecise and none is statistically significant. The point estimate that is in the unexpected direction is for non-angle accidents after installation of the cameras. One interpretation of this result is that it again highlights the challenge of using the endogenous camera start times to estimate the effect of electronic monitoring on vehicle accidents. Overall, we estimate that ending the cam-

[15]The cameras were installed in 2006 and 2007. There is no way to correctly assign the observation as pre- or post-treatment since the pre-2010 accident data are aggregated by year. We drop all data from 2006 and 2007. Estimates on an unbalanced panel that only drops camera intersection observations from the year of installation give very similar results.

era program reduced accidents by 3% (statistically insignificant), which is the same as the estimate for the Houston sample in table 3.

5.5.2 Alternative Specifications

Table 6 shows four robustness specifications. The relevant comparisons are the removal estimates for our main Houston sample (table 3, panel A).

Panel A shows OLS estimates that suggest a percentage change and statistical significance similar to those using the Poisson model. Panel B drops 2011 accidents from our analysis. The Houston cameras 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 company by turning off the cameras. The results are similar regardless of whether we include 2011 data in our post-referendum period.

Panel C estimates our model using inverse propensity score weighting (Manski and Lerman [1977]; Hirano et al. [2003]). Overall, table 2 shows that the accident characteristics are well-balanced. There are, however, slightly more non-angle accidents at camera intersections than non-camera intersections during the pre-referendum period (and therefore slightly more total accidents at camera intersections). If the propensity score correctly predicts the likelihood that a Houston intersection has a camera, then reweighting by the propensity score will eliminate selection bias. On the other hand, if the propensity score is not correctly specified, then reweighting could exacerbate underlying selection differences (Freedman and Berk [2008]). We do not know the exact selection rule used by Houston officials and view the propensity score as approximating the selection criteria. As such, our preferred specification does not weight by the propensity score. Nevertheless, our estimates are similar under inverse propensity score weighting.

Panel D estimates the model on a sample that drops observations outside the range of observations, for the camera and non-camera groups, respectively. Recall that our preferred sample already limits the sample to observations with propensity scores between 0.1 and 0.9. Restricting the sample to observations within the "common support" implies dropping two additional camera intersections with propensity scores greater than 0.75 (see Appendix figure 1). The estimated point estimates are similar to those from our preferred sample.

Finally, estimating our model on a sample that only includes frontage intersections (not shown) leads to larger, more negative difference-in-difference coefficients for each of the four dependent variables in table 6, relative to our baseline estimates. However, the estimates are imprecise, for they include only nine camera intersections and five non-camera intersections.

6 Social Welfare Analysis

6.1 Conceptual Framework

In this section we outline a framework to interpret how electronic monitoring at traffic intersections affects social welfare. Our discussion closely follows Chalfin and McCrary [Forthcoming].[16]

We assume that there are n identical individuals, all of whom drive, and that the social planner maximizes the expected utility of the representative agent. Let $\phi_j(R)$ be the probability of experiencing accident outcome j (i.e. fatality, injury, vehicle damage) when a city has R red light cameras. Define k_j as the average cost of outcome j. We write expected accident costs as $C \equiv C(R) = \sum_{j=1}^{N} k_j \phi_j(R)$.

Time delays associated with the camera program, T, are an additional cost. We model the cost of the time delay as $T \equiv T(R) = \sigma w m R$, where w is wage, m are the average number of minutes delayed per person per camera, and σ is a multiplier on the value of a driver's time. Multiplier σ captures two effects: the fraction of the wage at which a driver values travel time, and a delay multiplier that reflects the observation that travelers dislike waiting in

[16]There are three main differences between the models. First, Chalfin and McCrary [Forthcoming] model the size of the police force. Second, our model includes the cost of travel time delays associated with the camera program. Third, we use the model to evaluate the extensive margin of having a camera program (66 Houston cameras cover close to 7% of the citys major intersections (see table 2)). As such, we consider the social welfare comparative static derived from the model as an approximation.

traffic (e.g. Parry and Small [1999]; Anderson [2014]).[17]

Define $y(R) = A - \tau$ as consumption when there are no direct accident-related costs. A is assets and τ is the per-person lump-sum tax equal to the cost of running the camera program. Let $\tau = (rR)/n$, where r is the per camera cost of the program and n is the city's population.

$$V(R) = y(R) - C(R) - T(R) \tag{5}$$

Social welfare is maximized when the first derivate of equation 5 is zero. Social welfare will improve under an expansion of the electronic monitoring program if $V'(R) > 0$. We can use this first order condition to derive a simple comparative static, equation 6, to evaluate whether a change in the number of cameras is welfare improving.[18]

$$|\varepsilon| > \frac{rR + nT}{nC} \tag{6}$$

$\varepsilon = \frac{\sum_{j=1}^{N} k_j \phi_j(R)\varepsilon_j}{\sum_{j=1}^{N} k_j \phi_j(R)}$ is an aggregate elasticity equal to the cost weighted sum of the accident outcome elasticities ε_j. The right hand side of the inequality is a ratio of the total dollar costs under electronic monitoring to the total expected accident costs. Electronic monitoring of traffic intersections improves welfare if it passes the cost-benefit test in equation 6. The cost weighted improvement in accident safety under electronic monitoring must exceed the ratio of program costs to accident costs in order for electronic monitoring to be welfare improving.

The electronic monitoring program should be revised or suspended if $\varepsilon > 0$, or if $\varepsilon < 0$ but does not satisfy equation 6. When $\varepsilon > 0$, electronic monitoring increases accident costs (i.e., the benefit is negative). One exception to the

[17]Parry and Small [1999] estimate the value of travel time as half that of a drivers wage, and the delay multiplier as 1.8. In our context, we are interested in travel time delays that can be captured by the product of the two effects.

[18]We assume that all citizens drive and that each driver is a potential offender and victim, utility is linear (Chetty [2006]), traffic fines are lump-sum transfers that don't affect social welfare, and ϕ_j is differentiable and strictly convex. The key step in solving for equation 6 is multiplying the first order condition by R/C.

decision rule given by equation 6 is if the improvement in accident safety ($\varepsilon < 0$) does not satisfy the inequality, but the program allows for other law enforcement resources (e.g., police officers) to be used more effectively. We return to this possibility after evaluating the baseline model.

6.2 Houston Camera Program Social Welfare

Table 7 shows camera program and traffic accident statistics for Houston. The information in Table 7 can be used, along with equation 6, to evaluate whether the camera program had positive welfare effects.

The annual cost to operate each camera (including annualized fixed costs) is almost $90,000. We follow the recent literature and set the value of a drivers time at half the average wage (Anderson [2014]). We calculate the number of minutes delayed by multiplying the length of the average red light at one of the 66 camera intersections by the estimated number of additional vehicles that stop under the camera program (rather than continue through the light). The accident injury risk rates are calculated over the camera intersections using data for the two years prior to the referendum that shut off the cameras. Accident injury costs are provided by the National Highway Traffic Safety Administration and include direct injury costs (e.g., hospital), economic costs (e.g., lost wages), and quality-of-life costs (Blincoe et al. [2015]). We use the Department of Transportations recommended value of statistical life, $8,860,000, as the cost of a fatal accident (Blincoe et al. [2015]). Finally, we estimate the accident-related injury elasticities using our difference-in-differences model. For example, the non-incapacitating injury estimate (0.12) is the same as in table 4, panel A column 6.[19]

Panel B of the table shows the summary statistics necessary to evaluate equation 6. The expected annual accident cost for a Houston resident attributable to the 66 camera intersections during the last two years of the camera program is $72. Eighteen percent of this figure is the result of the four fatalities at these intersections during this period.

The ratio of the program costs to accident costs is provided under two assumptions. Recall that the cost-weighted elasticity must imply a beneficial effect, and be of a magnitude greater than this cost ratio, in order for the camera program to be welfare improving. We calculate a ratio of 0.094 under our most conservative assumptions, which includes the assumption that the increase in the number of vehicles stopping is due only to vehicles that would otherwise have run a red light. The ratio increases to 0.126 when we assume that there are just as many vehicles stopping under the camera program that would have passed through the intersection while the light was still yellow.

Using the injury coefficients from our model, we estimate that the cost-weighted elasticity is 0.123. In other words, our point estimates imply that the camera program led to an increase in accident injury-related costs and had a negative welfare effecteven before accounting for the costs of running the program. The injury estimates, though, are imprecise. If we were to use instead the upper end of the 95% confidence interval, then the camera program is welfare improving ($|-0.311| > 0.126$).

[19] All dollar estimates in the table are in 2010 $. Setting the value of a drivers time at half of the average wage is conservative as it effectively ignores the delay multiplier (Parry and Small [1999]). There is also recent research suggesting that the value of time may be non-linear and substantially higher in urban areas during rush hour traffic (Bento et al. [2017]). The average length of a red light (i.e., wait time) calculation is conservative as it assumes that there is no turning only phase of the traffic light. The number of additional vehicles stopping at the light is conservative as it is estimated only off of red light violations and assumes that no vehicles stop rather than pass through the yellow light. We multiply our regression point estimates by -1 to make the elasticity estimates more intuitive (since we estimate the response to a reduction in cameras, i.e., ending the program). The incapacitating injury estimate in table 4 differs from that in table 4, panel A column 5 because the table 4 estimate also includes fatalities. In table 7 we assume that the fatality elasticity is the same as for incapacitating injuries. The appendix provides further details on how each statistic is calculated and additional information on the data sources.

The welfare analysis is fairly insensitive to how we handle fatalities. On average, there are two fatalities per year during the program and two fatalities per year after the end of the program. The welfare conclusion remains unchanged, whether we use a lower VSL estimate, or completely ignore fatalities in equation 6. The larger challenge to analyzing social welfare is that the year-to-year variability in traffic accidents, when combined with the low frequency of the most costly injuries, lead to imprecise regression estimates. This imprecision makes it difficult to statistically reject the (cost-weighted) change of 9% (i.e., the lower bound of the cost ratio in equation 6 for Houston).

Finally, it is possible that an electronic monitoring program could fail to satisfy equation 6, but still improve social welfare for the city This scenario would include, for example, a reduction in cost-weighted accident injuries, and a reallocation of the law enforcement personnel previously dedicated to intersection monitoring to another welfare improving activity.[20]

There is no evidence of a significant reallocation of police resources related to traffic signal enforcement after the Houston camera program ends. The average number of red-light running citations per year during the last three years of the camera program (2008-2010) is 18,738. In the subsequent four years, law enforcement personnel issued an average of 16,998 tickets per year (2011-2014). The 9% reduction in citations implies that, if anything, police reallocate time away from monitoring intersections when the camera program ends.

7 Conclusion

Electronic monitoring of traffic intersections is a common policy to enforce traffic laws in the US. The stated goal of red light camera programs is to reduce cross road collisions and to improve public safety. However, a simple crime deterrence model predicts that a camera program will decrease angle accidents, while increasing non-angle accidents. An increase in non-angle accidents under

[20]The welfare gain from the new activity would need to be larger than the gap between the left and right hand sides of equation 6: $\frac{rR+nT}{nC} - |\varepsilon|$.

a camera program is not an incidental or anomalous outcome. The underlying mechanism is that drivers will knowingly trade off a higher accident risk from stopping in order to avoid the expected fine of running a red light. Whether a camera program improves safety is an empirical question.

One challenge in estimating the effect of electronic monitoring on vehicle accidents is that intersections with cameras are likely to be among the most dangerous intersections in the city. Moreover, the start of electronic surveillance is endogenous and could follow a spike in accidents at the intersection. We show that both empirical challenges are true in Houston, TX.

We estimate a difference-in-differences model using 12 years of geocoded police accident data and find evidence that angle accidents increased and non-angle accidents decreased in Houston after ending the camera program. We avoid the endogenous start of a camera program by examining driver behavior after the cameras are unexpectedly shut off via a voter referendum. The effect on total accidents is close to zero and statistically insignificant. We adapt the social welfare model of Chalfin and McCrary [Forthcoming], which allows us to incorporate the fact that some types of accidents are more dangerous than others. The social welfare impact of Houston's camera program is negative when we use the accident-related injury point estimates from our preferred model. However, the year-to-year variability in traffic accidents within a city, combined with the low frequency of the most serious injuries, makes definitive social welfare analysis difficult. Given the imprecision of the injury estimates, we cannot rule out the possibility that the program is welfare improving.

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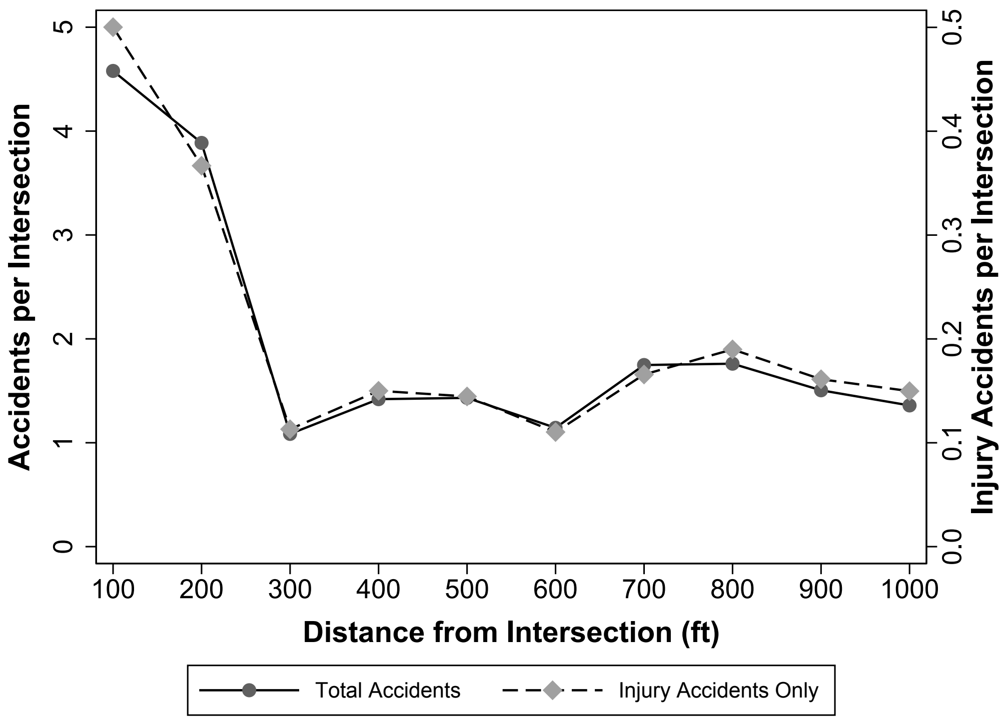
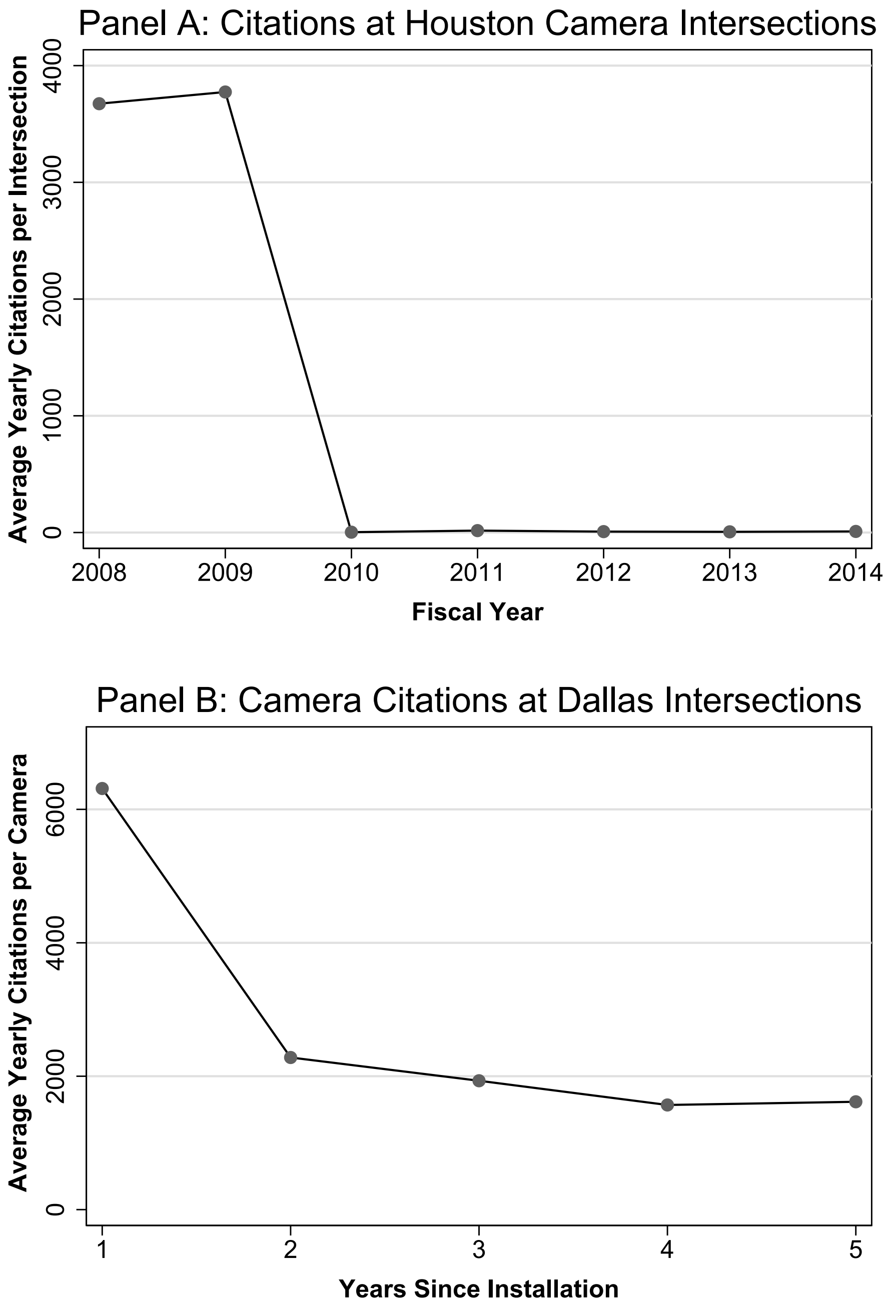
9 Figures and Tables

Figure 1: **Average Yearly Accidents and Injury Accidents by Distance from an Urban Intersection**



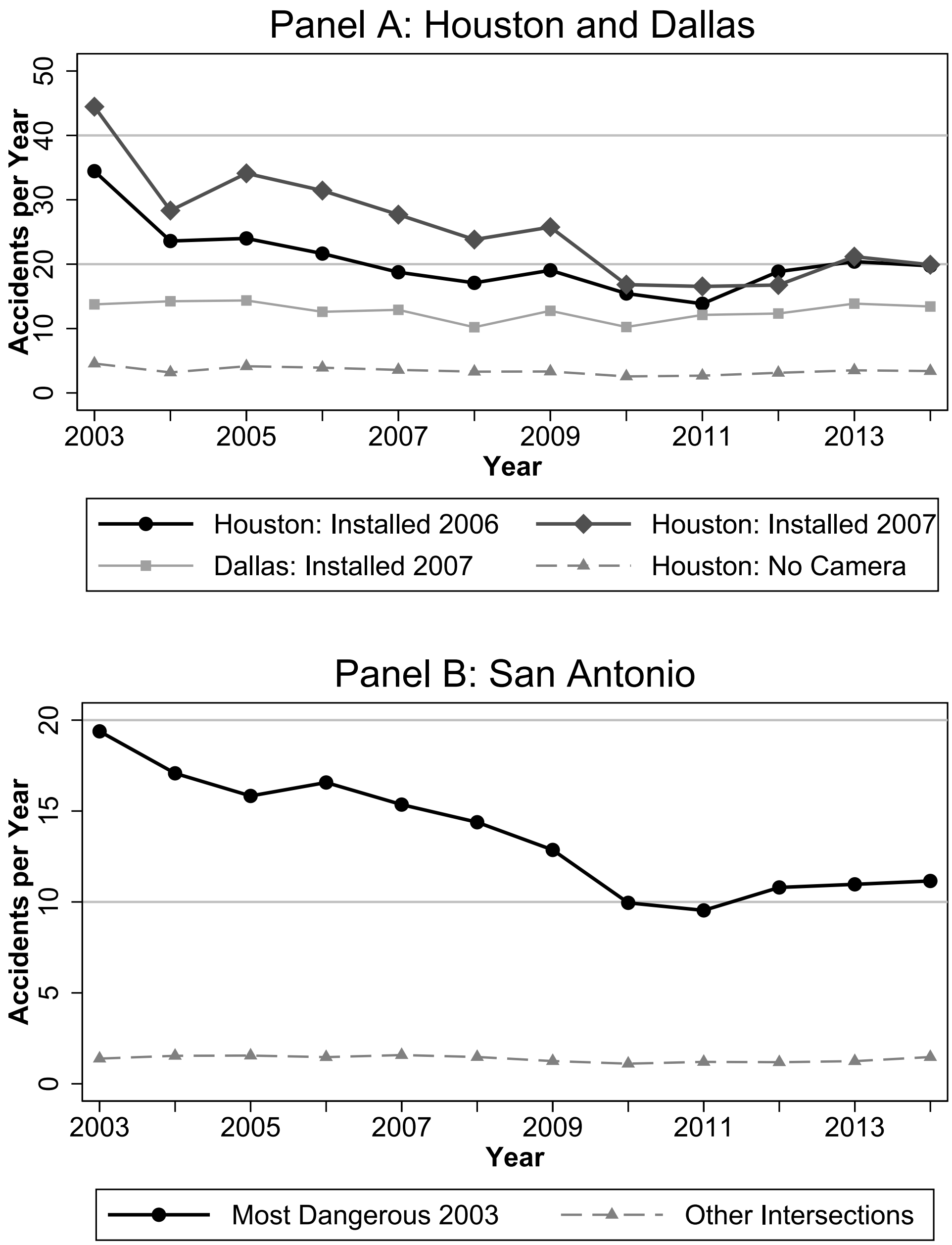
The figure plots average yearly total accidents and injury accidents by distance from a Houston intersection in 100-foot bins for the years 2003-2014. The data include all accidents classified as in or related to the intersection by the police who recorded the accident. An "injury accident" includes one or more non-incapacitating injury, incapacitating injury, or death. The figure does not control for the fact that many of the accidents that are farther away from the reference intersection may be less than 200 feet from another intersection. Data sources: Texas Department of Transportation.

Figure 2: **Red Light Citation Rates**



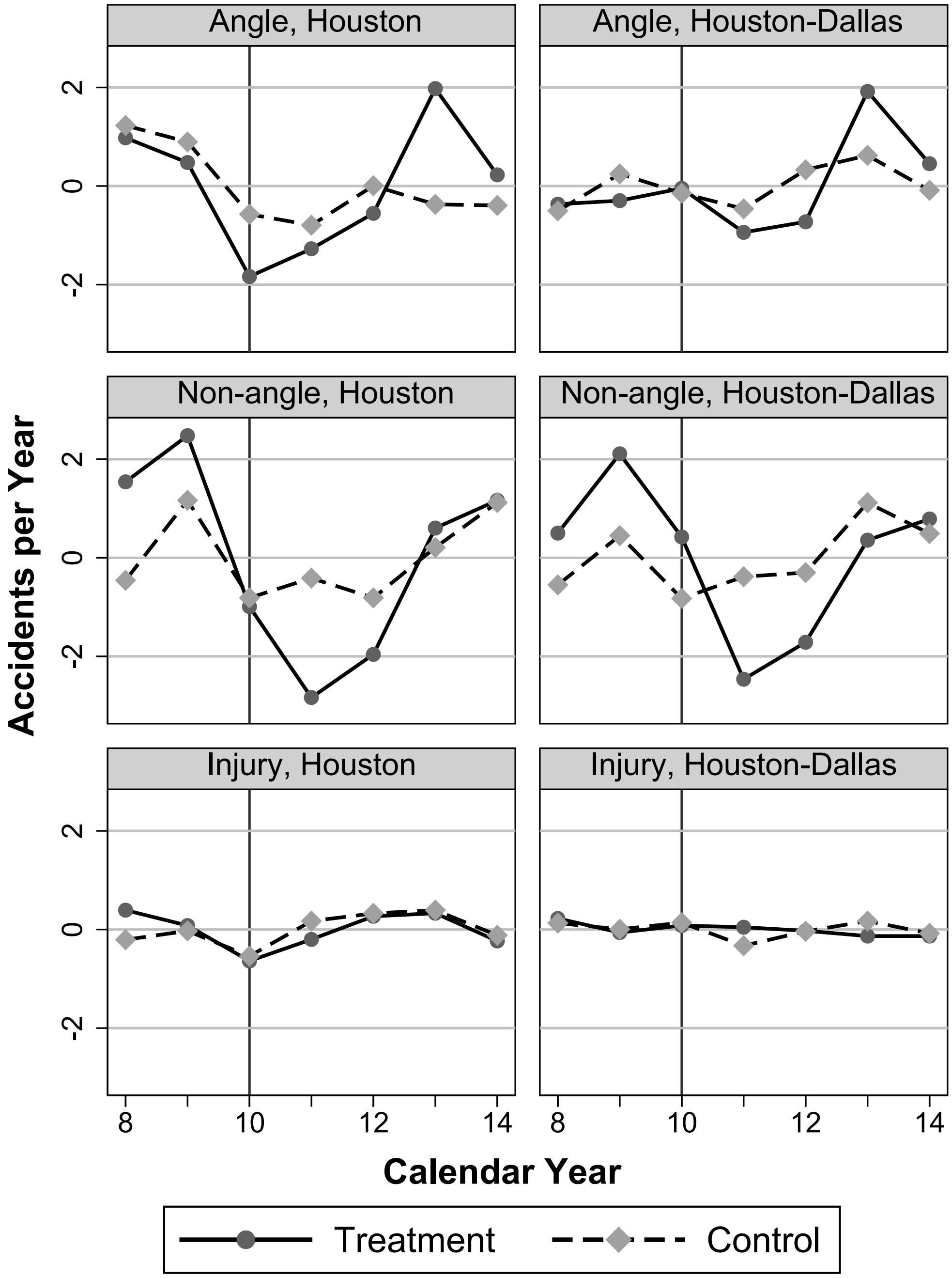
Panel A plots the number of annual (fiscal year) red-light-running citations at the 66 Houston camera intersections from 2008-2014. 2008 and 2009 include both camera initiated citations and citations from law enforcement officials. The points for 2010-2014 are for after the camera program ended and include only law enforcement citations. Missing from the figure are the camera citations for the first four months of fiscal year 2010 (July-October). To our knowledge, these data were never made public. Panel B plots the number of annual camera citations by intersection and years since installation for Dallas camera intersections. The figure reports citation data from two cameras for year one, 37 for years two to five, and 29 for year six. Fiscal year reports with camera citation information are not available (or not usable) for all years of the Dallas program. See the data appendix for details. Data sources: City of Houston, Texas Department of Transportation.

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



Panel A shows the 2003-2014 trends in yearly intersection traffic accidents in Houston and Dallas, for four groups of intersections based on the year of camera installation and city. The Houston program ended in 2010. The Dallas program continued through 2014. Panel B shows accident rates separately for the 66 most dangerous San Antonio intersections (equal to the number of Houston camera 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. San Antonio does not have a camera program. 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: **Treatment and Control Intersection Accident Trends 2008-2014 Houston and Houston-Dallas Samples**



The figure plots yearly accident residuals from an OLS regression of yearly angle (row 1), non-angle (row 2), and injury (row 3) 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 camera and (2008-2010) propensity score matched non-camera intersections. Treatment and control intersections in the Houston-Dallas sample (right column) are Houston and Dallas camera intersections. The accident data from 2010 are multiplied by 6/5 before running the regression, in order to account for 10 months of available data. Data source: Texas Department of Transportation.

Figure 5: **Treatment and Control Intersection Accident Trends 2003-2014 Houston Sample**

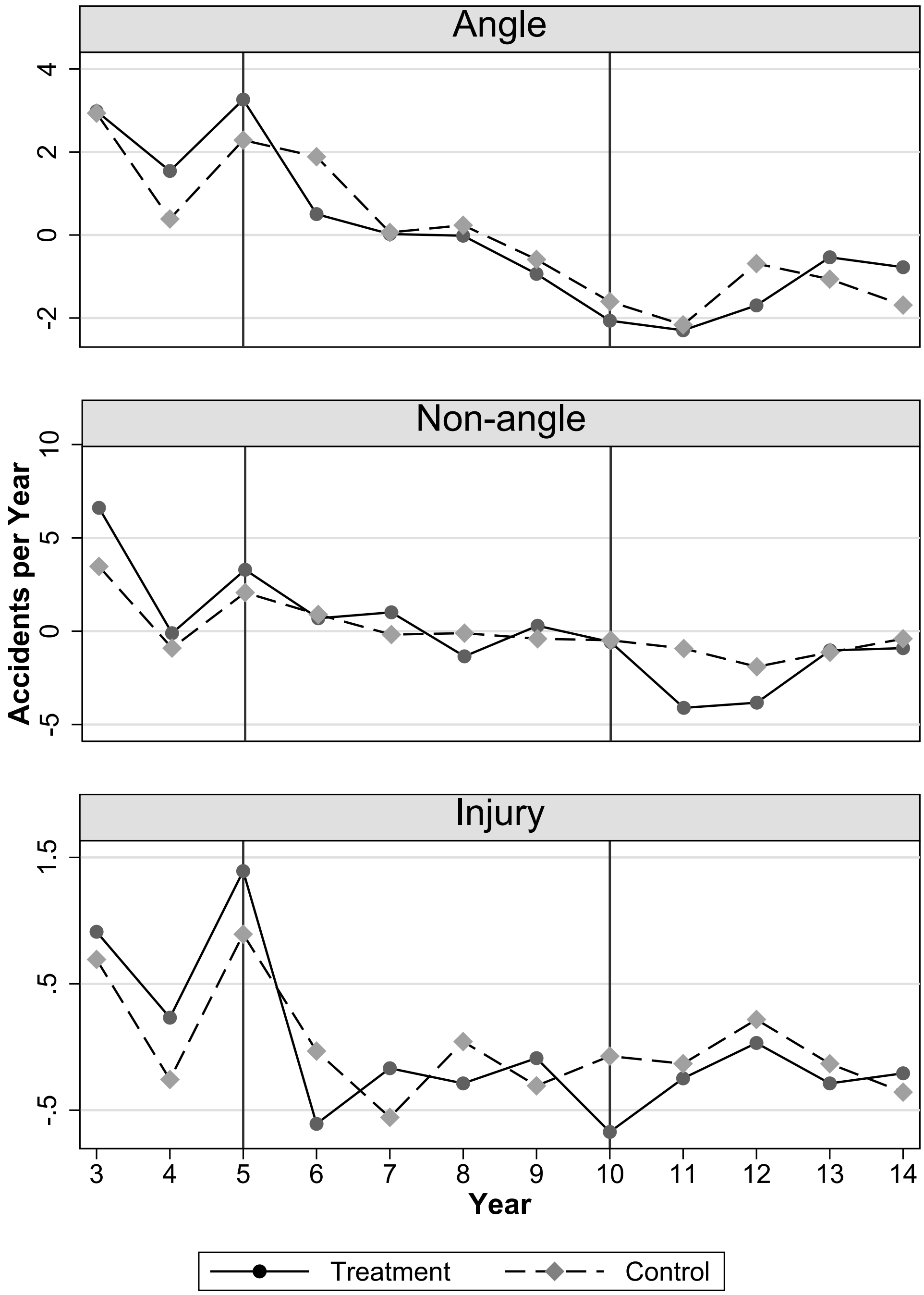


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Table 1: **Accident and Injury Descriptive Statistics**

Average Yearly Statistics	(1)	(2)	(3)
Accident Type:	All	Angle	Non-angle
Panel A: All Houston Accidents			
Total Accidents			
Number of Accidents	77,552	16,233	61,319
Fraction of Accidents by Type	1.00	0.21	0.79
Number of Fatalities	231.00	35.67	195.33
Fraction "In or Related to" Intersection	0.34	0.77	0.23
Injury Accidents, Fraction by Severity			
Fatality	0.003	0.002	0.003
Incapacitating Injury	0.016	0.020	0.015
Non-Incapacitating Injury	0.070	0.095	0.064
Possible Injury	0.285	0.387	0.257
Unknown Injury	0.371	0.212	0.413
No Injury Classification	0.360	0.397	0.351
Panel B: Houston Sample Intersection Accidents			
Total Accidents			
Number of Accidents	3,333	1,066	2,267
Fraction of Accidents by Type	1.00	0.32	0.68
Number of Fatalities	7.33	3.67	3.67
Injury Accidents, Fraction by Severity			
Fatality	0.002	0.003	0.002
Incapacitating Injury	0.015	0.022	0.012
Non-Incapacitating Injury	0.079	0.120	0.059
Possible Injury	0.338	0.433	0.293
Unknown Injury	0.320	0.211	0.371
No Injury Classification	0.370	0.342	0.383

The table shows average yearly accidents in Houston for the three years before the camera program (2003-2005). Panel A displays statistics for all accidents, while panel B displays statistics only for intersection accidents in our main Houston panel. There are six accident injury designations: fatality, incapacitating, non-incapacitating, possible, unknown, none. The categories are mutually exclusive. If there are multiple individuals injured in an accident, the accident designation corresponds to the most severe injury. Source: Texas Department of Transportation.

Table 2: **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	20.64	3.07	2.48	16.24	12.58	0.54
Angle	7.78	1.13	2.10	5.02	4.62	0.12
Non-angle	12.86	1.94	2.38	11.22	7.96	0.57
Injury	1.89	0.33	1.60	1.33	1.06	0.21
Red-light Running	6.43	0.74	2.12	3.81	3.52	0.11
Average Daily Traffic	58,540	29,811	1.49	59,223	48,796	0.38
Engineering Characteristics						
Frontage Road	0.82	0.01	3.33	0.78	0.04	1.54
Lanes	7.33	4.21	1.82	7.03	6.04	0.59
Speed Limit	39.93	33.38	1.35	39.86	36.78	0.64
Divided	0.92	0.70	0.49	1.00	0.91	0.40
Number of Intersections	66	938		32	45	
Panel B: Houston-Dallas Control (2008-2010)						
Accident Characteristics						
Total	20.64	11.07	0.69	13.11	10.21	0.33
Angle	7.78	2.93	0.67	4.26	2.75	0.38
Non-angle	12.86	8.14	0.55	8.85	7.46	0.21
Injury	1.89	1.43	0.23	1.21	1.18	0.02
Red-light Running	6.43	2.70	0.58	3.37	2.50	0.26
Average Daily Traffic	58,540	43,881	0.54	60,759	42,175	0.57
Engineering Characteristics						
Frontage Road	0.82	0.33	1.02	0.75	0.38	0.76
Lanes	7.33	7.55	-0.16	7.14	7.54	-0.26
Speed Limit	39.93	36.19	0.85	39.75	36.15	0.83
Divided	0.92	0.85	0.25	0.89	0.83	0.17
Number of Intersections	66	33		28	24	
Panel C: Houston Control (2003-2005)						
Accident Characteristics						
Total	33.12	3.97	2.84	24.59	18.43	0.68
Angle	14.93	1.63	2.59	8.93	7.58	0.27
Non-angle	18.19	2.33	2.64	15.65	10.85	0.60
Injury	3.25	0.41	2.24	2.57	2.08	0.31
Red-light Running	11.39	1.05	2.58	6.63	5.69	0.23
Engineering Characteristics						
Average Daily Traffic	58,540	29,811	1.49	51,812	44,214	0.31
Frontage Road	0.82	0.01	3.33	0.63	0.03	1.38
Lanes	7.36	4.21	1.83	7.04	6.30	0.45
Speed Limit	39.93	33.38	1.35	39.11	35.25	0.78
Divided	0.92	0.70	0.49	0.88	0.95	-0.28
Number of Intersections	66	938		24	40	

The table shows the means for accident and intersection characteristics for the three samples before and after propensity score trimming. Houston camera intersections are the treatment group for all three samples. The control groups include Houston non-camera intersections (Panels A and C) and Dallas camera intersections (Panel B). The means are taken over the years indicated for each sample. Data sources: City of Houston, Google Maps, North Central Texas Council of Governments, Texas Department of Transportation.

Table 3: **The Effect on Accidents from Ending the Camera Program**

	(1)	(2)	(3)
Dependent Variable:	Angle	Non-angle	Total
Panel A: Houston Sample			
*After Removal * Treated*	.261*	-.179*	-.03
	(.138)	(.099)	(.097)
Equality of Angle and Non-angle, p-value:	0.000		
Treatment Intersections	32	32	32
Control Intersections	45	45	45
Panel B: Houston-Dallas Sample			
*After Removal * Treated*	.013	-.276**	-.171
	(.179)	(.132)	(.12)
Equality of Angle and Non-angle, p-value:	0.123		
Treatment Intersections	28	28	28
Control Intersections	24	24	24

The table shows the difference-in-differences coefficient of interest for the *removal* of the Houston cameras from estimating equation 4 using a Poisson model. The dependent variable is the yearly number of angle (column 1), non-angle (column 2), and total accidents (column 3). All panels estimate propensity score trimmed samples. The Houston sample (2008-2014) uses Houston non-camera intersections as the control group. The Houston-Dallas sample (2008-2014) uses Dallas camera intersections as the control group. Both samples include all police-reported, "intersection-related" accidents within 200 feet of an intersection. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by intersection, * < 0.10, ** < 0.05, *** < 0.01. Source: Texas Department of Transportation.

Table 4: **The Effect on Injuries from Ending the Camera Program**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:	Injury Accidents			People Injured		
Injury Classification:	All	Incapacitating	Non-Incapacitating	All	Incapacitating	Non-Incapacitating
Panel A: Houston Sample						
*After Removal * Treated*	-.295*	-.621*	-.229	-.152	-.435	-.116
	(.176)	(.330)	(.194)	(.228)	(.357)	(.233)
Treatment Intersections	32	32	32	32	32	32
Control Intersections	45	45	45	45	45	45
Panel B: Houston-Dallas Sample						
*After Removal * Treated*	.018	-.575	.114	.027	-.547	.113
	(.221)	(.578)	(.229)	(.263)	(.600)	(.269)
Treatment Intersections	28	28	28	28	28	28
Control Intersections	24	24	24	24	24	24

The table shows the difference-in-differences coefficient of interest from estimating equation 4 using a Poisson model on the Houston and Houston-Dallas samples. An injury accident includes one or more injuries or fatalities. Incapacitating accidents include a fatality or incapacitating injury. Non-incapacitating accidents exclude injury accidents with a fatality or incapacitating injury. Columns (4)-(6) use the number of annual reported accident-related injuries for each intersection as the dependent variable. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by intersection, * $<$ 0.10, ** $<$ 0.05, *** $<$ 0.01. Source: Texas Department of Transportation.

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

	(1)	(2)	(3)	(4)
Dependent Variable:	Angle	Non-angle	Total	Injury
Panel A: Installation				
*Camera On * Treated*	-0.111	-0.090	-0.087	-.298*
	(0.126)	(0.119)	(0.091)	(0.156)
Equality of Angle and Non-angle, p-value:	0.902			
Treatment Intersections	25	25	25	25
Control Intersections	40	40	40	40
Panel B: Removal				
*After Removal * Treated*	0.098	-0.088	-0.025	0.095
	(0.132)	(0.112)	(0.103)	(0.190)
Equality of Angle and Non-angle, p-value:	0.141			
Treatment Intersections	25	25	25	25
Control Intersections	40	40	40	40

The table shows the difference-in-differences coefficient of interest for the installation and removal of the Houston cameras from estimating equation 4 using a Poisson model and the 2003-2014 Houston sample. The dependent variable is the yearly number of angle (column 1), non-angle (column 2), total (column 3), and injury accidents (column 4). All panels estimate propensity score trimmed samples. The sample uses Houston camera intersections as the treatment group and Houston non-camera 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. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by intersection, * $<$ 0.10, ** $<$ 0.05, *** $<$ 0.01. Source: Texas Department of Transportation.

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*	1.127	-1.820*	-.700	-.355
	(.728)	(.991)	(1.463)	(.265)
Percent Change	29.0	-24.7	-13.2	-6.0
Equality of Angle and Non-angle, p-value	0.002			
Treatment Intersections	32	32	32	32
Control Intersections	45	45	45	45
Panel B: Drop 2011				
*After Removal * Treated*	.309**	-.126	.021	-.235
	(.149)	(.099)	(.101)	(.197)
Equality of Angle and Non-angle, p-value	0.001			
Treatment Intersections	32	32	32	32
Control Intersections	45	45	45	45
Panel C: Propensity Score Weighted				
*After Removal * Treated*	.217	-.191*	-.051	-.394**
	(.144)	(.109)	(.106)	(.191)
Equality of Angle and Non-angle, p-value	0.001			
Treatment Intersections	32	32	32	32
Control Intersections	45	45	45	45
Panel D: Common Support				
*After Removal * Treated*	.233	-.193*	-.049	-.404**
	(.147)	(.106)	(.104)	(.178)
Equality of Angle and Non-angle, p-value	0.001			
Treatment Intersections	30	30	30	30
Control Intersections	45	45	45	45

The table shows four robustness specifications for the difference-in-differences coefficient of interest from estimating equation 4 on angle, non-angle, total, and injury accidents. The estimates in this table are comparable to those from table 3, panel A, and table 4, panel A, column (1). Panel A of this table estimates the same model using OLS rather than Poisson. Panel B excludes data from 2011 from the sample. Panel C uses inverse propensity score weighting. Panel D limits analysis to camera and non-camera observations that lie in the same propensity score range. Standard errors (in parentheses) are robust to heteroskedasticity and clustered by intersection, * $<$ 0.10, ** $<$ 0.05, *** $<$ 0.01. Source: Texas Department of Transportation.

Table 7: **Houston Camera Program Social Welfare**

Panel A: Welfare Model Statistics

Statistic	Value
Annual cost per camera [r]	89,496
Population age 18-65 [p]	1,331,812
Average wage [w]	28.3
Wage multiplier [σ]	0.5
Minutes delayed per capita per year [m]	0.0025
Accident injury risk per capita per year, multiplied by 100,000 [ϕ]:	
Fatality	0.15
Incapacitating	0.64
Non-incapacitating	5.26
Possible	20.42
No Injury	28.16
Accident injury costs (1,000's $) per person [kj]:	
Fatality	8,860
Incapacitating	1,001
Non-incapacitating	276
Possible	128
No Injury	42
Accident elasticity estimates [ε_j] (point estimate, 95% CI):	
Fatality	-
Incapacitating	0.39 [-0.28, 1.07]
Non-incapacitating	0.12 [-0.34, 0.58]
Possible	-0.02 [-0.33, 0.28]
No Injury	0.01 [-0.28, 0.31]

Panel B: Welfare Calculation

Yearly expected accident cost [C]	72
Ratio of program cost to accident costs:	
Assume no deterred yellow light vehicles	0.094
Include deterred yellow light vehicles	0.126
Cost-weighted elasticity estimates [ε]:	
Using estimated point estimates	0.123
Using 95% upper confidence inverval	-0.311

The statistics in the table can be used to evaluate the social welfare of the camera program using equation 6. All dollar estimates in the table are in 2010 $. We multiply our injury outcome difference-in-difference coefficient estimates by -1 to make the elasticity estimates more intuitive, since we estimate the response to a reduction in cameras (i.e., the end of the program). Sources: American Community Survey, Bureau of Labor Statistics, National Highway Traffic Safety Administration, Texas Comptroller, Texas Department of Administration, Texas Transportation Institute, US Department of Transportation.