Spatial Economics Research Centre

SERC DISCUSSION PAPER 221

Do Speed Cameras Save Lives?

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September 2017

This work is part of the research programme of the Urban Research Programme of the Centre for Economic Performance funded by a grant from the Economic and Social Research Council (ESRC). The views expressed are those of the authors and do not represent the views of the ESRC.

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I would like thank my supervisors, Steve Gibbons and Henry Overman for their detailed comments and guidance over the course of writing this paper. I am grateful to Vernon Henderson, Olmo Silva, Christian Hilber, Felipe Carozzi, Sefi Roth and all the other participants in the LSE WIP Seminar and Spatial Econometrics Association in SMU. I would also like to thank all the local camera partnerships for providing locations of enforcement cameras. All remaining errors are my own.

Abstract
I evaluate whether speed enforcement cameras reduce the number and severity of traffic accidents by penalizing drivers for exceeding speed limits. Relying on micro data on accidents and speed cameras across Great Britain, I find that installing these devices significantly enhance road safety. Putting another 1,000 cameras reduce around 1130 collisions, 330 serious injuries, and save 190 lives annually, generating net benefits of around £21 million. However, these effects are highly localised around the camera and dissipate over distance, and there is suggestive evidence of more collisions away from the camera, illustrating the possible limitations associated with fixed speed cameras.

Keywords: accidents, injuries, fatalities, speed camera, speeding

JEL Classifications: H23; I18; R41

1 Introduction

Across the world, an estimated 50 million individuals are hurt from traffic collisions, with 1.2 million succumbing to these injuries every year (Peden *et al.*, 2004). Traffic accident is a serious health issue that has disproportionately affected the younger generation, causing many life years lost. For instance, in United Kingdom (UK), motor crashes are the leading cause of deaths for those between 5 and 34 years old and account for more than 15% of the deaths in this age group. Overall, 186,189 individuals are involved in collisions, out of which 162,315 suffer from slight injuries, 22,144 are seriously harm and 1,730 eventually died (DfT, 2016) in 2015 across UK. It is 3 and 8 times more likely to die from traffic accidents relative to homicides and HIV/AIDs. Translating the cost of accidents to dollar value, collisions cost UK a total of £10.3 billion just in 2015[1].

Many reasons explain why crashes occur and why it can be fatal. The loss of control of the vehicle could be due to carelessness, distraction, intoxication and speeding. According to Department for Transport (DfT), speeding accounts for more than 60% of the fatal accidents in UK in 2015 (e.g exceeding speed limit, travelling too fast for conditions, loss control of vehicle, swerved vehicle). Similarly, other than the size and the built of auto-mobile, the use of seat belts, terrain, weather and road conditions, the severity of the crashes is dependent on the velocity of the colliding vehicle. While speeding might be considered a menial offence to many, it is evident that it is immense in determining both the probability and gravity of crashes.

Different laws and regulations have been introduced to prevent traffic accidents. Since the seminal paper by Peltzman (1975), evaluating these interventions have drawn considerable interest from economists. These include texting bans (Abouk & Adams, 2013), speed limits (Ashenfelter & Greenstone, 2004; van Benthem, 2015), traffic police (DeAngelo & Hansen, 2014), drinking (Dee, 1999; Hansen, 2015) and seat belt laws (Cohen & Einav, 2003). Falling back to the economic models of crime (Becker, 1968), these instruments deter reckless driving through punishment. Another widely used strategy is speed camera that penalizes driver for exceeding speed limits. They are often deployed at stretches of road particularly prone to collisions (e.g windy, hilly roads, near schools and petrol stations).

There are several reasons why it is important to evaluate the efficacy of speed cameras. First, it is a controversial instrument. There are concerns whether it improves road safety or it is simply an instrument to rake up revenues. Just in 2015, a total of 166,216 fines were issued in England and Wales that amount to more than £31 million[2]. It is argued that alternative strategies, such as vehicle-activated speed limit sign, could be equivalently

[1]These figures are much larger figures in United States. A federal study conducted by National Highway Traffic Safety Administration reveals that estimated economic cost from motor crashes is approximately US$242 billion in 2010 (Administration *et al.*, 2014).

[2]Read more at `http://www.bbc.co.uk/news/uk-38724301`

effective in improving road safety[3] but at a fraction of the cost. Interests groups have since put up multiple petitions to remove these cameras[4]. Second, there are immediate policy implications as many of the older devices are wet-film cameras that require upgrading to digital technology. However, upgrades have been held back due to cuts to the Road Safety Grants by the Coalition Government. In fact, several local partnerships, including Oxfordshire, West Midlands, Avon and Somerset, Wiltshire, Swindon and Northamptonshire, are forced to switch off their cameras. If fixed speed cameras are efficacious in improving road safety, then these devices should be upgraded and switched back on. Finally, questions are also raised whether these devices induce more collisions due to "kangaroo" effects (Elvik, 1997). That is when drivers abruptly slow down in proximity to the camera or immediately speed up beyond surveillance, causing more accidents further away from the camera. Thus, the objective of this paper is to address these questions through rigorous empirical analyses.

In this paper, I estimate the effects of fixed speed cameras on reducing occurrence and severity of collisions. To do so, I put together a rich dataset of more than 2,500 fixed speed cameras across England, Scotland and Wales (Great Britain). To measure accident outcomes, I rely STATS19 Road Accident Dataset that documents every reported collision from 1979 onwards. This allow me to conduct the analysis at a fine spatial scale and to capture enforcement effects moving away from the camera. In short, I compare accident outcomes before and after the camera is introduced with comparable sites using a quasi-experimental difference-in-difference framework.

For the estimates to be valid, it requires the mean differences in unobserved characteristics between sites not to be correlated with the installation of enforcement cameras. This assumption, however, is likely to be violated given the endogenous process of choosing sites. Cameras are often found at areas prone to collisions and this selection process is likely to accentuate the differences between sites with and without cameras. I adopt several strategies to mitigate endogeneity. First, I restrict the analysis to only sites that will ever have enforcement cameras and rely on time variation of installation for identification. That is, sites with cameras install in future (but no cameras now) will be employed as reference groups for sites having installation now. Second, I constraint reference groups to sites that received installations less than six years apart from those sites treated now. The notion is that sites treated further apart could be more dissimilar. This could be the case if the 'worst' sites receive cameras first. Next, I include a vector of time-variant city level characteristics

[3] See https://www.publications.parliament.uk/pa/cm200708/cmhansrd/cm080422/debtext/80422-0003.htm for more information

[4] Read http://www.safespeed.org.uk/ for more information

to control for region-specific shocks that could correlate with camera installations. Finally, to further attenuate observable differences, I pair each camera site with their most similar later-treated counterfactual with a stringent exact matching requirements based on camera and road characteristics .

I contribute to the existing literature in several ways. Earlier papers, largely restricted to the transportation literature (See Table 7 in Data Appendix for details), have either no or loosely constructed control groups and this could severely bias the estimates. I circumvent this issue by carefully selecting untreated sites to control for trends in accident outcomes in the absence of camera installations. Second, in contrast to previous papers, which are usually city-specific analyses restricted to a small sample of cameras, I exploit a representative national dataset to increase the external validity of the research. Third, with fine spatial temporal information on accidents and speed cameras, I can accurately capture how enforcement effects vary across space, allowing us understand whether cameras exacerbate collisions away from the site. Finally, utilizing estimates from my analysis, I compute the welfare effects associated with speed cameras to provide rigorous assessment whether these devices should be deployed.

The headline finding is that speed cameras unambiguously reduce both the counts and severity of collisions. After installing a camera, the number of accidents and minor injuries fell by 17%-39% and 17%-38%, which amounts to 0.89-2.36 and 1.19-2.87 per kilometre. As for seriousness of the crashes, the number of fatalities and serious injuries decrease by 0.08-0.19 and 0.25-0.58 per kilometre compared to pre-installation levels, which represents a drop of 58%-68% and 28%-55% respectively. Putting these estimates into perspective, installing another 1,000 speed cameras reduce around 1130 collisions[5], mitigate 330 serious injuries, and save 190 lives annually[6], generating benefits of around £21 million[7]. These findings are robust across a range of specifications that relaxes identification assumptions

[5] These estimates are taken from the preferred specification in Column (7) of Table 2.

[6] The ratio of lives save in my study is much higher than the average national accidents death ratio over the last 10 years from 1995 to 2015 (1.02%). There are several explanations to this. First, speed cameras are often found along roads with a much larger proportion of death related accidents. The pre-treatment percentage of deaths from collisions around speed camera sites is 2.50% (see Table 1) more than twice the national ratio. Second, by reducing speed through deterrence, cameras could have disproportionately mitigated more severe accidents. Another explanation is that speed cameras are less effective in preventing collisions compared to deaths. Possible kangaroo effects, such as sudden braking in front of camera, or speeding up beyond surveillance, could have attributed to more collisions.

[7] This is obtained from multiplying the net benefits from welfare analysis in Table 5 by 1,000.

I further allow enforcement effects to vary across different speed limits, road types, and across distance. My results show that it is more effective to install cameras along roads at higher speed limits as much larger reductions in collision outcomes are observed. This could be because these roads are more dangerous in the first place as drivers commute at higher speeds. In addition, enforcement effects appear highly localised around 500 metres from the camera and dissipate moving away. Beyond 1.5 kilometres from the camera, there are suggestive evidence of a rebound in collisions, injuries and deaths, indicating drivers could have speed up beyond camera surveillance and cause more accidents. These results, which illustrate the limitations associated with fixed speed cameras, suggest that newer prototypes, such as mobile or variable speed cameras, should be considered.

The remainder of this paper is structured as follows. Section 2 provides a background to speed enforcement cameras in UK. Section 3 describes the identification strategy adopted in this paper. Section 4 outlines the data used in this paper and Section 5 discusses the findings in this paper. Section 6 concludes the study.

2 Background

Different enforcement cameras, including fixed, mobile and variable speed, are employed across UK. Fixed speed camera, which is the earliest generation of speed detecting devices, could be found as early as 1992 in London. Mobile and Variable speed camera are newer prototypes that only grew in prominence in the last decade. Mobile speed cameras are fixated on auto-mobiles with the flexibility to be deployed in different locations but require manpower to operate. Variable speed cameras enforce speed limit over a stretch by measuring average speed between two points on the road, having the advantage of reducing speed over a longer span. For an illustration refer to Figure 1. The focus of this paper is on fixed speed cameras as I can reliably determine the location and operating dates. The minimum penalty for speeding is £100 and 3 demerit points but this depends on how much the speed limit is exceeded.

Cameras are typically enforced by safety camera partnership, which is a joint collaboration of police force, local government, highway agency and health authorities. They work hand-in-hand to identify dangerous sites for enforcement. Sites that are chosen for fixed

(a) Fixed Speed

(b) Average Speed

(c) Mobile Speed

Figure 1: Different types of Speed Cameras used in United Kingdom

speed camera installation must comply with the following national selection rules (DfT, 2004)[8]:

1. Site length must be at a length of between 0.4 and 1.5 kilometres;
2. At least 4 killed and serious collisions (KSI) & 8 personal injury collisions (PIC) per kilometre in the 3 years before installation[9];
3. Suitable for the loading and unloading of cameras
4. At least 85% of the traffic is travelling is at or above the Association of Chief Police Officers (ACPO) threshold based on speed surveys;
5. At least 20% of the drivers are exceeding speed limits;
6. No other more cost effective solutions to improve road safety as determined by the road engineers.

[8] One other strategy is to utilize a regression discontinuity design over these rules and to obtain some local estimates around these thresholds. This is not adopted due to the following reasons. First, I do not have information on average speed, site length, suitability that affect whether a site receives camera enforcement. Furthermore, these rules are not deterministic for installation. It is possible for sites to have installations without meeting these rules, impeding identification of effects around these thresholds.

[9] One crash can result in multiple causalities. Adding up the number of slight injury collisions and KSI will provide the PIC count.

The first two guidelines are considered more important for enforcement. While not stated explicitly, I do find that many of these cameras are often found near schools, bus stops and petrol stations to ensure pedestrians safety. Even when not all the above requirements are met, enforcement could take place if a large number of non-fatal collisions due to speeding are recorded. These sites are classified as *exceptional sites*. This ambiguity impedes the use of observable characteristics to identify comparable reference groups. The local partnership also decides whether to install mobile, average or fixed speed cameras. Fixed speed cameras are usually deployed when many accidents cluster around the site. To commission new sites for camera installation, partnerships are require to provide full details on these proposed sites for the forthcoming year, subjected to approval by the national board. They are allowed to recover penalty receipts to cover the cost of camera installations and enforcement. Since 2006, there are slight amendments to the guidelines. In particular, the KSI requirements fall from 4 to 3. A risk value is computed for each site of which every KSI and PIC collisions are given 5 points and 1 point respectively. To qualify for camera installation, a site must at least have 22 points if the speed limit is 40mph or less and 18 points for speed limits beyond 50mph. For more details, refer to DfT (2005).

Once installed, several clear signages must be placed less than 1 kilometre away from the camera. This is to warn drivers about the presence of camera and to inform them about the speed limit. Since 2002, all the cameras are painted in bold yellow and must at least be visible from 60 metres if the speed limit is less than 40 mph and at 100 metres if speed limits are higher. This is to improve visibility to ensure that drivers do not abruptly reduce speed in and around the camera, leading to more crashes. These cameras are unattended but it is more expensive to maintain wet-flim older camera as it requires flim replacement. Newer cameras uses wireless digital technology to transmit offending data.

Most of the cameras across UK are *Gatsometer BV Cameras* that are single direction and rear facing. This means the camera will only take images of the back of a speeding vehicle so as not to blind the offender and impede driving performance. However, some of the newer cameras could be bi-directional[10] or front facing[11]. Majority of the cameras operate though radar technology although there are some that rely on strips on the roads for speed detection (e.g *Truvelo D-Cam*, *SpeedCurb*). If there is a dispute to the fine, the

[10] Cameras installed in the central of the road could be turned periodically to target motorists at either side of the road. Sometimes, multiple housings could be installed on both sides of the road. Newer devices such as the Truvelo D-Cam can take pictures at both directions.

[11] The second most popular type is *Truvelo Cameras* that takes an image of the speeding offender from the front using non visible infra-red flashes. The advantage is that there are no disputes towards who is driving the vehicle.

white lines on the roads near the housing will provide a secondary instrument to determine [12] whether drivers exceed speed limits. For an illustration of how speed cameras operate, refer to Figure 2. Figure 9 in Data Appendix illustrates the different prototypes across UK.

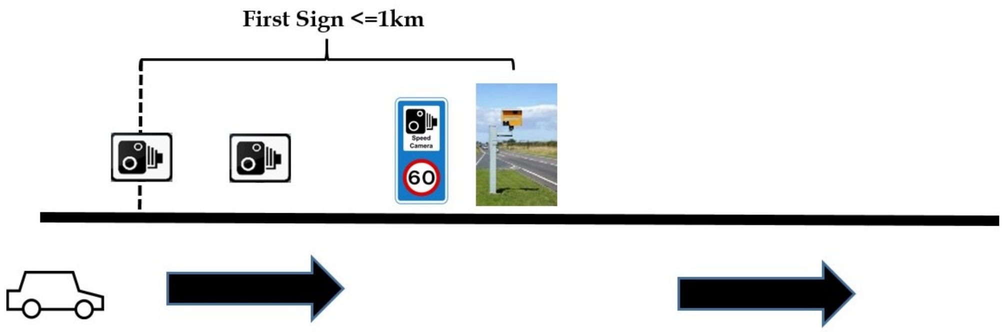

Figure 2: Illustration of how fixed speed cameras operate

3 Literature Review

Previous literature, largely from transport engineering , show that speed cameras reduce travel speed, accidents, injuries and fatalities near the camera (Gains *et al.*, 2004, 2005; Chen *et al.*, 2002; Shin *et al.*, 2009). These estimates, however, vary substantially across different studies. A survey of existing literature reveals that after enforcement cameras are installed, average speeds fell by between 1.7 and 4.4 miles per hour and crash reductions vary between 11% and 51%. For a review of the existing literature, refer to Wilson *et al.* (2010).

Existing empirical work, however, suffers from substantial limitations that questions the validity of the estimates. For one, researches are often limited to a small number of speed cameras constrained in a particular area (Chen *et al.*, 2002; Goldenbeld & van Schagen, 2005; Jones *et al.*, 2008; Shin *et al.*, 2009). This raise concerns on the external validity of these findings. This paper overcomes this limitation by analysing a more representative sample of cameras of up to 2,500 fixed speed cameras installed across England, Scotland and Wales.

Secondly, and perhaps most importantly, many studies are restricted to before-and-after analysis with either no or loosely constructed control groups to account for trends in accidents

[12]The distance between each of the white lines represent 5mph. Several images of moving vehicle over time will illustrate whether driver is speeding.

(Christie *et al.*, 2003; Jones *et al.*, 2008). Without controlling for the general downward trends of accidents due to technological advancements over time, such as better brake system, more robust car frame and improved road built, it is likely that the documented enforcement effects are biased. For studies with control groups, they do not account for the fact that camera location choices are endogenous. As mentioned before, selected sites are peculiar accident "black" spots with many drivers exceeding speed limits. Hence, sites with no camera installation are unlikely to be comparable. These differences, if unobserved or imprecisely measured, will enter the specification and bias the estimates. Without due consideration, studies also rely either on nearby roads (Newstead & Cameron, 2003; Perez *et al.*, 2007; Shin *et al.*, 2009) or identify reference groups based on road and traffic characteristics (Keall *et al.*, 2001; Cunningham *et al.*, 2008).

To create more comparable reference groups, some studies select sites based on data-generating methods like empirical bayes to identify reference groups with similar trends in accidents and traffic flow (Elvik, 1997; Chen *et al.*, 2002; Gains *et al.*, 2004, 2005). However, it is often not justified how reference groups are identified. To clarify on the matching process, Li *et al.* (2013) uses propensity score matching to determine reference groups based on observable co-variates (e.g traffic flow, pre-treatment accident rates) that are used for site selections. Still, it is improbable these strategies address the concerns as sites can have cameras even without meeting all the requirements. Moreover, it is possible that these sites are not treated because the surge in accidents are considered transient and it is likely to revert to mean levels even without intervention, underestimating enforcement effects. In this paper, I adopt the intuitive strategy of using only sites with cameras. That is, sites with cameras in future will be employed as reference groups for sites with installation now. In the subsequent sections, I will illustrate how the identification assumptions for this strategy are not violated.

Another point neglected by the literature is how the effectiveness of enforcement cameras vary over distance as the focus is usually around the camera. This is an important point as cameras could attribute to "kangaroo" effects (Elvik, 1997) - when drivers abruptly slow down their vehicle near the camera to avoid offending or speed up once beyond camera surveillance, inducing more collisions. Several studies, including Newstead & Cameron (2003); Mountain *et al.* (2004); Jones *et al.* (2008), try to break down the impacts across distance but the lack of fine spatial information meant that bandwidths are too big and results are thus uninformative. Relying on fine spatial information on accidents and speed cameras, I can delineate enforcement effects every 100 metres (up to 2 kilometres) to understand whether cameras cause kangaroo effects.

Finally, there is also a lack of analysis on how these enforcement cameras fare over time and over different speed limits. One of the few papers that addresses this is Christie *et al.* (2003) that look at how effects vary over time and speed limits. Their study, however, is constrained to an unrepresentative sample over a short period. Utilising detailed information on speed camera characteristics, and over a longer timespan, I inform how cameras perform over time and across roads with different speed limits. For a succinct summary of previous literature, refer to Table 7 in Data Appendix.

4 Data

To examine the effect of speed cameras on accidents, I put together a few data sources. First, I rely on STATS 19 Road Accident Database that provides detailed information for each reported accident to the Police in England, Wales and Scotland [13]. A wide set of details including location, time, date, road conditions, vehicle type, number of injuries, serious injuries and fatalities (pedestrians and inside the vehicle) are recorded. Shapefiles that provide detailed information of the road network and that delineate the boundaries for local authority districts[14] across United Kingdom are provided by Ordinance Survey.

Details of the speed cameras are hand-collected from websites of various local authorities provided by Department for Transport (DfT) [15]. For most of the local authorities, information on the location of camera housing, year of installation, speed limits and camera type are provided. For areas that did not provide these information, I request access using Freedom of Information Act (FOI). I classify whether these cameras are at rural or urban areas according to Rural-Urban classification shapefiles provided by Office of National Statistics (ONS).

Combining the various sources of information using Geographic Information System (GIS), I am able to match the location of speed cameras and accidents to the road network. To visualize, refer to Figure 3 and imagine the line as a particular stretch of road with a

[13] It is possible that there could be under-reporting of non fatal accidents to the police. This should not be an issue for more serious crashes that are usually reported to the Police. As long as the under-reporting of accidents is random across time and is not correlated with camera installation, it should not bias my estimates

[14] Local authorities are responsible of conferring government services within a district. In total, there are 353 different districts in England, 32 in Scotland and 22 in Wales.

[15] For more information on the list of https://www.gov.uk/government/publications/speed-camera-information

camera installed. With the exact location of each accident, I could sum accident outcomes along the road that the speed camera i is installed annually between k and $k - 100$ metres interval. For example, within 100 metres from the camera, all accidents that take place in area "A" in a particular year are accounted for. For my baseline estimates, which examine the effects 500 metres left and right of the housing, I will aggregate all the accidents that took place in "A", "B", "C", "D" and "E".

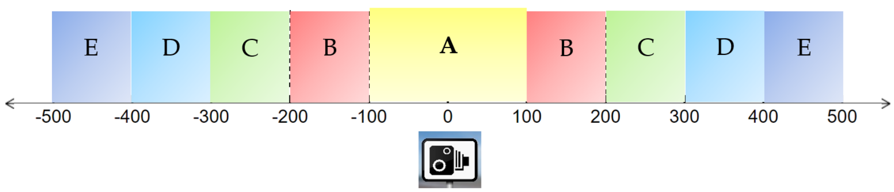

Figure 3: Illustration on how accident outcomes are computed across space

To capture the year-on-year variation in local authority characteristics, I rely on several sources. Information on the Annual Average Vehicle Miles Travelled (VMT) is collected from DfT. Details on the average earnings and number of hours worked are complied from Annual Labour Force Survey. Data on population profile are collected from Nomis Population Estimates. For details on how the variables are constructed, refer to Table 6 in Data Appendix.

5 Identification Strategy and Methodology

The research design adopted in this paper is a fixed effect, quasi-experimental difference-in-difference approach estimated using count regressions models. This is because collision outcomes follow an implicit count process that only takes non-negative integer values. Using Ordinary Least Squares (OLS) approaches, which specifies a conditional mean function that takes negative values, one could possibly yield inconsistent estimates (Cameron & Trivedi, 2013). Therefore, I implement two count models: Poisson and Negative Binomial. The latter was adopted because it relaxes the assumption that the conditional mean is equal to the conditional variance, allowing for over-dispersion in the data. This strong assumption for Poisson models are often violated. To correct for over-dispersion in Poisson regressions, following DeAngelo & Hansen (2014), I report sandwich (robust) standard errors.

Specifically, to examine the impact of speed cameras on traffic accidents, the following baseline specification is adopted:

$$E(Y_{ijt}) = exp(\alpha_i + \gamma \mathbf{T_{it}} + X'_{jt}\phi + \theta_t + \varepsilon_{ijt}), \tag{1}$$

where Y_{ijt} is the counts of Y (**Accidents, Slight Injuries, Serious Injuries, Deaths**[16]) within 0 & 500 metres from camera/site i in local authority j installed in year t.

The key variable of interest is $\mathbf{T_{it}}$, which is a binary variable that equals to unity after the speed camera is installed. If enforcement cameras are able to deter speeding along treacherous sites and improve road safety, I expect γ to be < 0. α_i represents the time invariant unobserved characteristics that influence whether a camera is installed. For instance, sites that are more precarious (e.g on a steep slope, windy roads) or are bypassing areas more susceptible to accidents (e.g schools, petrol stations) are more probable to be under enforcement. To partial out these effects, I exploit the variation of outcome Y over time with camera/site (i) fixed effects.

I further include a vector of time variant city-level controls at local authority j at year t (X'_{jt}). These variables include vehicle miles travelled, population size, percentage of population between 18 to 25 years old, gross annual pay and hours worked. This is to allay concern that there are regional shocks that could be correlated with installation of cameras and influence Y. For instance, if these devices are installed in areas that experience a spike in the proportion of teen drivers that could endanger road safety, γ could be underestimated. θ_t represents year fixed effects to partial any general time trends in Y across the regions. Technological advancements on car safety (better car frames, tires, air bags) and roads quality can reducing both the occurrences and severity of collisions over time. For more details on the description of the variables used in this paper, refer to Table 6 in Data Appendix.

ε_{ijt} is the error term. Endogeneity arises when $E[\varepsilon_{ijt}|\mathbf{T_{it}} \neq 0]$. This is likely to occur given the selection bias from camera installation. Roads with enforcement cameras are peculiar accident-prone roads with many drivers exceeding speed limits. Those without cameras are likely to very different from those with and such unobserved differences are

[16] According to the definition provided by the Department for Transport, slight injury is defined as an injury of minor character that do not require any medical attention. Serious injury is when the injury causes the person to be detained in the hospital for medical treatment and that the injury causes death more than 30 days after the collision. Deaths is defined as a human casualty who sustained injuries from the accident are die less than 30 days from the collision.

likely to enter into estimation via ε_{ijt}. Previous literature addresses this challenge by creating reference groups through trends in traffic flow and accidents using empirical bayes method (Elvik, 1997; Mountain *et al.*, 2005) or matching on observable characteristics Li *et al.* (2013). There are two main issues with using these sites. First, even with the stated parameters, it is problematic given that installations could occur even though some of the criterion are not met. In addition, eligible sites without cameras could be deemed by authorities as spots experiencing transient episodes of more collisions. Accident rates are likely to dip even in the absence of cameras and this could attribute to a downward bias to the enforcement effects.

Hence, the strategy adopted in this paper is to restrict the sample to only sites with cameras and exploit the variation in the timing of installation. This is possible as I can accurately determine the installation dates of fixed speed cameras. Identification stems from comparing changes in accident outcomes with changes on roads that will have cameras installed in the near future. The assumption is that sites that have enforcement cameras in the future are quite similar for sites that have cameras installations now. This could, however, be violated if "worse" sites are treated first. Thus, I restrict future reference sites (control group) to recently installed ones by removing any observations more than 3 years before and after the year the enforcement cameras are installed.

To visualize, refer to figure 4 that illustrates the timeline for a sample of four cameras (A,B,C & D). Unshaded areas denote the window 3 years before and after the cameras are installed with $T = 0$ representing pre-treatment and $T = 1$ representing post-treatment. Shaded areas denote observations outside the +3,-3 window that are not included in the analysis. In this example, CAM B and D are counterfactuals for CAM C. CAM B provides the baseline from 1998 to 1999 and CAM D from 2000 to 2001 after CAM C is installed. Conversely, CAM A is not a reference group for CAM C because the treatment dates are too far apart. This also means that only a future "recently" treated camera will enter as reference group.

While many papers focus on accident outcomes near the housing, they fail to capture how enforcement effects could change moving away. The concern is whether "kangaroo" effects could exacerbate collisions away from the housing. To precisely capture how the effects change with distance from the camera housing, the following specification is estimated:

$$E(Y_{ijt}^{k-100,k}) = exp(\alpha_i^{k-100,k} + \gamma \mathbf{T_{it}^{k-100,k}} + X_{jt}^{\prime k-100,k}\phi + \theta_t^{k-100,k} + \varepsilon_{ijt}) \tag{2}$$

where k represents the various distance bandwidths (eg. 0 to 100m, 100m to 200m... 1900m to 2000m) up to 2 kilometres left and right of the camera. In brevity, I am estimating

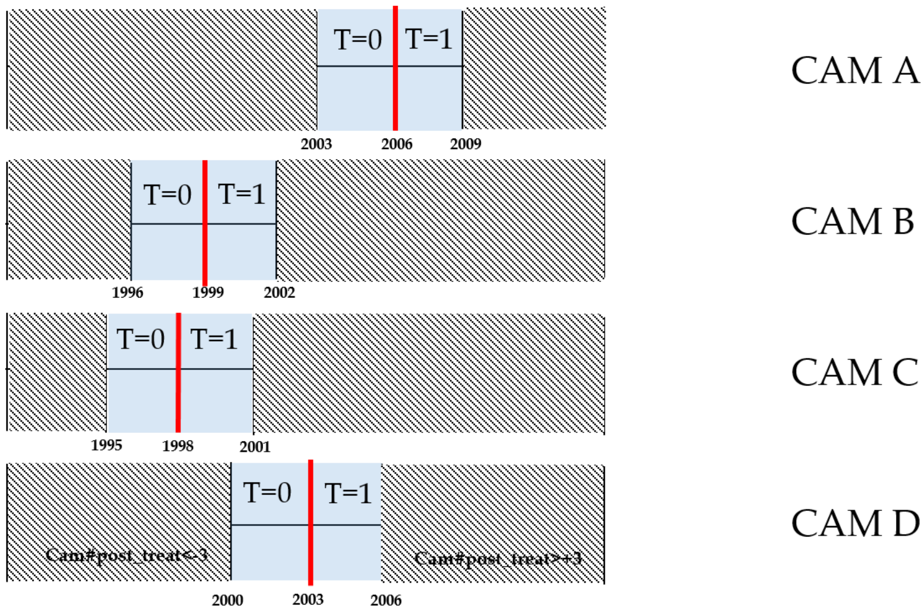

Figure 4: Illustration of Time lines for different cameras in sample. Bold lines represent the treatment year for each camera and unshaded window denotes 3 years before and after the camera is installed. Shaded areas denotes observations more than three years before or after installation and are omitted from the analysis. T=1: Treatment Period; T=0: Pre-Treatment

the enforcement effects for every 100m bandwidth to identify how enforcement effects vary moving away from the camera. I achieve this by running stratified regressions for every k and $k-100$ bandwidth for $k \in 0, 100, ...1900, 2000$ metres. If the effects are highly localised, I expect γ to be more negative as k is smaller. If there are displacement of accidents, I would expect γ_k to be positive outside camera surveillance. Otherwise, the rest of the specification is similar to equation 1.

6 Empirical Results

In this section, I estimate the effects of speed enforcement cameras on various accident outcomes. First, I provide some summary statistics for the sample of speed cameras in the analysis. Next, I present baseline estimates on the effect of speed cameras within 500 metres from the housing. I then put these estimates through various robustness and placebo tests that relax identification assumptions. Subsequently, I allow camera enforcement effects vary across different speed limits, road type, over time and distance. Finally, I compute welfare estimates associated with these devices.

6.1 Descriptive Statistics

Figure 5 and 6 shows the temporal and spatial distribution of fixed speed cameras from 1992 to 2016. In 1992, 24 cameras are installed in London in a pilot program before spreading to other larger cities. By 2000, there are more than 1,000 cameras distributed across more than half of the local authorities across Great Britain. Fixed speed cameras remain the predominant instrument to deter speeding with another 1,368 devices deployed in the next 8 years. Most of the local authorities have at least 1 speed camera installed by 2008 but it is less prominent after due to the increasing reliance on newer prototypes, such as variable and mobile speed cameras, due to larger coverage areas or flexibility in relocation. As of 2016, there are approximately 3,500 fixed speed cameras across England, Scotland and Wales. My dataset, which encompasses a total of 2,548 cameras, covers more than 70% of total number of fixed-speed cameras installed. The rest of the 30% are missing either because (1) the local camera partnerships did not respond to our data requests[17] or (2) the information provided do not allow me to accurately determine the location of cameras.

Next, I present some basic summary statistics for pre-treatment accident outcomes, camera, road and local authority characteristics in Table 1. Pre-treatment accident outcomes are computed by averaging the number of collisions within 2 kilometres from the housing five years before the camera is installed. For instance, if a camera is installed in 2000, I will take the mean of annual crashes from 1995 to 1999. There are approximately 0.41 accidents every 100 metres annually, resulting in 0.40 slight injuries, 0.08 serious injuries and 0.01 deaths. On average, the limit enforced by speed cameras is around 37mph although bulk of the cameras impose a 30mph limit (more than 70%). Most of the cameras (75%) are installed in A Roads - primary routes that are slightly smaller than motorways (or expressways). The rest are mostly installed in B (11%) and Minor Roads (14%), with less than 2% fixed along Motorways and C roads. There are not many fixed cameras on Motorways because variable speed cameras are usually deployed instead to enforce speed limit over a longer distance. Also, approximately 80% of the cameras located along busier roads in populated urban areas.

[17] This include Warwickshire, Suffolk, Norfolk, Wiltshire and Swindon.

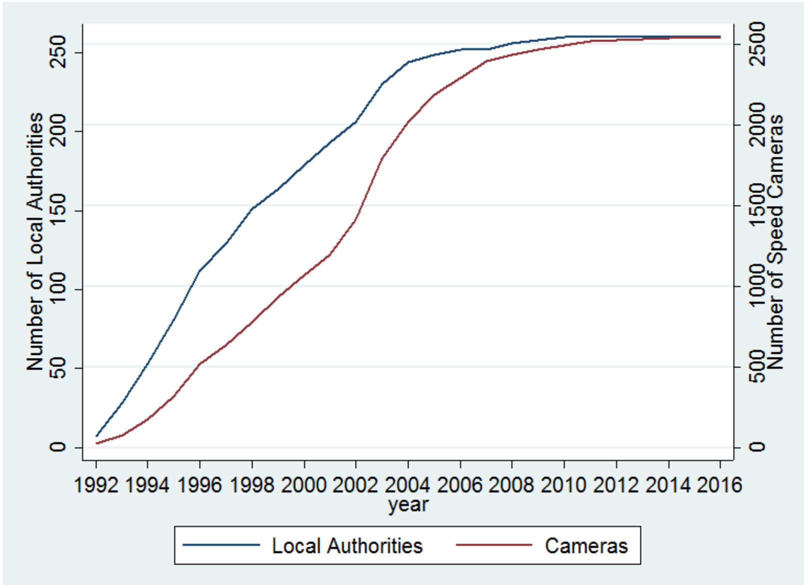

Figure 5: Number of Speed Cameras and Local Authorities with speed cameras from 1992 to 2016 across England, Scotland and Wales

As mentioned, one of the identification challenges is that earlier camera sites are different from those receiving installation later. To examine if this is the case, I split the sample into 5 groups (1992 to 1995, 1996 to 2000, 2001 to 2005, 2005 to 2010 and 2010 onwards) according to the year the cameras are installed. I do not find sites that have cameras first more dangerous than the latter ones. No evident differences are also observed in camera/road characteristics, local authority demographics and labour outcomes. If anything, there seems to be more crashes and injuries for cameras that are installed after 2006. These cameras are often found on roads with higher speed limit. One possible explanation is the change in the guidelines for selecting camera sites. As a precaution, I remove these cameras in my robustness tests but this did not materially affect the results.

6.2 Effects of Speed Cameras on Accidents

6.2.1 Baseline Estimates

Table 2 presents a set of baseline estimates from equation (1) that captures the effect of speed enforcement cameras 500 metres left and right of the camera housing on various accident outcomes, including number of Accidents, Slight Injuries, Serious Injuries and Deaths.

(a) 24 sites

(b) +1044 sites

(c) +1368 sites

(d) +109 sites

Figure 6: Development of Fixed Speed Cameras across England, Scotland and Wales from 1992 to 2016.

Due to space constraints, I only report results from Poisson regressions. Findings from Negative Binomial regressions in Table 8 in Data Appendix and are fairly similar. Only the coefficients (γ) for key estimate T_{it} are reported. To interpret these coefficients, I compute the semi-elasticity ($\%\Delta$) by taking the exponential of γ before subtracting by 1. The absolute reductions in collision outcomes (Absolute) are by computed by multiplying $\%\Delta$ with the pre-treatment mean. Only ever-treated sites are included in the specification and late treated sites act as reference sites for earlier ones. In short, I am comparing changes in collision outcomes for sites after camera is installed with sites that has camera installations in the future. The sample is smaller for Serious Injuries and Deaths. This is because there are several sites that experience no fatalities or severe injuries over the sample period and

Table 1: Summary statistics of camera sites across time

	All	1992 - 1995	1996 - 2000	2001 - 2005	2006 - 2010	2011 - 2016
Pre-treatment Accident Outcomes						
Accident Counts/100m	0.41	0.36	0.39	0.39	0.54	0.60
	(0.37)	(0.31)	(0.39)	(0.31)	(0.56)	(0.38)
Slight Injuries/100m	0.40	0.40	0.38	0.36	0.56	0.71
	(0.42)	(0.38)	(0.41)	(0.38)	(0.58)	(0.43)
Traffic Deaths/100m	0.01	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Serious Injuries/100m	0.08	0.07	0.08	0.08	0.08	0.11
	(0.07)	(0.05)	(0.08)	(0.07)	(0.06)	(0.06)
Camera/Road Characteristics						
Speed Limit	37.20	36.53	32.75	34.16	41.05	34.79
	(10.92)	(10.73)	(6.72)	(9.39)	(12.35)	(9.33)
A Road	0.75	0.76	0.69	0.79	0.77	0.73
	(0.43)	(0.43)	(0.46)	(0.41)	(0.42)	(0.44)
B Road	0.11	0.10	0.14	0.08	0.12	0.12
	(0.31)	(0.31)	(0.35)	(0.27)	(0.33)	(0.32)
C Road	0.00	0.01	0.02	0.00	0.00	0.02
	(0.06)	(0.12)	(0.15)	(0.06)	(0.00)	(0.12)
Motorway	0.01	0.01	0.01	0.01	0.04	0.01
	(0.08)	(0.11)	(0.07)	(0.08)	(0.19)	(0.09)
Minor Road	0.14	0.11	0.14	0.13	0.07	0.13
	(0.34)	(0.31)	(0.35)	(0.33)	(0.26)	(0.33)
Rural	0.18	0.21	0.12	0.09	0.14	0.15
	(0.38)	(0.41)	(0.32)	(0.28)	(0.35)	(0.36)
LA Characteristics						
Gross Annual Salary	25612.47	24170.51	23423.40	26221.43	23488.91	24245.93
	(4317.99)	(3650.61)	(4193.10)	(4143.65)	(2774.21)	(4143.14)
Hours worked	37.84	37.92	37.87	37.66	37.83	37.86
	(0.74)	(0.62)	(0.71)	(0.56)	(0.53)	(0.67)
Job Count	118697.66	112070.20	112079.69	143444.02	109272.51	116533.02
	(92509.82)	(84887.33)	(93917.90)	(120102.28)	(90747.35)	(95111.37)
Job Density	0.85	0.76	1.01	0.81	0.65	0.88
	(0.41)	(0.23)	(3.91)	(0.42)	(0.16)	(2.61)
% Pop 18 to 25	9.29	9.11	9.56	9.42	9.73	9.38
	(2.44)	(2.35)	(2.92)	(2.00)	(1.82)	(2.59)
Population Size	204907.15	221811.27	208318.24	262401.66	254666.19	219312.20
	(104695.46)	(135361.03)	(142054.29)	(177959.39)	(178190.47)	(142834.82)
Unemployment Rate (%)	6.33	6.63	6.95	7.25	8.40	6.84
	(1.94)	(1.95)	(1.81)	(1.99)	(2.89)	(1.95)
VMT	2425.91	2633.17	3271.23	2053.54	1619.41	2797.63
	(2370.39)	(2566.26)	(2878.41)	(2368.73)	(1410.15)	(2685.05)
Observations	2548	314	754	1123	301	57

Standard errors reported in parenthesis.

these sites are removed from the analysis.

Moving from left to right, additional covariates are included in the estimation. In the first column, I estimate the effects associated with the entire sample of speed cameras from 1992 to 2016. I restrict to a sub-sample of sites that I have a full set of control variables in the second column. This sample is significantly smaller. In both specifications, I include site and year fixed effects but did not add any control variables. I observe that enforcement cameras not only reduce the number of crashes, but also abate the severity of collisions. It is also comforting to observe that results are very consistent across the two columns, suggesting that the reduced sample is fairly representative.

Next, I include a vector of time-variant local authority (LA) characteristics to partial out regional specific shocks that could correlate with the camera installations and affect outcomes. This include demographic (population size and % of population between 18 to 25) and labour characteristics (gross annual salary and working hours). Controlling for these differences has an inconsequential effect on the estimates. Subsequently, I control for the annual average vehicle miles travelled (VMT) as more driving could induce more accidents. Estimates remain fairly stable. I further include a number of weather controls including temperature and wind speed. The concern is whether bad weather shocks, which could induce more accidents, are correlated with camera installations. This significantly reduce the sample by more than two-third due to missing data but again did not change the estimates much.

In column (6), I include a sample of non-camera sites[18] despite meeting the selection criterion (for more information refer to section 2). The rationale is to understand the bias from incorporating non-treated sites based on some matching-on-observables strategy frequently adopted in the previous literature. I find that estimated enforcement effects are much smaller. This could be driven by the fact the untreated sites experience a fall in collision outcomes even without camera installation because these surge in accidents are deemed to be transient and collision outcomes are expected to revert to mean levels. This explains why local authorities chose not to install cameras around these sites. The result illustrates the problem of using these sites to measure enforcement effects as it might underestimate the enforcement effects of cameras. Furthermore, as mentioned, sites that receive installations later could be different from those earlier treated sites. Thus, in column (7), I restrict the reference groups to just recently treated cameras by excluding any observations more than 3 years before and after the camera is installed. To illustrate, this is equivalent of removing the shaded areas in Figure 4 from estimation. Like before, estimates remain fairly

[18] To create a sample of non-camera sites, I first place random points along major roads (A & B roads) that are at least 2,000 metres from one another and 2,000 metres from the nearest speed camera. Following that I calculate the yearly collision, injuries and death counts within 500 metres from these random points. I only retain sites with more than 4 killed and serious injuries (KSI) and 8 personal injury collisions in a 3 year rolling window. In total, I find 694 sites that meet the selection criterion but are not treated.

similar compared to before, suggesting that sites that receive installations first are not that dissimilar from later treated sites.

Overall, I document that speed cameras not only attributed to significant reductions in the number of collisions, but also abated the severity of the crashes as well. Results are fairly steady and the addition of controls do not appear to matter much. Across the board, I observe substantial decreases for different accident outcomes that are significant at 1% level. After an enforcement camera is installed, I observe collisions fell by 17% to 39%, representing an absolute reduction of 0.89 to 2.36 per kilometre per annum. Slight injuries fell by between 1.19 and 2.87 per kilometre per annum, which amounts to a 17% to 38% decrease. There are between 0.25 and 0.58 less serious injuries surrounding the camera, equivalent to a 28% to 55% fall from pre-treatment levels. Largest reductions in relative levels are documented for deaths. On average, there are approximately 0.08 to 0.19 less fatalities per kilometre, which represents a massive 58% to 68% decline[19].

6.2.2 Robustness & Placebo Tests

Table 3 summarizes a battery of robustness tests that further relaxes identification assumptions to ensure that earlier estimates are not spurious. Like before, I estimate each test using both Poisson and Negative Binomial models and results did not differ much.

First, I limit my analysis to a sub-sample of Motorways and A-Roads. The notion is that traffic[20] is less likely to be displaced along these major roads after speed camera is introduced because there are less alternative routes available. Results in Columns (1) and (2) reveal that this did not matter much as enforcement effects are fairly similar compared to before. Next, in column (3) and (4), I remove sites that were installed from 2006 onwards because the change in the site selection criterion could induce these sites to be less comparable. Removing these later treated sites appear to reduce my estimates marginally but inconsequentially.

Subsequently, I make use of the rich information associated with each camera. This include (1) speed limits, (2) road type and (3) whether camera is installed in rural or urban areas (See Table 6 for more details). To do so, first, I match each each site i with another site j based on the following rules:

[19]This is because often there are very little reported deaths on roads, which is why the small estimate could generate significant changes.

[20]It will not be advisable to control for traffic as it is likely to be a "bad" control. The implementation of speed camera is likely to reduce traffic flow by displacing them to neighbouring unmonitored roads. Moreover, detailed road level traffic flow data is only available for a small sub-sample of roads.

Table 2: The Effects of Speed Camera on various accident outcomes within 500 metres from Camera using Poisson Regressions

	(1) All	(2) Baseline	(3) Demo	(4) VMT	(5) Weather	(6) Non-CAM	(7) -3,+3
Accidents	-0.469[a]	-0.488[a]	-0.268[a]	-0.243[a]	-0.184[a]	-0.095[a]	-0.222[a]
	(0.009)	(0.011)	(0.016)	(0.017)	(0.028)	(0.016)	(0.017)
Obs	66868	25720	25720	25720	7383	35929	9841
Absolute	-2.11	-2.36	-1.44	-1.32	-0.89	-0.82	-1.13
% Δ	-37.43	-38.65	-23.54	-21.55	-16.78	-9.09	-19.88
No.of CAM	2481	1555	1555	1555	659	2249	1481
Slight	-0.412[a]	-0.483[a]	-0.278[a]	-0.253[a]	-0.185[a]	-0.057[a]	-0.207[a]
	(0.010)	(0.013)	(0.019)	(0.019)	(0.038)	(0.019)	(0.021)
Obs	57224	21355	21355	21355	5483	31564	8175
Absolute	-2.25	-2.87	-1.82	-1.67	-1.19	-0.64	-1.33
% Δ	-33.74	-38.29	-24.30	-22.34	-16.87	-5.56	-18.70
No.of CAM	2123	1294	1294	1294	518	1988	1223
Serious	-0.788[a]	-0.747[a]	-0.454[a]	-0.414[a]	-0.326[a]	-0.326[a]	-0.373[a]
	(0.015)	(0.022)	(0.033)	(0.034)	(0.074)	(0.028)	(0.042)
Obs	63280	23650	23650	23650	6539	33823	8306
Absolute	-0.58	-0.57	-0.39	-0.37	-0.25	-0.35	-0.33
% Δ	-54.55	-52.63	-36.49	-33.93	-27.81	-27.84	-31.15
No.of CAM	2346	1428	1428	1428	572	2115	1240
Deaths	-0.956[a]	-1.071[a]	-1.029[a]	-1.018[a]	-1.124[a]	-0.761[a]	-0.858[a]
	(0.041)	(0.073)	(0.116)	(0.119)	(0.209)	(0.093)	(0.153)
Obs	42924	11394	11394	11394	2787	18765	2843
Absolute	-0.08	-0.12	-0.12	-0.12	-0.15	-0.09	-0.19
% Δ	-61.57	-65.75	-64.28	-63.85	-67.51	-53.27	-57.59
No.of CAM	1591	683	683	683	220	1155	426
CAM FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Demographics			✓	✓	✓	✓	✓
VMT				✓	✓	✓	✓
Weather					✓		

Note: Each reported coefficient is the γ from a different Poisson regression estimated using Maximum likelihood. Dependent variable is the annual Y count where Y=accident, injuries, serious injuries and deaths 500m left and right of camera housing. Absolute is the number of reductions in accident outcomes computed by multiplying the % Δ with the pre-treatment mean of Y. % Δ is the proportional change (semi-elasticity) of collision outcomes after treatment and is computed by taking $exp(\gamma) - 1$. In Column (1), I include the entire sample of cameras. In Column (2), I conduct the analysis for a sample of sites that I have full set of co-variates. In Column (3), I include a vector of controls that captures the variation in demographics and labour outcomes that include population size, % of 18 to 25, Gross Annual Pay & hours worked. In Column (4), I control for the annual average vehicle miles travelled. In Column (5), weather controls are added into the specification. In Column (6), I include a sample of non-camera sites that are eligible for camera installations into the estimation. In Column (7), I constraint the analysis to observations just 3 years before and after from the year of installation. Sandwich (robust) standard errors are reported in the parentheses. [c] p<0.10, [b] p<0.05, [a] p<0.01

1. Within 20 kilometres from one another
2. Same rural-urban classification
3. On the same road type (A,B,C,Minor or Motorways)
4. Same speed limits
5. Within 5 years from one another in installation dates
6. Within 70% - 130% in accident outcomes

The objective is to ensure that each camera is benchmarked with the most similar yet-to-be treated site. In short, I am comparing changes in collision outcomes for camera i with camera j based on these rules. I managed to pair up to 1229 sites. Instead of exploiting within camera variation, now the specification includes pair-match-camera fixed effects (or pair interacted with camera fixed effects). Alleviating these differences between sites again do not matter much as enforcement effects remain significant for all collision outcomes.

Overall, my results confirm that enforcement cameras have an immediate impact on road safety within the vicinity of the housing. However, it is unsure that how this effect could change across different road/camera characteristics, space and over time. This will be examined in the following sections.

6.2.3 Effects across Road Types, Speed Limits & Time

Next, I allow the effectiveness of speed cameras to vary across different road and camera characteristics and over time with the following specifications:

$$E(Y_{ijt}) = exp(\alpha_i + \gamma_{\mathbf{w}}(\mathbf{T_{it}} * \mathbf{H'_w}) + X'_{jt}\phi + \theta_t + \varepsilon_{ijt}), \tag{3}$$

where the $\mathbf{H'_w}$ represents a vector of binary variables that equals to unity denoting each:

1. Speed limit (30,40,50,60 & 70)
2. Road Class (Motorway, A, B & Minor)
3. Year after treatment (1,2...10 years after installation)

Table 3: Robustness Tests

	(1)	(2)	(3)	(4)	(5)	(6)
	Major Roads		No 2006-2016		Matched Pair FE	
	NB	Poisson	NB	Poisson	NB	Poisson
Accidents	-0.209[a]	-0.237[a]	-0.243[a]	-0.264[a]	-0.431[a]	-0.401[a]
	(0.015)	(0.013)	(0.019)	(0.020)	(0.029)	(0.022)
Obs	30579	30579	19936	19936	17986	17986
Absolute	-1.36	-1.52	-1.55	-1.67	-5.27	-4.97
% Δ	-18.87	-21.09	-21.61	-23.18	-35.03	-33.04
No.of CAM	1855	1855	1208	1208	1229	1229
Slight	-0.213[a]	-0.308[a]	-0.261[a]	-0.422[a]	-0.452[a]	-0.476[a]
	(0.017)	(0.014)	(0.022)	(0.025)	(0.029)	(0.027)
Obs	26967	26967	16143	16143	16688	16688
Absolute	-1.67	-2.31	-2.00	-2.99	-6.26	-6.53
% Δ	-19.19	-26.54	-22.98	-34.40	-36.35	-37.89
No.of CAM	1639	1639	982	982	1129	1129
Serious	-0.340[a]	-0.411[a]	-0.389[a]	-0.485[a]	-0.486[a]	-0.517[a]
	(0.031)	(0.028)	(0.041)	(0.050)	(0.054)	(0.031)
Obs	28978	28978	18405	18405	16852	16852
Absolute	-0.35	-0.40	-0.39	-0.46	-0.96	-1.00
% Δ	-28.85	-33.68	-32.22	-38.42	-38.46	-40.39
No.of CAM	1749	1749	1114	1114	1143	1143
Deaths	-0.762[a]	-0.789[a]	-0.822[a]	-0.879[a]	-0.757[a]	-0.468[a]
	(0.103)	(0.081)	(0.149)	(0.128)	(0.190)	(0.113)
Obs	14791	14791	7988	7988	8589	8589
Absolute	-0.10	-0.10	-0.11	-0.11	-0.20	-0.14
% Δ	-53.32	-54.56	-56.06	-58.47	-53.12	-37.38
No.of CAM	886	886	480	480	557	557
CAM FE	✓	✓	✓	✓		
Year FE	✓	✓	✓	✓	✓	✓
LA Controls	✓	✓	✓	✓	✓	✓
CAM FE*Pair-FE					✓	✓

Note: Each reported coefficient is the γ from a different regression. Columns 1,3,5 are estimated using Negative Binomial regressions, while 2,4,6 are estimated using Poisson regressions. Dependent variable is the annual Y count where Y=accident, injuries, serious injuries and deaths 500m left and right of camera housing. In columns (1) & (2), I restrict the analysis to cameras in A-Roads and Motorway to alleviate the effects of traffic displacement on collisions. In columns (3) & (4), I remove cameras installed from 2006 onwards as they could be different from the other cameras. In columns (5) & (6), I match each camera i with another camera j based on location, pre-treatment accident outcomes and various road characteristics. I exploit the variation now between two speed cameras by including speed cam interacted with pair-fixed effects. Bootstrapped and sandwich (robust) standard errors are reported for Negative Binomial and Poisson Regressions respectively. [c] $p<0.10$, [b] $p<0.05$, [a] $p<0.01$.

In short, I am allowing enforcement effects to vary across these characteristics. Specifications for speed limits and road types are summarized in Panel A and B of Table 4 respectively. Only the key estimates γ_w are reported.

From Panel A, although I find significant improvement in road safety across different speed limits, more pronounced enforcement effects are documented along roads with higher speed limits. Specifically, the number of collisions fell by 50 to 57% along 60mph roads compared to 22 to 25% along 20mph roads. Similar larger decreases are observed for serious injuries, at around 87-88%, and deaths, at around 94-95%, on 60mph roads. In contrast, serious injuries fell by 36-41% along 20mph roads and no significant reductions in deaths are reported. There are several explanations to this. First, it could be that drivers along the lower speed limit roads are already commuting slowly and insignificant reductions in speed achieve by cameras do not matter much in reducing the gravity of collisions. Second, attenuated enforcement effects for more binding speed limits suggest that drivers may be forced to hastily drop speed so as not to be fined, inducing more collisions in some instances that could reduce enforcement effects.

Panel B summarizes the results of camera enforcement effects on different road types. Motorways are inter-city major roads for long distance travelling. A-Roads are slightly less important compared to Motorways but can still be considered trunk roads that provides large scale transport links. B-Roads are slightly smaller linkage roads for traffic between A-Roads and Minor roads. Minor Roads are smallest roads intended to connect local traffic, linking an estate/village with the larger road links. I do not observe stark differences in enforcement effects across the different road types. This is except for Motorway where no significant reductions in slight injuries and deaths are reported. This is likely due to a sample issue as only 1% of the cameras are found along Motorways.

Next, I examine how the effectiveness of speed cameras vary over time. Results are summarized in Figure 10. This is to understand whether the enforcement effects diminish over time to justify the decision to switch off the cameras. Results reveal that cameras remain effective and in fact become more potent in reducing collisions and fatalities over time. Weaker effects in the beginning suggest that some drivers could be unfamiliar with the locations of camera. They could be forced to abruptly drop speed to avoid fines, inducing more crashes. Over time, drivers learn about these locations and are less prone to reckless braking, explaining stronger enforcement effects.

Table 4: Heterogeneous effects of Speed Camera on various accident outcomes across different roads and speed limits

	Accidents		Slight		Serious		Deaths	
	Panel A: Speed Limit							
	NB	Poisson	NB	Poisson	NB	Poisson	NB	Poisson
Speed Limit 20	-0.281[a]	-0.247[a]	-0.148	-0.130[b]	-0.533[b]	-0.446[b]	-0.621	-0.514
	(0.077)	(0.066)	(0.107)	(0.066)	(0.210)	(0.211)	(4.765)	(1.007)
	-24.46	-21.91	-13.74	-12.23	-41.33	-35.96	-46.28	-40.19
	-1.49	-1.34	-1.03	-0.92	-0.45	-0.39	-0.09	-0.07
Speed Limit 30	-0.294[a]	-0.227[a]	-0.352[a]	-0.241[a]	-0.432[a]	-0.356[a]	-0.876[a]	-0.891[a]
	(0.016)	(0.017)	(0.025)	(0.020)	(0.035)	(0.035)	(0.143)	(0.124)
	-25.45	-20.31	-29.64	-21.43	-35.05	-29.98	-58.37	-58.98
	-1.55	-1.24	-2.22	-1.61	-0.38	-0.32	-0.11	-0.11
Speed Limit 40	-0.454[a]	-0.371[a]	-0.515[a]	-0.358[a]	-0.748[a]	-0.657[a]	-1.225[a]	-1.277[a]
	(0.034)	(0.036)	(0.042)	(0.040)	(0.073)	(0.067)	(0.220)	(0.238)
	-36.46	-30.99	-40.27	-30.10	-52.65	-48.14	-70.63	-72.11
	-2.23	-1.89	-3.02	-2.25	-0.57	-0.52	-0.13	-0.13
Speed Limit 50	-0.312[a]	-0.213[a]	-0.410[a]	-0.217[a]	-0.658[a]	-0.538[a]	-1.146[a]	-1.188[a]
	(0.047)	(0.057)	(0.050)	(0.063)	(0.158)	(0.138)	(0.265)	(0.285)
	-26.79	-19.15	-33.62	-19.49	-48.19	-41.62	-68.22	-69.52
	-1.64	-1.17	-2.52	-1.46	-0.52	-0.45	-0.13	-0.13
Speed Limit 60	-0.851[a]	-0.697[a]	-0.931[a]	-0.598[a]	-1.682[a]	-1.645[a]	-2.871	-3.060[a]
	(0.164)	(0.162)	(0.185)	(0.203)	(0.294)	(0.275)	(4.342)	(0.755)
	-57.31	-50.21	-60.59	-45.01	-81.39	-80.70	-94.33	-95.31
	-3.50	-3.07	-4.54	-3.37	-0.88	-0.87	-0.18	-0.18
Speed Limit 70	-0.443[a]	-0.314[a]	-0.480[a]	-0.291[c]	-0.718[b]	-0.709[b]	-1.499[a]	-1.505[a]
	(0.106)	(0.115)	(0.176)	(0.163)	(0.321)	(0.286)	(0.356)	(0.307)
	-35.81	-26.94	-38.09	-25.21	-51.23	-50.79	-77.67	-77.80
	-2.19	-1.64	-2.85	-1.89	-0.55	-0.55	-0.14	-0.14
Obs	24871	24871	20522	20522	22833	22833	11004	11004
No.of CAM	1503	1503	1243	1243	1378	1378	659	659
	Panel B: Road Type							
A Road	-0.251[a]	-0.232[a]	-0.353[a]	-0.243[a]	-0.472[a]	-0.393[a]	-0.977[a]	-0.990[a]
	(0.018)	(0.017)	(0.019)	(0.019)	(0.029)	(0.035)	(0.106)	(0.121)
	-22.20	-20.70	-29.77	-21.60	-37.60	-32.49	-62.36	-62.84
	-1.36	-1.26	-2.23	-1.62	-0.41	-0.35	-0.12	-0.12
B Road	-0.367[a]	-0.325[a]	-0.506[a]	-0.345[a]	-0.633[a]	-0.514[a]	-1.359[a]	-1.289[a]
	(0.038)	(0.040)	(0.045)	(0.053)	(0.071)	(0.077)	(0.267)	(0.301)
	-30.72	-27.77	-39.70	-29.19	-46.89	-40.20	-74.31	-72.44
	-1.88	-1.70	-2.97	-2.19	-0.51	-0.43	-0.14	-0.13
Minor Road	-0.357[a]	-0.317[a]	-0.607[a]	-0.340[a]	-0.718[a]	-0.645[a]	-1.425[a]	-1.346[a]
	(0.068)	(0.055)	(0.084)	(0.094)	(0.113)	(0.096)	(0.353)	(0.344)
	-30.04	-27.20	-45.50	-28.82	-51.24	-47.52	-75.95	-73.98
	-1.83	-1.66	-3.41	-2.16	-0.55	-0.51	-0.14	-0.14
Motorway	-0.266[a]	-0.262[a]	-0.390	-0.105	-0.544[c]	-0.496[c]	-0.116	-0.135
	(0.103)	(0.077)	(0.246)	(0.141)	(0.306)	(0.283)	(4.551)	(0.470)
	-23.34	-23.05	-32.32	-9.95	-41.96	-39.13	-10.97	-12.62
	-1.43	-1.41	-2.42	-0.75	-0.45	-0.42	-0.02	-0.02
Obs	25720	25720	21355	21355	23650	23650	11394	11394
No.of CAM	1555	1555	1294	1294	1428	1428	683	683

Note: Each reported coefficient is the γ_w from a different regression from equation 3 estimated using maximum likelihood. Dependent variable is the annual Y counts where Y=accident, injuries, serious injuries and deaths 500m left and right of camera housing. I allow the effects to vary across different speed limits and road types in Panel A and B respectively. The specification adopted is similar to Column 4 of Table 2. Bootstrapped and sandwich (robust) standard errors are reported in parentheses for Negative Binomial and Poisson respectively. [c] $p<0.10$, [b] $p<0.05$, [a] $p<0.01$.

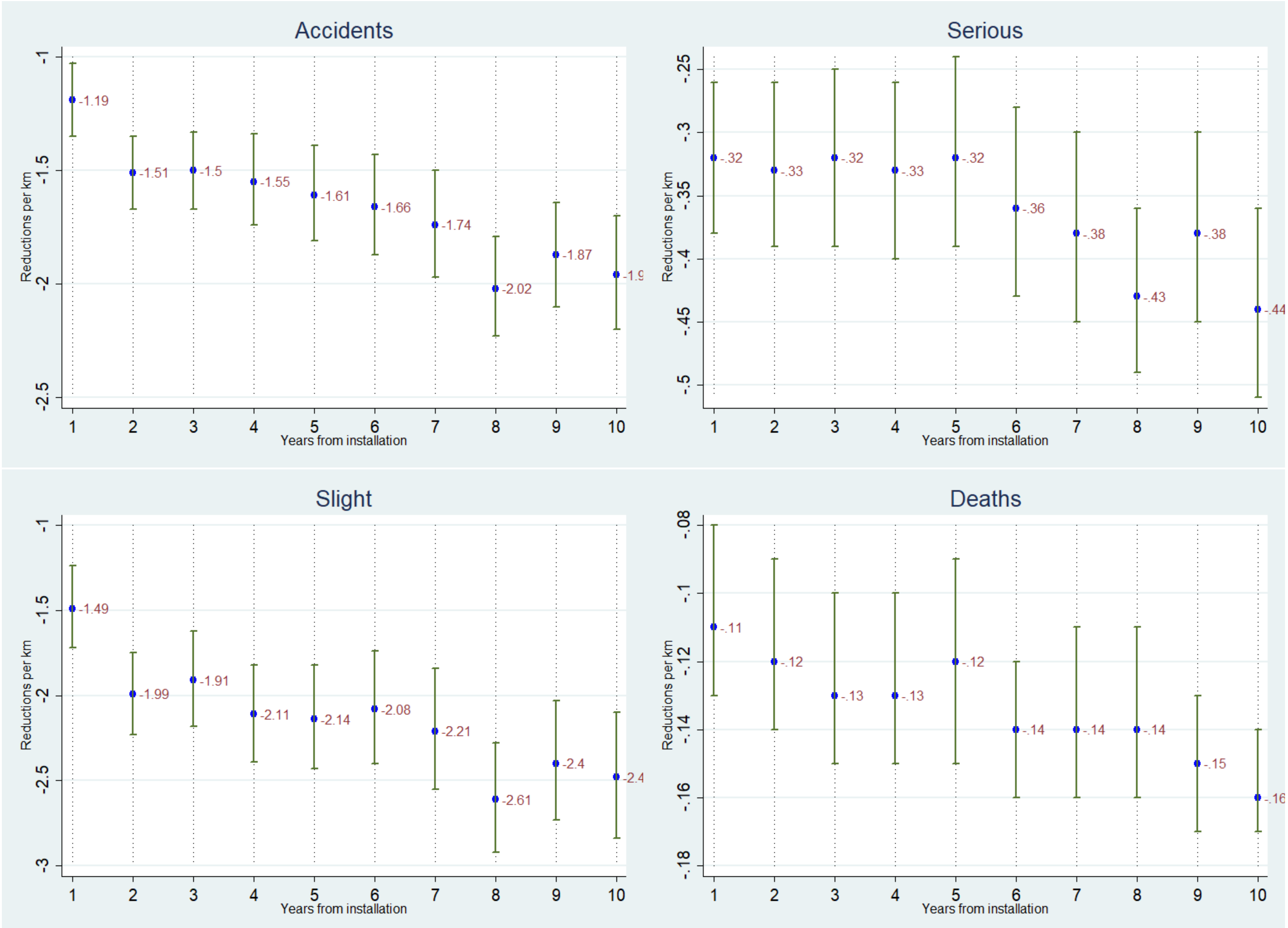

Figure 7: Effects of Speed Camera on Accidents 0 to 500m from housing across years. Effects can be interpreted as per km. Coefficients (γ_w) are obtained from the estimation of Equation 3 where H_w' represents a vector of dummies representing years from installation. Blue indicates that the coefficient is statistically significant at least at 10% level. 95% Confidence Intervals are denoted by the tails.

6.2.4 Effects over Distance

Figure 8 summarizes the estimates from equation (2) that show the effects of speed enforcement cameras moving away from the camera housing using Poisson regressions. Results from Negative Binomial models are summarized in Figure 11 in Data Appendix. Like before, results are fairly similar across these two models. Precisely, I am capturing the change in accident outcomes every 100 metres. Every dot represents the coefficients (γ_k) for key estimate $D_{k-100,k} * T_{it}$ from a different regression at k to $k-100$ bandwidth where $k = 0, 100, ...2000$. Tails denote the 95% confidence interval. If the estimate is denoted by dot, it is significant at least at 10% level. The coefficients can be interpreted as number of accident outcomes per 100 metre.

Unsurprisingly, I find localised enforcement effects around the camera and these effects dissipate quickly across distance. Reductions are largely around 0 to 500 metres around the camera and strongest effects are reported closest to the camera. This result is fairly consistent across the different accident outcomes and correspond to earlier literature (Li *et al.*, 2013). Beyond 700 metres from the device, fixed speed cameras are no longer able to enhance road safety. Moving further away, beyond 1500 metres from the camera, there are suggestive evidence of kangaroo effects as I report small rebound in the number of collisions, serious injuries and deaths. A small proportion of drivers could have speed up beyond the surveillance of cameras, inducing more collisions post implementation. However, these effects are quite small compared to the enforcement effects from cameras.

6.3 Cost-Benefit Analysis

This section reports a cost benefit analysis on speed cameras. The costs include the fixed and operating costs of camera and the time delays incurred by bypassing drivers, while the benefits include the savings from less collision, injuries and fatalities. Fines from speeding tickets are not considered as the government could redistribute these revenues to the society. While I am not able to recover all these estimates from my analysis, I rely on either previous literature or reports. The parameters are summarized in Table 5.

For the benefits, I rely on the savings per traffic accidents, injuries and deaths computed by DfT[21]. These values account for both (1) casualty-related costs (loss output, medical and

[21] For more information, refer to https://www.gov.uk/government/uploads/system/uploads/attachment_data/file/254720/rrcgb-valuation-methodology.pdf

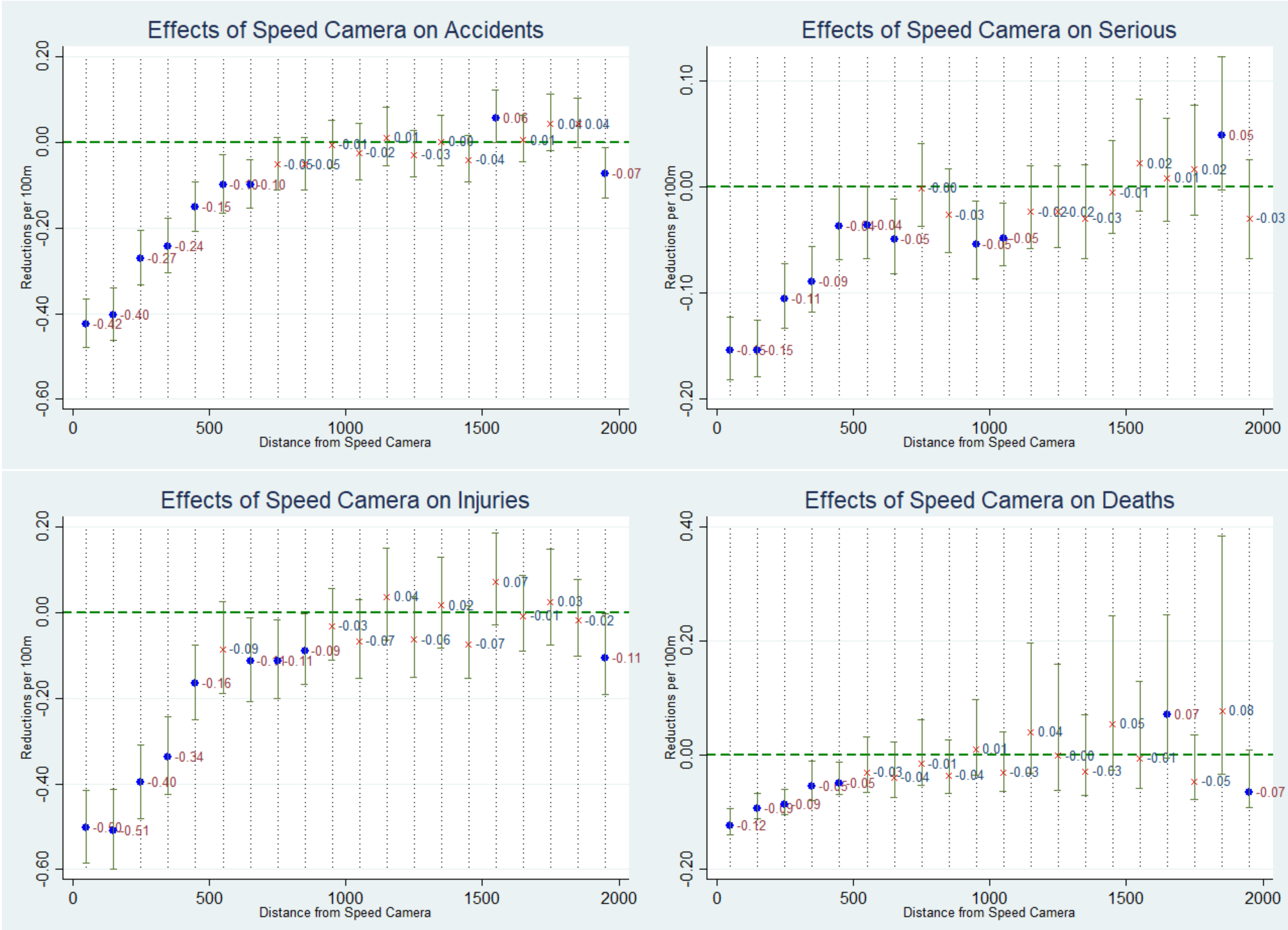

Figure 8: Effects of Speed Camera on Accident Outcomes at 100m Intervals (left and right of camera housing). Each dot represents the coefficient for every 100 metres distance bandwidth. Effects can be interpreted as per 100 metre. Coefficients (γ_k) are obtained from the estimation of Equation 2. Specification is similar to that of Column (4) in Table 3. Dot indicates that the coefficient is statistically significant at least at 10% level and cross indicates otherwise. 95% Confidence Intervals are denoted by the tails.

ambulance, human costs) and (2) accident-related costs (property damage, insurance and administrative and police costs). Total savings are computed by multiplying earlier estimates on reductions with the savings on per capita or accident basis.

For the costs, to compute the time delays from speed camera, I rely on estimates from Gains *et al.* (2005). Speed is approximately 10kmh slower after the camera is implemented. Taking the average speed limit of 58kmh[22] (30mph) and a distance of 1km around the camera, drivers incur a delay of 1.24 minutes whenever they pass a speed camera. Based on the Annual Average Daily Traffic (AADT) flow along roads with speed cameras from DfT, I assume that there are approximately 18,500 vehicles bypassing each camera every day, corresponding to around 6.75 million annually. In total, assuming an average occupancy of 1.56 per car, time delays incurred by all bypassing vehicles amount to 217,700 hours every year.

To compute loss of income from time delays, I assume that 63.3% of the commuters are between 16 to 64 and are working according to population estimates. As of 2015, employment rate in U.K is around 74.5%. After excluding holidays and weekends, there are about 261 working days annually and if individuals work around 8 hours every day, and taking median hourly wage as £14.17/hour, the annual net loss in income from delays per camera per annum amounts to £346,749. The cost of installing a fixed speed camera is approximately £59,000 and the operating and maintenance cost is around £12,441.

The estimated benefits are likely to be smaller from the actual benefits realized as I did not factor in other non-pecuniary perks. These include environmental benefits from slower travelling speed that could save more fuel, reduce emissions and improve health outcomes (van Benthem, 2015). Enforcement cameras could also enhance crime intelligence as images from these devices could help to solve other crimes (Hooke *et al.*, 1996). Even without considering these perks, net benefits generated per camera amount to around £21,119 per annum, justifying the implementation of cameras.

[22]Since most of the speed cameras impose a 30mph speed limit, time delays will be computed based on the scenario that drivers commute, on average, at a speed limit of 30mph before camera installation. Drivers are assumed to slow down along a 1km stretch around the camera housing, 500m left and right of the housing. Time delays per driver per trip is therefore approximately equal to $\frac{Distance around camera}{Original Speed - Reductions}$.

Table 5: Cost-Benefit Analysis per speed cameras across Great Britain

Parameter	Source	Value per Unit	Net Cost/Benefit/Year
		Savings from avoiding Accidents	
Damage-only	DfT(2015)	£2,142	$1.1 \times £2,142 = £2,356$
Slight Injuries	DfT(2015)	£15,450	$1.3 \times £15,450 = £20,085$
Serious Injuries	DfT(2015)	£200,422	$0.3 \times £200,422 = £60,126$
Deaths	DfT(2015)	£1,783,556	$0.2 \times £1,783,556 = £356,712$
(A)Total Benefits			**£439,279**
		Time Delays	
Speed Reductions	Gains(2005)	9.65kmh	$\frac{1km}{(58kmh-9.65kmh)} \approx 1.24mins$
Average No. of Cars	DfT(2016)	18,500 cars/day	$18,500 \times 365 = 6,752,500$
Average Occupancy/car	DfT	1.56/car	
Total Time Loss (h)			$6,752,500 \times \frac{1.24}{60} \times 1.56 =\approx 217,700hrs$
Employment Rate	ONS(2015)	74.5%	
% of Pop between 16-64	ONS(2015)	63.3%	
No. of working hours per day		8 hours	
No. of working days per year		261 days	
Median Gross Hourly Earnings	ONS(2015)	£14.17/hour	
(B)Loss of Income per cam		$\frac{8}{24} \times 74.5\% \times 63.3\% \times 217,700 \times £14.17 \times \frac{261}{365} =$ **£346,749**	
		Cost of Cameras	
Fixed Cost	Parliament(2008)	£50,000	$£50,000 \times 1.18 = £59,000$
Operating Cost	Hooke(1996)	£8,560/year	$£8,560 \times 1.45 = £12,441$
(C)Total Camera Costs per year			£71,411
(D)Total Costs			**B+C=£418,160**
Net Costs/Benefits			**A-D=+£21,119**

Note: All the dollar values are adjusted to 2015 price levels. Estimates on savings from avoiding accidents are obtained from Column (7) of Table 2. Fixed Costs include planning, signage, installation and procurement, and other fixed costs. Operating costs include operation, administrative, maintenance, publicity and liaison costs that recurs annually. These figures are obtained by averaging across a sample of cameras installed 10 study areas across UK in financial year 1995/96.

7 Conclusion

This paper utilizes micro geo-coded dataset on traffic accidents to evaluate the effectiveness of speed enforcement cameras. These devices deter reckless driving on roads particularly prone to collisions by imposing fines when drivers exceed speed limits. In contrast to earlier literature, I address the selection bias by analyzing only sites that will ever have a speed camera installed. The empirical strategy is a quasi-experimental difference-in-difference framework that relies on comparing accident outcomes before and after a speed camera is installed with other sites that will experience installation in the near future.

Assuming a linear relationship between cameras and collisions, putting another 1,000 speed cameras on roads could reduce approximately 1130 crashes, preventing around 330 serious injuries and in turn, saving 190 lives every year and generating benefits up to £21 million. These results remain robust across a range of specifications that relaxes the identification strategies. Dwelling further, however, reveal that these effects are largely localised within 0 to 500 metres from the camera and there are suggestive evidence of a rebound in collisions further away from the camera. This illustrates the possibility of drivers speeding up beyond the surveillance of cameras and inducing more accidents. Nevertheless, simple cost-benefit analysis reveals that the perks from installing a camera are marginally larger than the cost of cameras. But with technology advancement, newer prototypes, such as mobile and variable speed cameras, should be considered to circumvent the weaknesses associated with fixed speed cameras and more effectively deter speeding.

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8 Data Appendix

Table 6: List of Variables

Variable	Source	Description
Dependent Variable (Y_{ijt})		
Accident	STATS19	Number of Accidents at site i in LA j in year t
Slight Injuries	STATS19	Number of Slight Injuries at site i in LA j in year t
Serious Injuries	STATS19	Number of Serious Injuries at site i in LA j in year t
Deaths	STATS19	Number of Deaths at site i in LA j in year t
Local Authority Characteristics(X'_{jt})		
Gross Annual Salary	Annual Labour Force Survey	Average Gross annual salary at LA j
Hours worked	Annual Labour Force Survey	Average number of hours worked in LA j
Job Density	Nomis	Number of Jobs per unit area of LA j (hectare)
% of 18 to 25	Nomis Population Estimates	Percentage of population aged 18 to 25 in LA j
VMT	DfT	Annual average vehicles miles travelled in LA j
Max Temperature	MIDAS	Annual average max air temperature in LA j
Min Temperature	MIDAS	Annual average min air temperature in LA j
Wind Speed	MIDAS	Annual average wind speed in LA j
Camera/Road Characteristics		
Speed Limit	-	Binary variable denoting whether speed camera in site i has a speed limit of l where l=30,40,50,60 or 70
Road Type	-	Binary variable denoting whether speed camera in site i in road type r where r=Motorway, A, B, C or Minor
Rural	ONS Rural Urban 2011 classification	Binary variable denoting whether speed camera in site i is in rural area, otherwise it is located in urban area

Table 7: Review of Existing Literature on Speed Camera Evaluation

Authors	Dataset	Methodology	Results
Chen *et al.* (2002)	12 Photo Radar Programs (PRP) over 22km along a highway in British Columbia, Canada, 2 years pre post	EB	2.8 km/h (3%) ↓ in speed; 7% ↑ in traffic; overall 16% ↓ in collisions across entire corridor with positive spillover effects at non PRP locations. Unlike cameras, drivers are unsure of PRP deployment
Christie *et al.* (2003)	101 Mobile Speed cameras in South Wales, UK, 3 years pre, 1 year post	BA with circle and route based measures	50% (1.8) ↓ in injury crashes; effects are within 300 to 500m and no longer significant beyond; effects are stable across time and similar for 30mph and 60-70mph roads
Cunningham *et al.* (2008)	Mobile Speed cameras in Charlotte, North Carolina, US, 5 years	BA with comparable reference groups constructed based on characteristics	10% ↓ in collisions, decrease in travelling speed
Elvik (1997)	64 Speed cams in Norway,	EB	20% ↓ in injury crashes; 12% ↓ in property crashes; effects are largely driven by road sections with warrants - a certain level of crash and speed limit for the use of speed limit.
Gains *et al.* (2004, 2005)	2,300 speed cameras across 23 areas across UK, 3 years pre post	BA & EB	6% ↓ in speed; 91% ↓ in excessive speeding (>15mph); 22% ↓ in collisions; 42% ↓ in casualties
Goldenbeld & van Schagen (2005)	28 Rural Roads in Friesland, Netherlands, 5 years pre and 8 years post	BA with other rural roads as comparables	4 km/h ↓ in speed; overall 21% ↓ in collisions and casualties
Hess & Polak (2003)	43 fixed speed cams in Cambridgeshire, England, over 11 years	ARIMA, BA with comparable reference sites, long pre-treatment period to mitigate RTM	18% ↓ in collisions & 32% ↓ in injury crashes
Jones *et al.* (2008)	29 mobile cams in Norfolk, England, for 4 years	BA with 48 fixed speed cam sites as comparables	18% ↓ in collisions & 35% ↓ in fatal crashes; no evidence of migration of accidents
Li *et al.* (2013)	771 fixed speed cam sites across England, 9 years	DID-PSM, EB; reference groups by matching on observables	23-31% (0.9-1.4) ↓ in collisions; 0.12 - 0.34 ↓ in fatal crashes; effects smaller with PSM & localised within 200m ; no spillovers of accidents
Li & Graham (2016)	771 fixed speed cam sites across England, 9 years	DID-PSM, EB; reference groups by matching on observables	Cameras are more effective in reducing collisions on riskier sites, measured by higher historical collision counts.
Keall *et al.* (2001)	Visible and Hidden cameras in 4 regions in New Zealand, 1 year pre and post	BA with matching on road characteristics for comparables	0.7 km/h ↓ in speed; overall 11% ↓ in collisions, 19% ↓ casualties; hidden cameras has a more general effect across road
Mountain *et al.* (2004)	62 fixed speed cams across Great Britain, 3 years pre post	EB	35% ↓ in speeding, 26% (1.36) ↓ in collisions, 34% (0.31) ↓ in fatal crashes 500m from cam; effects ↓ moving away from cam
Mountain *et al.* (2005)	79 enforcement schemes (17 mobile, 62 fixed) across Great Britain, 3 years pre post	EB	4% ↓ for every 1mph ↓ in speed ;Larger ↓ reported for lower speed roads; Vertical deflections (speed humps) more effective in reducing speed and accidents
Newstead & Cameron (2003)	Speed cameras in Queensland, Australia, over a 5 year span (2006 to 2007)	Poisson BA with reference sites more than 6km away	21% ↓ in non-injury crashes, 31% ↓ in injury crashes, largest effects localised within 2km
Perez *et al.* (2007)	8 mobile cams in Barcelona, Spain	BA Poisson regressions with nearby reference sites	9 mph ↓ in speed; 27% ↓ in collisions and injuries, greater effects on weekends
Shin *et al.* (2009)	6 speed cameras in Scottsdale, Arizona US, over a 2 year span (2006 to 2007)	BA, EB with nearby reference sites	9 mph ↓ in speed; overall 44-55% ↓ in all collisions, 28-48% ↓ in injury crashes, but no effect on rear-end crashes; no discernable spillovers

EB - Empirical Bayes, BA - Before and after analysis, DID - Difference-in-Difference

(a) Original Gatsometer BV

(b) Digital Gatsometer

(c) Original Truvelo

(d) Truvelo D-Cam

(e) SpeedCurb

Figure 9: Different models of Fixed Speed Cameras across United Kingdom

Table 8: The Effects of Speed Camera on various accident outcomes within 500 metres from Camera using Negative Binomial Regressions

	(1) All	(2) Baseline	(3) Demo	(4) VMT	(5) Weather	(6) Non-CAM	(7) -3,+3
Accidents	-0.489[a]	-0.520[a]	-0.280[a]	-0.268[a]	-0.212[a]	-0.169[a]	-0.232[a]
	(0.009)	(0.011)	(0.018)	(0.012)	(0.032)	(0.014)	(0.020)
Obs	66868	25720	25720	25720	7383	38291	9841
Absolute	-2.18	-2.47	-1.49	-1.43	-1.01	-1.34	-1.18
% Δ	-38.68	-40.52	-24.40	-23.47	-19.07	-15.52	-20.70
No.of CAM	2481	1555	1555	1555	659	2429	1481
Slight	-0.433[a]	-0.527[a]	-0.423[a]	-0.379[a]	-0.332[a]	-0.222[a]	-0.310[a]
	(0.011)	(0.013)	(0.018)	(0.017)	(0.041)	(0.015)	(0.020)
Obs	57224	21355	21355	21355	5483	33926	8175
Absolute	-2.35	-3.07	-2.59	-2.36	-1.99	-2.19	-1.89
% Δ	-35.16	-40.97	-34.52	-31.52	-28.27	-19.91	-26.67
No.of CAM	2123	1294	1294	1294	518	2168	1223
Serious	-0.785[a]	-0.758[a]	-0.533[a]	-0.499[a]	-0.447[a]	-0.401[a]	-0.443[a]
	(0.016)	(0.022)	(0.029)	(0.032)	(0.059)	(0.026)	(0.045)
Obs	63280	23650	23650	23650	6539	36160	8306
Absolute	-0.58	-0.57	-0.45	-0.42	-0.32	-0.40	-0.38
% Δ	-54.40	-53.15	-41.30	-39.31	-36.07	-33.01	-35.81
No.of CAM	2346	1428	1428	1428	572	2290	1240
Deaths	-0.934[a]	-1.006[a]	-1.048[a]	-1.018[a]	-0.950[a]	-0.708[a]	-0.883[a]
	(0.040)	(0.065)	(0.105)	(0.098)	(0.200)	(0.061)	(0.124)
Obs	42924	11394	11394	11394	2787	20357	2843
Absolute	-0.08	-0.12	-0.12	-0.12	-0.14	-0.08	-0.19
% Δ	-60.69	-63.41	-64.95	-63.85	-61.32	-50.73	-58.63
No.of CAM	1591	683	683	683	220	1271	426
CAM FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
Demographics			✓	✓	✓	✓	✓
VMT				✓	✓	✓	✓
Weather					✓		

Note: Each reported coefficient is the γ from a different Negative Binomial regression estimated using Maximum likelihood. Dependent variable is the annual Y count where Y=accident, injuries, serious injuries and deaths 500m left and right of camera housing. Absolute is the number of reductions in accident outcomes computed by multiplying the % Δ with the pre-treatment mean of Y. % Δ is the proportional change (semi-elasticity) of collision outcomes after treatment and is computed by taking $exp(\gamma)-1$. In Column (1), I include the entire sample of cameras. In Column (2), I conduct the analysis for a sample of sites that I have full set of co-variates. In Column (3), I include a vector of controls that captures the variation in demographics and labour outcomes that include population size, % of 18 to 25, Gross Annual Pay & hours worked. In Column (4), I control for the annual average vehicle miles travelled. In Column (5), weather controls are added into the specification. In Column (6), I include a sample of non-camera sites that are eligible for camera installations into the estimation. In Column (7), I constraint the analysis to observations just 3 years before and after from the year of installation. Bootstrapped standard errors are reported in the parentheses. [c] $p<0.10$, [b] $p<0.05$, [a] $p<0.01$

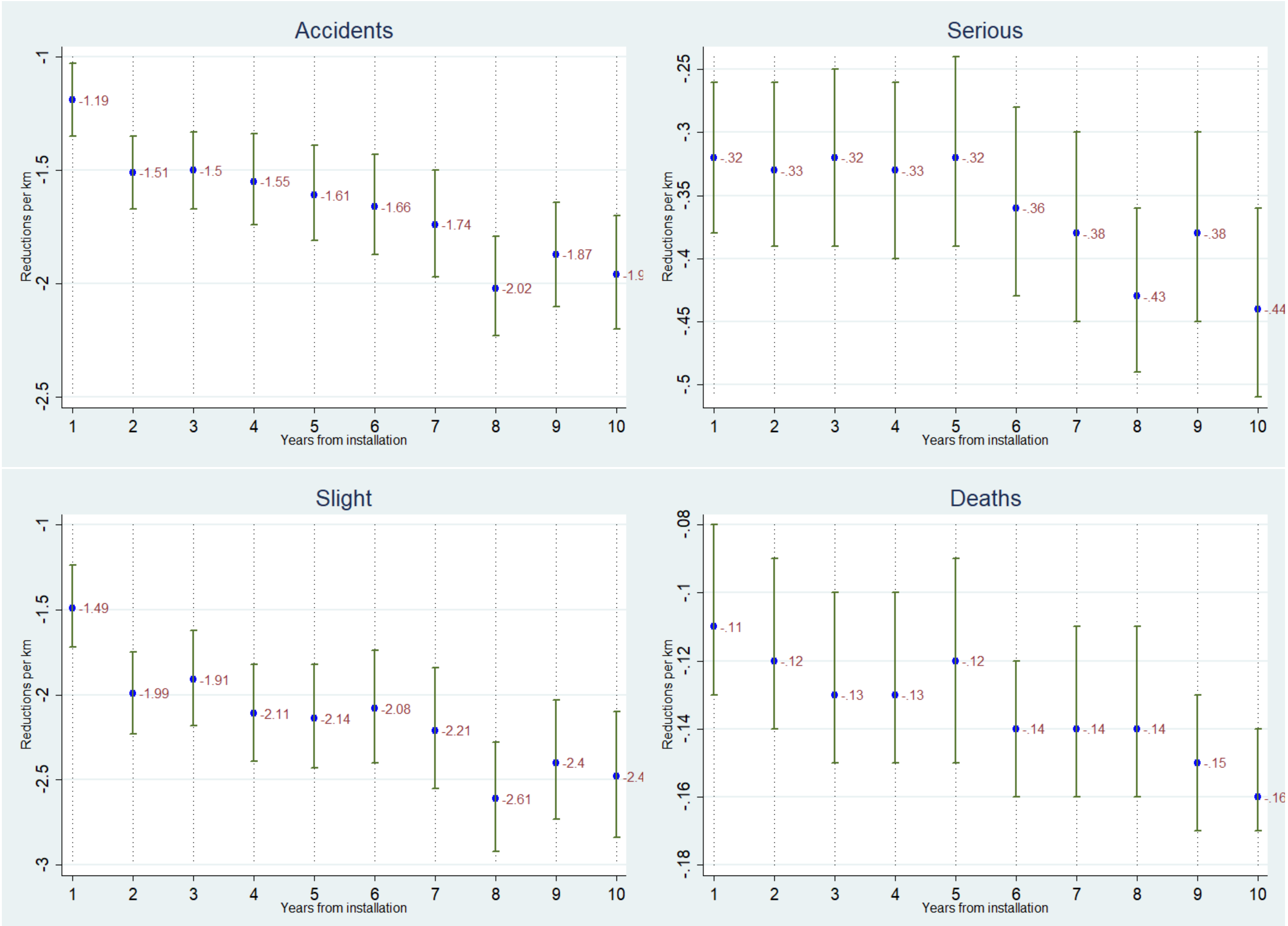

Figure 10: Effects of Speed Camera on Accidents 0 to 500m from housing across years from Negative Binomial regressions. Effects can be interpreted as per km. Coefficients (γ_w) are obtained from the estimation of Equation 3 where H'_w represents a vector of dummies representing years from installation. Blue indicates that the coefficient is statistically significant at least at 10% level. 95% Confidence Intervals are denoted by the tails.

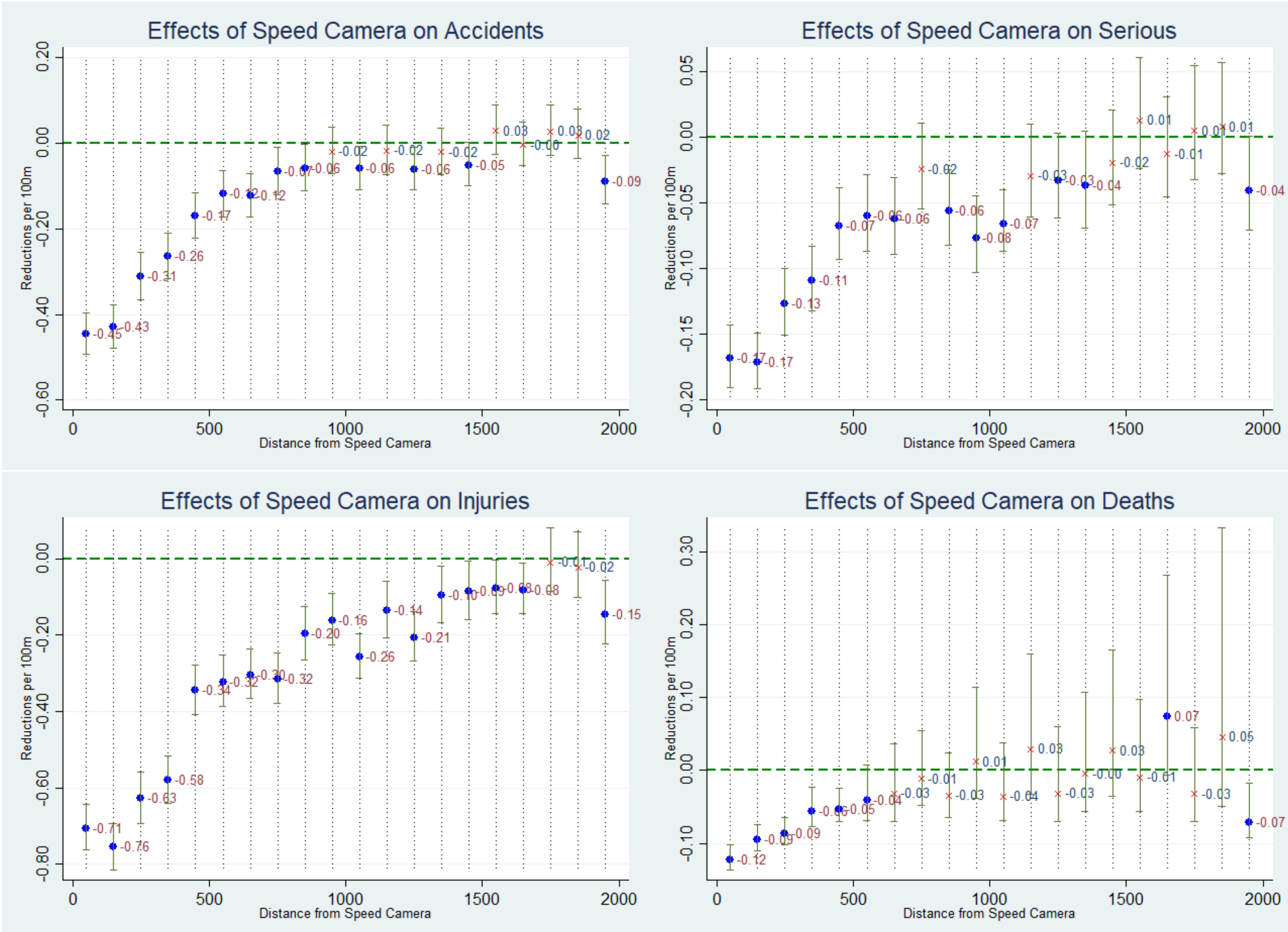

Figure 11: Effects of Speed Camera on Accident Outcomes at 100m Intervals (left and right of camera housing). Each dot represents the coefficient for every 100 metres distance bandwidth. Effects can be interpreted as per 100 metre. Coefficients (γ_k) are obtained from the estimation of Equation 2. Specification is similar to that of Column (4) in Table 3. Dot indicates that the coefficient is statistically significant at least at 10% level and cross indicates otherwise. 95% Confidence Intervals are denoted by the tails.

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