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# The Deterrent Effect of Surveillance Cameras on Crime\*

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## Abstract

From the US to Colombia to China, millions of public surveillance cameras are at the core of crime prevention strategies. Yet, we know little about the effects of surveillance cameras on criminal behavior, especially in developing economies. We study an installation program in Medellín and find that the quasi-random allocation of cameras led to a decrease in crimes and arrests. With no increase in the monitoring capacity and no chance to use camera footage in prosecution, these results suggest offenders were deterred rather than incapacitated. We test for spillovers and find no evidence of crime displacement or diffusion of benefits to surrounding locations.

*JEL codes:* C23, D04, H41, K42

*Keywords:* surveillance cameras, deterrence, incapacitation, law enforcement, crime, Colombia

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# 1 Introduction

Many countries are installing large numbers of public surveillance cameras to help law enforcement agencies in preventing crime and violence. China alone will have more than 500 million surveillance cameras installed by the end of 2020.<sup>1</sup> However, the extent to which surveillance cameras effectively reduce crime is still unclear and most of the evidence is restricted to a handful of highly developed economies. Moreover, there is a dearth of empirical evidence on the mechanisms linking the installation of surveillance cameras and criminal behavior.

In this paper, we study a relatively large installation program of public surveillance cameras in the city of Medellín, Colombia. In particular, we look at changes in crime and arrest trends at camera installation sites and assess the extent of spatial spillovers. We also build on current theories of criminal behavior, and qualitatively study the institutional setting of the city to understand whether these surveillance cameras helped law enforcement agencies in incapacitating offenders, or rather deterred them from engaging in crime.

We estimate the causal effect of the surveillance cameras on criminal behavior by exploiting the temporal and spatial variation in the installation of roughly 450 cameras in Medellín. A main concern that arises is the possible endogeneity of the allocation of surveillance cameras with respect to crime. As with other law enforcement policies, governments decide to allocate resources—in this case surveillance cameras—at crime hot spots. This decision may not be orthogonal to unobservable characteristics of the hot spots that also affect crime control outcomes. We address this issue by leveraging on the dynamic nature of the treatment, as these cameras were installed at different times in different places.

More specifically, our analysis relies on the fact that pre-selected hot spots were not ranked nor prioritized beforehand and the installation procedure was based upon bureaucratic and logistical considerations. A total of 587 places were simultaneously selected for camera installation, but the cameras were not installed at once. Instead, most of them were

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<sup>1</sup>See media coverage by the BBC.

progressively put operational over a period of two years between May 2013 and April 2015. By the end of this period, 448 out of 587 new cameras were installed and the remaining 139 were still projected for installation with no exact dates.<sup>2</sup>

We use administrative records in our analysis. The National Police of Colombia provided the data on reported crimes and arrests. Each case specifies exact coordinates, date and type of reported crime or arrest as being related to either property or violent crime. The Department of Security of Medellin provided the data on location coordinates and times of installation of all public surveillance cameras.

Since we have data on the exact dates and installation sites for the cameras and geo-located data on crimes and arrests, we build a panel data for which the time units are months, and the cross sectional units are grids of 70x70 meters. To enhance comparability, we restrict the analysis to all grids located within 500 meters of any of the 587 pre-selected locations.<sup>3</sup> Our final panel data consists of 22,571 grids covering a period of 41 months. We estimate the effect of public surveillance cameras on crime and arrests by following a difference-in-differences approach over multiple time periods. With the panel data, for each month, each grid falls into one of three categories: treated with direct surveillance (there is a camera installed within a distance of 120 meters from the geometrical center of the grid), exposed to spatial spillovers (there is a camera installed within a distance of 120 to 300 meters from the geometrical center of the grid), and controls (there is no camera installed nearby the geometrical center of the grid, up to a distance of 300 meters). The variation in the timing of installation as well as the high frequency occurrence of crimes and arrests at these grids allow us to control for invariant characteristics at individual locations. For instance, we control for the ability of police patrols at the location or environmental factors

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<sup>2</sup>Before this process started, 383 cameras were already operational in the city. All of them were installed prior to 2012 but exact dates of installation are not available. The 139 pending cameras were progressively installed during 2018 along with additional places in a process that involved the prioritization of crime hot spots. Therefore, we restrict our analysis to the 587 places originally selected.

<sup>3</sup>We are measuring the distance from the geometrical center of each grid to the actual coordinates where the camera was (or were to be) installed.

that favor or un-favor crime occurrence.<sup>4</sup> We also allow for different time shocks, at each period, at the level of *comunas*, the first order administrative division of the city of Medellin.

We highlight five main results. First, the baseline results suggests that there is a reduction in crime reports at grids under direct surveillance. These effects are statistically significant at conventional levels. In our preferred specification, we find that the number of reported crimes at grids under direct surveillance drops by 0.011 per month, on average. This is equivalent to a 19 percent decrease in reported crimes relative to the sample mean. If we break down the outcome by property or violent crime, we find statistically significant reductions in both. The effects are equivalent to decreases of 17 percent for property crime and 26 percent for violent crime, relative to the sample means.

Second, our results suggests that crime reports do not change much at grids nearby cameras. More specifically, in our preferred specification, we cannot reject the null hypothesis of no spillover effects over a range of 120 to 300 meters. We note, however, that spillover effects suffer from a mechanic problem of statistical power (e.g., Blattman et al., 2018).<sup>5</sup> Hence the results may suggest weak evidence of crime spillovers, but we are cautious on reaching this conclusion.

Third, we do not find strong evidence of direct effects on arrests. In our preferred specification, we cannot reject the null hypothesis that the number of arrests did not change at grids under direct surveillance. In our qualitative work we found that the number of camera operators remained constant at 12 people over the installation period. This implies that the number of cameras per operator changed from 32 to 69 with the installation process.<sup>6</sup> Additionally, the time window of two years leaves little to no chance to use camera footage

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<sup>4</sup>Police directives in Colombia dictate that surveillance police patrols are expected to remain for at least two years at a specific location. Even though the timing is probably not an exact match to the period of installation of the surveillance cameras, the cycle length points at some stability in the expected ability of police patrols at one place.

<sup>5</sup>For example, if there is a grid with an average of two crimes per month, and those crimes displace to nearby locations, the eight grids around would receive an average of 0,25 crimes per month.

<sup>6</sup>See for instance Piza et al. (2015) on the importance of monitoring surveillance cameras. They randomly assigned additional camera operators to CCTV schemes in Newark, NJ. Their results suggest that CCTV schemes assigned to treatment had significant reductions in violent crime and disorder.

in prosecution. Taken together, these results suggest the decrease in crime reports at grids under direct surveillance is mainly due to a deterrence effect, and not because offenders were incapacitated.

Fourth, given the dynamic nature of the installation process, we always include pre-treatment dummies in the regression to test for pre-trends in crime levels. We do this for both the direct and the spillover effects. The results suggest there is no difference in crime levels across treatment groups before the installation of the cameras. This is empirical support for the common trends assumption with a difference-in-differences estimation.

Finally, we also run a version of the differences-in-differences where we include post-treatment dummies to examine if these effects vary over time. We find that the direct effects become more negative over time. For the spillover effects the coefficients are positive but highly imprecise over all time periods. Moreover, we do not observe any relevant trend.

This paper contributes to the literature in several ways. First, we provide additional evidence on the effects of public surveillance cameras on crime. The current empirical evidence on this relation is inconclusive. Piza et al. (2019) present a systematic review of previous studies that we update in this brief summary.<sup>7</sup> Some evaluations find significant reductions in crime rates ranging from 18 to 57 percent (Armitage et al., 1999; Ditton and Short, 1999; Blixt, 2003; Brown, 1995; Caplan et al., 2011; Gill and Spriggs, 2005; Griffiths, 2003; Munyo and Rossi, 2019; Priks, 2015; Ratcliffe et al., 2009; Skinns, 1998). Others find significant increases of as high as 24 percent (Brown, 1995; Farrington et al., 2007; Gill and Spriggs, 2005; Winge and Knutsson, 2003).<sup>8</sup>

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<sup>7</sup>See also Welsh and Farrington (2009) for an earlier review.

<sup>8</sup>Note Gill and Spriggs (2005) find different results for different locations. There are three studies worth mentioning in more detail. Priks (2015) and Munyo and Rossi (2019) use relatively large samples, which may render more precise results. Priks (2015) studies an installation program in the Stockholm subway system and finds that crimes dropped 25 percent at stations in the city center. However, most property crime displaced outside the stations with increases as large as 300 percent. Munyo and Rossi (2019) study a large installation program in the city of Montevideo and find a decrease in crime of about 20 percent in coverage areas, with positive spillovers to neighboring locations. To identify causal effects, both studies exploit the variation in time and space in camera installation using difference-in-differences methods. We follow a similar approach in this paper. Piza et al. (2012), on the other hand, focus on the mechanisms. The authors compare case processing times and enforcement rates between crimes detected by camera footage and 911 calls for service. Results show a significant reduction in processing times by surveillance cameras,

Second, this is the first study focusing on a violent Latin American city. This region accounts for more than one-third of the world's homicides with only 8% of the population.<sup>9</sup> Medellín, Colombia, in particular, has a long history of violence and crime. The city's homicide rate per 100,000 people reached almost 400 in the early 1990s. In 2012, just before the cameras were installed, the homicide rate was 52. The installation of these surveillance cameras was part of a broader technology investment that included GPS devices to track police patrols and a network of strategic centers to facilitate, among other, hot spots hot spots policing strategies.<sup>10</sup> The dearth of studies from Latin American violent cities is probably related with the same institutional characteristics driving high crime rates. Hence Medellín presents an unusual opportunity to study the effects of public surveillance cameras in a high crime environment.

Finally and more broadly, this study is related to the literature on the deterrent effects of law enforcement policies (Becker, 1968; Ehrlich, 1973; Hansen, 2015).<sup>11</sup> For instance, studies on the deterrent effect of increases in police manpower (Greene, 2013; Levitt, 1998), or the deterrent effects of prison (Drago et al., 2009; Helland and Tabarrok, 2007; Kessler and Levitt, 1999; Zimring et al., 2001).<sup>12</sup>

This paper is organized in five sections including this introduction. In Section 2 we describe the setting, the broader strategy of technology investments, and the camera installation program. In Section 3 we present the data and the empirical strategy. In Section 4 we present the results. Finally, in Section 5 we examine whether this intervention was cost-effective or welfare enhancing, and compare it with alternative crime prevention strategies.

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which provides evidence of an effect through an increase in the operational capacity of law enforcement agencies.

<sup>9</sup>See for instance Consejo Ciudadano para la Seguridad Pública y Justicia Penal and United Nations Office on Drugs and Crime Global Study on Homicide 2013.

<sup>10</sup>See for instance Collazos et al. (2020), who report on the results of an experimental intervention of hot spots policing in Medellín. This intervention started after the camera installation period.

<sup>11</sup>See also Chalfin and McCrary (2017) for a review of the literature.

<sup>12</sup>Note Raphael (2006) and Webster et al. (2006) directly question the results from Kessler and Levitt (1999), with further reactions by Levitt (2006).

## 2 Setting

### 2.1 The City of Medellín

Medellin is Colombia's second largest city with a population close to 3 million. Antioquia, the region where the city is located, represents roughly 15 percent of the country's gross domestic product.<sup>13</sup> The city is relatively well funded and has roughly 350 police per 100,000 people, similar to many US cities of similar size.<sup>14</sup>

Back since the late 1980s the city was known for the Medellín cartel, one of the most notorious and violent criminal organizations in the world. Pablo Escobar, the leader of the cartel, led a war with the local and national governments leading to unprecedented levels of violence. We depict the evolution of the homicide rate per 100,000 people in Figure 1. Note that violence levels reached a peak in 1991, with a homicide rate of almost 400 (just before Escobar's passing in 1993). Since then, it has decreased to about 23-25 homicides per 100,000 in the past few years.

Crime in Medellín is highly organized. Blattman et al. (2020b) document that there are about 400 street gangs in the city, coordinated by roughly 15 mafia-like organizations that coexist in a cartel. These are all profit-seeking organizations, whose main sources of criminal rents are local taxation (extortion) and illegal drug sales. These are highly strategic actors that regulate crime across the city, banning homicides in some areas or taxing local thieves. In return for their forms of taxation, many of these gangs also govern local civilians, providing policing and security, dispute resolution, and other regulatory services in roughly two-thirds of the city's neighborhoods (Blattman et al., 2020a).

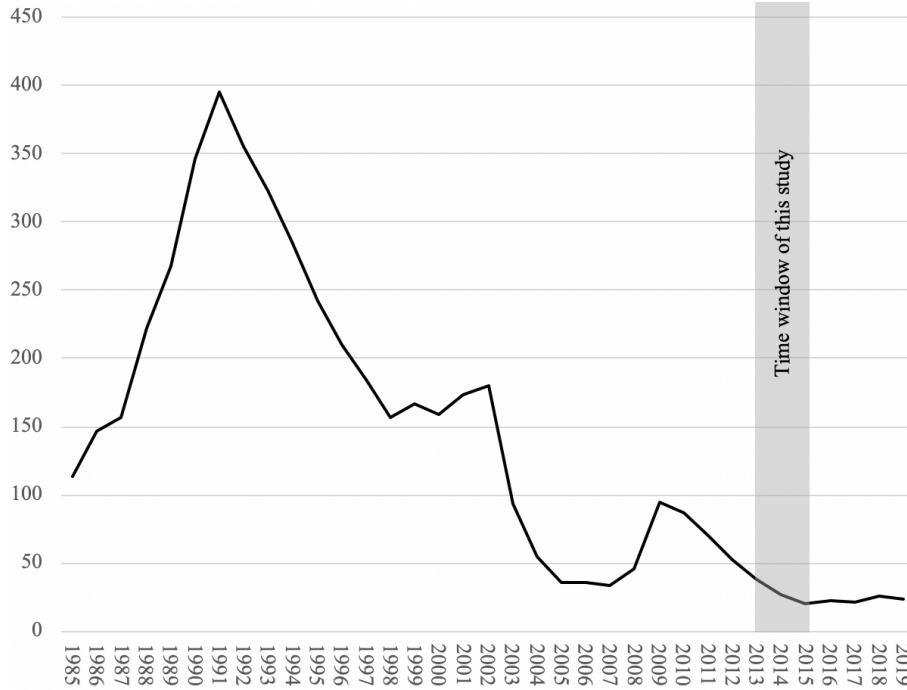
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<sup>13</sup>Data on population and gross domestic product is from the National Department of Statistics.

<sup>14</sup>See Department of Justice Statistics for further details on police personnel in the US.



Figure 1: Evolution of the homicide rate in Medellin



*Notes:* The figure depicts the evolution of the homicide rate in Medellin. Data on the number of homicides is from the Department of Security of Medellin.

## 2.2 The Integrated System for Security and Emergencies

Public surveillance cameras in Medellin are part of the Integrated System for Security and Emergencies, known as SIES-M for its Spanish acronym. This system was designed and funded at the beginning of the 2010s to improve security in the city. The system comprises five sub-systems. First, the 123 calls for service line which works similar to the 911 line in the US. Second, a scheme of GPS devices for police patrols that trigger red flags when abnormal situations are observed. For instance, when patrols move at high speed or leave their area of jurisdiction. Third, a network of strategic police centers located at each police station in the city. Fourth, a network of community alarm buttons. And finally, the sub-system of interconnected public surveillance cameras.

As of mid 2015, the sub-system of public surveillance cameras had 831 cameras connected and 139 were pending installation. Of the 831 cameras, 383 were installed prior to 2012 with no data available on dates of installation. By 2013, a group of 587 locations was selected for

new camera installations. Of those, 448 were installed between May 2013 and April 2015 with the remaining 139 uninstalled until 2018.<sup>15</sup> The technical characteristics of the cameras allow operators to observe criminal activities in detail whenever the camera is pointing in the direction of such situation. The cameras have high definition with optical zoom of up to 22x. This implies the camera can magnify the scene up to 22 times larger. In practice, this means that camera operators can see with good quality up to 120 meters around the camera installation site. Public surveillance cameras can turn 360 degrees horizontally and 270 degrees vertically, they all have night vision and are connected to the network by optical fiber and radio frequency.

Throughout the installation period, the sub-system of public surveillance cameras was operated by 36 people divided in three eight-hour shifts of 12 people each. Four of them were civilians hired by the Department of Security and eight were police personnel. The number of camera operators was not changed when new cameras were installed. This implies that by the beginning of 2013 there were about 32 cameras per operator while after the installation of the new 448 cameras took place there were about 69. Put it differently, the probability that a camera operator was using the right camera and pointing in the right direction when a crime occurred was low and, moreover, it decreased largely as the new group of cameras was installed. The sub-system has the capacity to store five years of data on camera footage, which is made available for criminal investigations under specific requests.

### **2.3 The Installation of New Public Surveillance Cameras**

The new group of 587 sites for new cameras was selected in early 2013. The selection and installation process took place in three stages. First, the office of the Information System for Citizen Security of the Department of Security of Medellin identified 587 candidate locations for camera installations. They used quantitative data on the amount and location of histor-

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<sup>15</sup>These cameras were installed in 2018 along with a new large group of cameras. The installation process involved especial targeting for crime hot spots, hence we restrict our analysis to the selected places at 2013 and the data up until 2015.

ical reported crimes, the location of the 383 previously installed cameras, and the number of additional cameras available, then they used a geographic information system software to identify suggested locations for new installations.<sup>16</sup> Importantly, these places were not ranked nor prioritized by the software but merely suggested as an un-ordered list.

Second, field teams from the National Police and the local liaisons of the Department of Security made on-site validation visits in order to determine the exact locations for new installations. These teams validated whether the suggested locations were actually crime hot spots and not places with high crime reports due of bias on the location of some crimes. For instance, there are hospitals with a large number of reported homicides because injured people dies there, or places nearby subway stations where people report thefts when indeed they may have been robbed inside railcars before stepping down. Also, these teams verified whether there were trees, power cables or other operational problems regarding the installation, and selected the exact places of installation for the cameras taking these limitations into account.

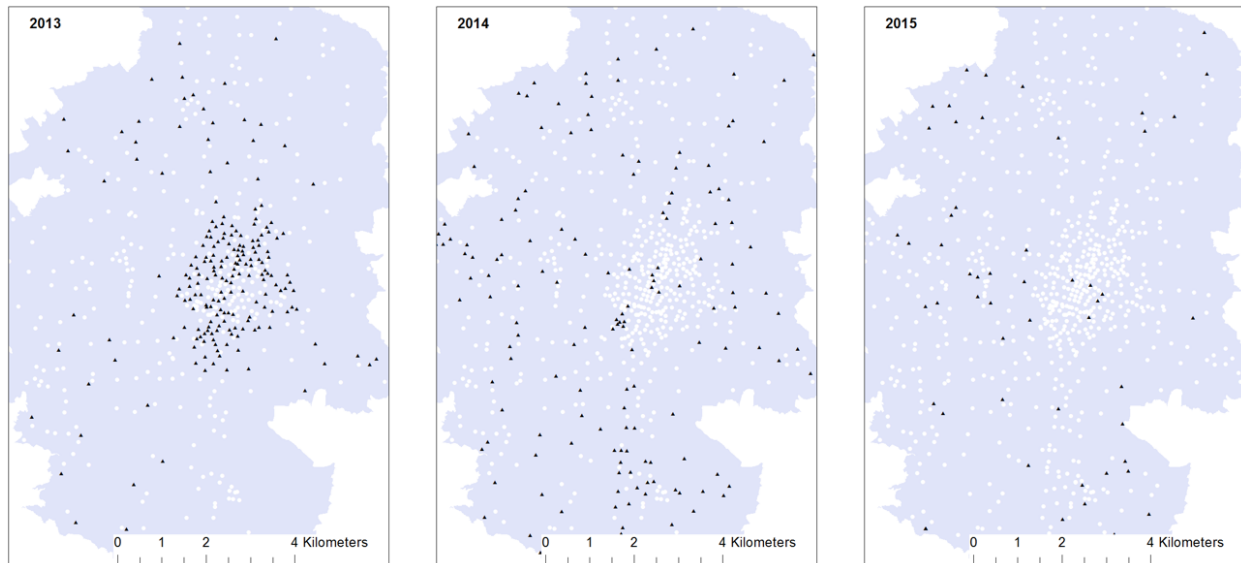
Finally, there was the administrative process for the installation and set up of each public surveillance camera. This stage was planned at the *comuna* level, and consisted in five main activities:

1. Requesting of an installation permit from the Planning Office of the city government, which must issue a favorable technical concept regarding the provision of most public goods
2. Requesting of an installation permit from the Transit Authority of the city government. This office must review any public goods that use or interfere with traffic infrastructure such as traffic lights and stop or crossing lines
3. Requesting of an installation permit from the environmental authority whenever the

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<sup>16</sup>More specifically, they used a component of Q-GIS, a free and open source geographic information system software. This component was originally designed for the selection of new points of sale for commercial businesses, using as inputs the demand, previously operational points of sale and budget availability for new ones. Thus, they mirrored its operation to select locations for new camera installations.

Figure 2: Evolution of the installation process



*Notes:* The figure depicts the evolution of the installation process. Each map shows the cameras installed each year.

process involved cutting off trees or any other environmental concern

4. Requesting of power and internet connections from local companies
5. After all other activities are finished, requesting of physical installation and final connectivity tests from local contractors.

Figure 2 shows the evolution of the installation process. During the first year, most of the cameras installed were located at the city center (*comuna* La Candelaria). Starting in 2014, more cameras were installed in other *comunas* of the city. We address the issues related to the *comuna*-level planning of the installation in Section 3.3.

## 3 Data and Empirical Strategy

### 3.1 Data

To study the effects of public surveillance cameras on crime we use two different data sources. First, we use data on reported crimes and arrests from the National Police of Colombia. We

code the crimes to create three outcome variables: property crimes, violent crimes and total crimes. Property crimes include all forms of robbery and theft, and violent crimes include homicides and assaults.<sup>17</sup> Total crimes are the sum of property and violent crimes. Our crime data includes all reports from January 2012 through July 2015, and the arrest data includes all arrests until December 2014.<sup>18</sup> We use the location and dates of all reported crimes and arrests to build a panel data for which the time units are months and the cross sectional units are grids of 70x70 meters.<sup>19</sup>

Since January 2014, the National Police records the exact coordinates. Until 2013, however, these reports include a physical address and we use an address geo-locator to obtain their exact coordinates.<sup>20</sup> Due to imprecise address data, about 75 percent of all reported crimes matched a location. This can be problematic if measurement error is correlated with treatment. We examine this by looking at the correlation between the number of crime reports per *comuna* per month in the geocoded database with the actual number of crime reports per *comuna* per month. This allows us to observe potential spatial and time patterns in the rate of successfully geo-coded crimes. Figure 3 shows the scatter-plot with a regression line. The correlation is close to unity. Lacking a formal test, we interpret this result as empirical evidence suggesting that measurement error is not systematically correlated with camera installations.

Second, we obtained detailed data on a group of 587 locations selected for camera in-

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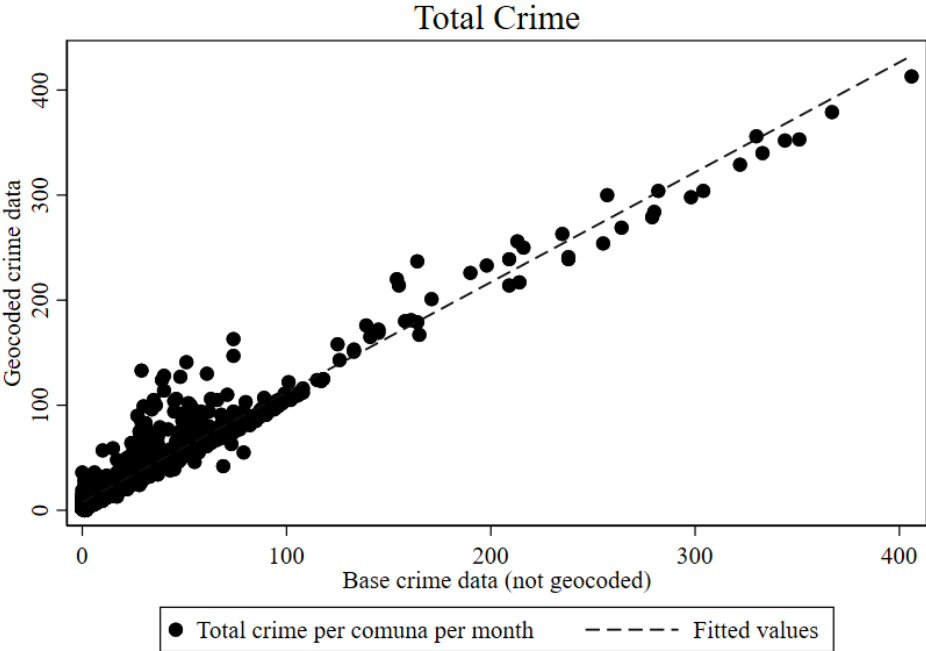
<sup>17</sup>The National Police of Colombia does not separate out theft cases according to the use of violence, as US authorities do. Hence, we count all thefts as property crimes, including all of which were mediated by violence.

<sup>18</sup>We do not have information on whether crimes occurred in public view or rather at indoor locations. However, according to aggregate statistics reported by the Metropolitan Police of Medellín, roughly 91% of motorbike thefts, 88% of car thefts, 75% of personal thefts, 71% of assaults and 74% of homicides during 2015 occurred in the public view. Hence, we believe that most of these crimes are subject to be observed by the cameras.

<sup>19</sup>Deciding the size of these grids is a trade-off between getting better spatial resolution and increasing noise across grids. We chose the 70 meters length based on the size of most blocks in the city. Most blocks are of roughly 90 meters, we chose a smaller size so that there are fewer grids with more than one intersection within them, and not many without any intersection. Most of the crimes are reported on intersections, so this helps in reducing the number of grids with no observations. Reducing the size not only increases the noise but also drastically raises the number of grids increasing the computational burden of our estimations.

<sup>20</sup>Specifically, we use a geo-locator add-on for ArcGIS which uses the address to obtain specific coordinates.

Figure 3: Scatter-plot of geo-coded crime reports and total crime reports, per *comuna* per month total



Notes: Geo-coded crime reports aggregated per *comuna* per month are in y-axis, and total crime reports per *comuna* per month are in the x-axis.

stallation in early 2013. These data was provided by the office of the Information System for Citizen Security of the Department of Security of Medellín. Out of those 587 sites, 448 cameras were installed between May 2013 and April 2015 and the remaining 139 were pending installation. In each case, we know the exact date of installation (if installed) and the geo-location of the camera. These cameras came in addition to 383 previously installed cameras that were operational before 2012. For those 383 previously installed cameras we do not have dates of installation and hence they do not constitute the main focus of our analyses. Nonetheless, we account for their potential interaction with the new cameras in our empirical application. For instance, they could constrain the areas that are available for potential spatial spillovers.

We match the data on the operation of the cameras with the 70x70 meters grids. To enhance comparability, we restrict all the analysis to grids located within 500 meters of any of the 587 pre-selected locations. We measure these distances from the geometrical center of each grid to the actual coordinates of the cameras. Our final panel data consists of 22,571 grids covering a period of 41 months. We classify the grids in one of three categories that vary by month:

- Grids treated with direct surveillance: Grids for which there is a camera installed within a distance of 120 meters, that is active in the current month. We chose this cutoff as this is the range of observation of the cameras, as reported by the Information System for Citizen Security of the Department of Security of Medellín
- Grids exposed to spatial spillovers: Grids for which there is a camera installed within a distance of 120 to 300 meters, that is active in the current month
- Control grids: Grids for which there is no camera installed nearby the geometrical center of the grid, up to a distance of 300 meters

Table 1 present summary statistics for monthly reports of crime and arrest at the level of the individual grids that we use in the estimation. This includes data from 2012 to 2015. On

average, there were 0.039 reported crimes per month per grid, with a maximum of 20 and a minimum of 0. Most of the reported crimes at these locations were property crimes. Also, there were about 0.009 arrests at each camera installation site per month, with a maximum of 13 and a minimum of 0. Note we have fewer observations for the arrest data, as we have information only until 2014.

Table 1: Summary Statistics

Variable	Obs (1)	Mean (2)	Std. Dev. (3)	Min (4)	Max (5)
Total crime	947,982	0.039	0.246	0	20
Property crime	947,982	0.031	0.214	0	17
Violent crime	947,982	0.008	0.105	0	20
Total arrests	789,985	0.009	0.124	0	13

Notes: The sample includes crime data from January 2012 through July 2015, and arrest data from January 2012 through 2014. Data is for 22,571 locations that are at some point in time within a 500 meter distance of an active camera. Reported crime levels and arrests are from the National Police of Colombia. Violent crime includes reported homicides and assaults. Property crime includes all kinds of theft.

### 3.2 Empirical Strategy

We compare reported crimes and arrests at camera installation sites by following a difference-in-differences approach with a dynamic treatment. In particular, we use ordinary least squares to estimate equation (1) below:

$$y_{ict} = \sum_{-5 \leq k \leq Post} \alpha_k D_{ict}^k + \sum_{-5 \leq k \leq Post} \beta_k S_{ict}^k + \delta_i + \gamma_t \times \psi_c + \varepsilon_{ict} \quad (1)$$

where  $i$  indexes grids of 70x70 meters,  $c$  indexes the *comuna* and  $t$  indexes time periods.  $y_{ict}$  is a measure of reported crimes or arrests at grid  $i$  from *comuna*  $c$  and time period  $t$ .  $D_{ict}^k$  is an indicator for whether grid  $i$  at *comuna*  $c$  was under direct surveillance of a camera at



time  $t$ , and  $k$  indexes a set of time indicator variables beginning five periods prior to the installation up to the post installation period. The parameters  $\alpha_k$  measure the impact of surveillance before and after the installation. We include a constant and set the coefficient  $\alpha_0$  equal to zero, hence all estimated coefficients are relative to the period immediately before the installation. Our parameter of interest to examine the direct effect of the cameras is  $\alpha_{Post}$ , which measures the average direct effect in the post-installation period.

Moreover,  $S_{ict}^k$  is an indicator for whether grid  $i$  at *comuna*  $c$  was exposed to spillovers at time  $t$ . The parameters  $\beta_k$  measure the impact of exposure to spillovers before and after the installation. We also include a constant and set the coefficient  $\beta_0$  equal to zero, hence all estimated coefficients are relative to the period immediately before the installation. In this case, our parameter of interest to examine the spillover effect of the cameras is  $\beta_{Post}$ , which measures the average spillover effect in the post-installation period.

Note the coefficients  $\alpha_k$  and  $\beta_k$  for  $k = -5, \dots, -1$  test whether the direct or spillover effects are correlated with crime levels before the installation. The statistical significance of these coefficients is evidence of dynamic selection of camera installations. Non-significance, on the other hand, would suggest that grid-level dynamic selection of camera installation is unlikely (a formal test of pre-trends).

Finally,  $\delta_i$  stand for a grid's time-invariant characteristics, and  $\gamma_t \times \psi_c$  are time fixed effects at the *comuna* level. These allow us to control, for instance, for time based public policies or crime shocks that take place at the *comuna* level, each month.

### 3.3 Challenges to identification

There are two main empirical challenges in our setting. Both are highlighted by Blattman et al. (2018) and Collazos et al. (2020). The first is related to identification. The possibility of crime displacement may induce bias in our estimation. If crime displaces from a grid under surveillance to neighboring grids, and those neighboring grids are controls, we would over-estimate the effects of surveillance cameras. On the other hand, if the benefits of surveillance

diffuse to neighboring grids, and those grids are controls, we would under-estimate the effects of the cameras. We make a first attempt to account for this problem by splitting the grids according to their level of exposure. As we describe in Section 3.1, we consider grids within 120 meters of a camera as the treatment group, grids between 120 and 300 meters from a camera as the spillover group, and the remaining grids as controls. However, we also allow for a more flexible estimation of direct and spillover effects, splitting the grids in five groups according to their distance to installation sites: 0 to 60 meters, 60 to 120 meters, 120 to 180 meters, 180 to 240 meters, and 240 to 300 meters. This specification, rather agnostic, allows for non-monotonic displacement patterns.

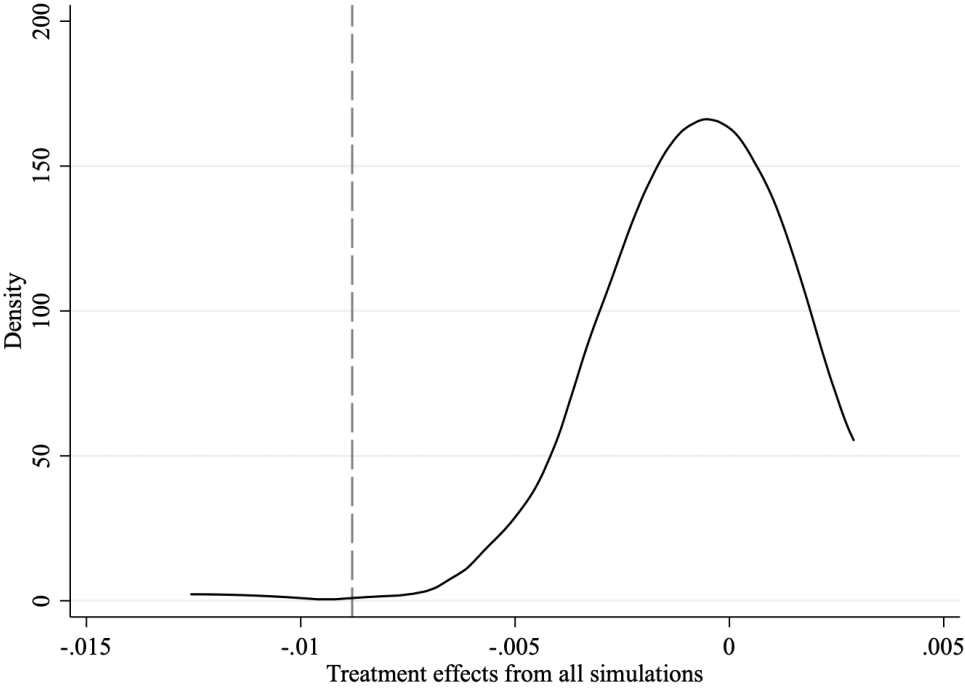
The second problem is related to inference. Assuming the presence of spillovers at buffers around camera installation sites creates a clustering structure that does not conform to standard geographical spaces, such as neighborhoods or *comunas*. This problem is known as fuzzy clustering (Abadie et al., 2017). We address this problem by performing two separate estimations of standard errors and p-values. First, we follow a standard approach and cluster standard errors at the grid level, which accounts for the temporal correlation. Second, we use randomization inference to estimate exact p-values under the sharp null hypothesis of no effect for any unit.<sup>21</sup> Specifically, we run our regression 100 times, but each time we re-assign randomly the location of installed cameras each month, within each *comuna*<sup>22</sup>. We estimate and save treatment effects under each randomization, and then estimate a p-value by looking at where in the distribution of treatment effects lie the estimates for  $\alpha_{Post}$  and  $\beta_{Post}$ . This procedure makes no assumption on the distribution of the error term. Figure 4 illustrates the use of randomization inference for the case of property crimes.

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<sup>21</sup>See for instance Gerber and Green (2012).

<sup>22</sup>Importantly, we keep the number of cameras that were installed at each month.

Figure 4: Distribution of treatment effects for property crimes, under 100 random re-assignments of camera locations



*Notes:* The figure draws the distribution of treatment effects for 100 regressions. Each time, we randomly re-assign the location of installed cameras each month, within each *comuna*. The vertical line highlights our estimate for treatment effects, which we compare with the distribution to retrieve the exact p-value under the sharp null hypothesis of no effect for any grid.

## 4 Results

### 4.1 The direct and spillovers effects

Table 2 present our baseline results. Column (1) reports the effect for total crime. Our estimates suggest there is a reduction of 0.0114 crimes per month per grid, for grids under direct surveillance, relative to control grids. The effects are statistically significant at conventional levels (Panel A). We do not find evidence of spillovers for total crime. The coefficient is small in magnitude and not statistically significant (Panel B). To test for pre-trends, we perform an F test of joint significance for all the pre-treatment dummies. The results suggest that both treatment and spillover grids follow similar trends as control grids. The coefficient for direct effects is equivalent to a decrease of 19 percent relative to the sample mean.

Columns (2) and (3) report the results for property and violent crimes, respectively. In both cases we find a crime reduction at grids under direct surveillance. For property crimes, we find a reduction of 0.0088 crimes per month per grid, equivalent to a 17 percent reduction relative to the sample mean. For violent crimes, we find a reduction of 0.0026 crimes per month per grid, equivalent to a reduction of 26 percent relative to the sample mean. We find no evidence of crime displacement for either type of crime. Moreover, as with total crime, we find no evidence of different pre-trends across the relevant treatment conditions.

Finally, Column (4) report the results on the number of arrests. We find no evidence for grids under direct surveillance. We do observe evidence of an increase of 0.0015 in the number of arrests at grids exposed to spillovers. This result is statistically significant at conventional levels. The coefficient for spillover effects is equivalent to an of roughly 5 percent relative to the sample mean. As in the other cases, we find no evidence of different pre-trends across the relevant treatment conditions. We interpret this result as consistent with a deterrent effect of the cameras, because potential offender may have perceived an increase in the probability of being arrested. We reach at this interpretation because of two additional reasons. First, in our qualitative work we found that the number of camera operators remained constant

at 12 people over the installation period. This implies that the number of cameras per operator changed from 32 to 69 with the installation process.<sup>23</sup> Second, the time window of the installation period leaves little to no chance to use camera footage in prosecution.

Table 2: Direct and spillover effects of surveillance cameras on reported crimes and arrests

	Total	Crimes Property	Violent	Arrests Total
	(1)	(2)	(3)	(4)
<i>A. Direct effects</i>				
0—120 meters	-0.0114	-0.0088	-0.0026	-0.0013
SE clustered at grid level	[0.0024]***	[0.0021]***	[0.0008]***	[0.0012]
SE RI	(0.01)**	(0.01)**	(0.01)**	(0.39)
<i>B. Spillover effects</i>				
120—300 meters	-0.0011	-0.0017	0.0006	0.0015
SE clustered at grid level	[0.0013]	[0.0011]	[0.0006]	[0.0006]**
RI p-value	(0.53)	(0.32)	(0.25)	(0.01)**
<i>C. Tests for pre-trends</i>				
Direct effects: Prob > F	0.8196	0.7731	0.7227	0.5093
Spillover effects: Prob > F	0.9933	0.9169	0.7618	0.8139
<i>D. Effect sizes</i>				
Outcome (mean)	0.0608	0.0507	0.0101	0.0290
Outcome (SE)	0.3009	0.2711	0.1102	0.2398
Implied effect size (direct)	-18.7%	-17.3%	-25.7%	—
Implied effect size (spillover)	—	—	—	5.2%
Observations	947,982	947,982	947,982	789,985
R-squared	0.009	0.008	0.003	0.001
Grid fixed effects	Yes	Yes	Yes	Yes
Time $\times$ <i>comuna</i> fixed effects	Yes	Yes	Yes	Yes

Notes: The reported mean and outcome, correspond to the mean effects of these variables in the locations that were under potential direct effects of a camera. We add pre-trends for the both direct and spillover areas, for a period of 12 months, before the installation of a camera, we show the F test of join significance of these coefficient. We have no evidence of changes before the installation. A more detailed view of this pre-trends is shown in figure 5.

<sup>23</sup>See for instance (Piza et al., 2015) on the importance of monitoring surveillance cameras.

## 4.2 Results over different distance ranges

In Table 3 we present the results from running a more flexible version of equation (1). More specifically, we divide the direct effects in two ranges: from 0 to 60 meters, and from 60 to 120 meters. We also split the spillover effects over three different ranges: from 120 to 180 meters, from 180 to 240 meters, and from 240 to 300 meters. This specification allows for non-monotonic patterns in crime displacement.

Column (1) presents the results for total crime. We find that the direct effects are larger the closer the grid is to the camera. Our estimates suggest there is a reduction of 0.0158 crimes per month per grid, for grids within 60 meters of a camera, relative to the sample mean. This effect is statistically significant at conventional levels. The magnitude of the coefficient is equivalent to a reduction of 26 percent in the number of reported crimes, relative to the control grids. We also find a reduction of 0.0082 crimes per month per grid, for grids between 60 and 120 meters of a camera, relative to control grids. This effect is equivalent to a 14 percent reduction in reported crimes, relative to the sample mean. We find no evidence of crime displacement or diffusion of benefit at any spillover range.

Columns (2) and (3) present the results for property and violent crimes. Broadly, we find a similar pattern as for violent crimes, with relatively larger effects for grids closer to the cameras, and no strong evidence of crime displacement, though we do see some patterns at the 180 to 240 meters range. More specifically, we see a decrease of 0.0034 property crime reports, and an increase of 0.0011 violent crime reports at this range. Both results are statistically significant.

Finally, Column (4) presents the results for arrests. We see a decrease of 0.0033 arrests at grids located within 60 meters of a camera. This result is statistically significant, equivalent to a reduction of 11 percent in the number of arrests, relative to the sample mean. We also observe an increase in the number of arrests at the 120 to 180 meters and the 180 to 240 meters ranges. Both coefficients are statistically significant at conventional levels.

In all cases, the tests for pre-trends suggest that the relevant treatment groups follow a

similar pattern before the installation of the cameras.

### **4.3 The effects of the public surveillance cameras over time**

To study if effects vary over time, we estimate an extended version of equation (1) that includes both pre- and post-installation dummies. We present the results in Figure 5. Broadly, we observe the same patterns as in tables 2 and 3. That is, before the installation of the cameras, there are no differences in crime patterns between treatment and control grids, and spillover and control grids. After the installation of the cameras, total crimes decrease at treatment grids, and there are no major changes in reported crimes at spillover grids.

Additionally, we observe evidence that direct effects on total crimes grow over time. This pattern seems to be mainly explained by property crimes. The evidence on the effects on violent crimes is less robust, and do not seem to follow a specific trend in the post-installation period.

Finally, we do not see robust evidence of changes in the number of arrests at both treatment and spillover grids.

## **5 Discussion and conclusions**

### **5.1 Was this intervention welfare enhancing?**

The results suggest that crime drops at grids under direct surveillance by 0.011 per month, on average. Given the large number of grids exposed to direct surveillance, these effects add up to a relatively large number of deterred crimes.

The number of grids per treatment status varies over time, given the dynamic nature of the installation. On average, there were about 5,868 grids under direct surveillance per month. We use this number and the estimated coefficient for marginal effects as the basis for a back-of-the-envelope analysis of aggregate effects. Roughly, we estimate that the total number of crimes deterred due to direct surveillance was about 67 per month. On average,

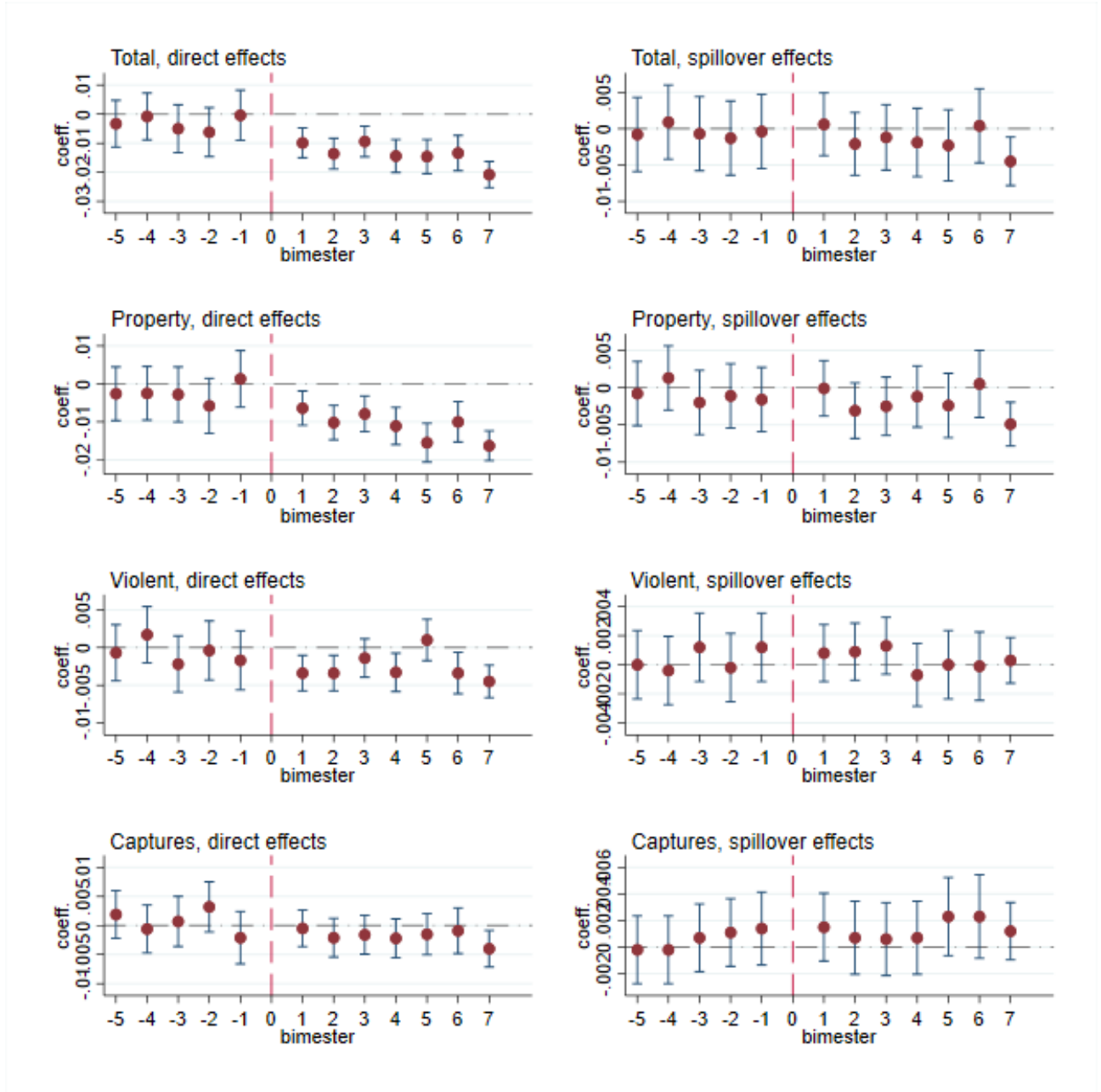
Table 3: Direct and spillover effects of surveillance cameras on reported crime and arrests over different distance ranges

	Total (1)	Crimes Property (2)	Violent (3)	Arrests Total (4)
<i>A. Direct effects</i>				
0—60 meters	-0.0158	-0.0131	-0.0027	-0.0033
SE clustered at grid level	[0.0038]***	[0.0034]***	[0.0012]**	[0.0020]*
RI p-value	(0.01)**	(0.01)**	(0.01)**	(0.01)**
60—120 meters	-0.0082	-0.0057	-0.0025	0.0004
SE clustered at grid level	[0.0019]***	[0.0017]***	[0.0009]***	[0.0013]
RI p-value	(0.01)**	(0.01)**	(0.01)**	(0.71)
<i>B. Spillover effects</i>				
120—180 meters	0.0000	-0.0005	0.0006	0.0023
SE clustered at grid level	[0.0018]	[0.0015]	[0.0008]	[0.0012]*
RI p-value	(0.67)	(0.82)	(0.51)	(0.01)**
180—240 meters	-0.0024	-0.0034	0.0011	0.0020
SE clustered at grid level	[0.0017]	[0.0015]**	[0.0008]	[0.0011]*
RI p-value	(0.19)	(0.05)*	(0.01)**	(0.02)**
240—300 meters	-0.0011	-0.0013	0.0002	0.0000
SE clustered at grid level	[0.0017]	[0.0015]	[0.0008]	[0.0011]
RI p-value	(0.63)	(0.54)	(0.11)	(0.3)
<i>C. Tests for pre-trends</i>				
Direct effects: Prob > F	0.6008	0.5263	0.6866	0.3266
Spillover effects: Prob > F	0.9943	0.8800	0.4920	0.5698
<i>D. Effect sizes</i>				
Outcome (mean)	0.0608	0.0507	0.0101	0.0290
Outcome (SE)	0.3009	0.2711	0.1102	0.2398
Implied effect size (direct 0 - 60)	-26.0%	-25.8%	-26.7%	-11.4%
Implied effect size (direct 60 - 120)	-13.5%	-11.2%	-24.7%	—
Implied effect size (spillover 120 - 180)	—	—	—	7.9%
Implied effect size (spillover 180 - 240)	—	-6.7%	10.9%	6.9%
Implied effect size (spillover 240 - 300)	—	—	—	—
Observations	947,982	947,982	947,982	789,985
R-squared	0.009	0.008	0.003	0.001
Grid fixed effects	Yes	Yes	Yes	Yes
Time $\times$ <i>comuna</i> fixed effects	Yes	Yes	Yes	Yes

Notes: The reported mean and outcome, correspond to the mean effects of these variables in the locations that were under potential direct effects of a camera.



Figure 5: Pre- and Post-installation evolution of effects



Notes: Each period in the graph corresponds to a two month period. Period zero is the two month period before the camera is installed. The coefficient for the last period (7) represents the average effect from that period onwards. The coefficients from direct and spillover effects for each type of crime are estimated in the same regression, but presented separately for ease of understanding.

grids were roughly 10 months under direct surveillance, hence we estimate that the total number of deterred crimes were 670 over the study period.

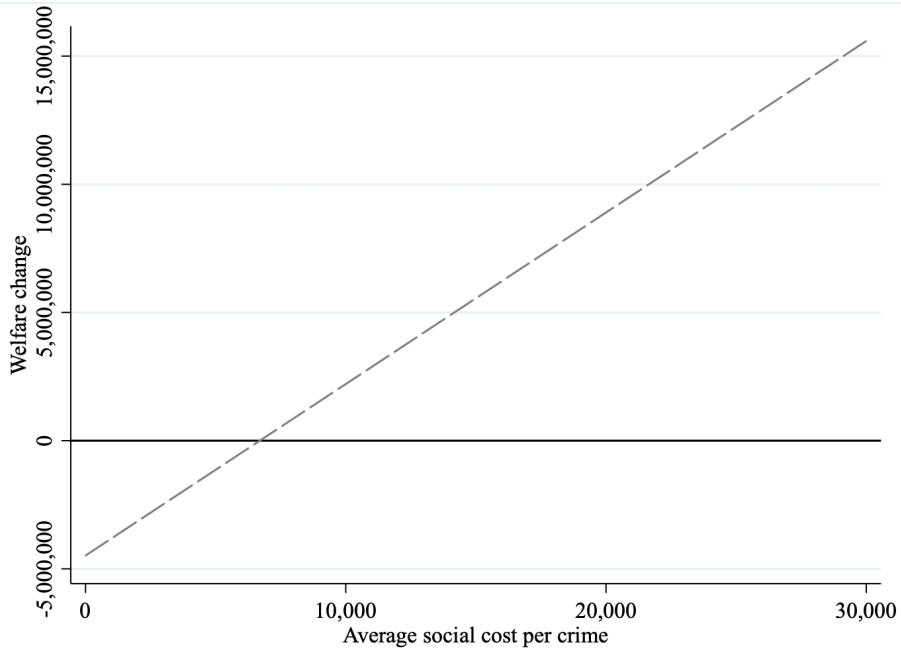
Next, we use our best guess of the aggregate effects of the installation program as well as the approximate cost of each camera to run a back-of-the-envelope cost-benefit analysis. The cost of each camera, as reported by city authorities, rounded \$10,000. The total cost adds up to \$4,480,000 for 448 cameras. Hence, the welfare gain per camera is positive if the average cost per crime is roughly \$6,690. This value is below the most conservative estimates for low-cost crimes (McCollister et al., 2010). Indeed, if crimes are more costly, the welfare gains associated with the intervention can be relatively large. Figure 6 presents the welfare change as a function of the social cost per crime. For instance, if the social cost per crime is \$15,000, the total welfare gain is equivalent to almost \$5,000,000. The point estimate on violent crimes is roughly one-quarter of the effect (with property crimes being the remaining three-quarters). Hence we believe it is plausible that the average social cost per crime is relatively large.

## **5.2 How do surveillance cameras compare with other alternatives?**

Many countries are installing large numbers of public surveillance cameras to prevent crime and violence. This study aims at informing policy-makers on the decision to make these kind of investments. The range of policy options to prevent crime is wide, so it is virtually impossible to analyze all of them. However, the case of Medellin presents an unusual opportunity to do a back-of-the-envelope comparison between the installation of public surveillance cameras and a common policing strategy: hot spots policing.

Collazos et al. (2020) report the results of a randomized controlled trial on a hot spots policing intervention that started in Medellin a few months after the last camera was installed. The units of experimentation were street segments, which have a relatively similar size to the grids we use in this study. Their results suggest that crime dropped by roughly

Figure 6: Estimated welfare change due to the camera installation program



*Notes:* The figure depicts the relation between the average cost per crime in the horizontal axis ( $x$ ) and the welfare change per prison slot in the vertical axis ( $y$ ). The exact equation is  $y = -4,480,000 + 670x$

0.014 crimes per month, on average.<sup>24</sup>

The key remaining factor is the cost of each type of intervention. As we discussed in the introduction, these surveillance cameras implied a lump sum cost of \$10,000 each, with some marginal additional costs for monitoring, which we assume to be zero for simplicity, and to be consistent with the fact that the city did not invest in sustaining the monitoring capacity. On the other hand, the cost of the hot spots policing intervention can be approximated using the hourly wages of Colombian patrolling agents. The extra patrolling time per segment was about 50 minutes per day or 25 hours per month. The hourly wage of a patrolling agent is \$3.80 and each patrol has two agents. Hence the total cost per month per hot spot is \$190.

Assuming the effects are linear and remain constant over time, investing \$10,000 in a

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<sup>24</sup>Note that Collazos et al. (2020) use the same five types of crimes we use in this study, which eases the comparison. They do not report the effect on total crime per month, but rather focus on specific individual types for a period of six months. Here we make the comparison using the effect on aggregate crime per month, which is available to us (two of us are also coauthors in the hot spots policing study).

camera would deter 0.684 crimes over a period of five years or 60 months. Alternatively, \$10,000 invested in police patrols deployed at crime hot spots would last for roughly 53 months, deterring 0.742 crimes. The lifespan of a camera is probably more than five years, but maintenance costs would probably become more relevant by that time. Ultimately, the question on which is the best policy probably depends on the dynamics of local crime. If criminal rents are easy to move around the corner, hot spots policing might be the right response, as it can be adapted and relocated easily. If criminal rents are tied to the specific context of the crime hot spot, direct camera surveillance is potentially the best policy response.

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