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Police-Monitored Cameras and Crime*

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Abstract

We study the effects of police monitoring on crime. We exploit detailed information on the location and date of installation of police-monitored surveillance cameras, coupled with data at the street-segment level on all reported crimes in the city of Montevideo, Uruguay. We find that the introduction of police-monitored surveillance cameras reduces crime by about 20 percent in monitored areas relative to a pure control group located outside the city. We also explore potential displacement effects, and we do not find evidence of such effects.

Keywords: Crime; monitoring cameras; police

JEL classification: *K*42

I. Introduction

A growing number of cities from all around the world are relying on surveillance cameras as a tool for preventing crimes and supporting investigations and prosecutions. However, not enough is known about the effect of surveillance cameras on crime and, in particular, little is known about whether cameras reduce crime or simply relocate criminal activity to other areas.

In this paper, we study the effects of a large-scale introduction of police-monitored cameras in Montevideo (the capital city of Uruguay, with 1.5 million inhabitants). In 2013, the Ministry of the Interior of Uruguay started to install surveillance cameras in some areas of the city. These cameras are continuously monitored by police officers located in a monitoring center that combines video surveillance technology with the action of police patrol

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response. In order to study the effects of this policy on crime, we take advantage of a really unique database that includes the universe of reported crime in the entire city of Montevideo, by street segment, for the three-year period between January 2012 and December 2014, and the location and the time of the installation of all police-monitored cameras in the city.

Our difference-in-differences estimates indicate that the presence of police-monitored cameras is associated with a reduction of about 20 percent in crime in treated areas of the city. What is the rationale behind these results? In principle, the effect that police monitoring has on crime could potentially work through deterrence (a police presence makes criminal activity less attractive) and incapacitation (police officers apprehend criminals, leaving fewer of them around to commit future crimes). Even though there is anecdotal evidence of arrests made by police patrols alerted by officers in the monitoring center,[1] our results are unlikely to reflect changes in the numbers of incarcerated criminals. Thus, we believe that our estimates should be interpreted as a deterrent effect on crime provided by police monitoring.[2]

It is well known that, potentially, the reduction in crime in areas with police monitoring can be compensated by an increase in crime somewhere else – a "displacement" effect.[3] Thus, we have explored potential displacement effects and we have not found evidence of such effects.

Our paper is related to the literature on the effects of police-monitored cameras on crime.[4] In particular, the effect of surveillance cameras on

[1]As an example, in May 2015 the newspaper *El País* reported a case in which surveillance cameras captured a robbery. Police officers in the monitoring center called a patrol in the area and nine minutes later two police officers apprehended the offender. In court, the suspect denied everything, without being aware that the surveillance cameras had registered the crime scene. The video recording was used as probative material in court and the offender was sentenced to prison.

[2]Gómez-Cardona *et al.* (2017) report no significant effects on apprehensions following the installation of surveillance cameras in Medellín, Colombia. Unfortunately, we cannot formally test the effect of surveillance cameras on apprehension rates because there is no information available.

[3]The criminology literature has a long tradition of recognizing the complexity of measuring a displacement effect. For example, Weisburd and Green (1995) analyzed the tension between research designs for measuring direct (or partial equilibrium) effects and displacement effects. In particular, they suggested that the usual design generates a potential bias toward the null hypothesis of no displacement. Along these lines, Braga *et al*. (1999) examined the effects of problem-oriented policing interventions on the problems caused by urban violent crime in Jersey City (NJ, United States), and they found positive local effects and no evidence for displacement.

[4]There is also an important and related body of literature on the effects that police monitoring has on crime; see, for example, Levitt (1997), Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), Poutvaara and Priks (2009), and Draca *et al.* (2011).

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crime is ambiguous in the literature.[5] Welsh and Farrington (2009) present a review of the empirical literature, which is mainly concentrated on small-scaled experiences and focuses on partial equilibrium effects.[6] In addition, only a few papers address the causality problem between the installation of cameras and crime – exceptions include King *et al.* (2008), Hayes and Downs (2011), La Vigne *et al.* (2011), Priks (2014, 2015), van Ours and Vollaard (2016), and Gómez-Cardona *et al.* (2017).[7] Thus, our first contribution to the literature is to study the causal impact of a larger-scaled introduction of police-monitored cameras compared with the previous literature.

Our second contribution is to explore general equilibrium effects. Evidence on general equilibrium effects from anti-crime policies is scarce. Priks (2015) has analyzed the effects of surveillance cameras on crime in the Stockholm subway system. He presents evidence that the introduction of the cameras reduced crime by approximately 25 percent, and he reports some evidence on local displacement. Van Ours and Vollaard (2016) have studied the effect of the introduction of the electronic engine immobilizer in the European Union. This anti-theft device reduced car theft on new (protected) cars. They also report some displacement to older (unprotected) cars. Our results suggest that crime displacement might depend on the particular setting of the policy implemented.

The paper continues as follows. In Section II, we present the data and describe the intervention. In Section III, we present the empirical strategy and report our results. We conclude in Section IV.

II. Intervention and Data

To investigate the effect of police monitoring on crime, we use two sources of data: a database with the date and location of the installations of surveillance cameras and a database on reported crime.

[5]Brown (1995), Skinns (1998), Armitage *et al.* (1999), Ditton and Short (1999), Blixt (2003), Griffiths (2003), Welsh and Farrington (2004), King *et al.* (2008), La Vigne *et al.* (2011), Hayes and Downs (2011), McLean *et al.* (2013), Piza *et al.* (2014), Priks (2014, 2015), van Ours and Vollaard (2016), and Gómez-Cardona *et al.* (2017) find reductions on crime following the installation of surveillance cameras. However, Winge and Knutsson (2003), Gill *et al.* (2006), Farrington *et al.* (2007), Ratcliffe *et al.* (2009), and Lim and Wilcox (2017) find no conclusive results.

[6]Alexandrie (2017) reviews recent studies that rely on random assignment and natural experiments in order to address some of the methodological shortcomings of previous literature.

[7]Priks (2014) presents evidence that surveillance cameras reduce unruly behavior inside soccer stadiums. Gómez-Cardona *et al.* (2017) find that the total number of crimes reported is 24 percent lower within the coverage zone following the installation of a camera, relative to the average baseline level of crime reported.

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The first database was provided by the Ministry of the Interior of Uruguay and it includes information on the date of installation and the location of all surveillance cameras installed in Montevideo since 2013.

Surveillance cameras in Montevideo are exposed to everyone's view and they are continuously monitored by police officers located in a monitoring center with more than a hundred employees. When police officers in the monitoring center observe a crime, they contact a mobile patrol in the area and ask them to go immediately to the crime scene. In those cases where the police officers in the monitoring center see suspicious activity, they can follow the suspect using the cameras (which are able to zoom in and rotate up to 360°) before deciding whether or not to contact the mobile patrol in the area. According to the police authorities in Montevideo, the average response is about five minutes (*El Observador*, 2015). When that happens, and the police officers arrest the offender, the video recording becomes part of the probative material.[8]

We divide Montevideo into 10,868 street segments by considering the geographical latitude and longitude of all the streets of the city. We define a treated street segment as an area under strict police monitoring. Each treated street segment can be protected by more than one camera in order to ensure a clear visual recognition of potential offenders. At the end of our sample period, there were 277 street segments in Montevideo monitored with surveillance cameras.

The second database was provided by the Police Department of Montevideo and this includes monthly information on the universe of criminal incidents reported in Montevideo, between January 2012 and December 2014 (273,700 offenses). The database is geo-located, which means that each criminal incident recorded includes information on the exact location through its geographical coordinates (latitude and longitude). For a subset of crimes (robberies and thefts), we also have monthly data at the jurisdiction level for all the country.

Figures 1 and 2 summarize the geographical distribution of the areas monitored by surveillance cameras, the variability in the installation dates across different street segments, and crime incidence in Montevideo in October 2012, before the beginning of the program. As usual, crime is clustered in small areas that include not only the street segments covered by police-monitored cameras, but also other areas all around the city.

Reported crime is classified by the Police Department using more than 130 different types of offenses. In the period of analysis (2012–2014), six

[8]The Supreme Court surveyed 20 criminal judges in Montevideo and all of them reported that they rely on video from surveillance cameras as an investigative tool (see *El Observador*, 2015). In fact, since the beginning of the program, 288 prosecutions have relied on images from surveillance cameras (see *El Observador*, https://www.elobservador.com.uy/nota/no-robe-lo-estamos-filmando-2016151090, 5 January 2016).

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Fig. 1. Police-monitored areas and previous crime [Colour figure can be viewed at wileyonlinelibrary.com]

categories account for 92 percent of total crime: thefts represent 50 percent of total reported crime; robberies, 14 percent; assault, 12 percent; domestic violence, 9 percent; damage, 7 percent; murder, 0.2 percent.[9]

In the Uruguayan Penal System, in line with international standards, theft is defined as depriving a person of property without the use of violence. Robbery is defined as depriving a person of property with the use of violence or threat of violence. Assault is an intentional physical attack or threat against another person, excluding domestic violence. Domestic violence is defined as a pattern of abusive behavior (physical, sexual, emotional, economic, or psychological actions or threats of actions) in any relationship that is used by one partner against an intimate partner. Damage is an act of vandalism involving deliberate destruction of or damage to public or private property. Murder implies causing the death of another person without extreme provocation or legal justification.

Summary statistics are reported in the top panel of Table 1. In the top panel, the unit of observation is the street segment and the data correspond to the whole city of Montevideo. On average, there is one

[9]The share of these six categories in total crime is quite stable over the period: 92 percent in 2012, 92 percent in 2013, and 91 percent in 2014.

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Fig. 2. Date of installation and location of the cameras [Colour figure can be viewed at wileyonlinelibrary.com]

reported crime per street segment every 45 days (0.66 crimes per month and street segment), with theft being the most prevalent type of crime. Table 2 provides additional summary statistics, distinguishing between eventually treated areas (i.e., those areas that at some point in the sample period are under surveillance) and untreated areas. As expected, crime is higher in the eventually treated areas relative to untreated areas.

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Table 1. *Summary statistics*

	Mean	Std dev.	Min.	Max.
Montevideo (street segment)				
Total crime	0.6675	1.5635	0	86
Assault	0.0801	0.3421	0	17
Theft	0.3314	0.9013	0	31
Domestic violence	0.0604	0.3057	0	23
Damage	0.0462	0.2405	0	13
Murder	0.0012	0.0347	0	2
Robbery	0.0937	0.3847	0	25
Indoor crime	0.1405	0.5143	0	36
Outdoor crime	0.4251	1.0770	0	50
Police monitoring	0.0064	0.0800	0	1
Whole country (jurisdiction)				
Outdoor crime	215.93	217.37	5	1,645
Theft	185.06	192.31	5	1,424
Robbery	30.41	37.87	0	242

Notes: Indoor crime includes domestic violence and assaults. Outdoor crime includes theft and robbery. The period is January 2012 to December 2014 (36 months). In Montevideo, the data are at the street-segment level (10,868 street segments). In the whole country, the data are at the jurisdiction level (42 jurisdictions).

In the bottom panel of Table 1, we report summary statistics for the whole country. For the whole country, we have monthly data at the jurisdiction level for robberies and thefts, which together represent approximately 65 percent of total crime.

III. Empirical Strategy and Results

To identify the effect of police-monitored cameras on crime, we use a difference-in-differences approach that exploits the variability over time and space of the introduction of the cameras.

The installation of surveillance cameras targeted five specific areas of the city and occurred simultaneously with other actions followed by the Police Department. Specifically, a continuous patrolling of the targeted areas was implemented in order to reduce police response time. The presence of other anti-crime initiatives in Montevideo might potentially affect our identification strategy, especially if the introduction of such policies were collinear to the introduction of surveillance cameras. Even though we do not have information on police patrols, after a series of meetings with key authorities we confirmed that the path followed by police patrols was orthogonal to the date of installation of each camera. We also confirmed

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Table 2. *Summary statistics: eventually treated and untreated areas in Montevideo*

	Untreated areas Mean (std dev.)		Eventually treated areas Mean (std dev.)	
Total crime	0.6408	(1.5252)	1.6864	(2.4293)
Indoor crime	0.1378	(0.5122)	0.2440	(0.5785)
Outdoor crime	0.4072	(1.0477)	1.1094	(1.7494)
Theft	0.3147	(0.8691)	0.9702	(1.6029)
Robbery	0.0925	(0.3838)	0.1392	(0.4157)
Domestic violence	0.0602	(0.3063)	0.0675	(0.2821)
Assault	0.0775	(0.3373)	0.1765	(0.4810)
Damage	0.0441	(0.2348)	0.1244	(0.3943)
Murder	0.0012	(0.0348)	0.0009	(0.0332)

Notes: Indoor crime includes domestic violence and assaults. Outdoor crime includes theft and robbery. Standard errors are shown in parentheses. The period is January 2012 to December 2014 (36 months). The total number of street segments is 10,868. In all cases, the mean is calculated using observations for the whole period of analysis (before and after the treatment).

with the authorities that the concentration of policing efforts across the targeted segments does not imply lower police surveillance in the street segments without monitoring cameras. Nonetheless, our findings should be interpreted as the joint effect of the cameras and the increased police presence. This is important because a general increase in police presence could imply that the effect of the cameras is different than it would have been without this general increase in police presence.

Formally, we estimate the following difference-in-differences equation,

$$Y_{it} = \beta T_{it} + \alpha_i + \mu_t + \varepsilon_{it}, \tag{1}$$

where Y_{it} is crime in street segment i at month t, T_{it} is a dummy variable that takes the value of 1 for treated street segments (i.e., those with a police-monitored surveillance camera at month t) and 0 for control street segments, α_i is a street-segment fixed effect, μ_t is a month fixed effect (36 months in our sample period), and ε_{it} is the usual error term. The parameter of interest is β.

In a first empirical strategy, we restrict the sample to eventually treated street segments (277). In this sample, the control group includes those street segments that are untreated at time t but eventually treated at some point in the future (during the sample period). In this strategy, identification exploits the variability in the installation dates across different street segments. Given that the installation procedure relied on operational decisions and that installation sites were not prioritized, then the order in which the cameras were installed can be considered exogenous to crime trends. Thus, we have

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Table 3. *Difference-in-differences results*

	Total crime	
	Eventually treated street segments	Whole city
	(1)	(2)
Police monitoring	−0.3351*** (0.1019)	−0.5305*** (0.0598)
Percentage change	−18.72	−29.64
Street segments	277	10,868
Observations	9,972	391,248

Notes: Standard errors clustered at the street-segment level are shown in parentheses. All regressions include street-segment dummies and month/year-combination dummies (36 months). Column 1 shows results restricted to eventually treated street segments. Column 2 shows results using the whole city of Montevideo. Percentage change is calculated relative to the mean of total crime in treated areas in the pre-treatment period (1.79). ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

a source of variability that can be exploited in order to identify causal effects.[10]

Column 1 in Table 3 reports difference-in-differences estimates of the effect of surveillance cameras on crime, using the sample of eventually treated street segments. Standard errors are clustered at the street-segment level. The estimated coefficient on police monitoring is negative and statistically significant at the 1 percent level. The result is not only statistically significant but also relevant: the incorporation of police-monitored surveillance cameras reduces crime by 19 percent in monitored areas relative to unmonitored areas that will eventually be monitored.[11, 12]

In a second strategy, we use all street segments in the city of Montevideo. As reported in Column 2 in Table 3, the coefficient on police monitoring is negative and statistically significant at the 1 percent level. This provides further evidence that the installation of surveillance cameras

[10]The period 2012–2014 was active in terms of new policies implemented by the Police Department. The government (Ministry of the Interior) increased the wages of the police force and the number of police officers, upgraded the equipment of the police, trained police officers with seminars given by well-known criminologists such as Lawrence Sherman, and even signed a cooperation agreement with the New York Police Department. Even though these policies affect the general effectiveness of the police and have the potential to affect crime rates in Uruguay, they do not invalidate our identification strategy as their effects should be captured by the time dummies.

[11]We estimate the percentage change relative to the mean of total crime in treated areas in the pre-treatment period.

[12]We obtain similar results using daily data instead of monthly data, and when we exclude month fixed effects and street fixed effects. All results mentioned and not shown are available from the authors upon request.

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has an important effect on reducing crime in monitored areas relative to unmonitored areas.[13]

Even though the differences are not statistically significant, the smaller estimated coefficient in the eventually treated sample compared with the estimated coefficient when we use the whole city of Montevideo suggests the presence of positive spillovers. The reason for this might be because, in the eventually treated sample, control and treatment street segments are geographically close to each other and criminals might avoid all areas close to the cameras. This implies that there is a question about the validity of the control group in the sample of eventually treated street segments.

Indeed, for these difference-in-differences strategies to be valid, the control group should have a similar crime trend as the treatment group in the absence of treatment, which is called the parallel trends assumption, and the crime levels in the control group should not be affected by the treatment, called the Stable Unit Treatment Value Assumption (SUTVA) assumption. In the following, we check the validity of these two assumptions.

Parallel Trends Assumption

While the parallel trends assumption cannot be tested, it is possible to check whether crime trends in treated street segments and control street segments were the same in the pre-treatment period. If time trends were parallel in the pre-treatment period, then it is likely that they would have continued to be parallel in the post-treatment period in the absence of the treatment. To test the parallel trends assumption, we ran an alternative specification with leads and lags. Formally,

$$Y_{it} = \sum_{k=q^-}^{q^+} \beta^k T_{it}^k + \alpha_i + \mu_t + \varepsilon_{it}, \tag{2}$$

where T_{it}^k is a dummy variable taking the value of 1 if treatment took place k periods ago, q^- is the pre-period furthest back, and q^+ is the post-period furthest after the installation of the cameras. In this way, β^k measures the effect k periods after treatment took place. If k is negative, then β^k measures the effect k periods before the treatment.

Table 4 reports estimates of equation (2) for total crime, for the two alternative samples (we set $k = 6$). As observed in Table 4, in the main empirical strategy reported in Column 1, all pre-event dummies are individually (and jointly) equal to zero, thus providing confidence regarding

[13]The significance of all coefficients reported in Table 3 remains unchanged if standard errors are clustered at the jurisdiction level.

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Table 4. *Pre-treatment trends and dynamic patterns*

	Total crime			
	Eventually treated street segments		Whole city	
	(1)		(2)	
t6 before	−0.0290	(0.1067)	−0.1006	(0.0878)
t5 before	−0.1249	(0.1162)	−0.2111**	(0.0857)
t4 before	0.0300	(0.1133)	−0.0853	(0.0808)
t3 before	0.1073	(0.1358)	0.0640	(0.0994)
t2 before	0.0922	(0.1241)	0.0628	(0.0853)
t1 before	0.0773	(0.1498)	0.0569	(0.1062)
t	0.0936	(0.1447)	−0.0342	(0.1066)
t1 after	−0.3528**	(0.1566)	−0.5144***	(0.0875)
t2 after	−0.2930*	(0.1665)	−0.4478***	(0.1020)
t3 after	−0.3583**	(0.1782)	−0.5117***	(0.0900)
t4 after	−0.4320**	(0.1998)	−0.5988***	(0.1054)
t5 after	−0.6375***	(0.2249)	−0.7485***	(0.1366)
t6 or more after	−0.5153**	(0.2239)	−0.6929***	(0.0771)
Street segments	277		10,868	
Observations	9,972		391,248	

Notes: Standard errors clustered at the street-segment level are shown in parentheses. All regressions include street-segment dummies and month/year-combination dummies (36 months). Column 1 shows results restricted to eventually treated street segments. Column 2 shows results using the whole city of Montevideo. Pre-treatment mean of total crime in treated areas is equal to 1.79. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

the difference-in-differences parallel trends assumption.[14] In Figure 3, we plot the sequence of β^k (and confidence bands) corresponding to the specification in Column 1 against the time line of events.

Column 2 reports estimates of equation (2) for the whole city of Montevideo. In this specification, five out of six coefficients associated with the pre-event dummies are individually equal to 0, providing additional support for the difference-in-differences parallel trends assumption.

The specification in equation (2) also allows us to examine the dynamic pattern following the installation of the cameras. The effect on crime is already observed in the first month following the installation of the cameras, and the effect is relatively stable over time.

SUTVA Assumption

As explained above, a second assumption is that the crime levels in the control group should not be affected by the treatment. If this assumption

[14]The p-value for the joint test that all six "leads" coefficients are equal to zero is 0.60.

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Fig. 3. Sequence of β^k against the time line of events

is violated, results from the difference-in-differences strategies might be biased. If the introduction of cameras does induce criminals to move to control street segments, then crime levels in the control group would go up, thereby biasing the effect downwards. The effect would instead be upward biased if the introduction of cameras in the treatment street segments caused crime to go down in the control group as well.

In order to account for the potential violation of the SUTVA assumption, we need a pure control area that is not affected by potential spillover effects. Under the assumption that potential spillovers only occur within Montevideo, we use jurisdictions outside Montevideo as the pure control group. In this way, the potential contamination of the control group can be addressed by comparing the evolution of crime in the treated area with the evolution of crime in the pure control area. That is, we divided the country into three areas: the first group includes all treated areas in Montevideo; the second group includes all untreated areas in Montevideo (this area is the most likely area to be exposed to spillovers or displacement effects); the third group includes all areas outside Montevideo (this is a pure control group that we assume is not exposed to spillovers or displacement).

As described in Section II, for those jurisdictions outside Montevideo, there is only monthly information at the jurisdiction level. In Column 1 of Table 5, we report difference-in-differences estimates using the pure

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Table 5. *Results using data at the jurisdiction level*

	Outdoor crime	
	(1)	(2)
Police monitoring	−43.2767*** (12.3622)	−33.3725*** (11.9686)
Percentage change	−20.44	−17.91
Jurisdictions	28	32
Observations	984	1,128

Notes: Standard errors clustered at the jurisdiction level are shown in parentheses. All regressions include jurisdiction dummies and month/year-combination dummies (36 months). Column 1 uses eventually treated jurisdictions and pure control jurisdictions. Column 2 uses untreated jurisdictions in Montevideo and pure control jurisdictions. Percentage change is calculated relative to the mean of total crime in treated areas in the pre-treatment period (211.77 for Column 1 and 186.27 for Column 2). ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

control group.[15] The value of the coefficient indicates that the introduction of police-monitored surveillance cameras reduces crime by 20 percent in monitored areas relative to the pure control group area. This result is similar to the result obtained with street-segment data and where the control group was constructed with all untreated areas within Montevideo.[16]

Overall, our results indicate that the introduction of surveillance cameras is associated with an important decrease in crime.

Displacement

Without evidence of the presence or not of displacement effects, little can be learnt from finding that the increase of police monitoring in certain areas reduces crime relative to unmonitored areas. For policy action, it is necessary to understand whether the reduction in crime in monitored areas is compensated or not by a similar increase in other areas of the city. Indeed, the discussion on general equilibrium effects of policy interventions is a critical topic, encompassing not only policy interventions on criminal activity but also other policy interventions that economists are interested in.[17]

[15]We consider as treated any area in which a surveillance camera was installed during that month.
[16]Column 1 in Table A1 in the Online Appendix reports pre- and post-trends. Four out of six of the pre-trend dummies are significant. The pattern suggests that crime was already lower in the treatment areas before the cameras were installed but there appears to be a further decline when cameras are installed. The decline appears to start one to two months before the installation of the cameras. Thus, while this analysis does not formally allow for causal interpretation, the results are consistent with a decline in crime in connection with installation of the cameras.
[17]Crépon *et al.* (2013), for example, report experimental evidence of the existence of displacement effects in labor market interventions.

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Thus, having established a link between surveillance cameras and crime, we now explore whether the introduction of surveillance cameras in certain areas of the city causes displacement effects in other areas of the city.

To formally explore potential displacement effects, we perform a difference-in-differences exercise in which the "treated" units are the unmonitored jurisdictions in Montevideo. The pure control units are all jurisdictions outside Montevideo. The "intervention" is the introduction of cameras in the monitored area of Montevideo (a dummy variable that takes the value of 1 in the unmonitored areas of Montevideo in the post-intervention period). Under the hypothesis of displacement, we should observe an increase of crime in the unmonitored areas of Montevideo relative to pure control jurisdictions upon the introduction of cameras in the city. Under the hypothesis of positive spillover effects, we should observe a decrease of crime in the unmonitored areas of Montevideo relative to pure control jurisdictions upon the introduction of cameras in the city.

Results from this difference-in-differences exercise are presented in Column 2 of Table 5. The coefficient on the intervention variable is negative and significant, indicating the presence of positive spillover effects. Even though the reduction in crime is smaller than the reduction observed in monitored areas, this finding suggests that surrounding areas also benefited from the protection provided by the increase in monitoring.[18]

Additional Results

According to police authorities in Montevideo, theft and robbery are the types of crime that are more likely to occur outdoors and, for this reason, they are also the type of crime more likely to be prevented by the use of surveillance cameras. In contrast, assault and domestic violence are the types of crime more likely to occur indoors and, therefore, are less likely to be prevented by the use of surveillance cameras. We follow their classification and we construct two variables: outdoor crime (thefts and robberies) and indoor crime (assaults and domestic violence).

Table 6 reports regressions for indoor crime and outdoor crime as the dependent variables.[19] For indoor crime, surveillance cameras should not

[18]Column 2 in Table A1 in the Online Appendix reports pre- and post-trends. This analysis shows that pre-trends are not clearly parallel and that the spillover effect found in Table 5 is driven by one significant post-parameter. Overall, this analysis does not clearly indicate whether there are spillover effects, but it does not show any sign of displacement effects.

[19]Table A2 in the Online Appendix reports pre- and post-trends. In the models in Columns 1–3, all coefficients associated with the pre-event dummies are individually equal to 0. In the model in Column 4, four out of six coefficients associated with the pre-event dummies are individually equal to 0. Overall, these results provide support for the difference-in-differences parallel trends assumption.

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Table 6. *Indoor crime and outdoor crime*

	Indoor crime		Outdoor crime	
	(1)	(2)	(3)	(4)
Police monitoring	0.0155	−0.0009	−0.2606***	−0.4209***
	(0.0237)	(0.0123)	(0.0773)	(0.0440)
Percentage change	6.46	−0.36	−21.72	−35.08
Street segments	277	10,868	277	10,868
Observations	9,972	391,248	9,972	391,248

Notes: Standard errors clustered at the street-segment level are shown in parentheses. All regressions include street-segment dummies and month/year-combination dummies (36 months). Columns 1 and 3 show results restricted to eventually treated street segments. Columns 2 and 4 show results using the whole city of Montevideo. Percentage change is calculated relative to the mean of total crime in treated areas in the pre-treatment period (0.24 for indoor crime and 1.2 for outdoor crime). ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively.

have an effect, or at least they should have a much smaller effect.[20] This is exactly what we find: there is no effect of surveillance cameras on indoor crime.[21] In addition, when we use outdoor crime as the dependent variable, the estimated coefficients on police monitoring are negative and statistically significant, and with similar values (in terms of percentage change) to those reported in Table 3.[22]

IV. Conclusions

In this paper, we present new evidence on the aggregate effects of police monitoring on crime. We find that the installation of police-monitored surveillance cameras significantly reduces street crime in monitored street segments relative to unmonitored street segments.

Given the nature of the intervention, our findings should be interpreted as the joint effect of the surveillance cameras and the increased police presence. This is important because a general increase in police presence could imply that the effect of the surveillance cameras is different than it would have been without a general increase in police presence. In addition, given that our data are on reported crime, it could be argued that the

[20]It is possible to argue that, even for indoor crimes, surveillance cameras can deter criminal behavior by providing potential evidence against aggressors.

[21]We estimate the models with leads and lags (not reported) and, considering the four cases, most pre-event dummies (from 24 coefficients corresponding to six pre-event coefficients for four specifications, with 22 of them not significant) are individually (and jointly) equal to 0, thus providing confidence in the difference-in-differences identifying assumption.

[22]The fact that most of the effect from the introduction of cameras is on outdoor crime validates the use of outdoor crime to perform the spillover analysis above.

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installation of surveillance cameras might affect the willingness to report crime. If this were the case, and assuming that reporting of crime increases with the installation of surveillance cameras (e.g., because victims believe that offenders are more likely to be apprehended), our estimates can be interpreted as lower-bound estimates on the effect of surveillance cameras on crime.

A simple cost–benefit analysis shows that introducing police-monitored surveillance cameras is a very efficient policy. On the one hand, government authorities estimate that the total cost of the program (including cameras, software, and police agents) is about US$350,000. This program reduces crime by around 35 offenses per month (or 420 per year) in monitored areas of Montevideo. Therefore, the estimated cost for each offense avoided is US$830. On the other hand, the Inter-American Development Bank (2017) estimates that the total expenditure in citizen security and crime prevention in Uruguay is 2.2 percent of GDP (or US$1,592 in current US dollars).[23] Given the total number of offenses reported per year in Uruguay, the cost incurred by the private sector and the government translates into US$4,200 per offense. This amount is five times higher than the estimated cost for each offense avoided with the use of police-monitored surveillance cameras.

Our findings have important policy implications: surveillance cameras seem to be an adequate instrument to keep crime lower in certain areas of the city.

Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Online Appendix

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[23] See Soares (2015) for details on the simple economic model followed to analyze the conceptual content underlying the estimates of the costs of crime.

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