



# Documentos de trabajo

## **Economía y Finanzas**

**N° 21-07**

2021

---

### **Broken windows policing and crime: Evidence from 80 Colombian cities**

Daniel Mejía, Ervyn Norza, Santiago Tobón,  
Martín Vanegas-Arias

# Broken windows policing and crime: Evidence from 80 Colombian cities\*

Daniel Mejía    Eryvn Norza    Santiago Tobón    Martín Vanegas<sup>†</sup>

September 1, 2021

## Abstract

We study the effects of broken windows policing on crime using geo-located crime and arrest reports for 80 Colombian cities. Broadly defined, broken windows policing consists of intensifying arrests—sometimes for minor offenses—to deter potential criminals. To estimate causal effects, we build grids of  $200 \times 200$  meters over the urban perimeter of all cities and produce event studies to look at the effects of shocks in police activity in the periods to follow. We use spikes in the number of arrests with no warrant—which are more likely associated with unplanned police presence—as a proxy for shocks in broken windows policing. As expected, we observe an increase in crimes during the shock period, as each arrest implies at least one crime report. In the following periods, crimes decrease both in the place of the arrests and the surroundings. With many treated grids and many places exposed to spillovers, these effects add up. On aggregate, the crime reduction offsets the observed increase during the shock period. Direct effects are more immediate and precise at low crime grids, but beneficial spillovers seem more relevant at crime hot spots. The effects of broken windows policing circumscribe to cities with low or moderate organized crime, consistent with criminal organizations planning their activities more systematically than disorganized criminals.

**JEL codes:** K42, O17, E26, J48, C93

**Keywords:** crime, violence, police, arrests, spillovers

---

\*This research is thanks to the collaboration of the National Police of Colombia, especially the Director, General Jorge Luis Vargas; the Head of Planning, General Luis García; and the Chief of Staff for the Director, Colonel Richard Ibáñez. For sharing the data, we thank the Observatorio del Delito from the National Police. Estefany Peña and Hoover Arbeláez provided excellent research assistance. For funding we thank Universidad EAFIT. The concepts expressed in this working paper are the sole responsibility of the authors and in no way commit the Universidad EAFIT or the Centro de Investigaciones Económicas y Financieras (Cief). Total or partial reproduction of the content is authorized, always citing the source.

<sup>†</sup>Mejía: Universidad de los Andes, Economics, [dmejia@uniandes.edu.co](mailto:dmejia@uniandes.edu.co); Norza: National Police of Colombia, [ervyn.norza@correo.policia.gov.co](mailto:ervyn.norza@correo.policia.gov.co); Tobón (corresponding author): Universidad EAFIT, Economics, [stobonz@eafit.edu.co](mailto:stobonz@eafit.edu.co); Vanegas: Universidad EAFIT, Economics, [mvanega3@eafit.edu.co](mailto:mvanega3@eafit.edu.co).

# 1 Introduction

Police manpower and deployment strategies are at the core of crime prevention efforts. The idea is simple: police presence should decrease the probability of crime occurrence by either deterring or incapacitating offenders.<sup>1</sup> Indeed, the consensus in the literature is that increases in police manpower lead to fewer crimes.<sup>2</sup> Moreover, previous studies suggest that deployment strategies—such as hot spots policing—not only reduce crime at high crime areas, but also spill benefits over to surrounding locations.<sup>3</sup> However, we know less about the effects of regular police intensity—or broken windows policing—on crime. This literature usually focus on the number of driving under the influence and disorderly conduct arrests as indirect measures of police intensity.<sup>4</sup> As noted by Chalfin and McCrary (2017), however, most studies “are plagued by problems of simultaneity bias, omitted variables, and the inevitable difficulty involved in finding a credible proxy.”

In this paper, we benefit from three characteristics of the Colombian context to examine the direct and indirect effects of broken windows policing practices on crime. First, the availability of the universe of arrest and crime records for 80 cities for 2019. The effects of police interventions that are not intense in nature, such as broken windows policing, are usually small in magnitude. Hence without a large enough sample, small effects could be undetectable. Crime records include the type of crime, exact coordinates and date of occurrence. Arrest records include the exact coordinates, date of occurrence, and information on whether the arrest was conducted following an arrest warrant or rather “in-the-act,” when police patrols happened to observe a crime as they were passing through. These 80 cities account for roughly 44% of the population of the country.<sup>5</sup> We restrict our analysis to the

---

<sup>1</sup>See for instance Becker (1968) and Ehrlich (1973).

<sup>2</sup>See Chalfin and McCrary (2017) for a complete discussion of the literature on police manpower and other crime prevention strategies.

<sup>3</sup>See for instance Braga et al. (2019) for a systematic review of this evidence.

<sup>4</sup>Sometimes broken windows policing is described as more aggressive enforcement against minor offenses in order to prevent more serious crime. Hence the focus on arrests for driving under the influence and disorderly conduct. See for instance Kubrin et al. (2010)

<sup>5</sup>These are the 80 largest cities of Colombia, excluding the capital Bogotá.

urban perimeter of these cities.

Second, a wide heterogeneity in police activity and local crime patterns both within and across cities. The key empirical challenge when estimating the effects of broken windows policing on crime is that changes in patrolling patterns are not random. We use the coordinates and date of occurrence of all crimes and arrests to build a bi-weekly panel data, where we use grids of  $200 \times 200$  meters within the urban perimeter of the 80 cities as the cross sectional unit. We use this panel to produce event studies where we look at the effects of shocks in police activity in a given grid and period. We build our direct treatment variable as any grid and period with three or more arrests conducted “in-the-act.” Two or fewer arrests are common, and we worried this looser definition would not pick up an actual intensification of police presence. We focus on arrests with no arrest warrant as these are more likely to proxy for unplanned police activities. Furthermore, because of the simultaneous determination of crimes and arrests, we expect the treatment period to show a large increase in crime. Hence our outcomes of interest are crimes occurring in the periods following the police activity shock, both where the shock took place as well as in the surrounding areas.

Finally, we leverage the variation within and across cities to look at heterogeneous treatment effects. On the one hand, we examine how the effects change based on the baseline crime levels of the specific locations where the arrests occurred. To some degree, this analysis complements the broad literature on hot spots policing by looking at the effects of policing places with moderate crime levels. On the other hand, we look at how the effects change across cities based on baseline characteristics of the local crime environment. In particular, we study whether the effects are different in cities with a stronger tradition of organized crime. Many criminal organizations plan their actions in advance and strategically adjust to police activity.<sup>6</sup> Hence the effects of broken windows policing may differ depending on how organized are criminal activities in a city.

This paper offers five sets of results. First, we observe a decrease in total crime in the

---

<sup>6</sup>Blattman et al. (2021a), for instance, document how street gangs in Medellín are part of an elaborate, hierarchical system. These groups coordinate their responses to state action.

periods following the shock in police activity. As expected, police activity shocks are associated with an increase in crime in the concurrent period—each arrest necessarily implies at least one crime report. But crimes fall monotonically after the shock, reaching a statistically significant and economically meaningful decrease in the fourth period. Relative to the average number of crimes occurred one period before the shock, the effects in the fourth period are equivalent to a 6% decrease. Furthermore, we observe a similar pattern in areas nearby the location of the shock. The coefficients in the third and fourth periods are statistically significant, and their magnitudes are equivalent to a decrease of roughly 3% and 5% in reported crimes, respectively. This suggests the benefits of police activity diffuse to the surroundings.<sup>7</sup>

Second, we conduct a back-of-the-envelope estimation of aggregate effects. With many treated locations, and many places exposed to spillovers for each treated grid, the benefits add up to potentially large reductions in crime. We find that the decrease in crimes in the periods after the shock is large enough to offset the observed increase at the shock period. Our estimate of this aggregate reduction in crime is not precise, but it is close to conventional levels of statistical significance.

Third, the direct effects are not circumscribed to a specific type of crime. Rather, we observe similar patterns for both violent and property crimes. For property crimes, however, the direct effects are imprecisely estimated. We do observe differences in spillover patterns. Beneficial spillovers are larger and more precise for property crimes.

Fourth, the direct effects of shocks in police activity seem to be more immediate and precise in low-crime areas. Following the shock, crimes drop between 9% and 13% in the subsequent periods, relative to the average number of crimes one period before the arrest in low-crime areas. Crimes in high-crime areas seem to fall by period four after the shock, but these effects are imprecisely estimated. Beneficial spillovers are more relevant at crime hot

---

<sup>7</sup>Jacob et al. (2007) find that crime shocks could also reduce future crime. Hence, an alternative interpretation of our results is that present crime—rather than police activity—could be driving the effects. However, we believe the most plausible interpretation in our context is that the main treatment is the police activity shock. We reach this conclusion due to the observed patterns of spatial displacement.

spots.

Fifth, both direct effects and beneficial spillovers seem to be more important in cities with low or moderate presence of organized crime. We proxy for organized crime using the rates of drugs and weapons seizures, as well as the presence of coca crops. Criminal organizations in Colombia are usually associated with local drug markets and widespread use of weapons. Also, criminal organizations are traditionally present in cities with coca crops, both to provide protection and control the supply of coca leaves for the production of cocaine. All results point in the same direction: direct and spillover effects are larger and more precisely estimated in cities with low or moderate presence of organized crime. The coefficients for direct and indirect effects are imprecise and sometimes positive in places where organized crime is presumably more active, as measured with all these proxies.

This paper contributes to a few strands of the literature. First, criminologists have long studied the effects of different police deployment strategies on crime. The closest precedent to this paper are studies focusing on broken windows policing. Sampson and Cohen (1988) use city-level data to study correlates of broken windows policing practices with robbery rates for a sample of 171 cities in the US. Their results suggest a negative association between broken windows policing and crime. MacDonald (2002), and later Kubrin et al. (2010) replicate the Sampson and Cohen (1988) study with variations in the time frame, main outcomes and approach. Their results also point to a negative elasticity of crime with respect to the implementation of broken windows policing practices. Two studies exploit variation within a single city. Harcourt and Ludwig (2006) and Rosenfeld et al. (2007) use precinct-level data from New York and find that misdemeanor arrests lead to very small effects on crime.

Second, also related to this paper are studies focusing on police deployment strategies such as hot spots policing. Broadly, hot spots policing consists of allocating disproportional police resources in high-crime areas. This has been a frequent subject of study both in criminology and economics.<sup>8</sup> A recent systematic review analyzes 65 studies (Braga et al.,

---

<sup>8</sup>See also Kennedy et al. (2001) and Raphael and Ludwig (2003) for a related literature on strategies known as “pulling levers.” Broadly speaking, these strategies intensify police-citizen interactions, and are

2019). The aggregate analysis suggests crime decreases by a small but precise amount, and that most likely benefits diffuse. Studies in the context of Latin America, however, point to the importance of local crime patterns. For instance, two hot spots policing experiments in Colombia show different results. Blattman et al. (2021b) find that hot spots policing led to adverse spillovers of property crime and beneficial spillovers of violent crime in Bogotá. Collazos et al. (2020) find that patrolling hot spots led to beneficial spillovers on property crimes in Medellín. Di Tella and Schargrofsky (2004) study a police re-deployment in Buenos Aires following a terrorist attack. Their results suggest motor vehicle thefts declined in places that received additional police. Re-analyzing their data, Donohue et al. (2013) find that crime displaced to the surroundings.

Third, this paper contributes to the literature on the effects of police manpower on crime.<sup>9</sup> This literature generally points to negative elasticities of crime with respect to the number of available police patrols in a city. Notable studies exploiting quasiexperimental variation in city-level police manpower in the US include Levitt (2002), Evans and Owens (2007), and Lin (2009).

Fourth, this paper relates to a growing literature focusing in how the presence of organized crime might shape the effects of intensifying police and other state resources. For instance, Magaloni et al. (2020) find that police crackdowns in Rio lead to more violence in areas heavily controlled by gangs. Also, Dell (2015) finds that crackdowns in cartel strongholds in Mexico displace violence to other cities. Finally, Blattman et al. (2021a) find that intensifying state presence in gang territories in Medellín does not crowd criminal rule out, probably due to a strategic response by gangs to state actions.

A final contribution of this paper is to broaden the regional scope of the literature on the relationship between police and crime. The literature on hot spots policing is useful as an example. Of the 65 studies analyzed in the systematic review by Braga et al. (2019), only

---

usually implemented at or nearby crime hot spots. Braga and Weisburd (2012) conduct a systematic review of these studies.

<sup>9</sup>For summaries of this literature, see Chalfin and McCrary (2017) and Levitt and Miles (2006)

four are outside developed economies. Indeed, 60 concentrate in only three countries: the US, the UK and Sweden.

This paper is organized in seven sections including this introduction. Section 2 describes the setting and presents the data. Section 3 describes the empirical strategy. Section 4 presents the baseline results. Section 5 examines the direct effects exploiting within city variations in baseline crime levels. Section 6 examines both direct and indirect effects exploiting variation between cities in the incidence of organized crime. Section 7 discusses policy implications.

## 2 Setting and data

### 2.1 Setting

Colombia has roughly 48 million people. Per capita income is about \$15,000 per year, adjusted for purchasing power parity. The country is divided into 1,102 cities or municipalities. Each city has an elected mayor and council. According to the Colombian Constitution, the head of police affairs in the city is the mayor. This role, however, is limited to broad policy decisions and budgeting for specific investments—such as the purchase of technology or motor vehicles. In practice, citizen security in a city is the responsibility of the National Police. The National Police is independent of all municipalities, and a branch of the Ministry of Defense. The Colombian Police is a relatively well organized and professional force, with roughly 167 thousand people—hence there are about 350 police officers per 100 thousand residents in the country.

Patrolling activities are organized in multiple layers of jurisdictions. Metropolitan or regional police departments are at the top of the hierarchy.<sup>10</sup> These departments are divided into police districts. In turn, each police district is divided into police stations. Stations

---

<sup>10</sup>Metropolitan departments are for large cities and metropolitan areas, covering a limited number of cities. Regional departments cover a larger number of smaller cities.



are divided into CAIs—*Comandos de Acción Inmediata*. Finally, each CAI is divided into police quadrants—which are equivalent to police beats. The size of each quadrant is usually relatively large. In Medellín, the second largest city, each quadrant covers 50 street blocks on average.

Police quadrants are the basis for police patrolling activities. Each police quadrant has six police officers that patrol in pairs, with each pair covering one of three daily shifts. Police patrols plan their patrolling activities in regular meetings at the police station. In these meetings, the station commander and the police agents in charge of the quadrant decide on routes, times and locations to patrol. Police guidelines include instructions to specifically implement different forms of broken windows policing. Normal duties to signal police presence include running background checks; stopping and frisking people; and conducting arrests.<sup>11</sup> Arrests are classified depending on whether there was an arrest warrant for the person, or otherwise it was conducted “in-the-act,” meaning police patrols happened to observe a crime as they were passing through.

## 2.2 Data

The National Police shared data on the universe of reported crimes and arrests for 80 Colombian cities in 2019. These are the largest cities by population, excluding the capital Bogotá and a few others where data was not available.

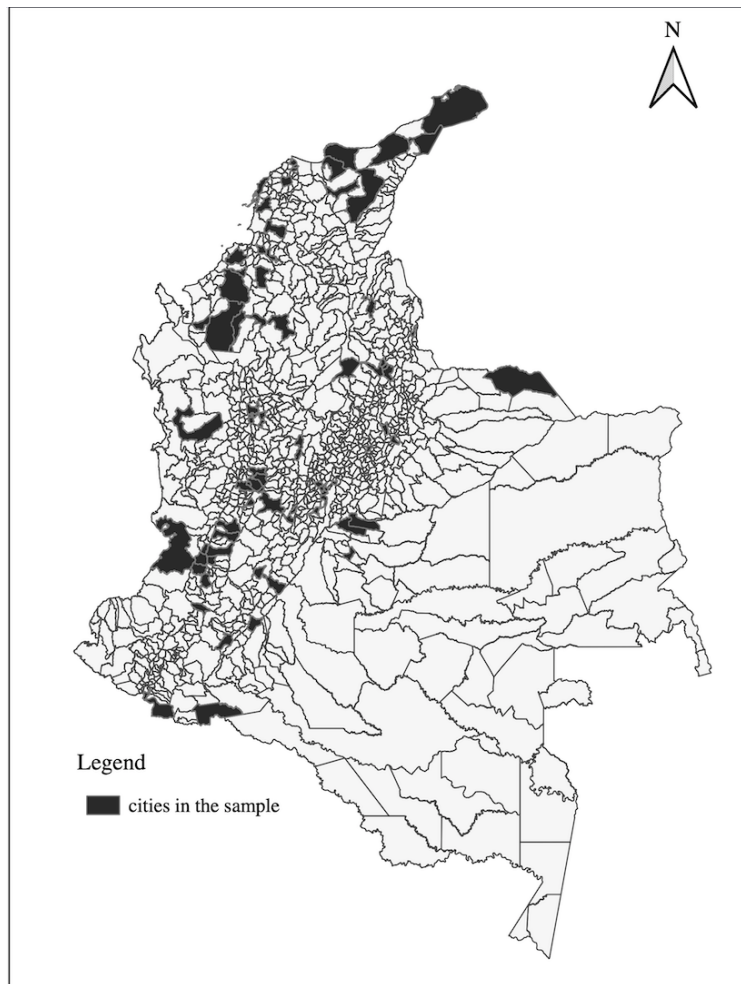
**Cities in the sample** Figure 1 displays the cities in our sample. These are relatively distributed throughout the Colombian territory. The only exception is the southeastern region, where the Amazon rainforest lies along with the relatively inhabited eastern plains.

**Description of cities** Table 1 presents summary statistics on baseline data for the cities in our sample. On average, these cities have a population of roughly 260 thousand residents. This population ranges from 65 thousand to 2.5 million residents. Crime also varies largely

---

<sup>11</sup>See the Police Surveillance Guidelines

Figure 1: Cities in the analytical sample



*Notes:* The map depicts the 80 cities in our analytical sample, in black.

along the distribution of cities. For instance, the average homicide rate per 100 thousand residents is 27 across all cities. The safest city has a homicide rate of 1.6, and the most insecure has a homicide rate of almost 170, probably placing it among the most murderous cities in the world. These cities also differ in terms of the incidence of organized crime. Drug seizure rates, for instance, vary from 18 to roughly 500 per thousand residents, with an average of 141. The average city in the sample has 162 hectares of coca crops, but a majority of cities (70) do not have these illegal crops.

Table 1: Baseline descriptive statistics for the sample of cities

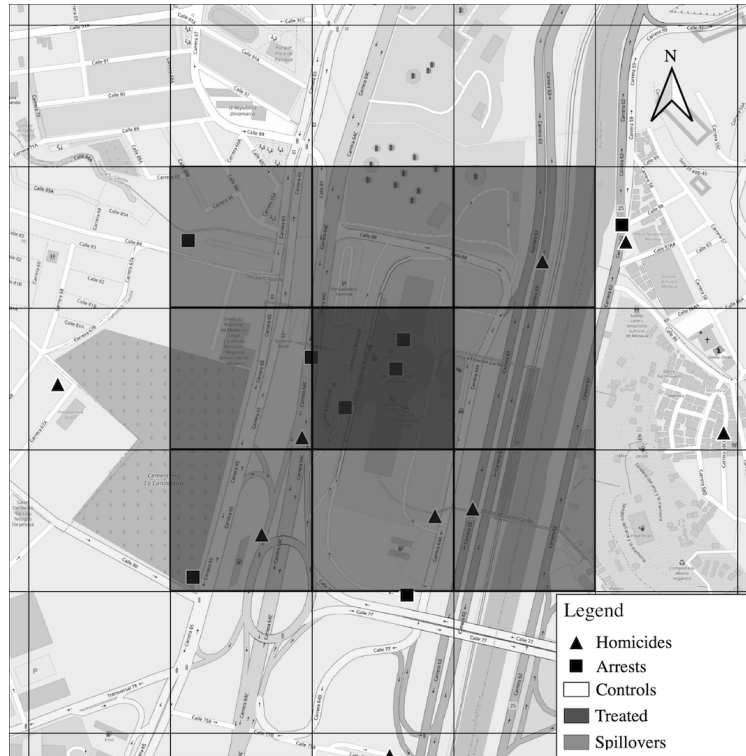
Variable	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)
Population	262,959.9	391,685.7	64,652.0	2,427,129.0
Total crimes rate	485.0	360.5	13.5	1,927.3
Violent crimes rate	123.0	74.8	4.9	341.5
Homicides rate	26.8	23.8	1.6	169.6
Assaults rate	317.8	125.2	22.0	658.2
Property crimes rate	361.9	302.4	6.5	1,625.5
Personal theft rate	410.9	223.5	24.9	977.5
Vehicle theft rate	12.8	14.9	0.0	80.7
Motorcycle theft rate	77.7	59.2	4.9	290.2
Drug seizure rate	140.9	11.3	18.4	498.8
Weapons seizure rate	42.2	24.8	7.1	155.4
Coca presence (ha.)	162.2	840.0	0.0	7,125.3
Number of cities	80			

*Notes:* The table reports descriptive statistics at the city level for the sample of cities for which data is available. Column (1) reports means, column (2) reports standard deviations, column (3) reports minimum values, and column (4) reports maximum values.

**Crime and arrest records** Criminal records include the exact location coordinates, the type of crime, and the time and date. Arrest records include the exact location coordinates, an indicator for whether the arrest followed a warrant or was conducted in-the-act, and the time and date. These data are from SIEDCO—*Sistema de Información Estadístico, Delincuencial, Contravencional y Operativo*. While some coordinates may be imprecise, in urban areas of large cities the extent of imprecision is relatively minor, as most reporting stations have interactive maps to help citizens locate their crime reports. Moreover, while under-reporting is a real concern, the research design accounts for idiosyncratic characteristics of places, which should partly mitigate the problem.

**Setup and data structure** To study the effects of shocks in police activities, we divide the urban area of all 80 cities into grids of  $200 \times 200$  meters. We build a panel data for which the time units are periods of two weeks and the cross-sectional units are these grids. We use the location coordinates of crimes and arrests to assign each of them to only one grid across

Figure 2: Illustration of treatment and spillover grids



*Notes:* The figure illustrates how grids are exposed to the direct treatment, spillover or control conditions in a given period.

all cities.

**Treatment conditions** We define a shock in broken windows policing as any period of time when three or more arrests were conducted in-the-act in a given grid. Two or fewer arrests are relatively common, and we worried this looser definition of the treatment would not pick up an actual intensification of police activities—i.e., citizens may fail to interpret more common events as actual intensification of police presence. We also define a spillover treatment. Any grid with a shared border—including the corners—with a treated grid is exposed to the spillover condition. For simplicity, we only exploit the extensive margin of both treatments. Finally, grids that are not exposed to either direct treatment or spillovers at a given period are controls. Figure 2 illustrates how grids are exposed to these treatment conditions.

**Outcomes** We use additive crime indexes for total crime, violent crime and property crime. The index for violent crime is the sum of homicides and assaults. The index for property crimes is the sum of personal thefts and motor vehicle thefts. The index for total crime consists of the sum of violent and property crimes. We do not include other types of crimes such as sexual assault or burglary as these are largely under-reported and happen mostly within houses and businesses.<sup>12</sup> We focus on these outcomes in the periods following the shock in broken windows policing.

**Descriptive statistics** Table 2 reports descriptive statistics at the grid-period level for the main analytical sample: grids that were exposed to either direct or spillover effects at any given period over 2019. Panel A presents data on the exposition to both treatments. Direct treatment occurs for roughly 0.8% of grids and periods. As expected, the spillover treatment occurs more frequently, for about 6.2% of grids and periods.<sup>13</sup> Panel B presents data on the main outcomes. On average, a total of 0.44 crimes occurred at any given grid and period. Most of these are property crimes. The average grid and period had 0.36 property crimes and 0.09 violent crimes.

### 3 Empirical strategy

We study the effects of broken windows policing on crime by following a difference-in-differences approach with differential treatment timing, including leads and lags to observe pre- and post-trends.<sup>14</sup> In particular, we use ordinary least squares to estimate equation (1):

$$y_{g,c,t} = \sum_{-3 < k < 4, k \neq 1} \beta_k B_{g,t-k} + \sum_{-3 < k < 4} \alpha_k S_{g,t-k} + \gamma_g + \eta_c \times \delta_t + \varepsilon_{g,c,t} \quad (1)$$

---

<sup>12</sup>See Blattman et al. (2021b), who conducted a large survey on crime reporting and other outcomes in Bogotá in 2016.

<sup>13</sup>Note for each grid directly treated there are eight grids exposed to spillovers. In some cases, however, one grid is exposed to spillovers resulting from more than one neighbor being treated.

<sup>14</sup>See for instance Gómez et al. (2021) for a similar empirical application using grids of 70 × 70 meters over Medellín to study the effects of surveillance cameras on crime.

Table 2: Descriptive statistics at the grid-period level

Variable	Mean (1)	Std. Dev. (2)	Min. (3)	Max. (4)
<i>Panel A: Exposition to treatments</i>				
Treatment	0.008	0.091	0	1
Spillovers	0.062	0.241	0	1
<i>Panel B: Outcomes</i>				
Total crime	0.444	1.212	0	89
Violent crime	0.088	0.368	0	12
Homicides	0.006	0.084	0	4
Assaults	0.081	0.354	0	12
Property crimes	0.366	1.122	0	89
Personal theft	0.320	1.091	0	89
Car theft	0.006	0.079	0	3
Motorcycle theft	0.029	0.180	0	6
Number of grids	338,208			

*Notes:* The table reports descriptive statistics at the grid-period level for the sample of 338,208 grids. Column (1) reports means, column (2) reports standard deviations, column (3) reports minimum values, and column (4) reports maximum values.

where  $g$  indexes the grids of  $200 \times 200$  meters,  $c$  indexes the city and  $t$  indexes the time periods.  $y_{g,c,t}$  is the relevant outcome at grid  $g$ , city  $c$  and period  $t$ .  $B_{g,t-k}$  is an indicator for whether grid  $g$  at city  $c$  had a broken windows policing shock at time  $t - k$ . The parameters  $\beta_k$  for  $k = -3, -2, \dots, 4$  with  $k \neq 1$  measure the direct impact of the broken windows policing shock at each period  $k$ , relative to the number of crimes in the period prior to the shock. Furthermore, the parameters  $\alpha_k$  for  $k = -3, -2, \dots, 4$  measure the spillover impact of the broken windows policing shock at each period  $k$ , relative to the number of crimes at grids directly exposed to the shock in the period prior to the shock.  $\gamma_g$  are fixed effects at the grid level, and control for unobserved idiosyncratic characteristics of the locations that are invariant over time.  $\eta_c \times \delta_t$  control for unobserved temporal shocks fixed for all grids, which we allow to vary across cities. Finally,  $\varepsilon_{g,c,t}$  is an error term.

This is a relatively standard event study with one difference. In our context, grids can be treated more than once over the study period. Hence we force early leads and late lags

into the  $\alpha_{-3}$  and  $\beta_{-3}$  coefficients for direct and spillover effects, respectively.

**Inference** Assuming that treatment can produce spatial spillovers creates a problem for inference. When one grid is exposed to the direct treatment condition, all the grids around it are exposed to the spillover condition as a cluster. With many grids being treated across each city, this creates a clustering structure that is hard to conform with a standard geographical unit such as a neighborhood. This is a problem of fuzzy clustering (Abadie et al., 2017).<sup>15</sup> To address this problem, we perform two estimations of standard errors and p-values. On the one hand, we use the standard approach in panel data and cluster standard errors at the grid level. On the other hand, we use randomization inference to estimate exact p-values under the sharp null hypothesis of no effect for any unit. While this is more standard in experiments, where treatment can be randomly reassigned across experimental units, we implement it in our context by randomly switching the places that are exposed to the direct treatment and spillover conditions.<sup>16</sup> We estimate treatment effects under 1,000 different random scenarios and estimate a p-value comparing our observed effects with the empirical distribution of treatment effects. This procedure is agnostic of the structure of the errors.

## 4 Baseline results

### 4.1 Direct and spillover effects

Table 3 presents the baseline results from our estimation of equation (1), and Figure 3 plots the conventional figure for event study estimates. In Table 3, the relevant independent variables are in panels A and B. Column (1) reports the coefficients for total crime. This is an additive index of all homicides, assaults, personal and motor vehicle thefts at a given grid and period. Columns (3) and (4) report standard errors clustered at the grid level and

---

<sup>15</sup>This problem is present in similar crime studies. See for instance Blattman et al. (2021b), Collazos et al. (2020) and Gómez et al. (2021)

<sup>16</sup>We randomly change treatment assignment and then assign the spillover condition to the grids that have a common border with a treatment grid.

randomization inference p-values, respectively.

Panel A reports results for direct effects. As expected, we observe a large increase in total crime at the period of the shock. Each arrest necessarily implies at least one crime report (or more, when offenders commit several crimes simultaneously). From this period onward, however, crimes decrease monotonically. Four periods after the shock, the effects are statistically significant at conventional levels when we cluster standard errors at the grid level. Randomization inference p-values suggest the coefficients are rather imprecise, however. The coefficient in the fourth period is  $-0.067$ , equivalent to a 6% decrease in crimes relative to the average number of crimes at treated grids in the period before the shock.

Panel B reports results for spillover effects. We do not observe a statistically significant increase in crimes at the period of the shock. This suggests crimes were localized where police happened to be present, with no concurrent beneficial or adverse spillovers. In the periods to follow, we also observe a decline in reported crimes in areas nearby the treated grid. The effects are statistically significant at conventional levels three and four periods after the shock, when we cluster standard errors at the grid level. As for the direct effects, randomization inference p-values suggest these coefficients might be imprecisely estimated, though in the fourth period the p-value is below 0.05. The coefficients in the third and fourth period are  $-0.033$  and  $-0.052$ , respectively. Relative to the average number of crimes at treated grids in the period before the shock, these effects are equivalent to a 3% decline in period three and a 5% decline in period four. These are beneficial spillovers resulting from broken windows policing activities. Moreover, the number of grids exposed to spillovers is substantially larger than grids exposed to direct treatment. Hence, the spillover results suggest the relative imprecision of the estimates for direct effects may be due to statistical power rather than to the absence of effects.

Furthermore, the results suggest that crime pre-trends were similar before the shock, both for grids directly exposed to broken windows policing activities and grids exposed to spillovers. Not only the coefficients are not statistically significant individually, but they are

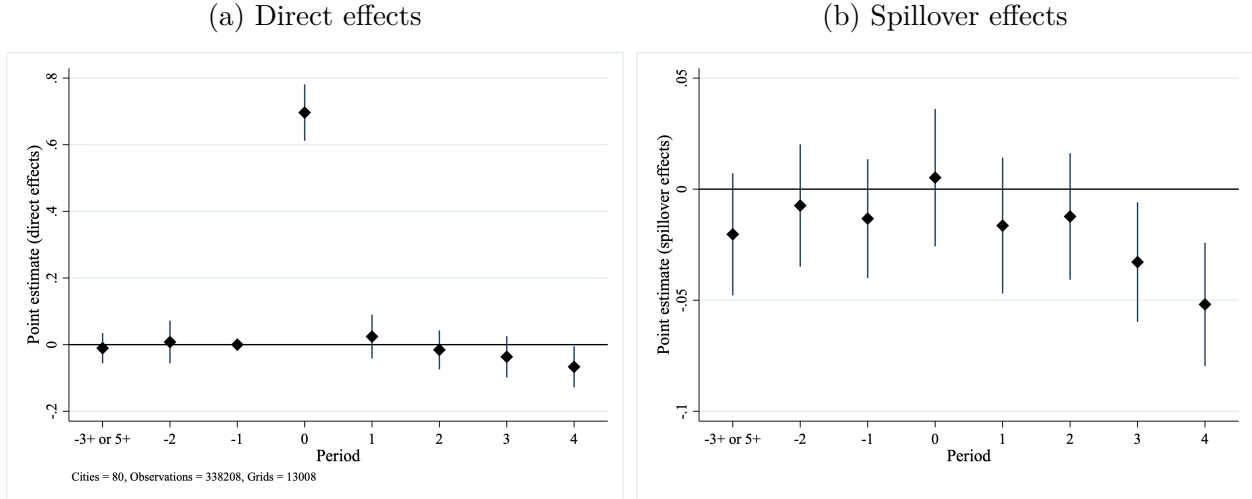


Table 3: Direct and spillover effects of broken windows policing on crime

	Coefficient (1)	Standard error (2)	RI p-value (3)
<i>Panel A: Direct effects</i>			
Period -3+ or 5+	-0.011	[0.028]	(0.924)
Period -2	0.008	[0.039]	(0.763)
Period -1	-	-	-
Period 0	0.697	[0.051]***	(0.000)***
Period 1	0.024	[0.040]	(0.741)
Period 2	-0.016	[0.036]	(0.799)
Period 3	-0.036	[0.038]	(0.601)
Period 4	-0.067	[0.038]*	(0.197)
<i>Panel B: Spillover effects</i>			
Period -3+ or 5+	-0.020	[0.017]	(0.446)
Period -2	-0.007	[0.017]	(0.964)
Period -1	-0.013	[0.016]	(0.783)
Period 0	0.005	[0.018]	(0.854)
Period 1	-0.016	[0.019]	(0.630)
Period 2	-0.012	[0.017]	(0.647)
Period 3	-0.033	[0.016]**	(0.337)
Period 4	-0.052	[0.017]***	(0.042)**
<i>Panel C: Tests for pre-trends</i>			
Direct effects: Prob >F		0.795	
Spillover effects: Prob >F		0.426	
Outcome mean in period -1		1.065	
Observations		338,208	
Number of grids		13,008	
Number of cities		80	
R-squared		0.414	
Grid fixed effects		Yes	
Period $\times$ city fixed effects		Yes	

*Notes:* The table reports baseline results for the estimation of equation (1). Column (1) reports coefficients, column (2) reports standard errors clustered at the grid level, and column (3) reports randomization inference p-values. We force early leads and late lags into the coefficients denoted as ‘Period -3+ or 5+’. \*\*\* $p < 0.01$ ; \*\* $p < 0.05$ ; \* $p < 0.10$ .

Figure 3: Direct and spillover effects for the baseline model



*Notes:* The figure plots the results estimating of equation (1) for total crimes. The lines denote confidence intervals at the 90% level. We force early leads and late lags into the period denoted as ‘-3+ or 5+’.

also not jointly statistically significant, as reported in Panel C.

## 4.2 Aggregate effects

Our baseline results suggest the benefits of the shock in broken windows policing materialize several periods after the shock, both in places exposed to treatment and the surroundings. Hence, as Blattman et al. (2021b) point out, the coefficients are important but also relevant is the number of places exposed to each condition.

In this section, we conduct a back-of-the-envelope estimation of aggregate effects. To do so, we multiply each estimated treatment effect by the number of grids exposed to each condition. Table 4 presents the results. In the shock period, we estimate there is an aggregate increase of 1,986 crimes in the grids where police patrols conducted the arrests. After the shock, the aggregate decrease in places exposed to direct treatment and spillover effects adds up to 2,503 crimes. Hence we estimate that these policing activities led to a net decrease of roughly 517 crimes. We use the 1,000 simulations of treatment effects to produce a p-value

of the aggregate net change in crimes. We find that our estimate for the net decrease is imprecise, but borderline significant at conventional levels.

Table 4: Aggregate effects of broken windows policing on crime

	Coefficient (1)	# grids (2)	Total = (1) $\times$ (2) (3)
<i>Panel A: Treatment effects, shock</i>			
<i>Direct effects</i>			
Period 0	0.697	2,849	1,985.8
<i>Panel B: Treatment effects, post-shock</i>			
<i>Direct effects</i>			
Period 1	0.024	2,766	6.4
Period 2	-0.016	2,713	-43.4
Period 3	-0.036	2,680	-96.5
Period 4	-0.067	2,651	-177.6
<i>Spillover effects</i>			
Period 1	-0.016	20,572	-329.2
Period 2	-0.012	20,274	-243.3
Period 3	-0.033	19,815	-653.9
Period 4	-0.052	19,718	-1,025.3
Net decrease in crime			-517.0
RI p-value			0.110

*Notes:* The table reports aggregate effects of preventive policing practices. Column (1) reports coefficients, column (2) reports the number of grids exposed to each condition, and column (3) reports the aggregate effect. We estimate RI p-values by simulating 1,000 different random scenarios and comparing our estimated aggregate effect with the empirical distribution of aggregate effects.

### 4.3 Effects by type of crime

We now turn to examine whether the effects of broken windows policing are different for violent and property crimes. Figure 4 plots the event study estimates. Subfigures 4a and 4b report results for violent crimes. This is an additive index of all homicides and assaults at a given grid and period. Violent crimes are less common, hence entail fewer variation. The direct effects generally point to a decrease in crimes in the periods following the broken windows policing shock. As with the total crime index, the coefficient turns significant at

conventional levels in the fourth period. Spillover effects are far less precise, though the estimates seem to follow a decreasing pattern.

Subfigures 4c and 4d report results for property crimes. This is an additive index of all personal and motor vehicle thefts at a given grid and period. The direct effects also point to a decrease in crimes in the periods following the broken windows policing shock, though this decrease is imprecisely estimated. Spillover effects are more precise. The coefficients in the periods following the shock are all negative. In periods three and four, the estimates are statistically significant at conventional levels.

#### 4.4 Robustness

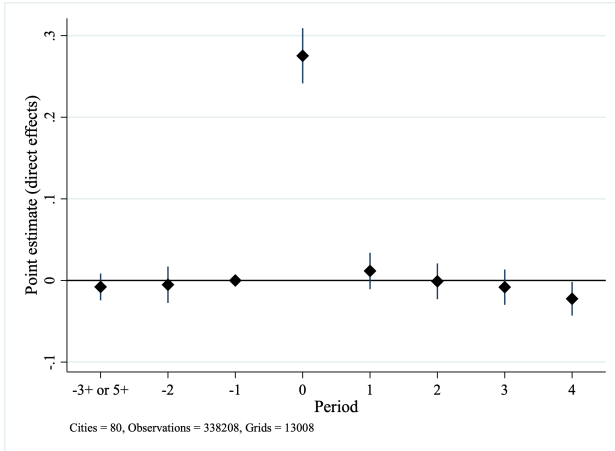
**Sample of grids** In our baseline analysis, we restrict the sample to grids that were exposed to either direct or spillover effects at any period during 2019. We believe this approach produces a relatively good comparison group: we directly compare average crimes in grids exposed to some treatment with average crimes in grids that were not exposed to any treatment in that same period, but that were exposed at other period or periods. Figure 5 plots the event studies for direct and spillover effects when we include the universe of 73,282 grids covering the urban perimeter of all 80 cities in our sample. The results are similar in both direction and precision to our baseline estimates.

**Treatment definition** In our baseline, preferred specification, we define a shock in broken windows policing as any period of time where three or more arrests were conducted in-the-act in a given grid. As we mention above, two or fewer arrests are relatively common, and we worried that this looser definition would not pick up an actual intensification of police activities. Figure 6 presents the results when we define treatment as any period of time when two or more arrests were conducted in-the-act. As we anticipated, this definition of our treatment leads to more imprecise estimates. The coefficients for direct effects fall monotonically, but become negative only in the fourth period. This coefficient, however, is

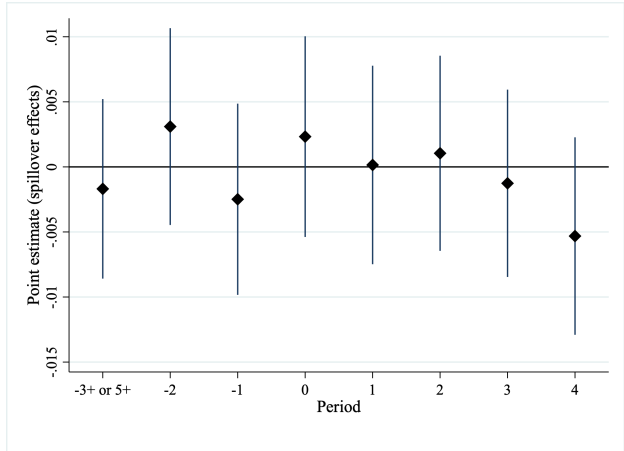
Figure 4: Direct and spillover effects for violent crime and property crime

Violent crime

(a) Direct effects

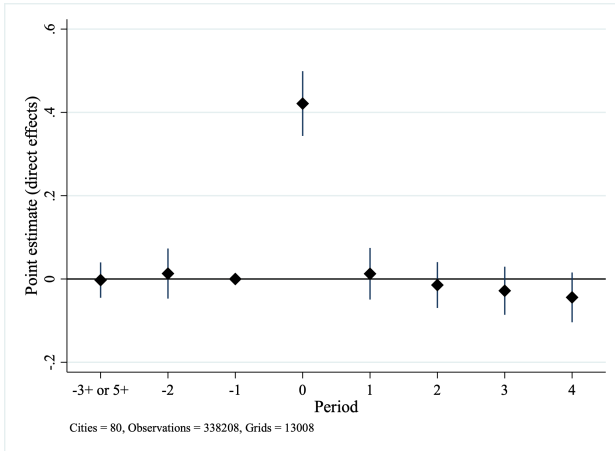


(b) Spillover effects

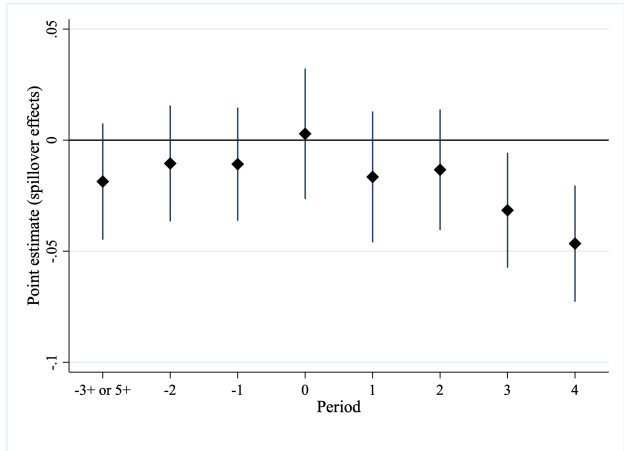


Property crime

(c) Direct effects



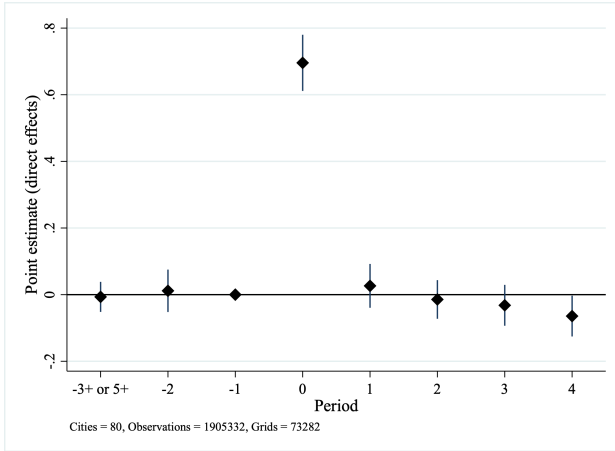
(d) Spillover effects



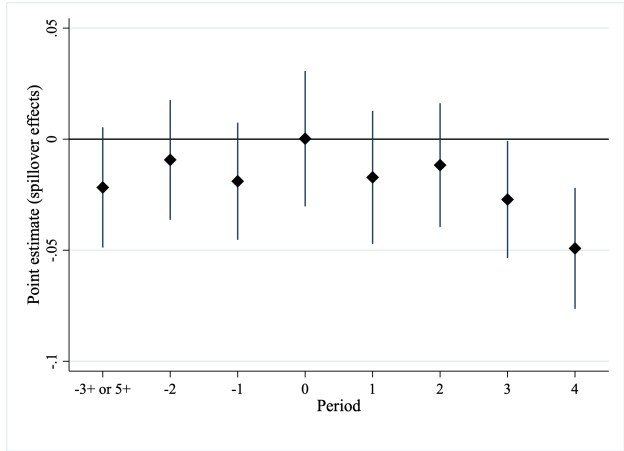
*Notes:* The figure plots the results estimating of equation (1) for violent and property crimes, separately. Subfigures 4a and 4b report results for violent crimes, and subfigures 4c and 4d report results for property crimes. The lines denote confidence intervals at the 90% level. We force early leads and late lags into the period denoted as ‘-3+ or 5+’.

Figure 5: Direct and spillover effects for the universe of grids

(a) Direct effects



(b) Spillover effects



*Notes:* The figure plots the results estimating of equation (1) for total crimes. The lines denote confidence intervals at the 90% level. We force early leads and late lags into the period denoted as ‘-3+ or 5+’. We include all grids in our sample.

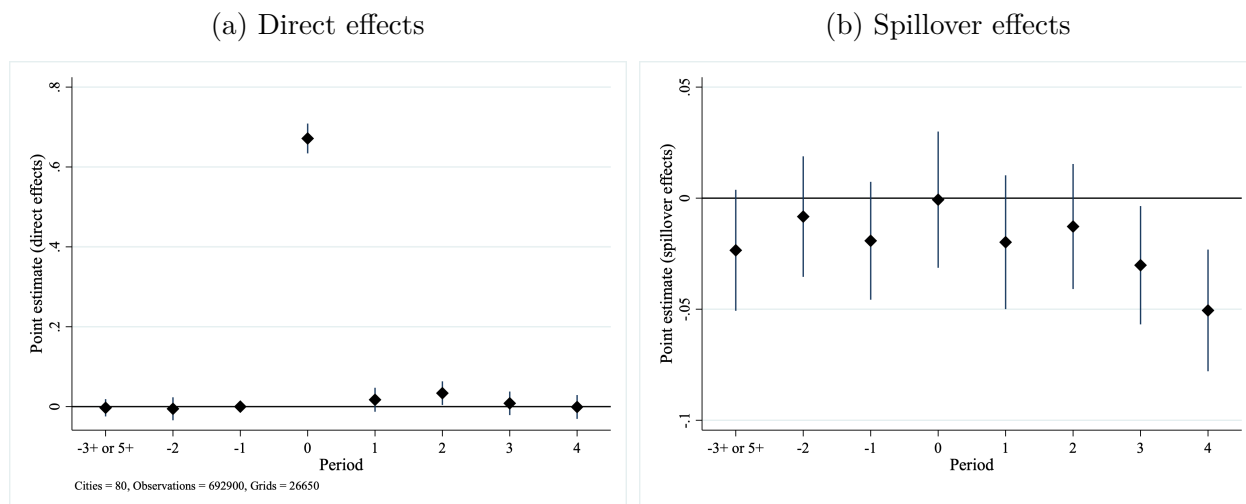
imprecisely estimated. Spillover effects behave broadly similar to our main estimates.

## 5 Within-city heterogeneity: Crime hot spots

We turn our attention to the heterogeneity of treatment effects depending on baseline characteristics of the grids. Most of the literature on policing activities focus on strategies such as hot spots policing, where police patrols are disproportionately concentrated in crime hot spots. This analysis broadens this scope, looking at effects in places with low or moderate levels of criminal activity. We estimate equation (1) but interact the vector of treatment effects with a dummy variable for being in the 100<sup>th</sup> percentile of total crimes according to the baseline crime data. Subfigures 7a and 7b report estimates for low crime grids, and subfigures 7c and 7d report estimates for high crime grids. We use baseline crime levels for two months of 2018.

Broadly speaking, the direct effects of shocks in police activity seem to be more immediate and precise in low-crime areas. Crimes in high-crime areas also seem to fall by period

Figure 6: Direct and spillover effects under a different definition of treatment effects



*Notes:* The figure plots the results estimating of equation (1) for total crimes, using an alternative definition for treatment. We define a shock in broken windows policing as any period of time when two or more arrests were conducted in-the-act in a given grid. The lines denote confidence intervals at the 90% level. We force early leads and late lags into the period denoted as ‘-3+ or 5+’.

four after the shock, but the corresponding coefficient is imprecisely estimated. Beneficial spillovers are larger and more precise in high-crime grids, as earlier findings from the hot spots policing literature suggest (Braga et al., 2019).

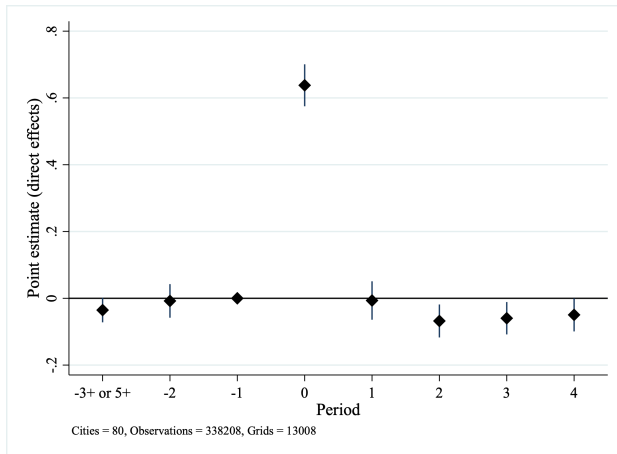
## 6 Between-city heterogeneity: Organized crime

In this section, we exploit the wide between-city heterogeneity in the local criminal environment, focusing on proxies for the presence of organized crime. Absent complete data of criminal organizations across the country, we use three alternative proxies: illegal drug seizure rates, illegal weapon seizure rates, and presence of coca crops. Criminal organizations typically engage in illegal drug markets and use illegal weapons to enforce their territorial control, hence our first two proxies. Furthermore, criminal organizations are usually present in cities with coca crops. Their presence serves many purposes, from providing protection to the crops to controlling the supply of coca leaves for the production of cocaine. For the

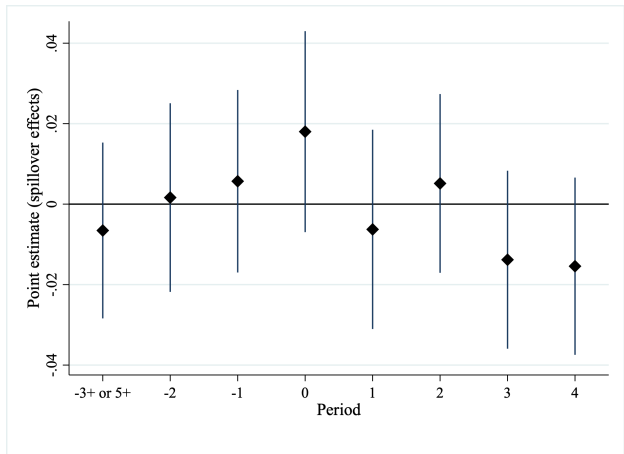
Figure 7: Direct and spillover effects in low- and high-crime grids

Low-crime grids

(a) Direct effects

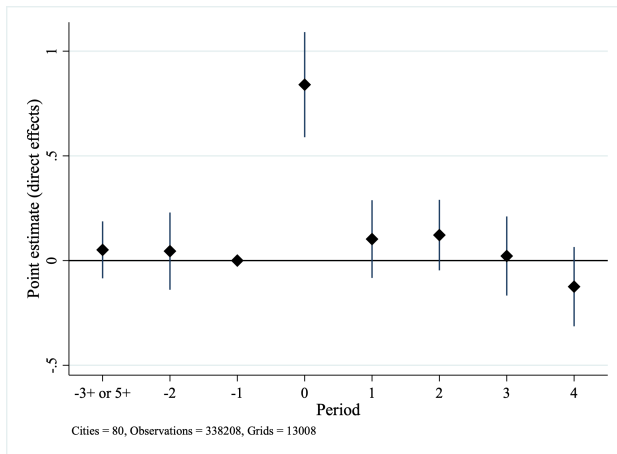


(b) Spillover effects

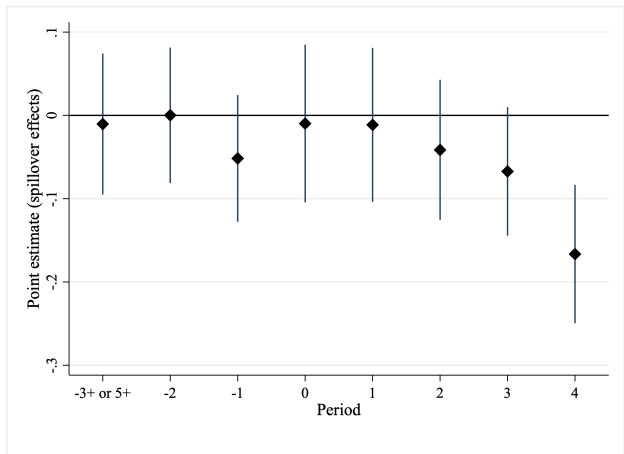


High-crime grids

(c) Direct effects



(d) Spillover effects



*Notes:* The figure plots the results estimating of equation (1) for low- and high-crime grids, separately. Subfigures 7a and 7b report results for low-crime grids, and subfigures 7c and 7d report results for high crime grids. The lines denote confidence intervals at the 90% level. We force early leads and late lags into the period denoted as ‘-3+ or 5+’.



first two proxies, we split the sample of cities at the median. For the third proxy, we split the sample in cities without or with coca crops.

**Drug seizures** Figure 8 report the results. Subfigures 8a and 8b present direct and spillover effects for the sub-sample of cities below the median level of the illegal drug seizure rates. Subfigures 8c and 8d report direct and spillover effects for cities above the median. Broadly, the results suggest the direct effects are similar in both sub-samples, but we only observe evidence of beneficial spillovers in cities with low or moderate levels of organized crime.

**Weapon seizures** Figure 9 report the results. Subfigures 9a and 9b present direct and spillover effects for the sub-sample of cities below the median level of the illegal weapon seizure rates. Subfigures 9c and 9d report direct and spillover effects for cities above the median. We observe direct treatment effects only for the sub-sample of cities below the median. Similarly, we only observe beneficial spillovers in cities with low or moderate levels of organized crime.

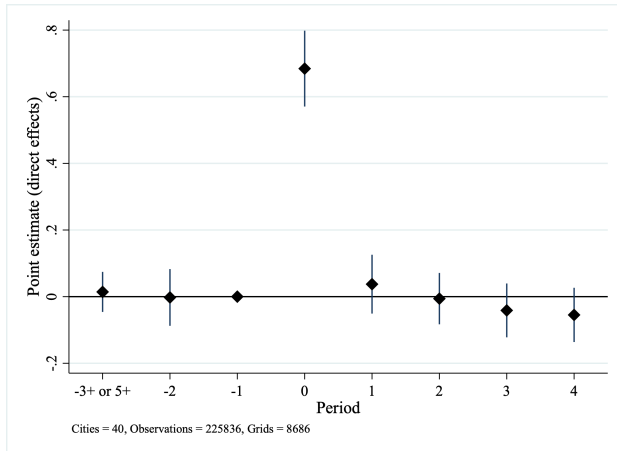
**Coca presence** Figure 10 report the results. Subfigures 10a and 10b present direct and spillover effects for the sub-sample of cities with no presence of coca crops. Subfigures 10c and 10d report direct and spillover effects for cities with presence of coca crops. Most of the cities in our sample have no presence of coca crops (70). We observe direct and spillover effects only for the sub-sample of cities with no coca presence. While we acknowledge that the smaller number of cities in the sub-sample of cities with coca implies more limited statistical power, the coefficients are either positive or close to zero in periods three and four for direct effects, and positive for spillover effects across all periods.

Taken together, these results suggest the effects of broken windows policing are rather circumscribed to cities with low or moderate presence of organized crime. We avoid testing whether the difference in governance treatment effects between the two subgroups is statis-

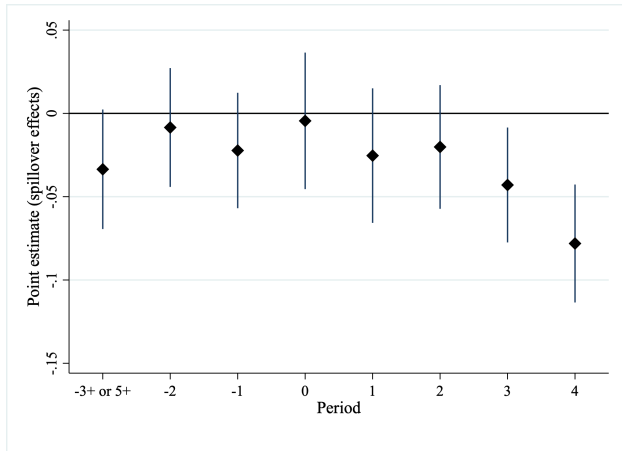
Figure 8: Direct and spillover effects for sub-samples based on the median of the illegal drug seizures rate

Below median

(a) Direct effects

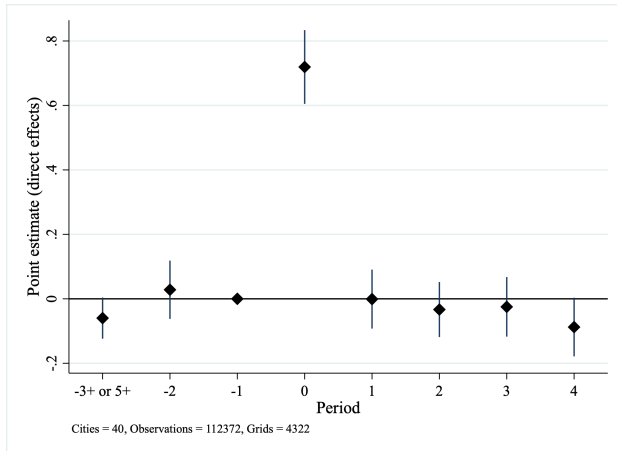


(b) Spillover effects

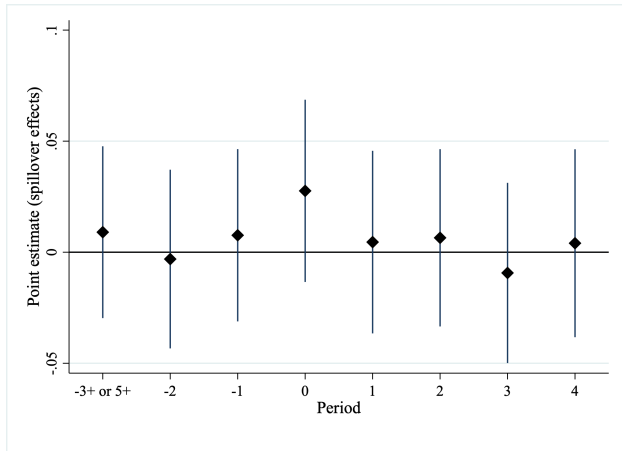


Above median

(c) Direct effects



(d) Spillover effects

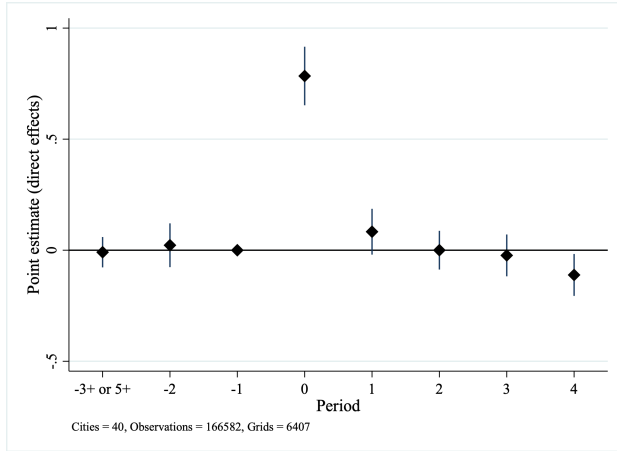


*Notes:* The figure plots the results estimating of equation (1) for two sub-samples of cities: Those below the median level of the illegal drug seizure rates, and those above the median level. Subfigures 8a and 8b report results for cities below the median, and subfigures 8c and 8d report results for cities above the median. The lines denote confidence intervals at the 90% level. We force early leads and late lags into the period denoted as ‘-3+ or 5+’.

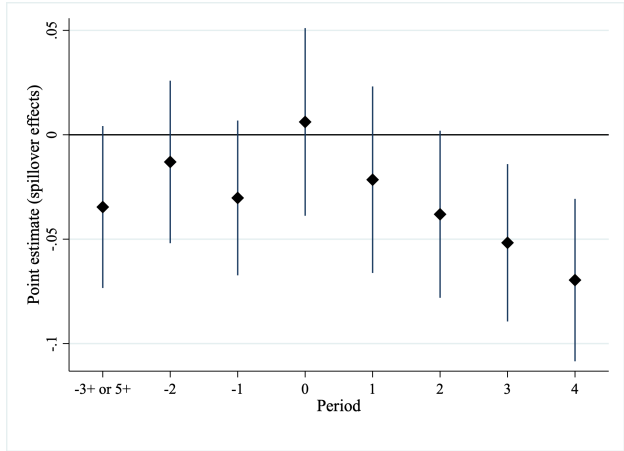
Figure 9: Direct and spillover effects for sub-samples based on the median of the illegal weapon seizures rate

Below median

(a) Direct effects

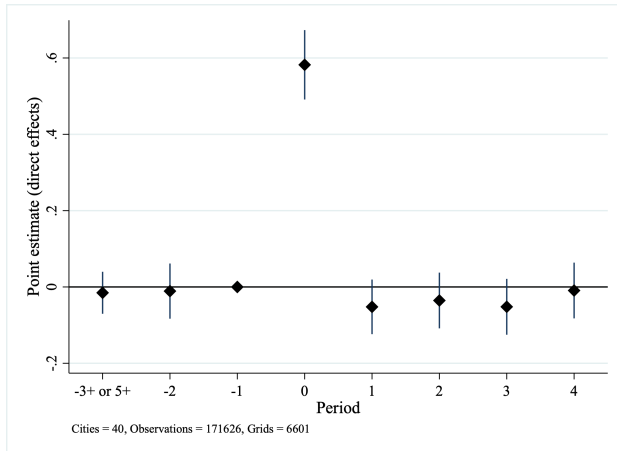


(b) Spillover effects

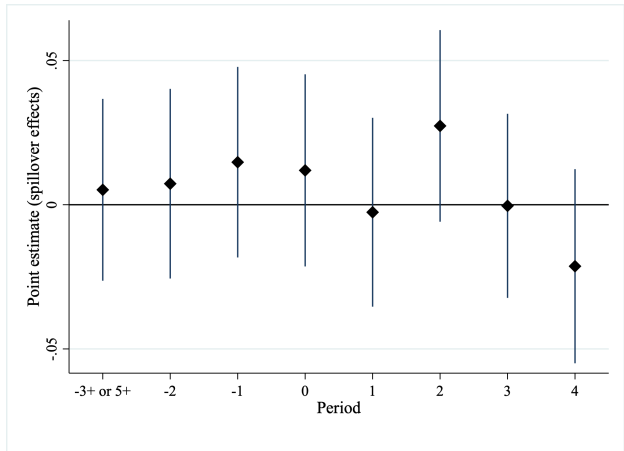


Above median

(c) Direct effects



(d) Spillover effects

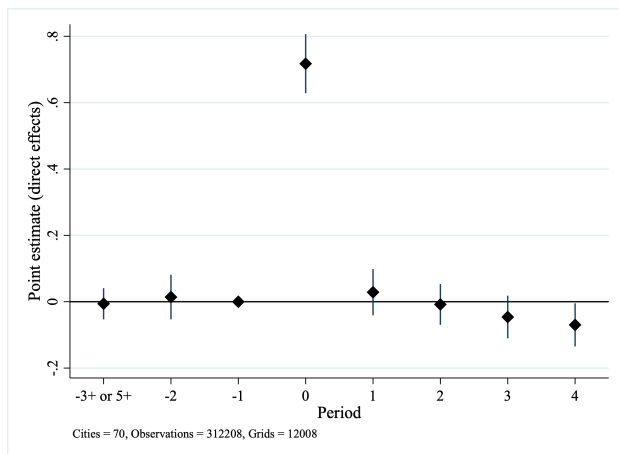


*Notes:* The figure plots the results estimating of equation (1) for two sub-samples of cities: Those below the median level of the illegal weapon seizure rates, and those above the median level. Subfigures 9a and 9b report results for cities below the median, and subfigures 9c and 9d report results for cities above the median. The lines denote confidence intervals at the 90% level. We force early leads and late lags into the period denoted as ‘-3+ or 5+’.

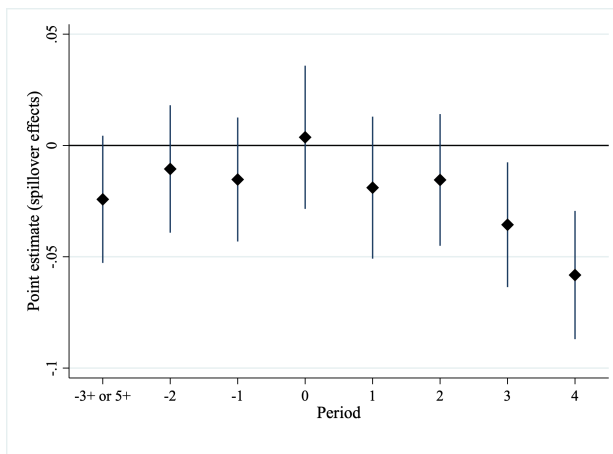
Figure 10: Direct and spillover effects for sub-samples based on the presence of coca crops

No presence

(a) Direct effects

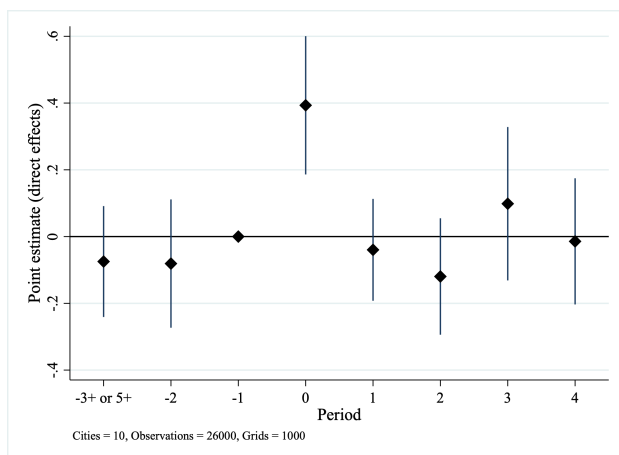


(b) Spillover effects

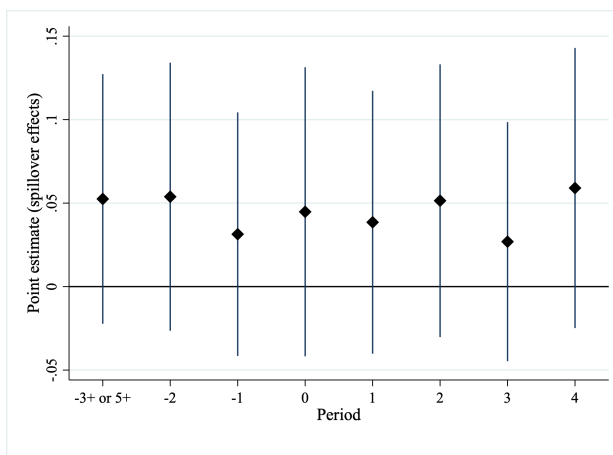


Presence

(c) Direct effects



(d) Spillover effects



*Notes:* The figure plots the results estimating of equation (1) for two sub-samples of cities: Those with no presence of coca crops, and those with any presence of coca crops. Subfigures 10a and 10b report results for cities below with no presence, and subfigures 10c and 10d report results for cities with presence. The lines denote confidence intervals at the 90% level. We force early leads and late lags into the period denoted as ‘-3+ or 5+’.

tically significant—as we are already testing too many coefficients at once, but overall, this result is consistent with criminal organizations planning their activities more systematically relative to disorganized criminals (e.g., Blattman et al., 2021a).

## 7 Discussion

Crime is a major problem in many parts of the world. Thousands of cities struggle with high violent and property crime rates. In Latin America, for instance, the aggregate homicide rate has remained virtually unchanged for at least three decades—at relatively high levels. Police forces are perhaps the primary policy tool that governments use to control crime. Yet most research in policing strategies concentrates in parts of the world where crime rates are lower, the criminal environment is less diverse and complex, and institutional capacity is stronger.

Our work suggests a few insights. First, on average, broken windows policing strategies seem to work in the Colombian context. Not only do crimes decrease, but also the benefits in crime reductions spill over to neighboring areas. The direct effects are important for both violent and property crimes, though there seem to be differences in the diffusion patterns. In light of the very few studies on broken windows policing overall, and what we believe to be the non-existent knowledge base produced in Latin America, these results are promising policywise, both for the region and regions other than the developed world.

Second, these strategies work differently in low- vs high-crime areas. Perhaps as expected, broken windows policing strategies have a more immediate and precise impact in low-crime areas. Reducing crimes in high crime hot spots seem to be out of reach for simple patrolling strategies such as broken windows policing. Other deployment tactics, such as hot spots policing, may be a better fit for high crime places. Hence we see our work as a complement to the growing literature on hot spots policing in the region (e.g., Collazos et al., 2020; Blattman et al., 2021b).

Third, somewhat obviously, the complexity of the criminal environment matters for the effects of broken windows policing strategies. Criminal organizations usually plan their activities beforehand and respond strategically to state efforts. Latin America is a region plagued with criminal organizations controlling retail drug markets, collecting extortion, and regulating other forms of crimes such as theft and homicides. Our results are consistent with this idea, as we only see effects in places with low to moderate organized crime (e.g., Blattman et al., 2021a; Dell, 2015; Magaloni et al., 2020). These results suggest policing strategies should follow the local criminal context. A standard approach is probably insufficient to tackle more complex criminal responses.

## References

- Abadie, A., Athey, S., Imbens, G. W., and Wooldridge, J. (2017). When should you adjust standard errors for clustering? Technical report, National Bureau of Economic Research.
- Becker, G. S. (1968). Crime and punishment: An economic approach. *Journal of Political Economy*, 76(2):169–217.
- Blattman, C., Duncan, G., Lessing, B., and Tobón, S. (2021a). Gang rule: Understanding and countering criminal governance. *Unpublished manuscript*.
- Blattman, C., Green, D., Ortega, D., and Tobon, S. (2021b). State building in the city: The effects of public security and services on crime, violence and state legitimacy in Bogotá. *Journal of the European Economic Association (forthcoming)*.
- Braga, A. A., Turchan, B., Papachristos, A. V., and Hureau, D. M. (2019). Hot spots policing of small geographic areas effects on crime. *Campbell Systematic Reviews*, 15(3):e1046.
- Braga, A. A. and Weisburd, D. L. (2012). The effects of “pulling levers” focused deterrence strategies on crime. *Campbell systematic reviews*, 8(1):1–90.

- Chalfin, A. and McCrary, J. (2017). Criminal deterrence: A review of the literature. *Journal of Economic Literature*, 55(1):5–48.
- Collazos, D., García, E., Mejía, D., Ortega, D., and Tobón, S. (2020). Hot spots policing in a high-crime environment: An experimental evaluation in medellin. *Journal of Experimental Criminology*, pages 1–34.
- Dell, M. (2015). Trafficking networks and the mexican drug war. *American Economic Review*, 105(6):1738–79.
- Di Tella, R. and Schargrotsky, E. (2004). Do police reduce crime? estimates using the allocation of police forces after a terrorist attack. *American Economic Review*, 94(1):115–133.
- Donohue, J. J., Ho, D., and Leahy, P. (2013). Do police reduce crime? a reexamination of a natural experiment. *Empirical Legal Analysis: Assessing the Performance of Legal Institutions*, pages 125–143.
- Ehrlich, I. (1973). Participation in illegitimate activities: A theoretical and empirical investigation. *Journal of Political Economy*, 81(3):521–565.
- Evans, W. N. and Owens, E. G. (2007). Cops and crime. *Journal of Public Economics*, 91(1-2):181–201.
- Gómez, S., Mejía, D., and Tobón, S. (2021). The deterrent effect of surveillance cameras on crime. *Journal of Policy Analysis and Management*, 40(2):553–571.
- Harcourt, B. E. and Ludwig, J. (2006). Broken windows: New evidence from new york city and a five-city social experiment. *The University of Chicago Law Review*, pages 271–320.
- Jacob, B., Lefgren, L., and Moretti, E. (2007). The dynamics of criminal behavior evidence from weather shocks. *Journal of Human Resources*, 42(3):489–527.

- Kennedy, D. M., Braga, A. A., and Piehl, A. M. (2001). *Reducing gun violence: the boston gun project's operation ceasefire*. US Department of Justice, Office of Justice Programs, National Institute of Justice Research Report 188741.
- Kubrin, C. E., Messner, S. F., Deane, G., McGeever, K., and Stucky, T. D. (2010). Proactive policing and robbery rates across us cities. *Criminology*, 48(1):57–97.
- Levitt, S. D. (2002). Using electoral cycles in police hiring to estimate the effects of police on crime: Reply. *American Economic Review*, 92(4):1244–1250.
- Levitt, S. D. and Miles, T. J. (2006). Economic contributions to the understanding of crime. *Annual Review Law Social Science*, 2:147–164.
- Lin, M.-J. (2009). More police, less crime: Evidence from us state data. *International Review of Law and Economics*, 29(2):73–80.
- MacDonald, J. M. (2002). The effectiveness of community policing in reducing urban violence. *Crime & delinquency*, 48(4):592–618.
- Magaloni, B., Franco-Vivanco, E., and Melo, V. (2020). Killing in the slums: Social order, criminal governance, and police violence in rio de janeiro. *American Political Science Review*, 114(2):552–572.
- Raphael, S. and Ludwig, J. (2003). Prison sentence enhancements: The case of project exile. *Evaluating gun policy: Effects on crime and violence*, 251:274–77.
- Rosenfeld, R., Fornango, R., and Rengifo, A. F. (2007). The impact of order-maintenance policing on new york city homicide and robbery rates: 1988-2001. *Criminology*, 45(2):355–384.
- Sampson, R. J. and Cohen, J. (1988). Deterrent effects of the police on crime: A replication and theoretical extension. *Law and Society Review*, pages 163–189.