Police and Crime: Further Evidence from a Quasi-Natural Experiment

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Abstract

The paper investigates the effect of police presence on homicides at the municipality level in Brazil during the January 2010 to December 2014 period. For this purpose, occasional and illegal police strikes are considered as relevant shocks in a quasi-natural experiment. After controlling for different variables that explain heterogeneity across municipalities, it is possible to identify a sizeable effect accruing from police strikes on the occurrence of homicides. Despite a conservative analysis that involves temporal and spatial aggregation of variables, the evidence indicates that police strikes lead, on average, to a 16% increase in the homicide rate if one considers a broader sample of 3597 municipalities. The focus of the analysis for a large and heterogeneous country also partially may mitigate concerns for external validity that had been raised in the context of previous studies in the related literature.

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1. Introduction

Crime deterrence is a recurring research topic that has given rise to a large and diversified literature that includes contributions in criminology and in economics [see Cameron (1988), Levitt (2004), Carriaga and Worrall (2015) and Chalfin and McCrary (2017) for comprehensive surveys].

The seminal contribution of Becker (1968) proposed economic logic for engaging in criminal activities. The static model postulates that rational agents consider the probability of being caught, the intensity of the punishment, and the availability of alternative labor market opportunities. Those aspects delineate three main research subareas in the study of crime deterrence.

In the present paper, the focus will be on the first strand of the literature and related implications pertaining to the association between police and crime. As intuitive as it might seem, evidence accruing from the empirical studies discussed in the aforementioned surveys is mixed and often indicates no substantial effects. An important challenge for identifying causal effects relates to the potential endogeneity of police presence. A first strategy for overcoming such difficulties involved the conduction of a Granger causality exercise by Marvell and Moody Jr. (1996); however, the evidence suggests a bidirectional causation. Despite the dynamic specification that includes a flexible lag structure, it disregards possible contemporaneous effects, and the analysis may be prone to omitted variable biases.

A second research strategy for mitigating potential endogeneity problems in police presence evolved by considering instrumental variable methods, as summarized in Worrall and Kovandzic (2010). The related challenge of finding

suitable instruments for police presence that are not correlated with crime constitutes a non-negligible task. Levitt (1997) proposed the use of mayoral and gubernatorial electoral cycles in policy hiring for attaining such purposes for an adequate instrument. The underlying logic is that incumbent candidates would tend to increase police hiring in the last year of office in order to signal toughness regarding crime. In principle, such variables should be exogenous with reference to crime and the evidence; at first, the results indicated a strong negative effect from police on violent crime, albeit a less significant effect on property crime. However, McCrary (2002) later showed that programming and classification errors had made Levitt's results appear more precise than justified, and thus the results overestimated the effects of police presence given the underestimation of standard errors.[1] Levitt (2002) concedes the indicated problems and readdresses the issue by using the number of firefighters as instruments and obtains similar magnitudes of the effects in comparison to the corrected estimates for his original instrument. Kovandzik et al. (2016) revisits the estimates obtained by Levitt (2002. 2004) and concludes that instruments for police levels (mayoral and gubernatorial election cycles and firefighter hiring) are weak by current econometric standards; therefore, doubts could be cast on the ability to properly address the potential endogeneity of police in crime.

Alternatively, more recent studies have attempted to mitigate the endogeneity problem by relying on exogenous shocks that may enable a relevant quasi-natural experiment. Di Tella and Schargrodsky (2004) focused on the exogenous event provided by a terrorist attack on the main Jewish center in Buenos Aires, Argentina, regarding the extra police protection

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[1] Downward biased standard errors and associated overestimation of causal effects are important issues in event studies as indicated by Bertrand et al. (2004) in the context of difference-in-difference models with serial correlation.

provided to Jewish institutions. The detailed data on the location of car thefts before and after the attack indicated significant crime reduction effects accruing from police presence. However, such effects are local and do not prevail beyond the narrow protected area. Thus, despite the painstaking care with the data-end examination of related local demographics, a potential shortcoming of the study pertains to its external validity and its ability to uncover a general causal association between police and crime. Klick and Tabarrok (2005) propose changes in the terror alert level in Washington, D.C., as a relevant exogenous shock for evaluating the causal effect of police on crime. The daily character of the data and multiple switching of the terror alert level within the sample period produced a valuable variability in the referred shock. The evidence suggests that a significant crime decrease appears to prevail in high-alert periods. However, external validity concerns may emerge regarding the restricted geographic area that was considered.

Draca et al. (2011) uses the additional police presence, in connection to the July 2005 terror attacks in London, as a quasi-natural experiment for investigating the police-crime nexus that intended to be more general than the aforementioned analogous studies by considering a large metropolitan area. Furthermore, the abrupt shift in police deployment and the related availability of detailed data favors the identification of a causal effect. The authors find evidence of declines in crime accruing from stronger police presence for modalities that likely are to be prevented by police visibility (e.g., robberies and thefts).

The present paper uses illegal police strikes as natural experiments and attempts to use its repercussions in the homicide rates of municipalities in

Brazil. We posit that we can assume, without difficulty, that the timing of police strikes are exogenous events, completely unrelated to variations in fatal violence. Even if the sheer existence of such institutional failure regarding the most basic provision of a public service is expected to be correlated with violence in general, we should not expect that rare events such as these rebellions are synchronized in any way with the factors being studied.

More recently, during an extensive police strike, dreadful scenes of barbarism happened during February 2017 in the city of Vitória, the main district of Espírito Santo state, Brazil. Sixty-two people were murdered in the course of only two days (Feb. 6-7). More than 200 people were killed since the beginning of the month, a 64% increase compared to last year's figures. In addition to deaths, more than 600 vehicles were stolen during this period, including 200 vehicles within a 24-hour period, while 300 stores were looted. The number of criminal offences increased tenfold over the state average. Such casual evidence is suggestive of motivating a large-scale study that considers police strikes as a pertinent exogenous shock. Pfuhl (1983) has investigated crimes in selected U.S. cities during police strikes in the 1970s by comparing the prestrike, strike and poststrike data for burglary, larceny, auto theft and robbery. The results are not unequivocal regarding the police-crime link; however, one has to emphasize the descriptive character of that study that does not perform any econometric analysis or properly control for other intervening factors.

The paper aims to contribute to least two aspects:

a) By considering a large and heterogeneous economy such as Brazil that is plagued with crime indicators compatible with war zones, especially in

some urban localities inflicted by drug traffic conflicts and thus has prompted a growing number of studies on violence [see Santosand, Kassouf (2008) for an overview]. The total number of homicides in Brazil has reached 1 million between 1980 and 2010 [see Murray et al. (2013)]. The associated social and economic costs of crime and violence in Brazil are extremely large; the total expenditures on police, prisons, private security, public health, and loss of human capital (from premature deaths caused by violence) as well as personal loss from robbery and theft are substantial. The total cost of crime in Brazil was estimated to be R$92 billion in 2004, or 5.1% of GDP [see e.g., Cerqueira et al. (2007)]. The sizeable heterogeneity across Brazilian municipalities includes aspects that are especially important in Brazil as related to income inequality, which appears to favor crime occurrence [see Sachsida et al. (2007)];

b) The illegal police strikes provide an appealing exogenous shock to identify a causal effect between police presence and crime. Moreover, concerns on external validity in previous quasi-natural experiments can be mitigated somewhat with a large sample that encompasses very diverse municipalities and a suitable set of control variables.

The paper is organized as follows. The second section provides a brief discussion on police institutions in Brazil. The third section discusses the data construction. The fourth section presents the empirical strategy for econometric estimation and presents the results. The fifth section undertakes some robustness exercises. The sixth section provides some final comments.

2. Police in Brazil: Institutional Background

The police apparatus in Brazil involves a complex institutional arrangement mostly defined at the state level. A dual system prevails with the coexistence of military and civil police forces. The former would be responsible for direct crime deterrence activities, whereas the latter would focus their activities on crime investigation. A complementary contribution is provided by municipal guards that are responsible for transit controls (and imposition of fines) and for disciplining the use of public spaces, such as with street vendors. Typically, those local guards have limited powers and are allowed to carry lethal weapons in some larger cities (currently in approximately 500 out of the 5570 municipalities that exist in Brazil).[2] Altogether, the system presents substantial challenges, including managerial aspects, that extrapolate severe budget restrictions. A general perception is that substantial degrees of inefficiency prevail both in crime deterrence as well as in the investigation/intelligence sphere with a substantial number of unsolved crimes, according to the Brazilian public ministry (*Conselho Nacional do Ministério Público*), not to mention frequent press reports regarding corrupt or abusive police practices. Scalco et al. (2012) investigate technical efficiency in the context of the military police of the state of Minas Gerais, and the issue of police underperformance and underlying factors cannot be neglected and warrant additional studies.

However, beyond the isolated focus on a particular police force, it is important to highlight the existence of important coordination failures in such multijurisdiction systems. In fact, Medeiros (2004) revisits the possibility of unifying military and civil police forces for improving efficiency.

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[2] Bretas and Rosemberg (2013) discuss historical accounts of police in Brazil.

The prevalence of coordination failures has become particularly evident in recent drug battles in Rio de Janeiro in September 2017 that led to the mobilization of the national force. During that occasion, the difficulty of coordinating local forces and federal entities (federal police and highway patrol) become somewhat more evident. The continental size of Brazil poses significant challenges regarding border control and related arms trafficking that supplies drug-inflicted locations. However, beyond a general discussion in association with a national security policy conceived at the federal level, it is apparent that a better coordination of local institutions such as the military and civil forces, and the municipal guard in an auxiliary role, is necessary.

Levitt (1994) attributes limited roles for better policing strategies to explaining the decrease in crimes in the United States during the 90s; however, given the current disarray in Brazil, one can conjecture that there might be significant room for institutional reforms that may favor police effectiveness.

The wedge between police institutions and the population is evident, and initiatives such as community policing are incipient. An exception was provided by pacifying policy units (*Unidades de Polícia Pacificadora-UPP*) that established the actual permanent presence of the police in shanty towns in Rio de Janeiro, especially in those areas with expressive drug trafficking. Butelli (2015), based on difference-in-difference estimations, obtained evidence that suggests positive impacts for reducing violent and property crimes. However, the focus on particularly poor communities (whether occupied by UPPs or not) may raise concerns regarding external validity and the possibility of

extrapolating the magnitude of the effect of police presence in the context of other locations.[3]

Finally, there appears to be some consensus that not all units performed equally well and that police occupation does not preclude the necessity of further improvements regarding infrastructure and services (such as education) to consolidate those areas for safety. Unfortunately, the recent fiscal collapse in the state of Rio de Janeiro curbed the progress of those initiatives. The previous discussion sets the brief background of the police institutions, but several of the mentioned aspects extrapolate the scope of the present paper.

3. Data Description

Our database is built with a panel data of homicide rates per 100,000 inhabitants at the municipal level on a monthly basis, calculated based on homicide figures provided by the Brazilian System of Death Registration (SIM) maintained by the Brazilian Ministry of Health (*Ministério da Saúde*) as well as population data according to IBGE and several other covariates that come from diverse sources. Our main sample includes 3597 municipalities. The population numbers are provided in an annual frequency; therefore, monthly frequency calculation was achieved by linear interpolation.[4] The description of variables and sources are summarized in Table 1.

INSERT TABLE 1 AROUND HERE

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[3] Another example of focused police intervention previously was assessed by Machin and Marie (2011) for the "Street Crime Initiative" that was introduced in England and Wales in 2002. The evidence suggested favorable crime deterrence effects with the policy of special targeting of particular areas.
[4] Similar procedures often are adopted in event studies when demographic variables are typically not available at higher frequencies. See, for example, Biderman et al. (2010).

Deaths classified with codes X85 to Y09 and Y87.1 in ICD-10 were counted as homicides. This classification matches the coding of violent deaths in the Global Burden Disease 2004 Update (World Health Organization, 2008). The focus of the present study on homicide rates reflects the stronger reliability when compared with other crime categories that have a higher tendency toward underreporting.

The police strike indicator was obtained by a multitude of sources that involved cross-checking between state secretaries of security and press sources; there is no unified source available for such types of data, and we have relied the main Brazilian newspaper websites such as *Estado de São Paulo, Jornal do Brasil*, *Folha de São Paulo* and *O Globo*. We have considered strikes that were sizeable and generated enough disturbances as reported in the press or that required federal intervention. This method resulted in a few cases, but almost all of them were treated as calamities in the reports. The reports, such as the case of Vitória mentioned in the introduction, usually stated that crime rates were rising in comparison to last year's figures, but precise accounts of the reductions of police force numbers are problematic. Nonetheless, the news provides some crude account statements regarding the decrease in military police officers in the street, which is about a 60-80% decrease. These numbers should be taken as approximate, since police officers on strike desire to exacerbate the strength of their position and leverage over authorities and the general public. Additionally, the length of strikes is inconsistent. Some strikes last more than two weeks, and others last only a few days. Finally, in most cases, the Army or the National Guard was called as an

emergency auxiliary to perform patrol activities as a substitute. The military police strike events are described in Table 2.

INSERT TABLE 2 AROUND HERE

Other covariates used as controls commonly are found in previous studies of the causes of violent crimes. Demographic variables such as a population aged 15-24 and the white percentage of population were considered. Young males are the vast majority of those arrested or imprisoned. Hence, one should expect that a higher proportion of young people in the population could lead to more violent crimes (Levitt, 1998). Brazil has a mostly mixed-race population, but a race variable is included to provide comparison estimates with previous inquiries. Socioeconomic covariates are also included. The city-level regressions are controlled by GDP per capita, Gini index, state poverty rate, and percentage of urbanized area at the state level. These variables should account for the economic and social environment influence on criminal behavior. Several papers stress those factors as drivers of the supply of criminal activities [see Freeman (1999)]. Unfortunately, unemployment data at this level are available only after 2012, although earlier papers (Levitt, 1997) made use of this variable. Annual data on state level incarceration rate per 100,000 inhabitants and police officers per capita were also considered. The incapacitation and deterrence effect of both incarceration and patrolling activity should, in principle, exert a negative influence on the rate of violent crime (Cameron, 1998). Table 3 displays the relevant summary statistics of the different variables.

INSERT TABLE 3 AROUND HERE

4. Empirical Analysis

4.1- Empirical strategy

We estimate some variants of the following model:

$$HOMICIDES_{it} = \beta_0 + \beta_1\ STRIKE + \sum_t \gamma_t\ MONTH_t + \sum_i \mu_i\ CITY_i + \theta..CONTROLS_{it} + \varepsilon_{it} \quad (1)$$

where HOMICIDES$_{it}$ represents the number of homicides per 100,000 inhabitants in municipality i at period t; STRIKE$_{it}$ is a dummy variable that assumes value 1 if a police strike (by a military police) took place at municipality i at period t and assumes value 0 otherwise; MONTH$_t$ indicates a set of dummy variables for each month; CITY$_i$ denotes a set of dummies for municipalities; and CONTROLS$_{it}$ is a set of time-varying covariates for different localities at the different time periods. Those variables attempt to control for heterogeneity with usual variables that are considered in the related literature as to allow a proper contrast between the municipalities where strikes took place and those where no such episode occurred. Thus, we include variables pertaining to inequality (Gini index), local economic strength (per capita GDP), incarceration rate, poverty rate, number of police officers per 100,000 inhabitants, percentage of the population between 15-24 years old, white percentage of population, percentage of urbanization and population without formal instruction. A more detailed description of the variables, which includes the data sources, was reported in Table 1. In the case of variables available at the state level, the referred value is assumed to prevail for each municipality belonging to the particular state. Furthermore, the shocks provided by the police strikes are shorter lived than the monthly observations of the control variables. Thus, spatial and temporal aggregation renders a conservative character to the present paper; therefore, any sizeable effect of such shocks on crime, if that is

the case, will be especially suggestive. In the actual implementation, we also attempt to control for the persistence of the homicide rate by including a lagged term.[5]

The observations are weighted by population, which penalizes small municipalities with high homicide variances during the years considered. The relevant weight in this case is the initial population in our sample. Adopting the procedure suggested by Bertrand et al. (2004), the standard errors are clustered at the municipality level. This should ensure a more consistent estimation of the standard errors because it provides estimates that are robust to serial correlation. Bertrand et al. (2004) stress the fact that the dependent variable in models such as the one used in this paper typically are serially correlated, over usually a long time series. Additionally, the treatment variables show little change between states. However, this bias should be small because of the short duration of the events studied.

4.2 - Discussion

The foremost objective of this paper is to evaluate the effect of police mutinies on the violent crime rate. We made use of homicides as our main approximation of violent crimes. Likewise, we discuss briefly the robustness of our results to a "placebo law" and aggregation of the homicide rates by states instead of municipalities. Additionally, it is worth addressing the external validity of our findings in this discussion. Table 4 summarizes the estimation results.

INSERT TABLE 4 AROUND HERE

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[5] Estimations were implemented in Stata 14. The density estimation was carried out in software R.

The obtained effect is non-negligible, as 0.35 represents, if one considers the mean value for the homicide rate, a 16% increase in that rate accruing from police strikes if we consider for reference the mean value of homicide rate of 2.23 that was observed in our sample.

However, the events studied in this paper have some dissimilar features in comparison with alternative research designs. It is difficult to estimate precisely the effect of the reduction in police because the lessening of law enforcement itself is not particularly exact. Nonetheless, we have some crude reports of the reduction of police personnel in the streets, which is in the range of 60-80%.

The control variables' coefficients indicate that higher municipality GDP is associated with higher levels of violence. One could speculate that increases in criminal activities such as drug trafficking are connected to higher incomes. It is possible to establish a link between big (and consequently richer) cities with criminality. Surprisingly, the percentage of young people aged 18-24 years was not significant, as most controls. This finding could reflect fact that covariates are measured at the state level. However, alternative regression that did not cluster the standard error by municipality yielded different results, whereas most covariates were significant at conventional levels.

Homicides are less prone to underreporting partly because, by force of law, the investigation must be conducted until a body is found. Additionally, the data used in this study are provided not by morbidity reports from doctors but are provided by the Brazilian Ministry of Health (DataSUS - *Ministério da Saúde*). Using official information on several kinds of crimes for all states given

by the Brazilian Ministry of Justice (*Ministério da Justiça*), a high correlation between crimes were found, approximately 75%. Dix-Carneiro et al. (2017) show that the correlation for homicides recorded by national health agencies is strong not only for police records but also for several types of crimes, including violent crimes against property.

Dix-Carneiro et al. (2017) point to the strong correlations between types of violent crimes because property crime and drug-trafficking activities are performed by heavily armed individuals. Gangs usually fight for territory, which leads to more homicides [Chimeli and Soares (2017)].

It also warrants noting that the increase in homicide rates is highly concentrated in space. Approximately 30% of murders are perpetrated in just 20 municipalities. This finding is considered by weighting assigned to city size.[6] Smaller cities exhibit much more volatility in crime rates, while larger metropolitan areas show higher and more stable violence.

5. Robustness and Further Comments on Validity

As a first attempt at assessing the robustness of our findings, we run a procedure aiming at emulating a placebo exercise [Bertrand et al. (2004)]. We randomly assign the treatment variable to all observations in our dataset. After that, the same specification of our model is regressed with the homicide rate as the dependent variable. The procedure is performed 1000 times while the variable's coefficients are stored. The density function of our parameter of interest then is plotted (Figure 1). The distribution is centered at zero. As

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[6] The *aweights* command available in Stata regression was used for this purpose.

expected, the parameter estimated from the original data (0.35) is placed in the tail of the distribution ($p < 0.01$). [7]

A regression using states as the unit of obervation was also performed to address the robustness of the results to the aggregation of homicide rates at the state level. The results, reported in Table 5, were unchanged, but it is worth mentioning that most of the control variables were significant this time, probably because there is a stronger link between homicide rates at the state level and the controls, which mostly are collected at the state level. The deterence set of covariates show the expected negative effect on homicide rates. Inequality, on the other hand, seems to increase homicides.

INSERT TABLE 5 AROUND HERE

INSERT FIGURE 2 AROUND HERE

Figure 2 illustrates the results of estimates of persistence of the treatment effect over time. As expected, the effect is indistinguishable from zero in previous months, reaching its peak when the strike event occurs. There is some evidence of persistance in the forthcoming month, but it rapidly wears off. The evidence highlights the random and surprising effect of the events under scrutiny on crime rates.

The estimated effect probably is due to the opportunistic behavior of criminals. This result should be clear from the Becker utility model of crime. When the police are on strike, their constraints are somewhat relaxed, allowing

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[7] Additionally, we performed a few tests regarding the power of our hypothesis testing. For hypothetical parameter values equal or near the estimated value and the standard deviation of our sample, the power was found to be 80% or higher. Both Stata 14 routines for power computation and the routine proposed by Gelman and Carlin (2014) were used.

criminals to operate with assigning a lower probability of getting caught. Even in more sophisticated models allowing for more periods and interactions, the probability of getting caught under police strike is so low that people should acknowledge that it is a one-time event and anticipate performing their criminal activities, notably premeditated murder. Additionally, there should be a feedback mechanism especially among rival gangs that could escalate in a full-blown armed conflict. Likewise, there is evidence of stronger presence of death squads in the vacuum left by the police. The news from those days similarly point to an increase in property crime, which could suggest that non-criminals also see these events as opportunities.

It is worth mentioning that the resolution rate of homicides in Brazil is very low, approximately 5%, according to the *Conselho Nacional do Ministério Público*. Consequently, it is reasonable to conclude that criminals already consider that they are subject to a low probability of going to prison or being punished.

Concerning the external validity of the findings of this paper, it is worth mentioning that the findings probably are confined to fixed and mobile police law enforcement. Police on patrol could cover a larger area so that they can surprise criminals performing their activities, especially if certain types of violent crime are planned in advance and require pursuing the victim. Furthermore, police on patrol actively can search and chase criminals when on service. Policymakers under pressure of the public usually focus on ostensible policing as a means of curbing crime, but it could result in inefficient police allocation (DiTella and Schargrodsky [2004]). In general, the pressure for the increase in ostensible policing is not matched by the increase in expenses in police

intelligence, equipment and investigative activities. In Brazil, the state civil polices are responsible directly for investigative activity and evidence searches, as previously mentioned in the introduction.

6. Final Comments

The evolution and increased availability of empirical methods gauging causality in economics, along with the availability of new datasets for criminal records have made it possible to check the response of unlawful activities to enhanced vigilance, patrolling activities, and police officers in the streets. Due to weak institutions and likely the anticipation of the low probability of punishment due to past amnesties, police officers went on strike on several Brazilian states, predominantly asking for better wages or working conditions. Several news reports of the events suggest that an increase in criminal activity ensued. After extensive data collection on homicides and several demographic and social controls, we attempted to estimate the magnitude of these events on the homicide rate.

Due to the distribution of the occurrence of strikes over time and the apparent lack of relationship of strikes with criminal activity, the happenings of military police insubordination is assumed to be exogenous in relation to homicides. The estimated effect of the strikes on the homicide rate was 0.35, but the strikes have different durations and intensities, mainly because there is variation in the participation of police officers in strikes and because the state governments usually call for federal aid in the form of army assistance for patrolling activities. The results are also robust to alternative specifications and

do not seem to be generated by spurious correlations associated with different crime dynamics for the treatment and control groups.

Altogether, the obtained evidence seems suggestive and indicates a sizeable effect of a 16% increase in the homicide rate stemming from police strikes in a leading case.

The conservative character of the analysis, which involves spatial and temporal aggregation, provides additional interest regarding the non-negligible effect that was detected in the econometric estimation. Moreover, the consideration of large sample that encompasses very heterogeneous municipalities partially mitigates concerns for the external validity of the results that had emerged in connection with some previous works in the related literature.

Finally, even if the present results are compelling in the sense of indicating a relevant link between police presence and crime deterrence, the evidence does not preclude the necessity of long-run strategies toward the reduction of economic inequality and institutional reforms in police and adequate articulation between the federal, state and municipal spheres of government regarding security policy. In fact, the latter aspect became salient in the ongoing federal intervention in the state of Rio de Janeiro in 2018, where drug dealers and militia achieved a substantial degree of organization with continuous access to high-powered smuggled weapons.

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Table 1

Description of variables

Variable	Description	Frequency	Source
Number of homicides per 100,000 inhabitants	Municipality homicide count / municipality population	month	IBGE / Health Ministry
Strike	Binary variable indicating a strike episode involving state military police	month	newspapers
Incarceration rate	% of state population recorded as imprisoned	year	Justice Ministry / IBGE
Gini index	State level Gini index based on income	year	IBGE
Poverty rate	State level poverty rate (%) based on poverty income threshold as calculated by IBGE	year	IBGE
% w/o education	State level percentage of population without formal education	year	IBGE
% Aged 18-24	State level percentage of population aged 18-24 years	year	IBGE
% urbanization	State level percentage of population living in urban areas	year	IBGE
% White	State level percentage of population self-described as white	year	IBGE
GDP per capita	Municipality GDP / municipality population	year	IBGE
Police officers per capita	Number of police officers in state / state population	year	Justice Ministry / IBGE

Table 2

Recorded State Military Police Strikes in Brazil

State	Month	Year	Duration
CE	4	2010	2 days
PR	3	2010	2-3 days
MA	11	2011	10 days
PB	2	2011	3 days
RO	4	2011	2-3 days
RO	12	2011	2-3 days
BA	2	2012	12 days
CE	1	2012	6 days
RJ	2	2012	4 days
RO	12	2012	2 days
BA	4	2014	2 days
PA	4	2014	6 days
PE	5	2014	2 days

Table 3

Summary statistics – municipalities (2010-2014)

Variable	Mean	Std. Dev.	Min.	Max
Number of homicides per 100000 inhabitants	2.230	4.237	0	86.066
strike	0.009	0.096	0	1
Incarceration rate	362.025	154.268	124.9	858.8
Gini index	0.506	0.0315	0.421	0.616
Poverty rate	14.517	9.758	2.861	36.696
% w/o education	11	5	2	23
% Aged 18-24	8.980	0.822	7.59	12.08
% urbanization	81.482	9.036	58.25	97.37
% white	44	19.438	18.01	85.75
GDP per capita	1565873	1.24E+07	4199	6.28E+08
Police officers per capita	504	103.030	172.663	920.044

Table 4

Estimation results

Variables	Coefficient	Robust std. error	p-value
strike	0.354	0.124	0.004
Homicide t-1	0.091	0.0138	0.000
% white	0.098	0.028	0.000
% urbanization	-0.065	0.018	0.000
% Aged 18-24	-0.133	0.133	0.318
Gini Index	-1.338	2.654	0.614
Incarceration rate	0.001	0.001	0.401
% w/o education	-0.023	0.024	0.322
Poverty rate	0.026	0.034	0.447
GDP per capita	0.043	0.009	0.000
Police officers per capita	0.001	0.001	0.344
trend	-0.004	0.006	0.487
Municipality fixed-effect	Yes		
Time fixed-effect	Yes		
Number of observations	172,153		
R^2	0.06		
$F_{(70,3596)}$	10.10 $P < 0.000$		

Table 5

Estimation results – state level regression

Variables	Coefficient	Robust Std. error	p-value
strike	3.504	1.609	0.039
Homicide t-1	0.316	0.056	0.000
% white	0.492	0.136	0.001
% urbanization	-0.452	0.137	0.003
% Aged 18-24	1.359	0.748	0.081
Gini Index	-28.695	9.904	0.008
Incarceration rate	-0.012	0.009	0.180
% w/o education	-0.180	0.205	0.386
Poverty rate	0.127	0.163	0.443
Police Officers per capita	0.007	0.007	0.333
trend	0.060	0.045	0.192
State fixed-effect	Yes		
Time fixed-effect	Yes		
No. of observations	1523		
R^2	0.35		
$F_{(26, 1427)}$	24.80 $P < 0.000$		

Figure 1

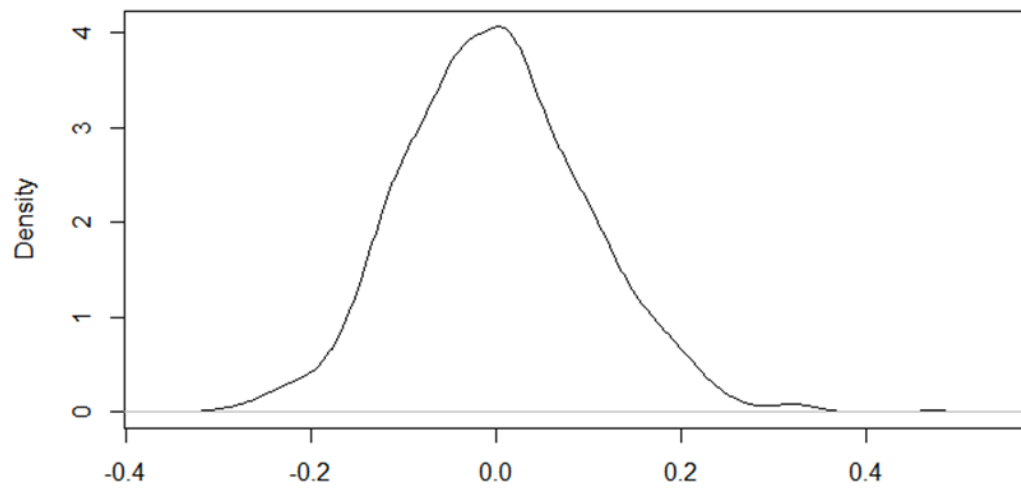Density function obtained from robustness simulation results – Gaussian

kernel

Figure 2

Treatment effect over time – persistence and confidence bands (±1.96 standard errors for each point estimate)

TIME PASSAGE IN RELATION TO STRIKE EVENT (MONTHS)