Pontifícia Universidade Católica do Rio de Janeiro

Laura de Carvalho Schiavon

Essays on crime and justice

Tese de Doutorado

Thesis presented to the Programa de Pós–graduação em Economia of PUC-Rio in partial fulfillment of the requirements for the degree of Doutor em Economia.

Advisor: Prof. Claudio Abramovay Ferraz do Amaral

Rio de Janeiro
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Abstract

Schiavon, Laura de Carvalho; Ferraz, Claudio (Advisor) . **Essays on crime and justice**. Rio de Janeiro, 2017. 140p. Tese de Doutorado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

This thesis consists of three essays on the economics of crime and the judicial system. The first essay investigates the determinants of judicial performance. We show that more developed localities in Brazil, measured by higher income, educational level and urbanization concentrate the more productive courts and judges creating large disparities across the country. The second essay evaluates the impact of legal capacity measured as judicial performance on violent crimes in Brazil. We use a Regression Discontinuity Design and show that changes in the judicial district classification that depends on number of voters increases judicial performance and this change is associated with a decrease in homicide rates. The third essay assess the impact of women's protection laws - The Maria da Penha law introduced in Brazil in 2006 - on domestic violence. We use a differences-in-differences strategy and compare homicides rates of males and females before and after the passage of the law. We find that the law significantly reduced the female household homicide rates and these effects were concentrated in small municipalities.

Keywords

Development; Crime; Judicial System; Legal Capacity; Domestic Violence;

Resumo

Schiavon, Laura de Carvalho; Claudio Abramovay Ferraz do Amaral . **Ensaios em crime e justiça**. Rio de Janeiro, 2017. 140p. Tese de Doutorado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Essa tese compreende três ensaios sobre economia do crime e sistema judiciário. O primeiro ensaio investiga os determinantes da performance do judiciário. Verifica-se que as localidades mais desenvolvidas no Brasil, aquelas com maior nível de renda, educação e urbanização, concentram os juízes e as varas com melhor performance, gerando importantes disparidades no país. O segundo artigo avalia o impacto da capacidade legal, medida pela performance do judiciário, em crimes violentos no Brasil. Nele, é explorado o método de Regressão Descontínua e mostrado que mudanças na classificação da comarca, segundo o número de votantes desta, geram um aumento na performance do judiciário associado a uma redução das taxas de homicídios. O terceiro artigo testa o efeito de leis de proteção à mulher sobre a incidência de violência doméstica. Utiliza-se um modelo de diferenças em diferenças para comparar as taxas de homicídios de homens e de mulheres antes e depois da aprovação no Brasil, em 2006, da Lei Maria da Penha. Verifica-se que a lei reduziu significativamente a taxa de homicídios de mulheres ocorridos nos domicílios e que os efeitos foram concentrados nos municípios pequenos.

Palavras-chave

Desenvolvimento; Crime; Justiça; Capacidade Legal; Violência Doméstica;

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Determinants of judicial system performance

1.1
Introduction

State capacity is a key factor for economic development ((1); (2); (3)). Recent theoretical and empirical studies discuss the relationship between legal capacity and development. Legal capacity affects the degree to which State is able to protect property rights, enforce contracts and punish crimes. The State's capability to secure property rights and enforce contracts stimulate economic transitions, foster credit markets and promote investment as well as competition and firm growth ((4); (5)). In turn, effective punishment is an important factor in reducing crime ((6), (7), (8)).

There are also evidences of the positive relationship between legal capacity and development for developing countries. (5) and (4) argue that the improvement of judicial system affects firm size and performance. (9) and (10) show that it stimulates entrepreneurship and (11) show that it stimulates the credit market performance. Furthermore, (12); (13) demonstrate that the certainty of punishment can reduce crimes in Latin America. (14) show that legal capacity promotes municipal long-term development.

Our paper studies the determinants of judicial districts' performance in Brazil. Firstly, we conduct a descriptive analysis of the judicial system characteristics. Hereafter, we present an exploratory spatial data analysis. Lastly, we estimate linear regression models to study the correlation between local level of development and judicial performance.

We construct a novel database on judicial system features, which includes average annual measures of districts' performance, judges' characteristics and locational attributes. We use as main source of data monthly reports on judicial system performance and structure from 2010 to 2014 taken from the survey Justiça Aberta (*Open Justice*), provided by the National Justice Council ((15)). Additionally, we use the Census 2010 ((16)) to characterize the set of municipalities comprised in each district in terms of income, inequality, educational level and urbanization. Finally, we exploit the Annual Report of

Social Information , RAIS ((17)), a matched employer-employee data of the Labor Ministry of Brazil, to obtain information on judges' experience, age and gender.

We analyze five districts' measures of judicial performance: average annual number of processes allocated per judge; average annual number of processes allocated per 1000 inhabitants; average annual number of sentences per judge; average annual number of sentences per 1000 inhabitants; and average annual number of sentences per 100 processes allocated. Since these outcomes affect each other, they reflect judicial demand, judges' productivity and judicial capacity of demand attendance in equilibrium.

This study reveals that judicial performance is very heterogeneous across Brazilian districts. The variability within States is more accentuated than the variability between States. This evidence indicates that differences in judicial performance are more related to local heterogeneities than to specificities of legal organization determined by the State Courts. Exploiting intra-States differences, we verify the positive correlation between local level of development and judicial performance. The regressions show that the number of processes allocated per judge and per capita and the number of sentences per judge and per capita are positively correlated with districts' level of income, education and urbanization. Additionally, we find that localities with higher income levels present higher number of sentences per processes allocated.

The positive correlation between judicial performance and local level of development may be explained by three central factors. First, the attraction of better bureaucrats due to municipal amenities. Second, the existence of specialized courts in the most developed areas as well as a higher number of courts. The results show that specialization and number of courts are positively correlated with districts' performance. Third, the mechanisms of promotion of judges. Specifically, the criteria of promotion are based on judges' performance and experience. Consequently, it is expected that most experienced and productive judges mostly work in highest level districts. Indeed, highest level districts have a higher number of sentences per judge and a higher average judges' experience. The highest level districts are the most developed districts in terms of income, educational level and urbanization rate. It occurs because the State Courts' rule of district classification are functions of judicial demand predictors, such as population and fiscal revenues. Furthermore, the concentration of more productive and experienced bureaucrats in the most developed areas can increase the overall productivity through peer effect.

This study aims at contributing to the understanding of legal capacity determinants as well as to the public service productivity literature, specially for Latin American countries. The researches about justice system mainly focus on its impacts on economy. There are few analyzes of policies that can improve the judicial system, specially for developing countries. Among the existing studies, some investigate the legal origins and the historical determinants of legal capacity ((18); (14)). Other studies conduct cross-country analyses. In general, differences in judicial performance across countries are associated with court specialization, computerization, management techniques and high judge salaries ((19); (20)). Recently published papers highlight the role of selection and incentives on judges' decisions ((21); (22)).

The public service literature mostly provides evidence on the impact of selection, incentives and monitoring on bureaucrats' performance ((23)). Among researches which focus on selection, (24) show that higher wages attract better candidates, even in less developed areas. In turn, the incentives literature demonstrates the effectiveness of salaries and career incentives on improving public service delivery ((24); (25); (26)). The role of agent motivation is reinforced by (27) and (1). Finally, studies on the effectiveness of monitoring reveal that it reduces corruption ((23); (28)). Recent empirical evidences argue that bureaucrats' performance are also influenced by management strategies, such as task allocation ((29)), and technical improvements, such as informational technology implementation ((30)).

This paper is organized as follows: Section 2 provides information on the main features of the Brazilian judicial system organization; Section 3 describes the database and presents the empirical strategy; Section 4 discusses the results; and Section 5 reports the conclusion remarks.

1.2 Institutional Background

The Brazilian judicial system is characterized by lengthy trials. According to (31), the system was able to conclude only 20% of the total number of processes existent in 2014. However, there is an important heterogeneity in terms of efficiency among courts, even considering those in the same State ((32)). In contrast, the country expenditure with justice is much higher than in other countries. The judicial system costs around 1.2% of the national GDP. In Argentina, in Chile and in the USA, for example, it corresponds to less than 0.4% of the national GDP.

This paper analyzes the determinants of the performance of Brazilian judicial system. The focus is on the first instance of State Justice, which is the most important part of the Brazilian judicial system in terms of processes allocated. In 2014, the first instance of State justice received 62% of the total number of process allocated to the judicial system in Brazil. Among those which could be appealed against, only 8.2% were allocated to the second instance ((31)).

The State Justice is part of the Ordinary Justice. The latter is responsible for the trial of most of the cases allocated to the judicial system, with the exception of those processes related to electoral, labor and military issues. The Ordinary Justice is composed by the Federal Justice, that hears basically the Union cases, and the State courts. The first instance of State justice is responsible for receiving the cases appropriated to the State courts and the second instance is responsible for hearing the appeals ((33)).

State justice is divided in districts. Districts are judiciary units that comprise one or more municipalities. The districts are classified as first, second or third level[1], according to their effective or potential judicial demand. The States determine their district classification criteria in the State Judicial Organization Law. Usually, they are functions of district's population, area, number of processes allocated and correlated measures.

In general, higher level districts have more courts, more judges and better structures because they receive a higher number of processes. Furthermore, higher level districts have more productive judges due to the judges' career stages. The promotion criteria are based on merit and experience. When judges are hired, they are classified as substitute judges. Once the substitution period is ended, they are classified as first level judges and work in a first level court. When the judges are promoted, they receive a wage increase and start working in a second level court. Each promotion occurs in conjunction with wage increase and judges' transfer to a higher level court.

1.3 Data and Empirical Strategy

This paper exploits a novel database on judicial system constructed by the authors. We develop a cross-sectional database containing information on districts' performance, districts' structure, judges' characteristics and locational attributes. We use data from 2010 to 2014 and take the average of annual measures for each judicial district.

The districts' performance and structure variables are constructed using data from the survey Open Justice (*Justiça Aberta*), published by the National Justice Council ((15)). It consists of a set of monthly reports on judges' and courts' performance filled by the judges. They cover all judges and courts

[1] Some States have special level districts and others have only unique level districts

in Brazil. In 2015, they were available online in PDF format. A dataset with information on Open Justice reports was provided to us by the National Justice Council.

We construct five performance variables: average annual number of processes allocated per judge; average annual number of processes allocated per 1000 inhabitants; average annual number of sentences per judge; average annual number of sentences per 1000 inhabitants; and average annual number of sentences per 100 processes allocated. They may be interpreted as measures of judicial demand, judges' productivity and judicial capacity of demand attendance in equilibrium, once they affect each other and are influenced by non-observed determinants. From the Open Justice we also calculate districts' number of courts, districts' number of judges, and districts' average number of courts worked per judge in one month and in one year. Additionally, we construct a dummy that equals 1 if the district has only courts with general functions and 0 if it has any specialized court, such as civil or criminal court.

The information on judges' characteristics are obtained from the Annual Report of Social Information, RAIS ((17)), a matched employer-employee data from Labor Ministry of Brazil. We use information on judges' age, gender and years of experience in the judicial system. In order to characterize the set of municipalities comprised in each district, we use the Census ((16)). We construct the following variables: population, average household per capita income, income inequality (ratio between 90th income percentile and 10th income percentile), population older than 23 years with high school degree, and urbanization rate. Finally, the list of municipalities grouped by judicial district were provided by (34) and the list of districts' level were obtained from the State courts websites.

We construct a cross-sectional data on districts' average annual measures. Our sample contains data for 2594 of the 2669 districts that existed in Brazil in 2012 ((34)). Most of the productivity variables exist for all of them and the judges' characteristics exist for around 1900 districts, because some information on the judicial system employees are not available in RAIS for part of the Northeast States. For those, we are unable to match the RAIS dataset with the Open Justice reports. Consequently, we use the full sample in most of the analyzes and use the small sample, with around 1900 districts, when judges' characteristics are included.

This data is exploited to study the determinants of judicial performance in Brazil. To the best of our knowledge, this is the first correlational analysis of a database with disaggregated information on judicial performance and structure of all Brazilian districts for an extensive period of time. Firstly, we report descriptive statistics of the judicial system characteristics and calculate the overall, within and between States standard deviations of the performance measures. Secondly, we present the spatial distribution of judicial of the five performance measures across Brazilian districts. Thirdly, we estimate linear regression models to study the correlation between local level of development and judicial performance. We estimate models as the following:

$$Y_{is} = \alpha + \beta' X_{is} + \gamma_s + \varepsilon_{is} \quad (1\text{-}1)$$

where Y_{is} is the log of the outcome variable of interest for district i in state s; X_{is} is the matrix of district's characteristics; γ_s is the state fixed effect; and ε_{is} is the error term. We estimate the models using Weighted Least Squares (WLS). Our regressions are weighted by the districts' average population. In addition, we estimate standard errors that are robust to heteroskedasticity.

The coefficients we estimate report the correlation between the dependent and the independent variable. It is not possible to interpret the results as causal relationships because of the existence of omitted variables that are correlated with the dependent and the independent variables and simultaneity between these variables. Consequently, it is important to reinforce that we aim at studying how judicial performance is correlated with local development and judicial structure.

1.4 Results

1.4.1 The Brazilian Judicial System characteristics

This paper exploits the Open Justice database in order to characterize the Brazilian Judicial System, focusing on the first instance of the State Justice. Our main measures of output are related to the number of sentences. The sentences are the decisions that conclude a case. The total number of sentences is a proxy for the number of processes ended. Initially, we observe that the number of sentences per process allocated varies significantly across districts. Table 1 shows that the average number of sentences per 100 process allocated is 53 per district. This variable is equal to 61 or more to the 25% more productive districts, and equal to 35 or less to the 25% less productive districts. A similar dispersion is observed in number of sentences per capita. They indicate that the Brazilian Judicial System is unable to handle its total demand, what generates a case backlog increase.

Judges' productivity have higher variability among courts. The average number of sentences per judge is 300, while the 25th percentile is 109 and the 75th percentile is 405. Similarly, the average number of processes allocated per judge is 683 and the standard deviation is also around 630. The number of processes allocated per judge is high in Brazil, which partially explains the justice slowness. (35) argues that the country has a high number of processes allocated per capita and also a high number of processes allocated per judge in comparison with other countries. Nevertheless, the number of processes allocated per judicial bureaucrats in Brazil are much lower than in other countries.

The judicial systems' structure are rather similar across districts. On average, they comprise 2 municipalities and a population of more than 80,000 inhabitants. They have 03 courts and 07 judges on average. Districts' population and resources are more disperse among their highest values. Analyzing judges' characteristics, we observe that most of the judges are male. In most of districts, judges' average experience equals 8 years or more and judges' average age is around 40 years old. Additionally, the statistics show that judges work in many courts at the same time. In most of districts, a judge work in 4 or more courts during one year on average. This is an important result and is a consequence of the high judicial demand in Brazil.

1.4.2 Determinants of judicial performance

As discussed before, output and demand measures vary importantly among districts. For this reason, we exploit the spatial heterogeneity in judicial system performance. Figures 1 to 5 show the locational distribution of judicial output and demand measures among Brazilian districts. The number of sentences per processes allocated is relatively high for most of South, Southeast and Center-West States and relatively low for most of North and Northeast States. Although, districts' capability of demand attendance vary significantly within States, such as in Bahia, Minas Gerais and Rio Grande do Sul.

Regional differences in the number of sentences per judge and in the number of sentences per capita are higher than in the number of sentences per processes allocated. Number of sentences per judge and number of sentences per capita are relatively high in South, Southeast and Center-West and relatively low in North and Northeast. Number of sentences per judge presents important dispersion within State. Their regional patterns are similar to regional patterns of other measures. Number of processes allocated per judge has high variability within State and presents higher values in South and Southeast than in other regions. In turn, number of processes allocated per capita in North and Northeast States clearly differs from other States.

In order to study the dispersion of judicial performance measures, we compute their total standard deviation, within State standard deviation and between State standard deviation. Table 2 shows that the within State variation is more accentuated than between State variation. The intra-State variability is specially higher than the inter-State variability for the number of sentences per 100 processes allocated, number of sentences per judge and number of processes allocated per judge. These results reinforce the role of districts' socio-economic characteristics and districts' structure on judicial performance, since the State courts are organized according to the same State Judicial Organization Law.

An analysis of determinants of judicial output is presented in Table 3 to Table 5. Firstly, we observe that number of sentences per processes allocated is positively correlated with average household per capita income in district. Educational level and urbanization rate are negatively correlated with number of sentences per processes allocated conditional on average income. On the one hand, these results indicate that income is more strongly correlated with number of sentences than with number of processes allocated, considering that income is positively correlated with both outcomes. On the other hand, these results indicate that, conditional on income, educational level and urbanization rate are less strongly correlated with number of sentences than with number of processes allocated, considering that educational level and urbanization rate are positively correlated with both outcomes.

As shown in columns 1 to 3 of Table 4, number of sentences per judge is positively correlated with districts' average income, level of education and urbanization rate. The specification in column 4 of Table 4 also controls for districts' level, districts' number of courts and the existence of non-general courts in district. In this specification, it is possible to observe that more productive judges are in highest level districts, in districts with more courts and in those with specialized courts. The positive correlation between number of sentences per judge and average income remains in this specification. However, educational level and income inequality are negatively correlated with judges' productivity in this model, while the coefficient of urbanization rate became statistically non-significant. These results indicate that most productive judges work in the highest level districts, which are the most developed in terms of income, education and urbanization, as shown in Table 9. These results are in line with the direct relationship between judges' career steps and districts' classification established in law as well as a process of the attraction of better bureaucrats due to municipal amenities. Table 5 presents similar pattern. We observe a positive correlation between sentences per 1000 inhabitants and local average household per capita income, districts' level and districts' number of courts as well as a negative correlation between outcome and income inequality.

The number of processes allocated is also correlated with local development. Columns 1 and 2 of Table 6 show that district's average number of processes allocated per judge is positively correlated with the local average household income and negatively correlated with local level of inequality. Column 3 shows that these first columns results are driven by the correlation between output and districts' average educational level and urbanization rate. The last regression of Table 6 presents a significant and positive correlation between number of processes allocated per judge and urbanization rate, court level and the existence of specialized courts. Additionally, it presents a negative correlation between income inequality and outcome. As shown in Table 7, number of processes allocated per capita have similar relationship with local characteristics.

Table 8 shows that there is a selection of more experienced judges in third and special level districts as well as in those with more courts, which comprise municipalities with highest income levels, educational levels and urbanization rates. We observe a positive correlation between local income and educational levels and average judges' experience even controlling for districts' judicial structure.

The selection of more experienced judges in highest level districts are in line with judges' career steps. When the judges are promoted, they work in a higher level district and receive a wage increase. The promotion process is based on merit criteria, related to decisions' quality and productivity, and experience criteria. Consequently, the most productive and experienced judges are allocated in highest level districts. Given that districts' classification criteria are functions of local judicial demand predictors, like population, highest level districts are the most developed ones, as shown in Table 9. Table 9 also shows that the most developed districts have a higher number of courts and have more frequently specialized courts, factors also positively correlated with number of sentences, as presented in Tables 4 and 5. In addition, local amenities may atract better bureaucrats and the productivity in those places may also be improving through peer effect.

1.5

Conclusion

This paper studies the determinants of judicial performance and exploits a novel database on judicial system features. We use information on districts' performance and structure from 2010 to 2014 taken from the Open Justice system ((15)), on judges' characteristicts taken from RAIS ((17)), and municipal data from the Census 2010 ((16)). The analysis' focus is the correlation between local level of development and judicial performance.

The Brazilian judicial system is characterized by a low number of concluded processes in comparison with the number of processes allocated. Judicial performance differs significantly among Brazilian courts, specially within States. We verify that the most developed districts in terms of income, education and urbanization have a higher number of processes allocated per capita and per judge as well as a higher number of sentences per capita and per judge. In addition, the districts with higher income levels have higher number of sentences per processes allocated. In a similar way, we find that most of the judicial performance measures are higher in the highest level districts and in districts with a higher number of courts and with specialized courts. The highest level districts are also the most developed ones due to the fact that the district level classification criteria are based on predictors of the demand for judicial services, such as population.

The positive correlation between districts' performance and their classification and level of development are related to three central features of judicial system. First, the judges' promotion process, that is based on merit and experience criteria. As a consequence, there is a selection of more productive and experienced judges in highest levels districts, as showed in this study. Second, the attraction of better bureaucrats in more developed areas due to amenities. The concentration of more productive workers in these places may also increase the judicial system performance through peer effect. Third, the existence of specialized courts as well as a higher number of courts in the most developed localities, factors also correlated with districts' performance. The found relationships are consistent with districts' classification rules and criteria of judges' promotion. However, we are unable to confirm they are causal relationships, since our sources of variation are endogenous.

Table 1.1: Descriptive statistics

Variable	All Municipalities	Small Municipalities	Medium and large Municipalities
Female domestic homicide rate	1.3	1.3	1.3
	[6.1]	[6.3]	[1.4]
Male domestic homicide rate	4.2	4.2	5.6
	[10.7]	[10.9]	[4.3]
Female homicide rate	3.3	3.3	4.8
	[9.5]	[9.6]	[3.5]
Male homicide rate	27.1	25.6	59.7
	[35.4]	[34.3]	[41.5]
Existence of domestic violence court 2009	7.0	6.5	18.3
	[25.5]	[24.6]	[38.7]
Existence of women's police station 2009	7.1	3.8	76.4
	[25.7]	[19.2]	[42.5]
Women's years of education	4.1	4.0	6.7
	[1.3]	[1.1]	[1.3]
Proportion of women in work	28.4	27.8	39.7
	[9.7]	[9.6]	[5.7]
Women's wage	136.9	131.0	259.0
	[77.6]	[72.4]	[81.9]
Proportion of divorced women	4.9	4.7	9.6
	[3.0]	[2.9]	[2.3]
Proportion of population aged 0-29	58.7	58.7	57.8
	[6.5]	[6.6]	[5.2]
Municipalities	5568	5314	254

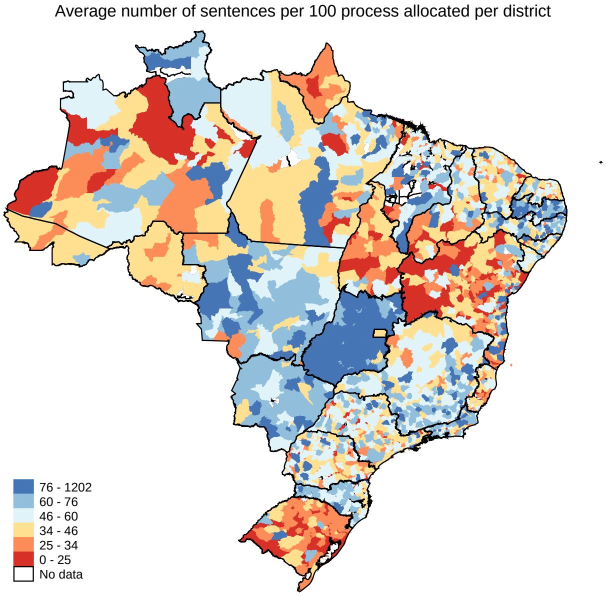
Notes: Statistics from the sample containing only district annual averages.

Table 1.2: Judicial performance measures standard deviations - within and between States

	SD	Between SD	Within SD
Sentences per 100 process allocated	41.3	14.5	37.9
Sentences per judge	282.7	142.3	229.7
Sentences per 1000 inhabitants	22.8	16.7	16.9
Processes allocated per 1000 inbabitants	45.7	33.8	34.2
Processes allocated per judge	632.1	357.2	488.7

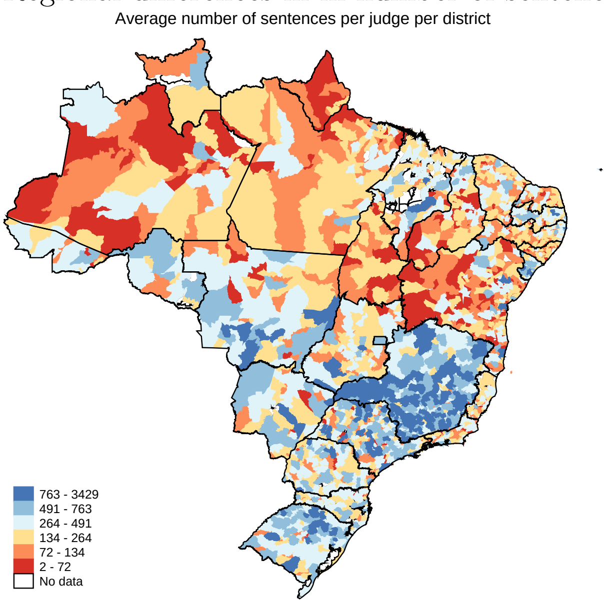
Notes: Statistics from the sample containing only district annual averages.

Figure 1.1: Regional differences in number of sentences per process allocated



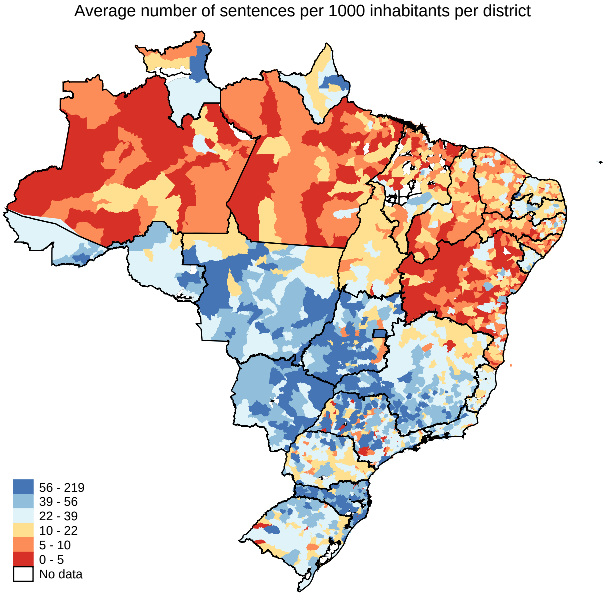
Notes: Statistics from the sample containing only district annual averages.

Figure 1.2: Regional differences in in number of sentences per judge



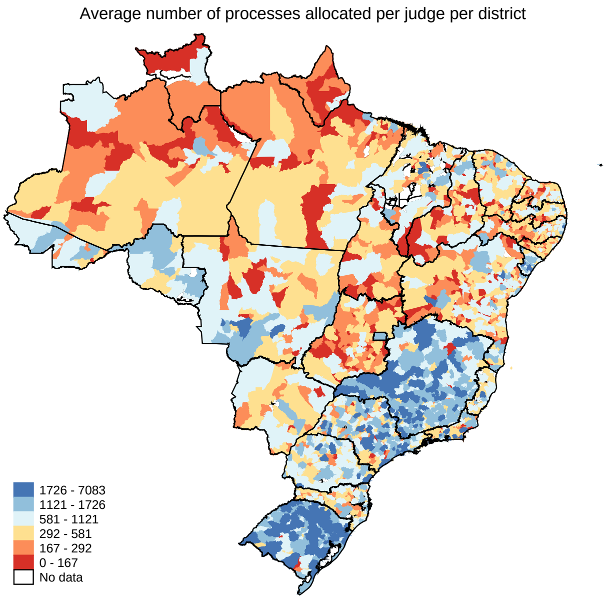
Notes: Statistics from the sample containing only district annual averages.

Figure 1.3: Regional differences in in number of sentences per 1000 inhabitants



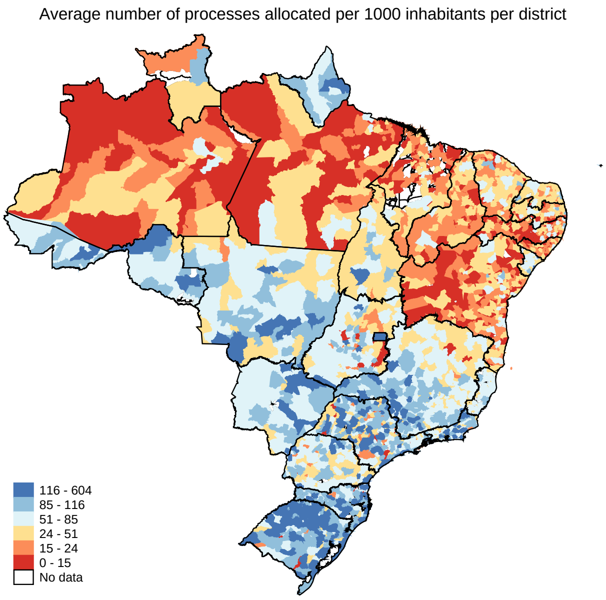
Notes: Statistics from the sample containing only district annual averages.

Figure 1.4: Regional differences in number of sentences per processes allocated per judge



Notes: Statistics from the sample containing only district annual averages.

Figure 1.5: Regional differences in number of sentences per processes allocated per 1000 inhabitants



Notes: Statistics from the sample containing only district annual averages.

Table 1.3: Determinants of districts' average number of sentences per processes allocated

	Log sentences per 100 process allocated			
	(1)	(2)	(3)	(4)
Log average household per capita income 2010	0.109***	0.105***	0.345***	0.295***
	(0.0420)	(0.0377)	(0.0872)	(0.0894)
Log inequality (p90/p10)		0.0609*	0.0395	0.0204
		(0.0311)	(0.0284)	(0.0235)
Percentage with high school completed 2010			-0.00661**	-0.00943***
			(0.00299)	(0.00303)
Urbanization rate 2010			-0.00267**	-0.00292***
			(0.00105)	(0.00108)
2nd level court				0.0319
				(0.0320)
3rd level court				0.0252
				(0.0542)
Special level court				-0.0525
				(0.100)
Log number of courts				0.0356
				(0.0252)
Only general courts				0.00227
				(0.0324)
Observations	2586	2586	2586	2462
Adjusted R^2	0.317	0.322	0.335	0.345
State FE	YES	YES	YES	YES

Notes: Statistics from the sample containing only district annual averages. Regressions weighted for average district population. Standard errors clustered at district level in parenthesis. Significance levels: * 10%, ** 5%, *** 1%

Table 1.4: Determinants of districts' average number of sentences per judge

	Log sentences per judge			
	(1)	(2)	(3)	(4)
Log average household per capita income 2010	0.897***	0.897***	0.476***	0.310***
	(0.0473)	(0.0468)	(0.0926)	(0.0869)
Log inequality (p90/p10)		-0.00156	0.0419	-0.0632**
		(0.0274)	(0.0296)	(0.0247)
Percentage with high school completed 2010			0.0107***	-0.00717**
			(0.00345)	(0.00342)
Urbanization rate 2010			0.00548***	-0.000428
			(0.00138)	(0.00128)
2nd level court				0.463***
				(0.0432)
3rd level court				0.615***
				(0.0661)
Special level court				0.604***
				(0.124)
Log number of courts				0.116***
				(0.0240)
Only general courts				-0.143***
				(0.0477)
Observations	2593	2593	2593	2469
Adjusted R^2	0.509	0.509	0.522	0.618
State FE	YES	YES	YES	YES

Notes: Statistics from the sample containing only district annual averages. Regressions weighted for average district population. Standard errors clustered at district level in parenthesis. Significance levels: * 10%, ** 5%, *** 1%

Table 1.5: Determinants of districts' average number of sentences per capita

	Log sentences per 1000 inhabitants			
	(1)	(2)	(3)	(4)
Log average household per capita income 2010	0.975***	0.980***	0.853***	0.680***
	(0.0546)	(0.0521)	(0.0873)	(0.0961)
Log inequality (p90/p10)		-0.0785***	-0.0611**	-0.128***
		(0.0268)	(0.0263)	(0.0289)
Percentage with high school completed 2010			0.00257	-0.00695
			(0.00342)	(0.00465)
Urbanization rate 2010			0.00223*	-0.000584
			(0.00134)	(0.00140)
2nd level court				0.188***
				(0.0511)
3rd level court				0.294***
				(0.0839)
Special level court				0.101
				(0.170)
Log number of courts				0.101**
				(0.0398)
Only general courts				-0.0511
				(0.0444)
Observations	2593	2593	2593	2469
Adjusted R^2	0.598	0.601	0.602	0.635
State FE	YES	YES	YES	YES

Notes: Statistics from the sample containing only district annual averages. Regressions weighted for average district population. Standard errors clustered at district level in parenthesis. Significance levels: * 10%, ** 5%, *** 1%

Table 1.6: Determinants of district average number of processes allocated per judge

	Log processes allocated per judge			
	(1)	(2)	(3)	(4)
Log average household per capita income 2010	0.745***	0.750***	-0.0278	-0.0833
	(0.0561)	(0.0507)	(0.105)	(0.121)
Log inequality (p90/p10)		-0.0835**	-0.00119	-0.0675**
		(0.0355)	(0.0278)	(0.0274)
Percentage with high school completed 2010			0.0194***	0.00609
			(0.00368)	(0.00413)
Urbanization rate 2010			0.0104***	0.00462***
			(0.00158)	(0.00150)
2nd level court				0.457***
				(0.0399)
3rd level court				0.610***
				(0.0703)
Special level court				0.666***
				(0.123)
Log number of courts				0.0461
				(0.0353)
Only general courts				-0.181***
				(0.0486)
Observations	2587	2587	2587	2463
Adjusted R^2	0.490	0.493	0.540	0.623
State FE	YES	YES	YES	YES

Notes: Statistics from the sample containing only district annual averages. Regressions weighted for average district population. Standard errors clustered at district level in parenthesis. Significance levels: * 10%, ** 5%, *** 1%

Table 1.7: Determinants of district average number of processes allocated per capita

	Log processes allocated per capita			
	(1)	(2)	(3)	(4)
Log average household per capita income 2010	0.804***	0.813***	0.339**	0.292**
	(0.0784)	(0.0672)	(0.137)	(0.131)
Log inequality (p90/p10)		-0.154***	-0.0986**	-0.123***
		(0.0484)	(0.0384)	(0.0335)
Percentage with high school completed 2010			0.0109***	0.00614
			(0.00399)	(0.00534)
Urbanization rate 2010			0.00713***	0.00430**
			(0.00181)	(0.00170)
2nd level court				0.175***
				(0.0458)
3rd level court				0.287***
				(0.0837)
Special level court				0.180
				(0.155)
Log number of courts				0.0266
				(0.0552)
Only general courts				-0.0940**
				(0.0440)
Observations	2587	2587	2587	2463
Adjusted R^2	0.583	0.593	0.612	0.645
State FE	YES	YES	YES	YES

Notes: Statistics from the sample containing only district annual averages. Regressions weighted for average district population. Standard errors clustered at district level in parenthesis. Significance levels: * 10%, ** 5%, *** 1%

Table 1.8: Relationship between districts' characteristics and average judges' experience

	Average experience			
	(1)	(2)	(3)	(4)
Log average household per capita income 2010	6.605***	6.589***	2.413***	1.567***
	(0.356)	(0.347)	(0.801)	(0.545)
Log inequality (p90/p10)		0.150	0.368	-0.448***
		(0.200)	(0.239)	(0.125)
Percentage with high school completed 2010			0.144***	0.0430**
			(0.0254)	(0.0184)
Urbanization rate 2010			0.0244**	-0.00616
			(0.00953)	(0.00842)
2nd level court				0.462*
				(0.244)
3rd level court				3.379***
				(0.409)
Special level court				5.380***
				(0.880)
Log number of courts				0.562***
				(0.114)
Only general courts				0.537
				(0.417)
Observations	1922	1922	1922	1799
Adjusted R^2	0.541	0.541	0.575	0.668
State FE	YES	YES	YES	YES

Notes: Standardized dependent variable. Standard errors clustered at district level in parenthesis. Significance levels: * 10%, ** 5%, *** 1%

Table 1.9: Relationship between districts' characteristics and judicial structure

	District's level			Number of courts			Only general courts		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log average household per capita income 2010	1.367***	1.365***	-0.165	3.409***	3.375***	1.802***	-0.225***	-0.227***	-0.132***
	(0.0699)	(0.0703)	(0.131)	(0.201)	(0.154)	(0.507)	(0.0223)	(0.0188)	(0.0280)
Log inequality (p90/p10)		0.0320	0.153***		0.574***	0.660***		0.0673***	0.0406***
		(0.0330)	(0.0335)		(0.164)	(0.190)		(0.0126)	(0.0127)
Percentage with high school completed 2010			0.0447***			0.0520***			0.000464
			(0.00463)			(0.0148)			(0.00140)
Urbanization rate 2010			0.0148***			0.00993***			-0.00365***
			(0.00178)			(0.00303)			(0.000662)
Observations	2593	2593	2593	2593	2593	2593	2469	2469	2469
Adjusted R^2	0.562	0.562	0.661	0.756	0.782	0.807	0.292	0.308	0.325
State FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Standardized dependent variable. Standard errors clustered at district level in parenthesis. Significance levels: * 10%, ** 5%, *** 1%

2
The impact of judicial performance on violent crimes

2.1
Introduction

The improvement of legal capacity has a central role on the development process, once it affects the degree to which the State is able to protect property rights and enforce contracts, stimulating economic transitions, fostering credit markets and promoting investment, competition and firm growth ((1); (4); (5); (32); (11); (14); (36)). In parallel, legal capacity may affect crime through increased punishment probability or the intensification of severity. This paper studies the relationship between legal capacity and violence in Brazil. The country registered 59,627 homicides in 2014, approximately 10% of the world total ((37); (38)). The Brazilian context is similar to that of other developing countries, especially those in Latin America. In 2012, the homicide rate was 25.2 in Brazil, 23 in South America and 6.2 in the world according to (39)[1]. In contrast with an elevated magnitude of violent crimes, Brazil has an enormous and expensive judicial system. Its maintenance costs the equivalent to 1.2% of national GDP, while in Argentina, Chile, USA, England, and Germany this relationship varies between 0.13 and 0.32% ((35)). Despite the important allocation of resources, justice is unable to meet society's demands. In 2014, only 20% of the total number of cases were concluded ((31)). Regarding criminal justice, only 8% of homicides in the country are solved ((42)). In addition, the system is heterogeneous with a high variability among local courts in terms of efficiency, even when considering units in the same state ((32)). The perception of more than half of the population is that it is easy to disobey the law and there are few reasons for respecting it. Most Brazilians do not trust the justice system and consider going to court to be expensive ((43)).

This paper aims at testing whether legal capacity, measured as judicial performance, has a significant impact on homicide rates. Firstly, we analyze

[1]According to Atlas da Violência ((37)), (40) and (41), the homicide rate in Brazil in 2012 was 28.3 per 100,000 inhabitants. These are the sources for the homicides rates used in this paper. We referred to UNODC data in order to use a unique reference to compare the rates among countries.

variations in judicial features and homicide rates among similar municipalities classified as first and second level judicial districts. We estimate the district classification effect on judicial performance, judicial structure and homicide rates employing a RDD approach. Since the district classification is not a deterministic function of the number of voters, we obtain an intention-to-treat (ITT) estimator. Secondly, using a fuzzy RDD, we estimate the potential impact of judicial performance on homicide rates, exploiting the discontinuity in district classification criteria.

We exploit the classification of judicial districts in Brazil. The districts are judicial units and can comprise one or more municipalities, which can have one or more courts. The districts are classified as first, second or third level according to criteria based on effective or potencial demand for judicial services [2]. As showed in the first chapter, the highest level districts have more experienced and productive judges, a higher number of courts and a higher probability of having specialized courts. The concentration of better judges in highest level districts, which are also the most developed ones in terms of income, education and urbanization [3], is potentially explained by judges' self-selection and promotion features. The judges' career stages coincide with district classification. Once promoted, based on merit or experience criteria, a judge works in higher level districts and receives higher wages. By contrast, there are no differences in judicial structure due to district classifications determined by the law. The observed differences in number and type of courts are potentially driven by endogenous responses to demand pressures.

The district levels are determined by different variables and thresholds in each State. The rules are usually discontinuous functions of the number of voters, population, taxes, number of processes allocated and other predictors of demand for judicial services. Most of the states include criteria based on number of voters and population. We tested both as predictors of the district level elevation individually in an unidimensional RDD and together in a multidimensional RDD. The number of voters predicts better the district level than the population variable. In turn, the multidimensional RDD estimates presented an elevated variance. Consequently, we exploit the discontinuity in the number of voters brought about by each state's rules of district classification. We find that the number of voters significantly increases the probability of a district being classified as second level instead of first level for the states we analyzed: São Paulo, Ceará and Sergipe[4]. For the other States,

[2]Some States have four district categories and others have only one category.

[3]As showed in chapter one

[4]The district must have at least 50000 voters in São Paulo (SP), 12500 in Ceará (CE) and 24500 in Sergipe (SE) to be classified as second level. The classification rule of São Paulo

number of voters and population are poor predictors of treatment.

We construct a novel database on judicial system performance and structure, homicides rates and municipal characteristics - a panel with annual observations per district from 2009 to 2013. We use as main source of data the survey Justiça Aberta (*Open Justice*), managed by the National Justice Council ((15)), which consists of a set of montlhy reports on judges' and courts' performance available online in PDF format. The dataset with information on all reports from 2009 to 2014 was provided to us by the National Justice Council. We exploit the annual number of sentences, sentences per judge and sentences per process allocated per district as performance measures. We use data on homicides and population from DATASUS, a databank maintained by the Ministry of Health ((40)). We calculate the local homicide rate per 100,000 inhabitants.

Our results indicate that an increase in legal capacity significantly reduces homicides. Firstly, we find that crossing a state's voter threshold raises by 45% the probability of a district being classified as second level instead of first level. Simultaneously, our estimates show an increase from 60% to 80% in the number of sentences and in the number of sentences per judge at the cutoff, as well as an imprecise 20% increase in the number of sentences per process allocated to each district. The reduction in district homicide rates and districts' seat homicide rates is approximately 60% at the threshold. We estimate that an increase in judicial performance reduces homicide rates, where a 1% increase in the number of sentences or in the number of sentenced per judge reduces homicide rates by 0.95% and 1.2%, respectively. Although less precise, a 1% growth in the number of sentences per process is associated with a reduction of more than 2.2% in homicide rates. The productivity increase is potentially promoted by the concentration of more experienced and more productive judges in second level districts, since we observe an increase in the number of sentences per judge and an imprecise raise in judges' experience levels at the threshold. The existence of specialized courts may also be a determinant of the differences judicial performance across districts.

To our knowledge, this is the first impact estimate of judicial performance on homicide rates. According to the seminal model of (6), crime is a rational decision based on predictions of benefits and losses. In line with this framework, many studies demonstrate that an increase in the probability of punishment reduces crimes ((12); (44); (13); (8); (45); (46); (7)). Most of them focus on the

takes into account the number of voters and the average number of processes allocated to each district in the last five years, while rules in Sergipe (SE) and Ceará (CE) take into account the number of voters, the number of processes allocated to each district and the population.

impact of police instead of justice. The effect of sanction severity on crimes has been considered significant in some analyses and nonsignificant in others ((47); (48); (49)). Probably due to the lack of data and identification strategies, the effectiveness of the judicial system in reducing crimes is unknown. According to the (6) model, it may affect criminality through an increase in the probability of punishment and severity changes. There are also few analyses of policy impacts on violence for developing countries and of policies that can improve the judicial system. This paper shows the high influence of justice on violence and reinforces the role of the criminal system, discussed in many empirical and theoretical studies focused mostly on police action. Our findings offer an alternative to police strategies in fighting crime, shedding light on the effectiveness of the judicial system and stimulating efforts to increase judicial productivity.

This paper is organized as follows: Section 2 describes the Brazilian judicial system and discusses the violence problem. Section 3 introduces the data and summary statistics. Section 4 explains the empirical strategy. Section 5 presents our results, and Section 6 concludes.

2.2 The Brazilian judicial system

2.2.1 Judicial organization

This paper analyzes the impact of increases in judicial performance in homicide rates. We focus on first and second level districts of first instance state courts. The first instance of a state is the most important part of the Brazilian judicial system in terms of allocated processes. In 2014, it heard 62% of the cases of the Brazilian judicial system and was responsible for 79% of the national backlog of pending cases ((31)). Most of the processes were evaluated solely at this instance, among the processes that could be revised only 8.2% were appealed against ((31)).

According to the Constitution of 1988, the Brazilian judicial system consists of the following bodies: Federal Justice (*Justiça Federal*), Labor Justice (*Justiça do Trabalho*), Electoral Justice (*Justiça Eleitoral*), Military Justice (*Justiça Militar*), State Military Justice (*Justiças Militares Estaduais*) and State Court (*Justiças Estaduais Ordinárias*). They are usually classified as Special Justice (*Justiça Especial*) and Ordinary Justice (*Justiça Comum*). Special Justice courts are responsible for specific matters established by the Constitution and is composed by the Labor Justice, the Electoral Justice, the Military Justice and the State Military Justice. The Ordinary Justice is responsible for crimes against life, among other matters, and is composed by the Federal Justice and State Courts. The Federal Justice has the jurisdiction to hear cases involving the Federal Government and some specific agents, while State Courts have the jurisdiction to hear other cases ((33)).

State Courts are organized based on the Federal and State Constitutions, on the Organic Law of National Magistrates (*Lei Orgânica Nacional da Magistratura Nacional*) and on States' Judicial Organization Laws (*Lei de Organização Judiciária*). State Courts are divided into first and second instances, where the latter deals with appeals and is responsible for managing those courts ((33)). Most cases are exclusively allocated to the first instance of State Courts, which are the ones we analyze in this study. The first instance of State Courts is divided into districts (*comarcas*), which can comprise one or more municipalities. These districts are classified as first, second or third level[5], according to state rules. They are determined by the State Judicial Organization Law and are usually based on a discontinuous function of the number of voters in the district as well as on other variables that reflect local judicial demand.

The classification of districts coincides with judges' career stages: when a judge is hired, she works as a substitute judge for approximately 2 years; then, she is allocated to a first level district; if promoted, she is allocated to a second level district; and so on. Promotions are based on experience or productivity/quality criteria. Therefore, second level districts usually have more experienced/productive judges than first level districts. They can also have more judges and courts due to a larger number of processes allocated to those districts. These differences are presented in the next sections.

2.2.2
Justice quality and violence

The Brazilian judicial system is expensive and inefficient, characterized by lengthy trials ((50); (51)). In 2004, the cost of the Brazilian and the state judicial system was approximately 31 and 17 billion dollars, respectively[6]. The judicial structure in the country accounts for 2.3% of total public expenditure, while that of states' judicial systems account for 5.2% of the sum of states' public expenditures ((31)). The national cost corresponds to 1.2% of national GDP. In other countries, the cost of the judicial system as a per-

[5]The number of categories varies from 1 to 4 between states. In analyzed states, there are 3 districts' categories, as most of the Brazilian states. Most of the districts are classified as first or second levels.

[6]Equivalent to 68.4 billion and 37.6 billion Reais, respectively. The amounts in dollars were calculated based on the exchange rate in 06/30/2014.

centage of the GDP is usually lower - around 0.13% in Argentina, 0.14% in USA and England, 0.22% in Chile and 0.32% in Germany. Most of the judicial budget, approximately 90%, are allocated to human resources, mainly to public servants. Compared to other countries, Brazil has a similar number of judges per 100,000 inhabitants, 8.2, while England has 3.8, Chile, 5, USA, 10.8, Argentina, 11.4 and Germany 24.7. On the other hand, Brazil has a high number of justice servants per 100,000 inhabitants, 205, while England has 30.6, Chile, 42.1, Germany, 66.9 and Argentina, 150 ((35)). The total number of cases in justice in the country is also large, 93 million, corresponding to more than 6,000 processes per judge and 0.5 per inhabitant in 2013 ((35)).

The amount of processes affect the perception of judicial efficiency among judges, judicial servants and the population in general. The Judiciary Census ((52)) showed that 84% of judges consider their workload to be high and impossible to handle within their working time, but 70% are satisfied with services offered to citizens. The same survey showed that 48% of justice servants consider their workload high and impossible to handle within their working time, but 80% are satisfied with services offered to citizens. According to the survey Justice Confidence Index (*Índice de Confiança na Justiça*, (43)), more than half of the people do not trust the justice system and consider going to court to be expensive. According to this index, more than 70% of the Brazilians disagree that the judicial system is honest while 96% disagree that it is swift. More than 80% of the sample say that it is easy to disobey to the law and 57% believe that there are few reasons for respecting the law ((53)).

The first instance of state courts would spend around 5 years in order to end the backlog of pending cases, based on the current judicial productivity statistics published by the National Council of Justice (*Conselho Nacional de Justiça, CNJ*). In 2014, the congestion rate was 80%, calculated as the total percentage of open cases ((31)). The number of cases per judge in that instance was 7,200 in Brazil and 11,300 in São Paulo, while the number of sentences per judge was 1,300 in Brazil and 1,600 in São Paulo. Despite the high congestion rate, the first instance of state courts concluded almost the same number of new cases in 2014, 99% precisely, which means that the backlog remained nearly stable.

In particular, the Brazilian Criminal Justice has serious investigative limitations and fails in incarceration processes. Only 8% of the homicides in Brazil are solved and most of them expire after a 20-year time limit ((42)). In 2014, Brazil had 579,423 prisoners, 401 per 100,000 inhabitants over 18 years old. Among them, 38% were awaiting trial ((54)).

Homicide rates in Brazil remained stable, despite being high, at around

26.5 from 2004 to 2011. It has grown since 2012 and was 29.1 per 100,000 people in 2014 ((37)). There is a spatial redistribution of these crimes with an increase in homicide rates in cities in the interior, these that do not belong to Metropolitan Regions ((42)). Cities with a population below 100,000, which means 94.2% of the Brazilian municipalites, had an increase of more than 40% in homicide rates between 2000 and 2010, while the others had a decrease in this period ((42)).

(44) argue that the incidence of crime in Latin America is expected to be high based on its socioeconomic and public policy characteristics, especially high inequality, the proportion of young people in the population, low incarceration rates and small police forces. According to (55) the main causes of homicide in the country are: poverty and income inequality, the proportion of young men in the population, the criminal justice system, the use of legal and illegal drugs, and the possession of weapons.

2.3
Data and summary statistics

We constructed a novel panel on judicial performance, homicide rates and municipal characteristics with judicial district level data from 2009 to 2013. The main data source is called Open Justice (*Justiça Aberta*), a database managed by the National Justice Council ((15)). This system allows individuals to consult online monthly reports on judges' and courts' productivity in PDF format from 2009 onward. A dataset with information on all Open Justice reports from 2009 to 2014, originally covering all judges and courts in Brazil, was exclusively provided to us by the National Justice Council. District level information and States' Judicial Organization Laws including district classification criteria are available on State Courts' websites. Contract features of judges are taken from the Annual Report of Social Information, RAIS ((17)), an annual administrative survey of the Labor Ministry of Brazil with detailed individual and firm information, like salaries, gender, race, age, education and occupation. Data on homicides was taken from DATASUS, a Ministry of Health database ((40)). To characterize the municipalities, we utilize the Census ((16)) and the Munic survey ((56)), published by the Brazilian Institute of Geography and Statistics (IBGE). The number of military and civilian police for São Paulo State is provided by Secretaria (57). The number of voters per municipality is provided by the Superior Electoral Court ((58)). Finally, the list of municipalities grouped by judicial district is used in (34) and was provided to us by the author.

In order to describe judicial resources, we calculate the number of judges, courts and municipalities per district as well as judges' average experience and gender distribution, using the Open Justice database, a list of municipalities per district and RAIS. From the Open Justice database, we construct the following judicial performance measures per district and year: number of processes allocated, number of sentences, number of sentences per judge and number of sentences per process allocated. The number of processes allocated is a proxy for judicial demand. The number of sentences is our output measure and is very informative once they are the decisions that conclude a case[7]. The sentences per judge are a type of output/input index and partially reflect judiciary efficiency. Finally, the sentences per process allocated to a court are a type of output/demand index and partially reflect response capacity. Since these variables affect each other and are also determined by unobserved factors, they may be interpreted as equilibrium outcomes. They are used in official reports developed by the National Justice Council.

In turn, district and municipal annual homicide rates are calculated based on DATASUS/Ministry of Health data and population estimates published by IBGE. They are the local number of homicides per 100,000 inhabitants. To construct local socio-demographic variables, we use the Census 2010 and calculate for district seats: the percentage of urban population, the proportion of the population over 18 years old who completed high school, the proportion of the population under 18 years old, the Gini coefficient, and average household income per capita. The district number of voters is the sum of the number of voters in municipalities in 2009, the first year we analyzed. To test for sudden increases in public security resources at the discontinuity, we use the Munic 2006 database and calculate the following for districts' seats and for all district municipalities: number of municipal police per 100,000 inhabitants and per capita public security expenditure. Additionally, we test if the number of military and civilian police per 100,000 inhabitants present a discontinuous variation at the threshold, considering measures for districts' seats and for all district municipalities. Information on military and civilian police is only available for São Paulo State.

This paper exploits the discontinuity in district level classification rules based on the number of voters, despite the existence of other determinants for each State, like population, taxes and number of processes allocated to courts. The sample contains three States for which the discontinuity in voters determines a significant increase in the probability of being classified as second level district instead of first level: São Paulo, Ceará and Sergipe. Their first

[7]Each case can be decided by one or more sentences.

and second level districts served 649 municipalities and more than 16 million people in 2009, 11.7% and 8.6% of the country's municipalities and population, respectively. In order to be classified as second level districts, the district must have at least 50,000 voters in São Paulo (SP), 12,500 in Ceará (CE) and 24,500 in Sergipe (SE). Table 2.1 shows State thresholds and sample information.

Descriptive statistics by district level and the percentage distance to the threshold for judicial performance and structure, seat characteristics, homicide rates, and public security resources are shown in tables 2.2 to 2.4. Differences in averages between second and first level districts are calculated with clustered standard errors at the district level. The average number of processes allocated is much higher in second level districts (8226) than it is in first level districts (1830), which is also true for the average number of sentences, around 3400 for second level districts and 800 for first level districts. The absolute difference decreases when restricting the data to those observations in which the percentage distance to states' voter thresholds is lower than 40% (smallest sample), but still remains very high. The mean number of sentences per judge is around 340 for second level districts (full sample and smallest sample). It is lower for first level districts, 292 in the full sample and 214 in the smallest sample. The mean number of sentences per process allocated is similar for first and second level districts. It is around 0.5 for both levels in the full sample, remains the same in the smallest sample for level 1 districts, and equals 0.58 in the smallest sample for level 2 districts. Second level districts seem to have more resources (3.8 courts and 8.8 judges on average) and a higher variability in that number than first level ones (1.3 courts and 4.1 judges on average), but they usually have a similar number of municipalities (approximately 2). First level judges usually have around 5 years' experience, while second level judges usually have more than 6 years. Also, more than 65% of judges are men.

Differences in homicide rates are very sensible to sample restrictions, reflecting the positive correlation between homicides and number of voters and the potential reversion of this trend around the threshold due to increases in judicial performance, the main hypothesis we test in this paper. District homicide rates are higher for second level districts (19.8) than they are for first level ones (17.6) in the full sample. In the sample closest to the threshold, rates are lower for second level districts (17.4) than they are for those of first level (20.5). The same trend is observed in seat homicide rates, around 20 for both groups in the full sample and equal to 17.6 for districts of second level and 21.6 for first level ones in the smallest sample.

Regarding the local characteristics, we observe some significant differences between first and second level districts. Seat urbanization rates are 83% for second level districts and 78% for first level districts, proportions of high school graduates are 36% and 31%, respectively, and average household incomes per capita are 672 and 597 Reais. The Gini coefficient is around 0.50 and the proportion of people under 18 years old is 29% for the whole sample. The number of police officers per 100,000 inhabitants is similar between first and second districts, especially in the smallest sample. The same occurs for per capita public security expenditure. Smoothness tests are shown in tables 2.15 to 2.18 and discussed in results section.

2.4 Empirical Strategy

We employ a regression discontinuity approach to estimate the impact of district level classification on judicial performance and its effect on homicide rates. District level classification criteria are determined differently for each State. The Brazilian Constitution says that it must consider predictors of the demand for judicial services, such as local population, number of voters, number of processes allocated, district area, and taxes. Most states determine the rule as a discontinuous function of a subgroup of those variables. For example, to be classified as second level in São Paulo (SP), a district must have at least 50,000 voters and 7,000 processes allocated to its courts on average in the last 5 years. Sergipe (SE) and Ceará (CE) take into account number of voters, number of processes allocated and population. Determinants and thresholds vary from State to State and are explained in each state's Judicial Organization Law. We have chosen a single-dimensional RD instead of a multidimensional RD due to its well-known inference properties and to increase the sample comparability considering the same factor for the whole sample. Number of voters is one of the most frequent determinants and is pivotal in some States, while some of the other determinants are unknown/noisy or are not themselves important determinants of the treatment. For example, in São Paulo (SP), the state where demand for judicial services is greater in Brazil, there is no population criteria and we do not have access to the average number of processes allocated in the last 5 years. The number of voters in a district is a very good predictor of treatment in São Paulo, Ceará and Sergipe, what explains the choice of those states. Since the probability of treatment - being classified as a second level district instead of a first level one - increases around states' voter thresholds, our basic model is defined as follows:

$$Y_{it} = \alpha + \beta' 1_{assignment} + f(Voters_i) + \gamma_t + \varepsilon_{it} \tag{2-1}$$

where Y_{it} is the log of the outcome variable of interest for district i in year t; $1_{assignment}$ is an indicator function of whether the number of voters in district i in 2009 was greater than or equal to the state's voter threshold; $f(Voters_i)$ is a function of the percentage difference between the number of voters in a district and the state's threshold; and γ_t is the year fixed effect. Despite the fact that year fixed effects are unnecessary for identification, we include them to increase precision ((59)). Our main specifications are estimated without controls, but we obtain similar results including them, as shown in Appendix[8]. The coefficient of interest is β, which estimates the effect of satisfying the voter condition on the outcomes.

We consider parametric and non-parametric functions of the percentage difference between the district number of voters and the threshold. Parametric specifications include quadratic and cubic splines, shown in the main tables, with standard errors clustered at the district level. Since mistakes in functional forms may lead to biased estimates, we exploit non-parametric specifications using local linear regressions ((60), (61)). They are estimated applying a kernel function on the distance of the number of voters to the state's threshold. In the main tables, we show the triangular kernel estimators and in the appendix we show similar rectangular kernel estimators. We estimate regression discontinuity bias-corrected coefficients and robust clustered standard errors at the district level, presented in (62). These are more robust to bandwidth choices and valid under conditions weaker than conventional. The authors present confidence intervals based on fixed-matches estimated errors, exploiting the 3 nearest neighbors of each observation. Since these results have very low standard errors for our database [9], we show a more conservative option: the fixed-matches estimated errors exploiting the 5 nearest neighbors of each observation. Results with the original standard errors are presented in the appendix.

Parametric regressions are estimated for the whole sample, first and second level districts, at each State. Non-parametric regressions are estimated for a broad range of bandwidths: 40, 60, 80, the Imbens-Kalyanaraman (IK) optimal bandwidth and the Calonico-Cattaneo-Titiunik (CCT) optimal bandwidth ((64) and (62)). The optimal bandwidth selection procedure is implemented using the (65) Stata package [10], and calculated based on specifications without

[8] We use as controls: proportion of seat population who live in urban areas, log of seat average household income per capita, Gini coefficient, percentage of people under 18 years old, proportion of adults (over 18) who completed high school.

[9] They have been widely used, like in (63).

[10] Available on May 2016 at http://www-personal.umich.edu/ cattaneo/software/rdrobust/stata

year fixed effects and clustered standard errors.

Once the treatment is not a deterministic function of the running variable, the reduced-form coefficient of interest, as described in equation 1, is the intention-to-treat (ITT) estimator. In order to estimate the effect of an increase in judicial performance on homicide rates, we run Two-Stage Least Squares (2SLS) regressions where the indicator function of whether the number of voters was greater than or equal to a state's voter threshold, $1_{assignment}$, is the excluded instrument. They are estimated parametrically using quadratic and cubic splines and non-parametrically using local linear regressions for triangular and rectangular kernels and the same bandwidths as the reduced-form estimates. We use the IK and CCT optimal bandwidths of the dependent variables. In fuzzy regression discontinuity designs, (59) suggest using the smallest bandwidth between the outcome and the treatment optimal bandwidths estimated separately . The bias-corrected coefficients and the cluster robust confidence intervals are calculated according to (62). The main tables report the conservative standard errors, while the default SEs are shown in Appendix.

A central identification assumption in the RD design is that agents are unable to control the official number of voters around the cutoff. Additionally, the existence of other policies determined by the same running variable discontinuity or differences in the distribution of the determinants of the outcome above and below the cutoff can also bias the treatment effect estimate. They are tested and discussed in section 5.3. Furthermore, it is necessary to correctly specify the function of the running variable, which motivates the use of a variety of functional forms. The 2SLS approach requires two additional untestable assumptions. First, satisfying the state voter condition cannot cause a reduction in the probability of treatment (monotonicity). Second, crossing the threshold must affect homicides exclusively through increases in judicial performance (exclusion restriction). They are also discussed in section 5.3.

2.5 Results

We begin by testing the increase in the probability of treatment at the threshold. Afterwards, we estimate the reduced-form effect of the district classification on judicial performance and homicide rates. The impact of judicial performance on homicide rates is calculated using OLS and 2SLS regressions. We investigate the mechanisms related to the performance increase analyzing reduced-form regressions. Our main results are presented for 5 non-parametric bandwidths (40, 60, 80, IK and CCT) and for 2 parametric specifications (quadratic and cubic splines). Finally, we discuss additional specifications, identification assumptions and placebo tests. The reduced-form results are followed by graphs of the local linear regression of the outcome on the running variable, with no controls. The graphs are constructed for the full sample, as the regressions, and for the compliers. We consider compliers to be those treated assigned to treatment, and non-treated not assigned to treatment.

2.5.1 Main results

The increase in the probability of treatment at the threshold is presented in table 2.7 and in figure 2.1. The satisfaction of the state voter conditions raises the probability of a district being classified as second level instead of first level by 45%. This result is statistically identical for all specifications and is significant at the 5% level in regressions using a bandwidth equal to or greater than 40. The graph shows the frequency of level 2 districts by the percentage normalized distance to the threshold. Most of the observations with negative distances refer to first level districts. Among those with positive distances close to the cutoff, approximately 50% are classified as second level districts. This relationship is as expected, once the voter criteria is a necessary, but not sufficient, condition for the level determination due to the existence of other determinants in each state.

Table 2.8 examines the reduced-form effects of district classification on judicial performance. Column 1 indicates a significant increase in the number of sentences from 60% to 80% for most specifications. This is a relevant measure because this is the decision that concludes a case, although it can be concluded by more than one sentence. We estimate a growth in the number of sentences per judge equal to 60% and significant at the 5% level in the non-parametric specifications close to the 40% threshold as well as in the parametric specifications. The estimated increase in the number of sentences per process allocated is around 20% or more in most regressions and significant at the 5% level in the cubic spline and at the 10% level only in non-parametric models with more than 1,300 observations. The estimated increase in the number of processes allocated to courts is lower and non-significant in the non-parametric regressions, but it is significant at the 1% level in the quadratic spline and significant at the 10% level in the cubic spline. This smooth relationship is expected assuming that judicial demand is a continuous function of the total number of voters.

Figures 2.2 to 2.4 graphically display the increase of these three performance measures at the threshold. The graphs show the mean of outcomes per evenly spaced bins of the running variable. They are constructed for the full sample and for the sample restricted to observations for compliers. The difference in the mean of outcomes between districts to the left of the cutoff and districts to the right of the cutoff are in line with results found econometrically. Firstly, figure 2 shows a clear rise in the number of sentences at the voters cutoff, reflecting the improvement in the absolute volume of services offered to the society in similar districts that are classified differently. Statistically, these districts are similar even in terms of number of processes allocated, which is positive and continuously correlated with voters. Secondly, figure 3 shows a rise in the number of sentences per judge, reflecting differences in individual efficiency and in judiciary efficiency, if we consider judges as a proxy for judicial resources as a whole. This measure is called ”Magistrate Productivity Index” in government reports. Thirdly, figure 4 shows a relevant rise in the number of sentences per process allocated to courts. It reflects the state capacity to respond to judicial demand and is called ”Demand Attendance Index” in government reports. The increase in this measure is more disperse than it is in other measures since it is simultaneously affected by the variability in the number of sentences and in the number of processes allocated. Its dispersion is also a consequence of the small number of observations and clusters, the imprecision in the determination of the treatment, the presence of outliers and the existence of potential measurement errors. In general, the mean of outcomes per evenly spaced bins of the running variable seems to be less spread in the graph for compliers than it is in the graph for the full sample, in spite of their similarity.

Table 2.9 reports the reduced-form effects of judicial classification on seat and district homicide rates. We find a 60% reduction in both rates, according to the regressions with bandwidths of 40%. The results are significant at the 5% levels. Specifically, the decrease in district homicide rates varies from 31% to 63% and is statistically significant at the 5% level for 3 non-parametric regressions and at the 10% level for the 2 remaining. The decrease in seat homicide rates varies from 27% to 66% for the non-parametric regressions and is statistically significant at the 5% level as the sample is restricted to bandwidths equal to 40% or lower. The coefficient is lower for higher samples and is close to zero in the parametric models. We should expect similar results for the different ranges if homicide rates are linearly correlated with voters. However, if this relationship is not linear, the larger the range, the greater the likelihood of a biased estimator will be. The small sample and the high non-compliers homicide rates reinforce the importance of the potential bias. Figures 2.5 and 2.6 clarify the positive correlation between homicide rates and the number of voters, reversed or at least attenuated around the threshold. The effect of judicial classification on these crimes is very evident in the graph for compliers, because non-compliers increase the average observed rate and its variability in the full sample graph. This suggests that our regressions estimate a lower-bound effect.

We assume that district classification reduces homicide rates exclusively through an increase in judicial performance. This is an untestable assumption, but there may exist other mechanisms through which district classification affects crime. To analyze this relationship, we first estimate a quadratic and a cubic spline through an OLS regression. Table 2.11 shows that a 1% increase in the number of sentences is correlated with a 0.09% decrease in homicide rates at the 1% level. The coefficients for sentences per processes and sentences per judge are close to zero and not significant at the 5% level. These results are distinct from a causal treatment effect estimate due to the existence of important sources of endogeneity. Firstly, simultaneity, once judicial performance can reduce homicides as well as homicides may affect the number of process allocated, the number of judges, or the number of sentences and shift performance measures in an unexpected direction. Secondly, omitted variable bias, since homicides rate and judicial performance can both be correlated with other factors, such as population and local income.

We address these sources of endogeneity using a 2SLS regression. We exploit the district level assignment dummy (which equals 0 if the district number of voters is lower than the threshold and 1 otherwise) as the excluded instrument for judicial performance. Tables 2.12 to 2.14 present second stage results, while first stage results are equivalent to the reduced-form regression in which the dependent variable is the instrumented variable. [11] We find that a 1% increase in the number of sentences leads to a 0.95% decrease in homicide rates, a slightly larger absolute value than the OLS coefficient. This estimate is significant at the 5% level for bandwidths equal to 40% or lower and similar, but is not significant at the 5% level for the other bandwidths. As discussed before in this section, the variation in homicide rates estimated applying broader ranges is potentially biased. Column 1 of table 2.13 shows the impact of a 1% increase in the number of sentences per judge in district homicide rates, which varies from -1.9% for CCT optimal bandwidth (19) to

[11] The p-value of the F-test for excluded instrument is equivalent to the p-value of the t-test for the significance of the judicial performance variable in the first stage regressions.

-0.86% for bandwidths of 80% in non-parametric regressions. It is significant at the 5% level for bandwidths of 80% and CCT and at the 10% level for bandwidths of 40%. Coefficients for the other samples and the parametric regressions are not significant. Column 2 displays the effect of a 1% increase in the number of sentences per judge in seat homicide rates. Results are similar to the district variable for each bandwidth, but slightly lower. Table 2.14 reports the effect of a 1% increment in sentences per process allocated to courts on homicide rates. We find a significant reduction (at the 5% level) of 2.3% for regressions for bandwidths of 40%. The coefficient is lower but larger than 1.4 and not significant at the 5% level for regressions for bandwidths greater than 40%. On the other hand, the coefficient is higher and significant at the 1% level for regressions for CCT optimal bandwidths, which is lower than 25%. The coefficient is lower and non-significant at the 10% level for non-parametric seat regressions. These results must be considered carefully once the effect of district classification on sentences per process, which corresponds to the first stage model, is non-significant at the 5% level.

We found that an increase in the number of sentences or in the number of sentences per judge promotes an important reduction in homicide rates, according to our preferred regressions, those restricting the sample to the districts with number of voters 40% larger or 40% lower than the state threshold. This result is significant even when considering conservative standard errors. The capacity of courts to deal with new cases also seems to be a key factor in reducing these crime rates. The three dimensions are intrinsically integrated, once the improvement in sentences is a consequence of the more efficient use of resources, as reflected by the number of sentences per judge, and of the availability of additional resources, as tested below. Variations in the total number of sentences per new cases, in turn, are mainly a result of the increase in the number of those decisions associated with the relatively lower increase in the number of processes allocated. Figures A.2 to A.4 in the appendix show how these variables are correlated. The magnitude of the effect is elevated and probably reflects the existence of legal bottlenecks in the criminal process, given that the number of cases solved per year is around 20% of the total number of processes.

Table 2.10 presents the reduced-form analysis of the mechanisms of judicial performance gains around the discontinuity. We observe a continuous variation in the number of judges and courts around the voter cutoff, with some significant evidence of increases in the number of courts reported by parametric regressions. Furthermore, there is some significant evidence of the allocation of judges with approximately 50% more years of experience to second level districts than to first level districts, as reported at the 10% level by non-parametric regressions with bandwidths of 40% and 33% and at the 1% level by parametric regressions. There are no differences in gender distribution. Despite the increase in the average availability of resources between first and second districts in the sample, that variation is continuous at the threshold and of low magnitude. This reinforces the importance of the productivity of judges in the improvement of judiciary performance over the availability of resources. The hypothesis of selection of more productive and/or experienced judges in the promotion processes from first to second level is sustained by the promotion criteria adopted by state courts, based on merit or experience, and by the empirical evidence, such as the increase in the number of sentences per judge itself and in their experience at the threshold. The higher productivity of judges in second level districts may also be explained by the attraction of better bureaucrats to those places due to local amenities and by an overall increase in productivity due to peer effect. In addition, they can be affected by other changes in judicial structure, such as the existence of specialized courts in second level districts.

2.5.2
Additional results

In order to check the robustness of the results to specification and methodological variations, we show additional results in the appendix. Tables A.1 to A.4 and tables A.13 to A.15 display the same regressions discussed in this section with robust standard errors calculated exactly as in (62). They exploit the variance of the 3 nearest neighbors of each observation, considering the clustered structure of the data in our base. Our main tables, discussed above, present more conservative standard errors, exploiting the variance of the 5 nearest neighbors. The coefficients estimated by both groups of results are identical, as they should be, but confidence intervals are much smaller in standard one. We observe significant variations at the 1% level of the probability of treatment, judicial performance and homicide rates for all non-parametric regressions. The increase in the number of judges and courts became significant at the 1% level, while the raise in judges' experience levels remained significant and the variation in gender insignificant. The impact of increases in judicial performance on homicide rates is also significant at the 1% level. Despite the important growth in the standard errors of the main regressions, we find significant effects of district classification on judicial performance and homicide rates. Appendix tables A.5 to A.8 and tables A.16 to A.18 show the models estimated controlling for the proportion of seat population who live in urban areas, log of seat average household income per capita, Gini coefficient, percentage of people under 18 years old, and proportion of adults (over 18) who completed high school. Appendix tables A.9 to A.12 and tables A.19 to A.21 show the local linear regressions estimated using rectangular kernels instead of triangular kernels. We obtain similar trends adding controls or using rectangular kernels as we do in the main tables, despite occasional differences.

We present a placebo test in the appendix using a fake discontinuity at the middle between the threshold of second and the third level voters for each state. Table A.22 displays non-significant impacts of the fake discontinuity dummy on district classification. Table A.23 shows reductions in performance measures at the cutoff, opposite to what we obtain, some of them significant at the 5% level. The variation in homicide rates is never statistically different from zero. Coefficients for the number of judges, number of courts, judges' experience levels and gender are negative and most of them are not significant at the 5% level in non-parametric models and non-significant in parametric models. These results reinforce the validity of our identification strategy.

2.5.3 Validity tests

A central condition for the validity of the identification is the agent's inability to manipulate figures of the number of voters in a district to alter their classification. We test the occurrence of sorting around the threshold performing the McCrary test, which verifies a discontinuous variation in the density function of voters at the cutoff[12] ((66)). The null hypothesis is that the discontinuity is zero. Figure A.1 in the appendix shows the test for a single year, once we consider as the running variable the number of voters in 2009. As expected, the result is the same for other years. We perform the test restricting the sample to bandwidths of 80% and 40%, and the discontinuity estimates are never significant at the 5% level. The manipulation of the number of voters is improbable. Firstly, electoral enrollment is compulsory for literate citizens aged 18 to 70, despite being optional for illiterate citizens, as well as for those aged 16 and 17 or those older than 70. Secondly, the Electoral Superior Court (*Superior Tribunal Eleitoral*), a federal body, is responsible for maintaining municipal voter statistics. Thirdly, in order to manipulate these figures, it would be necessary to manipulate estimates in one or more of their

[12]The test was implemented using the DCdensity Stata package available in June 2016 at: http://eml.berkeley.edu// jmccrary/DCdensity/

municipalities as well as the entry of non-seat municipalities in a district. A municipality can be a district seat if it satisfies some criteria established by each state, such as discontinuous population rules. Otherwise, it must be incorporated in a frontier municipality district (for further details see (32)). Fourth, the classification rules depend on other variables and are different among states. Fifth, state criteria are determined by previously established laws [13], while estimates regarding the number of voters change monthly.

The existence of other policies determined by the same running variable discontinuity or differences in the distribution of outcome determinants above and below the cutoff may invalidate our identification strategy. Tables 2.15 to 2.18 regress our reduced-form model exploiting as dependent variable some potential confounders. The regressions use only one-year cross-section data, since our running variable and potential confounder variables are known for 2010, 2009 and 2006 in the case of seat characteristics, voters and public security resources, respectively. Data on military and civilian police are available only for the year of 2008 for the state of São Paulo ((57)). We find no evidence of a discontinuous increase in seat urbanization rates, average household income per capita, Gini index, proportion of population aged 17 or lower and percentage of people over 18 years old who completed high school. Furthermore, the coefficients are close to zero in most of these regressions. As shown in table 2.18, there is no significant variation in the number of municipalities per district around the threshold. The same occurs for the number of police officers per 100,000 inhabitants and public security expenditure per capita, for which only the cubic splines report a significant decrease. We have data on the number of military and civilian police officers per municipality only for the state of São Paulo, which comprises most of the municipalities in our sample. The number of police officers per State is determined by laws proposed by state governments and passed (or not) by the legislative power. There are no federal or state allocation rules, to the best of our knowledge, what reduces the probability of a discontinuous increase in the number of police officers at the threshold. In addition, the existence of different thresholds for each State also reduces the probability of a discontinuous increase of some potential confounder at the threshold.

The 2SLS approach requires, additionally, that the monotonicity assumption and the exclusion restriction hold. They are untestable, but the monotonicity assumption is reasonable since the excluded instrument, the threshold dummy, significantly and positively impacts judicial performance. The validity

[13]The São Paulo code was created in 1964 and the classification criteria were modified in 2005; the Ceará code was created in 1994; and the Sergipe code was created in 2003

of the exclusion restriction, in turn, may be a caveat. Given that RD hypotheses are credible, homicide rates should vary at the discontinuity only due to justice differences related to district classification. However, district classification changes can influence homicide rates through judicial performance or other mechanisms, such as a subjective perception of justice efficiency related to how a court is classified, what undermines the hypothesis that the threshold dummy (excluded instrument) affects homicide rates exclusively through performance (instrumented variable), despite the relevance of this channel. Consequently, the effect of judicial performance on homicide rates can be biased.

2.6 Conclusion

In this paper we investigate the impact of judicial performance on homicide rates by exploiting district level classification. We use novel data on judicial system features and homicide rates. In order to be classified as a second level district, the local number of voters must be higher than the criteria defined by each state. Satisfying the threshold is a necessary, but not sufficient condition, due to the existence of other determinant variables. We use a Regression Discontinuity approach to estimate an ITT effect of district level on judicial performance and homicide rates as well as a 2SLS to estimate the effect of the former on the latter. We estimate an increase of around 45% in the probability of classification as a second level district at the threshold. We find improvements in judicial performance resulting from a difference in judicial levels. They correspond to a growth of 60% to 80% in the number of sentences and in the number of sentences per judge. There is also a noisy evidence of a 20% increase in the number of sentences per process. Reductions in seat and district homicide rates at the discontinuity are around 60%. We show that judicial performance increases lead to a reduction in homicide rates. Specifically, a 1% increase in the number of sentences and number of sentences per judge decreases homicide rates by approximately 0.9% and 1.2%, respectively.

We present the first causal evidence of the impact of legal capacity on homicide rates, to the best of our knowledge. We investigate how important role of the judiciary system is in reducing violence, in addition to evidences in the literature about the effect of police action and punishment certainty on those crimes. The lack of data on productivity levels of public and homicide rates, especially in developing countries, and of a clear identification strategy associated with a reasonable number of observations make this analysis hard to carry out. Moreover, it is motivated by elevated homicide rates in those countries and expensive and/or inefficient judicial systems.

The main limitations of this paper are the lack of identification of further mechanisms through which district classification affects the performance of courts and homicide rates and the potential existence of a crime displacement. Furthermore, the estimated effects may be underestimated because of non-compliers. The impact of judicial performance on homicide rates may be overestimated due to the existence of other channels through which differences in district levels reduce crime. The police response to changes in the judicial system also is an untestable source of bias.

Table 2.1: States rules of district classification and sample information

State	Court level	Voters threshold	Observations	Districts	Municipalities	Population
SP	Both	50000	1101	221	518	13120294
	1		761	153	330	5480504
	2		340	68	188	7639790
CE	Both	12500	285	57	58	1314780
	1		175	35	36	686133
	2		110	22	22	628647
SE	Both	24500	180	36	73	1991957
	1		130	26	62	837395
	2		50	10	11	1154562
3 states	Both		1566	314	649	16427031
	1		1066	214	428	7004032
	2		500	100	221	9422999

Notes: Sample restricted to districts classified as first or second level. Population estimates based on 2009 data.

Table 2.2: Summary statistics of districts' judicial performance

Bandwidth	Court level	Allocated	Sentences	Sentence per judge	Sentence per process	Observations	Districts
40	1	1598	645	214	0.50	475	95
		(2423)	(598)	(270)	(0.34)		
	2	5391	2410	338	0.58	180	36
		(8390)	(3260)	(346)	(0.94)		
	Difference	3794***	1766***	124**	0.08		
		[881]	[469]	[53]	[0.09]		
60	1	1816	772	284	0.49	771	155
		(3195)	(1196)	(430)	(0.31)		
	2	5644	2409	345	0.53	255	51
		(7742)	(3054)	(328)	(0.80)		
	Difference	3828***	1638***	61	0.05		
		[761]	[371]	[47]	[0.07]		
80	1	1854	808	296	0.49	1036	208
		(2833)	(1073)	(413)	(0.29)		
	2	6223	2686	359	0.53	315	63
		(7916)	(3159)	(330)	(0.73)		
	Difference	4369***	1877***	63	0.04		
		[755]	[354]	[41]	[0.05]		
Full sample	1	1830	800	292	0.49	1066	214
		(2796)	(1060)	(409)	(0.29)		
	2	8226	3394	349	0.50	500	100
		(12332)	(4994)	(298)	(0.59)		
	Difference	6396***	2595***	57*	0.00		
		[1065]	[479]	[33]	[0.04]		

Notes: Sample restricted to districts classified as first or second level. The set of rows restricts the sample to those observations in which the percentage distance to state threshold is smaller than 40%, 60% and 80%, with the exception of the last one.Clustered standard errors at district level for the difference in means are in brackets. Standard deviations are in parenthesis. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: Summary statistics of districts' justice structure

Bandwidth	Court level	Courts	Experience	Gender	Judges	Municipalities	Obs	Districts
40	1	1.16	5.31	64.63	4.30	2.01	475	95
		(0.45)	(3.86)	(23.68)	(2.81)	(1.24)		
	2	2.67	6.46	72.99	6.49	2.36	180	36
		(2.11)	(3.54)	(19.57)	(3.60)	(1.91)		
	Difference	1.51***	1.15	8.36**	2.19***	0.35		
		[0.35]	[0.74]	[3.97]	[0.54]	[0.34]		
60	1	1.23	5.17	64.50	4.18	2.08	771	155
		(0.48)	(3.57)	(27.47)	(2.71)	(1.24)		
	2	2.70	6.28	68.56	6.61	2.33	255	51
		(2.06)	(3.17)	(23.65)	(3.77)	(1.83)		
	Difference	1.47***	1.11**	4.06	2.43***	0.26		
		[0.29]	[0.51]	[3.94]	[0.46]	[0.27]		
80	1	1.34	5.09	66.46	4.13	2.03	1036	208
		(0.51)	(3.44)	(27.89)	(2.64)	(1.18)		
	2	2.97	6.31	69.85	7.06	2.41	315	63
		(2.20)	(3.17)	(22.81)	(4.31)	(1.83)		
	Difference	1.63***	1.23***	3.39	2.93***	0.38		
		[0.28]	[0.44]	[3.29]	[0.48]	[0.24]		
Full sample	1	1.34	5.12	66.48	4.14	2.00	1066	214
		(0.52)	(3.49)	(27.77)	(2.63)	(1.18)		
	2	3.83	6.21	66.15	8.79	2.21	500	100
		(5.15)	(2.96)	(22.95)	(9.16)	(1.72)		
	Difference	2.49***	1.09***	-0.34	4.65***	0.21		
		[0.52]	[0.37]	[2.89]	[0.90]	[0.19]		

*Notes:*Sample restricted to districts classified as first or second level. The set of rows restricts the sample to those observations in which the percentage distance to the state voters threshold is smaller than 40%, 60% and 80%, with the exception of the last one.Clustered standard errors at district level for the difference in means are in brackets. Standard deviations are in parenthesis. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.4: Summary statistics of district's seat characteristics and homicide rates

Bandwidth	Court level	Urban	Gini	High school	Young	Income	District homicide rate	Homicide rate	Observations	Districts
40	1	70.95	0.49	28.18	31.13	491	20.47	21.62	475	95
		(22.45)	(0.05)	(9.91)	(5.09)	(272)	(14.92)	(16.49)		
	2	81.85	0.51	34.96	28.30	670	17.44	17.61	180	36
		(19.50)	(0.04)	(10.01)	(4.65)	(288)	(13.29)	(13.23)		
	Difference	10.90***	0.02**	6.79***	-2.83***	179***	-3.03	-4.01*		
		[3.99]	[0.01]	[1.96]	[0.94]	[56]	[2.30]	[2.35]		
60	1	77.01	0.49	30.53	29.44	582	18.04	19.45	771	155
		(20.77)	(0.05)	(9.01)	(4.88)	(262)	(12.99)	(14.59)		
	2	82.68	0.51	35.53	28.22	695	17.34	17.63	255	51
		(19.05)	(0.04)	(10.88)	(4.55)	(328)	(12.78)	(12.79)		
	Difference	5.67*	0.02***	4.99***	-1.22	113**	-0.69	-1.83		
		[3.15]	[0.01]	[1.69]	[0.75]	[51]	[1.74]	[1.78]		
80	1	77.68	0.48	31.02	28.78	599	17.25	18.85	1036	208
		(20.03)	(0.05)	(8.36)	(4.75)	(241)	(12.13)	(13.59)		
	2	83.37	0.51	35.85	28.16	697	17.33	17.80	315	63
		(18.67)	(0.05)	(10.63)	(4.64)	(312)	(12.67)	(12.76)		
	Difference	5.69**	0.02***	4.83***	-0.62	98**	0.08	-1.05		
		[2.73]	[0.01]	[1.46]	[0.67]	[43]	[1.53]	[1.56]		
Full sample	1	77.70	0.48	30.97	28.81	597	17.57	19.15	1066	214
		(19.96)	(0.05)	(8.36)	(4.77)	(241)	(12.85)	(14.19)		
	2	83.19	0.50	35.54	28.96	672	19.79	20.12	500	100
		(19.58)	(0.05)	(10.93)	(4.69)	(330)	(14.54)	(14.54)		
	Difference	5.48**	0.02***	4.57***	0.16	75**	2.22	0.97		
		[2.39]	[0.01]	[1.24]	[0.57]	[37]	[1.47]	[1.49]		

Notes: Sample restricted to districts classified as first or second level. The set of rows restricts the sample to those observations in which the percentage distance to the state voters threshold is smaller than 40%, 60% and 80%, with the exception of the last one.Clustered standard errors at district level for the difference in means are in brackets. Standard deviations are in parenthesis. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.5: Summary statistics of public security resources

Bandwidth	Court level	Seat's municipal police	District's municipal police	Seat's security expenditure	District's security expenditure	Observations	Districts
40	1	34.08	32.49	4.61	4.04	475	95
		(57.08)	(52.26)	(10.38)	(9.03)		
	2	23.89	24.84	4.11	3.81	180	36
		(39.39)	(37.53)	(6.99)	(6.36)		
	Difference	-10.19	-7.66	-0.51	-0.23		
		[8.81]	[8.25]	[1.58]	[1.41]		
60	1	37.17	35.40	5.72	5.11	771	155
		(63.06)	(58.99)	(12.48)	(10.96)		
	2	27.21	27.45	5.67	5.24	255	51
		(42.23)	(40.64)	(11.53)	(10.63)		
	Difference	-9.96	-7.96	-0.06	0.13		
		[7.80]	[7.42]	[1.90]	[1.73]		
80	1	33.00	32.49	4.69	4.26	1036	208
		(59.56)	(55.18)	(11.22)	(9.90)		
	2	28.34	28.61	5.77	5.30	315	63
		(46.40)	(44.92)	(12.05)	(11.24)		
	Difference	-4.66	-3.88	1.08	1.04		
		[7.16]	[6.84]	[1.72]	[1.57]		
Full sample	1	32.07	31.57	4.56	4.14	1066	214
		(58.96)	(54.67)	(11.08)	(9.78)		
	2	39.48	37.53	6.48	5.89	500	100
		(55.69)	(52.10)	(12.11)	(11.15)		
	Difference	7.41	5.95	1.92	1.75		
		[6.88]	[6.42]	[1.44]	[1.30]		

Notes: Police per 100,000 inhabitants. Per capita public security expenditure. Sample restricted to districts classified as first or second level. The set of rows restricts the sample to those observations in which the percentage distance to the state voters threshold is smaller than 40%, 60% and 80%, with the exception of the last one.Clustered standard errors at district level for the difference in means are in brackets. Standard deviations are in parenthesis. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.6: Summary statistics of districts' police and seats' police

Bandwidth	Court level	Seat's military police	District's military police	Seat's civilian police	District's civilian police	Observations	Districts
40	1	149.22	146.38	75.89	70.51	475	95
		(113.11)	(98.23)	(62.21)	(42.97)		
	2	194.71	181.05	92.82	88.63	180	36
		(96.36)	(82.09)	(54.51)	(47.59)		
	Difference	45*	35	17	18		
		[26]	[22]	[15]	[12]		
60	1	130.94	133.65	72.30	67.76	771	155
		(86.55)	(76.07)	(51.04)	(38.68)		
	2	183.76	170.13	88.12	82.67	255	51
		(91.37)	(77.07)	(54.51)	(47.80)		
	Difference	53***	36**	16	15*		
		[18]	[15]	[10]	[9]		
80	1	124.81	129.96	74.17	70.05	1036	208
		(79.28)	(70.30)	(48.71)	(37.76)		
	2	177.47	167.31	90.27	84.50	315	63
		(85.89)	(71.68)	(53.04)	(46.24)		
	Difference	53***	37***	16*	14*		
		[14]	[12]	[9]	[7]		
Full sample	1	125.36	130.77	74.71	70.63	1066	214
		(78.98)	(70.39)	(48.51)	(37.82)		
	2	164.20	155.24	79.63	74.88	500	100
		(77.45)	(64.88)	(49.18)	(42.85)		
	Difference	39***	24**	5	4		
		[11]	[10]	[7]	[6]		

Notes:Police per 100,000 inhabitants. Data available only for São Paulo State. Sample restricted to districts classified as first or second level. The set of rows restricts the sample to those observations in which the percentage distance to the state voters threshold is smaller than 40%, 60% and 80%, with the exception of the last one.Clustered standard errors at district level for the difference in means are in brackets. Standard deviations are in parenthesis. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Figure 2.1: Frequency of level 2 districts

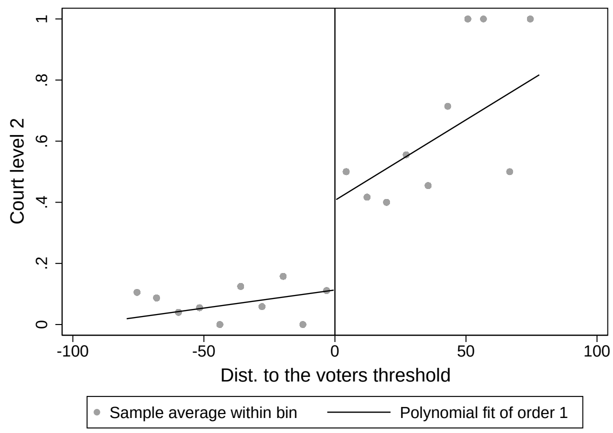


Figure 2.2: Number of sentences (log)

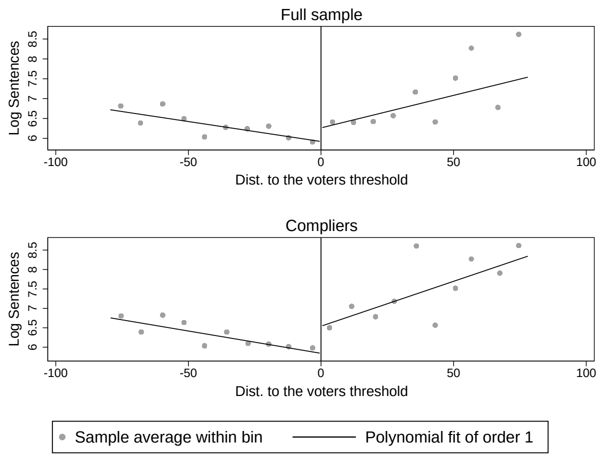


Figure 2.3: Number of sentences per judge (log)

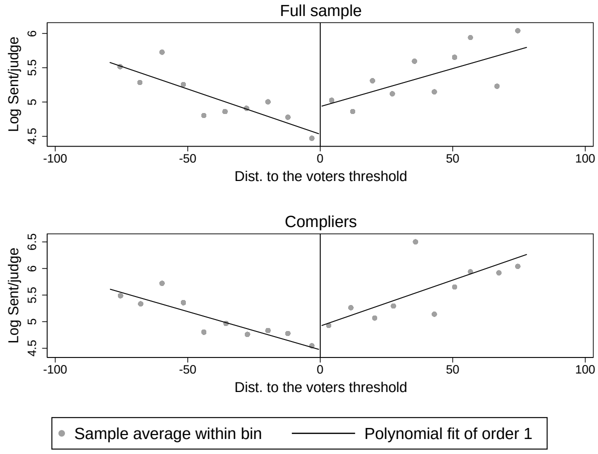


Figure 2.4: Number of sentences per process allocated (log)

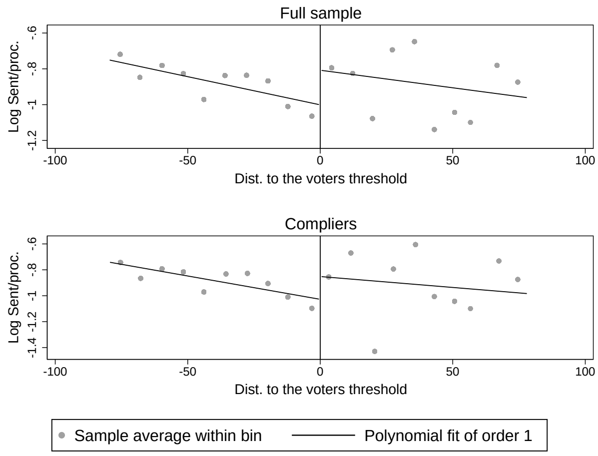


Figure 2.5: District homicide rate (log)

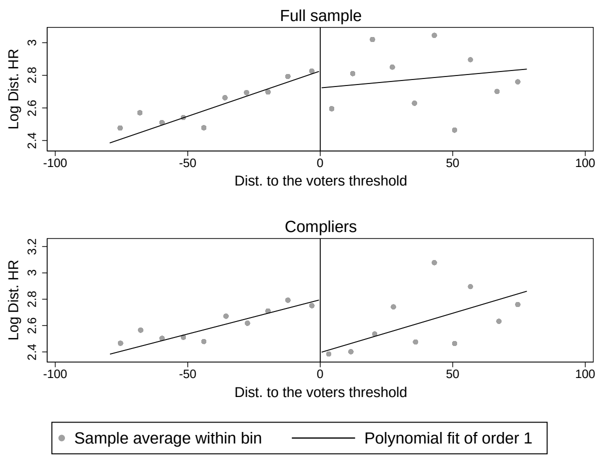


Figure 2.6: Homicide rate (log)

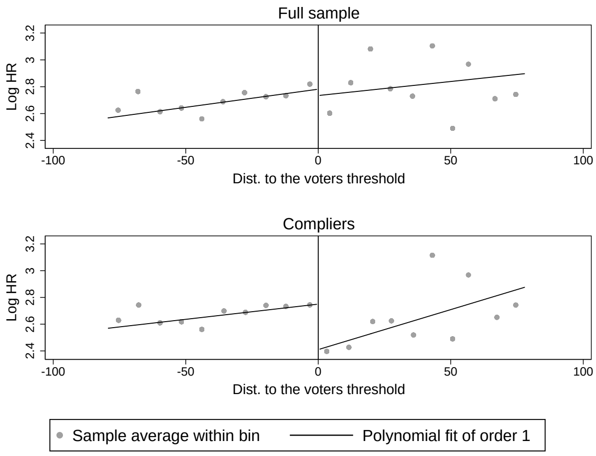


Table 2.7: Reduced-form effects - level 2 court

Dependent	District Level 2
	(1)
Voters >= cutoff	0.450**
	[0.196]
Bandwidth	40
Observations	655
Districts	131
Voters >= cutoff	0.461***
	[0.144]
Bandwidth	60
Observations	1,026
Districts	206
Voters >= cutoff	0.346***
	[0.117]
Bandwidth	80
Observations	1,351
Districts	271
Voters >= cutoff	0.443*
	[0.248]
IK Bandwidth	31.307
Observations	505
Districts	101
Voters >= cutoff	0.295
	[0.309]
CCT Bandwidth	25.574
Observations	415
Districts	83
Voters >= cutoff	0.543***
Quadratic spline	[0.077]
Bandwidth	Full sample
Observations	1,571
Districts	315
Voters >= cutoff	0.402***
Cubic spline	[0.082]
Bandwidth	Full sample
Observations	1,571
Districts	315
Year FE	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.8: Reduced-form effects - judicial performance

Dependent	Log Sentences	Log Sentence per process	Log Sentence per judge	Log Allocated
	(1)	(2)	(3)	(4)
Voters >= cutoff	0.785**	0.290	0.594**	0.248
	[0.313]	[0.179]	[0.282]	[0.321]
Bandwidth	40	40	40	40
Observations	655	653	655	655
Districts	131	131	131	131
Voters >= cutoff	0.574**	0.226	0.389	0.131
	[0.275]	[0.153]	[0.249]	[0.279]
Bandwidth	60	60	60	60
Observations	1,026	1,024	1,026	1,026
Districts	206	206	206	206
Voters >= cutoff	0.359	0.216*	0.253	-0.043
	[0.250]	[0.130]	[0.226]	[0.247]
Bandwidth	80	80	80	80
Observations	1,351	1,349	1,351	1,351
Districts	271	271	271	271
Voters >= cutoff	0.694**	0.214*	0.592**	0.186
	[0.283]	[0.125]	[0.272]	[0.286]
IK Bandwidth	53.499	89.248	45.129	55.633
Observations	926	1,389	770	951
Districts	186	279	154	191
Voters >= cutoff	0.369	0.097	0.168	0.477
	[0.429]	[0.196]	[0.423]	[0.340]
CCT Bandwidth	20.514	30.420	20.707	24.711
Observations	340	478	340	405
Districts	68	96	68	81
Voters >= cutoff	0.806***	0.125	0.693***	0.650***
Quadratic spline	[0.181]	[0.079]	[0.157]	[0.193]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,569	1,571	1,571
Districts	315	315	315	315
Voters >= cutoff	0.597***	0.202**	0.594***	0.346*
Cubic spline	[0.194]	[0.085]	[0.175]	[0.202]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,569	1,571	1,571
Districts	315	315	315	315
Year FE	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.9: Reduced-form effects - homicide rate

Dependent	Log District homicide rate	Log Seat homicide rate
	(1)	(2)
Voters >= cutoff	-0.631***	-0.595**
	[0.217]	[0.236]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Voters >= cutoff	-0.408**	-0.353*
	[0.187]	[0.203]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Voters >= cutoff	-0.310*	-0.271
	[0.164]	[0.177]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Voters >= cutoff	-0.433**	-0.265
	[0.194]	[0.175]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Voters >= cutoff	-0.576*	-0.657**
	[0.295]	[0.276]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Voters >= cutoff	-0.006	0.052
Quadratic spline	[0.108]	[0.114]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Voters >= cutoff	-0.072	-0.005
Cubic spline	[0.118]	[0.124]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.10: Reduced-form effects - mechanisms

Dependent	Log Experience	Log Judges	Log Courts	Log Gender
	(1)	(2)	(3)	(4)
Voters >= cutoff	0.479*	0.128	0.087	-0.158
	[0.262]	[0.197]	[0.060]	[0.240]
Bandwidth	40	40	40	40
Observations	357	655	655	351
Districts	80	131	131	80
Voters >= cutoff	0.204	0.134	0.024	-0.009
	[0.206]	[0.168]	[0.055]	[0.164]
Bandwidth	60	60	60	60
Observations	691	1,026	1,026	660
Districts	148	206	206	147
Voters >= cutoff	0.153	0.078	-0.023	0.047
	[0.179]	[0.141]	[0.054]	[0.129]
Bandwidth	80	80	80	80
Observations	986	1,351	1,351	942
Districts	208	271	271	207
Voters >= cutoff	0.152	0.075	0.066	0.001
	[0.175]	[0.140]	[0.056]	[0.152]
IK Bandwidth	83.521	81.770	49.228	65.329
Observations	996	1,361	840	733
Districts	210	273	168	163
Voters >= cutoff	0.570*	0.056	0.027	-0.268
	[0.301]	[0.223]	[0.038]	[0.529]
CCT Bandwidth	33.450	27.078	15.033	21.756
Observations	282	425	210	169
Districts	62	85	42	37
Voters >= cutoff	0.370***	0.090	0.332***	0.082
Quadratic spline	[0.120]	[0.086]	[0.059]	[0.077]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,126	1,571	1,571	1,082
Districts	238	315	315	237
Voters >= cutoff	0.333**	0.005	0.255***	0.147*
Cubic spline	[0.132]	[0.092]	[0.064]	[0.083]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,126	1,571	1,571	1,082
Districts	238	315	315	237
Year FE	Y	Y	Y	Y

Notes: Experience and gender information are unavailable for some States. The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.11: Effect of judicial performance on homicide rate (OLS)

Dependent	Log District homicide rate	Log Seat homicide rate	Log District homicide rate	Log Seat homicide rate	Log District homicide rate	Log Seat homicide rate
Independent	Log Sentences	Log Sentences	Log Sentence per process	Log Sentence per process	Log Sentence per judge	Log Sentence per judge
	(1)	(2)	(4)	(3)	(6)	(5)
Independent	-0.088***	-0.073***	-0.014	0.011	-0.014	-0.002
Quadratic spline	[0.023]	[0.024]	[0.029]	[0.031]	[0.024]	[0.025]
Bandwidth	Full sample	Full sample	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,571	1,569	1,569	1,571	1,571
Districts	315	315	315	315	315	315
Independent	-0.099***	-0.084***	-0.011	0.015	-0.020	-0.007
Cubic spline	[0.024]	[0.024]	[0.029]	[0.031]	[0.025]	[0.026]
Bandwidth	Full sample	Full sample	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,571	1,569	1,569	1,571	1,571
Districts	315	315	315	315	315	315
Year FE	Y	Y	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.12: Effect of sentences on homicide rate (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentences	Log Sentences
	(1)	(2)
Log Sentences	-0.945**	-0.948**
	[0.466]	[0.465]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Log Sentences	-0.830	-0.796
	[0.693]	[0.666]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Log Sentences	-0.890*	-0.784
	[0.466]	[0.494]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Log Sentences	-0.597	-0.772
	[0.728]	[0.496]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Log Sentences	-1.250***	-1.122***
	[0.469]	[0.410]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Log Sentences	-0.008	0.064
Quadratic spline	[0.133]	[0.144]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Log Sentences	-0.121	-0.009
Cubic spline	[0.192]	[0.207]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.13: Effect of sentences per judge on homicide rate (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentence per judge	Log Sentence per judge
	(1)	(2)
Log Sentence per judge	-1.241*	-1.228*
	[0.719]	[0.731]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Log Sentence per judge	-1.202	-1.066
	[0.805]	[0.806]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Log Sentence per judge	-0.865**	-0.716
	[0.435]	[0.449]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Log Sentence per judge	-0.933	-0.687
	[1.010]	[0.439]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Log Sentence per judge	-1.887**	-1.436**
	[0.787]	[0.694]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Log Sentence per judge	-0.009	0.075
Quadratic spline	[0.155]	[0.163]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Log Sentence per judge	-0.121	-0.009
Cubic spline	[0.203]	[0.209]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.14: Effect of sentences per process allocated on homicide rate (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentence per process	Log Sentence per process
	(1)	(2)
Log Sentence per process	-2.329**	-2.235**
	[0.954]	[0.988]
Bandwidth	40	40
Observations	653	653
Districts	131	131
Log Sentence per process	-1.857*	-1.612
	[0.951]	[0.997]
Bandwidth	60	60
Observations	1,024	1,024
Districts	206	206
Log Sentence per process	-1.459*	-1.291
	[0.822]	[0.892]
Bandwidth	80	80
Observations	1,349	1,349
Districts	271	271
Log Sentence per process	-1.815*	-1.273
	[1.062]	[0.893]
IK Bandwidth	54.232	82.487
Observations	934	1,364
Districts	188	274
Log Sentence per process	-13.966***	-4.785***
	[3.343]	[1.059]
CCT Bandwidth	19.509	24.442
Observations	308	403
Districts	62	81
Log Sentence per process	-0.027	0.440
Quadratic spline	[0.858]	[0.955]
Bandwidth	Full sample	Full sample
Observations	1,569	1,569
Districts	315	315
Log Sentence per process	-0.334	-0.003
Cubic spline	[0.600]	[0.612]
Bandwidth	Full sample	Full sample
Observations	1,569	1,569
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.15: Discontinuity test - seat's characteristics

Dependent	Log Urban	Log Income	Log Gini	Log Young	Log High school
	(1)	(2)	(3)	(4)	(5)
Voters >= cutoff	0.010	0.279	0.047	-0.027	0.061
	[0.242]	[0.369]	[0.052]	[0.094]	[0.242]
Bandwidth	40	40	40	40	40
Districts	131	131	131	131	131
Voters >= cutoff	-0.035	0.196	0.064	-0.032	-0.002
	[0.205]	[0.310]	[0.045]	[0.083]	[0.205]
Bandwidth	60	60	60	60	60
Districts	205	205	205	205	205
Voters >= cutoff	-0.040	0.179	0.056	-0.042	-0.005
	[0.177]	[0.266]	[0.038]	[0.073]	[0.176]
Bandwidth	80	80	80	80	80
Districts	270	270	270	270	270
Voters >= cutoff	-0.040	0.182	0.062	-0.039	-0.003
	[0.175]	[0.275]	[0.043]	[0.075]	[0.183]
IK Bandwidth	81.511	74.265	64.740	73.727	73.991
Districts	272	259	219	258	259
Voters >= cutoff	0.027	0.369	0.047	-0.035	0.081
	[0.258]	[0.411]	[0.057]	[0.101]	[0.249]
CCT Bandwidth	35.236	32.041	32.403	31.378	37.115
Districts	113	102	103	101	121
Voters >= cutoff	0.012	0.071	-0.019	-0.040	0.036
Quadratic spline	[0.061]	[0.101]	[0.020]	[0.031]	[0.065]
Bandwidth	Full sample	Full sample	Full sample	Full sample	Full sample
Districts	314	314	314	314	314
Voters >= cutoff	-0.036	0.052	-0.000	-0.049	0.004
Cubic spline	[0.066]	[0.110]	[0.021]	[0.033]	[0.070]
Bandwidth	Full sample	Full sample	Full sample	Full sample	Full sample
Districts	314	314	314	314	314

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.16: Discontinuity test - district's number of municipalities and municipal police

Dependent	Log Municipalities	Log District's municipal police	Log Seat's municipal police
	(1)	(2)	(3)
Voters >= cutoff	0.036	0.524	0.328
	[0.284]	[1.131]	[1.175]
Bandwidth	40	40	40
Districts	131	131	131
Voters >= cutoff	-0.073	0.205	0.099
	[0.240]	[0.996]	[1.032]
Bandwidth	60	60	60
Districts	205	205	205
Voters >= cutoff	-0.083	0.034	0.056
	[0.213]	[0.881]	[0.903]
Bandwidth	80	80	80
Districts	270	270	270
Voters >= cutoff	-0.020	-0.263	0.057
	[0.193]	[0.810]	[0.905]
IK Bandwidth	114.898	110.239	79.648
Districts	290	289	270
Voters >= cutoff	0.295	0.180	0.326
	[0.331]	[1.181]	[1.180]
CCT Bandwidth	30.221	33.228	39.271
Districts	96	108	128
Voters >= cutoff	-0.044	-0.588	-0.722*
Quadratic spline	[0.116]	[0.420]	[0.423]
Bandwidth	Full sample	Full sample	Full sample
Districts	314	314	314
Voters >= cutoff	-0.018	-0.970**	-1.138**
Cubic spline	[0.126]	[0.453]	[0.455]
Bandwidth	Full sample	Full sample	Full sample
Districts	314	314	314

Notes: Police per 100,000 inhabitants. The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.17: Discontinuity test - civilian police and military police (SP)

Dependent	Log Seat's military police	Log District's military police	Log Seat's civilian police	Log District's civilian police
	(1)	(2)	(3)	(4)
Voters >= cutoff	-0.133	-0.193	0.238	0.426
	[0.399]	[0.330]	[0.352]	[0.336]
Bandwidth	40	40	40	40
Districts	68	68	68	68
Voters >= cutoff	0.233	0.174	0.478	0.587*
	[0.308]	[0.260]	[0.324]	[0.310]
Bandwidth	60	60	60	60
Districts	135	135	134	135
Voters >= cutoff	0.219	0.158	0.436	0.428
	[0.254]	[0.218]	[0.304]	[0.284]
Bandwidth	80	80	80	80
Districts	194	194	193	194
Voters >= cutoff	0.245	0.135	0.434	0.355
	[0.271]	[0.196]	[0.307]	[0.271]
IK Bandwidth	71.964	114.795	75.924	110.171
Districts	175	207	187	206
Voters >= cutoff	-0.156	-0.239	0.043	0.330
	[0.403]	[0.338]	[0.376]	[0.368]
CCT Bandwidth	39.371	38.185	33.378	32.143
Districts	66	64	54	50
Voters >= cutoff	0.312*	0.240	0.414	0.314
Quadratic spline	[0.188]	[0.176]	[0.258]	[0.267]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Districts	220	220	219	220
Voters >= cutoff	0.565**	0.434*	0.624*	0.607
Cubic spline	[0.262]	[0.246]	[0.361]	[0.373]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Districts	220	220	219	220

Notes: Police per 100,000 inhabitants. Police information available only for São Paulo State. The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table 2.18: Discontinuity test - public security expenditure

Dependent	Log District's security expenditure	Log Seat's security expenditure
	(1)	(2)
Voters >= cutoff	0.324	0.209
	[0.700]	[0.718]
Bandwidth	40	40
Districts	131	130
Voters >= cutoff	0.323	0.219
	[0.625]	[0.640]
Bandwidth	60	60
Districts	205	204
Voters >= cutoff	0.274	0.193
	[0.542]	[0.555]
Bandwidth	80	80
Districts	270	268
Voters >= cutoff	0.277	0.243
	[0.547]	[0.606]
IK Bandwidth	78.366	67.805
Districts	267	235
Voters >= cutoff	0.287	0.250
	[0.743]	[0.763]
CCT Bandwidth	32.537	32.087
Districts	105	101
Voters >= cutoff	-0.294	-0.385
Quadratic spline	[0.244]	[0.253]
Bandwidth	Full sample	Full sample
Districts	314	311
Voters >= cutoff	-0.547**	-0.653**
Cubic spline	[0.262]	[0.272]
Bandwidth	Full sample	Full sample
Districts	314	311

Notes: Per capita public expenditure. The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

3
Breaking the cycle: the impact of legal reforms on domestic violence

3.1
Introduction

Domestic violence is the set of physical, psychological, moral and patrimonial abuse perpetrated by the intimate partner, by a family member or by someone else within the household ((67)). This kind of violence is widespread across several countries. Approximately 30% of ever-partnered women in the world have experienced this type of aggression ((68)). Besides being a criminal matter, it is a public health matter. Exposure to marital violence is correlated with suicide, depression and alcohol abuse among women ((68)). The victims are more likely to have complications during pregnancy, with a negative impact on children ((69); (70)). Additionally, it affects negativelly women's economic empowerment ((71)). Domestic violence is one of the leading causes of female mortality in the world. It accounted for 58% of women homicides in USA and 27% in Brazil in 2013 ((72);(73)). Around 38% of the global number of female murders are committed by intimate partners ((71)).

The prevalence of partner battering and homicide across countries has contributed to the implementation of targeted policies. In recent years, numerous legislative initiatives have been enacted to deal with domestic violence. Between 2006 and 2012, many Latin America countries have passed laws to prevent and punish it, such as Brazil, Costa Rica, Mexico, Venezuela, Colombia, Guatemala, Argentina, El Salvador, and Nicaragua ((74)). According to (71), this sort of laws exist in more than 127 countries. They are recent and were introduced over the past 25 years. Most of them covers physical, sexual and psychological violence. However, economic violence is rarely covered.

This paper studies the Maria da Penha Law (MPL), passed in Brazil in 2006. That act aimed at introducing effective mechanisms for preventing and punishing domestic violence against women. It covers physical, sexual, psychological and economic abuse. The MPL was recognized as one of the best legislative initiatives of the sort by the United Nations ((75)). The MPL introduced emergency protective measures and social assistance for women at risk, created special courts, increased the penalty for domestic violence against women and stimulated the improvement of public services and institutions of attention to domestic violence victims.

We assess the effect of a domestic violence law on female aggression incidence. Specifically, we estimate the impact of MPL on female homicides occurred within the household over the years. Given that MPL exclusively covers aggressions against women, we apply the differences-in-differences methodology to identify the causal impact of MPL on domestic violence against women. The high degree of comparability between male and female household homicide rate trends within municipalities before 2006 allows us to use the former as a counterfactual to the latter. We exploit the female household homicide rate as our measure of domestic violence incidence for three reasons. First, homicides which occurred within the household are more related to family conflicts than homicides which occurred outside the household. According to (76), around 90% of the global number of domestic murders are committed by people close to the victim. Second, the number of homicides are less likely to be underestimated than other domestic violence proxies, such as self-reported cases of aggression or data on hospitalization. When a homicide occurs, the cause of death must be reported in official documents in order to bury the dead. The homicides are registered as deaths caused by aggression. In contrast, self-reported data usually suffer from underreporting and may be biased according to the respondent's characteristics. Similarly, measures based on hospitalization may fail in the identification of injuries caused by domestic violence. Third, the local incidence of household homicides are potentially correlated with the local incidence of abuse episodes. The homicide act may be an intended or unintended consequence of frequent episodes of aggression.

We use data on homicides from 2001 to 2014 from DATASUS, a databank maintained by the Ministry of Health ((77)). The domestic violence measure is the household homicide rate per 100,000 inhabitants. It is constructed for each gender and municipality. The data on municipal population and municipal characteristics are taken from the Demographic Census 2000 ((78)), published by the Brazilian Institution of Geography and Statistics (IBGE). The data on the existence of women's police stations are taken from the survey Munic 2014 ((79)), also developed by IBGE. We combine this data with information on municipal judicial structure from *Justiça Aberta* (*Open Justice*). It is a database managed by National Justice Council ((80)) and was provided for us by National Justice Council.

We find that MPL prevented a 19% increase in female household homicide rates. We observe a progressive increase in treatment effect estimates over time. The MPL effectively reduced female homicide rates from 2010 onwards. The impact was concentrated in small municipalities, in which the MPL prevented a 39% increase in female household homicide rates. Localities with a higher proportion of women susceptible to partner abuse showed the greatest effect among those with less than 100,000 inhabitants. Specifically, these are municipalities where the population is younger and women have lower educational levels, lower incomes, divorce less and work in formal or informal labor market with less frequency.

The effect of MPL on domestic violence is independent of the presence of specialized legal bodies in town. Despite the significant reduction in female domestic homicide rate among small municipalities, less than 7% of them had domestic violence special courts in 2009 and only 4% of them had women's police stations in 2009. Municipalities that did not count with women's police stations showed a higher reduction in female household homicide rates than municipalities that had women's police stations, most of them created before 2006. However, conditional on urbanization rates, we find that municipalities that did not count with women's police stations showed a lower reduction in female household homicide rates than municipalities that had women's police stations. In turn, results are statistically equivalent between places with and without domestic violence special courts. Conditional on urbanization rates, municipalities that did not count with women's police stations showed a lower reduction in female household homicide rates than municipalities that had women's police stations. These results reflect the selection bias in the creation of women's police stations and specialized courts. These bodies are concentrated in municipalities that present high urbanization rates, high average household per capita income and high levels of education. Most of them were created in medium or large municipalities. The low prevalence of bodies specialized in domestic violence in places where the MPL had larger effect show that common measures introduced by the law are effective mechanisms of domestic violence reduction.

Our findings suggest that Maria da Penha Law has an important function of protecting women most at risk. The literature on the determinants of domestic violence usually analyzes the risk of aggression based on women's bargaining position in the household, men's desire for resources or partner control and social norms. According to the first view, female income and labour participation rates, together with their related outcomes, reduce the probability of partner violence ((81)). (82) verifies the reduction of violence against women due to a decrease in the gender wage gap even in households where women do not work. In line with the first and second views, (83) find that governmental transfers to women reduce physical abuse, but increase violent threats, which they interpret to be a mechanism used by men to extract rents from their partners.

In accordance with the social norm view, (84) attest the importance of historically determined norms about marriage, living arrangements and female labor on intrafamily violence. Additionally, they find that current women economic power is positively correlated with probability of aggression. Reinforcing the importance of gender identity, (85) argue that women's relative income negatively affects marriage satisfaction and probability of divorce, as well as women's work decisions. According to (86), social norms also determine the enforcement of laws, because they affect the whistle-blowing decision. The authors discuss that legal reforms which differ greatly from social norms may produce adverse incentives, while gradual legal reforms can change social norms.

Our study relates mostly to the incipient literature on policies aimed at combating domestic violence. (87) provides evidence that laws that require the arrest of abusers when domestic violence is reported increased spouse homicides due to a reduction in report rates. (88) concludes that policies which prohibit the prosecutor to drop charges of domestic violence increases the reporting. (89) exploit the introduction of unilateral divorce laws in the United States and find that they reduced the domestic violence for both men and women and reduced rates of females murdered by their spouses and female suicide. MPL was evaluated by (76). They estimate a decrease in female household homicide rates , which was greater in regions with the highest rates before the introduction of the law. Besides methodological differences, our analysis differs in that we investigate the response to a legal reform over the years and exploit regional differences in the effectiveness of the law according to the population characteristics and to the existence of special courts and women's police stations. (90) demonstrates that failures on explicitly modeling the response of a law over time can significantly bias the treatment effect estimate.

We contribute to the understanding of ways of reducing partner violence against women. The effect of this kind of policy is hard to predict since the assault is usually perpetrated by partners within the household and reported mostly by the victim or a family member. Maria da Penha Law implemented greater legal changes and institutional innovations than those evaluated in related researches, to the best of our knowledge. There are few quasi-experimental studies in this literature. The identification of the effect is possible because the policy focuses on women and male and female domestic homicides rates present similar trends before the law introduction. Given Brazilian regional heterogeneity, the implementation of MPL at the national level enhanced the comprehension of policy consequences in different contexts. We highlight our contribution to the recognition of effective policies for disproportionally affected women.

This paper is organized as follows. Section 2 dicusses determinants of domestic violence and the Maria da Penha law. Section 3 describes the data. Section 4 reports the empirical strategy and identification assumptions. Section 5 discusses the results. Section 6 presents the concluding remarks.

3.2 Institutional background

3.2.1 Determinants of domestic violence

Domestic violence prevalence around the world has stimulated the study of its patterns. Many surveys report that usually the victims suffer repeated aggressions. Almost 60% of the women who call to the Brazilian 180 hotline relate that they suffer daily abuses ((91)). Physical aggressions are reported in most of these records. According to (42), more than half of the women who were hospitalized because of domestic violence in 2011 had been hospitalized before for the same reason. Abusive behaviors are associated with physical aggression to increase the abuser control of victim's life.

Brazil is the seventh country in the world ranking of crimes against women ((92)). In country, women are more prone to be victims of domestic violence than men. Among those hospitalized because of domestic violence, sexual abuse or related aggressions in 2009, more than 65% were women. Most of these episodes occurred within the victim's household ((91)). Usually the abuser is the partner, especially considering victims aged from 20 to 50 ((42)). The female homicide rates are higher for women aged 15 to 29.

Poor and minority women are disproportionately affected by intimate parter violence around the world ((70); (71)). The prevalence of abuse among the poorest women are in line with the intra-household bargaining view of domestic violence. However, these evidences do not invalidate other views. Partner-control behavior and gender-related norms may be correlated with female economic empowerment and other characteristics of the victim.

Psychological and some economic researches focus on the cyclicality of domestic violence episodes. Walker's cycle theory of violence determines three phases of domestic aggression: the tension-building phase, the battering fase and the reconciliation phase ((93)). (72) discuss the time inconsistent preferences in the context of domestic violence. In line with this view, policies that offer psychological support to victims or that prohibit the drop of the charge may affect reporting and aggression incidence. Specifically, no-drop policies resulted in a increase in domestic violence reporting in the United States ((72)).

The increase in domestic violence report is a major policy concern. Many women who are victims of abuse do not report it to authorities. The main reasons for this choice are: fear of abuser, desire to protect him, belief that violence is a private matter, and perception of criminal justice system as ineffective in dealing with this sort of crimes ((94)). In next subsection, we discuss the Maria da Penha Law measures to increase reporting, improve the criminal justice system effectiveness in female agression and changing gender-norms.

3.2.2 Maria da Penha Law

The Maria da Penha Law was introduced in Brazil in August 2006. It implemented a set of measures to reduce domestic violence. The MPL exclusively covers acts of aggression perpetrated against women. The cases of domestic violence against men are covered by the law number 9099/1995. Before the introduction of MPL, domestic violence against women were also judged according to law number 9099/1995, with the exception of cases of sexual assault, severe aggression and homicides. This law establishes that mild domestic aggressions are minor offenses. The minor offenses are punished with less than two years of imprisonment. (95) report that around 90% of domestic violence cases tried in accordance with that law were closed. Among those punished, judicial decisions usually determined only monetary penalties.

The main objective of Maria da Penha Law (law number 11340/2006) is to curb and prevent domestic and family violence against women. The strategy implemented is twofold. First, the improvement and integration of specialized criminal justice and institutions that support women at risk in order to increase their effectiveness and adequacy. Second, the promotion of broad campaigns to change social norms.

MPL brings about a set of innovative policies to reduce female exposure to domestic violence. First, it introduces urgent protective measures, which must be determined by the judge within less than 96 hours after the risk reporting to police. The main measures targeted at agressors are the judicial orders for guaranteeing their eviction from home and physical separation. On the other hand, the main measures targeted at victims are referrals to support and protection services. Second, it creates domestic violence special courts. In districts without them, domestic violence cases are allocated to the criminal courts and have priority over other cases, according to MPL. Third, it rules out the application of pecuniary penalties in cases of domestic violence against women. The new punishment is 1 to 4 years of imprisonment. Fourth, MPL stimulates the creation of legal bodies to help women at risk, such as women's police stations, shelters and legal, psychological and health assistance centers. Fifth, it provides vulnerable women with the possibility of legal protection against dismissal and judicially determined access to social assistance benefits. Other MPL innovations include the specialization and intensification of public prosecution acts on domestic violence cases and the possibility of providing psychological and health assistance to agressors ((96)).

Legal changes introduced by the law have been valid since August 2006. However, complementary criminal justice institutions and assistance centers are being implemented gradually. A hotline for assisting women in this situation, the toll-free number 180, was created in 2005, promoted its first national-level campaign in 2009, and assumed the role of a whistle blowing hotline in 2013 ((97)). In 2013, the service was available for 56% of Brazilian municipalities ((97)). In turn, the first Brazilian women's police stations were created in 1985 ((98)). The number of municipalities in Brazil that had women's police stations was 345 in 2004 and 427 in 2012, most of them concentrated in more developed States, specially the state of São Paulo ((79); (96)). (74) show that the creation of women's police stations in Brazilian municipalities is associated to a reduction in women homicide in metropolitan regions and among those aged 15 to 24 between 2004 and 2009. The availability of exclusive domestic violence special courts or specialized centers in criminal courts is also limited. According to the *Justiça Aberta* database ((80)), only 359 municipalities were located in districts with that judicial structure.

The effectiveness of MPL instruments seems to have improved over the years. The number of men framed in Maria da Penha Law increased from 1,825 in 2008 to 4,248 in 2013[1] ((99)). The cumulative number of domestic violence reports received by the 180 hotline totalled 1 million in 2010 and 3 million in 2012 ((97)). According to (100), an opinion survey, the proportion of respondents who believed that anyone who is aware of aggression against

[1]There were 9 women framed in MPL in 2008 and 234 in 2013 ((99)). This statistic may reflect a higher enforcement of the law or a higher number of domestic violent events.

women by family members should report rose from 42% in 2009 to 64% in 2015. They show that nearly all women interviewed in both waves were aware of the law. As discussed by (76), MPL affects domestic violence through three basic channels. First, the increase in aggression penalties. Second, the reduction of risk for victims, the mitigation of their economic vulnerability and the changing of social norms, all of which incentivized greater reporting of those felonies. Third, the promotion of the effectiveness of legal mechanisms. The law impact on female homicide is consistent with deterrence and incapacitation mechanisms.

3.3 Data

We construct a pooled data panel containing annual information per gender for each municipality from 2001 to 2014. The measure of domestic violence used in this paper is the household homicide rate per 100,000 inhabitants per gender in each municipality. It matches the local number of homicides per 100,000 inhabitants in which the death occurred within the household, calculated separately according to the sex of the victim. Data on the annual number of homicides and population from 2001 to 2014 were extracted from DATASUS, a health database maintained by the Ministry of Health ((77)). We consider homicides those deaths caused by aggression.

In order to characterize municipalities, we rely on the Census ((78)). From this survey, we construct the following municipal variables for all women aged 15 or older: median years of education; the proportion who are employed; median wage; and proportion of ever-married women that are divorced. Information on the presence of women's police station in 2009 in each municipality is available in the survey Munic ((79)). We collect data on domestic violence special courts existence in 2009 from the *Justiça Aberta*, a database managed by National Justice Council ((80)).

Most of our analysis is conducted separately for small municipalities and medium or large municipalities due to the great difference among them in terms of socio-economic development. We apply the same criteria used by IBGE, which categorizes as small municipalities those with less than 100,000 inhabitants, while all others are considered to be medium or large. Table 1 presents the unweighted summary statistics of our annual municipal database from 2001 to 2014. Among the 5568 Brazilian municipalities studied[2], 5314 are small and 254 are medium or large. During the period studied,

[2] There were 5570 municipalities in Brazil in 2013

the first group comprised around 46% of the Brazilian population, while the second group comprised the remainder of the population. On average, Brazilian municipalities present a female domestic homicide rate per 100,000 inhabitants of 1.3, which is the same for both group of municipalities, and a corresponding male domestic homicide rate per 100,000 inhabitants of 4.2 for small municipalities and 5.6 for medium and large municipalities. These statistics have greater variability among small municipalities. In turn, the average female homicide rate per 100,000 inhabitants is 3.3 for small municipalities and 4.8 elsewhere. The average male homicide rates per 100,000 inhabitants are 25.6 and 59.7, respectively. We focus on domestic homicide rates and attest in section 5 that male and female rates follow a parallel trend before the MPL implementation, even when accounting for municipal subgroups.

Domestic violence courts and women's police stations had been implemented in 7% of the Brazilian municipalities in 2009. The first were present in 6.5% of small municipalities and the second in 3.8%. Among medium and large municipalities, 18.3% had those courts, while women's police stations were present in 76.4% of these municipalities. This suggests a selection in the creation of these specialized judicial bodies.

Analyzing characteristics of women aged 15 or older, we observe that in the year 2000 the average municipal median of their educational level was only 4.1 years. Their wages were concentrated around the minimum wage of 131 Reais, in small municipalties, and around 260 Reais, in medium and large municipalities. The average proportion of women in who were employed was 28% for the whole sample. While the average proportion of divorced women was 4.7% for small municipalities and 9.6% for all others. Finally, the proportion of the population aged 0 to 29 years was almost 60% on average.

3.4 Empirical Strategy

This study aims to identify the treatment effect over the years of legal reforms to reduce domestic violence against women on female household homicide rate. We exploit the fact that the law is only applicable in cases where the victim is a woman. We obtain differences-in-differences estimators through the following flexible model:

$$Y_{git} = \alpha + \beta' Women_g + \sum_{\tau=2001}^{2014} \eta'_\tau Year_t^\tau + \sum_{\tau=2001}^{2014} \gamma'_\tau Year_t^\tau * Women_g + \mu_i + \varepsilon_{git} \quad (3\text{-}1)$$

where Y_{git} is the homicide rate for gender g in municipality i and year t; $Women_g$ equals 1 for women observations and 0 otherwise; $Year_t^\tau$ equals 1 if year t is equal to τ and 0 otherwise; μ_i is the municipal fixed effect; and ε_{git} is the error term.

The coefficients of interest are γ_τ, which estimate difference in household homicide rates between gender groups foreach year, attributable to the legal reform. The main identification hypothesis is the non-existence of unobserved sources of variation that disproportionally affect male or female domestic homicide rate. The validity of the parallel trends hypothesis before the law implementation is testable. It is satisfied if the interactions between the pre-2006 dummy of years and the gender indicator are not statistically different from zero. We test this hypothesis for the overall set of regressions. The effect of MPL on male domestic homicide rates may also affect our estimates. The MPL can influence male domestic homicides through the law direct application for men or through the reduction of male deaths due to women self-defense. The first seems to be inexpressive given the small number of women framed by MPL. Even if both are significant, they introduce an attenuation-bias. In that case, the law effect is larger than estimated.

In order to obtain the treatment effect for the whole treatment period, we also estimate the following regression:

$$Y_{git} = \alpha + \beta' Women_g + \eta' Post_t + \gamma' Post_t * Women_g + \mu_i + \varepsilon_{git} \quad (3\text{-}2)$$

where Y_{git} is the homicide rate for gender g in municipality i and year t; $Women_g$ equals 1 for women observations and 0 otherwise; $Post_t$ equals 1 if year t is higher than 2006 and 0 otherwise; μ_i is the municipal fixed effect; and ε_{git} is the error term. The coefficient of interest is γ, which estimates the treatment effect.

The models are estimated using weighted least squares. We weight for average municipal population in order to approximate the average partial effect for the whole population in the potential presence of hetereogeneous effects and heteroskedastic error terms. Weighting for population helps to identify the average partial effect in samples with underrepresented and overrepresented subgroups ((101)). As suggested by (101), we study the heteronegeity and present treatment estimates for groups of municipalities separately, according to their population and socio-economic characteristics. The standard errors estimated are clustered at the district level[3] due to a potential serial correlation of error terms for municipalities within the same judicial structure ((102)).

3.5 Results

3.5.1 Effect of MPL on domestic violence

Maria da Penha Law is considered innefective by the public opinion at large because female homicide rates increased after 2006, year of introduction of the law. Figure 3.1 shows the evolution of these rates per gender. We observe that female homicide rate reduced between 2000 and 2007 and increased in the following years. The evaluation of the MPL impacts based on the number of homicides is inaccurate given that this total includes more deaths than those caused by domestic violence. Violent deaths are correlated with other factors besides familiar abuses, such as weapon availability and illegal drug markets ((55)). Moreover, this interpretation of the increase in fatal aggressions against women are inappropriate since the female homicide rate could be higher in the absence of the law. For this reason, it is necessary to compare the rates after the law introduction with the expected rates if MPL had not been passed. The male homicide rates differ from female homicide rates even before 2006, as shown in Figure 3.1. Consequently, the former can not be exploited as a counterfactual for the latter.

Figure 3.2 presents the evolution of the household homicide rates in Brazil. This measure reflects the relative number of deaths caused by aggression in which the death occurred within household. We observe that male and female household homicide rates varied similarly before the law introduction in 2006. Between 2006 and 2010 both female and male rates increased. In the last years, female household homicide rates stopped growing , while male household homicide rates increased throughout the same period. The graph suggests that male household homicide rates are adequate counterfactuals for female household homicide rates and that MPL moslty affected domestic violence after 2010. These hypotheses are formally tested and discussed below.

In order to obtain the treatment effect, we estimate the model specified in Equation 3-1 for two groups: small and medium or large municipalities. Figure 3.3 reports our coefficients of interest, those corresponding to the interaction between year dummies and a gender dummy. Firstly, the graphs show that the

[3] A district is a judiciary unit with one or more municipalities.

parallel trend hypothesis is satisfied, specially for small municipalities. Proportional changes in male and female household homicide rates were statistically equivalent before the law introduction. This comparability between men's and women's rates is a necessary condition for our identification strategy. Secondly, the graphs show that the effect is gradual for small municipalities and null for medium or large municipalities. We observe that the law effectively reduced the number of female homicides from 2010 onwards.

The progressive reduction in female deaths caused by aggression within the household reflects the progressive implementation of mechanisms to protect women at risk and the progressive confidence in these mechanisms after MPL introduction. As a consequence, we expect a rise in domestic violence reports and criminal justice effectiveness in jugding aggressors and protecting victims over the years. The social norm updating process may also be responsible for this dynamic. These interpretations are consistent with some available data on criminal justice system. According to (99), the number of incarcerated men framed by MPL presented a gradual increase over the years. It took place in conjunction with the evolution of 180 hotline reports. The accumulated number of reports in 2012 was three times higher than that number in 2010 ((97)). Additionally, a survey on female aggression assault show an increase in the proportion of interviewees who belive that abuses must be reported to authorities. This suggest a transition in social acceptance of domestic violence.

Table 3.2 presents the average treatment effect for the whole period. It is estimated by the coefficient of the interaction between *Post 2006* and *Women*, as indicated in Equation 3-2. Maria da Penha Law prevented a 18.9% increase in female household homicide rate. The effect occurred entirely in small municipalities, for which MPL prevented a 38.6% increase in that rate between 2007 and 2014. This coefficient indicates the female household homicide rate per 100,000 inhabitants would be 0.55 higher in the absence of the law in these towns.

The small municipalities are very important in country. More than 90% of Brazilian municipalities are classified as small (have less than 100,000 inhabitants). In addition, they comprise almost 50% of the national population. Despite the reduction in relative women's homicides in household in those municipalities, they experienced an increase in violence in recent years. From 2000 and 2010, the homicides rates in small municipalities had an increase of more than 40%. By other hand, medium and large municipalities experienced a reduction in homicides rate in the same period.

3.5.2

Heterogeneous effects and further results

In order to understand why the MPL effect is concentrated in small municipalities we test two hypotheses. First, the people more affected by the law resides in small municipalities. Second, the effectiveness of MPL is a function of per capita supply of resources to help women at risk. To test the first hypothesis, we estimate MPL heterogeneous effects across groups of municipalities classified according to the characteristics of their female populations. Women in small municipalities present lower levels of education, lower wages, lower probability of being employed, and lower divorce rates than women in medium and large municipalities. Consequently, we should find that women with these characteristics are the most affected by the law if the reason for MPL effect concentration is the heterogeneous spatial distribution of female population. To test the second hypothesis, we estimate the heterogeneous effects of MPL across municipalities with and without judicial bodies specialized in domestic violence. Specifically, we focus on the existence of women's police stations and specialized courts. If this hypothesis is valid, we should find that small municipalities which count with these bodies are the most affected by MPL or we should find that they exist in most of the small municipalities. These results are necessary for supporting the hypothesis that heterogeneous spatial distribution of these bodies in association with the local population are the reason for MPL effect concentration.

We conduct an analysis of heterogeneous effects for small municipalities. We focus on them because the MPL effect is fully concentrated in these places. Figures 3.4 and 3.5 show the effect of MPL on domestic violence incidence in municipalities where women had low and high levels of education in 2000, respectively. Municipalities with female educational levels below the median are considered to have a low level, with all others havinga high level. The effect is concentrated in the first group of locations, where MPL prevented and increase of almost 64% in female household homicide rates, as presented in 3.3. Educational level may affect domestic aggression through an increase in women's intrahousehold bargaining level, since it is positively correlated with financial independence and potentially correlated with knowledge on how to conduct a divorce and protect her rights. Couple's formal education levels, which are positively correlated, may affect gender identity norms and violence culture.

In figures 3.6 to 3.9 we observe that MPL had a greater impact in small municipalities where a lower proportion of women work in formal or informal labor market, and where women receive lower wages. The estimated reduction in domestic homicide rates is larger than 72% in these locations.

Figures 3.10 to 3.11 show that the reduction in domestic violence homicide rate was concentrated in municipalities with a low proportion of divorced women. Table 3.6 reports that MPL prevented an increase of almost 75% in domestic homicide rates in these locations. Rates of divorce among ever-partned women potentially reflect the local degree of female independence, as well as differences in the social perception of divorce in each municipality.

Heterogeneous effects analyses based on municipal characteristics of women are, as usual, correlational analyses. These municipal characteristics are correlated with other local traits which may have driven the differences in the estimated impact of MPL. However, our whole set of results are consistent with the literature on the determinants of domestic violence. Specifically, they are consistent with the views that emphasize the role of women's intrahousehold bargaining power and the role of social norms. This fact reinforces the importance of women educational level, work outcomes, and the probability of divorce on the incidence of domestic violence. Consequently, we may interpret the concentration of MPL effect on small municipalities as a result of the heterogeneous spatial distribution of women according to their characteristics.

We next investigate whether the decrease in violence is different for small municipalities that have or not domestic violence courts and women's police stations. First, we test if municipal characteristics can predict their existence. As shown in Table 3.7, we find that urbanization rate, median years of study, and log of median household per capita are positively correlated with the availability of domestic violence courts and women's police stations in the municipality. Figures 3.12 and 3.13 suggest a significant effect of MPL for small municipalities which have domestic violence courts and a non-significant effect for small municipalities which do not have these courts. Columns (2) and (6) of Table 3.8 show that the impact estimates are statistically identical for both, but have a larger variance for the group where these courts are available. Columns (4) and (8) of Table3.8 shows that controlling for urbanization rates, we find a higher effect in places that have access to domestic violence courts. This difference occurs because domestic violence courts were created mostly in more urbanized areas and the effect of MPL is larger where women have lower income levels and lower levels of other outcomes related to urbanization. In turn, for small municipalities with women's police station, most of them created before MPL was implemented, we find no effect of the policy, while for small municipalities without women's police station, we find a significant policy effect. In contrast, controlling for urbanization rates, we find a significant reduction in relative household homicide rate in places where women's police station are available, and these reductions are larger in those locations than in locations where these stations are not available.

We verify that the existence of women's police station and special domestic violence courts are correlated with the reduction in female household homicide rate conditional on urbanization rates. However, the MPL impact is not entirely driven by the creation of special criminal justice institutions, given that the legal reform had a large impact in small municipalities where they are not present, which are more than 90% of the small municipalities. Therefore, we conclude that common mechanisms implemented by the law play an important role in reducing domestic violence. The effect of MPL was not driven by the creation of specialized bodies, despite their role in reducing gender-related violence.

As robustness checks, we estimate our main model controlling for state-time dummies, presented in Table 3.10, and weighting for municipal population by gender, presented in Table 3.11. We obtain very similar coefficients. Additionally, we estimate our main model for a sample of municipalities with a record of at least one female household homicide for most of the period studied. We find a non-significant reduction of 22% in the difference between female and male household homicide rates.

3.6 Conclusion

This paper analyzes the effect of legal reforms aimed at reducing domestic violence on female household homicide rates. We investigate the Maria da Penha Law (MPL), passed in Brazil in 2006. The MPL introduced legal mechanisms for curbing domestic violence crimes and protecting the victims. One of its main innovation was the urgent protective measures for women at risk. The law also created domestic violence special courts and incentivized the creation of new women's police stations. Concurrently, the punishment for this sort of crimes became more severe. In general, the MPL introduced measures to increase abuse reports and criminal justice effectiveness in dealing with domestic violence. Furthermore, MPL stimulated campaings against gender-related aggressions.

We use a differences-in-differences approach to estimate the effect of MPL on domestic violence. We exploit the high degree of comparability between male and female household homicide rates before the introduction of the law. We find that MPL prevented an increase of 19% in female household homicide rates. This effect was concentrated in small municipalities, with less than 100,000 inhabitants. In those localities, MPL prevented an increase of 39%

in female household homicide rates. The relative reduction in those rates in comparison with male rates was significant after 2009. The gradual increase of the effect suggests that the law depends on the implementation of services to protect women, on popular confidence in its effectiveness, and on awareness campaigns. They potentially affect the incidence of domestic violence through an increase in reporting, an increase in the probability of punishment itself, and cultural changes.

The MPL effect is larger where women presented lower levels of education, lower labor market participation rates, lower wages and lower rates of divorce. These are the more vulnerable women, according to the literature based on intrahousehold bargaining and social norms. There was a relative reduction in female household homicide rates for small municipalities regardless of the existence of special domestic violence courts or women's police stations. Since the effect is concentrated in small municipalities and those legal bodies are present in few places, we conclude that the Maria da Penha Law effect is mostly driven by the common policies implemented by the reform. Unfortunately, our study is unable to disentangle the effective from the innefective measures introduced by Maria da Penha Law.

Table 3.1: Descriptive statistics

Variable	All Municipalities	Small Municipalities	Medium and large Municipalities
Female domestic homicide rate	1.3	1.3	1.3
	[6.1]	[6.3]	[1.4]
Male domestic homicide rate	4.2	4.2	5.6
	[10.7]	[10.9]	[4.3]
Female homicide rate	3.3	3.3	4.8
	[9.5]	[9.6]	[3.5]
Male homicide rate	27.1	25.6	59.7
	[35.4]	[34.3]	[41.5]
Existence of domestic violence court 2009	7.0	6.5	18.3
	[25.5]	[24.6]	[38.7]
Existence of women's police station 2009	7.1	3.8	76.4
	[25.7]	[19.2]	[42.5]
Women's years of education	4.1	4.0	6.7
	[1.3]	[1.1]	[1.3]
Proportion of women in work	28.4	27.8	39.7
	[9.7]	[9.6]	[5.7]
Women's wage	136.9	131.0	259.0
	[77.6]	[72.4]	[81.9]
Proportion of divorced women	4.9	4.7	9.6
	[3.0]	[2.9]	[2.3]
Proportion of population aged 0-29	58.7	58.7	57.8
	[6.5]	[6.6]	[5.2]
Municipalities	5568	5314	254

Notes: Averages of annual municipal statistics. Standard deviations in brackets. The municipalities with less than 100,000 inhabitants are classified as small municipalities. The municipalities with 100,000 inhabitants or more are classified as medium or large municipalities.

Figure 3.1: Homicide rates over time

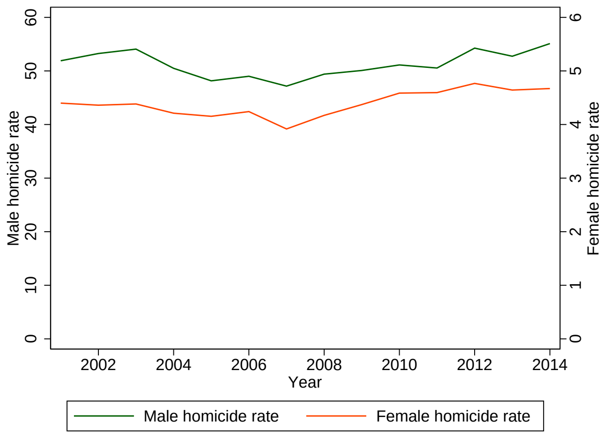


Figure 3.2: Household homicide rates over time

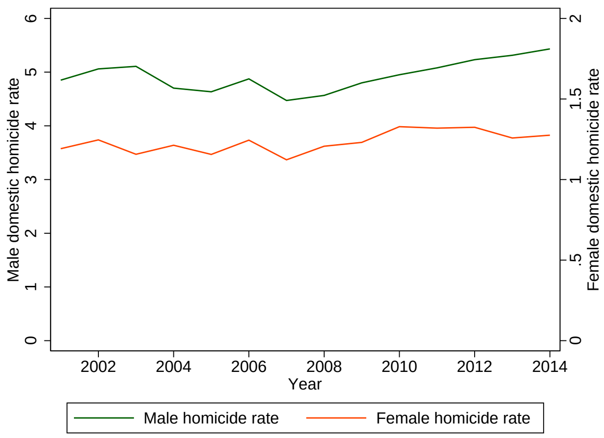


Figure 3.3: Effect of Maria da Penha Law on women household homicide rate

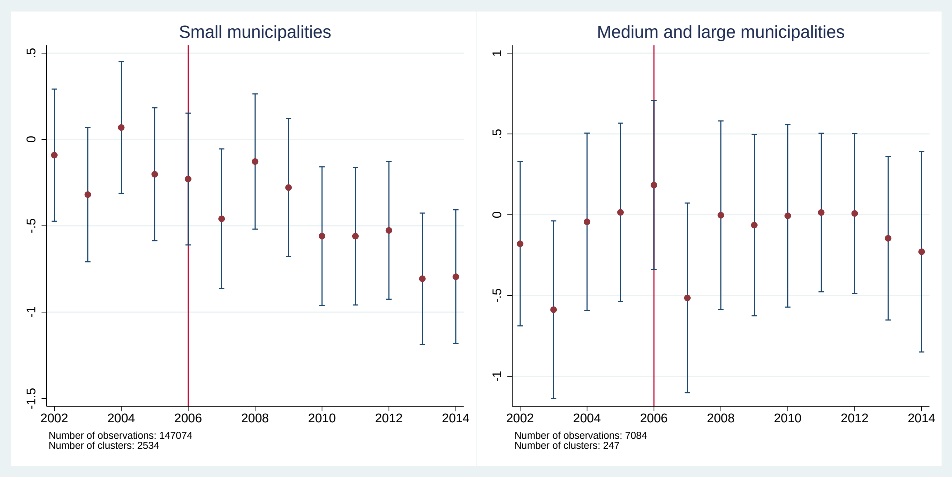


Table 3.2: Effect of Maria da Penha Law on women household homicide rate

	Log household homicide rate		
	(1)	(2)	(3)
Post 2006=1	0.323***	0.784***	-0.0829
	(0.0906)	(0.0703)	(0.108)
Women=1	-2.674***	-2.768***	-2.590***
	(0.111)	(0.0717)	(0.184)
Post 2006=1 × Women=1	-0.189**	-0.386***	-0.0155
	(0.0815)	(0.0807)	(0.120)
Observations	154158	147074	7084
R^2	0.483	0.251	0.338
Municipal classification	All	Small	Medium and Large

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population.
Significance levels: * 10%, ** 5%, *** 1%

Figure 3.4: Effect of Maria da Penha Law on women household homicide rate in small municipalities with low women's level of education

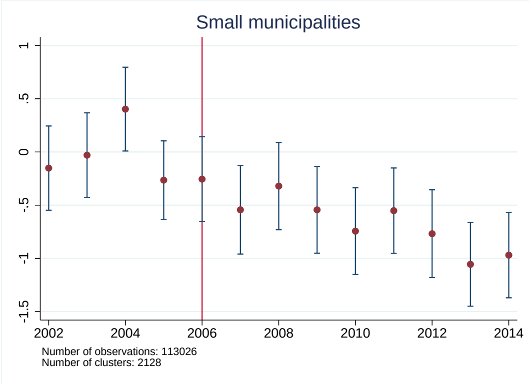


Figure 3.5: Effect of Maria da Penha Law on women household homicide rate in small municipalities with high women's level of education

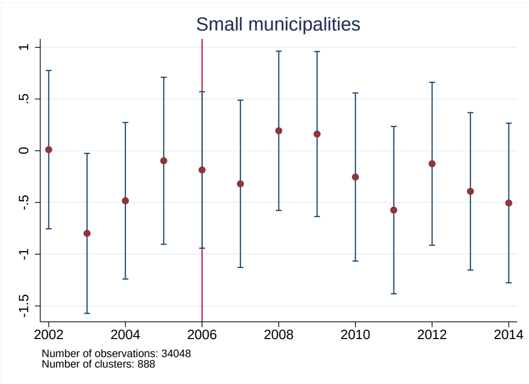


Table 3.3: Effect of Maria da Penha Law on women household homicide rate in small municipalities classified according to the women's level of education

	Low level		High level	
	(1)	(2)	(3)	(4)
Post 2006=1	1.088***	1.020***	-0.00643	0.392***
	(0.0834)	(0.0782)	(0.0923)	(0.127)
Women=1	-2.374***	-2.299***	-2.803***	-3.547***
	(0.0832)	(0.0750)	(0.164)	(0.133)
Post 2006=1 × Women=1	-0.621***	-0.637***	-0.00227	0.0320
	(0.0927)	(0.0831)	(0.0978)	(0.158)
Observations	113390	113026	40768	34048
R^2	0.244	0.228	0.440	0.262
Municipal classification	All	Small	All	Small

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Significance levels: * 10%, ** 5%, *** 1%

Figure 3.6: Effect of Maria da Penha Law on women household homicide rate in small municipalities with low proportion of women working (18+)

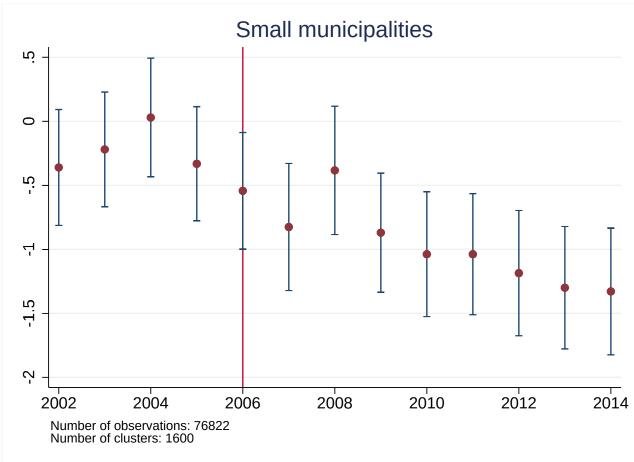


Figure 3.7: Effect of Maria da Penha Law on women household homicide rate in small municipalities with high proportion of women working (18+)

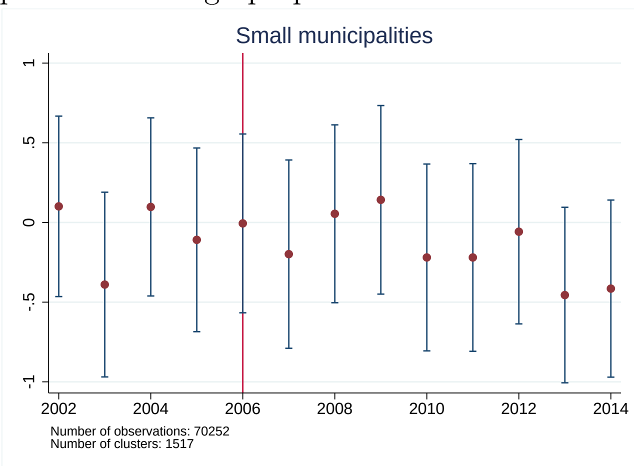


Table 3.4: Effect of Maria da Penha Law on women household homicide rate in small municipalities classified according to the proportion of women working (18+)

	Low level		High level	
	(1)	(2)	(3)	(4)
Post 2006=1	1.157***	1.080***	0.114	0.573***
	(0.102)	(0.0939)	(0.0930)	(0.0975)
Women=1	-2.317***	-2.263***	-2.763***	-3.127***
	(0.0973)	(0.0911)	(0.144)	(0.100)
Post 2006=1 × Women=1	-0.792***	-0.759***	-0.0375	-0.120
	(0.106)	(0.0996)	(0.0912)	(0.116)
Observations	77074	76822	77084	70252
R^2	0.234	0.221	0.465	0.258
Municipal classification	All	Small	All	Small

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Significance levels: * 10%, ** 5%, *** 1%

Figure 3.8: Effect of Maria da Penha Law on women household homicide rate in small municipalities with low women's wage

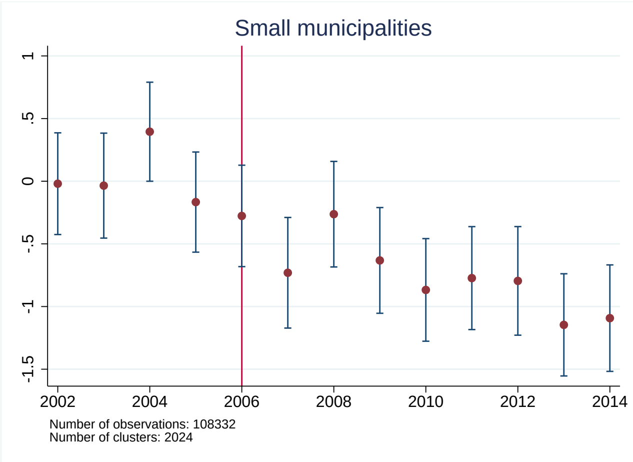


Figure 3.9: Effect of Maria da Penha Law on women household homicide rate in small municipalities with high women's wage

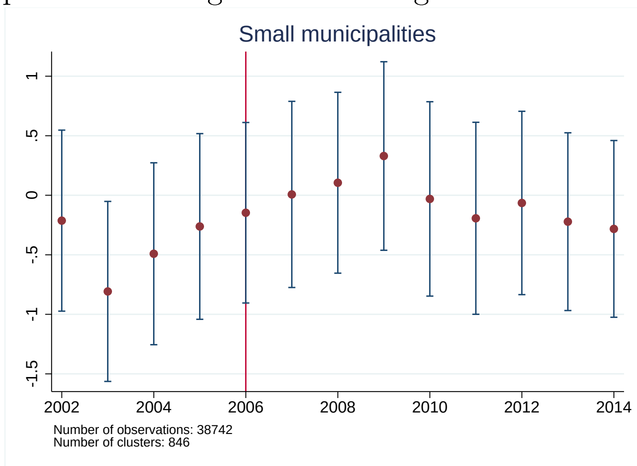


Table 3.5: Effect of Maria da Penha Law on women household homicide rate in small municipalities classified according to the women's wage level

	Low level		High level	
	(1)	(2)	(3)	(4)
Post 2006=1	1.272***	1.217***	-0.140*	0.0390
	(0.0840)	(0.0810)	(0.0834)	(0.119)
Women=1	-2.508***	-2.344***	-2.755***	-3.498***
	(0.0844)	(0.0770)	(0.165)	(0.134)
Post 2006=1 × Women=1	-0.728***	-0.770***	0.0748	0.276*
	(0.0972)	(0.0865)	(0.0965)	(0.152)
Observations	109312	108332	44846	38742
R^2	0.295	0.230	0.456	0.269
Municipal classification	All	Small	All	Small

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Significance levels: * 10%, ** 5%, *** 1%

Figure 3.10: Effect of Maria da Penha Law on women household homicide rate in small municipalities with low proportion of women separated or divorced

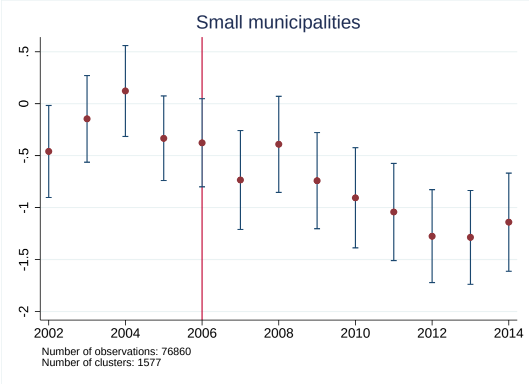


Figure 3.11: Effect of Maria da Penha Law on women household homicide rate in small municipalities with high proportion of women separated or divorced

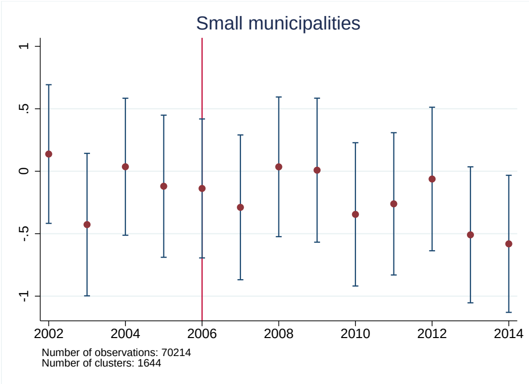


Table 3.6: Effect of Maria da Penha Law on women household homicide rate in small municipalities classified according to the proportion of women separated or divorced

	Low level		High level	
	(1)	(2)	(3)	(4)
Post 2006=1	1.219***	1.100***	0.120	0.588***
	(0.107)	(0.0949)	(0.0918)	(0.0950)
Women=1	-1.941***	-1.912***	-2.840***	-3.300***
	(0.0953)	(0.0893)	(0.148)	(0.0964)
Post 2006=1 × Women=1	-0.802***	-0.741***	-0.0496	-0.165
	(0.109)	(0.101)	(0.0902)	(0.112)
Observations	77084	76860	77074	70214
R^2	0.206	0.205	0.457	0.257
Municipal classification	All	Small	All	Small

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Significance levels: * 10%, ** 5%, *** 1%

Table 3.7: Correlation between urbanization rate and the existence of women's police station and domestic violence courts

	Domestic violence court						Women's police station					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Urbanization rate	0.00255***	0.000905***					0.0145***	0.00530***				
	(0.000977)	(0.000288)					(0.000613)	(0.000403)				
Log average household pc income			0.0303	0.0273*					0.569***	0.228***		
			(0.0449)	(0.0155)					(0.0180)	(0.0178)		
Log average years of study					0.0338**	0.0134**					0.153***	0.0865***
					(0.0158)	(0.00624)					(0.00415)	(0.00642)
Constant	-0.0431	0.0312*	-0.0268	-0.0722	-0.0107	0.0462**	-0.643***	-0.210***	-3.051***	-1.222***	-0.247***	-0.148***
	(0.0521)	(0.0174)	(0.264)	(0.0910)	(0.0534)	(0.0201)	(0.0324)	(0.0210)	(0.108)	(0.0999)	(0.0201)	(0.0166)
Observations	143584	137004	143528	136948	143584	137004	154158	147074	154102	147018	154158	147074
R^2	0.024	0.005	0.002	0.002	0.045	0.004	0.431	0.128	0.387	0.109	0.503	0.129
Municipal classification	All	Small	All	Small	All	Small	All	Small	All	Small	All	Small

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Significance levels: * 10%, ** 5%, *** 1%

Figure 3.12: Effect of Maria da Penha Law on women household homicide rate in small municipalities with domestic violence court

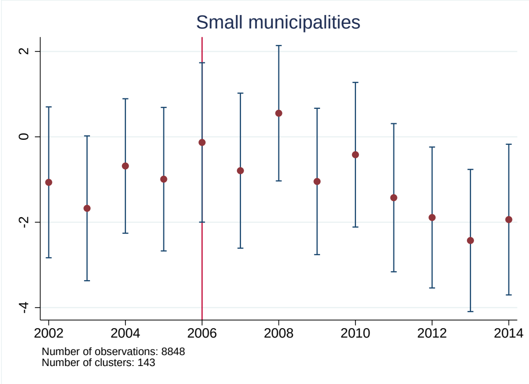


Figure 3.13: Effect of Maria da Penha Law on women household homicide rate in small municipalities without domestic violence court

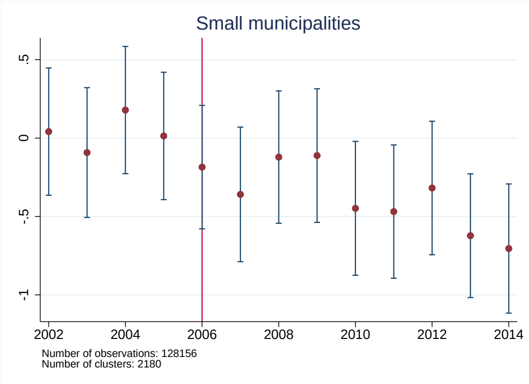


Table 3.8: Effect of Maria da Penha Law on women household homicide rate in small municipalities classified according to the existence of domestic violence court

	Without domestic violence court				With domestic violence court			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 2006=1	0.277**	0.756***	2.004***	1.390***	0.588***	1.133***	3.415***	1.563**
	(0.108)	(0.0759)	(0.184)	(0.195)	(0.169)	(0.278)	(0.815)	(0.719)
Women=1	-2.662***	-2.667***	-1.720***	-0.905***	-2.581***	-3.371***	-2.504***	0.592
	(0.132)	(0.0768)	(0.252)	(0.200)	(0.218)	(0.277)	(0.685)	(0.565)
Post 2006=1 × Women=1	-0.195**	-0.387***	-1.166***	-1.221***	-0.143	-0.415	-2.976***	-2.906***
	(0.0964)	(0.0874)	(0.207)	(0.225)	(0.183)	(0.327)	(0.759)	(0.775)
Observations	133532	128156	133532	128156	10052	8848	10052	8848
R^2	0.466	0.242	0.468	0.244	0.498	0.275	0.501	0.282
Municipal classification	All	Small	All	Small	All	Small	All	Small
Municipal characteristics	NO	NO	YES	YES	NO	NO	YES	YES

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Significance levels: * 10%, ** 5%, *** 1%

Figure 3.14: Effect of Maria da Penha Law on women household homicide rate in small municipalities with women's police station

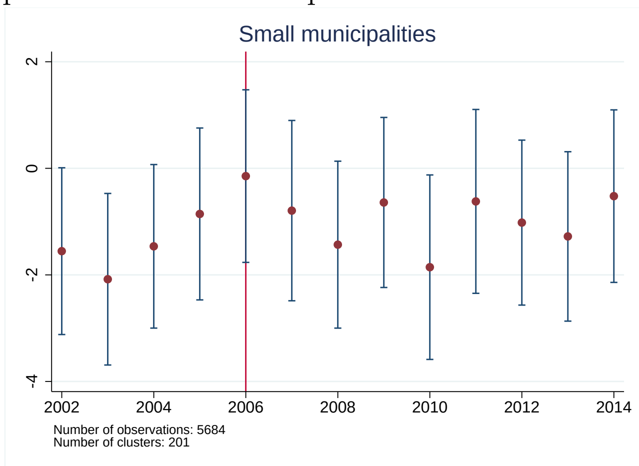


Figure 3.15: Effect of Maria da Penha Law on women household homicide rate in small municipalities without women's police station

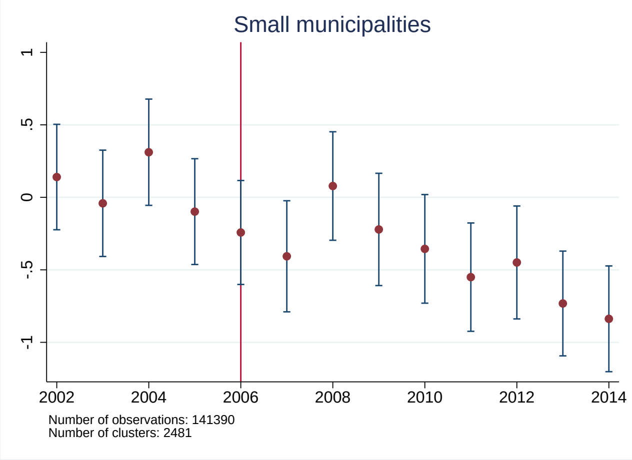


Table 3.9: Effect of Maria da Penha Law on women household homicide rate in small municipalities classified according to the existence of women's police station

	Without women's police station				With women's police station			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 2006=1	0.748***	0.795***	1.499***	1.269***	-0.0362	0.719***	6.082***	4.918***
	(0.0702)	(0.0694)	(0.185)	(0.183)	(0.111)	(0.258)	(1.371)	(1.737)
Women=1	-2.755***	-2.564***	-0.817***	-1.005***	-2.605***	-4.060***	-7.807***	-2.294*
	(0.0818)	(0.0724)	(0.205)	(0.193)	(0.180)	(0.245)	(1.499)	(1.345)
Post 2006=1 × Women=1	-0.439***	-0.446***	-1.110***	-1.075***	0.0231	-0.00323	-1.579	-4.429**
	(0.0882)	(0.0776)	(0.225)	(0.201)	(0.113)	(0.323)	(1.737)	(2.095)
Observations	143042	141390	143042	141390	11116	5684	11116	5684
R^2	0.302	0.238	0.305	0.240	0.388	0.263	0.395	0.264
Municipal classification	All	Small	All	Small	All	Small	All	Small
Municipal characteristics	NO	NO	YES	YES	NO	NO	YES	YES

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Significance levels: * 10%, ** 5%, *** 1%

Table 3.10: Effect of Maria da Penha Law on women household homicide rate - alternative specification

	Log household homicide rate		
	(1)	(2)	(3)
Post 2006=1	0	0	0
	(.)	(.)	(.)
Women=1	-2.674***	-2.768***	-2.590***
	(0.111)	(0.0718)	(0.189)
Post 2006=1 × Women=1	-0.189**	-0.386***	-0.0155
	(0.0816)	(0.0808)	(0.124)
Observations	154158	147074	7084
R^2	0.494	0.264	0.378
Municipal classification	All	Small	Medium and Large

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Significance levels: * 10%, ** 5%, *** 1%

Table 3.11: Effect of Maria da Penha Law on women household homicide rate - alternative weighting

	Log household homicide rate		
	(1)	(2)	(3)
Post 2006=1	0.323***	0.784***	-0.0834
	(0.0906)	(0.0704)	(0.108)
Women=1	-2.675***	-2.770***	-2.591***
	(0.111)	(0.0718)	(0.185)
Post 2006=1 × Women=1	-0.189**	-0.385***	-0.0157
	(0.0815)	(0.0807)	(0.120)
Observations	154158	147074	7084
R^2	0.483	0.251	0.338
Municipal classification	All	Small	Medium and Large

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for gender municipal population. Significance levels: * 10%, ** 5%, *** 1%

Table 3.12: Effect of Maria da Penha Law on women household homicide rate - alternative sample

	Log household homicide rate		
	(1)	(2)	(3)
Post 2006=1	0.0583	0.873***	-0.125
	(0.105)	(0.167)	(0.102)
Women=1	-3.002***	-5.015***	-2.549***
	(0.205)	(0.165)	(0.185)
Post 2006=1 × Women=1	-0.0235	-0.222	0.0211
	(0.105)	(0.212)	(0.116)
Observations	17136	10724	6412
R^2	0.309	0.251	0.300
Municipal classification	All	Small	Medium and Large

Notes: Standard errors clustered at district level in parenthesis. Regressions weighted for municipal population. Selected sample without low homicide rates municipalities. Significance levels: * 10%, ** 5%, *** 1%

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A
Chapter 2 - Additional results

Table A.1: Reduced-form effects with CCT default CI - level 2 court

Dependent	District Level 2
	(1)
Voters >= cutoff	0.450***
	[0.000]
Bandwidth	40
Observations	655
Districts	131
Voters >= cutoff	0.461***
	[0.000]
Bandwidth	60
Observations	1,026
Districts	206
Voters >= cutoff	0.346***
	[0.000]
Bandwidth	80
Observations	1,351
Districts	271
Voters >= cutoff	0.443***
	[0.000]
IK Bandwidth	31.307
Observations	505
Districts	101
Voters >= cutoff	0.295***
	[0.000]
CCT Bandwidth	25.574
Observations	415
Districts	83
Voters >= cutoff	0.543***
Quadratic spline	[0.077]
Bandwidth	Full sample
Observations	1,571
Districts	315
Voters >= cutoff	0.402***
Cubic spline	[0.082]
Bandwidth	Full sample
Observations	1,571
Districts	315
Year FE	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.2: Reduced-form effects with CCT default CI - judicial performance

Dependent	Log Sentences	Log Sentence per process	Log Sentence per judge	Log Allocated
	(1)	(2)	(3)	(4)
Voters >= cutoff	0.785***	0.290***	0.594***	0.248***
	[0.000]	[0.000]	[0.000]	[0.000]
Bandwidth	40	40	40	40
Observations	655	653	655	655
Districts	131	131	131	131
Voters >= cutoff	0.574***	0.226***	0.389***	0.131***
	[0.006]	[0.001]	[0.003]	[0.004]
Bandwidth	60	60	60	60
Observations	1,026	1,024	1,026	1,026
Districts	206	206	206	206
Voters >= cutoff	0.359***	0.216***	0.253***	-0.043***
	[0.006]	[0.001]	[0.003]	[0.004]
Bandwidth	80	80	80	80
Observations	1,351	1,349	1,351	1,351
Districts	271	271	271	271
Voters >= cutoff	0.694***	0.214***	0.592***	0.186***
	[0.005]	[0.002]	[0.000]	[0.004]
IK Bandwidth	53.499	89.248	45.129	55.633
Observations	926	1,389	770	951
Districts	186	279	154	191
Voters >= cutoff	0.369***	0.097***	0.168***	0.477***
	[0.000]	[0.000]	[0.000]	[0.000]
CCT Bandwidth	20.514	30.420	20.707	24.711
Observations	340	478	340	405
Districts	68	96	68	81
Voters >= cutoff	0.806***	0.125	0.693***	0.650***
Quadratic spline	[0.181]	[0.079]	[0.157]	[0.193]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,569	1,571	1,571
Districts	315	315	315	315
Voters >= cutoff	0.597***	0.202**	0.594***	0.346*
Cubic spline	[0.194]	[0.085]	[0.175]	[0.202]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,569	1,571	1,571
Districts	315	315	315	315
Year FE	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Reduced-form effects with CCT default CI - homicide rate

Dependent	Log District homicide rate	Log Seat homicide rate
	(1)	(2)
Voters >= cutoff	-0.631***	-0.595***
	[0.000]	[0.000]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Voters >= cutoff	-0.408***	-0.353***
	[0.000]	[0.000]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Voters >= cutoff	-0.310***	-0.271***
	[0.000]	[0.000]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Voters >= cutoff	-0.433***	-0.265***
	[0.000]	[0.000]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Voters >= cutoff	-0.576***	-0.657***
	[0.000]	[0.000]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Voters >= cutoff	-0.006	0.052
Quadratic spline	[0.108]	[0.114]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Voters >= cutoff	-0.072	-0.005
Cubic spline	[0.118]	[0.124]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Reduced-form effects with CCT default CI - mechanisms

Dependent	Log Experience	Log Judges	Log Courts	Log Gender
	(1)	(2)	(3)	(4)
Voters >= cutoff	0.479***	0.128***	0.087***	-0.158
	[0.028]	[0.000]	[0.000]	[0.147]
Bandwidth	40	40	40	40
Observations	357	655	655	351
Districts	80	131	131	80
Voters >= cutoff	0.204***	0.134***	0.024***	-0.009
	[0.020]	[0.002]	[0.000]	[0.088]
Bandwidth	60	60	60	60
Observations	691	1,026	1,026	660
Districts	148	206	206	147
Voters >= cutoff	0.153***	0.078***	-0.023***	0.047
	[0.020]	[0.002]	[0.000]	[0.062]
Bandwidth	80	80	80	80
Observations	986	1,351	1,351	942
Districts	208	271	271	207
Voters >= cutoff	0.152***	0.075***	0.066***	0.001
	[0.020]	[0.002]	[0.000]	[0.079]
IK Bandwidth	83.521	81.770	49.228	65.329
Observations	996	1,361	840	733
Districts	210	273	168	163
Voters >= cutoff	0.570***	0.056***	0.027***	-0.268
	[0.035]	[0.000]	[0.000]	[0.421]
CCT Bandwidth	33.450	27.078	15.033	21.756
Observations	282	425	210	169
Districts	62	85	42	37
Voters >= cutoff	0.370***	0.090	0.332***	0.082
Quadratic spline	[0.120]	[0.086]	[0.059]	[0.077]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,126	1,571	1,571	1,082
Districts	238	315	315	237
Voters >= cutoff	0.333**	0.005	0.255***	0.147*
Cubic spline	[0.132]	[0.092]	[0.064]	[0.083]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,126	1,571	1,571	1,082
Districts	238	315	315	237
Year FE	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Reduced-form effects with covariates - level 2 court

Dependent	District Level 2
	(1)
Voters >= cutoff	0.358*
	[0.191]
Bandwidth	40
Observations	655
Districts	131
Voters >= cutoff	0.358**
	[0.141]
Bandwidth	60
Observations	1,026
Districts	206
Voters >= cutoff	0.276**
	[0.114]
Bandwidth	80
Observations	1,351
Districts	271
Voters >= cutoff	0.368
	[0.247]
IK Bandwidth	31.307
Observations	505
Districts	101
Voters >= cutoff	0.245
	[0.297]
CCT Bandwidth	25.574
Observations	415
Districts	83
Voters >= cutoff	0.550***
Quadratic spline	[0.075]
Bandwidth	Full sample
Observations	1,571
Districts	315
Voters >= cutoff	0.399***
Cubic spline	[0.076]
Bandwidth	Full sample
Observations	1,571
Districts	315
Year FE	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.6: Reduced-form effects with covariates - judicial performance

Dependent	Log Sentences	Log Sentence per process	Log Sentence per judge	Log Allocated
	(1)	(2)	(3)	(4)
Voters >= cutoff	0.497*	0.370**	0.445	-0.161
	[0.261]	[0.164]	[0.298]	[0.252]
Bandwidth	40	40	40	40
Observations	655	653	655	655
Districts	131	131	131	131
Voters >= cutoff	0.374	0.251*	0.299	-0.112
	[0.242]	[0.140]	[0.254]	[0.230]
Bandwidth	60	60	60	60
Observations	1,026	1,024	1,026	1,026
Districts	206	206	206	206
Voters >= cutoff	0.216	0.229*	0.217	-0.202
	[0.225]	[0.122]	[0.228]	[0.211]
Bandwidth	80	80	80	80
Observations	1,351	1,349	1,351	1,351
Districts	271	271	271	271
Voters >= cutoff	0.498**	0.225*	0.461	-0.057
	[0.247]	[0.117]	[0.282]	[0.233]
IK Bandwidth	53.499	89.248	45.129	55.633
Observations	926	1,389	770	951
Districts	186	279	154	191
Voters >= cutoff	-0.179	0.259	-0.067	-0.372
	[0.445]	[0.179]	[0.496]	[0.333]
CCT Bandwidth	20.514	30.420	20.707	24.711
Observations	340	478	340	405
Districts	68	96	68	81
Voters >= cutoff	0.764***	0.126*	0.699***	0.612***
Quadratic spline	[0.162]	[0.071]	[0.156]	[0.148]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,569	1,571	1,571
Districts	315	315	315	315
Voters >= cutoff	0.597***	0.188**	0.636***	0.363**
Cubic spline	[0.176]	[0.078]	[0.178]	[0.158]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,569	1,571	1,571
Districts	315	315	315	315
Year FE	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Reduced-form effects with covariates - homicide rate

Dependent	Log District homicide rate	Log Seat homicide rate
	(1)	(2)
Voters >= cutoff	-0.592***	-0.533***
	[0.162]	[0.180]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Voters >= cutoff	-0.327**	-0.253
	[0.154]	[0.171]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Voters >= cutoff	-0.207	-0.157
	[0.143]	[0.159]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Voters >= cutoff	-0.383**	-0.148
	[0.156]	[0.158]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Voters >= cutoff	-0.758***	-0.639***
	[0.181]	[0.210]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Voters >= cutoff	0.073	0.128
Quadratic spline	[0.085]	[0.093]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Voters >= cutoff	0.024	0.084
Cubic spline	[0.093]	[0.101]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.8: Reduced-form effects with covariates - mechanisms

Dependent	Log Experience	Log Judges	Log Courts	Log Gender
	(1)	(2)	(3)	(4)
Voters >= cutoff	0.328	0.018	-0.003	-0.052
	[0.243]	[0.160]	[0.049]	[0.182]
Bandwidth	40	40	40	40
Observations	357	655	655	351
Districts	80	131	131	80
Voters >= cutoff	0.127	0.051	-0.049	-0.006
	[0.196]	[0.137]	[0.048]	[0.134]
Bandwidth	60	60	60	60
Observations	691	1,026	1,026	660
Districts	148	206	206	147
Voters >= cutoff	0.074	-0.002	-0.076	0.041
	[0.175]	[0.115]	[0.049]	[0.110]
Bandwidth	80	80	80	80
Observations	986	1,351	1,351	942
Districts	208	271	271	207
Voters >= cutoff	0.074	-0.004	-0.004	0.003
	[0.173]	[0.114]	[0.048]	[0.127]
IK Bandwidth	83.521	81.770	49.228	65.329
Observations	996	1,361	840	733
Districts	210	273	168	163
Voters >= cutoff	0.412	-0.135	-0.204***	0.075
	[0.288]	[0.184]	[0.063]	[0.307]
CCT Bandwidth	33.450	27.078	15.033	21.756
Observations	282	425	210	169
Districts	62	85	42	37
Voters >= cutoff	0.343***	0.051	0.314***	0.079
Quadratic spline	[0.117]	[0.068]	[0.056]	[0.072]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,126	1,571	1,571	1,082
Districts	238	315	315	237
Voters >= cutoff	0.271**	-0.027	0.242***	0.140*
Cubic spline	[0.127]	[0.072]	[0.061]	[0.078]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,126	1,571	1,571	1,082
Districts	238	315	315	237
Year FE	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: Reduced-form effects with rectangular kernel - level 2 court

Dependent	District Level 2
	(1)
Voters >= cutoff	0.464***
	[0.163]
Bandwidth	40
Observations	655
Districts	131
Voters >= cutoff	0.438***
	[0.124]
Bandwidth	60
Observations	1,026
Districts	206
Voters >= cutoff	0.259**
	[0.105]
Bandwidth	80
Observations	1,351
Districts	271
Voters >= cutoff	0.518***
	[0.199]
IK Bandwidth	31.307
Observations	505
Districts	101
Voters >= cutoff	0.530**
	[0.244]
CCT Bandwidth	25.574
Observations	415
Districts	83
Voters >= cutoff	0.543***
Quadratic spline	[0.077]
Bandwidth	Full sample
Observations	1,571
Districts	315
Voters >= cutoff	0.402***
Cubic spline	[0.082]
Bandwidth	Full sample
Observations	1,571
Districts	315
Year FE	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: Reduced-form effects with rectangular kernel - judicial performance

Dependent	Log Sentences	Log Sentence per process	Log Sentence per judge	Log Allocated
	(1)	(2)	(3)	(4)
Voters >= cutoff	0.724**	0.285*	0.548*	0.208
	[0.326]	[0.173]	[0.290]	[0.338]
Bandwidth	40	40	40	40
Observations	655	653	655	655
Districts	131	131	131	131
Voters >= cutoff	0.295	0.129	0.065	-0.028
	[0.284]	[0.140]	[0.264]	[0.279]
Bandwidth	60	60	60	60
Observations	1,026	1,024	1,026	1,026
Districts	206	206	206	206
Voters >= cutoff	0.368	0.221*	0.330	-0.013
	[0.242]	[0.119]	[0.229]	[0.233]
Bandwidth	80	80	80	80
Observations	1,351	1,349	1,351	1,351
Districts	271	271	271	271
Voters >= cutoff	0.475	0.189	0.476*	0.017
	[0.292]	[0.116]	[0.276]	[0.287]
IK Bandwidth	53.499	89.248	45.129	55.633
Observations	926	1,389	770	951
Districts	186	279	154	191
Voters >= cutoff	0.624	0.356*	0.580	0.458
	[0.420]	[0.195]	[0.408]	[0.397]
CCT Bandwidth	20.514	30.420	20.707	24.711
Observations	340	478	340	405
Districts	68	96	68	81
Voters >= cutoff	0.806***	0.125	0.693***	0.650***
Quadratic spline	[0.181]	[0.079]	[0.157]	[0.193]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,569	1,571	1,571
Districts	315	315	315	315
Voters >= cutoff	0.597***	0.202**	0.594***	0.346*
Cubic spline	[0.194]	[0.085]	[0.175]	[0.202]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,571	1,569	1,571	1,571
Districts	315	315	315	315
Year FE	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.11: Reduced-form effects with rectangular kernel - homicide rate

Dependent	Log District homicide rate	Log Seat homicide rate
	(1)	(2)
Voters >= cutoff	-0.497**	-0.402*
	[0.213]	[0.232]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Voters >= cutoff	-0.339*	-0.302
	[0.176]	[0.190]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Voters >= cutoff	-0.247	-0.212
	[0.154]	[0.165]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Voters >= cutoff	-0.345*	-0.195
	[0.183]	[0.163]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Voters >= cutoff	-0.396	-0.618**
	[0.291]	[0.279]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Voters >= cutoff	-0.006	0.052
Quadratic spline	[0.108]	[0.114]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Voters >= cutoff	-0.072	-0.005
Cubic spline	[0.118]	[0.124]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: Reduced-form effects with rectangular kernel - mechanisms

Dependent	Log Experience	Log Judges	Log Courts	Log Gender
	(1)	(2)	(3)	(4)
Voters >= cutoff	0.348	0.123	0.064	-0.081
	[0.256]	[0.194]	[0.075]	[0.198]
Bandwidth	40	40	40	40
Observations	357	655	655	351
Districts	80	131	131	80
Voters >= cutoff	0.074	0.170	-0.036	0.004
	[0.199]	[0.156]	[0.067]	[0.137]
Bandwidth	60	60	60	60
Observations	691	1,026	1,026	660
Districts	148	206	206	147
Voters >= cutoff	0.101	0.037	-0.008	0.122
	[0.170]	[0.127]	[0.057]	[0.112]
Bandwidth	80	80	80	80
Observations	986	1,351	1,351	942
Districts	208	271	271	207
Voters >= cutoff	0.125	0.043	0.068	0.071
	[0.169]	[0.126]	[0.061]	[0.129]
IK Bandwidth	83.521	81.770	49.228	65.329
Observations	996	1,361	840	733
Districts	210	273	168	163
Voters >= cutoff	0.639**	-0.091	0.009	-0.105
	[0.270]	[0.223]	[0.071]	[0.414]
CCT Bandwidth	33.450	27.078	15.033	21.756
Observations	282	425	210	169
Districts	62	85	42	37
Voters >= cutoff	0.370***	0.090	0.332***	0.082
Quadratic spline	[0.120]	[0.086]	[0.059]	[0.077]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,126	1,571	1,571	1,082
Districts	238	315	315	237
Voters >= cutoff	0.333**	0.005	0.255***	0.147*
Cubic spline	[0.132]	[0.092]	[0.064]	[0.083]
Bandwidth	Full sample	Full sample	Full sample	Full sample
Observations	1,126	1,571	1,571	1,082
Districts	238	315	315	237
Year FE	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.13: Effect of sentences on homicide rate with CCT default CI (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentences	Log Sentences
	(1)	(2)
Log Sentences	-0.945***	-0.948***
	[0.000]	[0.000]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Log Sentences	-0.830***	-0.796***
	[0.011]	[0.008]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Log Sentences	-0.890***	-0.784***
	[0.004]	[0.003]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Log Sentences	-0.597***	-0.772***
	[0.011]	[0.002]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Log Sentences	-1.250***	-1.122***
	[0.000]	[0.000]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Log Sentences	-0.008	0.064
Quadratic spline	[0.133]	[0.144]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Log Sentences	-0.121	-0.009
Cubic spline	[0.192]	[0.207]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.14: Effect of sentences per judge on homicide rate with CCT default CI (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentence per judge	Log Sentence per judge
	(1)	(2)
Log Sentence per judge	-1.241***	-1.228***
	[0.000]	[0.000]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Log Sentence per judge	-1.202***	-1.066***
	[0.006]	[0.005]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Log Sentence per judge	-0.865***	-0.716***
	[0.002]	[0.001]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Log Sentence per judge	-0.933***	-0.687***
	[0.009]	[0.001]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Log Sentence per judge	-1.887***	-1.436***
	[0.000]	[0.000]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Log Sentence per judge	-0.009	0.075
Quadratic spline	[0.155]	[0.163]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Log Sentence per judge	-0.121	-0.009
Cubic spline	[0.203]	[0.209]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.15: Effect of sentences per process allocated on homicide rate with CCT default CI (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentence per process	Log Sentence per process
	(1)	(2)
Log Sentence per process	-2.329***	-2.235***
	[0.152]	[0.154]
Bandwidth	40	40
Observations	653	653
Districts	131	131
Log Sentence per process	-1.857***	-1.612***
	[0.159]	[0.160]
Bandwidth	60	60
Observations	1,024	1,024
Districts	206	206
Log Sentence per process	-1.459***	-1.291***
	[0.139]	[0.140]
Bandwidth	80	80
Observations	1,349	1,349
Districts	271	271
Log Sentence per process	-1.815***	-1.273***
	[0.170]	[0.139]
IK Bandwidth	54.232	82.487
Observations	934	1,364
Districts	188	274
Log Sentence per process	-13.966***	-4.785***
	[0.168]	[0.124]
CCT Bandwidth	19.509	24.442
Observations	308	403
Districts	62	81
Log Sentence per process	-0.027	0.440
Quadratic spline	[0.858]	[0.955]
Bandwidth	Full sample	Full sample
Observations	1,569	1,569
Districts	315	315
Log Sentence per process	-0.334	-0.003
Cubic spline	[0.600]	[0.612]
Bandwidth	Full sample	Full sample
Observations	1,569	1,569
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.16: Effect of sentences on homicide rate with covariates (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentences	Log Sentences
	(1)	(2)
Log Sentences	-1.431***	-1.354***
	[0.493]	[0.478]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Log Sentences	-1.216*	-1.063
	[0.642]	[0.725]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Log Sentences	-0.643	-0.430
	[0.401]	[0.482]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Log Sentences	-1.204	-0.398
	[0.742]	[0.477]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Log Sentences	-4.699***	-2.233***
	[1.731]	[0.647]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Log Sentences	0.096	0.168
Quadratic spline	[0.112]	[0.124]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Log Sentences	0.039	0.141
Cubic spline	[0.155]	[0.171]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.17: Effect of sentences per judge on homicide rate with covariates (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentence per judge	Log Sentence per judge
	(1)	(2)
Log Sentence per judge	-1.538**	-1.434**
	[0.623]	[0.590]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Log Sentence per judge	-1.113**	-0.864
	[0.537]	[0.584]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Log Sentence per judge	-0.522	-0.334
	[0.329]	[0.371]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Log Sentence per judge	-1.260*	-0.309
	[0.664]	[0.366]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Log Sentence per judge	-3.686**	-1.890**
	[1.527]	[0.852]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Log Sentence per judge	0.105	0.183
Quadratic spline	[0.122]	[0.134]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Log Sentence per judge	0.037	0.132
Cubic spline	[0.145]	[0.157]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.18: Effect of sentences per process allocated on homicide rate with covariates (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentence per process	Log Sentence per process
	(1)	(2)
Log Sentence per process	-2.049***	-2.007***
	[0.718]	[0.745]
Bandwidth	40	40
Observations	653	653
Districts	131	131
Log Sentence per process	-1.583**	-1.293
	[0.753]	[0.886]
Bandwidth	60	60
Observations	1,024	1,024
Districts	206	206
Log Sentence per process	-1.071	-0.882
	[0.753]	[0.911]
Bandwidth	80	80
Observations	1,349	1,349
Districts	271	271
Log Sentence per process	-1.769**	-0.849
	[0.778]	[0.919]
IK Bandwidth	54.232	82.487
Observations	934	1,364
Districts	188	274
Log Sentence per process	-5.035***	-2.535***
	[1.156]	[0.617]
CCT Bandwidth	19.509	24.442
Observations	308	403
Districts	62	81
Log Sentence per process	0.593	1.031
Quadratic spline	[0.818]	[1.034]
Bandwidth	Full sample	Full sample
Observations	1,569	1,569
Districts	315	315
Log Sentence per process	0.133	0.456
Cubic spline	[0.507]	[0.596]
Bandwidth	Full sample	Full sample
Observations	1,569	1,569
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.19: Effect of sentences on homicide rate with rectangular kernel (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentences	Log Sentences
	(1)	(2)
Log Sentences	-1.098*	-0.942
	[0.659]	[0.692]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Log Sentences	-1.100**	-0.973*
	[0.551]	[0.588]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Log Sentences	-0.762	-0.683
	[0.473]	[0.551]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Log Sentences	-1.083	-0.629
	[1.043]	[0.556]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Log Sentences	-0.977***	-0.727
	[0.372]	[0.559]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Log Sentences	-0.008	0.064
Quadratic spline	[0.133]	[0.144]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Log Sentences	-0.121	-0.009
Cubic spline	[0.192]	[0.207]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.20: Effect of sentences per judge on homicide rate with rectangular kernel (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentence per judge	Log Sentence per judge
	(1)	(2)
Log Sentence per judge	-1.358	-1.139
	[0.838]	[0.871]
Bandwidth	40	40
Observations	655	655
Districts	131	131
Log Sentence per judge	-0.925**	-0.741*
	[0.414]	[0.433]
Bandwidth	60	60
Observations	1,026	1,026
Districts	206	206
Log Sentence per judge	-0.564	-0.444
	[0.347]	[0.365]
Bandwidth	80	80
Observations	1,351	1,351
Districts	271	271
Log Sentence per judge	-1.314	-0.422
	[0.839]	[0.372]
IK Bandwidth	54.232	82.487
Observations	936	1,366
Districts	188	274
Log Sentence per judge	-1.057	-0.386
	[0.677]	[1.234]
CCT Bandwidth	19.509	24.442
Observations	310	405
Districts	62	81
Log Sentence per judge	-0.009	0.075
Quadratic spline	[0.155]	[0.163]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Log Sentence per judge	-0.121	-0.009
Cubic spline	[0.203]	[0.209]
Bandwidth	Full sample	Full sample
Observations	1,571	1,571
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.21: Effect of sentences per process allocated on homicide rate with rectangular kernel (IV)

Dependent	Log District homicide rate	Log Seat homicide rate
Instrumented	Log Sentence per process	Log Sentence per process
	(1)	(2)
Log Sentence per process	-2.136**	-1.738
	[1.000]	[1.095]
Bandwidth	40	40
Observations	653	653
Districts	131	131
Log Sentence per process	-1.507**	-1.248
	[0.728]	[0.801]
Bandwidth	60	60
Observations	1,024	1,024
Districts	206	206
Log Sentence per process	-1.292	-1.186
	[0.866]	[0.994]
Bandwidth	80	80
Observations	1,349	1,349
Districts	271	271
Log Sentence per process	-1.620*	-1.177
	[0.834]	[1.036]
IK Bandwidth	54.232	82.487
Observations	934	1,364
Districts	188	274
Log Sentence per process	-3.392***	-2.186***
	[1.007]	[0.799]
CCT Bandwidth	19.509	24.442
Observations	308	403
Districts	62	81
Log Sentence per process	-0.027	0.440
Quadratic spline	[0.858]	[0.955]
Bandwidth	Full sample	Full sample
Observations	1,569	1,569
Districts	315	315
Log Sentence per process	-0.334	-0.003
Cubic spline	[0.600]	[0.612]
Bandwidth	Full sample	Full sample
Observations	1,569	1,569
Districts	315	315
Year FE	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.22: Placebo test reduced-form effects - level 2 court

Dependent	District Level 2
	(1)
Voters >= cutoff	-0.244
	[0.292]
Bandwidth	40
Observations	530
Districts	106
Voters >= cutoff	-0.346
	[0.222]
IK Bandwidth	67.384
Observations	915
Districts	183
Voters >= cutoff	-0.180
	[0.386]
CCT Bandwidth	24.612
Observations	305
Districts	61
Year FE	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters pseudo-threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.23: Placebo test reduced-form effects - judicial productivity

Dependent	Log Sentences	Log Sentence per process	Log Sentence per judge	Log Allocated
	(1)	(2)	(3)	(4)
Voters >= cutoff	-1.137	-0.511***	-0.791	-0.626
	[0.848]	[0.197]	[0.535]	[0.905]
Bandwidth	40	40	40	40
Observations	530	530	530	530
Districts	106	106	106	106
Voters >= cutoff	-1.009	-0.233	-0.734	-0.610
	[0.734]	[0.150]	[0.463]	[0.780]
IK Bandwidth	47.986	60.936	48.079	48.648
Observations	645	828	645	650
Districts	129	166	129	130
Voters >= cutoff	-1.566	-0.314	-1.393*	-1.309
	[1.154]	[0.273]	[0.825]	[1.434]
CCT Bandwidth	26.497	20.309	21.502	22.110
Observations	340	245	260	265
Districts	68	49	52	53
Year FE	Y	Y	Y	Y

Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters pseudo-threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.24: Placebo test reduced-form effects - homicide rate

Dependent	Log District homicide rate	Log Seat homicide rate
	(1)	(2)
Voters >= cutoff	0.147	0.096
	[0.344]	[0.357]
Bandwidth	40	40
Observations	530	530
Districts	106	106
Voters >= cutoff	0.144	0.096
	[0.348]	[0.295]
IK Bandwidth	39.658	49.990
Observations	520	660
Districts	104	132
Voters >= cutoff	0.111	0.079
	[0.433]	[0.372]
CCT Bandwidth	30.120	38.436
Observations	405	495
Districts	81	99
Year FE	Y	Y

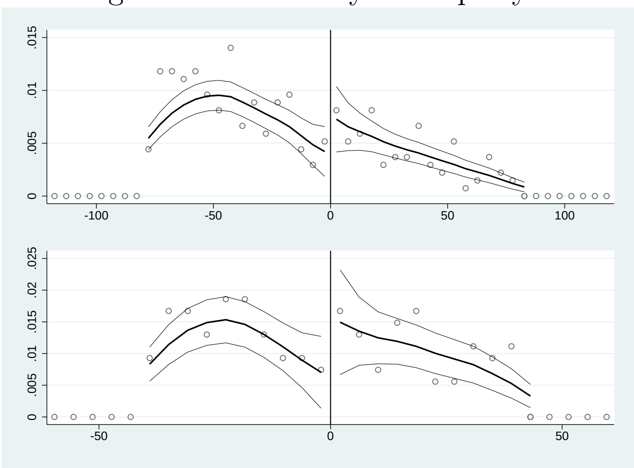
Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters pseudo-threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A.25: Placebo test reduced-form effects - mechanisms

Dependent	Log Judges	Log Courts	Log Experience	Log Gender
	(1)	(2)	(3)	(4)
Voters >= cutoff	-0.324	-0.381	-0.509*	-0.141
	[0.387]	[0.355]	[0.305]	[0.088]
Bandwidth	40	40	40	40
Observations	530	530	275	268
Districts	106	106	63	63
Voters >= cutoff	-0.270	-0.303	-0.318	-0.031
	[0.335]	[0.289]	[0.283]	[0.081]
IK Bandwidth	46.920	51.229	51.687	71.336
Observations	615	675	362	635
Districts	123	135	81	141
Voters >= cutoff	-0.310	-0.442	-0.634**	-0.188**
	[0.672]	[0.397]	[0.318]	[0.091]
CCT Bandwidth	22.122	33.876	33.902	20.577
Observations	265	455	222	122
Districts	53	91	52	31
Year FE	Y	Y	Y	Y

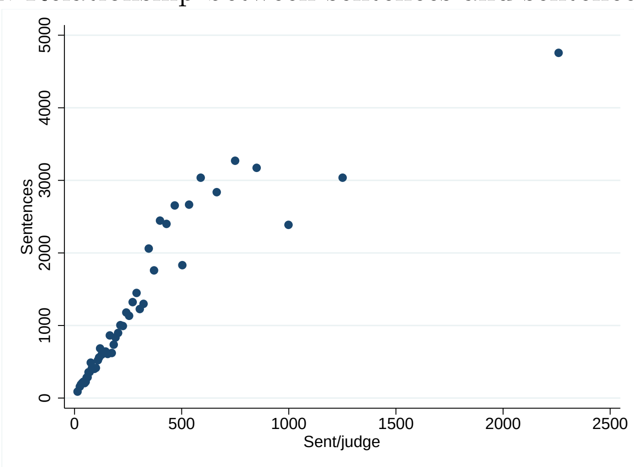
Notes: The set of rows restrict the sample to first and second level districts in which the percentage distance to the voters pseudo-threshold is smaller than 40%, 60%, 80%, Imbens-Kalyanarman (IK) and Calonico-Cattaneo-Titiunik (CCT) optimal bandwidths. Clustered standard errors at district level are in brackets.Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Figure A.1: McCrary test - per year



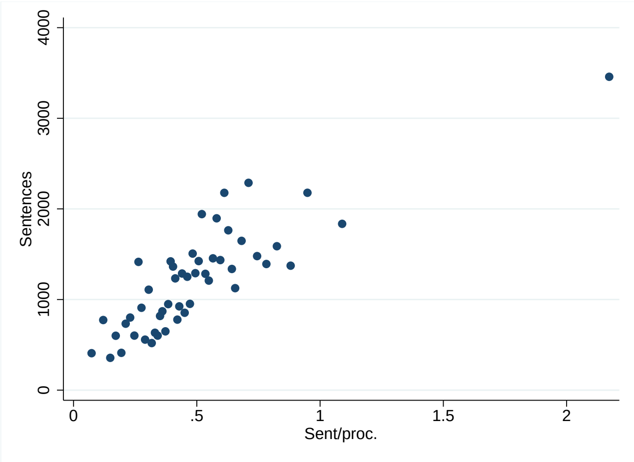
Notes: The sample is restricted to the first and second level districts in which the percentage distance to the threshold is smaller than 80% and 40%. The discontinuity estimates (log difference in height) and standard errors, in parenthesis, are respectively 0.73 (0.43) and 0.93(0.64) . The estimates are identical for the years once the district classification and distance to the threshold are the same for the 5 years.

Figure A.2: Relationship between sentences and sentences per judge



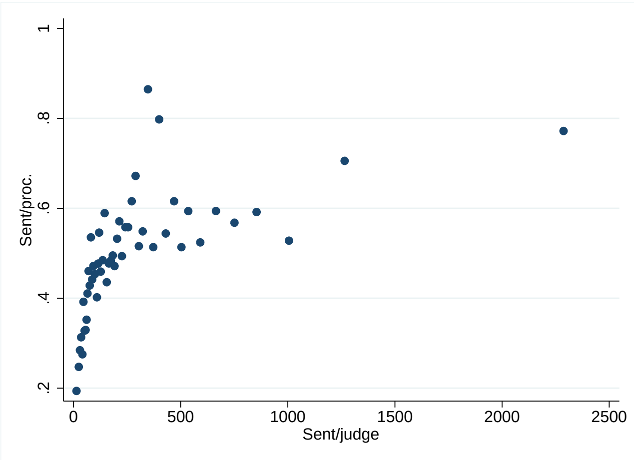
Notes: The sample is restricted to the first and second level districts in which the percentage distance to the threshold is smaller than 80%.

Figure A.3: Relationship between sentences and sentences per process allocated



Notes: The sample is restricted to the first and second level districts in which the percentage distance to the threshold is smaller than 80%.

Figure A.4: Relationship between sentences per process allocated and sentences per judge



Notes: The sample is restricted to the first and second level districts in which the percentage distance to the threshold is smaller than 80%.