Competing for change: The role of political contests in fostering local development*

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Abstract

We use a Regression Discontinuity design to assess the effects of political competition on a set of multidimensional development measures. Results show that, in municipalities eligible to have two-ballot-system mayoral elections – which previous research has shown to increase political competition –, there is an increase of 7.1 percentage points in the share of the population employed in formal jobs (a 21% change), a decrease in general and juvenile homicide rates by 8.8 and 15.6 occurrences per 1,000 people (a 28% and a 21% change) respectively, and an increase in access to waste disposal services to the poor by 4.4 percentage points (a 5% change). Our results corroborate with previous evidence suggesting that political competition is important for the expansion of key public services, which can significantly contribute to the achievement of a municipality's development goals.

Keywords: Regression Discontinuity, Two-Ballot System, Political Competition, Multidimensional Development Measures.

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1 Introduction

The achievement of sustainable development goals depends on effective multidimensional policies and governance (United Nations, 2015). When considering political settings, a large body of the literature acknowledges the pivotal role played by political institutions in shaping social outcomes (e.g., Acemoglu et al., 2005, 2014; Fujiwara, 2015; Acemoglu et al., 2019). It is also widely recognized that diverse types of social improvements can be achieved through the establishment and evolution of institutions, such as economic growth, inequality reduction, combating corruption, and formulating sound fiscal policies, among others.

Despite the abundance of evidence highlighting the numerous benefits derived from the design of political institutions, there remains a dearth of knowledge regarding how the absence of political competition may impede the realization of these potential achievements. To address this gap, we recognize the prevailing notion that the efficacy of political competition holds significant importance and strive to delve deeper into the foundations of this argument. Through an examination of the relationship between political competition and a set of mutidimensional development outcomes, our objective is to provide insights into the potential impact of electoral dynamics on the welfare of communities.

More precisely, we aim to evaluate the effects of political competition on a set of development indicators related to employment and income, public security, habitation, nutrition, health, education, and social assistance. To achieve this objective, we exploit the exogenous variation generated by the assignment of different electoral rules across Brazilian municipalities, determined by a clear-cut threshold in the number of registered voters, and employ a Regression Discontinuity (RD) design. The Brazilian Constitution mandates that municipalities with less than 200,000 registered voters use a single-ballot plurality rule[1] to elect their mayors, while those with an electorate size exceeding this threshold should use dual-ballots.[2]

[1]The single-ballot plurality rule, also known as the plurality rule or first-past-the-post, is a voting system in which the candidate who receives the highest number of votes is elected, even if they do not obtain an outright majority of the votes. This system is commonly used in various countries, including the United States for the House of Representatives and the United Kingdom for the House of Commons.

[2]The dual-ballot system is widely employed as the electoral system for presidential elections globally

As evidenced by previous research, this dual-ballot rule is responsible for increasing political competition, allowing us to interpret its effects as political competition effects (Fujiwara, 2011). Under this framework, we provide evidence that political competition increases formal employment, decreases (juvenile) homicide rates, and increases access to adequate waste disposal services among the poor. Our results are robust to several score manipulation tests.

The limited progress in empirical research pertaining to the impacts of political competition can be attributed, in part, to two challenges. Firstly, political systems are often structured in a manner that precludes the establishment of a reliable measure for assessing political competition. Secondly, even when a measure of political competition is proposed, evaluating its effects on relevant variables is frequently hindered by issues of endogeneity. This suggests that political decisions and subsequent social outcomes are influenced not only by political competition but also by a multitude of confounding factors. As a result, establishing clear causal relationships becomes challenging due to the complex interplay between these variables.

This paper addresses the aforementioned challenges by employing a cleaner method to examine the effects of political competition on public health outcomes. By utilizing a RD method, the assignment of electoral rules in this context can be considered as good as random, enabling causal inference of their effects.[3] In essence, the underlying rationale is that municipalities just below the threshold should be, on average, similar in terms of observed and unobserved characteristics to those just above it. Therefore, any disparities in outcomes between these two groups can be attributed to the different electoral rules.

The connection between this electoral-rule discontinuity and political competition may be more evident to political scientists than to economists, as it originates from the seminal work

(Golder, 2006). This system is commonly utilized in primary elections in the Southern United States, several major American cities, as well as regional elections in France, Italy, and Switzerland. In the dual-ballot system, the electoral process consists of two rounds. If no candidate receives an absolute majority of votes (usually set at 50%), a second round is held, typically involving the top two candidates from the initial round. This second round determines the final winner of the election.

[3]The genesis of this methodology dates back to seminal work in psychology during the 1950s (Thistlethwaite and Campbell, 1960). However, its integration into mainstream economics literature was not realized until the early 2000s. Comprehensive reviews include those in Imbens and Lemieux (2008), Lee and Lemieux (2010), Van der Klaauw (2008), and Skovron and Titiunik (2015).

of Duverger (1954). Specifically, Duverger's prediction suggests that a simple-majority single-ballot system favors a two-party system, whereas a simple majority with a second ballot or proportional representation favors multipartyism. Thus, this study motivates the work of Fujiwara (2011), which leverages the Brazilian electoral settings to demonstrate that the strategic behavior of voters aligns with the principles of the so-called Duverger's law.

Several studies have capitalized on this source of exogenous variation in political competition to gain insights into its impact on various important outcomes that are relevant to the movement towards sustainable development. Examples include research on public spending (Chamon et al., 2018), and the breadth of politicians' appeal (Chin, 2021), among others. Chamon et al. (2018) find that municipalities eligible for the dual-ballot system in mayoral elections experience an improvement in the quality of public spending, with mayors favoring the channelling of resources towards investment spending instead of current spending. Impacts are larger when incumbents pretend to run for reelection. In her work, Chin (2021) shows that the dual-ballot plurality rule induces a broader base of support for the mayor in office (i.e., supporters from a more diverse set of geographic locations) and a broader provision of public goods. Her evidence suggests that effects are driven by the strategic response of candidates. We move a step further to investigate whether this same dual-ballot rule, and the resulting higher political competition (Fujiwara, 2011), are effective in improving development measures across multiple dimensions.

The findings of this study have the potential to inform evidence-based policy decisions and contribute to the development of effective strategies to foster sustainable development in developing democracies. By exploring effects of political competition on a set of multi-sectoral development outcomes, we can gain valuable insights regarding the dynamics of democratic governance and its impact on the well-being of communities.

The remainder of the paper is organized as follows. Section 2 provides a comprehensive review of the political background in Brazil. Section 3 describes the data we use in the study. Section 4 outlines our empirical strategy and provides details on our identifying assumptions.

Section 5 presents our main empirical findings. Section 6 presents results of several robustness checks. And, finally, Section 7 concludes the paper.

2 Institutional Background

Brazil is composed of over 5,500 municipalities, which serve as the smallest level of government in the country, comparable to towns or cities in the United States. Each municipality is led by a single mayor, known as the *Prefeito*, and has a municipal legislature called the *Câmara de Vereadores*. The mayors and members of the municipal legislature are elected every four years through a regulated electoral process determined by federal legislation. Uniform election and inauguration dates apply to all municipalities.

Unlike some peer countries, Brazilian municipalities are not divided into districts, resulting in at-large elections. As per Brazilian legislation, all citizens aged 18 or older are required to register to vote in their municipality of residence.[4] The Brazilian Constitution also specifies that mayoral elections in municipalities with less than 200,000 registered voters should be conducted using the single-ballot plurality rule system. In contrast, municipalities with 200,000 or more registered voters must adopt the dual-ballot plurality rule system.

This rule, based on a threshold, establishes a cutoff suitable to be used in a RD design. The validity of this design depends on the 200,000-voter threshold being somewhat arbitrary and not utilized for assigning other aspects to municipalities. To the best of our knowledge, this is indeed the case. While certain regulations of municipal governments may depend on population size (distinct from the number of voters), none of them possess a threshold close to 200,000 voters. The 200,000-voter cutoff is determined by the Brazilian Constitution, ratified in 1988. The probable reason behind this rule was that, although the Constituent Assembly considered the dual-ballot system superior to the single-ballot system, the cost of potential second rounds of elections across all municipalities would have been prohibitive. Furthermore,

[4]In Brazil, voting and electoral registration are mandatory for people aged 18 or older and optional for illiterate people, people aged more than 70, and people aged between 16 and 18.

by the time of the first election in which the new regulation applied (2000), any initial effects resulting from different rates of population and registration growth between municipalities would likely have dissipated.

Although all levels of government, from the most local to the federal one, interact to generate development, the importance of the local dimension has gained much attention in sustainable development debates as it is at the local level that governments can actually coproduce outcomes with and for its population.[5] With the Brazilian Constitution of 1988, the local capacity gained substantial importance and the role of municipalities became clearer. In Brazil, it is the responsibility of the municipal government, under the leadership of its mayor, to implement social and economic policies on the ground (Enap, 2018). Therefore, mayors play a crucial role in how the country advances towards sustainable development.

3 Data

We use data from three different sources to construct a municipality-by-year panel dataset. From *Tribunal Superior Eleitoral* (TSE), we obtained data on municipal elections, including data on the number of voters registered in each municipality and elected mayors' characteristics – such as age, sex, and marital status – for the 2008, 2012, and 2016 mayoral elections.[6] From *Instituto Brasileiro de Geografia e Estatística* (IBGE), we obtained data on demographic characteristics of Brazilian municipalities, such as total resident population and percentage of people aged 60 or more. And from the *Painel de Indicadores Municipais de Pobreza Multidimensional* ("Panel of Multidimensional Poverty Indicators at the Municipality Level"), proposed by the *Pacto Brasil Sem Pobreza* ("Brazil Without Poverty Pact") in partnership with *Openn Social*, we obtained a rich set of development outcomes at the municipality-by-

[5]The term *coproduction* is used by scholars and development practitioners to denote "the joint production of services by government officials and citizens" (Brudney et al., 2022).

[6]We use the already clean dataset provided by *Base dos Dados*, which is publicly available at https://basedosdados.org/.

year level.[7] The panel includes indicators produced from official government data sources, which summarize municipal conditions in the dimensions of employment and income, public security, habitation, nutrition, health, education, and social assistance. Detailed information on the indicators used in this paper is presented in Table 1 to Table 6.

Our final dataset comprises all variables presented in Table 7 and Table 8 for the 5,570 Brazilian municipalities. Electoral data correspond to the 2008 and 2012 mayoral election years, whereas other data correspond to the fourth mandate years of mayors elected in such elections. That is, other data correspond to years 2012 and 2016.[8] We hypothesize that incumbents are more prone to show the social development results of their leadership when about to face new elections; therefore, we estimate effects of the dual-ballot rule on development outcomes in the last year of mayors' mandates.

By looking at summary statistics for observations below *versus* those above the cutoff adopted in our RD design, we note that municipalities eligible for the dual-ballot system are very similar to the ineligible ones in terms of elected mayors' characteristics. Elected mayors are, on average, aged 48.34-49.47 years, have a 6-10% chance of being female, and a 77-81% chance of being married. In terms of development indicators, on average, municipalities ineligible for the dual-ballot system perform worse in all dimensions.

4 Empirical Strategy

We are interested in estimating the effects of political competition on various municipal development measures. In order to do so, we exploit exogenous variation in political competition that comes from a discontinuity in electoral rules around the 200,000 registered voters threshold.

In our setting, treatment assignment (dual-ballot rule assignment) is a deterministic func-

[7] Data is publicly available in an online data visualization tool at https://brasilsempobreza.com.br/acesso-painel-de-indicadores/. However, the complete micro-level data was obtained upon request.

[8] It is worth noting that we left the 2016 elections and the corresponding fourth mandate year of elected mayors, 2020, out of the sample in order to avoid confounding/mediating factors that could come from the COVID-19 pandemic.

tion of the number of registered voters in the municipality. Namely, municipalities with 200,000 or more registered votes have elections in a dual-ballot system, whereas municipalities with a number of voters below this threshold have a single-ballot system.

As discussed in Fujiwara (2011), there is an increase in political competition associated with such a change in the electoral rule. The result follows from the famous Duverger (1954) work, which shows that single-ballot systems favour a two-party system as voters behave strategically. In a dual-ballot system, however, voters have incentives to vote "sincerely" in the first electoral round, which increases vote share of the third candidate in equilibrium. Hence, the discontinuity in electoral rules provides exogenous variation on the level of political competition across municipalities.

Let Y denote a scalar variable representing a development outcome, X denote the number of voters, $Y(1)$ and $Y(0)$ denote potential development outcomes under dual- and single-ballot elections, respectively. As long as the potential development outcome is a continuous function of the registered voters around the 200,000 cutoff, then the effect of the change in electoral rule on the development outcome of interest for municipalities in the cutoff can be non-parametrically identified (Hahn et al., 2001). Let $\overline{x}$ denote the registered voters' cutoff. The discontinuity allows us to identify

$$\theta = \mathbb{E}\left[Y(1) - Y(0)|X = \overline{x}\right].$$

Under the continuity condition, θ can be identified by the following estimand:

$$\theta_{RD} = \lim_{X \to \overline{x}^+} \mathbb{E}\left[Y|X = x\right] - \lim_{X \to \overline{x}^-} \mathbb{E}\left[Y|X = x\right].$$

Estimation of frontier points, such as limits around discontinuities, is not straightforward, since parametric estimators are sensitive to functional form violations around the cutoff and there is substantial difference in performance across non-parametric estimators in practice.

We follow Calonico et al. (2014) (hereafter CCT) and estimate treatment effects – i.e.,

effects of the dual-ballot rule – using non-parametric local polynomial estimations (Fan and Gijbels, 1996) for the expected outcomes at each side of the cutoff. We use a triangular kernel and mean-squared-error (MSE)-optimal bandwidths, estimated using "leave-one-out" cross-validation. The MSE-optimal bandwidth does not satisfy the regularity conditions in local polynomial estimators that ensure that first-order bias converges to zero asymptotically. Hence, there is bias in the distributional approximation, which means that inference will be biased (see CCT for a thorough discussion on inference in RD designs). Therefore, we use bias-corrected estimators for confidence interval from CCT throughout this paper.

It is possible to test for violations of the continuity assumption in the RD design. A first source of violation could come from municipalities sorting around the threshold and endogenously selecting into treatment. In Table 9, we present the McCrary (2008) density test. This test is implemented with the estimation of the density separately for observations below and above the cutoff (Cattaneo et al., 2019) and its results are also presented graphically in Figure 1. Density test results show no statistically significant evidence of manipulation around the 200,000 voters threshold.

Another potential source of violation could come from the possibility that other determinants of development outcomes are varying discontinuously around the cutoff, that is, municipality and/or elected mayors' characteristics are not balanced around the cutoff. Nonetheless, in Table 7 of Section 3 and also in Section 6, we show evidence of the non-existence of important differences in pre-treatment municipality demographic characteristics or in the profile of elected mayors around the cutoff.[9]

A third and possible violation would come discontinuities in outcomes and pre-treatment characteristics in the neighborhood of the cutoff, which would contaminate the RD estimates. We conduct several tests, including those in which placebo cutoffs are used, and find no compelling evidence of such phenomenon.

[9]The validity of the design has also been asserted by several previous papers. See, for instance, Firpo et al. (2015) and Chin (2021). Their main results corroborate with the validity of our identification assumptions.

5 The Role of Political Contests in Fostering Local Development

The main results of this paper are presented in Table 10. Although we observe that the dual-ballot system can foster improvements in the employment and income, public security, and habitation dimensions, we note that it does not have any significant effects on the nutrition, health, or education dimensions. Moreover, results show that the change in electoral rules has a harmful effect on an important indicator of the social assistance dimension, the percentage of births to teenage mothers; nonetheless, this effect is not robust to the exclusion of observations close to the cutoff – as revealed by the donut hole tests, presented in Section 6 – nor to an alternative type of bandwidth selection – as revealed by an estimation using the CER-optimal bandwidth, also presented in Section 6. We, therefore, choose to focus on the other, more robust, significant results.

More specifically, we find that the dual-ballot electoral rule increases the percentage of the population employed in formal jobs by 7.1 percentage points (a 21.04% increase from the baseline), decreases homicides per 1,000 people by 8.8 points (a 28.45% decrease from the baseline), decreases juvenile homicides per 1,000 people aged 15-29 by 15.6 points (a 21.28% decrease from the baseline), and increases the percentage of poor households with access to waste disposal services by 4.4 percentage points (a 4.74% increase from the baseline). These results are robust to several tests.[10]

6 Robustness

In this section, we aim to present further evidence that manipulation around the cutoff is unlikely. We start by showing results of tests of the balance in elected mayors' characteristics

[10]If we consider the optimal MSE-bandwidths calculated non-parametrically, the baseline level of the percentage of the population employed in formal jobs, the homicide rate, the juvenile homicide rate (ages 15-29), and the percentage of poor households with access to waste disposal services for municipalities below the cutoff are, respectively, the following: 33.75%, 30.93 per 1,000 people, 73.32 per 1,000 people aged 15-29, and 92.84%.

around the cutoff, now performed with MSE-optimal bandwidths defined non-parametrically.[11] Balance test results, presented in Table 11, show non-significant differences in elected mayors' age and sex for observations below and above the cutoff; however, results show that municipalities eligible for the dual-ballot rule are 4.1 percentage points less likely to have a married elected mayor and this estimate is significant at the 10% level. We argue that this difference is not of high importance because: [i] given the preliminary balance tests presented in Section 3, Table 7, this effect does not seem to be robust to different bandwidths; [ii] we re-estimate the effects of the dual-ballot rule on the outcome variables for which we found significant results in our main estimations, but including elected mayors' characteristics as controls – see Table 12 –, and find point estimates pointing to the same direction and of similar size;[12] and [iii] the obvious argument against the validity of our results would probably favor the idea that married mayors would be more likely to present a higher performance in office, as being married is usually consistent with a more privileged profile; however, the result presented in Table 11 goes in the opposite direction.

To further investigate the presence of systematic manipulation of the score (number of registered voters) around the cutoff, we apply a falsification approach, often called "donut hole approach". To implement this falsification test, we exclude observations of municipalities that have a number of registered voters within the interval of 250-1,250 voters around the cutoff point, and then we run estimations and inference analysis with the remaining sample (Cattaneo et al., 2019). If results are substantially sensitive to the exclusion of units located very close to the cutoff, then we should be worried about manipulation.[13]

Results of donut hole tests are presented in Figure 5, Figure 6 and Figure 7. Each graph shows, for each outcome variable of interest, the conventional RD estimate and correspond-

[11] Note that we have already presented balance tests in Section 3, where we found no significant differences in elected mayors' characteristics between observations below and above the cutoff. However, these tests were performed using a pre-established bandwidth – which ranged from zero to 400,000 registered voters, that is, 200,000 voters below and above the cutoff –, so to match the observations used in the calculation of summary statistics.

[12] We note that calculated p-values in this case may not be adequate for inference; however, we note also that point estimates, on the other hand, are adequately estimated, allowing us to use them for comparison.

[13] Recall that, in a RD setting, manipulation of the score is much more likely around the cutoff.

ing robust bias-corrected 95% confidence interval resulting from estimations that involve the exclusion of: [i] zero observations from the sample (our benchmark); [ii] observations that fall within a window of 250 registered voters around the cutoff; [iii] observations that fall within a window of 500 registered voters around the cutoff; [iv] observations that fall within a window of 750 registered voters around the cutoff; [v] observations that fall within a window of 1,000 registered voters around the cutoff; and [vi] observations that fall within a window of 1,250 registered voters around the cutoff. By looking at the graphs, we note that the exclusion of observations around the cutoff does not affect the direction and magnitude of estimated effects on the percentage of people employed in formal jobs, homicide and juvenile homicide rates, and the percentage of poor people with access to adequate waste disposal services, confirming the robustness of these results.

However, the effect found on the percentage of births to teenage mothers is only sustained with the exclusion of observations very close to the cutoff. On one side, this finding does not raise concerns of score manipulation, because it just says that effects are not too local, so that the exclusion of observations within a window of 250-500 voters around the cutoff results in estimated effects similar to that obtained using a non-restricted sample. On the other side, this finding may raise concerns of bias regarding the size of the bandwidth needed for one to find significant effects.[14] To provide a better analysis regarding the validity of the effect on the percentage of births to teenage mothers and also further strengthen our conclusions regarding the significant effects found for other outcomes, we move forward to present estimates of the dual-ballot rule effects produced through RD non-parametric estimations using an alternative method of optimal bandwidth selection.

As an additional robustness test, we run the non-parametric RD analysis using CER-optimal bandwidths. Results are presented in Table 13. If we consider point estimates and the robust p-values obtained with the use of CER-optimal bandwidths, we reach the same

[14]Recall that, in a RD setting, there is a trade-off regarding bias and efficiency. A larger bandwidth favors efficiency due to a larger number of observations, while a smaller bandwidth favors unbiasedness due to the higher similarity of units treated and untreated in terms of fundamental characteristics.

conclusions regarding the significance of policy effects that are reached when MSE-optimal bandwidths are used, except for the percentage of births to teenage women. In line with donut hole test results, when we consider the smaller CER-bandwidth, the effect of the dual-ballot rule on this variable does not remain significant. We, therefore, choose to abdicate from including the effect on the percentage of births to teenage women in our set of main results.

Finally, the last set of robustness checks we perform comprises placebo tests. We consider two placebo cutoffs, one of 100,000 and the other of 300,000 registered voters, and estimate effects using the same strategy described in Section 4. Results are presented in Figure 8, Figure 9, and Figure 10. For all five variables on which we find significant effects in our main estimations, we cannot rule out that estimates are equal to zero when using placebo cutoff points.

Taken together, our robustness test results strengthen our confidence in our RD identification assumptions, showing no evidence of score manipulation around the threshold of 200,000 registered voters.

7 Conclusion

In this paper, we aim to contribute to the empirical literature that investigates the impacts of political competition so to shed light on whether it can improve the welfare of communities. Using a sharp Regression Discontinuity Design, we are able to estimate effects on a set of multidimensional development outcomes.

Our results reveal that political competition improves development measures within the employment, public security, and habitation dimensions, but has no significant effect on indicators of nutrition, health, education, and public assistance. More specifically, political competition increases the share of the population formally employed by 7.1 percentage points – a 21 % increase from the baseline level –, decreases the homicide rate by 8.8 points – a 28% decrease from the baseline level –, decreases the juvenile homicide rate by 15.6 points – a 21% decrease from the baseline level –, and increases the percentage of poor households with

access to waste disposal services by 4.4 percentage points – a 5% increase from the baseline level.

Overall, we note that development outcomes on which we find significant effects of political competition are those achievable (and more easily perceived by the population) in the shorter term through targeted policies. It is more likely, for instance, that employment and public security policies implemented within a mandate of four years are successful in increasing formal employment and reducing homicides than it is that more complex health policies are successful in reducing respiratory/metabolic diseases or mortality. It has been recently documented, for instance, that a collective pay-for-performance program implemented in a Brazilian police agency led to a significant large reduction in violent crimes after only five or six months (Cabral et al., 2023).

Therefore, we understand that our results configure new evidence that increased competition in mayoral elections improves the welfare of the population in dimensions where improvements can be observed in the shorter term. If we consider that mayors in office are motivated by the coming elections – because they plan on either being reelected or supporting a candidate who is running for election –, and thus seek to expand their electoral reach (Chin, 2021), this idea is consistent with results found by Camargo (2021), which brings about evidence that new physical health facilities, a very visible (and easily perceivable) improvement in health infrastructure, positively affect incumbent mayor's vote share in the city of Rio de Janeiro.

In terms of the mechanisms that drive our results, although we recognize the multifaceted nature of the development measures assessed, in light of recent evidence regarding the higher probability of one engaging in criminal behavior when faced with job loss (Britto et al., 2022), it is important to mention that the effect we find of political competition on formal employment may be a mediator of the effect on crime, measured by homicide rates.

Overall, our results point to positive impacts of political competition, at the executive municipal level, on the welfare of the poor. Nonetheless, further investigations are needed to

understand how these impacts are achieved in practice.

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Table 1: Development indicators: Employment and Income

Development indicator	Definition	Source
% employed in formal job	(formally employed people aged 15-64)/ (total resident population) *100	RAIS/MTE and IBGE.
% low income	(people registered in Cadastro Unico with income up to 1/2 min. wage)/ (total resident population) *100	Estimates produced based on MDS/SEDES/SAGI/ DECAU/CECAD and IBGE.
GDP per capita	(value added at 2010 constant prices)/ (total resident population)	Estimates produced based on IBGE/IPEA and IBGE.

Notes. This table presents definitions and official data sources for some of the development indicators of the *Painel de Indicadores Municipais de Pobreza Multidimensional* ("Panel of Multidimensional Poverty Indicators at the Municipality Level") included in our dataset. The information presented in the table consists of a translated version of the data dictionary provided by the *Pacto Brasil Sem Pobreza* ("Brazil Without Poverty Pact") in partnership with *Openn Social*.

Table 2: Development indicators: Public Security

Development indicator	Definition	Source
Homicide rate	(deaths due to aggression and legal intervention)/ (total resident population) *100,000	TABNET-DATASUS/IPEA
Homicide rate (ages 15-29)	(deaths due to aggression and legal intervention of people aged 15-29)/ (resident population aged 15-29) *100,000	TABNET-DATASUS/IPEA
Death rate by transport accident	(deaths in transport accident of people aged 15-29)/ (resident population aged 15-29) *100,000	TABNET-DATASUS/IPEA

Notes. This table presents definitions and official data sources for some of the development indicators of the *Painel de Indicadores Municipais de Pobreza Multidimensional* ("Panel of Multidimensional Poverty Indicators at the Municipality Level") included in our dataset. The information presented in the table consists of a translated version of the data dictionary provided by the *Pacto Brasil Sem Pobreza* ("Brazil Without Poverty Pact") in partnership with *Openn Social*.

Table 3: Development indicators: Habitation

Development indicator	Definition	Source
% access to clean water (poor)	(poor households with access to clean water)/ (total number of households) *100	Estimates produced based on Cadastro Unico/ MDS.
% access to waste disposal services (poor)	(poor households with access to waste disposal services)/ (total number of households) *100	Estimates produced based on Cadastro Unico/ MDS.
% access to sanitation (poor)	(poor households with access to adequate sanitation)/ (total resident population) *100	Estimates produced based on Cadastro Unico/ MDS.
% access to clean water (all)	(total resident population with access to clean water)/ (total resident population) *100	SNIS.
% access to waste disposal services (all)	(total resident population with access to waste disposal services)/ (total resident population) *100	SNIS.
% access to sanitation (All)	(total resident population with access to adequate sanitation)/ (total resident population) *100	SNIS.

Notes. This table presents definitions and official data sources for some of the development indicators of the *Painel de Indicadores Municipais de Pobreza Multidimensional* ("Panel of Multidimensional Poverty Indicators at the Municipality Level") included in our dataset. The information presented in the table consists of a translated version of the data dictionary provided by the *Pacto Brasil Sem Pobreza* ("Brazil Without Poverty Pact") in partnership with *Openn Social*.

Table 4: Development indicators: Nutrition

Development indicator	Definition	Source
% low birthweight (all)	(number of live births of babies w/ ≤2.5 kg per municipality of mothers' residence)/ (total live births per municipality of mothers' residence) *100	Estimates produced based on SINASC/MS.
% low birthweight (low education)	(number of live births of babies w/ ≤2.5 kg to mothers w/ ≤7 years of education per municipality of mothers' residence)/ (total live births per municipality of mothers' residence) *100	Estimates produced based on SINASC/MS.

Notes. This table presents definitions and official data sources for some of the development indicators of the *Painel de Indicadores Municipais de Pobreza Multidimensional* ("Panel of Multidimensional Poverty Indicators at the Municipality Level") included in our dataset. The information presented in the table consists of a translated version of the data dictionary provided by the *Pacto Brasil Sem Pobreza* ("Brazil Without Poverty Pact") in partnership with *Openn Social*. Adequate prenatal care involves starting care in the first month of pregnancy and attending at least six medical visits. Live births correspond to those given to mothers that reside in the municipality for which the indicator is calculated.

Table 5: Development indicators: Health

Development indicator	Definition	Source
% adequate vaccination	(number of people who got adequate number of doses)/ (target population) *100	SI-PNI/CGPNI/DEIDT/ SVS/MS.
Rate of metabolic diseases (ages 15-64)	(number of hospitalizations of people aged 15-64 due to endocrine, nutritional and metabolic diseases)/ (resident population aged 15-64) *100	Estimates produced based on SIH/SUS/MS and on MS/SVS/DASNT/CGIAE.
Rate of respiratory diseases (ages 15-64)	(number of hospitalizations of people aged 15-64 due to respiratory diseases)/ (resident population aged 15-64) *100	Estimates produced based on SIH/SUS/MS and on MS/SVS/DASNT/CGIAE.
% prenatal care coverage	(number of live births to mothers who went through adequate prenatal care)/ (number of live births) *100	Estimates produced based SINASC/MS.
Infant mortality rate	(number of deaths of children aged < 12 months)/ (number of live births) *1,000	Estimates produced based on MS/SVS/CGIAE-SIM and on SINASC/MS.
Mortality rate	(number of deaths)/ (total resident population) *1,000	Estimates produced based on MS/SVS/CGIAE-SIM and on IBGE.

Notes. This table presents definitions and official data sources for some of the development indicators of the *Painel de Indicadores Municipais de Pobreza Multidimensional* ("Panel of Multidimensional Poverty Indicators at the Municipality Level") included in our dataset. The information presented in the table consists of a translated version of the data dictionary provided by the *Pacto Brasil Sem Pobreza* ("Brazil Without Poverty Pact") in partnership with *Openn Social*. Adequate prenatal care involves starting care in the first month of pregnancy and attending at least six medical visits. Live births correspond to those given to mothers that reside in the municipality for which the indicator is calculated.

Table 6: Development indicators: Education and Social Assistance

Development indicator	Definition	Source
% out of school (ages 5-17)	((resident population aged 5-17)- (number of people aged 5-17 enrolled in schools))/ (resident population aged 5-17) *100	Estimates produced based on Censo Escolar/Inep and on MS/SVS/DASNT/CGIAE.
Age-grade distortion	(number of students enrolled in final years of elementary school with ≥2 years of age-grade lag)/ (number of students enrolled in final years of elementary school) *100	Estimates produced based on Censo Escolar/Inep.
Level of institutionalization of elderly	(number of resident people aged 60+ living in institutional shelter)/ (number of resident people aged 60+) *100	Estimates produced based on Censo SUAS and on MS/SVS/DASNT/CGIAE.
% births to teenage women	(number of live births to women aged ≤ 19)/ (total number of live births) *100	Estimates produced based on SINASC-MS.
% street population	(number of people in street situation)/ (total resident population) *100	Estimates produced based on managerial data from SUAS and on IBGE.

Notes. This table presents definitions and official data sources for some of the development indicators of the *Painel de Indicadores Municipais de Pobreza Multidimensional* ("Panel of Multidimensional Poverty Indicators at the Municipality Level") included in our dataset. The information presented in the table consists of a translated version of the data dictionary provided by the *Pacto Brasil Sem Pobreza* ("Brazil Without Poverty Pact") in partnership with *Openn Social*. Live births correspond to those given to mothers that reside in the municipality for which the indicator is calculated.

Table 7: Summary statistics and balance tests: Municipality and elected mayor's characteristics

	Obs. below the cutoff					Obs. above the cutoff					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N	p-value
Municipality characteristics											
Total resident population	22739.56	35983.54	807.00	358164.00	10971	421178.80	93572.59	260180.00	654786.00	102	0.389
% at age 60 plus	0.13	0.04	0.02	0.34	10971	0.12	0.03	0.05	0.22	102	0.466
Voters registered	15521.52	22801.11	924.00	199692.00	10971	276323.50	56661.45	201368.00	397626.00	102	1.000
Elected mayor's characteristics											
Elected mayor's age	48.34	9.79	21.00	91.00	10892	49.47	9.57	28.00	71.00	102	0.552
Elected mayor female	0.10	0.30	0.00	1.00	10893	0.06	0.24	0.00	1.00	102	0.936
Elected mayor married	0.77	0.42	0.00	1.00	10893	0.81	0.39	0.00	1.00	102	0.226

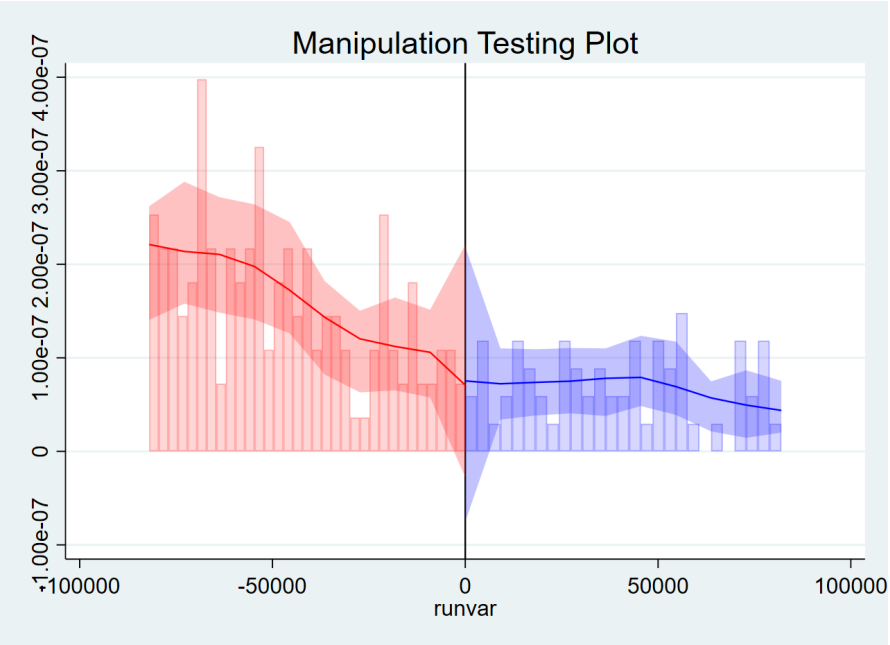
Notes. This table presents summary statistics of all variables contained in the final dataset for observations below (columns 1-5) and above (columns 6-10) the 200,000 registered voters cutoff. A window of 200,000 registered voters below and above the cutoff is considered. Column 11 presents the p-values – calculated using bias corrected regression discontinuity estimates and robust standard errors – of regression discontinuity balance tests, performed following the empirical design detailed in Section 4 and using each variable as the outcome.

Table 8: Summary statistics and balance tests: Multidimensional development indicators

	Obs. below the cutoff					Obs. above the cutoff					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N	p-value
% employed in formal job	19.80	15.43	0.06	100.00	10971	42.40	19.07	9.65	100.00	102	0.030
% low income	50.81	21.98	1.43	100.00	10971	23.32	9.84	6.03	47.70	102	0.030
GDP per capita	12.20	13.65	-8.97	658.95	10971	20.87	13.61	7.37	105.86	102	0.307
Homicide rate	28.62	21.62	0.69	231.24	7583	30.03	19.40	5.71	85.59	102	0.031
Homicide rate (ages 15-29)	67.55	55.15	2.65	548.25	5525	66.13	47.91	4.83	194.95	102	0.027
Death rate by transport accident	62.3	60.00	2.27	1190.00	6555	21.39	9.52	4.86	49.00	102	0.000
% access to clean water (poor)	95.49	7.46	11.06	100.00	10971	96.97	3.12	82.99	99.96	102	0.589
% access to waste disp (poor)	68.93	23.85	0.00	100.00	10971	95.09	7.09	49.14	99.86	102	0.001
% access to sanitation (poor)	53.44	30.90	0.00	100.00	10971	82.60	16.99	24.17	99.74	102	0.268
% access to clean water (all)	67.81	24.26	0.00	100.00	10060	91.44	16.74	27.20	100.00	102	0.027
% access to waste disp (all)	77.38	21.7	10.09	100.00	6553	98.34	2.90	80.40	100.00	97	0.000
% access to sanitation (all)	55.03	31.39	0.00	100.00	4550	67.69	30.59	0.75	100.00	101	0.259
% low birthweight (low educ)	8.87	7.79	0.00	100.00	10916	10.85	2.00	6.90	19.02	102	0.009
% low birthweight (all)	7.90	3.54	0.00	75.00	10971	9.09	1.00	7.12	12.65	102	0.018
% adequate vaccination	69.78	22.61	0.00	668.17	10971	63.16	17.83	19.01	93.50	102	0.834
Rate of metabolic diseases (ages 15-64)	1.39	2.17	0.00	81.69	6820	0.81	0.62	0.01	3.37	102	0.702
Rate of respiratory diseases (ages 15-64)	4.56	6.58	0.00	108.61	6890	2.37	1.52	0.00	8.35	102	0.080
% prenatal care coverage	68.50	15.49	2.97	100.00	5485	70.00	11.93	38.80	86.18	52	0.591
Infant mortality rate	1.81	1.25	0.13	20.00	8244	1.20	0.31	0.51	2.24	102	0.002
Mortality rate	6.28	1.85	0.81	16.52	10971	6.39	1.32	4.03	9.87	102	0.495
% out of school (ages 5-17)	1.63	5.43	0.00	94.44	10971	1.08	4.24	0.00	38.02	102	0.752
Age-grade distortion	29.70	13.94	0.00	80.63	10971	22.25	11.01	4.39	44.49	102	0.208
Level of institutionalization of elderly	0.55	0.81	0.00	10.19	3388	0.21	0.17	0.00	0.79	101	0.188
% births to teenage women	20.87	6.96	1.39	60.00	10897	15.20	3.38	7.49	24.09	102	0.706
% street population	0.13	1.40	0.00	33.80	1216	0.08	0.13	0.00	0.64	39	0.657

Notes. This table presents summary statistics of all variables contained in the final dataset for observations below (columns 1-5) and above (columns 6-10) the 200,000 registered voters cutoff. A window of 200,000 registered voters below and above the cutoff is considered. Column 11 presents the p-values – calculated using bias corrected regression discontinuity estimates and robust standard errors – of regression discontinuity balance tests, performed following the empirical design detailed in Section 4 and using each variable as the outcome.

Figure 1: Graphic presentation of density test results



Notes. This figure illustrates the results of the regression discontinuity manipulation test, using local polynomial density estimation, presented in Table 9.

Table 9: Density test results

	(1) T-statistic	(2) p-value
Robust estimates	0.2474	0.8046
N	11,131	

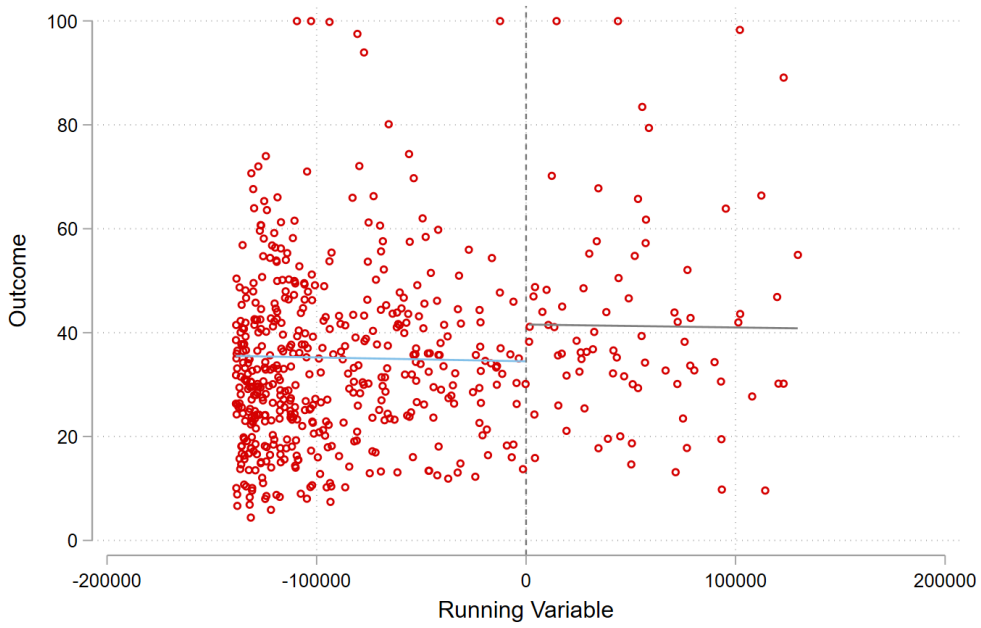
Notes. This table presents results of regression discontinuity manipulation test, using local polynomial density estimation. Elections considered are those of 2008 and 2012. Significant at the 10%, 5%, and 1% level is indicated, respectively, as follows: *, **, and ***.

Table 10: Main results

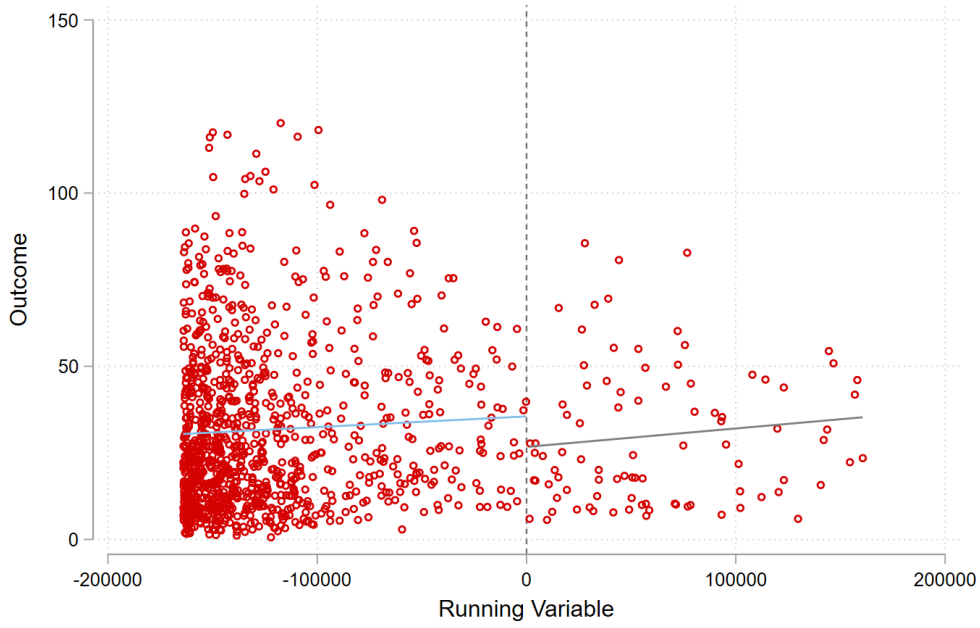
	(1) BW	(2) p	(3) Conv RD-est	(4) BC RD-est	(5) Robust SE	(6) p-value	(7) N
Labor and income							
% employed in formal job	138578	1	7.078**	9.270	4.560	0.042	547
% low income	187216	1	4.047	1.166	2.877	0.685	3740
GDP per capita	200125	1	-0.901	-1.126	3.204	0.725	11073
Public security							
Homicide rate	164097	1	-8.803***	-12.326	4.403	0.005	1102
Homicide rate (ages 15-29)	112301	1	-15.595**	-23.760	11.734	0.043	322
Death rate by transport accident	45868	1	1.641	-1.324	4.786	0.782	98
Habitation							
% access to clean water (poor)	151331	1	1.005	-0.195	1.195	0.870	763
% access to waste disp (poor)	99198	1	4.445**	5.936	2.947	0.044	258
% access to sanitation (poor)	210010	1	-9.78	-1.249	4.299	0.771	11074
% access to clean water (all)	99142	1	3.985	5.308	3.621	0.143	258
% access to waste disp (all)	59920	1	2.528	3.429	2.323	0.140	130
% access to sanitation (all)	182897	1	7.123	-3.572	7.652	0.641	1832
Nutrition							
% low birthweight (low educ)	223694	1	0.424	0.369	0.605	0.542	11022
% low birthweight (all)	165798	1	0.291	0.084	0.24	0.726	1160
Health							
% adequate vaccination	131548	1	0.582	-4.152	4.857	0.393	478
Rate of metabolic diseases	227568	1	0.442	0.255	0.156	0.102	6927
Rate of respiratory diseases	82908	1	0.320	0.033	0.316	0.917	215
% prenatal care coverage	132868	1	2.551	0.079	4.11	0.985	258
Infant mortality rate	62483	1	-0.113	-0.212	0.137	0.122	151
Mortality rate	155142	1	0.068	-0.161	0.283	0.569	840
Education							
% out of school (ages 5-17)	112247	1	0.886	0.284	2.170	0.896	323
Age-grade distortion	163325	1	-1.745	0.276	2.277	0.904	1070
Social assistance							
Level of institutionalization of elderly	88750	1	0.164	0.105	0.068	0.123	222
% births to teenage women	194767	1	1.756***	3.332	0.911	0.000	7435
% street population	161095	1	0.072	-0.001	0.101	0.992	278

Notes. This table presents results of non-parametric local polynomial estimations of treatment effects, as described in Section 4. P-values are calculated using bias corrected estimates and robust standard errors. Mayoral elections considered are those of 2008 and 2012. Significant at the 10%, 5%, and 1% level is indicated, respectively, as follows: *, **, and ***.

Figure 2: RD plots



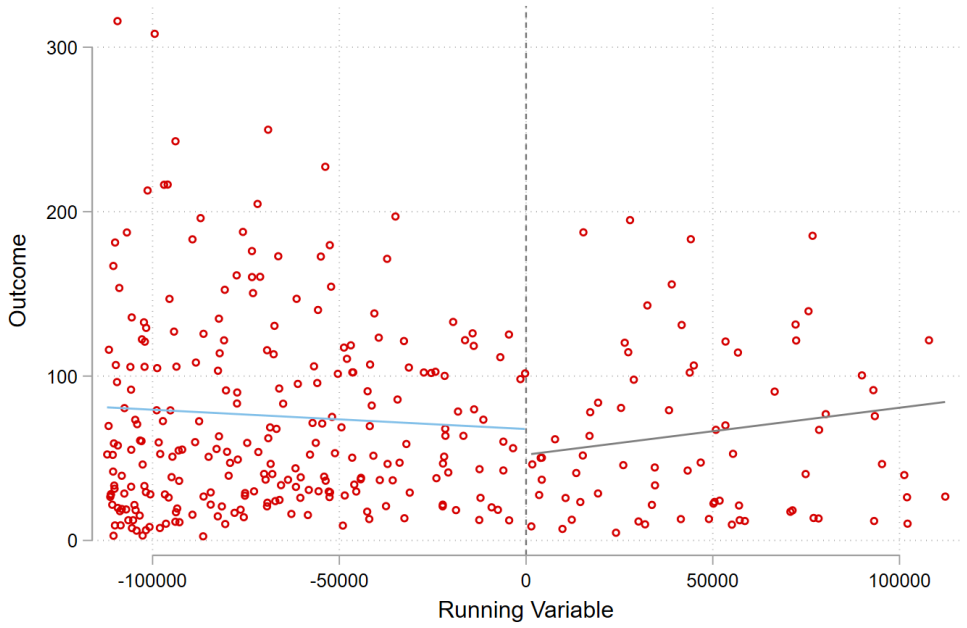
(a) % employed in formal job



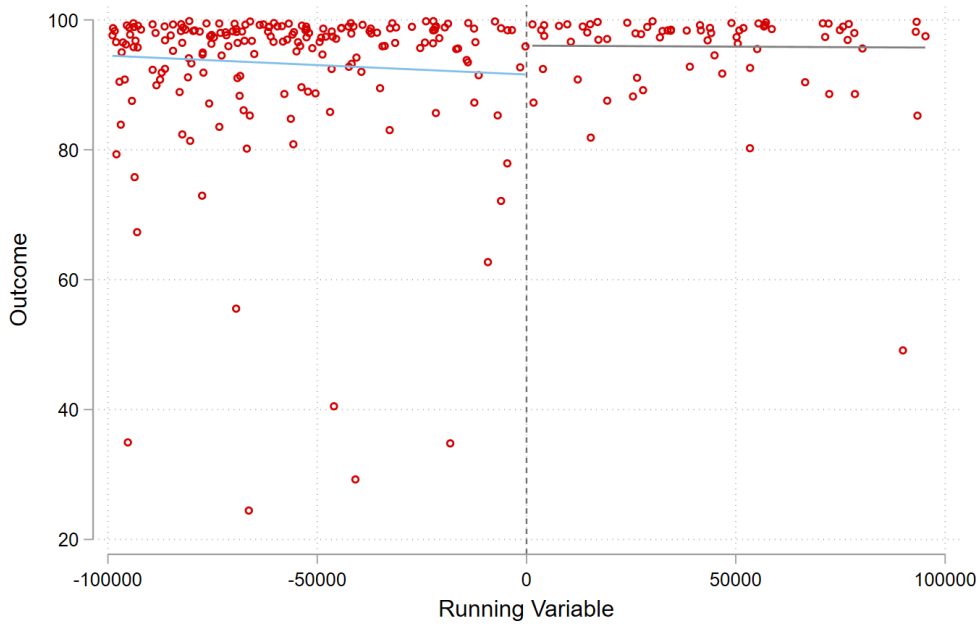
(b) Homicide rate

Notes. This figure presents predicted values obtained through parametric weighted-least-square regressions – with weights created according to the triangular kernel formula – of outcome variables on the running variable, estimated separately for observations below and above the cutoff. Bandwidths were chosen to match those defined non-parametrically in local polynomial estimations, presented in Table 10.

Figure 3: RD plots



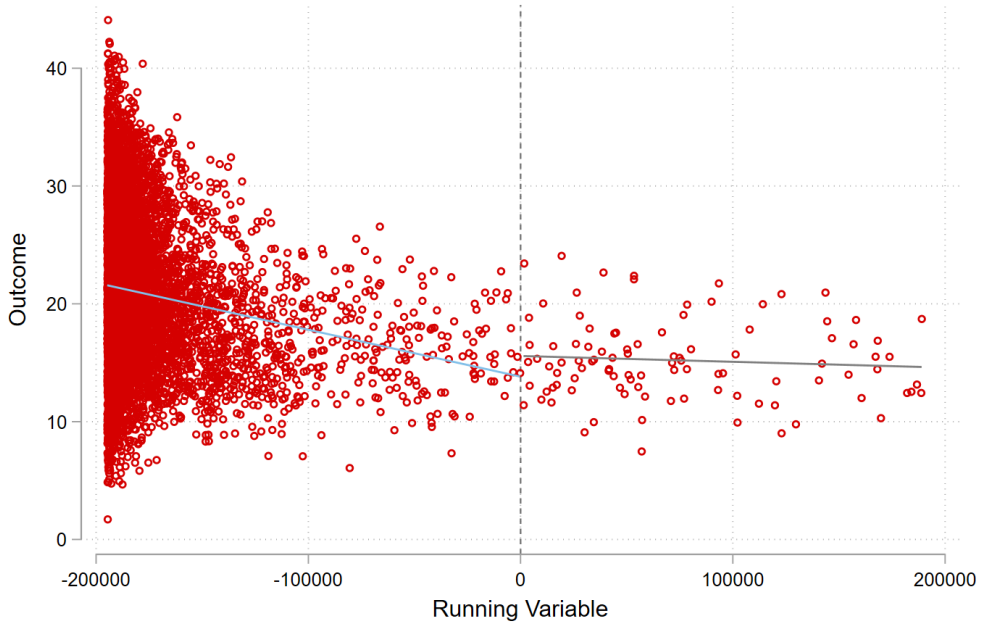
(a) Homicide rate (ages 15-29)



(b) % access to waste disposal services (poor)

Notes. This figure presents predicted values obtained through parametric weighted-least-square regressions – with weights created according to the triangular kernel formula – of outcome variables on the running variable, estimated separately for observations below and above the cutoff. Bandwidths were chosen to match those defined non-parametrically in local polynomial estimations, presented in Table 10.

Figure 4: RD plots



(a) % births to teenage women

Notes. This figure presents predicted values obtained through parametric weighted-least-square regressions – with weights created according to the triangular kernel formula – of outcome variables on the running variable, estimated separately for observations below and above the cutoff. Bandwidths were chosen to match those defined non-parametrically in local polynomial estimations, presented in Table 10.

Table 11: Balance in elected mayors' characteristics

	(1) BW	(2) p	(3) Conv RD-est	(4) BC RD-est	(5) Robust SE	(6) p-value	(7) N
Elected mayor's age	215387	1	-5.641	-2.255	2.290	0.325	10996
Elected mayor female	377487	1	-0.036	-0.056	0.061	0.359	11022
Elected mayor married	271860	1	-0.041*	-0.154	0.090	0.087	11004

Notes. This table presents results of non-parametric local polynomial estimations of treatment effects, as described in Section 4. P-values are calculated using bias corrected estimates and robust standard errors. Mayoral elections considered are those of 2008 and 2012. Significant at the 10%, 5%, and 1% level is indicated, respectively, as follows: *, **, and ***.

Table 12: Controlling for elected mayors' characteristics

	(1) BW	(2) p	(3) Conv RD-est	(4) BC RD-est	(5) Robust SE	(6) p-value	(7) N
% employed in formal job	125712	1	8.957**	10.550	4.661	0.024	418
Homicide rate	157746	1	-8.139***	-11.555	4.334	0.008	897
Homicide rate (ages 15-29)	105154	1	-12.800*	-19.683	11.669	0.092	281
% access to waste disp (poor)	85295	1	4.422*	5.519	3.155	0.080	218
% births to teenage women	189721	1	1.298***	2.729	0.919	0.003	4546

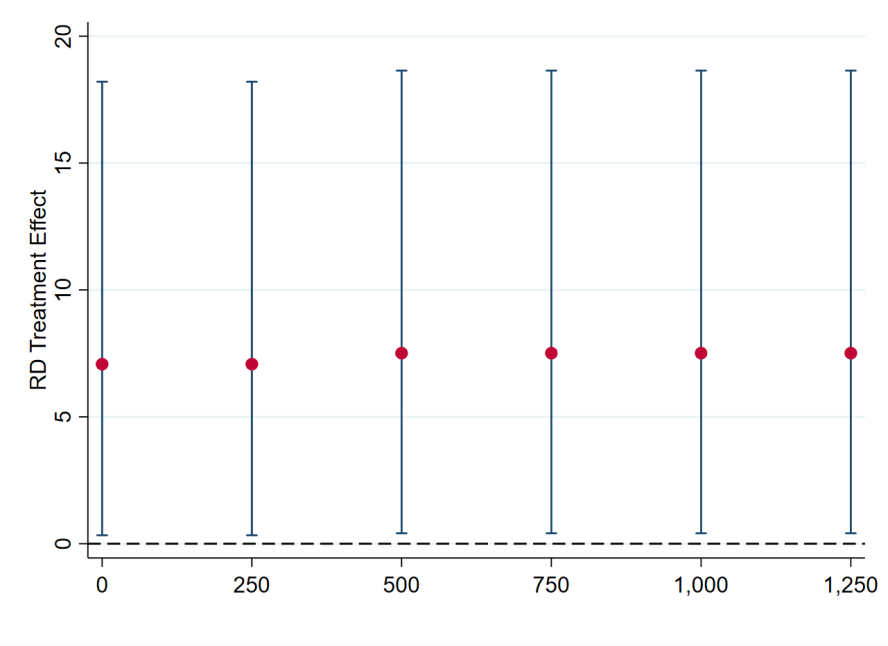
Notes. This table presents results of non-parametric local polynomial estimations of treatment effects, as described in Section 4, but including a set of controls for elected mayors' characteristics – age, sex, and marital status. P-values are calculated using bias corrected estimates and robust standard errors. Mayoral elections considered are those of 2008 and 2012. Significant at the 10%, 5%, and 1% level is indicated, respectively, as follows: *, **, and ***.

Table 13: RD estimates: CER bandwidths

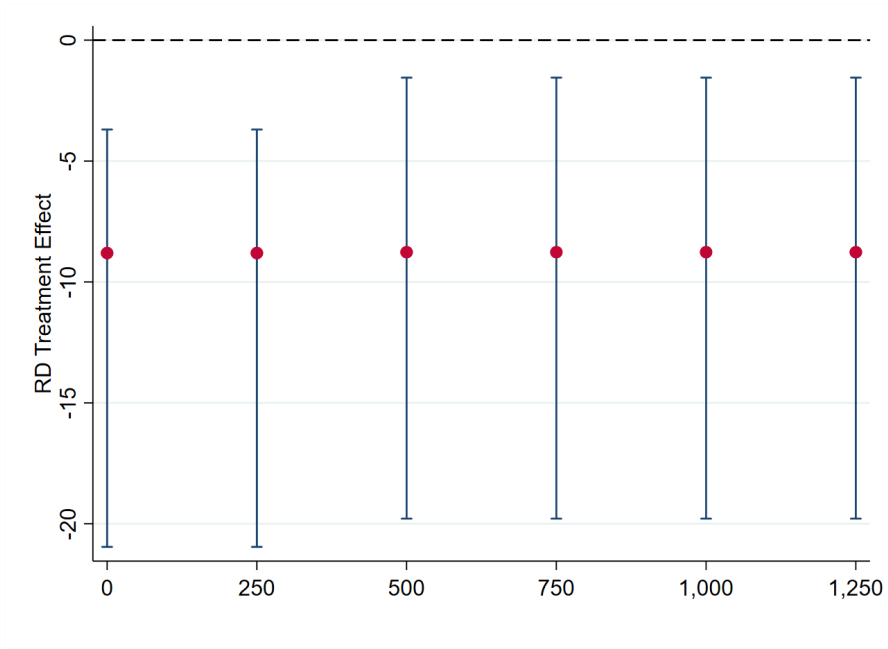
	(1) BW	(2) p	(3) Conv RD-est	(4) BC RD-est	(5) Robust SE	(6) p-value	(7) N
% employed in formal job	86970	1	10.235**	10.920	5.410	0.044	222
Homicide rate	104871	1	-8.184*	-9.214	4.893	0.060	282
Homicide rate (ages 15-29)	72887	1	-20.775*	-23.946	13.014	0.066	179
% access to waste disp (poor)	62255	1	4.999	5.592	3.528	0.113	151
% births to teenage women	122274	1	-0.188	0.184	1.083	0.865	393

Notes. This table presents results of non-parametric local polynomial estimations of treatment effects, as described in Section 4. P-values are calculated using bias corrected estimates and robust standard errors. Mayoral elections considered are those of 2008 and 2012. Significant at the 10%, 5%, and 1% level is indicated, respectively, as follows: *, **, and ***.

Figure 5: Donut hole tests



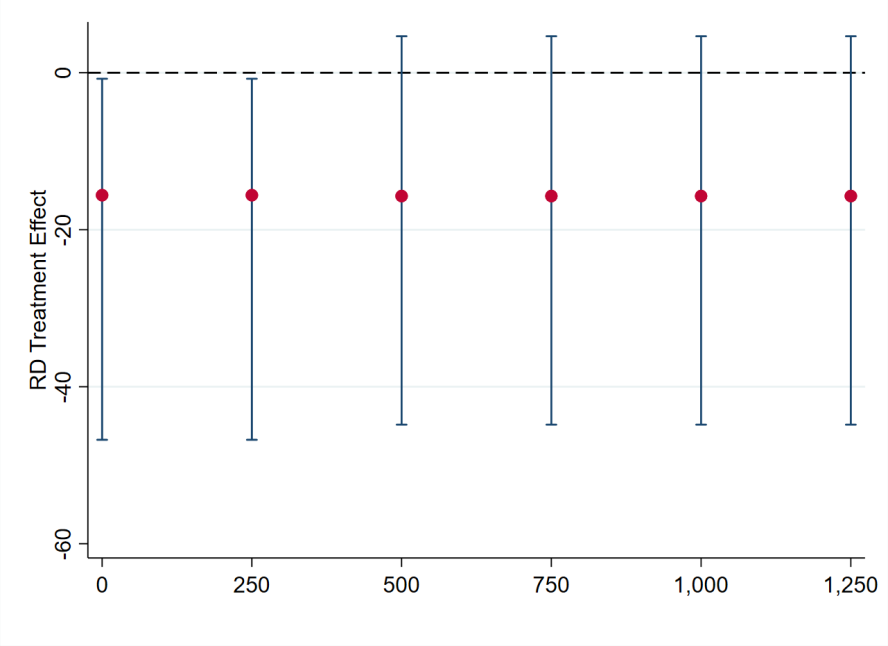
(a) % employed in formal job



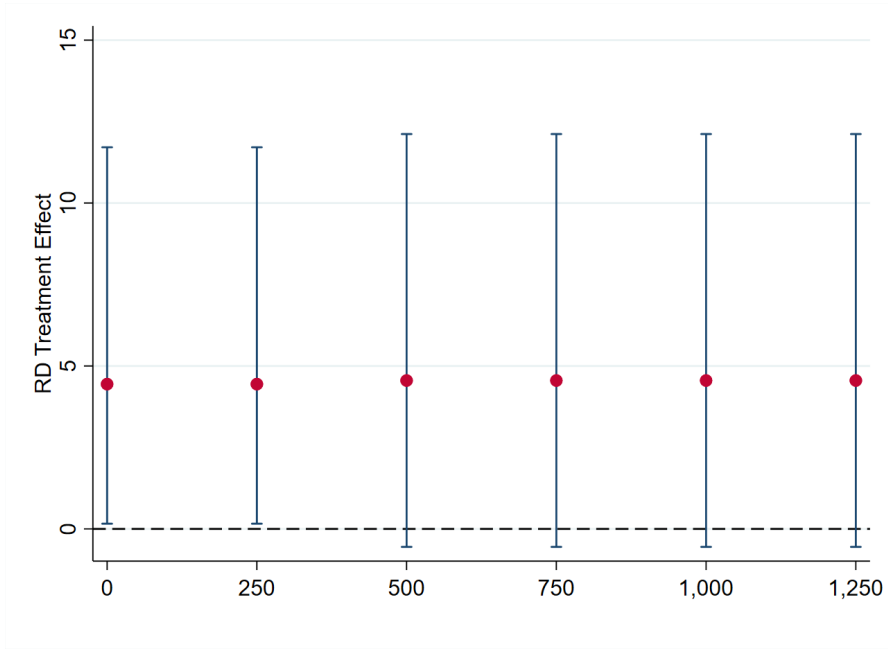
(b) Homicide rate

Notes. This figure presents conventional local polynomial RD estimates and corresponding 95-percent robust bias-corrected confidence intervals. In the x-axis, we specify the sample considered in each estimation, which excludes, first, no observations around the cutoff and, then, observations for which the base of registered voters is within a window of 250, 500, 750, 1,000, and 1,250 voters, respectively, below or above the cutoff.

Figure 6: Donut hole tests



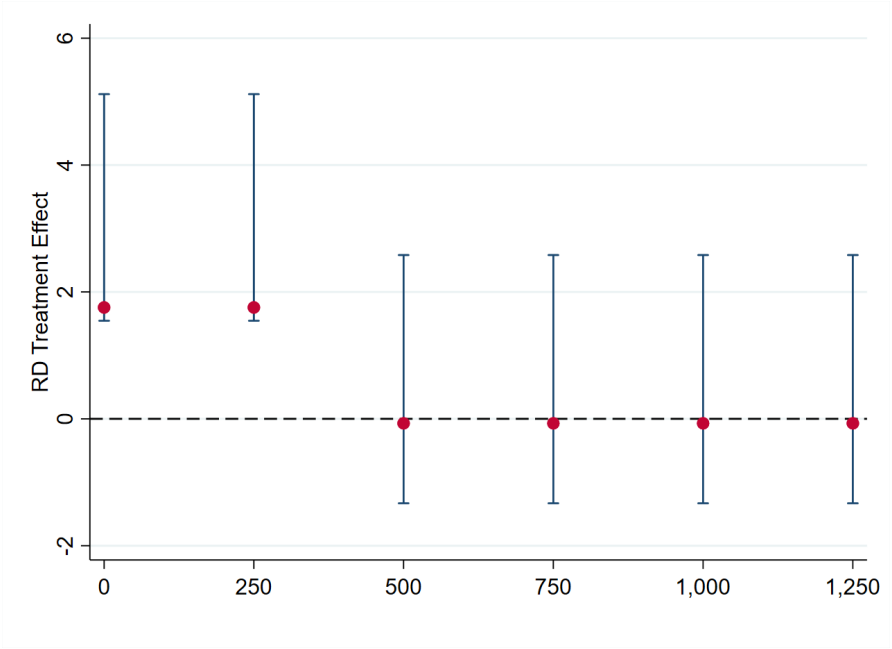
(a) Homicide rate (ages 15-29)



(b) % access to waste disposal services (poor)

Notes. This figure presents conventional local polynomial RD estimates and corresponding 95-percent robust bias-corrected confidence intervals. In the x-axis, we specify the sample considered in each estimation, which excludes, first, no observations around the cutoff and, then, observations for which the base of registered voters is within a window of 250, 500, 750, 1,000, and 1,250 voters, respectively, below or above the cutoff.

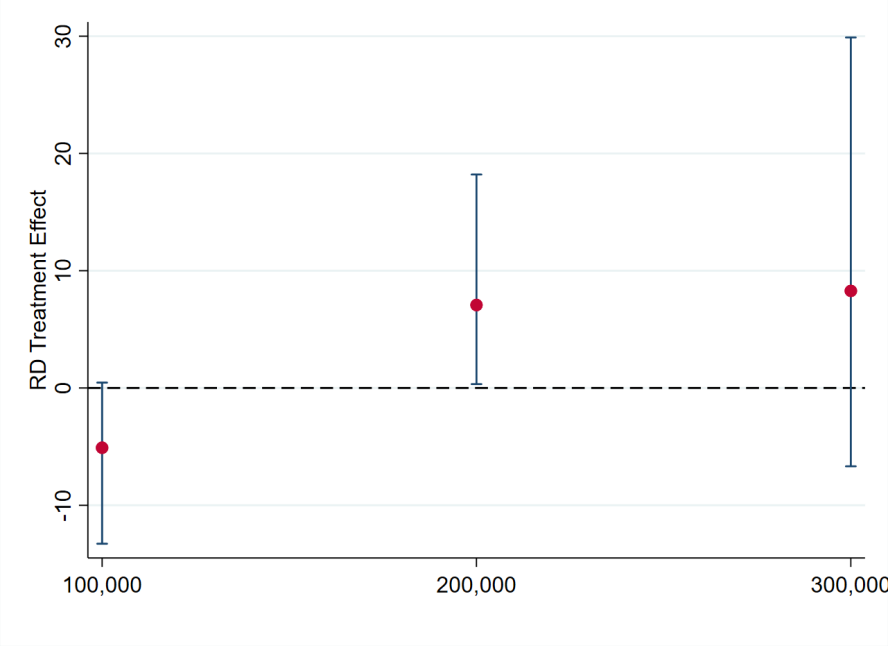
Figure 7: Donut hole tests



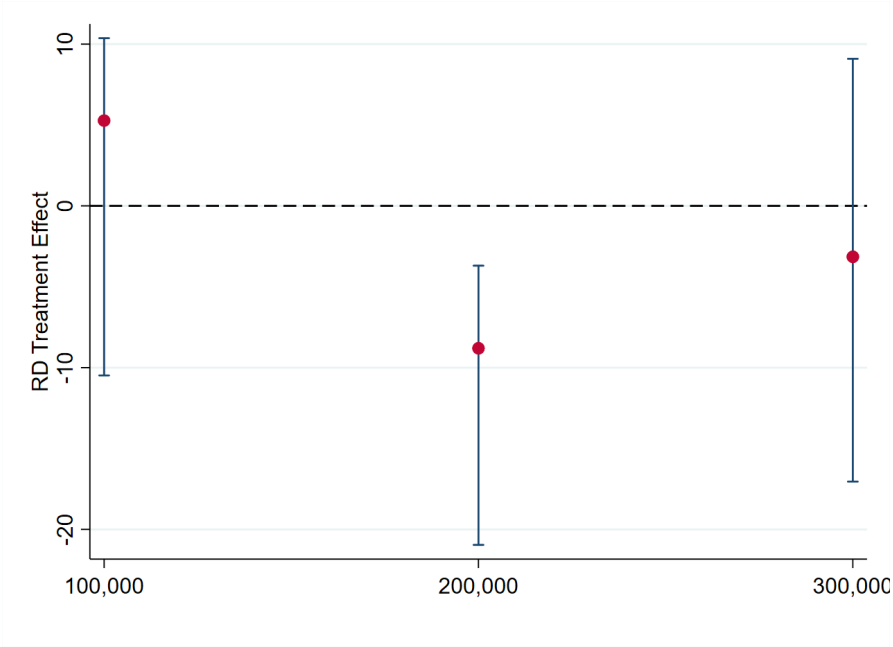
(a) % births to teenage women

Notes. This figure presents conventional local polynomial RD estimates and corresponding 95-percent robust bias-corrected confidence intervals. In the x-axis, we specify the sample considered in each estimation, which excludes, first, no observations around the cutoff and, then, observations for which the base of registered voters is within a window of 250, 500, 750, 1,000, and 1,250 voters, respectively, below or above the cutoff.

Figure 8: Placebo cutoff tests



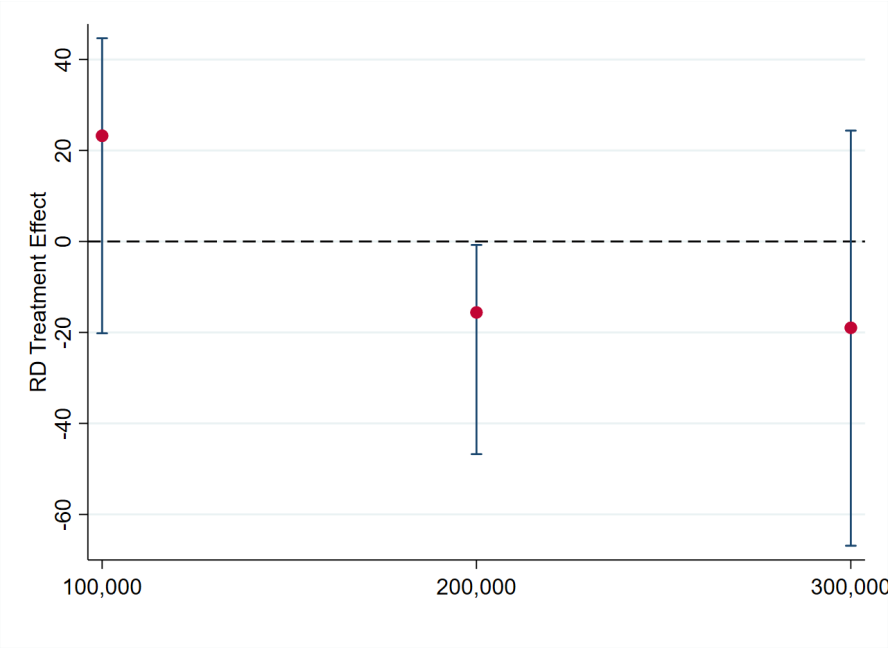
(a) % employed in formal job



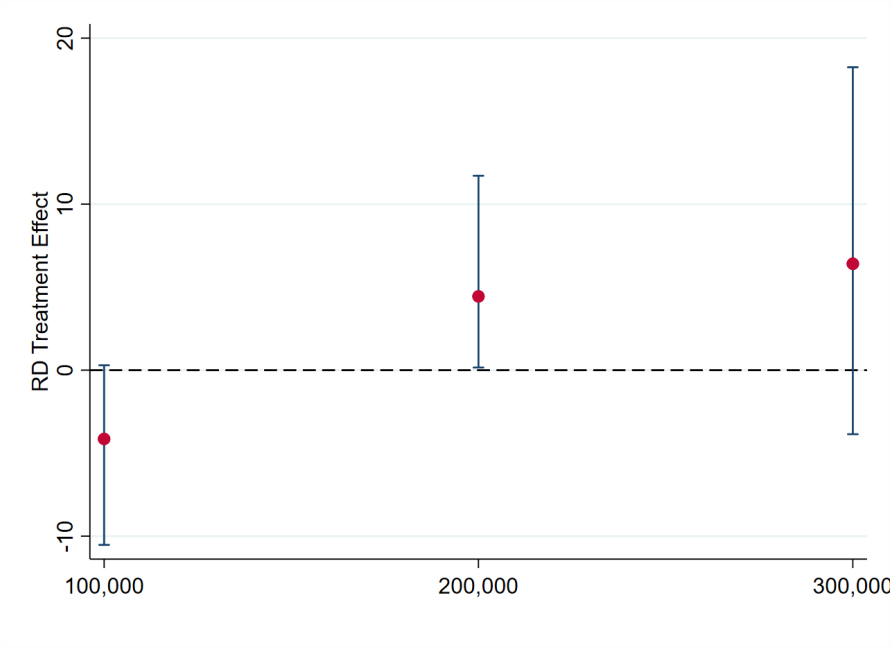
(b) Homicide rate

Notes. This figure presents conventional local polynomial RD estimates and corresponding 95-percent robust bias-corrected confidence intervals. In the x-axis, we specify the cutoff considered in each estimation: [i] the alternative cutoff of 100,000 registered voters; [ii] the true cutoff of 200,000 voters; and [iii] the alternative cutoff of 300,000 registered voters.

Figure 9: Placebo cutoff tests



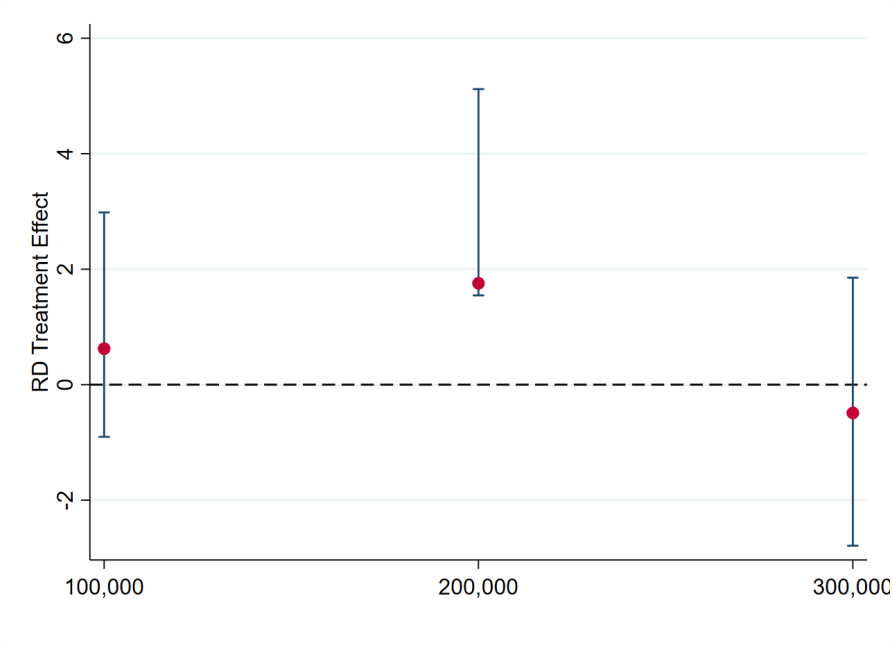
(a) Homicide rate (15-29



(b) % access to waste disposal services (poor)

Notes. This figure presents conventional local polynomial RD estimates and corresponding 95-percent robust bias-corrected confidence intervals. In the x-axis, we specify the cutoff considered in each estimation: [i] the alternative cutoff of 100,000 registered voters; [ii] the true cutoff of 200,000 voters; and [iii] the alternative cutoff of 300,000 registered voters.

Figure 10: Placebo cutoff tests



(a) % births to teenage women

Notes. This figure presents conventional local polynomial RD estimates and corresponding 95-percent robust bias-corrected confidence intervals. In the x-axis, we specify the cutoff considered in each estimation: [i] the alternative cutoff of 100,000 registered voters; [ii] the true cutoff of 200,000 voters; and [iii] the alternative cutoff of 300,000 registered voters.