

# Poverty, Party Alignment, and Reducing Corruption through Modernization: Evidence from Guatemala\*

Michael Denly<sup>†</sup>

Akshat Gautam<sup>‡</sup>

September 30, 2020

For the most recent version of this paper, please visit:

[www.mikedenly.com/files/dg-corruption.pdf](http://www.mikedenly.com/files/dg-corruption.pdf)

## Abstract

We show that once low or decreasing poverty reduces voter need for clientelism, it ultimately reduces corruption through political institutions. Our theoretical and empirical framework focuses on party alignment—i.e., when politicians' share the same party at higher and lower levels of government. Aligned politicians generally enjoy resource advantages due to their affiliation with the executive. However, we show that close elections discipline aligned politicians to engage in less corruption once economic circumstances are better. For identification, we rely on close-election regression discontinuity designs that analyze the number of audit violations committed and the amount of money misappropriated in Guatemalan municipalities. The results of our study help document how the modernization (poverty) and institutional approaches to reducing corruption interact.

JEL codes: D72, D73, F63

---

\*For research assistance, we thank the following current and former Research Affiliates and Graduate Research Fellows at the University of Texas at Austin's Innovations for Peace and Development: Caleb Rudow, Erin Eggleston, Nicole Pownall, Vanessa Gonzales, and Magdalena Ibarra. For feedback or advice, we thank José Cheibub, Mike Findley, John Gerring, Ken Greene, Kyosuke Kikuta, Xiaobo Lu, Alex Norris, Jan Pierskalla, Leslie Schwindt-Bayer, and participants at the Texas Comparative Politics Circle and the Academia against Corruption in the Americas conference. All errors are those of the authors.

<sup>†</sup>Ph.D. Candidate, Department of Government, University of Texas at Austin, [mdenly@utexas.edu](mailto:mdenly@utexas.edu).

<sup>‡</sup>First-year Ph.D. student, Department of Economics, Columbia University, [ag4322@columbia.edu](mailto:ag4322@columbia.edu). Previously, Akshat was an undergraduate Research Affiliate at University of Texas at Austin's Innovations for Peace and Development.

The practice of misusing entrusted power or public office for private gain has a familiar name: corruption.<sup>1</sup> The consequences of corruption extend far and wide, hindering the achievement of development outcomes in rich and poor countries alike (e.g., [Olken and Pande, 2012](#); [Findley, Nielson and Sharman, 2014](#)). Often, politics is a driving force behind corruption’s intractability, which is why researchers have studied which types of political and institutional configurations facilitate or reduce corruption (e.g., [Gerring and Thacker, 2004](#); [Kunicová and Rose-Ackerman, 2005](#); [Treisman, 2007](#); [Golden and Mahdavi, 2015](#)).

One particularly prominent antidote to corruption in countries with democratic institutions is clarity of responsibility.<sup>2</sup> It “refers to institutional and partisan arrangements that make it easy for voters to monitor their representatives, identify those responsible for undesirable outcomes, and hold them accountable by voting them out of office” ([Schwindt-Bayer and Tavits, 2016](#), 1). Clarity of responsibility can take different forms, but a salient manifestation is the political-institutional configuration of party alignment: that is, when politicians’ parties match at higher and lower levels of government.

Party alignment, however, does not only facilitate clarity of responsibility. For example, party alignment yields greater access to the spoils of the bureaucracy, which incites clientelism and unfair party competition ([Greene, 2007, 2010](#)). Similarly, the decentralization literature convincingly shows that party alignment fuels politically-motivated spending and budget cycles in both developed and developing countries.<sup>3</sup> Under what conditions,

<sup>1</sup> For more on the definition of corruption, see, for example, [Søreide \(2014\)](#) and [Rose-Ackerman and Palifka \(2016\)](#).

<sup>2</sup> Note: we specifically say “democratic institutions” because authoritarian countries can still have some democratic institutions, and the argument in this paper especially applies to full democracies, less-than-full democracies, and authoritarian regimes with democratic institutions.

<sup>3</sup> There is documented evidence of political budget-cycles and favoritism in intergovernmental transfer allocation in at least the following countries: Brazil ([Brollo and Nannicini, 2012](#); [Bueno, 2018](#)); Chile ([Corvalan, Cox and Osorio, 2018](#); [Lara and Toro, 2019](#); [Livert, Gainza and Acuña, 2019](#)); China ([Guo, 2009](#); [Lü, 2015](#)); Colombia ([Drazen and Eslava, 2010](#)); England ([Fourinaies and Mutlu-Eren, 2015](#)); Germany ([Kauder, Potrafke and Reischmann, 2016](#)); Ghana ([Banful, 2011a,b](#)); Guatemala ([Sandberg and Tally, 2015](#)); India ([Velasco Rivera, 2020](#)); Italy ([Carozzi and Repetto, 2016](#); [Alesina and Paradisi, 2017](#)); Mexico ([Timmons and Broidy, 2013](#)); Philippines ([Labonne, 2016](#)); Pakistan ([Callen, Gulzar and Rezaee, 2020](#)); Portugal ([Veiga and Veiga, 2007](#); [Veiga and Pinho, 2007](#); [Aidt, Veiga and Veiga, 2011](#); [Veiga and Veiga, 2013](#)); Russia ([Treisman and Gimpelson, 2001](#)); Spain ([Solé-Ollé and Sorribas-Navarro, 2008](#)); USA ([Ansolabehere, Snyder and Ting, 2003](#); [Kriner and Reeves, 2012, 2015](#); [Christenson, Kriner and Reeves, 2017](#); [Hill and Jones, 2017](#)); Uruguay ([Manacorda, Miguel and Vigorito, 2011](#)); and West Germany ([Schneider, 2010](#)).

then, does party alignment yield less corruption from precisely the politicians with more possibilities to engage in it?

Using a formal model, we provide a theory to explain when aligned politicians engage in less corruption. To that end, clarity of responsibility does not necessarily lower corruption from aligned politicians, but it may do so if two conditions are met. First, aligned politicians must live in an area where levels of poverty are low or have recently declined. Essentially, economic circumstances must be relatively better by national standards. Second, aligned politicians must be susceptible to significant electoral competition, having won their position by a small margin of victory in the most recent election.

When economic circumstances are better, voters tend to rely less on clientelistic exchanges to meet basic needs and, in turn, vote more on the basis of programmatic (policy-based) appeals.<sup>4</sup> By reducing the need for “request-fulfilling”,<sup>5</sup> we argue that better economic circumstances lead to less voter tolerance of corruption from aligned politicians as well. By contrast, in more difficult economic circumstances, voters are more supportive of aligned politicians due to their access and willingness to share the spoils of the bureaucracy for electoral gain.

Electoral competition amplifies the effects of better economic circumstances on politicians’ corruption levels. Winning elections by small margins, for example, signals to politicians that they have less room to capture rents if they wish to gain re-election—and obtain rents in the future. Given that politicians in most countries earn more in office than out of office (Fisman, Schulz and Vig, 2014), reelection prospects drive politicians to temper their corruption levels if their close-election win gives them less ability to extract rents. For their part, parties want to gain as many positions as possible, too. Accordingly, aligned parties have an incentive to discourage corruption from their members. That is especially the case

<sup>4</sup> For general overviews regarding the relationship between poverty and clientelism, see Kitschelt and Wilkinson (2007) and Stokes et al. (2013, Chapter 6). For related empirical analyses, see Kitschelt and Kselman (2013), Gonzalez-Ocantos, Kiewiet de Jonge and Nickerson (2014), Jensen and Justesen (2014), and Szwarcberg (2015).

<sup>5</sup> Request-fulfilling entails “citizens demand[ing] clientelistic benefits” (Nichter and Peress, 2017).

in or after close races—i.e., when voters are more engaged, clarity of responsibility is highest, and corruption scandals are thus more electorally costly.

Under *both* improved economic circumstances and credible electoral competition, clarity of responsibility then becomes salient. More specifically, it motivates the aligned politicians to reduce their rent-seeking behavior relative to unaligned politicians—even though aligned politicians enjoy greater resource advantages. Overall, our theory aims to depict how politics, political institutions, and economic development interact to reduce corruption through modernization.<sup>6</sup>

To support our theory, we use objective, municipality-level data on corruption from Guatemala. The country is not only relatively poor and has a long history of clientelism and corruption but also, in 2019, expelled its United Nations-backed anti-corruption body, the International Commission Against Impunity (CICIG) (González, 2014; Sandberg and Tally, 2015; *The Economist*, 2019; Malkin, 2019). The debate and myriad protests relating to the expulsion of the CICIG underscores the relevance of corruption in Guatemala’s political discourse.

To obtain objective measures for corruption, we follow some pioneering recent work on Brazil, Mexico, and Bulgaria,<sup>7</sup> and rely on measures of municipal-level infractions and spending misappropriations captured in audit reports. Our political data constitute the electoral results of municipal elections. The poverty data come from the results of the 2002 and 2011 censuses of Guatemala.

To operationalize whether a municipality is performing better economically, we specifically compare municipalities with low and high poverty levels; municipalities with increased and decreased poverty rates relative to the previous census; and all municipalities—i.e., not subsetting by poverty. We exploit a series of close-election regression discontinuity designs

<sup>6</sup> By “modernization”, we are referring to the prediction of modernization theory that economic growth or education leads to democratization (see Acemoglu and Robinson, 2018, 26).

<sup>7</sup> See, for example, Ferraz and Finan (2008, 2011), Brollo et al. (2013), Nikolova and Marinov (2017), and Larreguy, Marshall and Snyder (2020).

to causally identify the effects of alignment in the different samples. To accommodate the concept of alignment, we modify [Lee's \(2008\)](#) framework for the incumbency advantage along the lines of [Brollo and Nannicini \(2012\)](#).

Under numerous specifications, we consistently find that alignment yielded a significant decrease in both of our measures of corruption in the municipalities with decreased and lower poverty. For example, in our base specification for infractions committed in each electoral term, aligned municipalities commit an average of 13.73 less infractions in the decreased poverty sample, and 6.09 less infractions in the low poverty sample. To ensure our results are robust, we use [Calonico et al.'s \(2019\)](#) new method to consider covariates in our regression discontinuity analyses. When controlling for reelection ([Ferraz and Finan, 2011](#); [Vuković, 2020](#)), log public goods spending per capita, log population, and inequality (Gini coefficient), results are mostly unchanged for the low poverty and poverty-decreasing samples.

In some but not all cases, alignment reduces corruption in municipalities with low or decreasing extreme poverty as well, suggesting that the theory has broad reach. None of these results travel to municipalities in the high-poverty or poverty-increasing samples. When analyzing the full sample (i.e., not splitting the sample according to poverty levels or changes), the results under all specifications are both substantively and statistically insignificant, suggesting the limits of current understanding of clarity of responsibility theory (see [Schwindt-Bayer and Tavits, 2016](#)).

At the broadest possible level, the results of this study help scholars re-evaluate how the modernization/poverty and institutional approaches to corruption interact. As [Fisman and Golden \(2017, 15-16\)](#) explain, previous research has not found much empirical support for the modernization approach in poor countries. We would argue that is the case because poverty cannot be analyzed in isolation from the institutions that cause it ([Acemoglu, Johnson and Robinson, 2005](#)). To be clear, our identification strategy based on subgroup analysis cannot definitively show that either that low or decreasing poverty reduces corruption levels. However, our approach does show that poverty sets areas within a poor country on different

corruption paths or starting points given the causal effects of party alignment and electoral competition on corruption. Since we find empirical support for our theory examining the effects of short-term poverty changes and longer-term poverty levels, and the poverty and corruption data are not endogenous (see Appendix M), our approach yields robust results.

The paper proceeds as follows. Section 1 provides a theoretical framework to understand how the combination of low/decreasing poverty, alignment, and close elections yield reduced levels of corruption. Section 2 constitutes the Research Design, which introduces the data, institutional context, and identification strategy underpinning this paper. Section 3 provides the main results. We supplement these results with an analysis of the poverty, alignment, and close election mechanisms in Section 4. Section 5 concludes.

# 1. Theoretical Framework

## 1.1. Model Setup

We provide a simple theoretical framework to understand the mechanisms through which modernization decreases corruption through political institutions. Our framework focuses on party alignment for a simple reason: aligned politicians have more possibilities than politicians from other parties to capture the spoils of the bureaucracy for both clientelistic and corrupt purposes (Greene, 2010; Brollo and Nannicini, 2012; Bueno, 2018; Corvalan, Cox and Osorio, 2018; Velasco Rivera, 2020).

To better understand the advantages of alignment and how they are crucial to reduce corruption through political institutions and modernization, let us first consider local-level politician  $i$ 's maximization problem. Consistent with Magaloni, Díaz-Cayeros and Estévez (2007), local-level politician  $i$ 's personal budget constraint,  $b_i$ , comprises spending on public

expenses and goods,  $g_i$ , as well as her private rents,  $r_i$ :

$$b_i = g_i + r_i^8 \quad (1)$$

Magaloni, Díaz-Cayeros and Estévez (2007) equate  $r$  merely with clientelism. By contrast, total rents,  $r$ , in our model consists of both money set aside for clientelism,  $c$ , and the personal benefits of public office (corruption),  $p$ :

$$r = c + p, \text{ where } c = \gamma r^9 \quad (2)$$

Under Equation (2), we assume that  $c$  increases with  $r$ , meaning that local-level politician  $i$  devotes at least some portion of her rents toward clientelism. Although the politician may prefer to keep all of the rents for personal gain ( $c = 0$ ), doing so would drastically hurt reelection prospects and thus future potential rent extraction levels as well. Given the possibility of reelection and how it drives politician behavior,<sup>10</sup> we distinguish between local-level politician  $i$ 's favored levels of rent extraction in the current electoral period,  $r_{i,1}$ , as well as a potential future one,  $r_{i,2}$ :

$$r_i = r_{i,1} + r_{i,2}^{11} \quad (3)$$

Since local-level politician  $i$ 's chance of gaining reelection is a probabilistic outcome, we represent it with  $\pi$ , where  $\pi' > 0$ ,  $\pi'_{MV} > 0$  and  $\pi'' < 0$ . That re-election probability,  $\pi$ , is also dependent on constituents' levels of satisfaction with the local-level politician,  $s_i$ , which

<sup>8</sup> We assume  $b$  is exogenous and normalized to 1 without a loss of generality. We recognize that  $b$  could decrease as a result of corruption and/or clientelism in previous periods, but we assume exogeneity for simplicity purposes.

<sup>9</sup> Because we cannot directly observe the distinction between  $c$  and  $p$  in Equation (2), we need to introduce  $\gamma \in (0, 1)$ . It denotes the fraction of rents used for clientelistic purposes, which we use to for the calculation of the maximization problem in Appendix C.

<sup>10</sup> See, for example, Barro (1973), Ferejohn (1986), Ferraz and Finan (2011), and de Janvry, Finan and Sadoulet (2012).

<sup>11</sup> We frame the model explicitly for rents in period 1,  $r_{i,1}$ , where  $r_{i,2}$  is taken to be given and assumed by the local-level politician as a future expectation of rents in period 2.

we define for the current period as follows:

$$\begin{aligned} s_{i,1} &= W(g_{i,1}) + \beta_i^{1+a} W(\gamma r_{i,1}) + (2a - 1)t(MV) \\ &= W(1 - r_{i,1}) + \beta_i^{1+a} W(\gamma r_{i,1}) + (2a - 1)t(MV) \end{aligned} \quad (4)$$

In Equation (4),  $W(\cdot)$  corresponds to the satisfaction that citizens derive from local-level politician  $i$ 's rents and spending on public expenses or goods in the current period, such that  $W' > 0$  and  $W'' < 0$  (Baleiras, 1997; Baleiras and da Silva Costa, 2004);  $a$  corresponds to party alignment, which takes a value of 1 if local-level politician  $i$  is aligned or 0 otherwise;  $t(\cdot)$  captures citizens' satisfaction from clarity of responsibility, measured by local-level politician  $i$ 's margin of victory in the last election ( $MV$ ), such that  $t(\cdot)$  is a positive function,<sup>12</sup>  $t' > 0$ , and  $t'' < 0$ ; and  $\beta_i$  represents that effect of low or decreased poverty on citizens' pre-existing discount rates of clientelistic and other benefits that corrupt politicians may bring through  $W(\gamma r_{i,1})$ .<sup>13</sup>

## 1.2. Clarity of Responsibility and Discount Rates of Corruption-Related Benefits

The model incorporates two independent channels thorough which clarity of responsibility affects citizens' satisfaction levels with local-level politician  $i$ . Those levels of satisfaction, in turn, affect both the politician's reelection probability ( $\pi_i$ ) and incentives to extract rents ( $r$ ) for corrupt ( $p$ ) and clientelistic ( $c$ ) purposes.

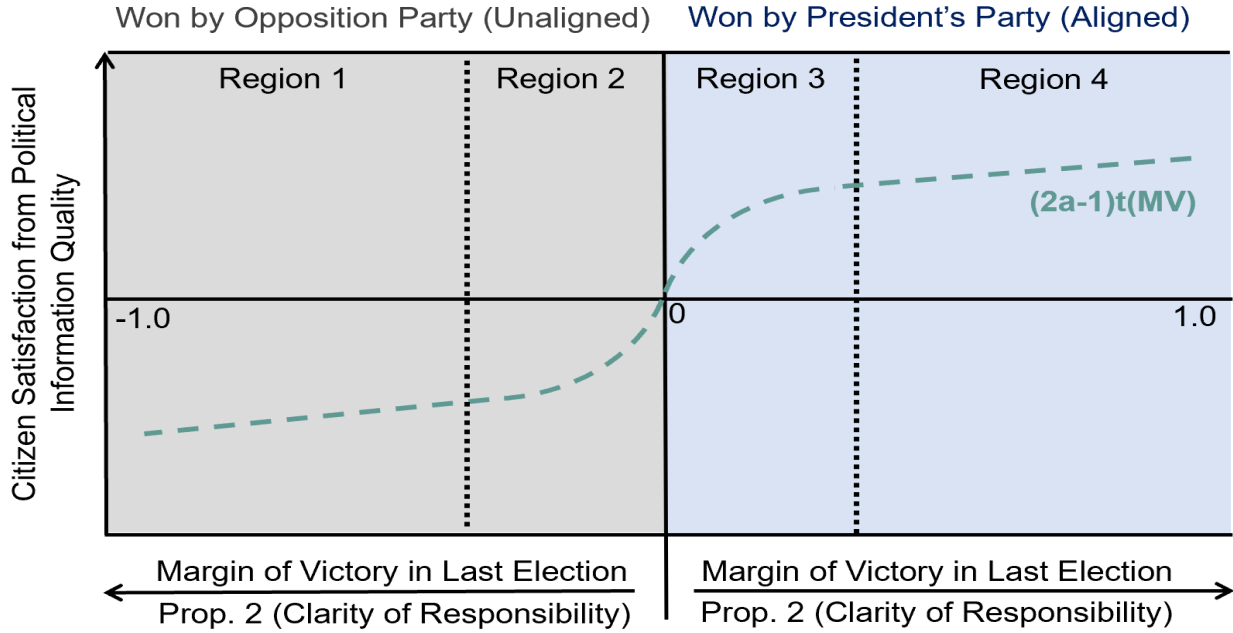
The first channel focuses on the direct effects of clarity of responsibility on citizens' overall level of satisfaction with local-level politician  $i$  in a given area,  $s_i$ . As we show in Appendix D, the direct effects of clarity of responsibility on  $s_i$  jointly depend on local-level politician  $i$ 's margin of victory in the last election ( $MV$ ) and party alignment status ( $a$ ).

<sup>12</sup> Mathematically,  $t(0) = 0$ .

<sup>13</sup> Given Equation (1), Equation (4) also captures the inverse benefits that the electorate derives from the local-level politician's rents in the current period,  $r_{i,1}$ .



Figure 1: Margin of Victory, Party Alignment, Information, and Clarity of Responsibility



We capture this joint dependency and its ability to be positive when aligned or negative when unaligned with the term  $(2a - 1)t(MV)$ .<sup>14</sup> Per Figure 1 and Appendix D,  $(2a - 1)t(MV)$  reflects citizens' levels of satisfaction via the quality of political information that they receive, which is highest in Region 4.

Alignment plays an independent role as well. When local-level politician  $i$  shares the same party as the executive, citizens can easily discern which politician(s), party, or governing coalition is responsible for corruption or effective government. By contrast, citizens' abilities to make such snap judgments are not as robust under divided government (Schwindt-Bayer and Tavits, 2016; Appendix D). That is particularly the case in poor areas, as they tend to suffer from political market imperfections, such as voters lacking information about politician performance,<sup>15</sup> identity voting,<sup>16</sup> and politicians' inability to make credible promises to voters (Keefer, 2004, 2007a,b; Keefer and Khemani, 2005; Keefer and Vlaicu, 2008).

The second channel through which clarity of responsibility affects  $s_i$  relates to a primary

<sup>14</sup> When the politicians is aligned ( $a = 1$ ), then  $2(1) - 1 * t(MV)$  must be positive. When the politician is unaligned ( $a = 0$ ), then  $(2(0) - 1) * t(MV)$  must be negative.

<sup>15</sup> See, for example, Pande (2011), Banerjee et al. (2014), and Lieberman, Posner and Tsai (2014).

<sup>16</sup> See, for example, Chandra (2004), De La O and Rodden (2008).

consequence of political market imperfections: the extent to which citizens value corrupt politicians and clientelism.<sup>17</sup> A large literature establishes that lower poverty leads voters to discount clientelistic benefits more with respect to policy-based, programmatic benefits.<sup>18</sup> Citizens also discount other benefits that corrupt politicians may bring in a similar manner,<sup>19</sup> and we posit that clarity of responsibility amplifies these discounting patterns.

We account for the *additional* discounting brought about by low or decreased poverty on  $W(\gamma r_{i1})$  through  $\beta_i$ . In lower poverty electorates  $\beta_i \in (0, 1)$ , and  $\beta_i = 1$  in higher poverty electorates. In other words, citizens' *a priori* discount rate of  $W(\gamma r_{i1})$  remains unchanged except under the scenario in which poverty is low or has recently reduced.

Especially given information's mixed record in fostering political accountability in poor environments,<sup>20</sup> it is crucial to understand how clarity of responsibility fosters different discount rates of corruption-related benefits. Per [Schwindt-Bayer and Tavits \(2016\)](#) and [Figure 1](#), alignment makes identifying clarity of responsibility easier. Accordingly, we suggest that  $\alpha$  magnifies the penalization imposed by low or reduced poverty ( $\beta_i \in (0, 1)$ ) on the

<sup>17</sup> Clientelism entails the the contingent distribution of material and non-material goods and services in exchange for political support. There are many varieties of clientelism, including vote-buying, (e.g. [Auyero, 1999](#); [Stokes, 2005](#); [Finan and Schechter, 2012](#); [Hidalgo and Nichter, 2016](#)); turnout buying (e.g. [Nichter, 2008](#); [Larreguy, Marshall and Querubín, 2016](#)); abstention-buying (e.g. [Gans-Morse, Mazzuca and Nichter, 2014](#)); double persuasion (e.g. [Gans-Morse, Mazzuca and Nichter, 2014](#)); and patronage ([Robinson and Verdier, 2013](#)). In making our argument, we make no distinction between the different forms of clientelism; our argument applies to the phenomenon as a whole.

<sup>18</sup> By programmatic benefits, we mean that the rules concerning their distribution are public, followed, and are not targeted at a particular group or area ([Hicken, 2011, 296](#); [Stokes et al., 2013, 7](#)). For an overview of why reducing poverty also leads to a reduction in clientelism, see [Stokes et al. \(2013, Chapter 6\)](#). Qualitative work, notably from [Chubb \(1982\)](#) and [Auyero \(1999, 2000\)](#), provided the basis for the poverty-clientelism relationship. Recent studies from [Gonzalez-Ocantos, Kiewiet de Jonge and Nickerson \(2014\)](#), [Jensen and Justesen \(2014\)](#), [Szwarcberg \(2015\)](#), and [Muños \(2019, 228-229\)](#) have provided quantitative confirmation as well.

<sup>19</sup> Here, we are referring to the trade-off hypothesis, commonly known through the Portuguese expression "*rouba mas faz*" [he steals but gets things done]. In short, voters trade-off the value of a corrupt politician against the clientelistic benefits and other benefits (e.g. ideology) the politician can bring ([Magaloni, Díaz-Cayeros and Estévez, 2007](#); [Manzetti and Wilson, 2007](#); [Pereira, Rennó and Samuels, 2011](#); [Winters and Weitz-Shapiro, 2013](#); [Pereira and Melo, 2015](#); [Muñoz, Anduiza and Gallego, 2016](#); [Solaz, De Vries and de Geus, 2019](#); [Leight et al., 2020](#)). That is particularly the case when voters are poor and less educated ([Keefer, 2007a](#); [Zechmeister and Zizumbo-Colunga, 2013](#); [Del Mar Martínez Rosón, 2016](#); [Nichter and Peress, 2017](#)); and when voters believe that the corruption is self-reinforcing to the extent that there are no clean alternatives in the candidate pool ([Charron and Bågenholm, 2016](#); [Pavão, 2018](#); [Agerberg, 2020](#)).

<sup>20</sup> See, for example, [Keefer \(2004, 2007a,b\)](#), [Kosack and Fung \(2014\)](#), [Chong et al. \(2015\)](#), [Fox \(2015\)](#), [Dunning et al. \(2019\)](#).

pre-existing discount rate,  $W(\gamma r_{i1})$ , such that:  $\beta^{1+a} = \beta^{1+1} \implies \beta^2 < \beta^1$ . In words, alignment leads to even higher discount rates for clientelistic and other benefits than the unaligned case due to clarity of responsibility when poverty is low or decreasing. Given that  $\beta = 1$  when poverty is higher, the effects of clarity of responsibility do not travel beyond the lower poverty scenario:  $\beta^{1+a} = \beta^{1+1} \implies \beta^2 = 1^2 = 1 = \beta^1$ .

### 1.3. Solving the Local-Level Politician's Maximization Problem

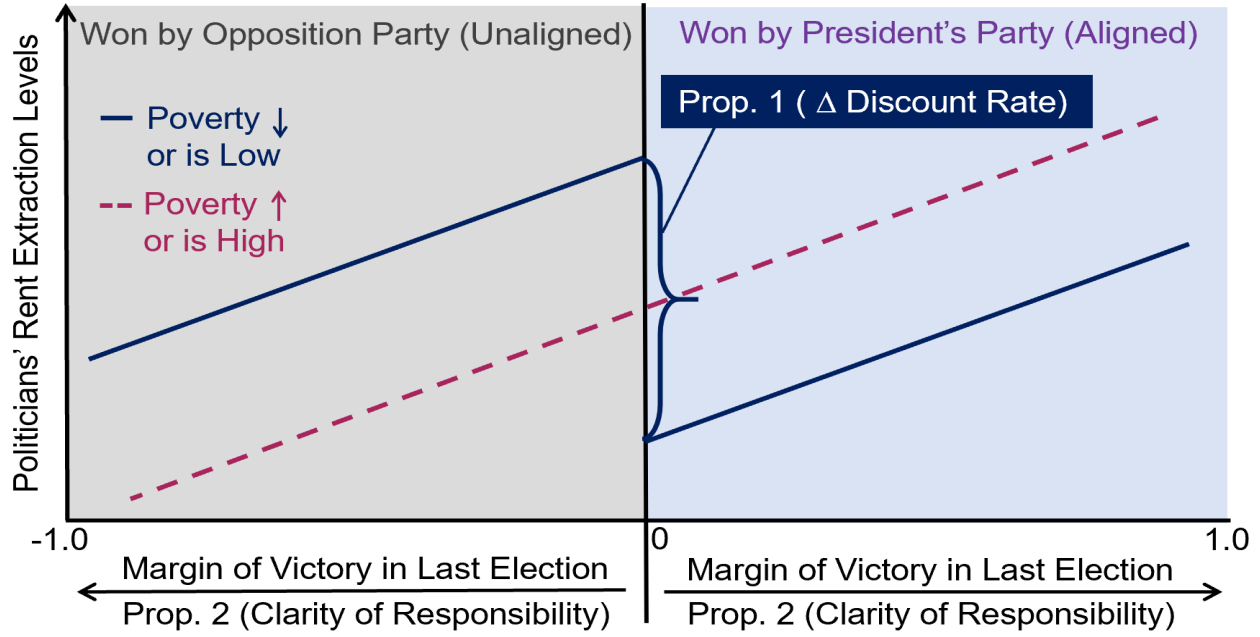
To represent local-level politicians  $i$ 's full utility function, we introduce  $U(\cdot)$ . It captures local-level politician  $i$ 's utility from rent extraction in the current period,  $r_{i,1}$ , rent extraction in a future period,  $r_{i,2}$ , and the private income that she can earn while out of office in that future period,  $x_{i,2}$ , such that  $U' > 0$  and  $U'' < 0$  (Brollo and Nannicini, 2012). It is necessary to complement  $r_{i,1}$  and  $r_{i,2}$  with  $x_{i,2}$ , because politicians trade-off rent extraction in the current period against that of a potential future period (Niehaus and Sukhtankar, 2013). To that end, since politicians in serving in areas with relatively high levels of corruption and clientelism can generally earn more in office than as a private citizen (Querubín and Snyder, 2013; Fisman, Schulz and Vig, 2014), we specify that  $x_{i,2} < r_{i,2}$ . For its part, the political party of local-level politician  $i$  also wishes to maximize its representation, so its incentives are to ensure that  $r_{i,1}$  are not high enough to potentially cause a corruption scandal that hurts the party brand. Against this backdrop, and given Equations (3) and (4), the maximization problem for local-level politician  $i$  can be represented as:

$$\begin{aligned} \max_{r_{i,1}} \quad & U(r_{i,1}) + \pi(s_{i,1}) U(r_{i,2}) + (1 - \pi(s_{i,1})) U(x_{i,2}) \\ \text{where } s_{i,1} = \quad & W(g_{i,1}) + \beta_i^{1+a} W(\gamma r_{i,1}) + (2a - 1)t(MV) \end{aligned} \tag{5}$$

**Proposition 1:** *Optimal rents for aligned politicians are less than rents for unaligned politicians at  $MV = 0$  when the electorate's economic circumstances are good or have improved.*

The differing discounting rates for aligned and unaligned electorates drives Proposition

Figure 2: Graphic Presentation of Propositions 1 and 2



1. Specifically, the higher penalization of clientelistic and other benefits in the aligned electorates reduces the reelection probability of aligned local-level politicians with respect to the unaligned ones. Therefore, when  $MV$  approaches 0, or right at the cutoff, this difference in discount rate results in a discontinuity between the optimal rents extracted, where aligned politicians extract less than the unaligned politicians. Refer to the solid blue line in Figure 2.

*Proof:* See Appendix C.

**Corollary 1** also shows the case when economic circumstances are poor or worsen in a given electorate. In such a case, because citizens do not discount any differently in either the aligned or the non-aligned electorates, there does not exist any discontinuity at the cutoff. The dotted red line in Figure 2 captures such a scenario.

*Proof:* See Appendix C.

**Proposition 2:** *Optimal rents for aligned politicians increase with respect to  $MV$ , while they decrease with respect to  $MV$  for the unaligned politicians.*

The direct effect of clarity of responsibility on citizen’s levels of satisfaction with their local-level politician underpins Proposition 2, which does not depend on poverty. For the unaligned electorates, the lack of clarity of responsibility negatively affects citizens’ satisfaction with local-level politician  $i$  through the varying means described in Appendix D. Unaligned local-level politicians, in turn, react by reducing their optimal rent-seeking behavior in a manner consistent with the levels of  $MV$ . The opposite effect takes place in the aligned municipalities. Since the clarity of responsibility from alignment positively affects citizens’ satisfaction with local-level politician  $i$  (see Figure 1), it provides aligned politicians with additional opportunity for rent extraction as  $MV$  increases (see Figure 2). Accordingly, our model is consistent with previous literature underscoring that party alignment fuels greater levels of rent extraction and politically-motivated spending (Greene, 2010; Brollo and Nannicini, 2012; Carozzi and Repetto, 2016; Corvalan, Cox and Osorio, 2018; Lara and Toro, 2019).

*Proof:* See Appendix C.

## 2. Research Design

### 2.1. Institutional Context for Guatemala

Guatemala is a poor Central American country with a population of roughly 18 million people, of which 59% live in poverty and 23% live in extreme poverty (World Bank, 2017). Like many countries in the region, Guatemala officially has a presidential democracy but is not fully democratic. The country emerged from a devastating, 36-year civil war in 1996, and since then Guatemala registered some democratic advances but maintains significant authoritarian enclaves and rather weak institutions (González, 2014).

Corruption, clientelism, and organized crime present particularly onerous challenges for Guatemala. The country’s 2006-2019 partnership with the United Nations’ Interna-

tional Commission Against Impunity (CICIG) helped uncover some high-level corruption and dismantle some powerful drug-trafficking networks (Fisman and Golden, 2017; Trejo and Nieto-Matis, 2019). Nevertheless, the country still ranks 144/180 on Transparency International’s (2018) Corruption Perceptions Index, part of the reason for which is likely due to clientelistic pressures. For example, vote buying is a concern in social programs, and CICIG investigations have revealed significant use of state resources in the financing of party campaigns (Sandberg and Tally, 2015; Meilán, 2016).

General elections for both the national and municipal levels take place concurrently every four years. For departments, which comprise administrative level-2 units akin to a state or province, the president appoints governors from his or her same political party. Accordingly, Guatemala does not have political variation at the department level.

## 2.2. Identification Strategy

Although the lack of political variation at the department level may not be ideal for democracy, it is a boon for our identification strategy. Because there is no political variation in Guatemalan governors, it allows us to directly estimate the effects of mayor-president party alignment on corruption. To causally identify these effects in each of our samples, we employ a series of sharp electoral regression discontinuity designs. They leverage random variation in close elections to as-if randomly assign winning mayors into alignment or non-alignment with the president on the basis of both the mayoral and presidential elections. In line with Brollo and Nannicini (2012), we identify the parameter of theoretical interest, the Average Treatment Effect (ATE), as:

$$\begin{aligned} \tau &= \mathbf{E}[r_{it}^{(aligned)} - r_{it}^{(unaligned)} | MV_{it} = 0] = \\ & \lim_{MV \downarrow 0} \mathbf{E}[r_{it} | MV_{it} = MV] - \lim_{MV \uparrow 0} \mathbf{E}[r_{it} | MV_{it} = MV], \text{ such that } MV \in (-h, h) \end{aligned} \quad (6)$$

where  $r_{it}$  reflects the amount of corruption in the aligned/unaligned municipality  $i$  at time  $t$

after a close election; the running variable,  $MV_{it}$ , is the margin of victory for aligned/unaligned mayor  $i$  in the most recent election for time  $t$ ; and  $\pm h$  corresponds to the upper/lower limit of an automatically derived, optimal close-election bandwidth for  $MV$ , following [Calonico, Cattaneo and Titiunik \(2014\)](#). For  $MV_{it} \in (-h, h)$ , we estimate  $\tau$  through a local polynomial regression following [Cattaneo, Idrobo and Titiunik \(2019, 70\)](#):

$$r_i = \alpha + f(MV_i) + \tau D_i + Z_i' \rho + \eta_i \quad (7)$$

$$\text{where } f(MV_i) = \sum_{k=1}^p \beta_k MV_i^k + \sum_{k=1}^p \gamma_k D_i \cdot MV_i^k$$

where  $\alpha$  is the intercept,  $\eta$  is a normally-distributed error term,  $D_i$  is the municipality alignment treatment dummy variable, and  $Z_i$  are the additional covariates that we include to ensure the robustness per [Calonico et al. \(2019\)](#). Following [Gelman and Imbens's \(2019\)](#) advice on avoiding potential bias-variance trade-offs, the estimation relies on polynomials fits of the first and the second order—i.e.,  $p \in (1, 2)$ . We also cluster the standard errors at the municipality level per [Bartalotti and Brummet \(2017\)](#), and follow [Frey \(2019\)](#) by including fixed effects where possible—a falsification test that is very uncommon, even among the most sophisticated regression discontinuity analyses (e.g., [Klašnja and Titiunik, 2017](#)).

### 2.3. Poverty Data and Samples for Estimation

The municipality-level poverty data in this paper come from Guatemala's National Statistics Institute (INE, *Instituto Nacional de Estadística*) poverty maps. The data specifically refer to the percent of people below the poverty and extreme poverty lines. As with most countries in the world, Guatemala does not measure municipal-level poverty rates on a yearly basis. Instead, the country only measures municipal-level poverty rates for the whole country during each census. The latest two years for which poverty map/census data are available are 2002 and 2011.

Given the lack of panel poverty data and inability of regression discontinuity designs

to accommodate interactions,<sup>21</sup> we split our sample into the following groups: low-poverty, high-poverty, poverty-increasing, poverty-decreasing, extreme poverty-decreasing, and extreme poverty-increasing municipalities. We construct the low/high poverty measures on the basis of the median. The poverty-decreasing and poverty-increasing samples correspond to municipalities in which poverty decreased or increased from one census measure to the next. For comparison with the macro-level predictions of [Schwindt-Bayer and Tavits \(2016\)](#), we also provide estimations using the whole sample—i.e., not dividing the sample by the poverty levels or changes.

For the analysis by high and low levels of poverty, the sample corresponds to the years 2004-2015. We provide the estimates by poverty or extreme poverty changes for the years 2010-2015 (main analysis), 2011-2015 (Appendix [N](#)), 2009-2015 (Appendix [O](#)), and 2008-2015 (Appendix [P](#)). To accommodate analysis with years other than 2011-2015, we backdate the 2011 poverty rate measure by one, two, or three years. This backdating is justifiable because census poverty measurements for 2011 took place between 2008-2011 ([Instituto Nacional de Estadística de Guatemala, 2014](#)), it is unlikely that estimates fluctuate much from year-to-year, and it is improbable that most citizens are aware or respond to INE's poverty rate announcements. Policy commitments and information are generally not very credible or abundant in a context of poverty like Guatemala, but people generally have a sense of whether their economic conditions are improving ([Banerjee and Duflo, 2007, 2011](#); [Keefer, 2004, 2007a,b](#); [Keefer and Khemani, 2005](#); [Keefer and Vlaicu, 2008](#); [Dunning et al., 2019](#)).

From our 331-municipality cross-section in the panel, poverty data are missing from 32 urban municipalities in 2011.<sup>22</sup> Accordingly, we provide a relevant analysis of these missing

<sup>21</sup> A recent working paper from Carril et al (2017) provides a first attempt to conduct subgroup analysis for regression discontinuity designs. Even though the method does not produce bias-corrected inference or accommodate data-driven bandwidth selection, we attempted to use the paper's accompanying Stata routine, `rddsga`, for estimation. However, the Stata routine produced many bugs, and would not estimate properly. For all of these reasons, our main estimates do not rely on the `rddsga` Stata routine.

<sup>22</sup> According to an email communication with the Guatemalan National Statistics Institute (INE), the 2011 municipal poverty mapping exercise was funded entirely by the World Bank, and funding was not provided to the ascertain the poverty rates for all municipalities.



data in Appendix [R](#). On the basis of this analysis, we conclude that these missing data do not suggest any potential biases.

## 2.4. Electoral Data

We draw the municipal electoral data for this study from Guatemala’s Supreme Electoral Institute (TSE, *Tribunal Supremo Electoral*). After each election the TSE publishes a *Memoria Electoral*, which is an electoral almanac documenting the results of all electoral races in each respective election. For each election, we collected panel data on (i) the names of each winning mayor; (ii) the political party of each winning mayor; (iii) the political party of each second-place candidate; (iv) the number of votes acquired by each winning mayor; (v) the number of votes received by each second-place candidate; (vi) the total number of votes received in the municipalities; and (vii) the number of spoiled ballots. With these data, we first calculate the number of valid votes for each race by subtracting the number of spoiled ballots from the total votes. We then calculate the valid vote shares for the winning and second-place candidates by dividing the number of votes each received by the total number of valid votes. The margin of victory is thus the winning mayor’s share of valid votes received subtracted by those of the second-place candidate. Similar to [Brollo and Nannicini \(2012\)](#), our running variable for the regression discontinuity design is the margin of victory for the aligned/unaligned party mayor. To capture the aligned/unaligned distinction, we follow [Brollo and Nannicini \(2012\)](#) and multiply the margin of victory for the unaligned mayors by negative one (see Figures [1](#) and [2](#)). If neither the first- nor second-place candidate is from the aligned party, we exclude it from the analysis. Such a strategy allows the empirical analysis to focus on close races in line with our theory and is consistent with the regression discontinuity analyses of [Meyersson \(2014\)](#), [Dell \(2015\)](#), and [Fergusson et al. \(2020\)](#).

Given that the TSE’s funding and capacity are limited ([Meilán, 2016](#)), we take additional steps to ensure that the data are not marred by electoral fraud and are suitable for analysis, etc. In Appendices [L.1](#), [L.2](#), [L.3](#), [L.4](#), [L.5](#), and [L.6](#), we run [McCrary \(2008\)](#) den-

sity tests corresponding to our running variable for all of the different samples in the main analyses and appendices. To do so, we use Cattaneo, Jansson and Ma’s (2018, 2020) new method. All tests corresponding to the original electoral term data pass. The failing tests only correspond to some year-wise perspectives of the electoral data.<sup>23</sup>

## 2.5. Corruption Data

The corruption data for this study come from Guatemala’s National Audit Office (*Contraloría General de Cuentas*). Although corruption remains a significant problem in Guatemala, the country’s constitution and many laws protect the integrity of the office and its findings. Notably, Article 233 of the current Guatemalan constitution (i.e. from 1985) stipulates that the head of the office (*Controlador de Cuentas*) is elected to four-year, non-reelectable terms by the Congress, not the President. Removing the *Controlador de Cuentas* is also uniquely within the purview of the Congress. It can only remove the *Controlador de Cuentas* by majority vote only for reasons pertaining to “negligence, crime, and lack of aptitude.” In short, Guatemala’s National Audit Office is not a patronage body that serves the interests of the president, making its data suitable for the purposes of this study on alignment and corruption.

Each year, the National Audit Office audits circa 320 of Guatemala’s 340 municipalities. As shown in Appendix L.9, unaligned municipalities are not more likely to be audited than their aligned counterparts. That is accurate for all of the samples that we examine in this study (see Section 2.3). Accordingly, there are no concerns regarding the partisan distribution of audits.

For each audited municipality from 2004-present, the National Audit Office publishes

---

<sup>23</sup> For example, a year-wise perspective on the 2010-2015 sample comprise the December 2007 election results twice (for the years 2009 and 2010); the December 2011 election results four times (for the years 2012, 2013, 2014, 2015); and the corruption (i.e. dependent variable) data for each respective year. A term-wise perspective for the same 2010-2015 period, by contrast, comprises the results from the December 2011 and December 2015 elections one time, with the respective corruption (i.e. dependent variable) data aggregated for each electoral term for the respective years in question. Accordingly, there is no concern regarding the original distributions of the electoral data.

on its website: the number of overall infractions committed (*sancciones*), and the amount of money in the local currency (Quetzales) associated with these infractions. Both of these variables serve as our study’s dependent variables. Unlike the problematic, perceptions-based measures that dominate the literature (see Kurtz and Schrank, 2007; Hollyer, 2018), these dependent variables are objective and correspond most closely with bureaucratic corruption. It encompasses both what Brollo et al. (2013, 1774) call “broad corruption” and “narrow corruption”.<sup>24</sup> For comparability purposes, we first deflate the money version of the infractions variable and then take its log. We do not transform the number of infractions committed variable.

Table B2 presents descriptive statistics for the infractions variable. We disaggregate the data according to whether they correspond to poverty-increasing/poverty-decreasing and aligned/unaligned municipalities.

## 2.6. Other Data

Although most sharp regression discontinuity analyses typically assume that treatment assignment is as good as random within the data-driven bandwidth, we use Calonico et al.’s (2019) method to control for the influence of covariates within the bandwidth. We take covariate data on population and inequality (Gini Coefficient) from Guatemala’s National Statistics Institute. We include data on public goods spending from the Guatemalan Ministry of Finance, which made its data publicly available through the World Bank’s (2019) BOOST Initiative.<sup>25</sup> Table B4 presents descriptive statistics of these covariate data.

<sup>24</sup> *Broad corruption* refers to “irregularities that could also be interpreted as bad administration as rather than as overt corruption.” *Narrow corruption* refers to “severe irregularities that are also more visible to the voters” (Brollo et al., 2013, 1774).

<sup>25</sup> The data aggregate spending on the following categories: Care and natural disaster management; defense and homeland security; defense; education; environmental protection; health; internal security; public order and safety; social protection; sports, culture, recreation, and religion; and urban community services.

### 3. Results

#### 3.1. Corruption Results Disaggregated by Poverty

Figure 3 presents the main results for the infractions dependent variable by electoral term. We show the term-wise results for the (log) infraction amounts dependent variable in Figure 4. The figures corresponding to the year-wise results for the same dependent variables, Figures A.1 and A.2, can be found in Appendix A. All of the above figures report second-order polynomial fits per Gelman and Imbens (2019). Appendices E, G, and K contain full tables.

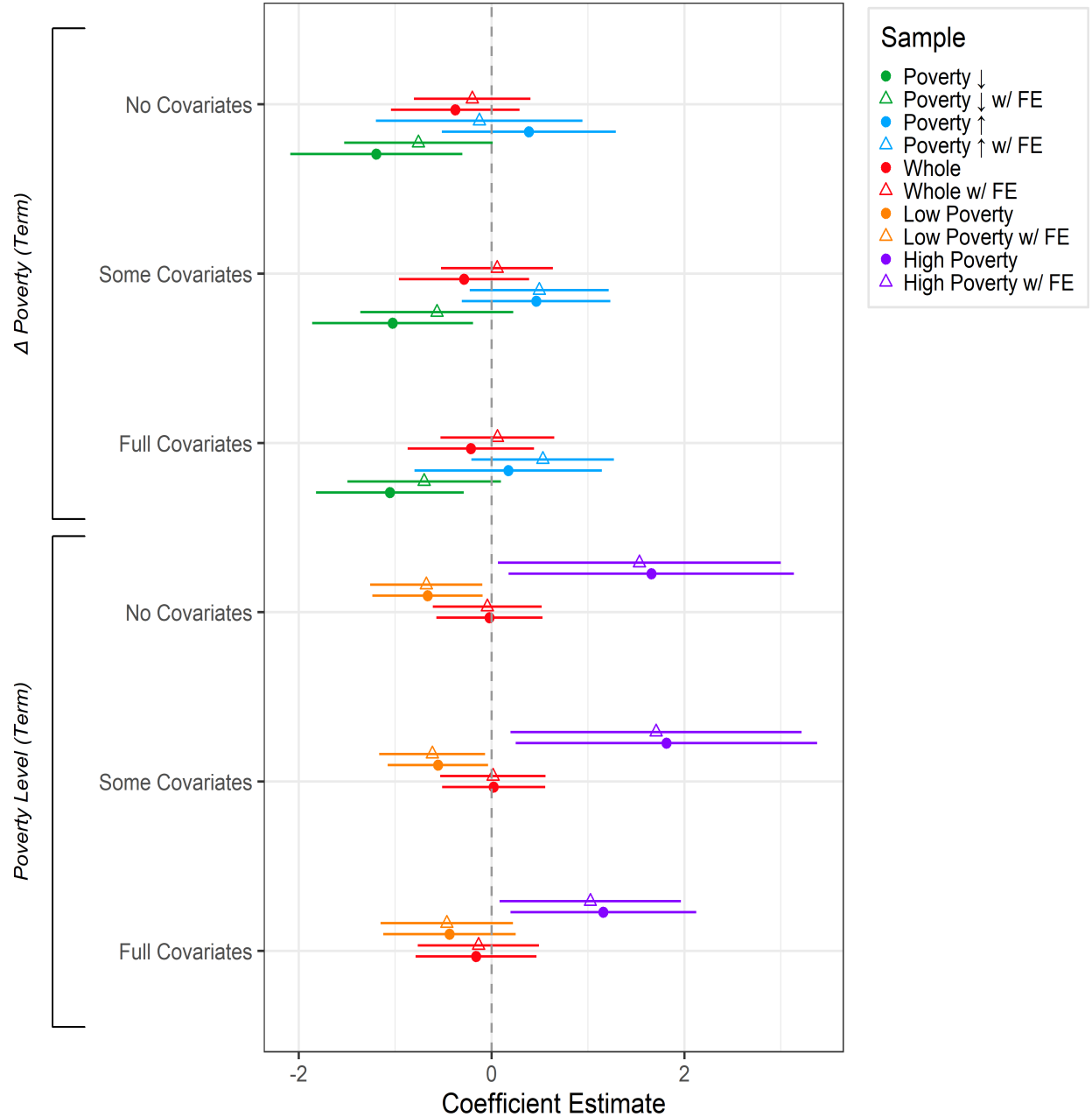
Overall, the results are similar for both yearly and electoral term data: party alignment consistently yields less corruption in the low-poverty and poverty-reducing samples. The results for these samples are not only statistically significant but substantively significant as well. For example, in our base term specification without fixed effects, aligned municipalities commit an average of 13.73 less infractions in the poverty-reducing sample or 6.09 less infractions in the low-poverty sample. Once we add municipal fixed effects in line with Frey (2019), the results remain statistically and substantively significant: aligned municipalities commit an average of 5.13 less infractions in the poverty-reducing sample or 6.49 less infractions in the low-poverty sample. The log amount results remain consistent, too.

Controlling for the influence of covariates within the automatically-derived, data-driven bandwidth in line with Calonico et al. (2019) does not alter the interpretation of our results. In Appendix L.7, we further show that these results are not due to outliers. When we change the samples to encompass different years in Appendices N.1, O.1, P.1, we also find similar results. Given that tests reveal that poverty is not empirically endogenous to corruption (See Appendix M), the results for the low-poverty and poverty-reducing sample are robust.

The effects of alignment on reducing corruption in the poverty-reducing sample are more pronounced within the final two years of the electoral term. Tables I1 and I2 show



Figure 4: Log Infraction Amount by Term



Note: The above estimates are second-order polynomial fits in line with [Gelman and Imbens \(2019\)](#), with standard errors clustered by municipality and confidence intervals at the 90% level. “FE” corresponds to municipality fixed effects. “Some Covariates” refer to (log) population and a mayor re-election dummy variable. “Full Covariates” refer to (log) population, a mayor re-election dummy, inequality (gini coefficient), and (log) public goods per capita. Full tables corresponding to the above Figure can be found in Appendices [E](#), [G](#), and [K](#).

that the results for the last two years. When compared to the results from the first two years in Tables [J1](#) and [J2](#), it is clear that the final two years of each electoral term are mostly driving the decrease in corruption in the low-poverty and poverty-reducing samples. Overall, these results are consistent with [Ferraz and Finan \(2008\)](#) and [Bobonis, Cámara Fuertes and Schwabe \(2016\)](#), who find that audits in Brazil and Puerto Rico are most effective at reducing corruption closer to elections. More broadly, the results of our analysis are consistent with [Barro \(1973\)](#), [Ferejohn \(1986\)](#), [Ferraz and Finan \(2011\)](#), and [de Janvry, Finan and Sadoulet \(2012\)](#): elections help discipline politicians. In our case, that applies even to aligned politicians, who generally enjoy resource advantages relative to non-aligned politicians (e.g. [Brollo and Nannicini, 2012](#); [Carozzi and Repetto, 2016](#); [Corvalan, Cox and Osorio, 2018](#); [Lara and Toro, 2019](#)).

As predicted by our theory, alignment only reduces corruption in the poverty-reducing and low-poverty samples. Appendix [G.1](#) disaggregates results for the sample in which poverty increased from one census to next, and Appendix [G.2](#) shows the results for the municipalities with poverty higher than the median level. In both Appendices [G.1](#) and [G.2](#), results generally shift in the opposite direction from the low-poverty and poverty-reducing samples (see Appendix [E](#)). When poverty is high or increased from one census to the next, there is an uptick in corruption—again, measured by infractions or the log amounts associated with those infractions. Theoretically, it is logical that poorer voters may be more forgiving of mayors’ corruption, as long as the mayors share their rents with voters through clientelistic or other means means ([Fernández-Vázquez, Barberá and Rivero, 2016](#)). However, the year-wise specifications for the poverty-increasing sample fail the [McCrary \(2008\)](#) density tests in Appendices [L.1](#), [L.2](#), [L.3](#), [L.4](#), and [L.5](#), and none of the specifications for the poverty-increasing sample have statistically significant results. The same is true for when we alter the sample in Appendices [N.2](#), [O.2](#), and [P.2](#). Accordingly, we caution against interpreting the results from the high-poverty and poverty-increasing samples as definitive evidence of higher poverty facilitating aligned mayors to extract higher levels of rents.

For purposes of comparison with current predictions of clarity of responsibility theory (see [Schwindt-Bayer and Tavits, 2016](#)), Appendix [K](#) shows the results for the whole sample—i.e., when not disaggregating by poverty. Overall, these findings from the whole sample are inconsistent. Sometimes, alignment yields less corruption; other times, it leads to more corruption. In all instances, though, none of the results are statistically significant. We thus interpret the whole sample results as evidence of the fact that alignment both provides resource advantages and increases clarity of responsibility. When not disaggregating the sample by poverty, these countervailing effects often cancel each other out, which is what the data show here.

### 3.2. Corruption Results Disaggregated by Extreme Poverty

To further assess the extent to which better economic conditions can reduce corruption from aligned politicians, we also examine the extent to which low or decreasing extreme poverty yields similar results as those of low or decreasing poverty. In all specifications, which are detailed in Appendix [F](#), alignment reduces corruption when extreme poverty is low or declines. In our base specification with municipal fixed effects, aligned municipalities commit an average 3.66 less infractions in the poverty-reducing sample or 1.8 less infractions in the low-poverty sample in each term. Results are a bit weaker for the log amounts, as less specifications are statistically significant. Nevertheless, the results with the log amount as the dependent variable are still suggestive of the same overall pattern: reductions in extreme poverty yields a situation in which aligned politicians reduce their overall corruption levels.

As with the previous subsection, the same results do not hold for the high-extreme-poverty or increasing-extreme-poverty samples (see Appendix [H](#)). In nearly all specifications entailing counts of the number of infractions and the (log) amounts associated with those infractions, the coefficient for alignment is positive, indicating that alignment yields an increase in corruption. However, similar to the results for the high-poverty and the poverty-increasing samples, none of the results are statistically significant for the high-extreme-



poverty or increasing-extreme-poverty samples, and the year-wise specification does not pass the McCrary (2008) density test (see Appendix L.6).

## 4. Analysis of the Poverty, Alignment, and Close Elections Mechanisms

### 4.1. Alignment as a Mechanism to Signal Politicians' Clarity of Responsibility for Misgovernance to Voters

A premise of the above results is that alignment can act as a mechanism to signal politicians' clarity of responsibility for misgovernance to voters, and that politicians are aware and take mitigating measures (see Appendix D). Although Schwindt-Bayer and Tavits (2016) clearly and comprehensively demonstrate the power of the mechanism, it is necessary to empirically reaffirm with data from Guatemala. We do so with an analysis of municipal corruption levels before and after Guatemala experienced an alignment and party system shock in 2016.

In Guatemala's October 25, 2015 run-off election, the people elected a populist outsider, Jimmy Morales, as president. Since not a single candidate from Morales' party, National Convergence Front (FCN), won a mayoral race during the same general election, it ensured that there were no mayoral-presidential party alignments for the 2016-2019 period.<sup>26</sup> The lack of alignments for the 2016-2019 period limits the ability of the results in the previous sections to travel to other instances party system instability. By the same token, the shock of electing a populist outsider and its consequent effects on alignment allows us to credibly identify the power of the alignment mechanism and thus support the results presented above.

Both the mean number of municipal-level infractions and amount of misappropriated

---

<sup>26</sup> New presidents in Guatemala take power in January, and the relevant elections take place late in the previous year.

Table 1: Infractions and Misappropriated Money Amounts by Alignment Shares

Term	Municipalities Aligned %	Infractions Mean	Amount Mean	Log Amount Mean
2008-2011 (Colom)	31%	5.33	213,240.9 Q	12.27
2012-2015 (Molina/Maldonado)	36%	6.56	215,885.7 Q	12.28
2016-2019 (Morales)	0%	12.84	429,378.8 Q	12.97

Note: All amounts adjusted for inflation in the local currency, Quetzales. We exclude the 2004-2007 term since the number of audits taking place from 2004-2006 was minimal.

money increased significantly after the election of Morales (see Table 1). By the same token, the (quasi) natural experiment of Morales’ election is probably not sufficient for these descriptive statistics in Table 1 to be interpreted on their own. We therefore supplement these descriptive statistics with the regression analyses presented in Table 2 and the additional tables in Appendix S. Each regression contains the main, time-varying covariates used in our regression discontinuity analyses throughout the paper as well as the poverty indicators used to construct our samples. We exclude the alignment variable because it is collinear with the Morales Term variable, which serves as our main independent variable for the analysis. We also do not interpret the control variables per Cinelli and Hazlett (2020, 45). Given that infractions is a count variable, we estimate those respective regressions with Poisson and negative binomial models, and the log amounts regressions are estimated with linear regression.

Consistent with our expectations, the Morales Term variable is mostly positive and highly statistically significant throughout. The results are slightly stronger for the number of infractions than the log amounts associated with those infractions, but the overwhelming evidence points to increased corruption following the election of populist outsider Morales. In short, party system instability is associated with more corruption. Since the party system instability makes it more difficult to discern clarity of responsibility due to the lack of alignments, mayors take advantage of the institutional configuration and oversee municipalities that commit more corruption.

Table 2: Number of Infractions Committed (2008-2019)

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.786*** (0.022)	0.748*** (0.022)	0.443*** (0.046)	0.787*** (0.021)	0.573*** (0.036)	0.487*** (0.049)
Poverty Reduced		-0.071** (0.035)	-0.073** (0.036)			
Population (log)					1.571*** (0.209)	-0.337 (0.301)
Re-elected Mayor					0.008 (0.034)	0.002 (0.031)
Observations	3801	3357	3357	3801	3518	3518
Municipality FE	no	no	no	yes	yes	yes
Year FE	no	no	yes	no	no	yes

Note: Poisson regression model, since infractions are a count variable.

Standard errors clustered by municipality in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## 4.2. Analysis of the Poverty Mechanism

For the main results presented in Sections 3.1 and 3.2 to map well to our theory, it is necessary to further demonstrate the power and appropriateness of the poverty mechanism. To do so, first, we show that poverty is exogenous to corruption. Second, we provide an empirical analysis of corrupt vs. non-corrupt mayors by alignment status in our low-poverty, high-poverty, poverty-reducing and poverty-increasing samples.

If poverty is endogenous to corruption in our empirical analysis, it is not appropriate to interpret the results presented in Sections 3.1 and 3.2 as causal. We therefore test for endogeneity between poverty and corruption in Appendix M. Since endogeneity entails a correlation between the independent variable and the error term, we first directly test for such a relationship using two-stage regression analysis (see Appendix M.2). In the first stage, we separately run a regression of poverty on each of corruption variables: the number of infractions committed and the log amounts associated with those infractions. In the second stage, we regress the residuals from the first-stage equation on each corruption variable.

In all instances, the results suggest no overall relationship and R-squared values that are essentially 0, indicating that there is no endogeneity between poverty and corruption. Since the lack of endogeneity is so critical to our results, we undertake a second set of regression analyses as well. More specifically, in Appendix M.1 we test whether corruption predicts poverty in a conventional linear regression. Using numerous specifications for both the year-wise and term-wise results, we find no empirical support for the proposition that poverty predicts corruption.

As a final piece of evidence in favor both our overall results in Sections 3.1 and 3.2 as well as the poverty mechanism, we present descriptive statistics on how poverty and alignment condition behavior by both corrupt and non-corrupt mayors in Appendix Q. To facilitate such analysis, we use the median number of infractions committed and the log amounts associated with those infractions to divide the sample into corrupt and non-corrupt mayors. Although the median measures of corrupt and non-corrupt mayors are crude, they help demonstrate how each mechanism melds together to support our theory.

Consider, for example, Panel A of Table Q1, which presents the number of infractions committed in the poverty-reducing sample. Under such circumstances, approximately 58% of aligned mayors are less corrupt than the median, whereas 42% are more corrupt than the median. For unaligned mayors in the poverty-reducing sample, the results present the opposite pattern: 67% of mayors are more corrupt than the median, and 32% of mayors are less corrupt than the median. We can find results that similarly conform with our theory in Panel A of Table Q2, which presents the distribution of amounts in the poverty-decreasing sample. When the mayor is aligned, 69% of mayors are less corrupt than the median, whereas 31% of mayors are more corrupt than the median. For unaligned mayors the pattern again flips: 56% of mayors are more corrupt than the median, and 44% of mayors are less corrupt than the median. Overall, the combination of poverty and alignment contributes to differential municipal-level corruption patterns. Appendix Q provides even more tables and relevant analysis.

### 4.3. Close Elections as a Mechanism to Temper Rent-Seeking from Aligned Politicians

Proposition 1 in our model suggests that aligned politicians engage in less rent-seeking than their unaligned counterparts as their margin of victory in the most recent election approaches zero. In Section 3, we found causal evidence consistent with Proposition 1 using a series of regression discontinuity designs.

In this subsection, we subject Proposition 1 to further scrutiny by conducting placebo tests that examine corruption activity of aligned and unaligned politicians away from the cutoff. In our regression discontinuity models, Calonico, Cattaneo and Titiunik’s (2014) algorithm usually resulted in data-driven bandwidths for  $MV$  at around 10% on either side of the cutoff. Accordingly, in this section we analyze the data in which  $MV > 10\%$  or  $MV < -10\%$ . Although the analyses in this subsection cannot facilitate the same type of causal interpretation as our earlier regression discontinuity analyses, the analyses show some useful correlations. More specifically, these correlations allow us to discern whether the same patterns generally hold away from cutoff. For our argument to find empirical support, the data away from the cutoff should not exhibit the same pattern as those in earlier sections.

Table 3 and the additional tables in Appendix T present the main results from the analysis of infractions outside the close election bandwidth. Under myriad negative binomial and poisson model specifications, alignment does not correlate with the number of infractions committed in less competitive elections. Results are similar when analyzing the log amount of misappropriated money in Appendix T as well. The effect of alignment is only statistically distinguishable from zero when controlling for poverty reduction without municipal fixed effects. After adding the municipal fixed effects and control variables, the effect of alignment quickly becomes null. In short, the placebo tests we have conducted here do not show a causal relationship, but they provide support for the existence of a close election mechanism, which Propositions 1 and 2 buttress.

Table 3: Infractions: How Close Elections Matter (2010-2015)

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	-0.065 (0.045)	-0.061 (0.048)	-0.073 (0.048)	0.028 (0.056)	0.039 (0.065)	0.014 (0.065)
Poverty Reduction		-0.019 (0.049)	-0.017 (0.049)			
Log Population					2.813*** (0.500)	-1.017 (0.998)
Reelected Mayor					0.064 (0.066)	0.065 (0.064)
Observations	1260	1125	1125	1260	1178	1178
Municipality FE	No	No	No	Yes	Yes	Yes
Year FE	No	No	Yes	No	No	Yes

Note: Poisson regression models; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Standard errors clustered by municipality in parentheses.

## 5. Conclusion

In a recent review, eminent corruption scholars [Golden and Mahdavi \(2015, 414\)](#) suggest that “[t]o understand variations in the frequency of bureaucratic corruption requires a theory of electoral incentives governing strategies of bureaucratic slippage, something that is a long way off.” By showing how economic development and political institutions interact with electoral incentives to produce different levels of bureaucratic corruption, we demonstrated that such a theory is no longer “a long way off”. More specifically, our above analysis shows that the combination of low or decreasing poverty and electoral competition reduces corruption from aligned politicians. These findings echo [Weitz-Shapiro’s \(2012, 2014\)](#) work about why politicians opt-out of clientelism in Argentina, but note that our work on the distinct phenomenon of corruption applies to aligned politicians.

We find causal support for our theory using regression discontinuity designs that measure corruption both through audit infractions and the (log) amounts of money associated with those infractions. Of these two dependent variables, results are marginally stronger for

infractions than log amounts. That pattern is likely due to the greater electoral risk associated with stealing large amounts of money vis-à-vis committing lots of small infractions that are less visible to voters. Our results are similar, albeit somewhat weaker, when party alignment dovetails with significant electoral competition and extreme poverty reduction. From a measurement perspective, our paper undertakes numerous checks that scholars can follow to credibly analyze corruption outside a context with randomized audits like Brazil, which has heretofore served as the main country examined in the literature.<sup>27</sup>

For our above theory to hold, it is necessary to have some form of party system stability. When voters succumb to the appeal of populist outsiders who claim to be able to “fix” the corrupt system, it often leads to even more corruption and the gradual death of democracy (e.g., [Levitsky and Ziblatt, 2018](#)). Our analysis adds to this literature, showing that party system instability fuels local-level corruption by eliminating or decreasing alignment relationships that foster clarity of responsibility. Local-level politicians, in turn, take advantage of these institutional circumstances to oversee more corrupt governments at the local level.

When there is some form of party system stability and party alignment relationships, however, it is possible for modernization forces to decrease corruption through alignment. The focus on alignment is critical because aligned politicians are most likely to enjoy significant resource advantages, use these advantages to gain an electoral advantage over opposition parties, and hurt the quality of democracy in the process. That is true for both developed and developing countries. Thus, from an external validity perspective, this study’s results should hold when using similar *objective* corruption measures to examine poorer countries with democratic institutions and stable party systems. The data also need to include time periods before elections, which discipline corrupt behavior from politicians.

Poverty is not endogenous to corruption in our models (see Appendix [M](#)), but the subgroup analyses that we performed throughout the paper only allowed us to make causal inferences about each subgroup independently. Regression discontinuity designs cannot in-

<sup>27</sup> See, for example, [Ferraz and Finan \(2008, 2011\)](#), [Brollo et al. \(2013\)](#), [Avis, Ferraz and Finan \(2018\)](#), [Cavalcanti, Daniele and Galletta \(2018\)](#), and [Zamboni and Litschig \(2018\)](#).

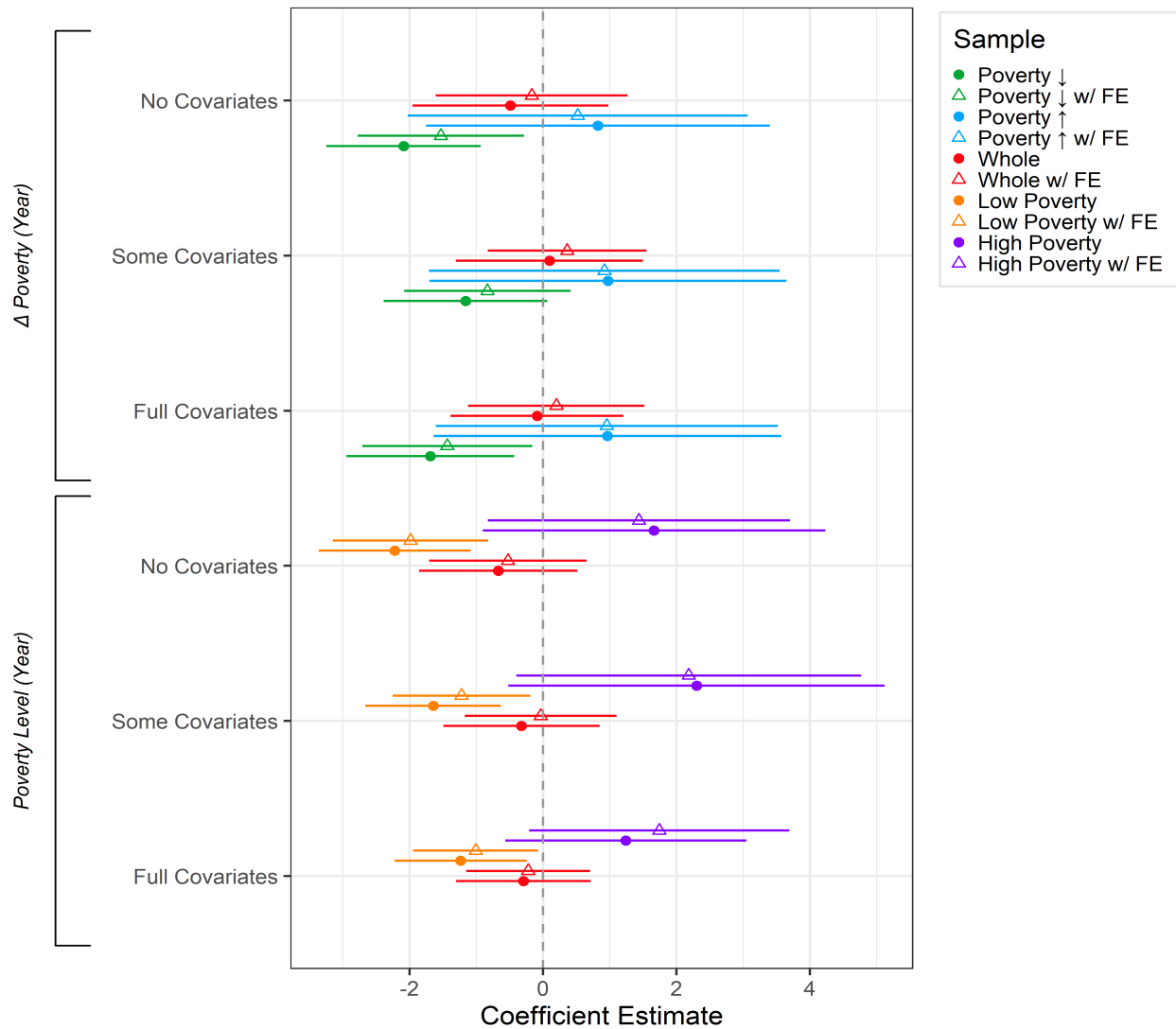
corporate interactions, and Guatemala (and most developing countries) only measure local-level poverty intermittently, so subgroup analysis was our only means to test our hypothesis. Despite these limitations, our regression discontinuity analyses allow us to make causal inferences about each poverty subgroup independently. In turn, our overall results suggest that modernization and political-institutional forces combine to place subnational units within a polity on different starting points or corruption paths.



## Appendix

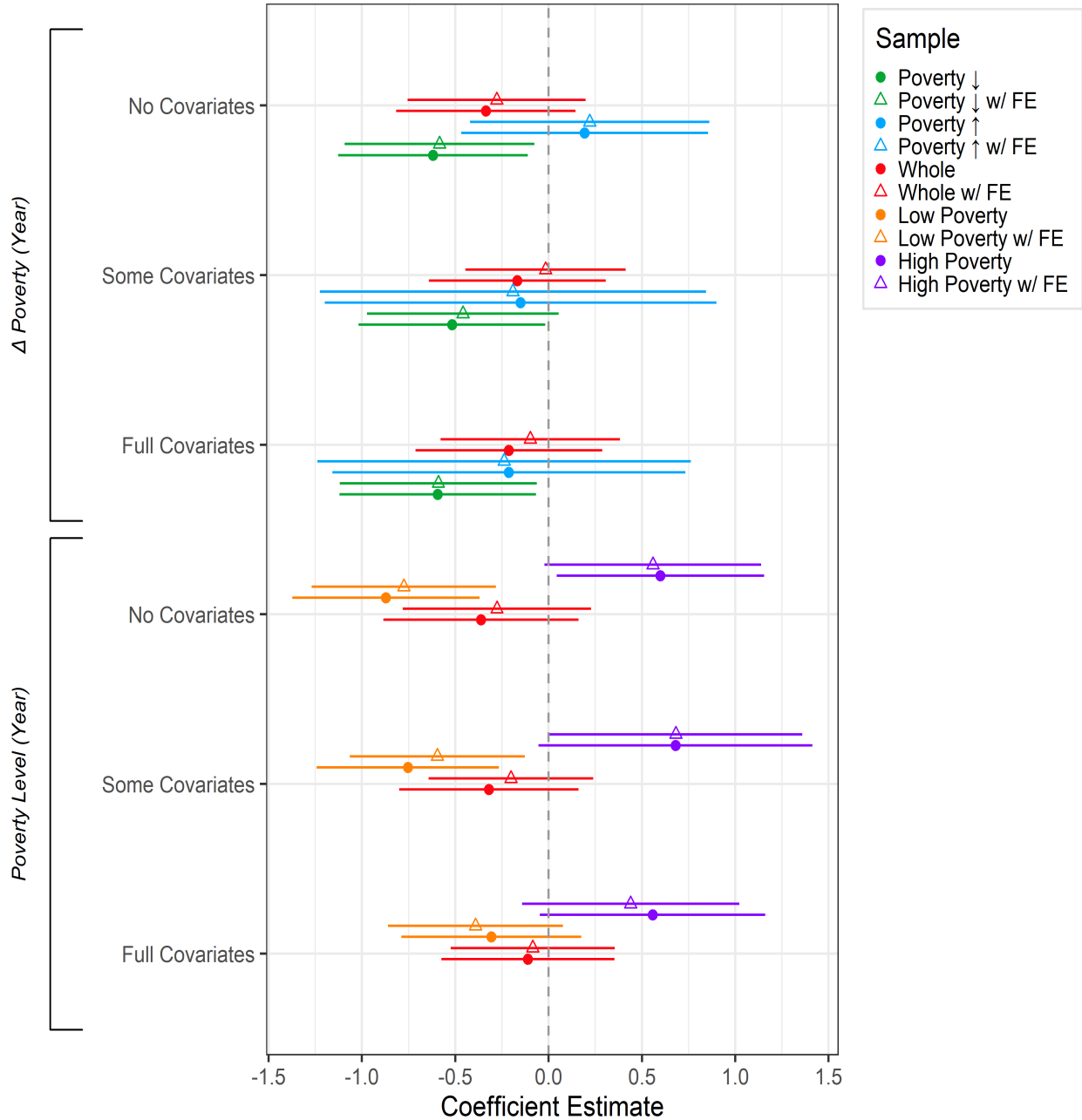
### A. Additional Coefficient Plots for Year-Wise Results

Figure A.1: Infraction Count by Year



Note: The above estimates are second-order polynomial fits in line with [Gelman and Imbens \(2019\)](#), with standard errors clustered by municipality and confidence intervals at the 90% level. “FE” corresponds to municipality fixed effects. “Some Covariates” refer to (log) population and a mayor re-election dummy variable. “Full covariates” refer to (log) population, a mayor re-election dummy, inequality (gini coefficient), and (log) public goods per capita. Full tables corresponding to the above Figure can be found in Appendices [E](#), [G](#), and [K](#).

Figure A.2: Log Infraction Amount by Year



Note: The above estimates are second-order polynomial fits in line with [Gelman and Imbens \(2019\)](#), with standard errors clustered by municipality and confidence intervals at the 90% level. “FE” corresponds to municipality fixed effects. “Some Covariates” refer to (log) population and a mayor re-election dummy variable. “Full covariates” refer to (log) population, a mayor re-election dummy, inequality (gini coefficient), and (log) public goods per capita. Full tables corresponding to the above Figure can be found in Appendices [E](#), [G](#), and [K](#).

## B. Descriptive Statistics

Table B1: Descriptive Statistics of Infraction Variables (Poverty Increasing/Decreasing Sample)

Panel A: Infractions (Year Viewpoint)		Increase		Increase		Decrease		Decrease	
VARIABLES		Unaligned	N	Aligned	N	Unaligned	N	Aligned	N
		Mean		Mean		Mean		Mean	
Number of Infractions: All Years		9.453	1,107	6.376	348	8.704	1,043	5.443	271
Log Amount of Infractions: All Years		11.53	1,106	11.40	347	11.50	1,041	11.20	270
Number of Infractions: First 2 years of Term		6	184	6.286	126	5.985	194	5.233	90
Log Amount of Infractions: First 2 years of Term		11.21	183	11.29	125	11.24	193	10.91	89
Number of Infractions: Last 2 years of Term		6.071	395	6.428	222	6.438	384	5.547	181
Log Amount of Infractions: Last 2 years of Term		11.53	395	11.47	222	11.56	383	11.34	181
Number of Infractions: Last year of Term		6.894	198	7.387	111	7.373	193	6.154	91
Log Amount of Infractions: Last year of Term		11.83	198	11.89	111	11.84	192	11.61	91

Panel B: Infractions (Electoral Term)		Increase		Increase		Decrease		Decrease	
VARIABLES		Unaligned	N	Aligned	N	Unaligned	N	Aligned	N
		Mean		Mean		Mean		Mean	
Number of Infractions: All Years		29.56	354	19.99	111	27.10	335	16.21	91
Log Amount of Infractions: All Years		13.14	354	12.87	111	13.12	335	12.48	91
f Number of Infractions: First 2 years of Term		12	92	12.77	62	12.09	96	10.47	45
Log Amount of Infractions: First 2 years of Term		12.08	92	12.27	62	12.21	96	11.72	45
Number of Infractions: Last 2 years of Term		12.05	199	12.86	111	12.88	192	11.03	91
Log Amount of Infractions: Last 2 years of Term		12.39	199	12.43	111	12.47	192	12.20	91
Number of Infractions: Last year of Term		6.894	198	7.387	111	7.411	192	6.154	91
Log Amount of Infractions: Last year of Term		11.83	198	11.89	111	11.84	192	11.61	91

Note: Panel A shows results by years, while the Panel B shows results by electoral term. “Decrease” refers to the sample of municipalities where poverty had decreased between 2002 and 2011, while “Increase” refers to the sample where poverty increased between 2002 and 2011. All amounts are expressed in real terms and are deflated by the respective yearly GDP deflator.

Table B2: Descriptive Statistics of Infraction Variables (Poverty High/Low Sample)

Panel A: Infractions (Year Viewpoint)		High		High		Low		Low	
		Unaligned	N	Aligned	N	Unaligned	N	Aligned	N
VARIABLES		Mean		Mean		Mean		Mean	
Number of Infractions: All Years		6.854	1,393	5.574	432	8.139	1,265	5.638	475
Log Amount of Infractions: All Years		11.32	1,390	11.34	431	11.62	1,265	11.34	474
Number of Infractions: First 2 years of Term		5.438	416	5.441	204	6.063	365	5.513	197
Log Amount of Infractions: First 2 years of Term		11.23	414	11.24	203	11.38	365	11.18	196
Number of Infractions: Last 2 years of Term		5.882	407	5.944	198	6.656	372	6.117	205
Log Amount of Infractions: Last 2 years of Term		11.50	406	11.34	198	11.59	372	11.48	205
Number of Infractions: Last year of Term		6.754	199	6.698	96	7.521	192	6.953	106
Log Amount of Infractions: Last year of Term		11.83	198	11.76	96	11.83	192	11.77	106
Panel B: Infractions (Electoral Term)		High		High		Low		Low	
		Unaligned	N	Aligned	N	Unaligned	N	Aligned	N
VARIABLES		Mean		Mean		Mean		Mean	
Number of Infractions: All Years		24.60	468	20.80	118	28.86	409	19.35	136
Log Amount of Infractions: All Years		13.13	468	13.14	118	13.37	409	12.94	136
Number of Infractions: First 2 years of Term		10.88	208	10.88	102	12.09	183	10.97	99
Log Amount of Infractions: First 2 years of Term		12.16	208	12.25	102	12.32	183	12.06	99
Number of Infractions: Last 2 years of Term		11.76	208	11.87	103	13.24	183	12.20	99
Log Amount of Infractions: Last 2 years of Term		12.40	208	12.35	103	12.46	183	12.30	99
Number of Infractions: Last year of Term		6.716	208	6.689	103	7.643	182	6.980	99
Log Amount of Infractions: Last year of Term		11.80	208	11.77	103	11.87	182	11.76	99

Note: Panel A shows results by years, while the Panel B shows results by electoral term. “Decrease” refers to the sample of municipalities where poverty had decreased between 2002 and 2011, while “Increase” refers to the sample where poverty increased between 2002 and 2011. All amounts are expressed in real terms and are deflated by the respective yearly GDP deflator.

Table B3: Descriptive Statistics of Covariates (Poverty Increasing/Decreasing Sample)

Panel A: Year Viewpoint		Increase		Increase		Decrease		Decrease	
		Unaligned		Aligned		Unaligned		Aligned	
VARIABLES		Mean	N	Mean	N	Mean	N	Mean	N
Percentage of Mayor Reelected		0.305	1,160	0.217	332	0.326	1,110	0.0945	254
Extreme Poverty Rate		25.11	1,202	25.35	348	16.32	1,148	15.53	272
Gini coefficient		24.95	1,202	25.29	348	24.99	1,148	23.94	272
Total Poverty Rate		72.70	1,202	70.96	348	66.05	1,148	65.09	272
Log Population		10.29	1,202	10.22	348	10.34	1,148	10.12	272
Log Public Goods Spending (per capita)		6.428	582	6.144	348	6.148	580	6.382	272

Panel B: Electoral Term		Increase		Increase		Decrease		Decrease	
		Unaligned		Aligned		Unaligned		Aligned	
VARIABLES		mean	N	mean	N	mean	N	mean	N
Percentage of Mayor Reelected		0.306	333	0.214	103	0.320	316	0.122	82
Extreme Poverty Rate		26.13	354	27.91	111	19.13	335	19.83	91
Gini coefficient		25.56	354	26.17	111	25.56	335	25.26	91
Total Poverty Rate		73.87	354	73.37	111	68.44	335	68.84	91
Log Population		10.27	354	10.23	111	10.34	335	10.10	91
Log Public Goods Spending (per capita)		7.308	199	7.186	111	6.983	193	7.260	91

Note: Panel A shows results by years, while the Panel B shows results by term. “Decrease” refers to the sample of municipalities where poverty decreased between 2002 and 2011, while “Increase” refers to the sample where poverty increased between 2002 and 2011. Public Goods Spending amount is expressed in real terms and deflated by the respective yearly GDP deflator.

Table B4: Descriptive Statistics of Covariates (Poverty High/Low Sample)

Panel A: Year Viewpoint	High Unaligned Mean	N	High Aligned Mean	N	Low Unaligned Mean	N	Low Aligned Mean	N
VARIABLES								
Percentage of Mayor Reelected	0.270	1,387	0.159	390	0.365	1,286	0.231	450
Extreme Poverty Rate	30.22	1,493	31.36	435	10.73	1,342	10.62	477
Gini coefficient	22.17	1,493	21.43	435	25.04	1,342	25.15	477
Total Poverty Rate	82.25	1,493	82.72	435	54.01	1,342	53.16	477
Log Population	10.31	1,493	10.15	435	10.21	1,342	10.14	477
Log Public Goods Spending (per capita)	6.009	729	5.958	355	6.465	650	6.494	345

---

Panel B: Electoral Term	High Unaligned mean	N	High Aligned mean	N	Low Unaligned mean	N	Low Aligned mean	N
VARIABLES								
Percentage of Mayor Reelected	0.275	440	0.179	106	0.359	396	0.248	129
Extreme Poverty Rate	31.61	468	33.66	118	12.09	409	12.54	136
Gini coefficient	22.92	468	23.42	118	25.83	409	26.48	136
Total Poverty Rate	82.84	468	83.78	118	56.51	409	55.45	136
Log Population	10.31	468	10.15	118	10.20	409	10.12	136
Log Public Goods Spending (per capita)	7.359	209	7.462	103	7.775	183	7.791	99

Note: Panel A shows results by years, while the Panel B shows results by term. “Decrease” refers to the sample of municipalities where poverty decreased between 2002 and 2011, while “Increase” refers to the sample where poverty increased between 2002 and 2011. Public Goods Spending amount is expressed in real terms and deflated by the respective yearly GDP deflator.

## C. Theoretical Derivation

**Proposition 1.** *Optimal rent levels for aligned politicians are less than rents levels for unaligned politicians at  $MV = 0$  when the electorate's economic circumstances are good or have improved.*

*Proof.* We solve for the following problem for the local-level politician in as in Equation (5):

$$\begin{aligned} \max_{r_{i,1}} & U(r_{i,1}) + \pi(s_i)U(r_{i,2}) + [1 - \pi(s_i)]U(x_{i,2}) \\ \text{where } s_i &= W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a - 1)t(MV) \end{aligned} \quad (8)$$

Accordingly, we can rewrite the maximization problem as follows:

$$\begin{aligned} \max_{r_{i,1}} & U(r_{i,1}) + \pi(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a - 1)t(MV))U(r_{i,2}) \\ & + [1 - \pi(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a - 1)t(MV))]U(x_{i,2}) \end{aligned} \quad (9)$$

The corresponding First-Order Condition (F.O.C.) for Equation (9) is:

$$\begin{aligned} 0 = & U'(r_{i,1}) + U(r_{i,2})\pi'(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a - 1)t(MV))[-W'(1 - r_{i,1}) \\ & + \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] - U(x_{i,2})\pi'(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) \\ & + (2a - 1)t(MV))[-W'(1 - r_{i,1}) + \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] \end{aligned} \quad (10)$$

Collecting like terms and bringing them to the other side, Equation (10) can be rewritten as:

$$\begin{aligned} U'(r_{i,1}) = & [U(r_{i,2}) - U(x_{i,2})]\pi'(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a - 1)t(MV))[W'(1 - r_{i,1}) - \\ & \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] \end{aligned} \quad (11)$$

Now from the assumption on  $t(\cdot)$ , we know that as  $MV \rightarrow 0$ ,  $t(MV) \rightarrow 0$  since  $t(\cdot)$  increases

with respect to  $MV$ . Thus, as  $MV \rightarrow 0$ , Equation (11) can be written as:

$$U'(r_{i,1}) = [U(r_{i,2}) - U(x_{i,2})]\pi'(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}))[W'(1 - r_{i,1}) - \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] \quad (12)$$

The F.O.C. for aligned municipalities ( $a = 1$ ) is then:

$$U'(\overline{r}_{i,1}) = [U(r_{i,2}) - U(x_{i,2})]\pi'(W(1 - \overline{r}_{i,1}) + \beta_i^2W(\gamma\overline{r}_{i,1}))[W'(1 - \overline{r}_{i,1}) - \gamma\beta_i^2W'(\gamma\overline{r}_{i,1})] \quad (13)$$

and the F.O.C. for unaligned municipalities ( $a = 0$ ) is:

$$U'(\underline{r}_{i,1}) = [U(r_{i,2}) - U(x_{i,2})]\pi'(W(1 - \underline{r}_{i,1}) + \beta_i W(\gamma\underline{r}_{i,1}))[W'(1 - \underline{r}_{i,1}) - \gamma\beta_i W'(\gamma\underline{r}_{i,1})] \quad (14)$$

where  $\overline{r}_{i,1}$  and  $\underline{r}_{i,1}$  are the optimal rent for the aligned and unaligned mayors, respectively.

Accordingly, it follows that  $\overline{r}_{i,1} = r_{i,1} * -z < r_{i,1} * < r_{i,1} * +k = \underline{r}_{i,1}$  where  $z, k > 0$ .<sup>28</sup>

**Corollary 1:** *Optimal rents extraction levels for aligned and unaligned politicians do not differ at  $MV = 0$  if economic circumstances are poor or worsen.*

This proof follows from replacing  $\beta_i = 1$  in Equation (11) to show that both the aligned and unaligned cases result in the same First-Order Equation. ■ □

**Proposition 2.** *Optimal rent levels for aligned politicians increase with respect to  $MV$ , while they decrease with respect to  $MV$  for the unaligned politicians.*

*Proof.* The proof of this Proposition follows a similar structure as Brollo and Nannicini (2012, Proof of Proposition 2). Per Equation (10), we define the first-order condition as  $g(r_{i,1}, MV) = 0$ , so by implicit differentiation  $\partial r_{i,1} / \partial MV = -(\partial g / \partial MV) / (\partial g / \partial r_{i,1})$ , where

<sup>28</sup>The result follows from similar structural implications as derived in Brollo and Nannicini (2012, Proof of Proposition 1).



$\partial g/\partial r_{i,1} < 0$  due to the maximisation second order condition. By extension, therefore:

$$\begin{aligned} \partial g/\partial MV = & [U(r_{i,2}) - U(x_{i,2})]\pi'_{MV}(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + \\ & (2a - 1)t(MV))[W'(1 - r_{i,1}) - \gamma\beta_i^{1+a}W'(\gamma r_{i,1})][(2a - 1)t'(MV)] \end{aligned} \quad (15)$$

When  $a = 1$ :

$$\begin{aligned} \partial g/\partial MV = & [U(r_{i,2}) - U(x_{i,2})]\pi'_{MV}(W(1 - r_{i,1}) + \beta_i^2W(\gamma r_{i,1}) + \\ & t(MV))[W'(1 - r_{i,1}) - \gamma\beta_i^2W'(\gamma r_{i,1})]t'(MV) > 0 \end{aligned} \quad (16)$$

Therefore,  $-(\partial g/\partial MV)/(\partial g/\partial r_{i,1}) > 0$  when  $a = 1$ , or  $\partial r_{i,1}/\partial MV > 0$  when  $a = 1$ .

When  $a = 0$ :

$$\begin{aligned} \partial g/\partial MV = & -[U(r_{i,2}) - U(x_{i,2})]\pi'_{MV}(W(1 - r_{i,1}) + \beta_i W(\gamma r_{i,1}) \\ & - t(MV))[W'(1 - r_{i,1}) - \gamma\beta_i W'(\gamma r_{i,1})]t'(MV) < 0 \end{aligned} \quad (17)$$

Therefore,  $\partial r_{i,1}/\partial MV < 0$  when  $a = 0$ . ■

□

## D. Party Alignment's Effects on Clarity of Responsibility and Citizen Satisfaction

Party alignment signals clarity of responsibility for corruption: when local-level and national politicians share the same party, it makes it easier for voters to discern which political party is responsible for corruption. By contrast, under divided government, voters cannot make such snap judgments as easily (Schwindt-Bayer and Tavits, 2016). Consistent with how we represent  $t(\cdot)$  in Equation (4), we make two related arguments to underscore why alignment's effects are conditional on  $MV$ . First, citizens' levels of satisfaction with a local-level politician depend on the quality of political information available. Second, the latter is also at least partly a function of the joint effects of a local-level politician's margin of victory in the last election and party alignment status.

Figure 1 graphically depicts our argument on the information-related satisfaction benefits that citizens derive from clarity of responsibility, denoted by  $(2a - 1)t(MV)$  in Equation (4). Regions 3 and 4 correspond to the positive effects of clarity of responsibility, which the  $(2a - 1)$  term helps capture.<sup>29</sup> In Region 4, where the local-level politician won by a large margin and is aligned, citizens gain satisfaction from knowing that the benefits they received are attributable to one party, which makes understanding and engaging in politics easier. Citizens also derive some satisfaction from information clarity in Region 3, where the politician is still aligned but won by a smaller margin of victory. Nevertheless, the smaller margin of victory indicates that Region 3 is likely more winnable in the next election, which draws more attention from opposition party campaigns in the lead-up to the next election. In turn, the political information environment becomes less clear to citizens in Region 3 than in Region 4, and information clarity likely drops even more precipitously as  $MV \rightarrow 0$ —hence the shape of  $(2a - 1)t(MV)$  in Figure 1. Again, electoral competition is the primary driver of these information flows and intensifies with smaller margins of victory for the incumbent.

<sup>29</sup> When the politicians is aligned ( $a = 1$ ), then  $2(1) - 1 * MV$  must be positive. When the politician is unaligned ( $a = 0$ ), then  $(2(0) - 1) * MV$  must be negative.

When politicians are unaligned, as in Regions 1 and 2, returns to citizen-level satisfaction follow the reverse pattern. More specifically, citizens start to derive negative returns to information in Region 2, where the politician is unaligned and only won the last election by a small margin. The reason is that Region 2 is likely to attract very significant attention from the ruling party at the national level. Given that control of the bureaucracy tends to grant these parties with significant resource advantages over unaligned parties (Greene, 2010; Brollo and Nannicini, 2012; Corvalan, Cox and Osorio, 2018; Lara and Toro, 2019), the aligned party can overwhelm voters with information. At the same time, the unaligned party has an incentive to keep its position, creating a situation of information overload for citizens. The same information overload is unlikely to occur in Region 1, where the local-level politician is unaligned and won by a large margin of victory. Instead, citizens in Region 1 likely do not receive enough high-quality information about the political process, yielding lower levels of citizen satisfaction.<sup>30</sup> Both the national ruling party and other opposition parties have lower incentives to invest in electoral competition, so citizens cannot clearly discern who is responsible for their current situations in Region 1. While such concerns may not be salient when welfare is high, opposition politicians are at disadvantage given their lower levels of access to the spoils of the bureaucracy. Consequently, accurate evaluation of political candidates is most difficult for citizens in Region 1.

---

<sup>30</sup>This relative lack of clarity and information leads to higher dissatisfaction in Region 1 than the information-overload encountered in Region 2

## E. When Poverty is Decreasing/Low

### E.1. When Poverty Decreases

Table E1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.649*** (0.549)	-1.985*** (0.698)	-0.802 (0.560)	-1.025 (0.731)	-0.938* (0.564)	-1.587** (0.761)
Observations	601	601	569	569	569	569
Effective Observations	[192,138]	[198,139]	[170,112]	[174,128]	[170,112]	[154,104]
Covariates	None	None	Some	Some	All	All
p-value	0.00266	0.00448	0.152	0.161	0.0963	0.0371
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.105	0.0943	0.0982	0.0936	0.0851
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.219** (0.582)	-1.453* (0.761)	-0.522 (0.591)	-0.727 (0.759)	-0.712 (0.608)	-1.365* (0.779)
Observations	601	601	569	569	569	569
Effective Observations	[182,138]	[198,139]	[170,112]	[174,128]	[166,112]	[154,104]
Covariates	None	None	Some	Some	All	All
p-value	0.0361	0.0563	0.378	0.338	0.242	0.0797
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0992	0.110	0.0912	0.0991	0.0905	0.0869

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table E2: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-10.98*** (2.996)	-13.73*** (4.113)	-7.952** (3.358)	-9.853** (4.261)	-7.339** (3.245)	-8.692** (4.052)
Observations	195	195	179	179	179	179
Effective Observations	[56,45]	[62,49]	[46,35]	[58,45]	[45,34]	[57,44]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.000247	0.000845	0.0179	0.0208	0.0237	0.0320
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0958	0.106	0.0819	0.112	0.0772	0.107
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-3.805* (1.982)	-5.131* (2.717)	-1.614 (2.118)	-2.859 (2.820)	-2.344 (2.252)	-4.842 (2.976)
Observations	195	195	179	179	179	179
Effective Observations	[57,47]	[62,49]	[48,36]	[53,43]	[45,34]	[47,35]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0548	0.0590	0.446	0.311	0.298	0.104
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0967	0.106	0.0872	0.0987	0.0796	0.0866

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table E3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.773*** (0.216)	-0.627** (0.311)	-0.564** (0.256)	-0.515* (0.304)	-0.660** (0.264)	-0.598* (0.320)
Observations	598	598	566	566	566	566
Effective Observations	[206,147]	[182,136]	[144,98]	[170,112]	[146,102]	[188,129]
Covariates	None	None	Some	Some	All	All
p-value	0.000354	0.0436	0.0274	0.0908	0.0123	0.0619
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.120	0.0971	0.0730	0.0939	0.0777	0.109
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.731*** (0.213)	-0.587* (0.310)	-0.488* (0.263)	-0.454 (0.312)	-0.574** (0.267)	-0.588* (0.322)
Observations	598	598	566	566	566	566
Effective Observations	[208,151]	[182,136]	[140,86]	[170,118]	[144,98]	[170,118]
Covariates	None	None	Some	Some	All	All
p-value	0.000613	0.0581	0.0633	0.146	0.0315	0.0676
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.123	0.0972	0.0707	0.0947	0.0732	0.0952

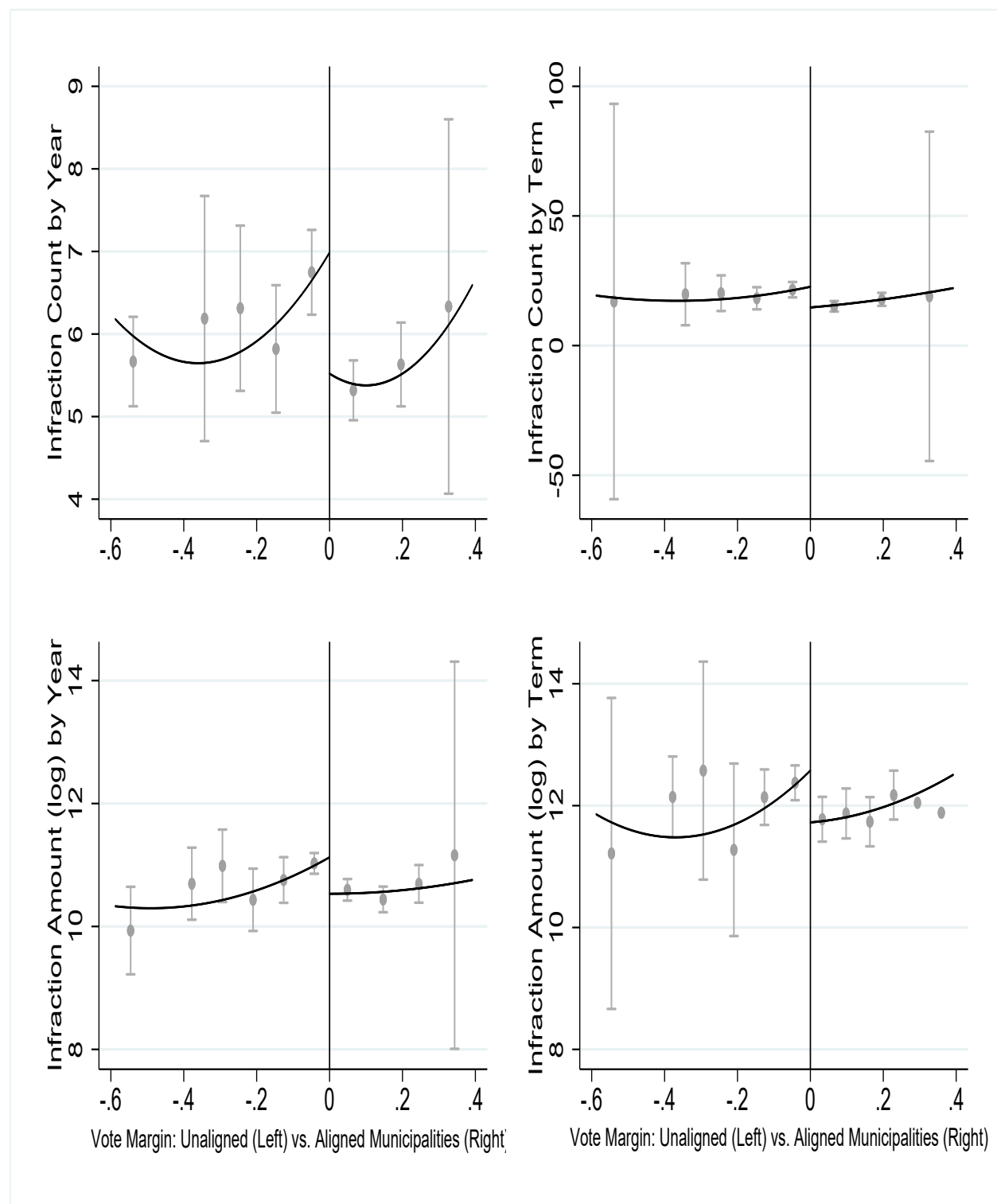
Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table E4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.240*** (0.427)	-1.197** (0.544)	-1.080*** (0.389)	-1.024** (0.508)	-1.016*** (0.371)	-1.053** (0.466)
Observations	195	195	179	179	179	179
Effective Observations	[48,37]	[56,43]	[45,34]	[51,38]	[47,35]	[51,38]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00368	0.0280	0.00547	0.0437	0.00615	0.0239
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0739	0.0942	0.0792	0.0903	0.0865	0.0908
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.819** (0.369)	-0.762 (0.470)	-0.645* (0.381)	-0.564 (0.483)	-0.730* (0.374)	-0.697 (0.485)
Observations	195	195	179	179	179	179
Effective Observations	[49,39]	[57,47]	[45,34]	[52,40]	[47,35]	[53,43]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0267	0.105	0.0904	0.243	0.0508	0.151
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0795	0.0971	0.0764	0.0955	0.0852	0.0985

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Figure E.1: RDD plots for Infraction Count and Amount (Poverty Reduction)





## E.2. When Poverty is Low

Table E5: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.991*** (0.616)	-2.225*** (0.692)	-1.588*** (0.541)	-1.646*** (0.618)	-1.158** (0.494)	-1.235** (0.603)
Observations	970	970	906	906	647	647
Effective Observations	[248,229]	[343,318]	[252,225]	[321,318]	[197,175]	[231,229]
Covariates	None	None	Some	Some	All	All
p-value	0.00122	0.00131	0.00331	0.00773	0.0192	0.0407
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0822	0.143	0.0923	0.156	0.104	0.152
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.792*** (0.619)	-1.988*** (0.707)	-1.289** (0.545)	-1.223* (0.626)	-1.043** (0.487)	-1.011* (0.569)
Observations	970	970	906	906	647	647
Effective Observations	[254,229]	[343,318]	[256,225]	[324,330]	[203,175]	[243,266]
Covariates	None	None	Some	Some	All	All
p-value	0.00378	0.00492	0.0180	0.0508	0.0323	0.0755
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0857	0.144	0.0948	0.163	0.110	0.179

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table E6: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-5.810** (2.826)	-6.086* (3.422)	-4.698* (2.517)	-3.821 (3.188)	-7.646*** (2.589)	-9.284*** (2.909)
Observations	284	284	267	267	192	192
Effective observations	[90,75]	[105,100]	[88,77]	[94,86]	[46,42]	[67,63]
Covariates	None	None	Some	Some	All	All
p-value	0.0398	0.0753	0.0620	0.231	0.00315	0.00142
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.111	0.171	0.120	0.141	0.0742	0.133
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-5.568** (2.393)	-6.486** (2.727)	-5.332*** (1.958)	-5.963*** (2.262)	-5.948** (2.398)	-6.227** (2.788)
Observations	284	284	267	267	192	192
Effective observations	[75,65]	[101,90]	[69,62]	[94,87]	[50,45]	[68,63]
Covariates	None	None	Some	Some	All	All
p-value	0.0200	0.0174	0.00647	0.00837	0.0131	0.0255
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0849	0.141	0.0858	0.142	0.0847	0.137

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table E7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.735*** (0.265)	-0.872*** (0.305)	-0.632*** (0.244)	-0.754** (0.297)	-0.243 (0.244)	-0.307 (0.293)
Observations	969	969	905	905	646	646
Effective Observations	[231,212]	[318,273]	[221,211]	[295,267]	[187,165]	[228,217]
Covariates	None	None	Some	Some	All	All
p-value	0.00555	0.00430	0.00969	0.0112	0.319	0.295
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0726	0.117	0.0811	0.122	0.0942	0.140
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.667*** (0.246)	-0.776*** (0.300)	-0.554** (0.223)	-0.596** (0.285)	-0.334 (0.236)	-0.392 (0.285)
Observations	969	969	905	905	646	646
Effective Observations	[240,225]	[318,275]	[250,225]	[295,267]	[187,165]	[228,224]
Covariates	None	None	Some	Some	All	All
p-value	0.00672	0.00963	0.0131	0.0368	0.158	0.169
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0799	0.119	0.0913	0.122	0.0955	0.147

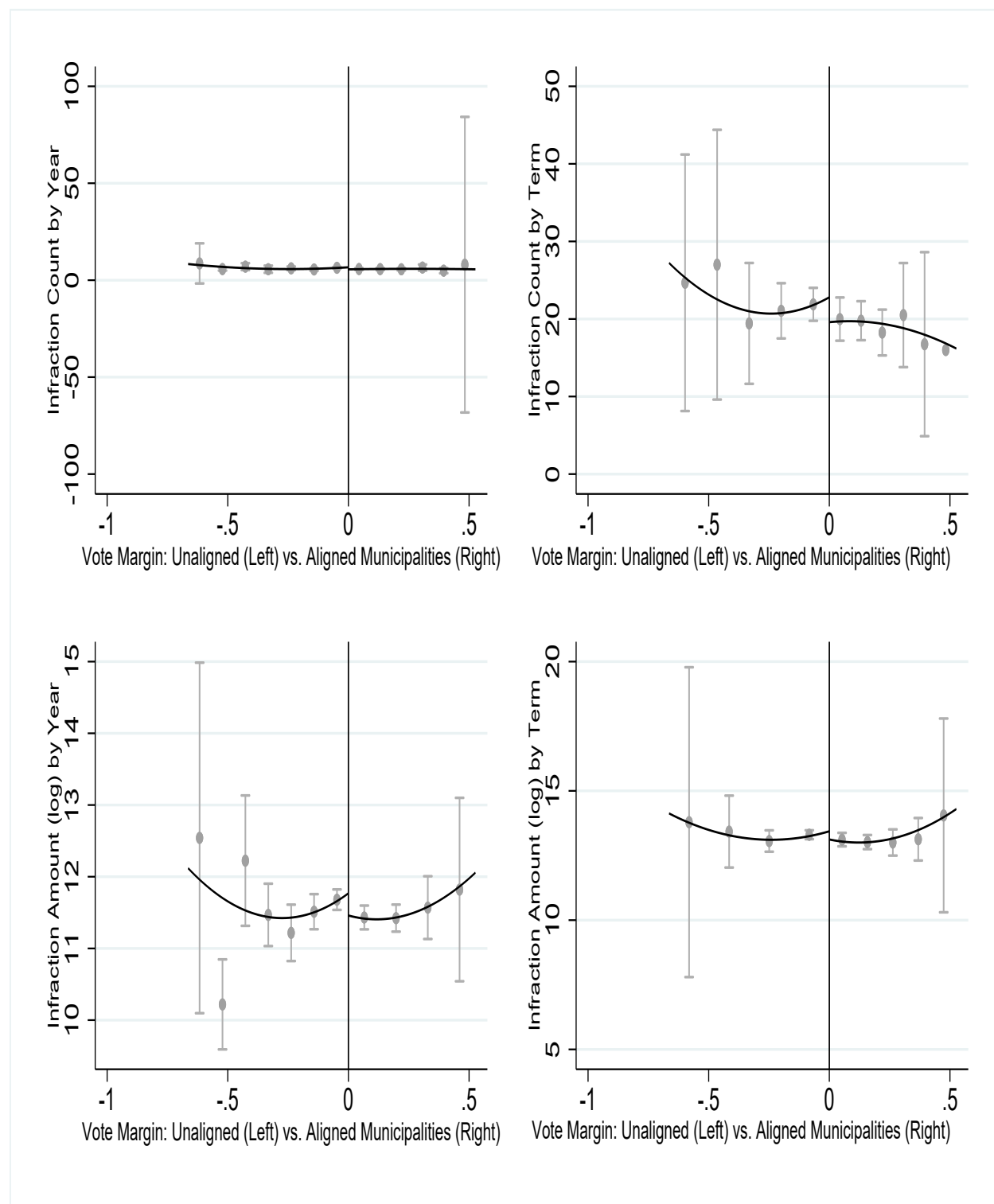
Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table E8: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.562* (0.299)	-0.665* (0.348)	-0.506* (0.264)	-0.557* (0.317)	-0.295 (0.291)	-0.437 (0.417)
Observations	284	284	267	267	192	192
Effective observations	[83,73]	[101,89]	[80,70]	[94,86]	[47,44]	[62,52]
Covariates	None	None	Some	Some	All	All
p-value	0.0600	0.0563	0.0552	0.0784	0.311	0.295
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.140	0.105	0.142	0.0776	0.113
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.592** (0.300)	-0.678* (0.353)	-0.596** (0.274)	-0.616* (0.334)	-0.344 (0.287)	-0.465 (0.418)
Observations	284	284	267	267	192	192
Effective observations	[80,68]	[95,84]	[71,63]	[88,77]	[47,44]	[62,52]
Covariates	None	None	Some	Some	All	All
p-value	0.0487	0.0551	0.0294	0.0652	0.231	0.266
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0914	0.125	0.0897	0.121	0.0787	0.114

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Figure E.2: RDD plots for Infraction Count and Amount (Low Poverty Sample)



## F. When Extreme Poverty is Low/Decreasing

### F.1. When Extreme Poverty Decreases

Table F1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.489*** (0.557)	-1.837** (0.724)	-1.152* (0.612)	-1.255 (0.763)	-1.278** (0.648)	-1.939** (0.806)
Observations	670	670	625	625	625	625
Effective Observations	[191,162]	[211,173]	[160,134]	[196,161]	[140,130]	[172,144]
Covariates	None	None	Some	Some	All	All
p-value	0.00746	0.0111	0.0597	0.100	0.0485	0.0162
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0959	0.104	0.0896	0.109	0.0792	0.0930
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.302** (0.585)	-1.604** (0.756)	-0.992 (0.632)	-1.060 (0.781)	-1.147* (0.666)	-1.524* (0.809)
Observations	670	670	625	625	625	625
Effective Observations	[191,156]	[213,173]	[152,134]	[200,165]	[140,130]	[188,160]
Covariates	None	None	Some	Some	All	All
p-value	0.0262	0.0338	0.117	0.175	0.0852	0.0595
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0942	0.107	0.0877	0.112	0.0785	0.101

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table F2: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-6.891** (2.959)	-7.099** (3.526)	-7.733*** (2.867)	-7.943** (3.342)	-9.355*** (3.010)	-10.34*** (3.838)
Observations	217	217	194	194	194	194
Effective Observations	[61,58]	[79,81]	[51,48]	[69,69]	[42,43]	[58,54]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0199	0.0441	0.00700	0.0175	0.00188	0.00708
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0976	0.155	0.0936	0.151	0.0785	0.109
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-3.655* (2.040)	-4.751* (2.713)	-3.573 (2.258)	-4.167 (2.827)	-4.639** (2.362)	-5.992** (2.914)
Observations	217	217	194	194	194	194
Effective Observations	[58,54]	[67,60]	[43,44]	[58,54]	[42,43]	[56,54]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0731	0.0799	0.114	0.141	0.0495	0.0398
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0908	0.105	0.0824	0.110	0.0767	0.103

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table F3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.454* (0.237)	-0.487 (0.308)	-0.315 (0.257)	-0.285 (0.317)	-0.363 (0.268)	-0.340 (0.335)
Observations	667	667	622	622	622	622
Effective Observations	[187,156]	[195,172]	[144,130]	[180,160]	[144,132]	[196,161]
Covariates	None	None	Some	Some	All	All
p-value	0.0555	0.113	0.221	0.368	0.174	0.310
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0934	0.0987	0.0811	0.0995	0.0835	0.108
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.443* (0.238)	-0.475 (0.308)	-0.294 (0.259)	-0.267 (0.321)	-0.321 (0.266)	-0.396 (0.327)
Observations	667	667	622	622	622	622
Effective Observations	[187,156]	[195,172]	[140,130]	[180,160]	[140,130]	[176,150]
Covariates	None	None	Some	Some	All	All
p-value	0.0627	0.123	0.256	0.406	0.226	0.226
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0928	0.0987	0.0805	0.0999	0.0788	0.0954

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

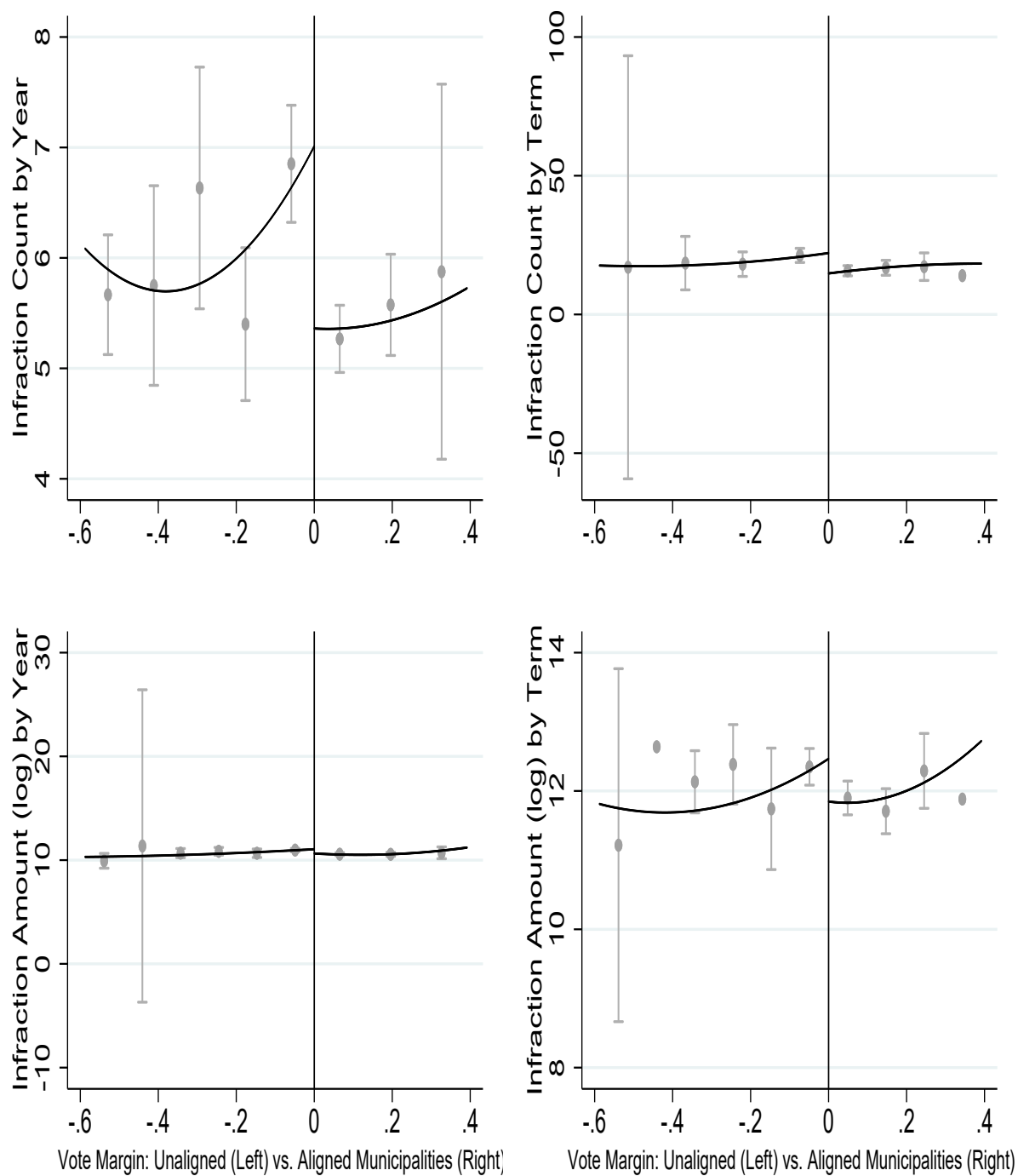


Table F4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.710* (0.397)	-0.764 (0.488)	-0.618* (0.354)	-0.695 (0.492)	-0.692** (0.333)	-0.934** (0.470)
Observations	217	217	194	194	194	194
Effective Observations	[49,46]	[60,56]	[51,48]	[51,48]	[53,53]	[43,44]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0736	0.118	0.0812	0.158	0.0378	0.0469
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0715	0.0953	0.0918	0.0925	0.0984	0.0844
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.550 (0.358)	-0.565 (0.429)	-0.367 (0.328)	-0.367 (0.465)	-0.509 (0.321)	-0.636 (0.463)
Observations	217	217	194	194	194	194
Effective Observations	[49,46]	[61,59]	[52,52]	[53,53]	[55,53]	[52,50]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.125	0.188	0.263	0.430	0.113	0.170
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0723	0.0991	0.0965	0.0992	0.101	0.0954

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Figure F.3: RDD plots for Infraction Count and Amount (Extreme Poverty Reduction)



## F.2. When Extreme Poverty is Low

Table F5: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.844*** (0.656)	-2.016*** (0.701)	-1.264** (0.564)	-1.328** (0.620)	-0.812 (0.583)	-0.885 (0.760)
Observations	954	954	886	886	634	634
Effective Observations	[224,219]	[340,343]	[216,206]	[315,326]	[148,148]	[199,170]
Covariates	None	None	Some	Some	All	All
p-value	0.00498	0.00402	0.0250	0.0324	0.164	0.245
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0785	0.159	0.0849	0.163	0.0779	0.114
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.798*** (0.651)	-1.914*** (0.702)	-1.049* (0.552)	-1.052* (0.623)	-0.732 (0.553)	-0.665 (0.819)
Observations	954	954	886	886	634	634
Effective Observations	[224,219]	[340,343]	[224,206]	[315,326]	[154,149]	[190,163]
Covariates	None	None	Some	Some	All	All
p-value	0.00571	0.00636	0.0573	0.0911	0.185	0.417
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0804	0.160	0.0875	0.162	0.0831	0.105

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table F6: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-3.968 (3.127)	-4.172 (3.617)	-2.367 (2.945)	-2.598 (3.622)	-7.362*** (2.815)	-9.000*** (3.172)
Observations	281	281	263	263	188	188
Effective observations	[85,69]	[105,104]	[80,68]	[93,90]	[41,41]	[66,61]
Covariates	None	None	Some	Some	All	All
p-value	0.204	0.249	0.422	0.473	0.00892	0.00456
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.106	0.179	0.111	0.157	0.0740	0.141
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-4.823* (2.541)	-5.305* (2.781)	-4.175** (2.073)	-4.469* (2.310)	-6.156** (2.476)	-7.063** (2.771)
Observations	281	281	263	263	188	188
Effective observations	[72,63]	[103,97]	[63,60]	[93,93]	[43,43]	[64,59]
Covariates	None	None	Some	Some	All	All
p-value	0.0577	0.0565	0.0440	0.0531	0.0129	0.0108
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0841	0.161	0.0820	0.160	0.0786	0.133

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table F7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.466* (0.270)	-0.632* (0.347)	-0.303 (0.247)	-0.388 (0.331)	-0.0730 (0.265)	-0.115 (0.283)
Observations	953	953	885	885	633	633
Effective Observations	[269,230]	[316,287]	[255,227]	[294,274]	[160,149]	[236,242]
Covariates	None	None	Some	Some	All	All
p-value	0.0843	0.0681	0.220	0.242	0.783	0.685
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0950	0.130	0.103	0.132	0.0854	0.171
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.441* (0.249)	-0.546* (0.323)	-0.171 (0.238)	-0.206 (0.292)	-0.0458 (0.261)	-0.0920 (0.273)
Observations	953	953	885	885	633	633
Effective Observations	[281,240]	[322,294]	[237,216]	[294,277]	[163,149]	[240,261]
Covariates	None	None	Some	Some	All	All
p-value	0.0766	0.0911	0.472	0.482	0.860	0.736
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.135	0.0914	0.134	0.0859	0.185

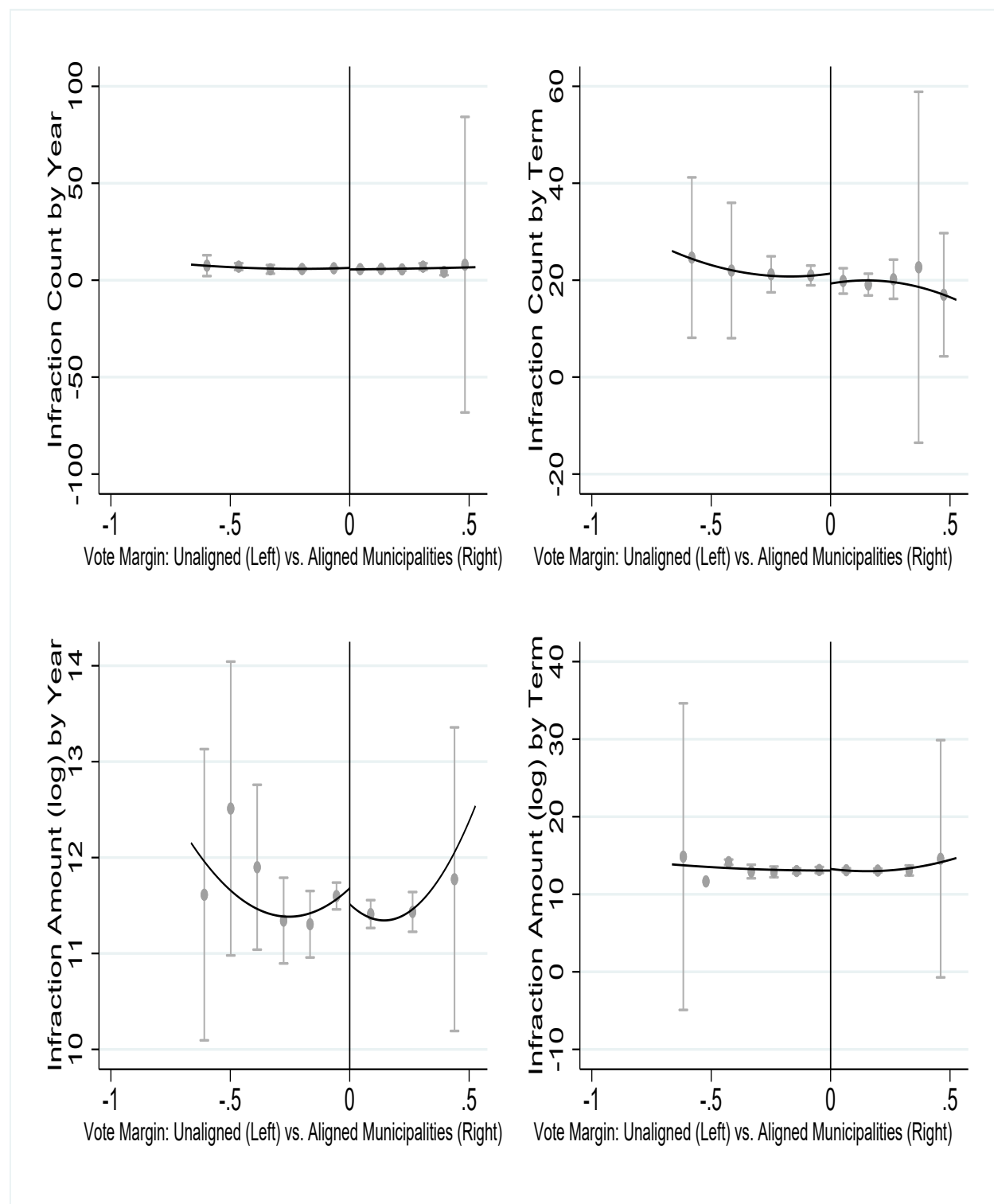
Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table F8: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.334 (0.701)	0.503 (0.848)	0.671 (0.801)	0.835 (0.932)	-0.208 (0.339)	-0.408 (0.439)
Observations	281	281	263	263	188	188
Effective observations	[90,72]	[105,104]	[80,67]	[98,107]	[38,39]	[56,47]
Covariates	None	None	Some	Some	All	All
p-value	0.634	0.553	0.403	0.370	0.539	0.353
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.113	0.177	0.109	0.191	0.0708	0.102
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.315 (0.694)	0.457 (0.830)	0.615 (0.809)	0.763 (0.923)	-0.275 (0.325)	-0.440 (0.440)
Observations	281	281	263	263	188	188
Effective observations	[89,71]	[105,105]	[78,66]	[98,107]	[41,41]	[56,47]
Covariates	None	None	Some	Some	All	All
p-value	0.650	0.582	0.447	0.409	0.398	0.318
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.112	0.181	0.105	0.190	0.0735	0.105

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Figure F.4: RDD plots for Infraction Count and Amount (Low Extreme Poverty)



## G. When Poverty is Increasing/High

### G.1. When Poverty Increases

Table G1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.570 (0.942)	0.824 (1.566)	0.519 (1.030)	0.975 (1.627)	0.547 (1.081)	0.970 (1.586)
Observations	605	605	562	562	562	562
Effective Observations	[159,198]	[159,234]	[130,176]	[138,222]	[130,168]	[138,228]
Covariates	None	None	Some	Some	All	All
p-value	0.545	0.599	0.614	0.549	0.613	0.541
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.118	0.133	0.101	0.131	0.0969	0.135
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.415 (0.983)	0.521 (1.549)	0.547 (1.090)	0.919 (1.599)	0.593 (1.089)	0.960 (1.560)
Observations	605	605	562	562	562	562
Effective Observations	[155,194]	[159,236]	[130,164]	[138,224]	[130,168]	[138,230]
Covariates	None	None	Some	Some	All	All
p-value	0.673	0.737	0.616	0.565	0.586	0.538
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.135	0.0955	0.131	0.0965	0.136

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table G2: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	4.224 (3.797)	6.547 (6.035)	1.230 (4.063)	3.371 (6.561)	-2.501 (4.456)	3.318 (8.434)
Observations	196	196	174	174	174	174
Effective Observations	[55,62]	[57,76]	[44,56]	[46,71]	[44,55]	[44,57]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.266	0.278	0.762	0.607	0.575	0.694
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.136	0.109	0.132	0.104	0.111
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	1.448 (3.180)	1.881 (4.584)	1.701 (3.540)	3.016 (5.265)	0.936 (3.735)	2.655 (5.101)
Observations	196	196	174	174	174	174
Effective Observations	[54,59]	[59,79]	[43,53]	[46,67]	[41,52]	[46,71]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.649	0.682	0.631	0.567	0.802	0.603
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.109	0.144	0.0958	0.128	0.0923	0.133

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table G3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.305 (0.302)	-0.0806 (0.466)	0.255 (0.315)	-0.146 (0.641)	0.265 (0.315)	-0.229 (0.596)
Observations	603	603	560	560	560	560
Effective Observations	[158,212]	[164,238]	[131,184]	[131,176]	[131,182]	[131,184]
Covariates	None	None	Some	Some	All	All
p-value	0.312	0.863	0.419	0.820	0.399	0.701
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.121	0.141	0.114	0.106	0.113	0.114
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.350 (0.285)	0.176 (0.401)	0.0991 (0.355)	-0.190 (0.632)	0.0759 (0.376)	-0.221 (0.618)
Observations	603	603	560	560	560	560
Effective Observations	[164,238]	[200,274]	[129,164]	[129,176]	[119,158]	[131,176]
Covariates	None	None	Some	Some	All	All
p-value	0.219	0.660	0.780	0.764	0.840	0.721
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.139	0.175	0.0944	0.102	0.0888	0.108

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table G4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.602 (0.371)	0.391 (0.548)	0.413 (0.398)	0.455 (0.478)	0.301 (0.389)	0.169 (0.595)
Observations	196	196	174	174	174	174
Effective Observations	[55,61]	[60,79]	[44,57]	[58,88]	[45,60]	[48,75]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.104	0.476	0.299	0.341	0.439	0.776
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.114	0.147	0.113	0.196	0.116	0.149
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.516 (0.329)	-0.129 (0.651)	0.555 (0.346)	0.502 (0.448)	0.600* (0.334)	0.530 (0.449)
Observations	196	196	174	174	174	174
Effective Observations	[57,74]	[57,63]	[46,69]	[58,88]	[46,73]	[58,86]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.117	0.843	0.109	0.262	0.0720	0.238
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.133	0.118	0.131	0.193	0.137	0.188

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## G.2. When Poverty is High

Table G5: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	1.089 (1.060)	1.664 (1.561)	1.486 (1.195)	2.301 (1.715)	0.829 (0.771)	1.241 (1.099)
Observations	906	906	804	804	607	607
Effective Observations	[220,210]	[276,276]	[178,179]	[227,236]	[153,165]	[186,207]
Covariates	None	None	Some	Some	All	All
p-value	0.305	0.287	0.214	0.180	0.282	0.259
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0892	0.123	0.0825	0.116	0.0956	0.134
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.996 (0.912)	1.438 (1.377)	1.389 (1.030)	2.185 (1.573)	0.605 (0.651)	1.742 (1.187)
Observations	906	906	804	804	607	607
Effective Observations	[226,218]	[276,276]	[182,183]	[223,228]	[166,184]	[163,177]
Covariates	None	None	Some	Some	All	All
p-value	0.275	0.296	0.178	0.165	0.352	0.142
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0927	0.123	0.0870	0.113	0.114	0.110

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table G6: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	5.120 (4.747)	8.026 (7.297)	7.291 (5.527)	10.90 (8.057)	7.115 (5.269)	13.48* (8.065)
Observations	258	258	230	230	181	181
Effective observations	[64,61]	[80,76]	[51,50]	[67,65]	[39,41]	[47,52]
Covariates	None	None	Some	Some	All	All
p-value	0.281	0.271	0.187	0.176	0.177	0.0946
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0924	0.122	0.0819	0.114	0.0795	0.105
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	4.130 (3.425)	5.662 (5.127)	6.039 (3.968)	8.339 (5.740)	4.323 (3.645)	10.78 (7.324)
Observations	258	258	230	230	181	181
Effective observations	[63,61]	[80,77]	[52,51]	[69,66]	[45,51]	[47,53]
Covariates	None	None	Some	Some	All	All
p-value	0.228	0.269	0.128	0.146	0.236	0.141
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0919	0.123	0.0855	0.119	0.102	0.109

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table G7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.500 (0.311)	0.599* (0.338)	0.543* (0.310)	0.680 (0.446)	0.413 (0.257)	0.557 (0.367)
Observations	902	902	800	800	603	603
Effective Observations	[219,210]	[337,369]	[188,187]	[222,232]	[149,151]	[162,173]
Covariates	None	None	Some	Some	All	All
p-value	0.108	0.0766	0.0798	0.128	0.109	0.129
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0901	0.173	0.0900	0.114	0.0908	0.106
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.501* (0.293)	0.559 (0.353)	0.548** (0.277)	0.682* (0.412)	0.345 (0.228)	0.440 (0.354)
Observations	902	902	800	800	603	603
Effective Observations	[219,210]	[302,309]	[181,187]	[216,220]	[162,177]	[159,173]
Covariates	None	None	Some	Some	All	All
p-value	0.0874	0.113	0.0481	0.0979	0.130	0.214
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0893	0.143	0.0883	0.107	0.110	0.104

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table G8: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	1.266* (0.726)	1.656* (0.900)	1.408* (0.800)	1.814* (0.950)	0.562 (0.408)	1.159** (0.586)
Observations	258	258	230	230	181	181
Effective observations	[68,66]	[84,80]	[57,58]	[74,72]	[47,53]	[47,54]
Covariates	None	None	Some	Some	All	All
p-value	0.0812	0.0657	0.0783	0.0561	0.168	0.0481
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.135	0.0970	0.135	0.109	0.112
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	1.203* (0.690)	1.532* (0.891)	1.365* (0.749)	1.705* (0.917)	0.511 (0.349)	1.023* (0.571)
Observations	258	258	230	230	181	181
Effective observations	[69,68]	[87,82]	[60,59]	[76,74]	[54,64]	[47,54]
Covariates	None	None	Some	Some	All	All
p-value	0.0815	0.0858	0.0683	0.0629	0.144	0.0730
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.104	0.137	0.100	0.140	0.139	0.113

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## H. When Extreme Poverty is Increasing/High

### H.1. When Extreme Poverty Increases

Table H1: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.913 (0.967)	1.191 (1.772)	0.920 (1.068)	2.295 (2.156)	0.652 (1.276)	1.576 (1.890)
Observations	536	536	506	506	506	506
Effective Observations	[148,196]	[142,192]	[128,158]	[128,158]	[124,144]	[130,184]
Covariates	None	None	Some	Some	All	All
p-value	0.345	0.501	0.389	0.287	0.609	0.404
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.140	0.135	0.115	0.116	0.0995	0.129
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	2.999 (4.751)	3.247 (6.555)	1.366 (4.782)	4.537 (7.647)	0.806 (4.067)	6.185 (9.568)
Observations	174	174	159	159	159	159
Effective Observations	[49,47]	[54,67]	[43,46]	[45,56]	[45,58]	[43,47]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.528	0.620	0.775	0.553	0.843	0.518
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.105	0.152	0.110	0.128	0.130	0.111

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table H2: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.157 (0.344)	-0.129 (0.700)	0.105 (0.360)	-0.0200 (0.731)	0.0987 (0.366)	-0.0855 (0.668)
Observations	534	534	504	504	504	504
Effective Observations	[141,182]	[135,154]	[123,152]	[123,144]	[123,150]	[123,150]
Covariates	None	None	Some	Some	All	All
p-value	0.648	0.854	0.771	0.978	0.787	0.898
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.124	0.111	0.113	0.100	0.112	0.112
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.321 (0.441)	0.237 (0.550)	0.210 (0.461)	0.116 (0.702)	0.139 (0.456)	0.139 (0.768)
Observations	174	174	159	159	159	159
Effective Observations	[51,55]	[64,80]	[44,51]	[46,61]	[45,52]	[45,57]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.467	0.666	0.649	0.868	0.761	0.857
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.120	0.191	0.117	0.142	0.118	0.129

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## H.2. When Extreme Poverty is High

Table H3: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.735 (1.042)	0.986 (1.318)	1.120 (1.211)	1.792 (1.749)	0.381 (0.696)	1.263 (1.292)
Observations	922	922	824	824	620	620
Effective Observations	[232,219]	[312,329]	[195,185]	[246,251]	[176,199]	[173,196]
Covariates	None	None	Some	Some	All	All
p-value	0.481	0.454	0.355	0.306	0.584	0.328
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0893	0.144	0.0814	0.118	0.118	0.114
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	4.126 (4.866)	7.773 (7.202)	6.297 (5.683)	10.44 (8.168)	4.910 (5.036)	9.364 (7.785)
Observations	261	261	234	234	185	185
Effective observations	[62,58]	[78,76]	[54,50]	[66,67]	[43,44]	[50,56]
Covariates	None	None	Some	Some	All	All
p-value	0.397	0.280	0.268	0.201	0.330	0.229
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0833	0.114	0.0780	0.111	0.0817	0.111

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table H4: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.157 (0.267)	0.0686 (0.353)	0.131 (0.295)	0.0852 (0.331)	0.243 (0.241)	0.305 (0.395)
Observations	918	918	820	820	616	616
Effective Observations	[241,235]	[295,294]	[194,181]	[277,305]	[176,202]	[159,174]
Covariates	None	None	Some	Some	All	All
p-value	0.557	0.846	0.656	0.797	0.315	0.440
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0946	0.131	0.0809	0.148	0.118	0.0955
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.203 (0.350)	0.320 (0.488)	0.236 (0.424)	0.310 (0.509)	0.372 (0.412)	0.895 (0.596)
Observations	261	261	234	234	185	185
Effective observations	[85,81]	[93,98]	[62,63]	[89,89]	[50,55]	[50,56]
Covariates	None	None	Some	Some	All	All
p-value	0.562	0.512	0.578	0.543	0.366	0.133
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.127	0.157	0.103	0.167	0.105	0.110

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

# I. Last Two Years of the Electoral Term

## I.1. When Poverty Decreases

Table I1: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.925** (0.806)	-2.466** (1.068)	-1.133 (0.804)	-1.546 (1.092)	-1.173 (0.850)	-2.057* (1.240)
Observations	389	389	357	357	357	357
Effective Observations	[114,96]	[126,99]	[106,86]	[114,87]	[104,84]	[94,70]
Covariates	None	None	Some	Some	All	All
p-value	0.0170	0.0210	0.159	0.157	0.167	0.0971
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0997	0.112	0.0994	0.106	0.0963	0.0869
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-3.885** (1.634)	-5.039** (2.158)	-2.335 (1.650)	-3.189 (2.196)	-2.390 (1.917)	-3.532 (2.488)
Observations	194	194	178	178	178	178
Effective Observations	[57,47]	[62,49]	[52,40]	[57,44]	[45,34]	[51,38]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0174	0.0196	0.157	0.147	0.213	0.156
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0976	0.110	0.0953	0.107	0.0784	0.0902

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table I2: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.225*** (0.373)	-1.296*** (0.449)	-0.997*** (0.350)	-1.139** (0.453)	-1.011*** (0.360)	-1.157** (0.461)
Observations	388	388	356	356	356	356
Effective Observations	[96,74]	[118,96]	[104,76]	[110,86]	[104,84]	[120,93]
Covariates	None	None	Some	Some	All	All
p-value	0.00102	0.00390	0.00441	0.0118	0.00501	0.0120
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0729	0.101	0.0912	0.101	0.0964	0.124
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B						
RD Estimate	-1.165*** (0.423)	-1.197** (0.512)	-0.997** (0.410)	-1.051** (0.514)	-0.923** (0.386)	-1.014** (0.513)
Observations	194	194	178	178	178	178
Effective Observations	[48,38]	[61,49]	[45,34]	[55,43]	[53,43]	[59,46]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00593	0.0195	0.0150	0.0409	0.0169	0.0481
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0747	0.104	0.0806	0.102	0.0988	0.120

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, whereas Panel B shows results by term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## I.2. When Poverty Is Low

Table I3: RDD Estimates for Infraction Count by Year and Term (Final 2 Years of Term)

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.608* (0.910)	-1.719 (1.094)	-1.173 (0.919)	-1.009 (1.102)	0.243 (0.930)	0.316 (1.023)
Observations	406	406	375	375	264	264
Effective Observations	[121,109]	[143,135]	[113,101]	[130,127]	[76,73]	[99,114]
Covariates	None	None	Some	Some	All	All
p-value	0.0773	0.116	0.202	0.360	0.794	0.757
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.100	0.144	0.102	0.144	0.0980	0.184
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.892 (1.846)	-3.091 (2.187)	-3.364* (1.825)	-3.277 (2.144)	-4.214** (1.864)	-4.307** (2.145)
Observations	208	208	192	192	192	192
Effective observations	[63,50]	[76,67]	[56,47]	[69,64]	[51,45]	[67,63]
Covariates	None	None	Some	Some	All	All
p-value	0.117	0.158	0.0653	0.126	0.0238	0.0447
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0945	0.144	0.0935	0.143	0.0861	0.135

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table I4: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.861*** (0.318)	-1.009*** (0.356)	-0.816** (0.331)	-0.942** (0.395)	-0.433 (0.355)	-0.603 (0.466)
Observations	406	406	375	375	264	264
Effective Observations	[102,97]	[143,133]	[90,89]	[120,112]	[82,78]	[89,92]
Covariates	None	None	Some	Some	All	All
p-value	0.00682	0.00455	0.0137	0.0171	0.223	0.196
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0808	0.141	0.0764	0.122	0.113	0.138
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B						
RD Estimate	-0.530 (0.358)	-0.699 (0.428)	-0.531 (0.354)	-0.607 (0.456)	-0.684* (0.355)	-0.839* (0.481)
Observations	208	208	192	192	192	192
Effective observations	[60,50]	[73,63]	[47,44]	[62,52]	[47,44]	[61,50]
Covariates	None	None	Some	Some	All	All
p-value	0.139	0.102	0.134	0.183	0.0543	0.0815
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0900	0.129	0.0767	0.114	0.0763	0.110

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, whereas Panel B shows results by term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

### I.3. When Extreme Poverty Decreases

Table I5: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.479** (0.722)	-1.910* (1.000)	-1.278 (0.844)	-1.519 (1.103)	-1.111 (0.885)	-2.093* (1.221)
Observations	432	432	387	387	387	387
Effective Observations	[133,119]	[133,119]	[102,96]	[116,107]	[96,90]	[96,90]
Covariates	None	None	Some	Some	All	All
p-value	0.0407	0.0560	0.130	0.168	0.210	0.0865
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.108	0.110	0.0927	0.110	0.0893	0.0893
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.545 (1.609)	-3.239 (2.106)	-2.588 (1.700)	-3.158 (2.227)	-3.525* (1.930)	-4.544* (2.409)
Observations	216	216	193	193	193	193
Effective Observations	[59,54]	[67,60]	[51,48]	[58,54]	[42,43]	[53,53]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.114	0.124	0.128	0.156	0.0678	0.0593
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0935	0.110	0.0924	0.109	0.0771	0.0992

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table I6: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.908** (0.376)	-1.048** (0.435)	-0.595* (0.344)	-0.779* (0.453)	-0.564* (0.342)	-0.710* (0.420)
Observations	431	431	386	386	386	386
Effective Observations	[91,86]	[121,118]	[102,96]	[116,107]	[110,106]	[138,135]
Covariates	None	None	Some	Some	All	All
p-value	0.0156	0.0159	0.0840	0.0857	0.0984	0.0911
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0697	0.0990	0.0938	0.108	0.102	0.147
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.840** (0.404)	-0.917* (0.472)	-0.564 (0.392)	-0.742 (0.505)	-0.572 (0.351)	-0.802 (0.506)
Observations	216	216	193	193	193	193
Effective Observations	[48,44]	[65,60]	[46,45]	[58,54]	[58,54]	[60,55]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0375	0.0521	0.150	0.142	0.103	0.113
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0708	0.104	0.0884	0.107	0.108	0.115

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results year, while Panel B shows results term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## I.4. When Extreme Poverty is Low

Table I7: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.018** (0.905)	-2.550** (1.205)	-1.062 (0.866)	-1.245 (1.179)	0.132 (0.834)	-0.0489 (1.200)
Observations	400	400	365	365	258	258
Effective Observations	[116,100]	[130,114]	[109,97]	[115,106]	[80,69]	[86,80]
Covariates	None	None	Some	Some	All	All
p-value	0.0258	0.0343	0.220	0.291	0.874	0.967
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0968	0.121	0.111	0.121	0.109	0.128
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-3.649* (1.885)	-4.337* (2.497)	-3.196* (1.740)	-3.937* (2.303)	-5.568*** (1.805)	-5.743*** (2.048)
Observations	205	205	188	188	188	188
Effective observations	[58,48]	[68,54]	[54,47]	[62,53]	[43,43]	[64,58]
Covariates	None	None	Some	Some	All	All
p-value	0.0529	0.0824	0.0663	0.0873	0.00204	0.00504
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0909	0.119	0.100	0.122	0.0787	0.131 height

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table I8: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.717** (0.313)	-0.826** (0.353)	-0.597** (0.304)	-0.654* (0.392)	-0.375 (0.384)	-0.500 (0.501)
Observations	400	400	365	365	258	258
Effective Observations	[105,94]	[142,142]	[88,86]	[115,106]	[74,66]	[87,84]
Covariates	None	None	Some	Some	All	All
p-value	0.0218	0.0194	0.0499	0.0951	0.329	0.318
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0876	0.153	0.0854	0.122	0.0950	0.133
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.520 (0.401)	-0.711 (0.443)	-0.507 (0.393)	-0.512 (0.479)	-0.678* (0.411)	-0.826 (0.514)
Observations	205	205	188	188	188	188
Effective observations	[50,46]	[71,62]	[42,41]	[59,49]	[39,39]	[54,47]
Covariates	None	None	Some	Some	All	All
p-value	0.195	0.109	0.197	0.285	0.0990	0.108
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0785	0.134	0.0744	0.113	0.0709	0.0985

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results year, while Panel B shows results term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## J. First Two Years

### J.1. When Poverty Decreases

Table J1: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.970 (0.752)	-0.958 (1.023)	-0.195 (0.823)	-0.195 (1.022)	-0.604 (0.914)	-1.144 (1.206)
Observations	212	212	212	212	212	212
Effective Observations	[72,42]	[74,42]	[62,36]	[72,42]	[66,38]	[72,42]
Covariates	None	None	Some	Some	All	All
p-value	0.197	0.349	0.813	0.848	0.509	0.343
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.105	0.0900	0.101	0.0952	0.102
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.972 (1.503)	-1.945 (2.092)	-0.637 (1.577)	-0.415 (2.094)	-1.373 (1.747)	-2.358 (2.454)
Observations	105	105	105	105	105	105
Effective Observations	[37,21]	[36,21]	[36,21]	[34,21]	[35,21]	[36,21]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.189	0.352	0.686	0.843	0.432	0.336
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.109	0.103	0.103	0.0998	0.100	0.102

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table J2: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.146 (0.307)	0.289 (0.354)	0.299 (0.299)	0.379 (0.346)	0.179 (0.362)	0.132 (0.415)
Observations	210	210	210	210	210	210
Effective Observations	[52,24]	[60,34]	[50,24]	[60,34]	[50,24]	[60,34]
Covariates	None	None	Some	Some	All	All
p-value	0.635	0.415	0.318	0.273	0.621	0.751
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0610	0.0868	0.0602	0.0866	0.0587	0.0859
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.0195 (0.390)	0.165 (0.447)	0.136 (0.374)	0.179 (0.435)	0.0784 (0.478)	-0.0730 (0.556)
Observations	105	105	105	105	105	105
Effective Observations	[26,12]	[31,17]	[26,12]	[33,18]	[25,12]	[30,17]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.960	0.711	0.716	0.680	0.870	0.896
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0612	0.0893	0.0612	0.0911	0.0579	0.0868

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## J.2. When Poverty is Low

Table J3: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.470*** (0.805)	-2.940*** (0.936)	-2.052** (0.804)	-2.329** (0.944)	-1.880** (0.751)	-2.213** (0.910)
Observations	414	414	383	383	383	383
Effective Observations	[107,94]	[147,131]	[100,90]	[140,133]	[98,90]	[134,125]
Covariates	None	None	Some	Some	All	All
p-value	0.00214	0.00169	0.0107	0.0136	0.0123	0.0150
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0761	0.134	0.0846	0.153	0.0827	0.133
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-4.257** (1.794)	-5.180** (2.127)	-4.085** (1.624)	-4.699** (1.930)	-3.672** (1.623)	-4.944** (2.048)
Observations	208	208	192	192	192	192
Effective observations	[54,47]	[74,66]	[49,45]	[69,64]	[47,44]	[67,63]
Covariates	None	None	Some	Some	All	All
p-value	0.0176	0.0149	0.0119	0.0149	0.0237	0.0158
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0796	0.137	0.0833	0.145	0.0784	0.134

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table J4: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.274 (0.328)	-0.405 (0.393)	-0.149 (0.309)	-0.184 (0.348)	-0.102 (0.307)	-0.182 (0.348)
Observations	413	413	382	382	382	382
Effective Observations	[117,96]	[151,131]	[112,94]	[144,149]	[112,94]	[144,149]
Covariates	None	None	Some	Some	All	All
p-value	0.403	0.302	0.630	0.597	0.739	0.600
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0877	0.138	0.0957	0.173	0.0940	0.173
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.219 (0.350)	-0.233 (0.434)	-0.225 (0.321)	-0.272 (0.384)	-0.156 (0.318)	-0.302 (0.392)
Observations	208	208	192	192	192	192
Effective observations	[63,50]	[74,66]	[55,47]	[69,64]	[53,45]	[69,64]
Covariates	None	None	Some	Some	All	All
p-value	0.532	0.591	0.483	0.478	0.625	0.441
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0929	0.137	0.0915	0.142	0.0896	0.143

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

### J.3. When Extreme Poverty Decreases

Table J5: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.331* (0.770)	-1.425 (0.971)	-0.791 (0.807)	-0.835 (0.997)	-0.891 (0.836)	-1.331 (1.050)
Observations	238	238	238	238	238	238
Effective Observations	[58,44]	[82,56]	[58,44]	[82,56]	[58,44]	[80,54]
Covariates	None	None	Some	Some	All	All
p-value	0.0837	0.142	0.327	0.402	0.287	0.205
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0830	0.114	0.0824	0.113	0.0818	0.106
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.661* (1.543)	-2.872 (1.957)	-1.601 (1.610)	-1.667 (2.030)	-1.925 (1.675)	-2.818 (2.160)
Observations	118	118	118	118	118	118
Effective Observations	[29,22]	[42,28]	[29,22]	[41,28]	[29,22]	[40,27]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0847	0.142	0.320	0.411	0.251	0.192
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0842	0.115	0.0845	0.112	0.0840	0.105

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table J6: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.172 (0.351)	0.264 (0.392)	0.169 (0.342)	0.271 (0.397)	0.231 (0.353)	0.114 (0.420)
Observations	236	236	236	236	236	236
Effective Observations	[52,34]	[70,48]	[56,42]	[60,44]	[54,36]	[60,44]
Covariates	None	None	Some	Some	All	All
p-value	0.625	0.501	0.621	0.495	0.513	0.786
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0650	0.0917	0.0714	0.0869	0.0695	0.0867
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0309 (0.384)	0.166 (0.471)	-0.157 (0.355)	0.143 (0.473)	-0.000151 (0.386)	-0.0117 (0.523)
Observations	118	118	118	118	118	118
Effective Observations	[28,22]	[35,24]	[33,23]	[35,24]	[28,22]	[33,23]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.936	0.725	0.658	0.763	1	0.982
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0769	0.0935	0.0901	0.0922	0.0781	0.0902

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## J.4. When Extreme Poverty is Low

Table J7: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.033** (0.931)	-2.577** (1.075)	-1.392 (0.941)	-1.607 (1.124)	-1.426* (0.863)	-1.741* (1.016)
Observations	407	407	376	376	376	376
Effective Observations	[97,88]	[143,124]	[90,86]	[126,114]	[86,86]	[128,118]
Covariates	None	None	Some	Some	All	All
p-value	0.0290	0.0165	0.139	0.153	0.0984	0.0866
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0749	0.137	0.0833	0.129	0.0794	0.135
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-3.305 (2.056)	-4.235* (2.404)	-2.789 (1.890)	-3.225 (2.266)	-2.105 (1.882)	-3.603 (2.367)
Observations	205	205	188	188	188	188
Effective observations	[50,46]	[74,64]	[45,43]	[63,58]	[43,43]	[64,59]
Covariates	None	None	Some	Some	All	All
p-value	0.108	0.0782	0.140	0.155	0.263	0.128
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0788	0.141	0.0833	0.129	0.0786	0.137

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table J8: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.135 (0.376)	-0.323 (0.437)	0.137 (0.361)	0.0483 (0.428)	0.185 (0.360)	0.0978 (0.438)
Observations	406	406	375	375	375	375
Effective Observations	[99,92]	[141,124]	[94,86]	[128,118]	[92,86]	[128,116]
Covariates	None	None	Some	Some	All	All
p-value	0.721	0.460	0.705	0.910	0.607	0.823
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0804	0.133	0.0851	0.136	0.0844	0.131
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0912 (0.400)	-0.192 (0.491)	-0.0704 (0.362)	-0.211 (0.416)	0.102 (0.361)	-0.190 (0.395)
Observations	205	205	188	188	188	188
Effective observations	[53,46]	[70,61]	[45,43]	[66,61]	[43,43]	[70,70]
Covariates	None	None	Some	Some	All	All
p-value	0.820	0.695	0.846	0.612	0.778	0.630
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0841	0.129	0.0834	0.143	0.0807	0.170

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results by year, while Panel B shows results by electoral term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## K. Results for the Whole Sample (i.e. When Poverty Is Not Considered)

### K.1. For Poverty Increasing/Decreasing Sample

Table K1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0346 (0.581)	-0.455 (0.885)	0.110 (0.532)	0.158 (0.812)	-0.132 (0.634)	-0.193 (0.724)
Observations	1,357	1,357	1,275	1,275	1,151	1,151
Effective Observations	[451,419]	[467,461]	[472,487]	[486,505]	[343,333]	[470,517]
Covariates	None	None	Some	Some	All	All
p-value	0.952	0.607	0.837	0.845	0.835	0.789
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.132	0.149	0.170	0.178	0.120	0.212
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.0420 (0.624)	-0.143 (0.868)	0.370 (0.578)	0.338 (0.713)	0.0368 (0.654)	0.231 (0.846)
Observations	1,357	1,357	1,275	1,275	1,151	1,151
Effective Observations	[421,377]	[471,473]	[430,417]	[536,559]	[331,322]	[415,447]
Covariates	None	None	Some	Some	All	All
p-value	0.946	0.869	0.522	0.636	0.955	0.785
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.118	0.153	0.142	0.219	0.114	0.169

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table K2: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.232 (2.695)	-3.996 (3.911)	-2.892 (2.848)	-4.365 (3.998)	-5.220* (3.141)	-4.038 (4.206)
Observations	440	440	398	398	372	372
Effective Observations	[132,119]	[148,142]	[117,108]	[134,134]	[99,93]	[121,123]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.408	0.307	0.310	0.275	0.0966	0.337
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.111	0.136	0.107	0.141	0.0935	0.132
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.276 (1.954)	-0.196 (2.696)	1.232 (1.989)	1.225 (2.425)	-0.758 (2.234)	0.0322 (3.030)
Observations	440	440	398	398	372	372
Effective Observations	[135,122]	[154,157]	[126,120]	[160,171]	[102,101]	[124,129]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.888	0.942	0.536	0.613	0.734	0.992
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.152	0.122	0.194	0.100	0.142

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table K3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.173 (0.217)	-0.336 (0.292)	-0.0737 (0.210)	-0.113 (0.275)	-0.0910 (0.211)	-0.207 (0.303)
Observations	1,352	1,352	1,270	1,270	1,146	1,146
Effective Observations	[398,353]	[459,431]	[389,361]	[462,477]	[339,331]	[375,393]
Covariates	None	None	Some	Some	All	All
p-value	0.424	0.249	0.726	0.680	0.667	0.495
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.106	0.141	0.119	0.164	0.117	0.142
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.139 (0.221)	-0.272 (0.290)	-0.0318 (0.213)	-0.0187 (0.255)	-0.0378 (0.207)	-0.0905 (0.291)
Observations	1,352	1,352	1,270	1,270	1,146	1,146
Effective Observations	[386,352]	[461,443]	[381,349]	[513,529]	[341,333]	[379,409]
Covariates	None	None	Some	Some	All	All
p-value	0.530	0.348	0.882	0.942	0.856	0.755
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.142	0.114	0.189	0.118	0.151

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table K4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0541 (0.247)	-0.379 (0.405)	0.0199 (0.247)	-0.288 (0.412)	-0.203 (0.287)	-0.215 (0.394)
Observations	440	440	398	398	372	372
Effective Observations	[150,144]	[146,136]	[136,142]	[132,132]	[108,103]	[125,135]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.827	0.350	0.936	0.484	0.480	0.586
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.138	0.130	0.148	0.136	0.109	0.148
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0200 (0.253)	-0.200 (0.366)	0.161 (0.244)	0.0577 (0.353)	0.0716 (0.256)	0.0626 (0.358)
Observations	440	440	398	398	372	372
Effective Observations	[133,120]	[147,136]	[127,124]	[136,142]	[110,107]	[125,135]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.937	0.585	0.509	0.870	0.780	0.861
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.112	0.131	0.125	0.148	0.114	0.148

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## K.2. For Poverty High/Low Sample

Table K5: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.326 (0.476)	-0.671 (0.722)	-0.233 (0.523)	-0.324 (0.712)	-0.296 (0.448)	-0.294 (0.613)
Observations	2,004	2,004	1,834	1,834	1,254	1,254
Effective Observations	[666,606]	[700,664]	[562,515]	[640,658]	[369,358]	[421,465]
Covariates	None	None	Some	Some	All	All
p-value	0.493	0.353	0.657	0.649	0.509	0.631
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.130	0.144	0.115	0.154	0.112	0.150
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.107 (0.438)	-0.527 (0.718)	0.128 (0.457)	-0.0374 (0.692)	-0.202 (0.444)	-0.222 (0.565)
Observations	2,004	2,004	1,834	1,834	1,254	1,254
Effective Observations	[700,668]	[690,640]	[614,584]	[636,638]	[369,366]	[454,509]
Covariates	None	None	Some	Some	All	All
p-value	0.807	0.462	0.779	0.957	0.650	0.694
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.144	0.138	0.135	0.150	0.114	0.170

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table K6: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.323 (2.158)	-2.044 (2.585)	-1.105 (2.360)	-0.758 (3.186)	-2.631 (2.323)	-3.089 (3.079)
Observations	567	567	522	522	373	373
Effective observations	[191,169]	[236,238]	[164,147]	[186,185]	[108,102]	[129,146]
Covariates	None	None	Some	Some	All	All
p-value	0.540	0.429	0.640	0.812	0.257	0.316
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.131	0.212	0.117	0.156	0.108	0.159
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0818 (1.626)	-1.540 (2.637)	0.349 (1.683)	-0.526 (2.545)	-1.718 (2.383)	-2.156 (2.491)
Observations	567	567	522	522	373	373
Effective observations	[198,178]	[198,178]	[174,158]	[183,177]	[102,101]	[153,169]
Covariates	None	None	Some	Some	All	All
p-value	0.960	0.559	0.836	0.836	0.471	0.387
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.140	0.140	0.131	0.149	0.100	0.211

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table K7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.144 (0.193)	-0.362 (0.318)	-0.0799 (0.182)	-0.320 (0.292)	-0.0297 (0.190)	-0.111 (0.282)
Observations	1,999	1,999	1,829	1,829	1,249	1,249
Effective Observations	[599,537]	[625,553]	[585,556]	[592,556]	[352,346]	[400,403]
Covariates	None	None	Some	Some	All	All
p-value	0.456	0.254	0.660	0.273	0.876	0.694
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.111	0.116	0.125	0.126	0.102	0.129
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.120 (0.194)	-0.277 (0.307)	-0.0630 (0.185)	-0.202 (0.268)	-0.0232 (0.186)	-0.0849 (0.267)
Observations	1,999	1,999	1,829	1,829	1,249	1,249
Effective Observations	[599,533]	[637,564]	[573,523]	[616,588]	[359,348]	[406,431]
Covariates	None	None	Some	Some	All	All
p-value	0.538	0.367	0.733	0.450	0.901	0.750
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.108	0.119	0.118	0.137	0.105	0.137

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table K8: RDD Estimates for Infraction Amount (log) by Term

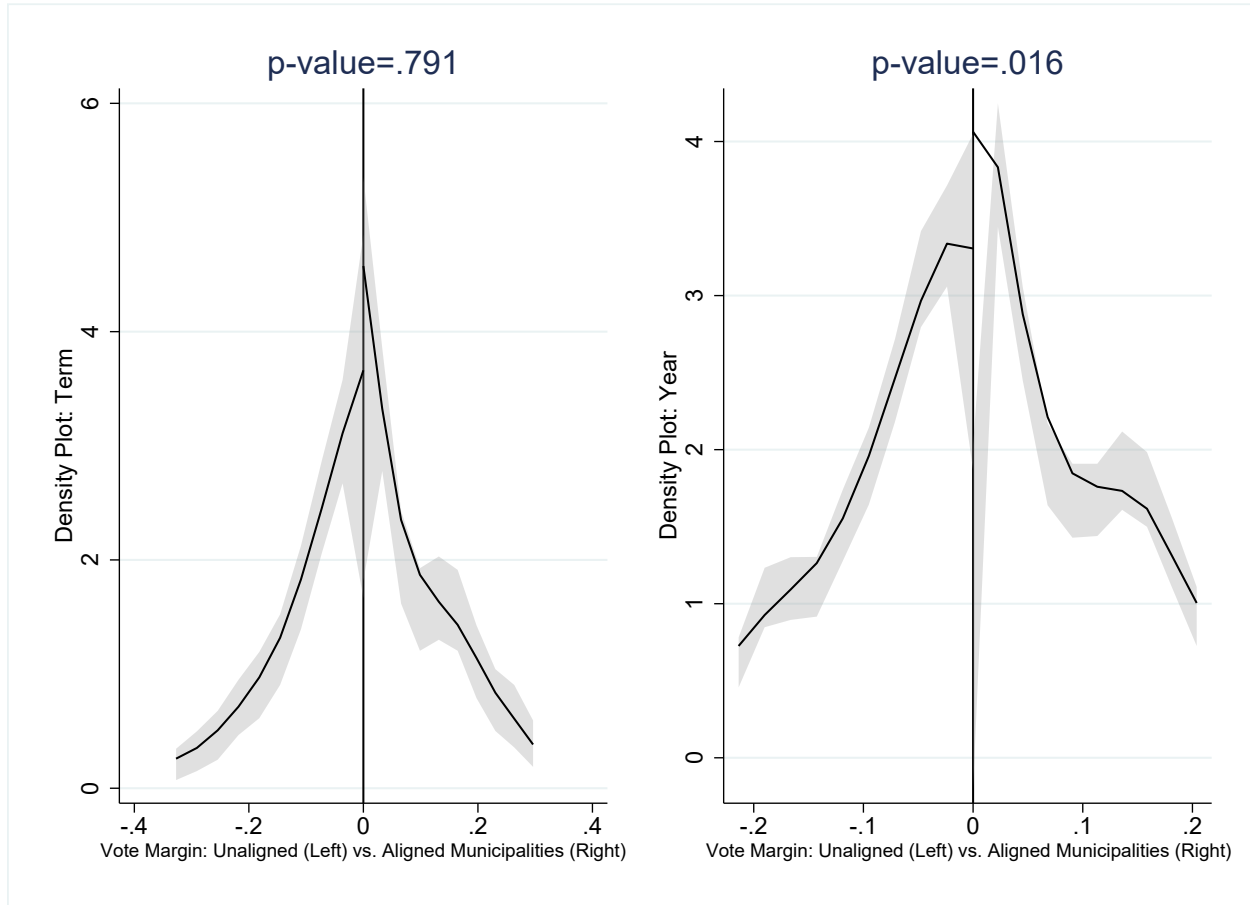
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.00483 (0.230)	-0.0220 (0.335)	-0.0127 (0.241)	0.0211 (0.325)	-0.0485 (0.255)	-0.162 (0.381)
Observations	567	567	522	522	373	373
Effective observations	[193,174]	[202,197]	[167,152]	[193,188]	[108,102]	[119,119]
Covariates	None	None	Some	Some	All	All
p-value	0.983	0.948	0.958	0.948	0.849	0.672
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.134	0.154	0.122	0.162	0.108	0.129
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.0117 (0.231)	-0.0456 (0.343)	0.0177 (0.238)	0.0136 (0.332)	-0.0244 (0.257)	-0.136 (0.382)
Observations	567	567	522	522	373	373
Effective observations	[189,169]	[201,186]	[168,152]	[184,183]	[108,102]	[119,118]
Covariates	None	None	Some	Some	All	All
p-value	0.959	0.894	0.941	0.967	0.924	0.722
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.130	0.146	0.122	0.154	0.107	0.128

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## L. RDD Robustness Checks: Term and Year

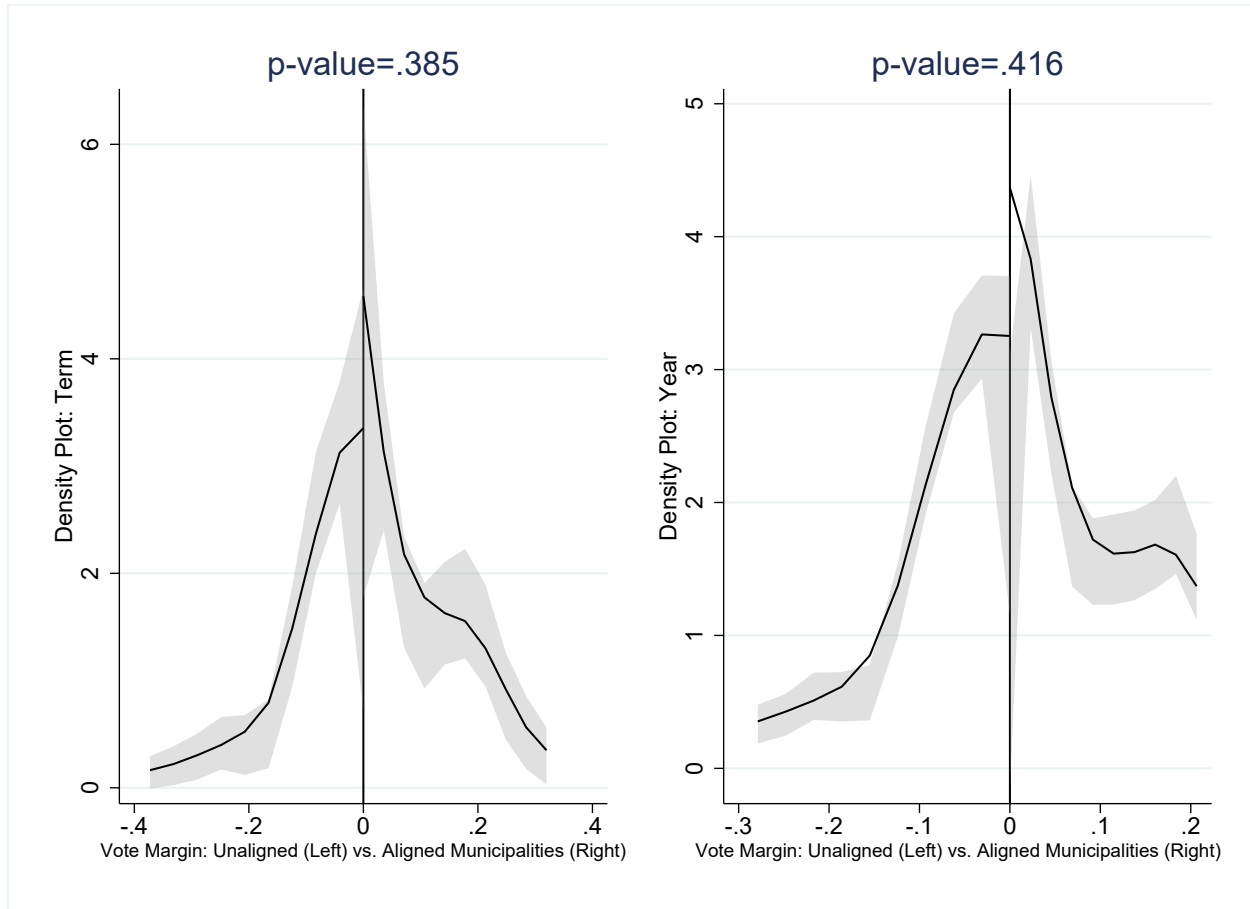
### L.1. Density Plots for Poverty High/Low Sample

Figure L.1: RDD Density Plots for Infraction Count and Amount (Whole Sample)



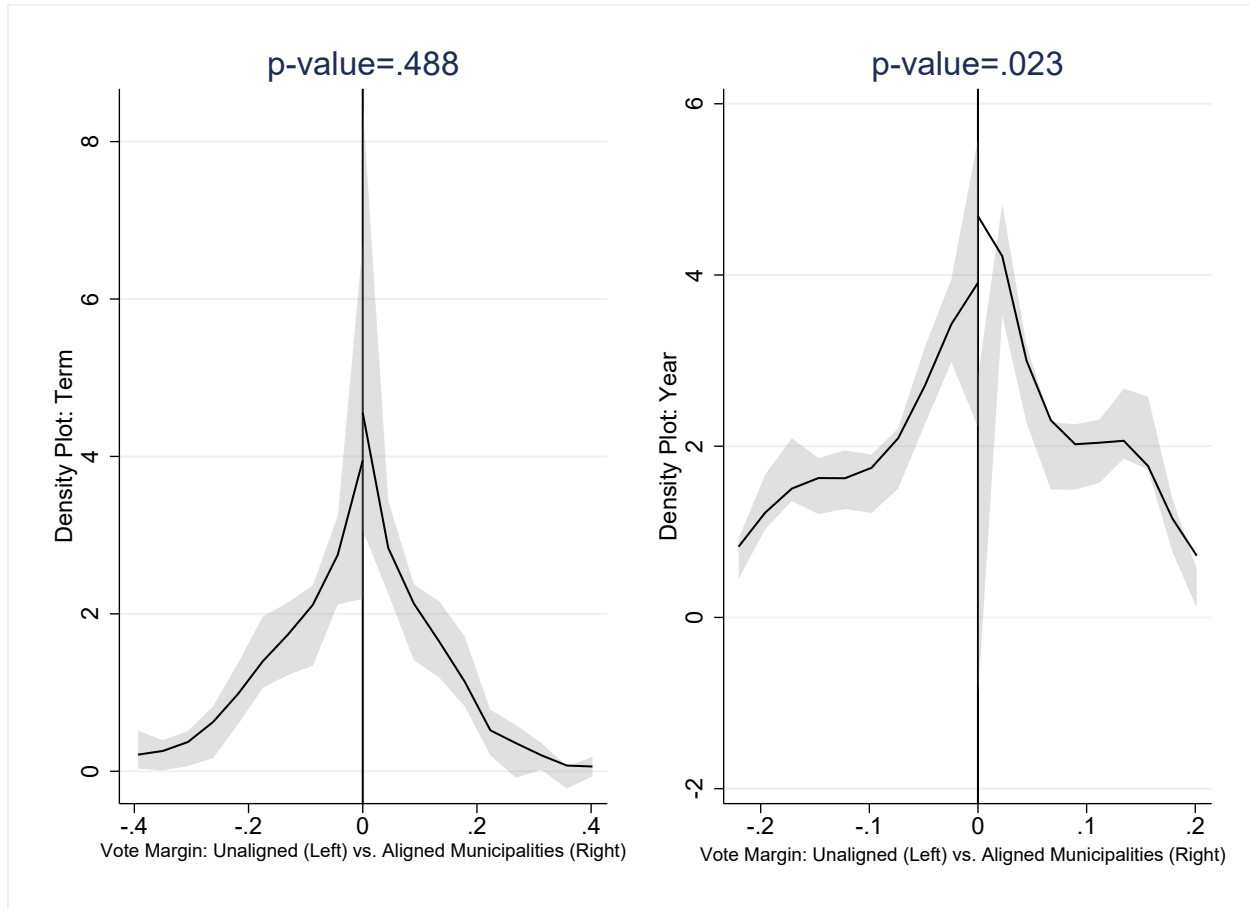
Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. The electoral term are results are not statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis in this sample. The year-wise results for this sample do not pass the [McCrary \(2008\)](#) density tests, indicating a potential problem with using the margin of victory as a running variable for this sample. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff.

Figure L.2: RDD Density Plots for Infraction Count and Amount (Low-Poverty Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

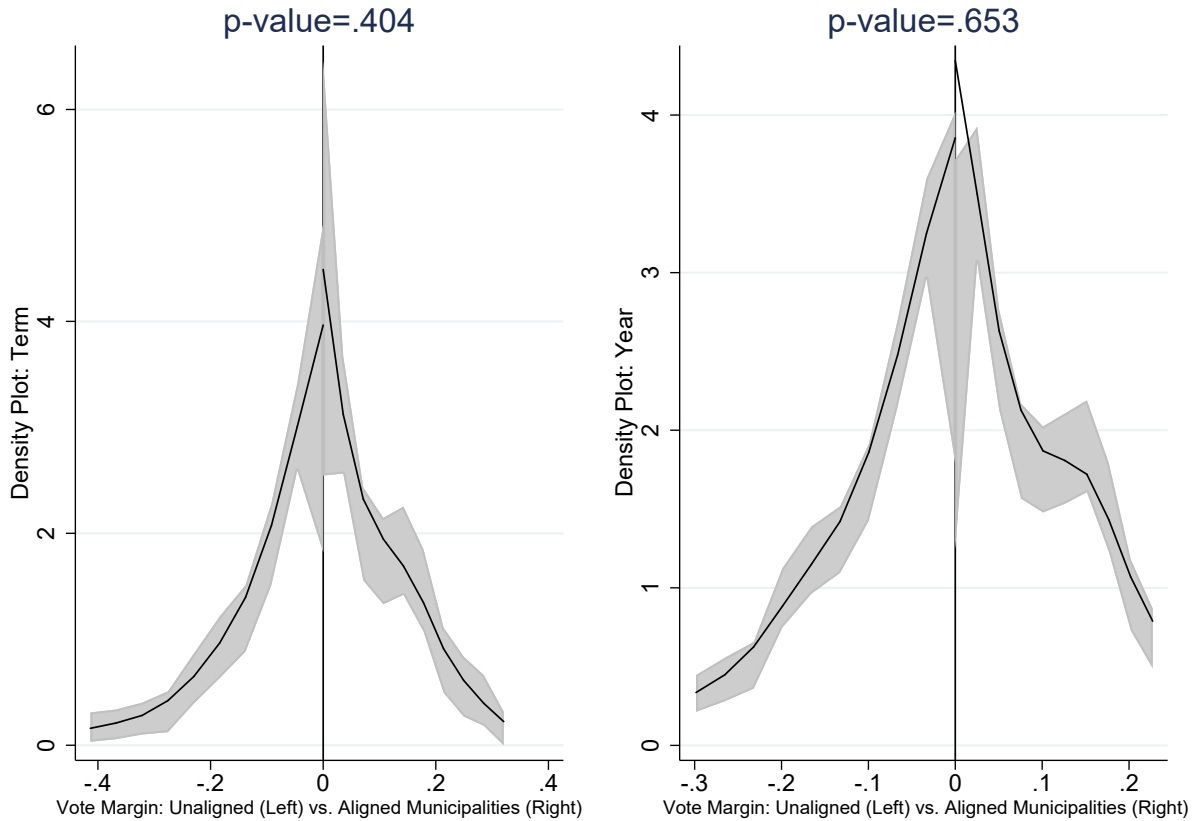
Figure L.3: RDD Density Plots for Infraction Count and Amount (High-Poverty Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. The electoral term are results are not statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis in this sample. The year-wise results for this sample do not pass the [McCrary \(2008\)](#) density tests, indicating a potential problem with using the margin of victory as a running variable for this sample. The above plots provide further evidence via the confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

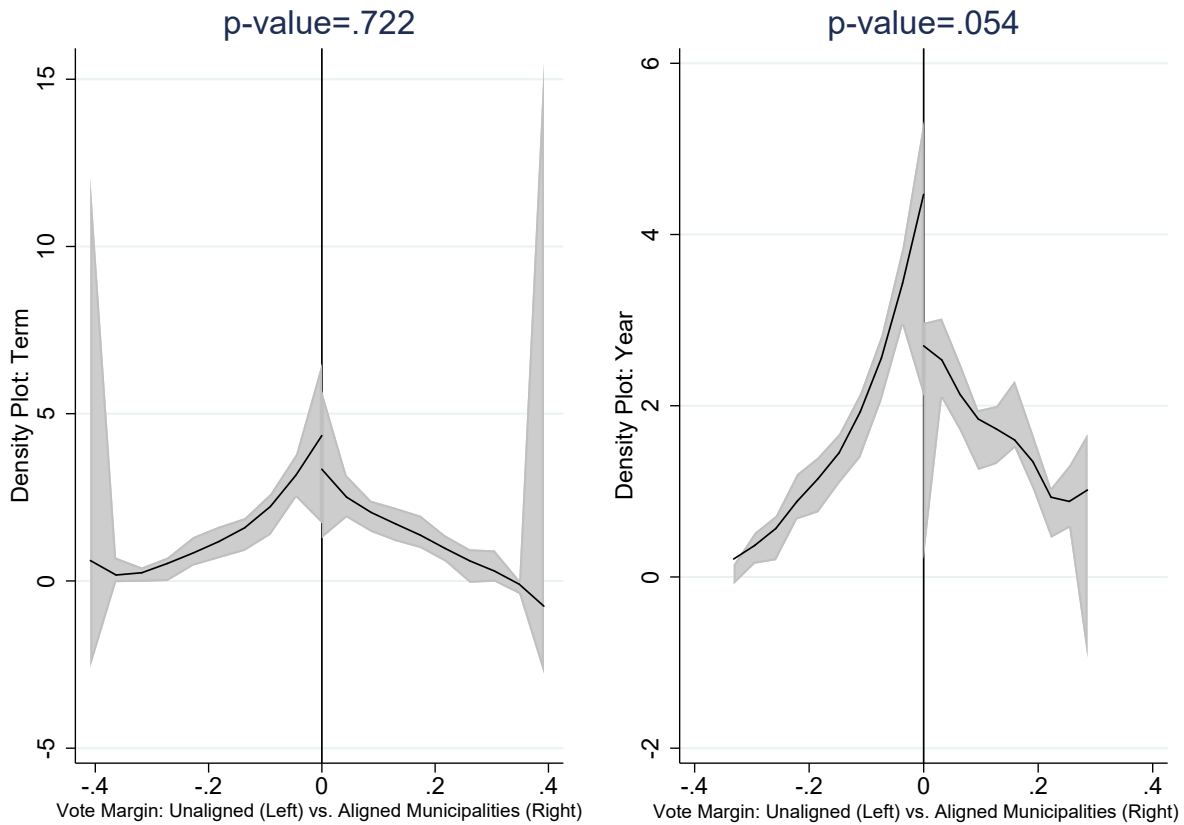
## L.2. Density Plots for Poverty Increasing/Decreasing Sample: 2010-2015 (Main Results)

Figure L.4: RDD Density Plots for Infraction Count and Amount (Whole Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

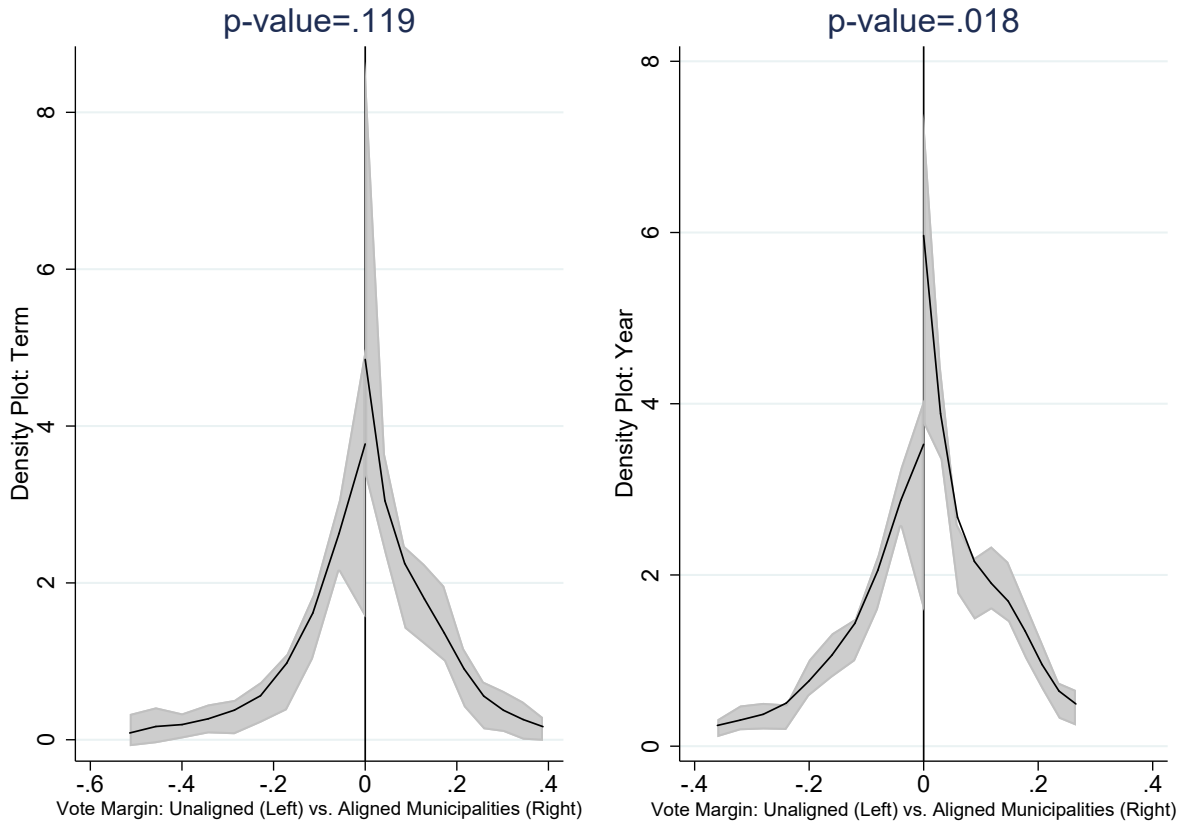
Figure L.5: RDD Density Plots for Infraction Count and Amount (Poverty-Decreasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.



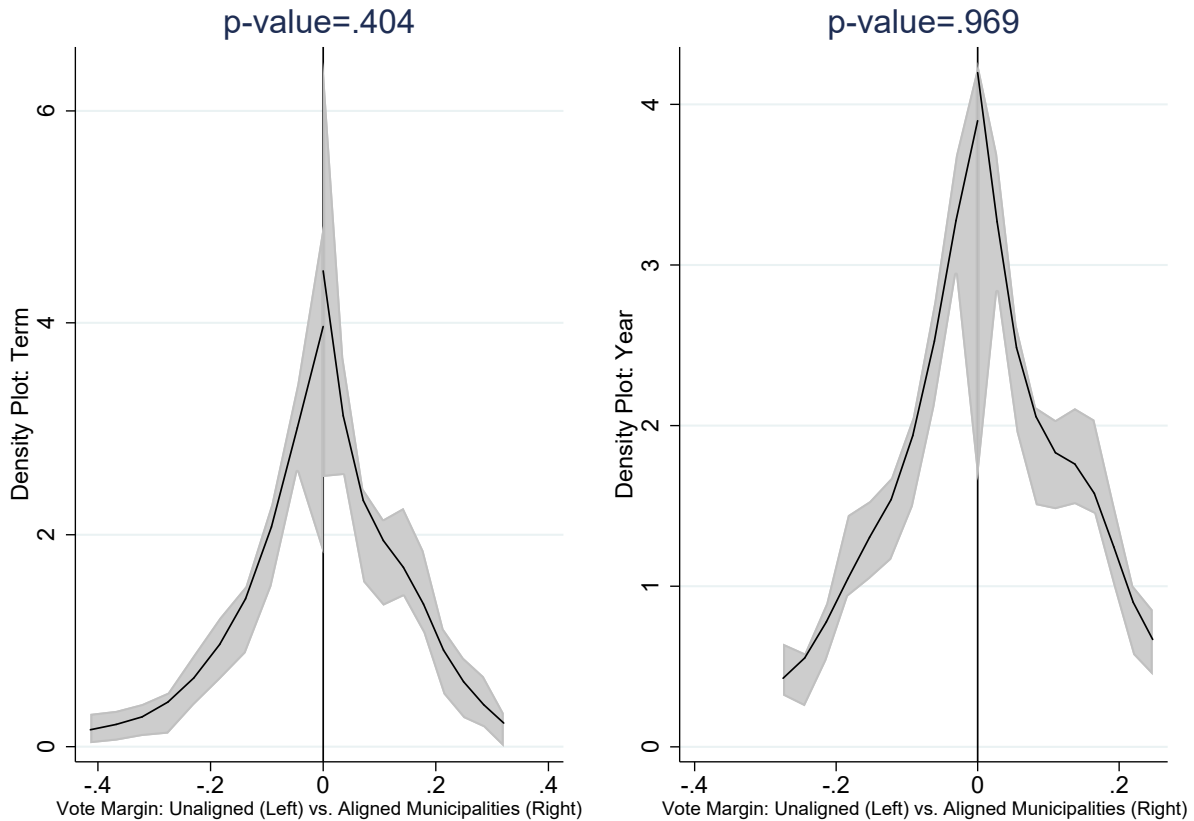
Figure L.6: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. The electoral term results are not statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis in this sample. The year-wise results for this sample do not pass the [McCrary \(2008\)](#) density tests, indicating a potential problem with using the margin of victory as a running variable for this sample. The above plots provide further evidence via the confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

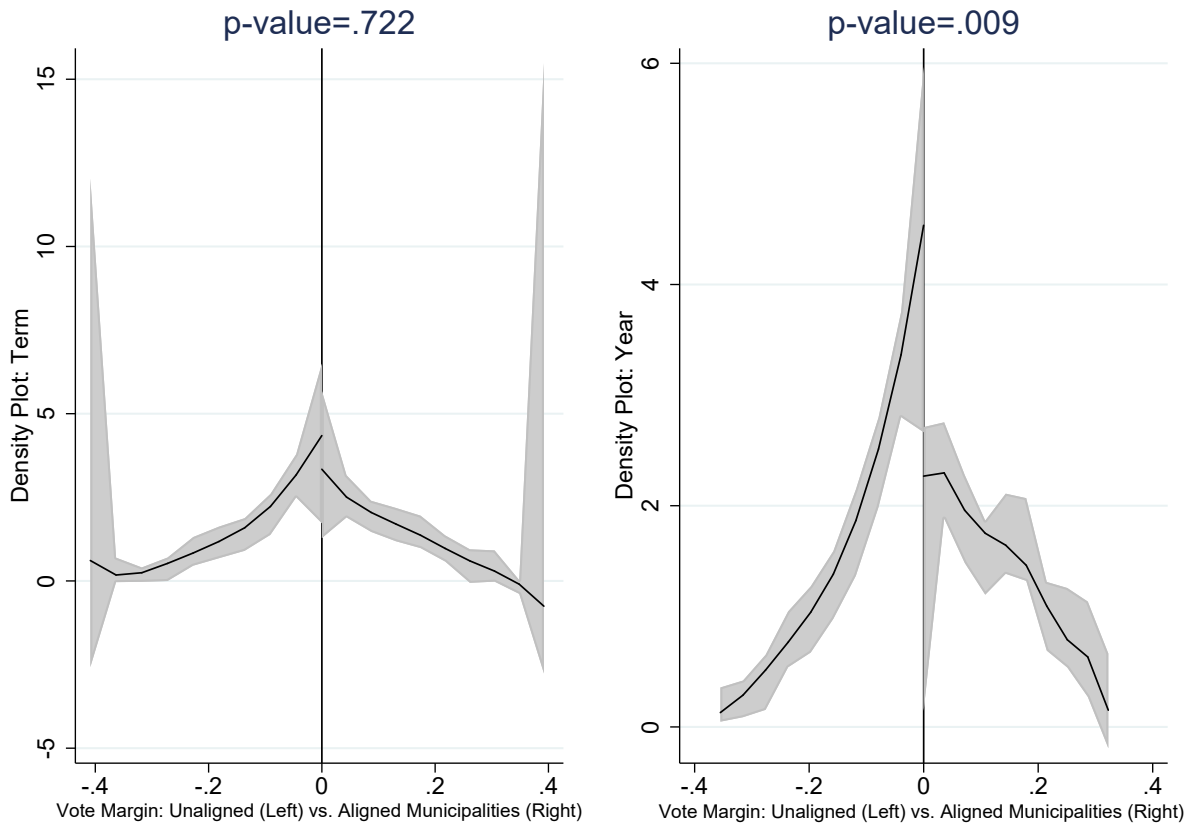
### L.3. Density Plots for Poverty Increasing/Decreasing Sample: 2011-2015

Figure L.7: RDD Density Plots for Infraction Count and Amount (Whole Sample)



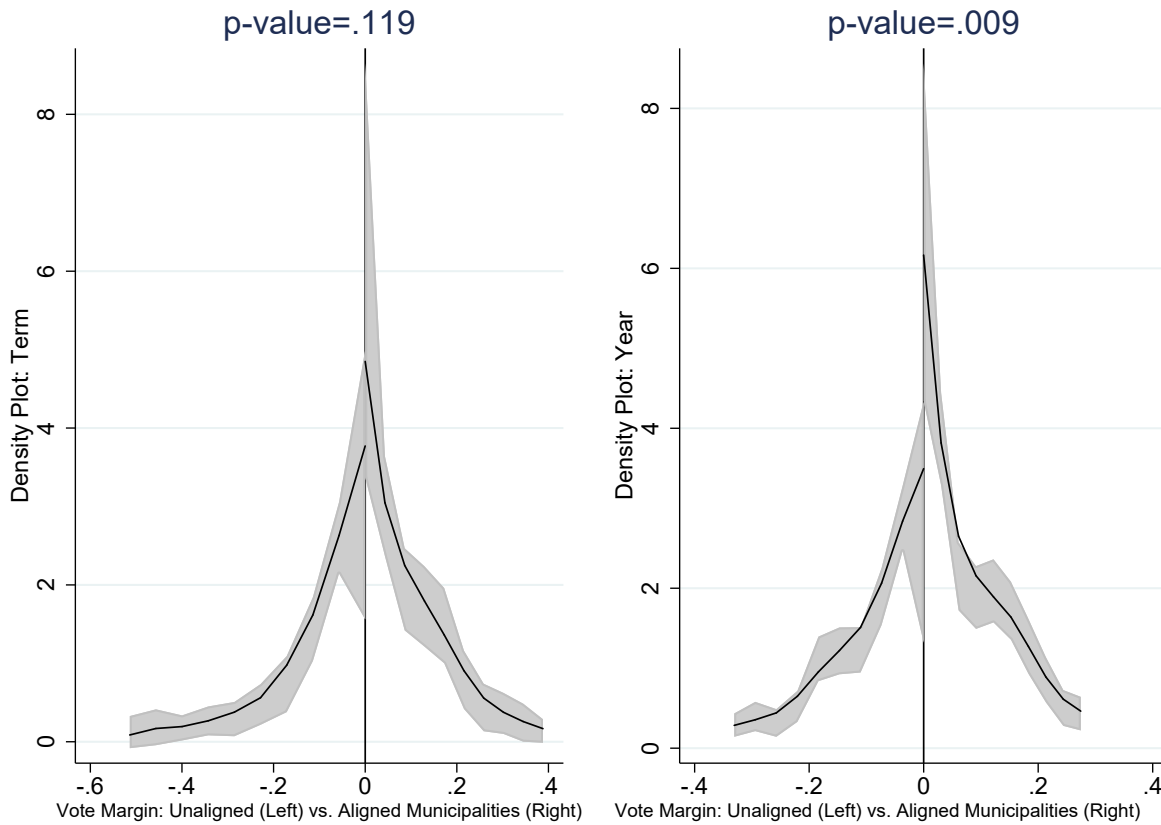
Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

Figure L.8: RDD Density Plots for Infraction Count and Amount (Poverty-Decreasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. The electoral term are results are not statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis in this sample. The year-wise results for this sample do not pass the [McCrary \(2008\)](#) density tests, indicating a potential problem with using the margin victory data for this sample. The above plots provide further evidence via the confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

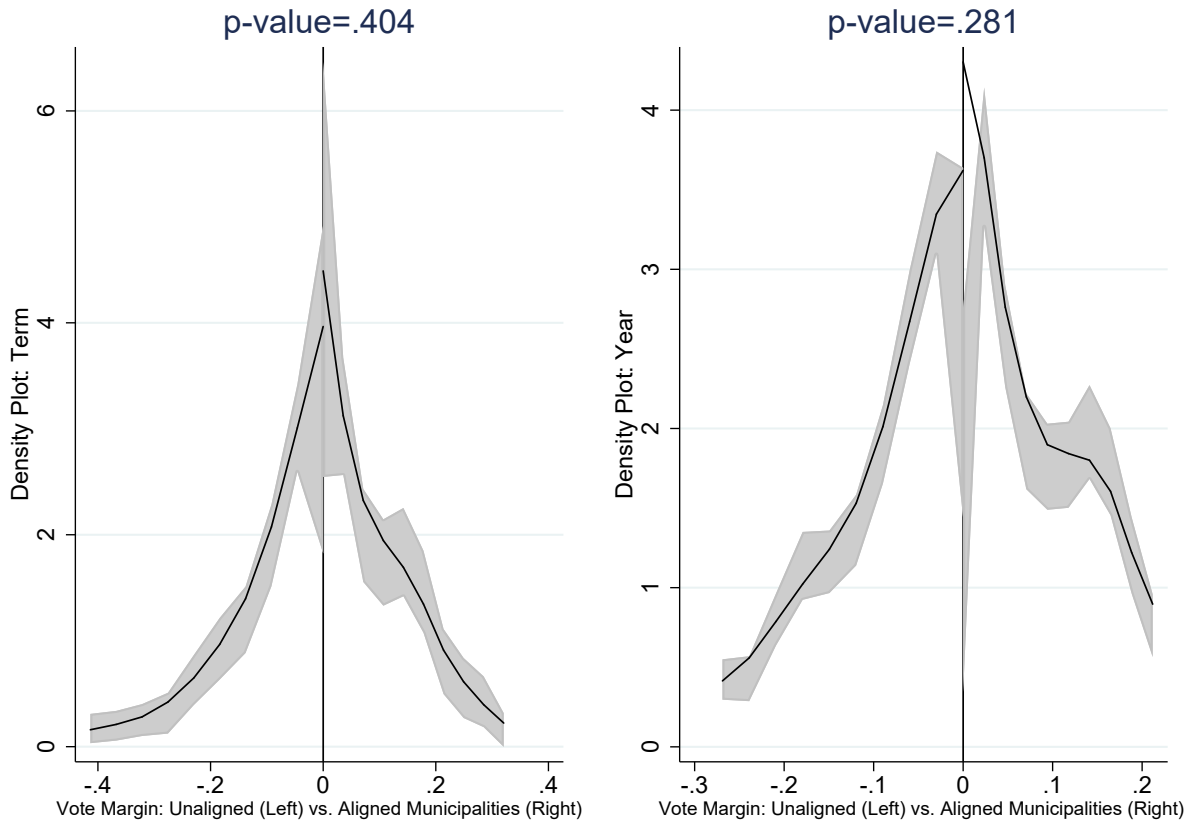
Figure L.9: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. The electoral term results are not statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis in this sample. The year-wise results for this sample do not pass the [McCrary \(2008\)](#) density tests, indicating a potential problem with using the margin of victory as a running variable for this sample. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

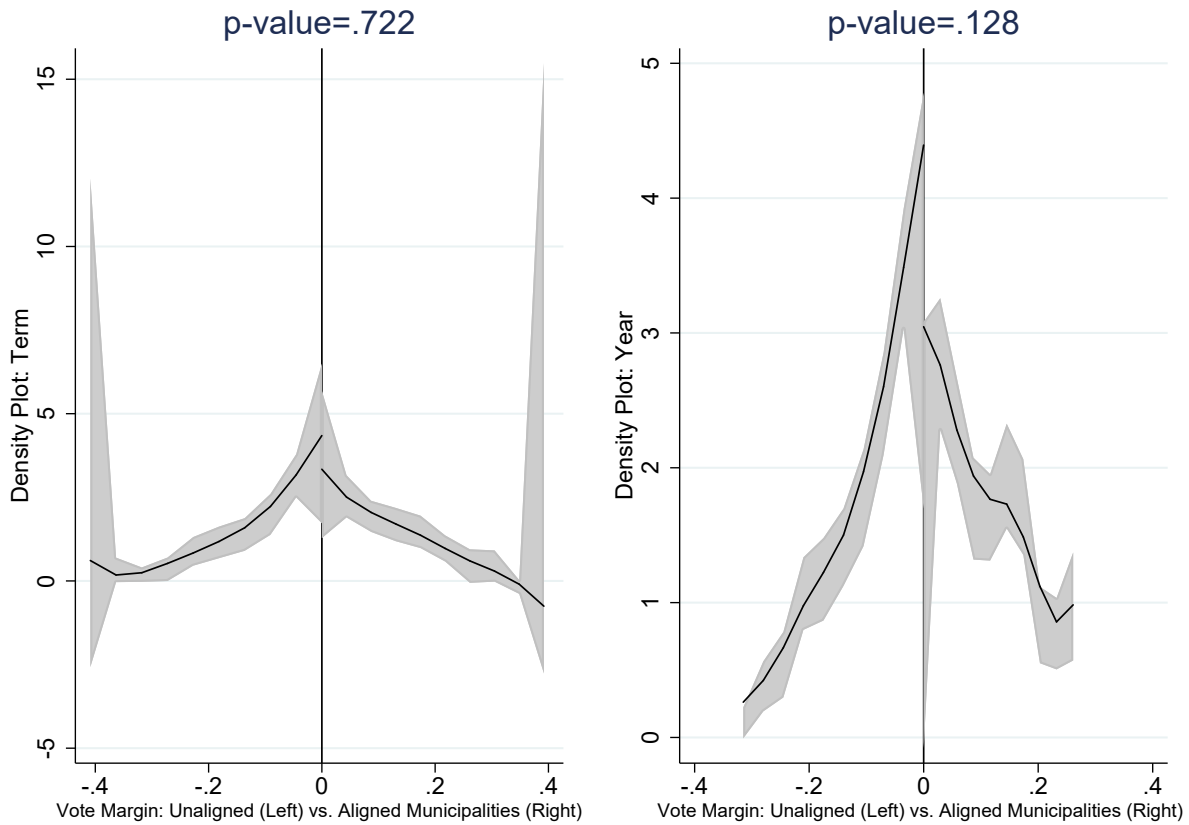
## L.4. Density Plots for Poverty Increasing/Decreasing Sample: 2009-2015

Figure L.10: RDD Density Plots for Infraction Count and Amount (Whole Sample)



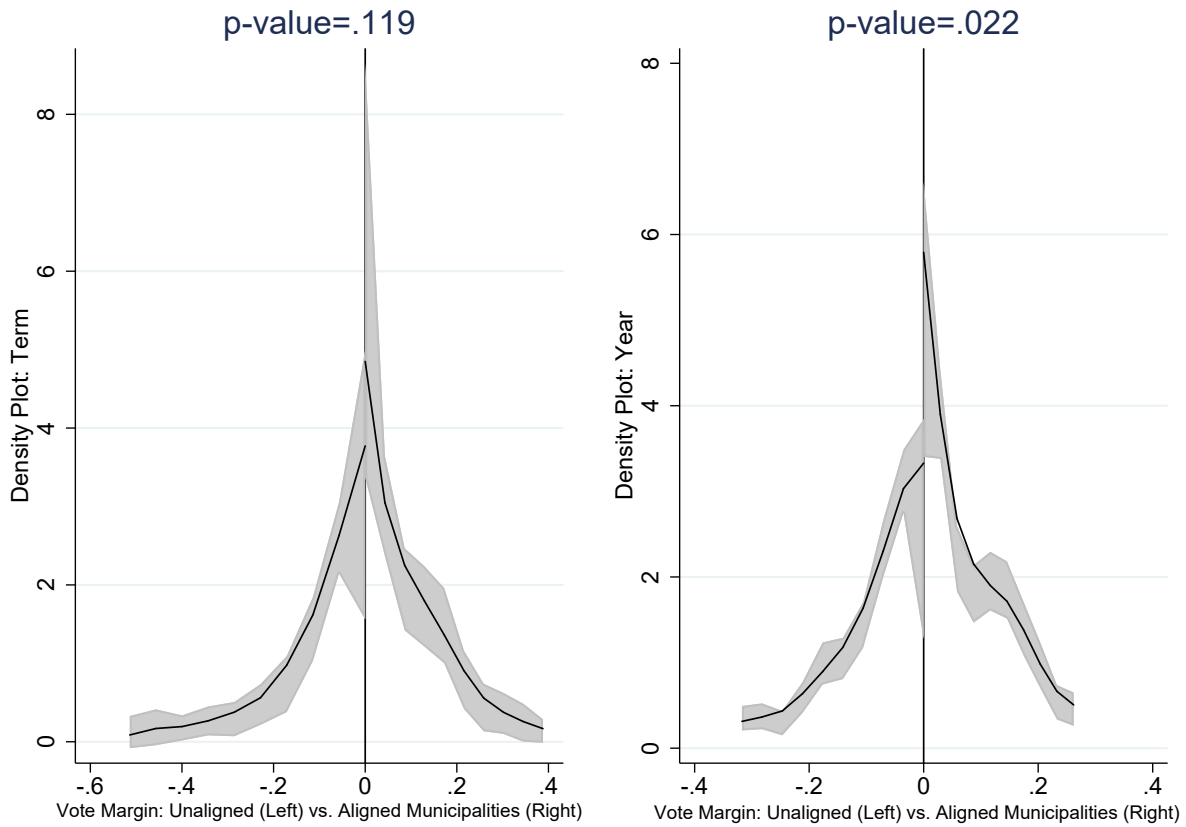
Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

Figure L.11: RDD Density Plots for Infraction Count and Amount (Poverty-Decreasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

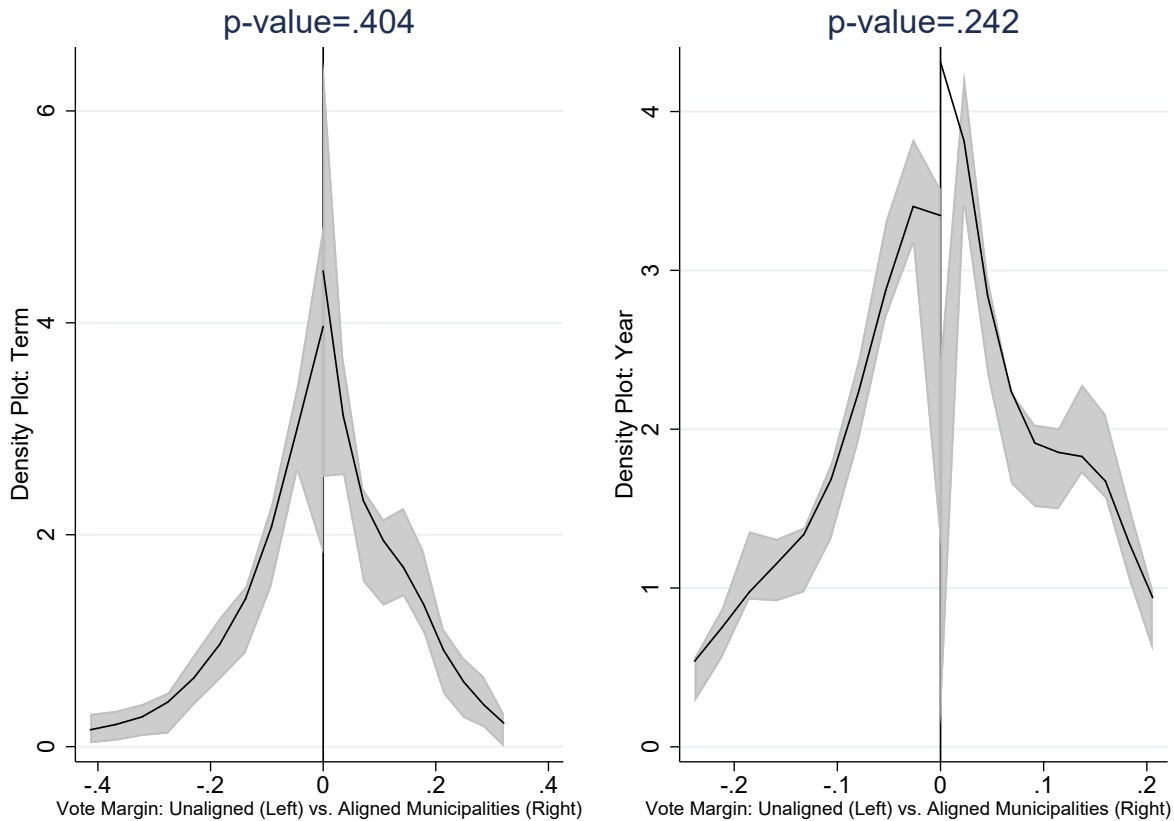
Figure L.12: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. The electoral term are results are not statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis in this sample. The year-wise results for this sample do not pass the [McCrary \(2008\)](#) density tests, indicating a potential problem with using the margin of victory as a running variable for this sample. The above plots provide further evidence via the confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

## L.5. Density Plots for Poverty Increasing/Decreasing Sample: 2008-2015

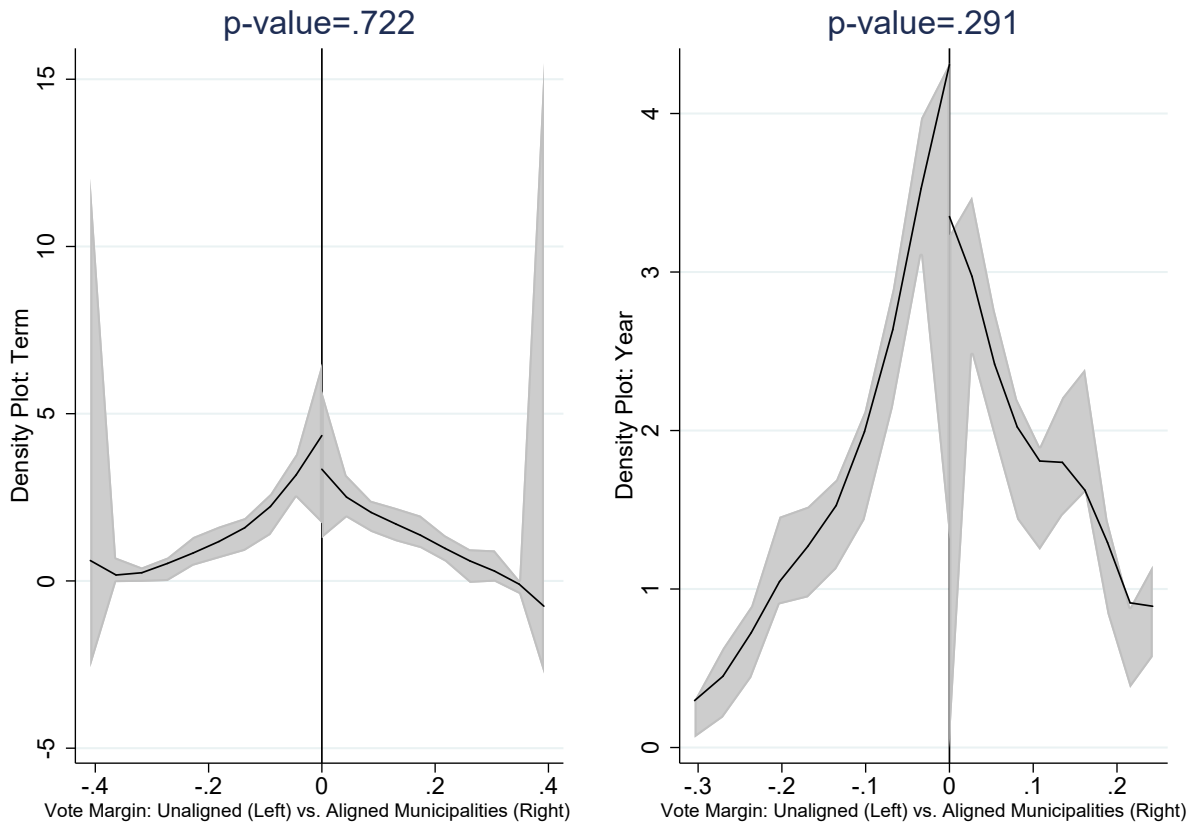
Figure L.13: RDD Density Plots for Infraction Count and Amount (Whole Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

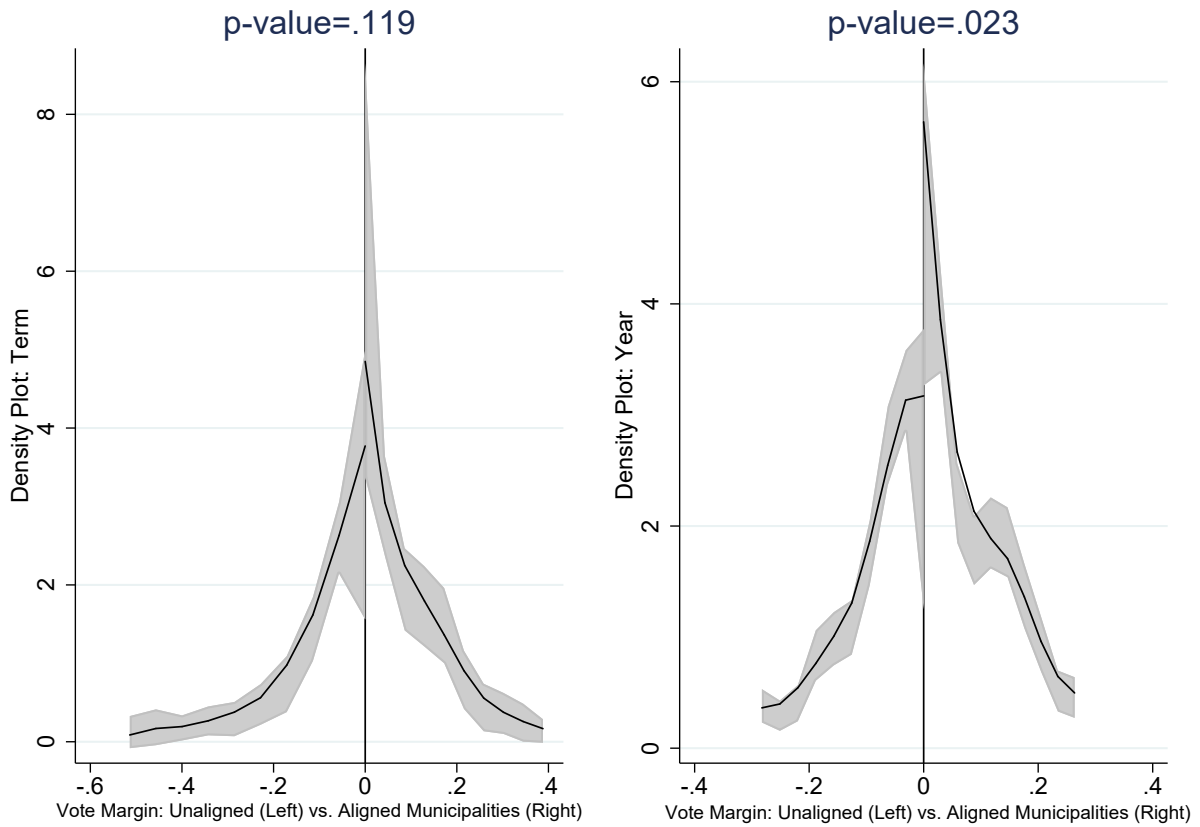


Figure L.14: RDD Density Plots for Infraction Count and Amount (Poverty-Decreasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

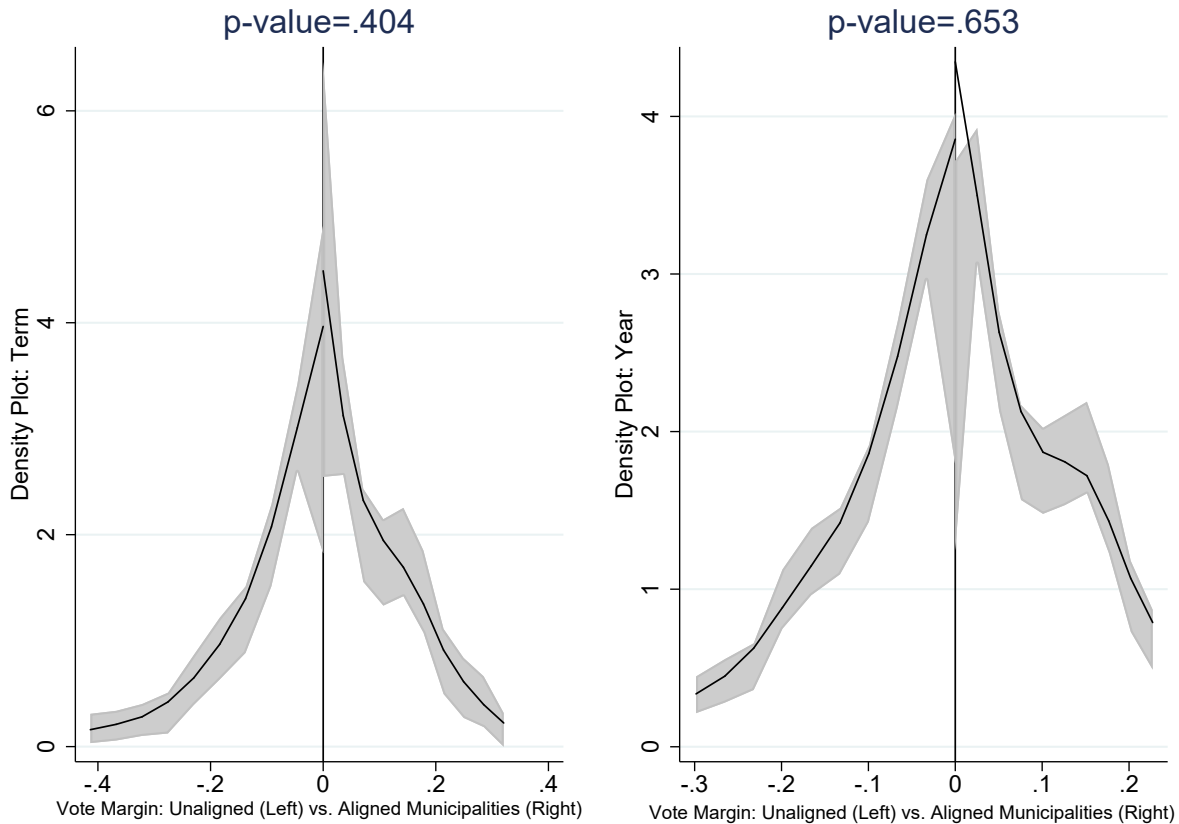
Figure L.15: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. The electoral term are results are not statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis in this sample. The year-wise results for this sample do not pass the [McCrary \(2008\)](#) density tests, indicating a potential problem with using the margin victory as the running for this sample. The above plots provide further evidence via the confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

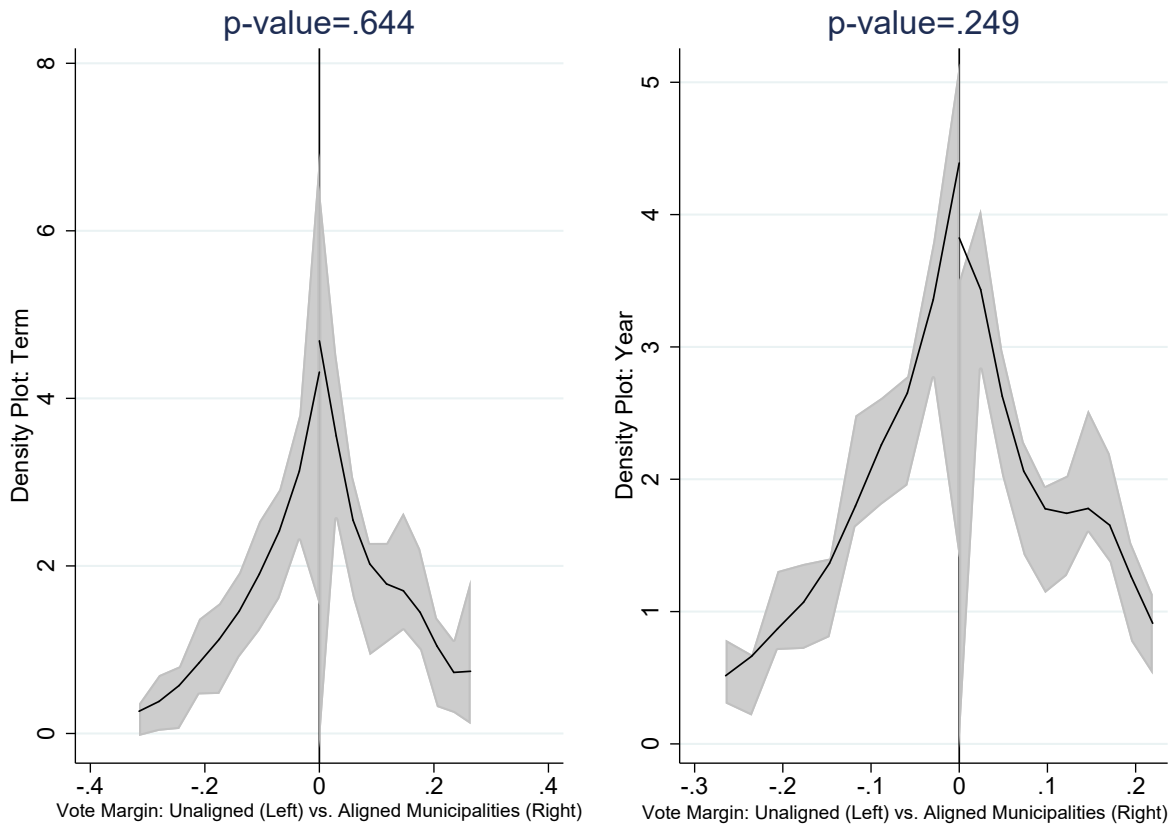
## L.6. Extreme Poverty Density Plots for 2010-2015: Year and Term

Figure L.16: RDD Density Plots for Infraction Count and Amount (Whole Sample)



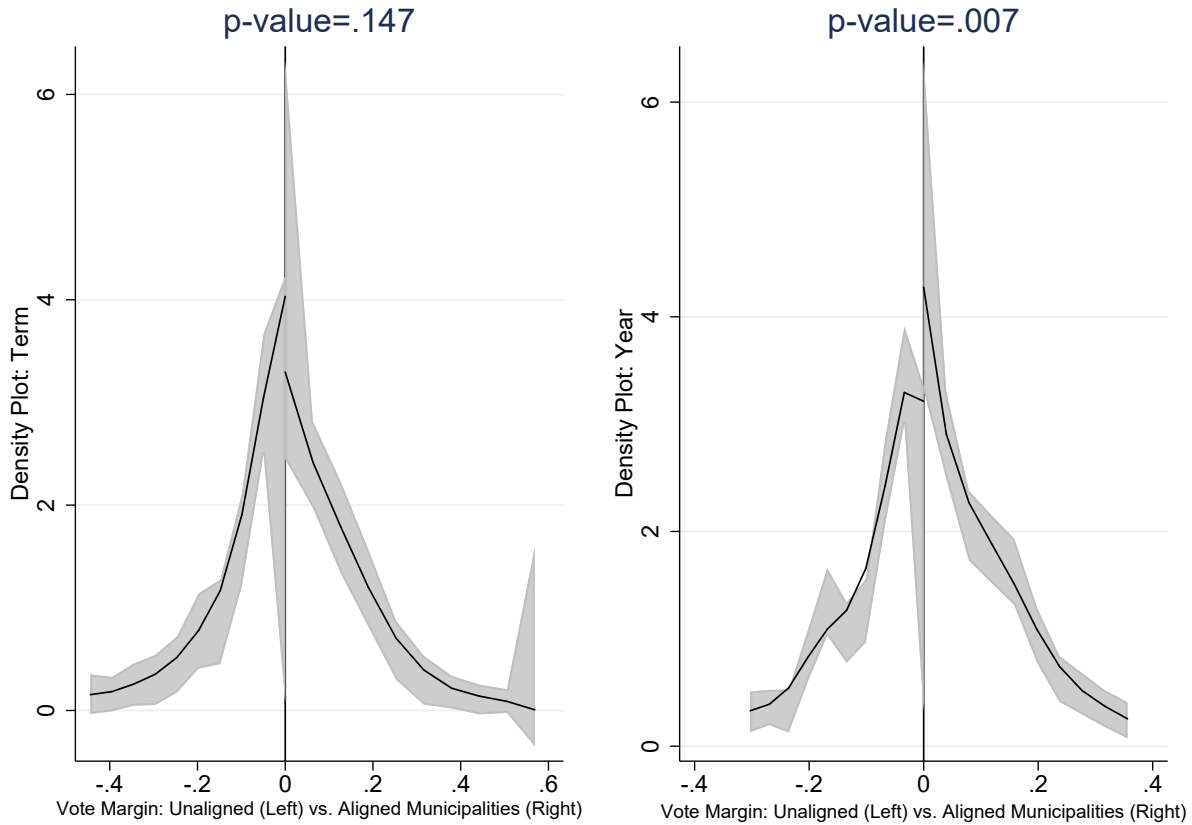
Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

Figure L.17: RDD Density Plots for Infraction Count and Amount (Extreme Poverty-Decreasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. Neither the electoral term nor year results are statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis. The above plots provide further evidence via the overlapping confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

Figure L.18: RDD Density Plots for Infraction Count and Amount (Extreme Poverty-Increasing Sample)



Note: “Term” refers to the margin of victory for mayors in each electoral term. “Year” refers to the same margin of victory variable but corresponding to a year-wise perspective. Following [Cattaneo, Jansson and Ma \(2018\)](#), all [McCrary \(2008\)](#) density tests are fit with second-order polynomials. The electoral term are results are not statistically significant at the conventional threshold ( $p < .05$ ), indicating that the running variable, margin of victory, is suitable for regression discontinuity analysis in this sample. The year-wise results for this sample do not pass the [McCrary \(2008\)](#) density tests, indicating a potential problem with using the margin victory as a running variable for this sample. The above plots provide further evidence via the confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

## L.7. RDD Estimates Eliminating Outliers

### L.7.1. When Poverty is Decreasing

Table L1: RDD Estimates for Infraction Count by Term and Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-10.97*** (3.076)	-14.04*** (4.137)	-8.391** (3.264)	-10.76** (4.199)	-9.023*** (3.359)	-9.964** (3.981)
Observations	192	192	176	176	176	176
Effective Observations	[54,42]	[61,48]	[46,34]	[57,44]	[42,29]	[56,43]
Covariates	None	None	Some	Some	All	All
p-value	0.000363	0.000686	0.0101	0.0104	0.00723	0.0123
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0906	0.105	0.0849	0.112	0.0702	0.105
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.532*** (0.543)	-2.009*** (0.699)	-0.855 (0.539)	-1.202* (0.703)	-1.224** (0.587)	-2.020*** (0.722)
Observations	592	592	560	560	560	560
Effective Observations	[180,138]	[195,139]	[168,126]	[181,129]	[144,102]	[148,104]
Covariates	None	None	Some	Some	All	All
p-value	0.00478	0.00408	0.113	0.0874	0.0370	0.00516
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0985	0.106	0.0959	0.103	0.0788	0.0825

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results for term, while Panel B shows results year. Results are winsorized at top/bottom 1% level. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table L2: RDD Estimates for Infraction Amount (log) by Term and Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.188*** (0.418)	-1.192** (0.542)	-1.081*** (0.385)	-1.024** (0.508)	-1.017*** (0.349)	-1.031** (0.464)
Observations	191	191	176	176	176	176
Effective Observations	[48,39]	[55,45]	[46,34]	[51,38]	[53,43]	[52,40]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00446	0.0278	0.00495	0.0438	0.00357	0.0261
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0762	0.0954	0.0811	0.0903	0.0983	0.0944
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.591*** (0.200)	-0.451* (0.259)	-0.364 (0.225)	-0.342 (0.254)	-0.508** (0.230)	-0.430 (0.272)
Observations	588	588	558	558	558	558
Effective Observations	[187,136]	[171,120]	[130,75]	[160,111]	[142,93]	[186,128]
Covariates	None	None	Some	Some	All	All
p-value	0.00311	0.0819	0.105	0.179	0.0275	0.114
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.0904	0.0646	0.0898	0.0723	0.107

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results for term, while Panel B shows results year. Results are winsorized at top/bottom 1% level. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

**L.7.2. When Poverty is Low**

Table L3: RDD Estimates for Infraction Count by Term and Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-5.363* (2.831)	-5.548 (3.556)	-4.403* (2.568)	-3.244 (3.281)	-6.989*** (2.490)	-8.569*** (2.930)
Observations	282	282	265	265	191	191
Effective Observations	[93,75]	[103,98]	[87,75]	[94,84]	[47,43]	[67,62]
Covariates	None	None	Some	Some	All	All
p-value	0.0582	0.119	0.0864	0.323	0.00499	0.00345
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.113	0.161	0.118	0.139	0.0761	0.136
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.932*** (0.603)	-2.135*** (0.691)	-1.448*** (0.525)	-1.530** (0.599)	-1.064** (0.492)	-1.137* (0.625)
Observations	966	966	903	903	646	646
Effective Observations	[266,228]	[342,317]	[268,241]	[328,333]	[212,189]	[231,228]
Covariates	None	None	Some	Some	All	All
p-value	0.00137	0.00201	0.00581	0.0106	0.0306	0.0688
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0886	0.144	0.103	0.168	0.120	0.152

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results for term, while Panel B shows results year. Results are winsorized at top/bottom 1% level. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table L4: RDD Estimates for Infraction Amount (log) by Term and Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.563* (0.296)	-0.664* (0.349)	-0.525** (0.261)	-0.556* (0.317)	-0.306 (0.287)	-0.442 (0.415)
Observations	282	282	265	265	190	190
Effective observations	[85,73]	[101,89]	[83,71]	[94,86]	[47,44]	[62,52]
Covariates	None	None	Some	Some	All	All
p-value	0.0572	0.0571	0.0443	0.0794	0.286	0.287
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.104	0.139	0.110	0.141	0.0791	0.114
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.605** (0.241)	-0.732** (0.287)	-0.499** (0.229)	-0.603** (0.281)	-0.143 (0.223)	-0.118 (0.267)
Observations	962	962	898	898	641	641
Effective Observations	[238,221]	[317,269]	[241,211]	[294,275]	[196,172]	[227,217]
Covariates	None	None	Some	Some	All	All
p-value	0.0121	0.0107	0.0294	0.0318	0.523	0.658
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0766	0.118	0.0895	0.124	0.103	0.145

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results for term, while Panel B shows results year. Results are winsorized at top/bottom 1% level. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## L.8. RDD Estimates at Varying Cutoffs (Placebo Tests)

### L.8.1. When Poverty is Decreasing

Table L5: RDD Estimates for Infraction Count and Amount (log) by Term

Panel A	(-5%)	(5%)	(-10%)	(10%)	(-15%)	(15%)
RD Estimate	1.956 (5.234)	-9.964 (6.869)	-2.710 (5.727)	-13.20*** (4.559)	0.322 (7.178)	4.946 (6.476)
Observations	195	195	195	195	195	195
Effective Observations	[44,77]	[42,22]	[35,80]	[44,28]	[24,66]	[18,16]
Covariates	None	None	None	None	None	None
Conventional p-value	0.709	0.147	0.636	0.00378	0.964	0.445
Order of Polynomial	2	2	2	2	2	2
Bandwidth	0.133	0.0693	0.141	0.0898	0.141	0.0545
Panel B	(-5%)	(5%)	(-10%)	(10%)	(-15%)	(15%)
RD Estimate	0.104 (0.517)	-1.037* (0.614)	-0.791 (0.604)	-0.688 (0.657)	-2.054 (2.228)	0.837 (0.815)
Observations	195	195	195	195	195	195
Effective Observations	[36,66]	[60,27]	[32,60]	[40,26]	[17,26]	[20,16]
Covariates	None	None	None	None	None	None
Conventional p-value	0.840	0.0916	0.190	0.295	0.356	0.305
Order of Polynomial	2	2	2	2	2	2
Bandwidth	0.101	0.0901	0.108	0.0818	0.0794	0.0561

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results for infraction count, while Panel B shows results infraction amount. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Results are similar when looking at Years and not Terms. Significant effects in Panel A and Panel B were found to be due to the effect of outlier and reduced sample away from cutoff.

**L.8.2. When Poverty is Low**

Table L6: RDD Estimates for Infraction Count and Amount (log) by Term

Panel A	(-5%)	(5%)	(-10%)	(10%)	(-15%)	(15%)
RD Estimate	3.339 (4.211)	8.435 (5.933)	2.210 (5.145)	-1.888 (5.337)	1.996 (7.231)	11.49*** (3.702)
Observations	284	284	284	284	284	284
Effective observations	[66,132]	[133,73]	[34,136]	[105,58]	[16,65]	[42,36]
Covariates	None	None	None	None	None	None
p-value	0.428	0.155	0.668	0.724	0.783	0.00191
Order of Polynomial	2	2	2	2	2	2
Bandwidth	0.179	0.154	0.165	0.137	0.109	0.0883
Panel B	(-5%)	(5%)	(-10%)	(10%)	(-15%)	(15%)
RD Estimate	0.739** (0.376)	-0.876 (0.561)	0.336 (0.466)	-0.778 (0.789)	0.0217 (0.577)	0.521 (0.491)
Observations	284	284	284	284	284	284
Effective observations	[60,112]	[95,47]	[34,136]	[81,50]	[17,73]	[47,38]
Covariates	None	None	None	None	None	None
p-value	0.0495	0.119	0.472	0.324	0.970	0.289
Order of Polynomial	2	2	2	2	2	2
Bandwidth	0.139	0.0997	0.161	0.110	0.117	0.0971

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results for infraction count, while Panel B shows results infraction amount. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Results are similar when looking at Years and not Terms. Significant effects in Panel A and Panel B were found to be due to the effect of outlier and reduced sample away from cutoff.

## L.9. RDD Estimates for Number of Audits in a Term

### L.9.1. For Poverty Decreasing/Increasing Sample

Table L7: RDD Estimates for the Poverty-Decreasing Sample

	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0 (6.42e-09)	0 (9.83e-09)	0.0251 (0.0188)	-0 (5.69e-09)	0 (1.73e-09)	-0.00990 (0.0131)
Observations	195	195	179	179	179	179
Effective observations	[37,29]	[59,48]	[66,49]	[53,42]	[53,43]	[57,44]
Covariates	None	None	Some	Some	All	All
p-value	1	1	0.181	1	1	0.449
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0505	0.102	0.137	0.0972	0.0988	0.109

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Variable of interest is the number of times a municipality gets audited in the term. All specifications use standard errors clustered by municipality, and term fixed effects. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Columns (1) and (2) do not use any additional covariates, (3) and (4) use log of population and dummy for reelection, while (5) and (6) use log of population, dummy for reelection and log of real public good spending (per capita). Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order.

Table L8: RDD Estimates for Poverty-Increasing Sample

	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.0690 (0.0700)	0.0969 (0.0971)	0.0685 (0.0699)	0.0965 (0.0972)	0.0711 (0.0699)	0.0936 (0.0928)
Observations	196	196	196	196	196	196
Effective observations	[57,69]	[63,83]	[57,69]	[63,82]	[57,69]	[67,84]
Covariates	None	None	Some	Some	All	All
p-value	0.324	0.319	0.327	0.321	0.309	0.313
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.123	0.158	0.123	0.158	0.123	0.165

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Variable of interest is the number of times a municipality gets audited in the term. All specifications use standard errors clustered by municipality, and term fixed effects. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Columns (1) and (2) do not use any additional covariates, (3) and (4) use log of population, while (5) and (6) use log of population and log of real public good spending (per capita). Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order.

Table L9: RDD Estimates for Whole Sample

	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.0387 (0.0338)	0.0528 (0.0439)	0.00824 (0.00685)	0.00643 (0.00406)	0.00239 (0.00193)	0.00643* (0.00360)
Observations	441	441	399	399	399	399
Effective observations	[130,117]	[157,165]	[132,129]	[139,150]	[117,108]	[137,148]
Covariates	None	None	Some	Some	All	All
p-value	0.252	0.229	0.229	0.113	0.215	0.0740
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.106	0.159	0.133	0.156	0.106	0.154

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Variable of interest is the number of times a municipality gets audited in the term. All specifications use standard errors clustered by municipality, and term fixed effects. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Columns (1) and (2) do not use any additional covariates, (3) and (4) use log of population and dummy for reelection, while (5) and (6) use log of population, dummy for reelection and log of real public good spending (per capita). Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order.

**L.9.2. For Poverty Low/High Sample**

Table L10: RDD Estimates for the Low Poverty Sample

	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.130 (0.111)	0.159 (0.150)	-0.0155 (0.0114)	-0.0251 (0.0169)	0 (0)	0 (0)
Observations	284	284	267	267	192	192
Effective observations	[85,73]	[101,95]	[60,59]	[73,65]	[50,45]	[53,45]
Covariates	None	None	Some	Some	All	All
p-value	0.241	0.289	0.174	0.136	0.991	0.391
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.150	0.0738	0.0920	0.0844	0.0885

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Variable of interest is the number of times a municipality gets audited in the term. All specifications use standard errors clustered by municipality, and term fixed effects. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Columns (1) and (2) do not use any additional covariates, (3) and (4) use log of population and dummy for reelection, while (5) and (6) use log of population, dummy for reelection and log of real public good spending (per capita). Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order.

Table L11: RDD Estimates for High Poverty Sample

	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.0617 (0.0869)	0.00677 (0.118)	0.0458 (0.0863)	-0.0120 (0.119)	-0.0180 (0.0502)	-0.0365 (0.0252)
Observations	258	258	258	258	207	207
Effective observations	[82,77]	[79,73]	[82,77]	[78,73]	[62,66]	[50,50]
Covariates	None	None	Some	Some	All	All
p-value	0.478	0.954	0.595	0.920	0.721	0.146
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.129	0.119	0.129	0.117	0.127	0.0903

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Variable of interest is the number of times a municipality gets audited in the term. All specifications use standard errors clustered by municipality, and term fixed effects. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Columns (1) and (2) do not use any additional covariates, (3) and (4) use log of population, while (5) and (6) use log of population and log of real public good spending (per capita). Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order.



Table L12: RDD Estimates for Whole Sample

	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.0860 (0.0651)	0.110 (0.0722)	0.0474 (0.0458)	0.0596 (0.0541)	-0.00646 (0.0233)	0.000883 (0.0134)
Observations	568	568	523	523	399	399
Effective observations	[177,154]	[238,239]	[173,158]	[216,218]	[123,117]	[83,77]
Covariates	None	None	Some	Some	All	All
p-value	0.187	0.129	0.300	0.271	0.781	0.947
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.114	0.216	0.129	0.206	0.119	0.0680

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Variable of interest is the number of times a municipality gets audited in the term. All specifications use standard errors clustered by municipality, and term fixed effects. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Columns (1) and (2) do not use any additional covariates, (3) and (4) use log of population and dummy for reelection, while (5) and (6) use log of population, dummy for reelection and log of real public good spending (per capita). Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order.

## M. Potential Endogeneity between Poverty and Corruption

### M.1. Regression of Poverty Rate on Corruption

#### M.1.1. For Poverty Decreasing/Increasing Sample

Table M1: Term-wise Regression of Poverty Rate on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.00908 (0.0546)	0.0100 (0.0544)	0.0432 (0.0470)
Population (log)		3.609 (14.55)	11.62 (16.32)
Public Good Spending per capita (log)		0.216** (0.0876)	0.261*** (0.0966)
Constant	72.60*** (0.642)	34.39 (148.5)	-47.96 (166.9)
Observations	632	632	566
R-squared	0.275	0.276	0.297
Number of Municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Term FE	Yes	Yes	Yes
Electoral Controls	No	No	Yes

Note: Standard errors in parentheses; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. All specifications use standard errors clustered by municipality. Dependent variable is the average total poverty rate in the municipality in the given term. All columns use baseline Term and Municipality fixed-effects. Column (2) includes log of population and log of per capita real public goods spending as covariates. Column (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M2: Term-wise Regression of Poverty Rate on Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	0.290 (0.344)	0.253 (0.371)	0.154 (0.418)
Population (log)		3.198 (14.65)	9.758 (16.57)
Public Good Spending per capita (log)		0.147 (0.178)	0.219 (0.177)
Constant	69.33*** (3.961)	36.17 (149.2)	-30.27 (169.3)
Observations	632	632	566
R-squared	0.277	0.277	0.295
Number of Municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Term FE	Yes	Yes	Yes
Electoral Controls	No	No	Yes

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . All specifications use standard errors clustered by municipality. Dependent variable is the average total poverty rate in the municipality in the given term. Infraction amount (log) is the log of real infraction in the term. All columns use baseline Term and Municipality fixed-effects. Column (2) includes log of population and log of per capita real public goods spending as covariates. Column (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M3: Year-wise Regression of Poverty Rate on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.0465 (0.0807)	0.0509 (0.0808)	0.0677 (0.0759)
Population (log)		4.819 (13.14)	0.272 (14.16)
Public Good Spending per capita (log)		-0.138** (0.0669)	-0.0990 (0.0701)
Constant	66.52*** (0.923)	18.16 (133.9)	62.78 (144.2)
Observations	1,819	1,819	1,694
R-squared	0.016	0.016	0.027
Number of Municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Electoral Controls	No	No	Yes

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . All specifications use standard errors clustered by municipality. Dependent variable is the total poverty rate in the municipality in the given year. All columns use baseline Year and Municipality fixed-effects. Column (2) includes log of population and log of per capita real public goods spending as covariates. Column (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M4: Year-wise Regression of Poverty Rate on Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	0.217 (0.179)	0.238 (0.182)	0.117 (0.179)
Population (log)		4.373 (13.16)	-0.381 (14.27)
Public Good Spending per capita (log)		-0.267** (0.113)	-0.189 (0.156)
Constant	64.47*** (1.933)	21.19 (134.0)	68.93 (145.2)
Observations	1,814	1,814	1,689
R-squared	0.017	0.018	0.027
Number of Municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Electoral Controls	No	No	Yes

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . All specifications use standard errors clustered by municipality. Dependent variable is the total poverty rate in the municipality in the given year. Infraction amount (log) is the log of real infraction in the year. All columns use baseline Year and Municipality fixed-effects. Column (2) includes log of population and log of per capita real public goods spending as covariates. Column (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

**M.1.2. For Poverty Low/High Sample**

Table M5: Term-wise Regression of Poverty Rate on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.0907 (0.0623)	0.0377 (0.0408)	0.0531 (0.0387)
Population (log)		0.177 (11.14)	4.154 (12.28)
Public Good Spending per capita (log)		-3.733 (2.739)	-5.189* (2.886)
Constant	64.33*** (0.753)	98.84 (121.3)	69.19 (132.5)
Observations	963	632	566
R-squared	0.146	0.281	0.305
Number of Municipalities	333	333	327
Municipality FE	YES	YES	YES
Term FE	YES	YES	YES
Electoral Controls			YES

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . All specifications use standard errors clustered by municipality. Dependent variable is the average total poverty rate in the municipality in the given term. All columns use baseline Term and Municipality fixed-effects. Column (2) includes log of population and log of per capita real public goods spending as covariates. Column (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M6: Term-wise Regression of Poverty Rate on Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	-0.301 (0.317)	0.524 (0.442)	0.386 (0.518)
Population (log)		-0.645 (11.18)	2.496 (12.50)
Public Good Spending per capita (log)		-3.857 (2.721)	-5.084* (2.981)
Constant	68.95*** (3.939)	102.1 (121.3)	81.01 (134.4)
Observations	963	632	566
R-squared	0.143	0.283	0.304
Number of Municipalities	333	333	327
Municipality FE	YES	YES	YES
Term FE	YES	YES	YES
Electoral Controls			YES

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . All specifications use standard errors clustered by municipality. Dependent variable is the average total poverty rate in the municipality in the given term. Infraction amount (log) is the log of real infraction in the term. All columns use baseline Term and Municipality fixed-effects. Column (2) includes log of population and log of per capita real public goods spending as covariates. Column (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M7: Year-wise Regression of Poverty Rate on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.0731 (0.109)	0.143 (0.138)	0.163 (0.145)
log-pop		7.300 (18.38)	0.541 (20.78)
Public Good Spending per capita (log)		-0.304 (0.216)	-0.291 (0.277)
Constant	65.31*** (0.647)	-7.204 (186.7)	59.25 (211.2)
Observations	3,121	2,177	1,929
R-squared	0.016	0.019	0.030
Number of Municipalities	333	333	327
Municipality FE	YES	YES	YES
Year FE	YES	YES	YES
Electoral Controls			YES

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . All specifications use standard errors clustered by municipality. Dependent variable is the total poverty rate in the municipality in the given year. All columns use baseline Year and Municipality fixed-effects. Column (2) includes log of population and log of per capita real public goods spending as covariates. Column (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.



Table M8: Year-wise Regression of Poverty Rate on Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	0.0627 (0.116)	0.418** (0.208)	0.325 (0.221)
log_pop		7.138 (18.36)	0.0528 (20.89)
Public Good Spending per capita (log)		-0.385 (0.238)	-0.376 (0.325)
Constant	64.84*** (1.331)	-9.130 (186.7)	61.68 (212.4)
Observations	3,115	2,172	1,924
R-squared	0.015	0.020	0.030
Number of Municipalities	333	333	327
Municipality FE	YES	YES	YES
Year FE	YES	YES	YES
Electoral Controls			YES

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . All specifications use standard errors clustered by municipality. Dependent variable is the total poverty rate in the municipality in the given year. Infraction amount (log) is the log of real infraction in the year. All columns use baseline Year and Municipality fixed-effects. Column (2) includes log of population and log of per capita real public goods spending as covariates. Column (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

## M.2. Two-Stage Regression of Residuals on Corruption

### M.2.1. For Poverty Decreasing/Increasing Sample

Table M9: Term-wise Regression of Residuals on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.00252 (0.0296)	0.00276 (0.0296)	0.0116 (0.0288)
Constant	-0.0447 (0.525)	-0.0491 (0.525)	-0.218 (0.538)
Observations	632	632	566
R-squared	0.000	0.000	0.001
Number of Municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Term FE	Yes	Yes	Yes
Controls	No	Some	All

Note: Standard errors in parentheses; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Results here show the second stage regression result of residuals on infraction count. Residuals from the first stage are obtained by regressing average total poverty in a term on covariates. All three specifications included Term and Municipality fixed-effects in the first stage. Column (2) includes log population and log of per capita real public good spending. Column (3) adds additional covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M10: Term-wise Regression of Residuals on Log Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	0.200 (0.264)	0.163 (0.263)	0.0993 (0.287)
Constant	-2.411 (3.192)	-1.973 (3.170)	-1.205 (3.487)
Observations	632	632	566
R-squared	0.001	0.001	0.000
Number of Municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	Some	All

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results here show the second stage regression result of residuals on log of real infraction amount. Residuals from the first stage are obtained by regressing average total poverty in a term on covariates. All three specifications included Term and Municipality fixed-effects in the first stage. Column (2) includes log population and log of per capita real public good spending. Column (3) adds additional covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M11: Year-wise Regression of Residuals on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.0401 (0.0689)	0.0434 (0.0688)	0.0564 (0.0659)
Constant	-0.252 (0.418)	-0.271 (0.418)	-0.358 (0.405)
Observations	1,819	1,819	1,694
R-squared	0.000	0.000	0.001
Number of municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	Some	All

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results here show the second stage regression result of residuals on infraction count. Residuals from the first stage are obtained by regressing average total poverty in a year on covariates. All three specifications included Year and Municipality fixed-effects in the first stage. Column (2) includes log population and log of per capita real public good spending. Column (3) adds additional covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M12: Year-wise Regression of Residuals on Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	0.196 (0.165)	0.206 (0.164)	0.0988 (0.160)
Constant	-2.119 (1.775)	-2.223 (1.766)	-1.075 (1.724)
Observations	1,814	1,814	1,689
R-squared	0.001	0.001	0.000
Number of municipality	333	333	327
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	No	Some	All

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results here show the second stage regression result of residuals on log of real infraction amount. Residuals from the first stage are obtained by regressing average total poverty in a year on covariates. All three specifications included Year and Municipality fixed-effects in the first stage. Column (2) includes log population and log of per capita real public good spending. Column (3) adds additional covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

### M.2.2. For Poverty Low/High Sample

Table M13: Term-wise Regression of Residuals on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.0338 (0.0441)	0.0308 (0.0370)	0.0431 (0.0340)
Constant	-0.619 (0.808)	-0.718 (0.862)	-1.027 (0.810)
Observations	963	632	566
R-squared	0.002	0.002	0.003
Number of Municipalities	333	333	327
Municipality FE	YES	YES	YES
Term FE	YES	YES	YES
Controls	NO	SOME	ALL

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results here show the second stage regression result of residuals on infraction count. Residuals from the first stage are obtained by regressing average total poverty in a term on covariates. All three specifications included Term and Municipality fixed-effects in the first stage. Column (2) includes log population and log of per capita real public good spending. Column (3) adds additional covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M14: Term-wise Regression of Residuals on Log Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	-0.284 (0.290)	0.516 (0.426)	0.374 (0.484)
Constant	3.706 (3.776)	-6.800 (5.612)	-4.932 (6.387)
Observations	963	632	566
R-squared	0.001	0.005	0.002
Number of Municipalities	333	333	327
Municipality FE	YES	YES	YES
Term FE	YES	YES	YES
Controls	NO	SOME	ALL

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results here show the second stage regression result of residuals on log of real infraction amount. Residuals from the first stage are obtained by regressing average total poverty in a term on covariates. All three specifications included Term and Municipality fixed-effects in the first stage. Column (2) includes log population and log of per capita real public good spending. Column (3) adds additional covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

Table M15: Year-wise Regression of Residuals on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.0789 (0.104)	0.141 (0.136)	0.159 (0.146)
Constant	-0.476 (0.611)	-0.790 (0.748)	-0.907 (0.814)
Observations	2,476	2,177	1,929
R-squared	0.001	0.002	0.002
Number of Municipalities	333	333	327
Municipality FE	YES	YES	YES
Year FE	YES	YES	YES
Controls	NO	SOME	ALL

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results here show the second stage regression result of residuals on infraction count. Residuals from the first stage are obtained by regressing average total poverty in a year on covariates. All three specifications included Year and Municipality fixed-effects in the first stage. Column (2) includes log population and log of per capita real public good spending. Column (3) adds additional covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.



Table M16: Year-wise Regression of Residuals on Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	0.305 (0.189)	0.396** (0.197)	0.299 (0.206)
Constant	-3.485 (2.153)	-4.485** (2.227)	-3.402 (2.326)
Observations	2,471	2,172	1,924
R-squared	0.002	0.003	0.002
Number of Municipalities	333	333	327
Municipality FE	YES	YES	YES
Year FE	YES	YES	YES
Controls	NO	SOME	ALL

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Results here show the second stage regression result of residuals on log of real infraction amount. Residuals from the first stage are obtained by regressing average total poverty in a year on covariates. All three specifications included Year and Municipality fixed-effects in the first stage. Column (2) includes log population and log of per capita real public good spending. Column (3) adds additional covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummy for mayor being aligned with national party and dummy for mayor's gender.

## N. Results for 2011-2015

### N.1. Results When Poverty Decreases

Table N1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-1.431** (0.614)	-1.458* (0.777)	-0.536 (0.600)	-0.544 (0.794)	-0.788 (0.609)	-1.209 (0.831)
Observations	513	513	497	497	497	497
Effective Observations	[159,110]	[177,116]	[151,92]	[155,106]	[151,92]	[141,87]
Covariates	None	None	Some	Some	All	All
p-value	0.0197	0.0605	0.371	0.493	0.196	0.146
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0971	0.112	0.0912	0.0978	0.0922	0.0874
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-1.130* (0.664)	-1.192 (0.844)	-0.337 (0.645)	-0.345 (0.833)	-0.597 (0.682)	-1.088 (0.887)
Observations	513	513	497	497	497	497
Effective Observations	[155,102]	[181,117]	[143,87]	[164,106]	[143,92]	[143,87]
Covariates	None	None	Some	Some	All	All
p-value	0.0887	0.158	0.602	0.679	0.381	0.220
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0945	0.115	0.0888	0.103	0.0898	0.0890

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table N2: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-12.58*** (3.767)	-15.62*** (5.100)	-9.410** (4.142)	-11.76** (5.457)	-5.226* (3.146)	-6.466* (3.749)
Observations	195	195	179	179	179	179
Effective Observations	[57,48]	[67,53]	[48,36]	[59,46]	[46,35]	[57,44]
Covariates	None	None	Some	Some	All	All
p-value	0.000837	0.00219	0.0231	0.0311	0.0967	0.0846
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0990	0.125	0.0884	0.116	0.0826	0.110
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-3.000 (2.053)	-3.570 (2.724)	-0.873 (2.141)	-1.569 (2.905)	-1.190 (2.257)	-3.025 (3.113)
Observations	195	195	179	179	179	179
Effective Observations	[54,41]	[63,51]	[48,36]	[57,44]	[47,35]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.144	0.190	0.683	0.589	0.598	0.331
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0892	0.113	0.0885	0.105	0.0852	0.0935

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table N3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.643***	-0.473	-0.465*	-0.387	-0.527*	-0.459
RD_Estimate	-0.633** (0.248)	-0.476 (0.329)	-0.465* (0.269)	-0.387 (0.333)	-0.535* (0.275)	-0.454 (0.341)
Observations	510	510	494	494	494	494
Effective Observations	[159,111]	[155,110]	[128,85]	[155,106]	[129,85]	[172,112]
Covariates	None	None	Some	Some	All	All
p-value	0.0106	0.148	0.0841	0.245	0.0518	0.183
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0991	0.0961	0.0759	0.0995	0.0798	0.114
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.687*** (0.243)	-0.526 (0.325)	-0.485* (0.270)	-0.472 (0.325)	-0.543** (0.276)	-0.530 (0.336)
Observations	510	510	494	494	494	494
Effective Observations	[167,111]	[155,97]	[128,83]	[151,92]	[128,85]	[155,106]
Covariates	None	None	Some	Some	All	All
p-value	0.00475	0.105	0.0725	0.146	0.0495	0.115
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.0938	0.0741	0.0939	0.0758	0.0981

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table N4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-1.492*** (0.502)	-1.306* (0.678)	-1.215*** (0.426)	-1.200* (0.619)	-0.861** (0.395)	-0.975* (0.505)
Observations	195	195	179	179	179	179
Effective Observations	[49,39]	[51,40]	[52,38]	[47,35]	[56,43]	[46,35]
Covariates	None	None	Some	Some	All	All
p-value	0.00293	0.0542	0.00431	0.0527	0.0293	0.0537
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0772	0.0860	0.0931	0.0864	0.103	0.0846
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.783** (0.370)	-0.558 (0.544)	-0.597 (0.389)	-0.481 (0.566)	-0.687* (0.383)	-0.619 (0.565)
Observations	195	195	179	179	179	179
Effective Observations	[57,48]	[55,43]	[53,42]	[52,38]	[56,44]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.0341	0.305	0.125	0.395	0.0727	0.274
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0981	0.0907	0.0969	0.0931	0.104	0.0943

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## N.2. Results When Poverty Increases

Table N5: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.717 (1.062)	1.155 (1.927)	0.450 (1.389)	1.251 (2.235)	0.497 (1.381)	1.174 (2.130)
Observations	517	517	495	495	495	495
Effective Observations	[120,163]	[126,193]	[92,137]	[114,164]	[96,137]	[115,177]
Covariates	None	None	Some	Some	All	All
p-value	0.499	0.549	0.746	0.576	0.719	0.581
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.114	0.124	0.0856	0.116	0.0863	0.120
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.600 (1.084)	0.899 (1.908)	0.466 (1.395)	1.182 (2.224)	0.536 (1.387)	0.975 (1.971)
Observations	517	517	495	495	495	495
Effective Observations	[120,162]	[126,193]	[92,137]	[114,168]	[96,137]	[115,190]
Covariates	None	None	Some	Some	All	All
p-value	0.580	0.638	0.738	0.595	0.699	0.621
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.113	0.125	0.0852	0.116	0.0860	0.125

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table N6: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	5.162 (4.621)	8.191 (6.961)	0.791 (5.001)	2.865 (7.448)	-3.459 (4.541)	-1.699 (7.943)
Observations	196	196	174	174	174	174
Effective Observations	[54,58]	[57,73]	[43,55]	[46,71]	[43,52]	[44,56]
Covariates	None	None	Some	Some	All	All
p-value	0.264	0.239	0.874	0.700	0.446	0.831
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.104	0.132	0.101	0.133	0.0954	0.109
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	1.258 (3.039)	1.534 (4.616)	1.182 (3.710)	2.385 (5.300)	0.327 (3.846)	1.961 (5.141)
Observations	196	196	174	174	174	174
Effective Observations	[54,60]	[59,77]	[41,52]	[46,67]	[40,50]	[46,71]
Covariates	None	None	Some	Some	All	All
p-value	0.679	0.740	0.750	0.653	0.932	0.703
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.111	0.139	0.0913	0.128	0.0891	0.132

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table N7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.248 (0.347)	0.0581 (0.459)	0.177 (0.367)	-0.472 (0.670)	0.193 (0.366)	-0.476 (0.669)
Observations	515	515	493	493	493	493
Effective Observations	[123,167]	[162,227]	[108,154]	[109,155]	[109,154]	[109,155]
Covariates	None	None	Some	Some	All	All
p-value	0.475	0.899	0.630	0.481	0.597	0.477
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.166	0.103	0.109	0.104	0.109
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.249 (0.348)	-0.0624 (0.489)	0.189 (0.366)	-0.477 (0.671)	0.215 (0.365)	-0.480 (0.672)
Observations	515	515	493	493	493	493
Effective Observations	[123,167]	[146,216]	[109,154]	[109,155]	[109,154]	[109,155]
Covariates	None	None	Some	Some	All	All
p-value	0.474	0.898	0.606	0.477	0.556	0.475
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.155	0.104	0.109	0.105	0.110

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table N8: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.463 (0.533)	0.236 (0.679)	0.0207 (0.554)	-0.183 (0.713)	-0.0501 (0.448)	-0.00795 (0.581)
Observations	196	196	174	174	174	174
Effective Observations	[48,52]	[57,75]	[38,50]	[46,73]	[44,55]	[55,81]
Covariates	None	None	Some	Some	All	All
p-value	0.385	0.729	0.970	0.798	0.911	0.989
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0820	0.134	0.0853	0.136	0.106	0.165
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.219 (0.426)	-0.603 (0.714)	0.585 (0.371)	-0.394 (0.703)	0.621* (0.370)	-0.441 (0.696)
Observations	196	196	174	174	174	174
Effective Observations	[53,56]	[53,58]	[47,73]	[44,58]	[47,73]	[44,57]
Covariates	None	None	Some	Some	All	All
p-value	0.608	0.398	0.115	0.575	0.0933	0.526
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0961	0.103	0.142	0.114	0.141	0.111

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## O. Results for 2009-2015

### O.1. Results When Poverty Decreases

Table O1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-1.865*** (0.566)	-2.038*** (0.647)	-0.836 (0.558)	-1.049 (0.705)	-0.877 (0.535)	-1.495** (0.753)
Observations	687	687	639	639	639	639
Effective Observations	[187,139]	[261,189]	[171,121]	[204,150]	[189,138]	[175,124]
Covariates	None	None	Some	Some	All	All
p-value	0.000993	0.00164	0.134	0.137	0.101	0.0473
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0873	0.139	0.0858	0.103	0.0956	0.0875
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-1.299** (0.614)	-1.294* (0.673)	-0.480 (0.605)	-0.613 (0.774)	-0.593 (0.577)	-1.082 (0.781)
Observations	687	687	639	639	639	639
Effective Observations	[183,136]	[267,238]	[167,121]	[208,150]	[189,138]	[189,131]
Covariates	None	None	Some	Some	All	All
p-value	0.0345	0.0547	0.428	0.429	0.304	0.166
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0868	0.160	0.0840	0.108	0.0957	0.0915

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table O2: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-9.326*** (2.315)	-11.25*** (3.058)	-5.849*** (2.216)	-7.433** (3.096)	-6.417*** (2.259)	-8.011** (3.155)
Observations	195	195	179	179	179	179
Effective Observations	[62,49]	[62,49]	[59,46]	[57,44]	[55,43]	[57,44]
Covariates	None	None	Some	Some	All	All
p-value	5.63e-05	0.000234	0.00830	0.0163	0.00449	0.0111
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.107	0.110	0.115	0.110	0.101	0.108
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-4.786** (2.216)	-5.611** (2.735)	-2.363 (2.180)	-2.769 (2.965)	-2.725 (2.131)	-5.190* (2.876)
Observations	195	195	179	179	179	179
Effective Observations	[56,43]	[70,53]	[52,42]	[57,44]	[52,38]	[50,36]
Covariates	None	None	Some	Some	All	All
p-value	0.0308	0.0403	0.278	0.350	0.201	0.0712
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0924	0.132	0.0965	0.108	0.0938	0.0895

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table O3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.634*** (0.243)	-0.510 (0.322)	-0.321 (0.269)	-0.313 (0.320)	-0.409 (0.279)	-0.384 (0.336)
Observations	684	684	636	636	636	636
Effective Observations	[205,164]	[201,146]	[146,89]	[181,124]	[146,96]	[189,131]
Covariates	None	None	Some	Some	All	All
p-value	0.00911	0.114	0.232	0.327	0.143	0.253
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0998	0.0923	0.0642	0.0892	0.0686	0.0942 height
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.606** (0.244)	-0.475 (0.329)	-0.221 (0.289)	-0.244 (0.347)	-0.276 (0.285)	-0.298 (0.344)
Observations	684	684	636	636	636	636
Effective Observations	[213,164]	[197,146]	[146,89]	[175,124]	[146,89]	[181,124]
Covariates	None	None	Some	Some	All	All
p-value	0.0128	0.149	0.445	0.482	0.333	0.387
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.0909	0.0607	0.0875	0.0633	0.0888

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table O4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.736* (0.394)	-0.600 (0.508)	-0.468 (0.444)	-0.349 (0.513)	-0.575 (0.428)	-0.451 (0.496)
Observations	195	195	179	179	179	179
Effective Observations	[56,47]	[56,45]	[44,32]	[52,40]	[45,34]	[52,42]
Covariates	None	None	Some	Some	All	All
p-value	0.0614	0.238	0.291	0.496	0.179	0.364
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0966	0.0950	0.0732	0.0953	0.0797	0.0965
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.641* (0.388)	-0.452 (0.517)	-0.222 (0.487)	-0.138 (0.567)	-0.364 (0.475)	-0.280 (0.550)
Observations	195	195	179	179	179	179
Effective Observations	[59,48]	[56,43]	[40,28]	[52,40]	[44,32]	[52,40]
Covariates	None	None	Some	Some	All	All
p-value	0.0988	0.383	0.649	0.807	0.443	0.611
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.0938	0.0679	0.0948	0.0731	0.0953

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## O.2. Results When Poverty Increases

Table O5: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.364 (0.868)	0.363 (1.326)	0.192 (0.997)	0.426 (1.466)	0.230 (1.026)	0.457 (1.436)
Observations	692	692	628	628	628	628
Effective Observations	[189,225]	[203,280]	[151,190]	[161,257]	[147,186]	[161,260]
Covariates	None	None	Some	Some	All	All
p-value	0.675	0.784	0.847	0.772	0.823	0.750
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.117	0.148	0.0961	0.133	0.0935	0.136
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.147 (0.939)	0.0928 (1.300)	0.228 (1.025)	0.368 (1.445)	0.238 (1.047)	0.407 (1.426)
Observations	692	692	628	628	628	628
Effective Observations	[182,207]	[207,287]	[147,186]	[161,257]	[144,186]	[161,260]
Covariates	None	None	Some	Some	All	All
p-value	0.875	0.943	0.824	0.799	0.820	0.775
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.108	0.155	0.0935	0.134	0.0920	0.137

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table O6: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	3.284 (2.876)	4.093 (5.435)	0.953 (3.204)	1.501 (5.748)	-1.100 (3.988)	1.156 (5.900)
Observations	196	196	174	174	174	174
Effective Observations	[60,79]	[59,78]	[46,64]	[46,72]	[43,55]	[46,72]
Covariates	None	None	Some	Some	All	All
p-value	0.254	0.451	0.766	0.794	0.783	0.845
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.149	0.143	0.122	0.135	0.103	0.134
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	1.225 (3.432)	1.268 (4.723)	0.804 (3.726)	1.247 (5.148)	0.0901 (3.937)	1.012 (5.131)
Observations	196	196	174	174	174	174
Effective Observations	[54,58]	[61,81]	[42,52]	[46,73]	[40,50]	[47,73]
Covariates	None	None	Some	Some	All	All
p-value	0.721	0.788	0.829	0.809	0.982	0.844
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.107	0.153	0.0932	0.136	0.0900	0.137

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table O7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.295 (0.258)	-0.0951 (0.416)	0.0979 (0.287)	-0.367 (0.455)	0.119 (0.286)	-0.262 (0.429)
Observations	690	690	626	626	626	626
Effective Observations	[202,280]	[202,280]	[153,205]	[160,248]	[153,205]	[164,260]
Covariates	None	None	Some	Some	All	All
p-value	0.254	0.819	0.733	0.420	0.677	0.541
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.146	0.148	0.112	0.131	0.112	0.140
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.250 (0.274)	-0.378 (0.449)	-0.0480 (0.325)	-0.443 (0.484)	-0.0878 (0.357)	-0.379 (0.454)
Observations	690	690	626	626	626	626
Effective Observations	[191,266]	[191,263]	[146,186]	[160,234]	[131,175]	[160,251]
Covariates	None	None	Some	Some	All	All
p-value	0.361	0.400	0.883	0.361	0.806	0.404
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.135	0.132	0.0938	0.123	0.0830	0.132

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table O8: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.475 (0.302)	0.180 (0.517)	0.388 (0.311)	-0.00515 (0.520)	0.387 (0.310)	-0.000242 (0.515)
Observations	196	196	174	174	174	174
Effective Observations	[60,79]	[59,77]	[47,74]	[48,75]	[47,74]	[48,75]
Covariates	None	None	Some	Some	All	All
p-value	0.115	0.728	0.212	0.992	0.211	1
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.148	0.141	0.142	0.151	0.142	0.151
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.384 (0.322)	-0.188 (0.634)	0.305 (0.352)	-0.0460 (0.544)	0.320 (0.348)	-0.0877 (0.544)
Observations	196	196	174	174	174	174
Effective Observations	[57,74]	[57,64]	[46,64]	[47,75]	[46,64]	[47,73]
Covariates	None	None	Some	Some	All	All
p-value	0.233	0.766	0.386	0.933	0.357	0.872
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.133	0.118	0.121	0.144	0.121	0.139

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## P. Results for 2008-2015

### P.1. Results When Poverty Decreases

Table P1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-1.963*** (0.548)	-2.114*** (0.616)	-1.157** (0.523)	-1.394** (0.701)	-1.045* (0.538)	-1.756** (0.758)
Observations	776	776	712	712	712	712
Effective Observations	[224,179]	[296,277]	[224,171]	[228,173]	[228,173]	[200,151]
Covariates	None	None	Some	Some	All	All
p-value	0.000343	0.000593	0.0268	0.0468	0.0522	0.0206
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0946	0.163	0.103	0.108	0.107	0.0899
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-1.375** (0.559)	-1.578** (0.689)	-0.802 (0.546)	-0.890 (0.736)	-0.826 (0.560)	-1.362* (0.774)
Observations	776	776	712	712	712	712
Effective Observations	[228,191]	[284,217]	[224,173]	[228,173]	[228,177]	[208,151]
Covariates	None	None	Some	Some	All	All
p-value	0.0139	0.0219	0.142	0.227	0.140	0.0784
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0995	0.135	0.104	0.111	0.112	0.0935

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table P2: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-8.090*** (2.207)	-9.698*** (2.703)	-4.888** (2.121)	-6.034** (2.862)	-5.559*** (2.141)	-7.524*** (2.835)
Observations	195	195	179	179	179	179
Effective Observations	[58,48]	[69,53]	[59,46]	[57,44]	[53,43]	[54,43]
Covariates	None	None	Some	Some	All	All
p-value	0.000247	0.000334	0.0212	0.0350	0.00942	0.00796
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.129	0.118	0.108	0.0995	0.100
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-5.724** (2.253)	-6.714** (2.844)	-3.531 (2.211)	-3.921 (2.990)	-3.666 (2.306)	-6.608** (2.798)
Observations	195	195	179	179	179	179
Effective Observations	[60,48]	[69,53]	[58,46]	[58,45]	[52,40]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.0111	0.0182	0.110	0.190	0.112	0.0182
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.129	0.113	0.112	0.0957	0.0925

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table P3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.548** (0.233)	-0.423 (0.314)	-0.332 (0.266)	-0.299 (0.316)	-0.362 (0.280)	-0.329 (0.334)
Observations	773	773	709	709	709	709
Effective Observations	[232,191]	[216,163]	[160,103]	[200,143]	[160,103]	[208,151]
Covariates	None	None	Some	Some	All	All
p-value	0.0187	0.178	0.213	0.345	0.196	0.326
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.100	0.0897	0.0629	0.0889	0.0644	0.0921
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.532** (0.232)	-0.393 (0.320)	-0.233 (0.282)	-0.241 (0.332)	-0.267 (0.284)	-0.274 (0.340)
Observations	773	773	709	709	709	709
Effective Observations	[240,193]	[208,163]	[156,103]	[188,139]	[160,103]	[192,143]
Covariates	None	None	Some	Some	All	All
p-value	0.0219	0.220	0.408	0.468	0.348	0.420
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.104	0.0874	0.0590	0.0858	0.0611	0.0878

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table P4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.612* (0.348)	-0.326 (0.472)	-0.394 (0.429)	-0.248 (0.499)	-0.445 (0.413)	-0.326 (0.475)
Observations	195	195	179	179	179	179
Effective Observations	[62,49]	[57,48]	[44,31]	[53,43]	[45,34]	[52,42]
Covariates	None	None	Some	Some	All	All
p-value	0.0792	0.490	0.358	0.620	0.281	0.493
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.110	0.0992	0.0721	0.0995	0.0763	0.0962
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.645* (0.349)	-0.442 (0.462)	-0.354 (0.439)	-0.270 (0.499)	-0.409 (0.429)	-0.319 (0.480)
Observations	195	195	179	179	179	179
Effective Observations	[62,49]	[56,43]	[40,28]	[52,40]	[43,29]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.0648	0.339	0.420	0.589	0.341	0.507
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.106	0.0940	0.0682	0.0946	0.0706	0.0933

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## P.2. Results When Poverty Increases

Table P5: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.283 (0.800)	0.279 (1.227)	0.137 (0.915)	0.324 (1.346)	0.168 (0.954)	0.374 (1.335)
Observations	781	781	695	695	695	695
Effective Observations	[226,279]	[242,319]	[172,216]	[184,287]	[172,208]	[184,291]
Covariates	None	None	Some	Some	All	All
p-value	0.724	0.820	0.881	0.810	0.860	0.779
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.124	0.153	0.0980	0.134	0.0951	0.136
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.0379 (0.876)	-0.0117 (1.201)	0.178 (0.971)	0.264 (1.312)	0.159 (0.988)	0.284 (1.306)
Observations	781	781	695	695	695	695
Effective Observations	[214,240]	[250,327]	[164,204]	[184,287]	[160,200]	[184,291]
Covariates	None	None	Some	Some	All	All
p-value	0.966	0.992	0.855	0.840	0.872	0.828
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.112	0.156	0.0907	0.134	0.0899	0.137

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table P6: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	2.000 (3.324)	2.333 (5.142)	0.535 (3.647)	1.155 (5.401)	0.0550 (3.905)	-0.424 (5.610)
Observations	196	196	174	174	174	174
Effective Observations	[57,70]	[60,79]	[43,55]	[46,72]	[43,52]	[47,73]
Covariates	None	None	Some	Some	All	All
p-value	0.547	0.650	0.883	0.831	0.989	0.940
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.124	0.150	0.0988	0.135	0.0943	0.139
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.854 (3.622)	0.949 (4.934)	0.697 (3.863)	0.998 (5.280)	-0.148 (4.086)	-1.052 (5.608)
Observations	196	196	174	174	174	174
Effective Observations	[54,59]	[63,82]	[41,52]	[46,72]	[40,50]	[46,73]
Covariates	None	None	Some	Some	All	All
p-value	0.814	0.847	0.857	0.850	0.971	0.851
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.110	0.156	0.0917	0.135	0.0892	0.135

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table P7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.315 (0.244)	-0.136 (0.405)	0.112 (0.275)	-0.258 (0.405)	0.0997 (0.285)	0.0636 (0.368)
Observations	779	779	693	693	693	693
Effective Observations	[237,315]	[233,307]	[175,228]	[187,291]	[175,220]	[219,331]
Covariates	None	None	Some	Some	All	All
p-value	0.196	0.738	0.683	0.525	0.727	0.863
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.149	0.141	0.112	0.139	0.107	0.174
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.196 (0.274)	-0.427 (0.453)	-0.0590 (0.348)	-0.330 (0.408)	-0.0601 (0.342)	0.0592 (0.366)
Observations	779	779	693	693	693	693
Effective Observations	[225,267]	[225,271]	[151,192]	[183,287]	[151,196]	[219,331]
Covariates	None	None	Some	Some	All	All
p-value	0.473	0.345	0.865	0.418	0.860	0.871
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.122	0.123	0.0802	0.134	0.0818	0.174

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without year fixed effects, while Panel B shows results with year fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.



Table P8: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.452 (0.310)	0.111 (0.509)	0.387 (0.326)	0.0247 (0.509)	0.368 (0.327)	-0.00815 (0.510)
Observations	196	196	174	174	174	174
Effective Observations	[57,70]	[57,74]	[46,64]	[47,73]	[46,63]	[47,73]
Covariates	None	None	Some	Some	All	All
p-value	0.145	0.827	0.236	0.961	0.261	0.987
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.125	0.132	0.121	0.139	0.120	0.139
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.412 (0.316)	-0.0330 (0.566)	0.338 (0.349)	0.0141 (0.517)	0.284 (0.357)	-0.0905 (0.528)
Observations	196	196	174	174	174	174
Effective Observations	[57,70]	[57,67]	[44,57]	[47,73]	[44,55]	[46,72]
Covariates	None	None	Some	Some	All	All
p-value	0.192	0.954	0.332	0.978	0.426	0.864
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.124	0.121	0.111	0.138	0.106	0.135

Note: Standard errors in parentheses; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Panel A shows results without term fixed effects, while Panel B shows results with term fixed effects. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that [Calonico, Cattaneo and Titiunik's \(2014\)](#) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per [Gelman and Imbens \(2019\)](#), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

## Q. Corruption Levels for the Poverty-Reducing, Poverty-Increasing, and Whole Samples (Dichotomous View)

### Q.1. Dichotomous Corruption Results for the 2012-2015 Electoral Term

Table Q1: Corrupt Mayors Defined by Count of Infractions (Term 2012-2015)

Panel A	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	26 (57.78%)	19 (42.22%)	45 (100.00%)
Not-Aligned	32 (32.99%)	65 (67.01%)	97 (100.00%)
Panel B	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	25 (39.68%)	38 (60.32%)	63 (100.00%)
Not-Aligned	46 (50.00%)	46 (50.00%)	92 (100.00%)
Panel C	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	54 (45.38%)	65 (54.62%)	119 (100.00%)
Not-Aligned	90 (42.06%)	124 (57.94%)	214 (100.00%)

Note: “Mayor Not Corrupt” and “Mayor Corrupt” are defined as the count of municipalities with the total number of infractions being above/below the median for the 2012-2015 electoral term. Panel A reports the results by alignment status for the poverty-decreasing sample, Panel B presents results by alignment status for the poverty-increasing sample, and Panel C provides the same results but for the whole sample.

Table Q2: Corrupt Mayors Defined by Amount (log) of Infraction for the 2012-2015 Electoral Term

Panel A	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	31 (68.89%)	14 (31.11%)	45 (100.00%)
Not-Aligned	43 (44.33%)	54 (55.67%)	97 (100.00%)
Panel B	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	29 (46.03%)	34 (53.97%)	63 (100.00%)
Not-Aligned	48 (52.17%)	44 (47.83%)	92 (100.00%)
Panel C	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	65 (54.62%)	54 (45.38%)	119 (100.00%)
Not-Aligned	101 (47.20%)	113 (52.80%)	214 (100.00%)

Note: “Mayor Not Corrupt” and “Mayor Corrupt” are defined as the count of municipalities with the log amount of misappropriated money associated with audit infractions being above/below the median for the 2012-2015 electoral term. Panel A reports the results by alignment status for the poverty-decreasing sample, Panel B presents results by alignment status for the poverty-increasing sample, and Panel C provides the same results but for the whole sample.

## Q.2. Dichotomous Corruption Results for the 2008-2011 Electoral Term

Table Q3: Corrupt Mayors Defined by Count of Infraction for the 2008-2011 Electoral Term

Panel A	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	25 (54.35%)	21 (45.65%)	46 (100.00%)
Not-Aligned	44 (45.83%)	52 (54.17%)	96 (100.00%)
Panel B	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	26 (54.17%)	22 (45.83%)	48 (100.00%)
Not-Aligned	47 (43.93%)	60 (56.07%)	107 (100.00%)
Panel C	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	54 (51.92%)	50 (48.08%)	104 (100.00%)
Not-Aligned	107 (46.93%)	121 (53.07%)	228 (100.00%)

Note: “Mayor Not Corrupt” and “Mayor Corrupt” are defined as the count of municipalities with the total number of infractions being above/below the median for the 2008-2011 electoral term. Panel A reports the results by alignment status for the poverty-decreasing sample, Panel B presents results by alignment status for the poverty-increasing sample, and Panel C provides the same results but for the whole sample.

Table Q4: Corrupt Mayors Defined by Amount (log) of Infraction for the 2008-2011 Electoral Term

	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	28 (60.87%)	18 (39.13%)	46 (100.00%)
Not-Aligned	46 (47.92%)	50 (52.08%)	96 (100.00%)
	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	22 (45.83%)	26 (54.17%)	48 (100.00%)
Not-Aligned	54 (50.47%)	53 (49.53%)	107 (100.00%)
	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	52 (50.00%)	52 (50.00%)	104 (100.00%)
Not-Aligned	112 (49.12%)	116 (50.88%)	228 (100.00%)

Note: ‘Mayor Not Corrupt’ and ‘Mayor Corrupt’ are defined as the count of municipalities with the log amount of misappropriated money associated with audit infractions being above/below the median for the 2008-2011 electoral term. Panel A reports the results by alignment status for the poverty-decreasing sample, Panel B presents results by alignment status for the poverty-increasing sample, and Panel C provides the same results but for the whole sample.

## R. Poverty Rates For Different Samples

Table R1: Total Poverty Rates from 2002 & 2011 Waves

Sample	Mean Total Poverty-2002 (%)	Mean Total Poverty-2011 (%)
Whole Sample	63.87 (21.46)	69.51 (16.87)
Whole Sample (including missing 2011)	63.87 (21.46)	65.84 (20.21)
Municipalities Both in 2002 & 2011	67.34 (18.91)	69.51 (16.87)
Municipalities Only in 2002	33.59 (18.55)	NA NA
Poverty-Reducing Sample	76.12 (13.25)	64.72 (15.90)
Poverty-Increasing Sample	59.30 (19.76)	73.75 (16.67)
Low-Poverty Sample	46.30 (15.67)	59.41 (15.97)
High-Poverty Sample	81.34 (7.82)	77.61 (12.72)

Note: Standard deviations are in parentheses. Total poverty rates are from the 2002 and 2011 census. “Whole Sample (including missing 2011)” (row 2) included values from 2002 for the 32 municipalities with missing information in 2011. “Municipalities only in 2002” (row 4) refer to the 34 municipalities that had data in the 2002 census only.

As shown in Tables [R1](#) and [R2](#), the 34 urban municipalities for which there are only poverty and extreme poverty data in 2002 exhibit less poverty and extreme poverty than the 299 other municipalities in the whole sample. Additionally, the literatures on poverty traps (e.g., [Sachs, 2005](#); [Banerjee and Duflo, 2011](#)), clientelism (e.g., [Scott, 1972](#); [Keefer, 2007a](#)), and modernization itself (e.g., [Lerner, 1958](#); [Lipset, 1959, 1960](#); [Rostow, 1960](#); [Gershenkron, 1962](#); [Inglehart and Welzel, 2005](#)) indicate that more rural areas are less likely to undergo modernization processes. In short, the results that we find in this article based on more rural areas are less likely from a theoretical perspective. Accordingly, we conjecture that the inclusion of the missing poverty data from the less-poor, urban municipalities would, if anything, reinforce our results.

In all likelihood, though, the missing data would not change much of anything. First, if the data actually existed (and they do not according to email communication Guatemala’s National Statistical Office), the data would be divided between the low-poverty and high-

Table R2: Extreme Poverty Rates from 2002 &amp; 2011 Waves

Sample	Mean Total Poverty-2002 (%)	Mean Total Poverty-2011 (%)
Whole Sample	19.79 (14.27)	20.84 (15.47)
Whole Sample (including missing 2011)	19.79 (14.27)	19.28 (15.51)
Municipalities Both in 2002 & 2011	21.42 (14.01)	20.84 (15.47)
Municipalities Only in 2002	5.59 (6.61)	NA NA
Poverty-Reducing Sample	26.99 (13.66)	13.92 (8.49)
Poverty-Increasing Sample	15.33 (11.69)	28.31 (17.78)
Low-Poverty Sample	8.22 (4.50)	15.66 (11.08)
High-Poverty Sample	31.29 (10.96)	25.09 (17.21)

Standard deviations are in parentheses. Total poverty rates are from the 2002 and 2011 census. “Whole Sample (including missing 2011)” (row 2) included values from 2002 for the 32 municipalities with missing information in 2011. “Municipalities only in 2002” (row 4) refer to the 34 municipalities that had data in the 2002 census only.

poverty sample, or the poverty-increasing sample and the poverty-decreasing sample. Second, the data in each sample would be further attenuated based on whether Calonico, Cattaneo and Titiunik’s (2014) algorithm for regression discontinuity analysis classified the municipality-year as having a close election. In technical terms, the observation would have to be an “effective observation”, and the likelihood of any particular observation being an effective observation is circa 50-60% in our models. Therefore, adding the missing the observations would likely only add a minimal number of observations to each sample, thereby making the missing data rather insignificant from a statistical power perspective.

## S. Additional Results for Morales Term Regressions

### S.1. When Poverty is Low/High

Table S1: Number of Infractions Committed (2008-2019) [Poisson]

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.786*** (0.022)	0.788*** (0.022)	0.468*** (0.043)	0.787*** (0.021)	0.573*** (0.036)	0.487*** (0.049)
Low Poverty		0.003 (0.037)	0.012 (0.037)			
Population (log)					1.571*** (0.209)	-0.337 (0.301)
Re-elected Mayor					0.008 (0.034)	0.002 (0.031)
Observations	3801	3790	3790	3801	3518	3518
Municipality FE	no	no	no	yes	yes	yes
Year FE	no	no	yes	no	no	yes

Note: Poisson regression model, since infractions are a count variable.

Standard errors clustered by municipality in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



Table S2: Number of Infractions Committed (2008-2019) [Negative Binomial]

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.786*** (0.022)	0.788*** (0.022)	0.469*** (0.043)	0.777*** (0.020)	0.568*** (0.032)	0.497*** (0.050)
Low Poverty		0.014 (0.035)	0.024 (0.036)			
Population (log)					1.493*** (0.192)	-0.286 (0.290)
Re-elected Mayor					0.019 (0.031)	0.001 (0.030)
Observations	3801	3790	3790	3801	3518	3518
Municipality FE	no	no	no	yes	yes	yes
Year FE	no	no	yes	no	no	yes

Note: Negative binomial regression model, since infractions are a count variable.

Standard errors clustered by municipality in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table S3: Log Amounts of Misappropriated Funds (2008-2019)

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.263*** (0.081)	0.261*** (0.081)	0.352*** (0.134)	0.270*** (0.079)	-0.055 (0.115)	0.107 (0.280)
Low Poverty		0.077 (0.074)	0.091 (0.074)			
Population (log)					2.472*** (0.488)	0.816 (0.841)
Re-elected Mayor					0.020 (0.094)	0.010 (0.095)
Observations	3796	3785	3785	3796	3513	3513
$R^2$	0.004	0.005	0.035	0.005	0.012	0.042
Municipality FE	no	no	no	yes	yes	yes
Year FE	no	no	yes	no	no	yes

Note: linear regression model.

Standard errors clustered by municipality in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## S.2. When Poverty Decreases/Increases

Table S4: Number of Infractions Committed (2008-2019) [Negative Binomial]

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.786*** (0.022)	0.747*** (0.022)	0.442*** (0.046)	0.777*** (0.020)	0.568*** (0.032)	0.497*** (0.050)
Poverty Reduced		-0.060* (0.034)	-0.063* (0.035)			
Population (log)					1.493*** (0.192)	-0.286 (0.290)
Re-elected Mayor					0.019 (0.031)	0.001 (0.030)
Observations	3801	3357	3357	3801	3518	3518
Municipality FE	no	no	no	yes	yes	yes
Year FE	no	no	yes	no	no	yes

Note: Negative binomial regression model, since infractions are a count variable.

Standard errors clustered by municipality in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table S5: Log Amounts of Misappropriated Funds (2007-2018)

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.263*** (0.081)	0.218** (0.086)	0.254* (0.148)	0.270*** (0.079)	-0.055 (0.115)	0.107 (0.280)
Poverty Reduced		-0.051 (0.077)	-0.054 (0.077)			
Population (log)					2.472*** (0.488)	0.816 (0.841)
Re-elected Mayor					0.020 (0.094)	0.010 (0.095)
Observations	3796	3352	3352	3796	3513	3513
$R^2$	0.004	0.003	0.034	0.005	0.012	0.042
Municipality FE	no	no	no	yes	yes	yes
Year FE	no	no	yes	no	no	yes

Note: linear regression model.

Standard errors clustered by municipality in parentheses; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

## T. Additional Close Election Mechanism Regressions

### T.1. When Poverty is Low/High

Table T1: Infractions: How Much Do Close Elections Matter (2004-2015)?

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	0.004 (0.039)	-0.005 (0.041)	-0.036 (0.042)	0.016 (0.047)	0.047 (0.051)	-0.014 (0.053)
Low Poverty		0.055 (0.039)	0.078** (0.039)			
Log Population					0.213*** (0.072)	0.106* (0.062)
Reelected Mayor					0.032 (0.047)	0.038 (0.045)
Observations	2088	2078	2078	2088	1924	1924
Municipality FE	no	no	no	yes	yes	
Year FE	no	no	yes	no	no	

Note: poisson regressions; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Note: standard errors clustered by municipality in parentheses.

Table T2: Infractions: How Much Do Close Elections Matter (2004-2015)? [Negative Binomial]

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	0.004 (0.039)	-0.005 (0.041)	-0.036 (0.042)	0.016 (0.047)	0.047 (0.051)	-0.014 (0.053)
Low Poverty		0.055 (0.039)	0.078** (0.039)			
Log Population					0.213*** (0.072)	0.106* (0.062)
Reelected Mayor					0.032 (0.047)	0.038 (0.045)
Observations	2088	2078	2078	2088	1924	1924
Municipality FE	no	no	no	yes	yes	
Year FE	no	no	yes	no	no	

Note: negative binomial regressions; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Note: standard errors clustered by municipality in parentheses.

Table T3: Log Amounts of Misappropriated Funds (2004-2015)

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	-0.140 (0.118)	-0.164 (0.122)	-0.174 (0.121)	-0.159 (0.120)	-0.163 (0.124)	-0.099 (0.137)
Low Poverty		0.190** (0.094)	0.206** (0.094)			
Log Population					0.445 (0.606)	1.989 (1.638)
Reelected Mayor					0.173 (0.114)	0.281** (0.119)
Observations	2083	2073	2073	2083	1919	1428
$R^2$	0.001	0.005	0.037	0.001	0.003	0.166
Municipality FE	no	no	no	yes	yes	yes
Year FE	no	no	yes	no	no	yes

Note: Linear regression models; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Standard errors clustered by municipality in parentheses

## T.2. When Poverty Decreases/Increases

Table T4: Infractions: How Much Do Close Elections Matter (2010-2015)?

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	-0.065 (0.045)	-0.061 (0.048)	-0.073 (0.047)	0.030 (0.056)	0.040 (0.065)	0.014 (0.065)
Poverty Reduction		-0.019 (0.049)	-0.019 (0.048)			
Log Population					2.719*** (0.479)	-1.017 (0.998)
Reelected Mayor					0.065 (0.066)	0.065 (0.064)
Observations	1260	1125	1125	1260	1178	1178
Municipality FE	no	no	no	yes	yes	
Year FE	no	no	yes	no	no	

Note: negative binomial regressions; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Note: standard errors clustered by municipality in parentheses.

Note: model with municipality and year fixed effects would not converge.

Table T5: Log Amounts of Misappropriated Funds (2007-2018)

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	-0.264** (0.125)	-0.267** (0.132)	-0.279** (0.133)	-0.049 (0.132)	-0.036 (0.162)	-0.038 (0.155)
Poverty Reduction		-0.042 (0.103)	-0.043 (0.103)			
Log Population					5.704*** (1.316)	2.581 (2.389)
Reelected Mayor					0.265 (0.174)	0.279* (0.161)
Observations	1256	1121	1121	1256	1174	1077
$R^2$	0.007	0.008	0.079	0.000	0.039	0.198
Municipality FE	no	no	no	yes	yes	yes
Year FE	no	no	yes	no	no	yes

Note: Linear regression models; \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Standard errors clustered by municipality in parentheses

## Bibliography

- Acemoglu, Daron and James A. Robinson. 2018. "Beyond Modernization Theory." *Annals of Comparative Democratization* 16(3):26–31.
- Acemoglu, Daron, Simon Johnson and James A. Robinson. 2005. "Institutions as a Fundamental Cause of Long-Run Growth." *Handbook of Economic Growth* 1(SUPPL. PART A):385–472.
- Agerberg, Mattias. 2020. "The Lesser Evil? Corruption Voting and the Importance of Clean Alternatives." *Comparative Political Studies* 53(2):253–287.
- Aidt, Toke S., Francisco José Veiga and Linda Gonçalves Veiga. 2011. "Election Results and Opportunistic Policies: A New Test of the Rational Political Business Cycle Model." *Public Choice* 148(1-2):21–44.
- Alesina, Alberto and Matteo Paradisi. 2017. "Political Budget Cycles: Evidence from Italian Cities." *Economics and Politics* 29(2):157–177.
- Ansolabehere, Stephen, James M. Snyder and Michael M. Ting. 2003. "Bargaining in Bicameral Legislatures: When and Why Does Malapportionment Matter?" *American Political Science Review* 97(3):471–481.
- Auyero, Javier. 1999. "From the Client's Point(s) of View: How Poor People Perceive and Evaluate Political Clientelism." *Theory and Society* 28(2):297–334.
- Auyero, Javier. 2000. "The Logic of Clientelism in Argentina: An Ethnographic Account." *Latin American Research Review* 35(3):55–81.
- Avis, Eric, Claudio Ferraz and Frederico Finan. 2018. "Do Government Audits Reduce Corruption? Estimating the Impacts of Exposing Corrupt Politicians." *Journal of Political Economy* 126(5):1912–1964.

- Baleiras, Rui Nuno. 1997. "Electoral Defeats and Local Political Expenditure Cycles." *Economics Letters* 56(2):201–207.
- Baleiras, Rui Nuno and José da Silva Costa. 2004. "To Be Or Not To Be in Office Again: An Empirical Test of a Local Political Business Cycle Rationale." *European Journal of Political Economy* 20(3):655–671.
- Banerjee, Abhijit V., Donald P. Green, Jeffrey McManus and Rohini Pande. 2014. "Are Poor Voters Indifferent to Whether Elected Leaders are Criminal or Corrupt?" *Political Communication* 31(April 2015):37–41.
- Banerjee, Abhijit V. and Esther Duflo. 2007. "The Economic Lives of the Poor." *Journal of Economic Perspectives* 21(1):141–168.
- Banerjee, Abhijit V. and Esther Duflo. 2011. *Poor Economics: A Radical Rethinking of the Way to Fight Global Poverty*. New York: PublicAffairs.
- Banful, Afua Branoah. 2011a. "Do Formula-Based Intergovernmental Transfer Mechanisms Eliminate Politically Motivated Targeting? Evidence from Ghana." *Journal of Development Economics* 96(2):380–390.
- Banful, Afua Branoah. 2011b. "Old Problems in the New Solutions? Politically Motivated Allocation of Program Benefits and the "New" Fertilizer Subsidies." *World Development* 39(7):1166–1176.
- Barro, Robert J. 1973. "The Control of Politicians: An Economic Model." *Public Choice* 14(Spring 1973):19–42.
- Bartalotti, Otávio and Quentin Brummet. 2017. Regression Discontinuity Designs with Clustered Data. In *Advances in Econometrics*, ed. Matias D. Cattaneo and Juan Carlos Escanciano. Vol. 38 Bingley, United Kingdom: Emerald Publishing pp. 383–420.
- Bobonis, Gustavo J., Luis R. Cámara Fuertes and Rainer Schwabe. 2016. "Monitoring Corruptible Politicians." *American Economic Review* 106(8):2371–2405.

- Brollo, Fernanda and Tommaso Nannicini. 2012. "Tying Your Enemy's Hands in Close Races: The Politics of Federal Transfers in Brazil." *American Political Science Review* 106(4):742–761.
- Brollo, Fernanda, Tommaso Nannicini, Roberto Perotti and Guido Tabellini. 2013. "The Political Resource Curse." *American Economic Review* 103(5):1759–1796.
- Bueno, Natália S. 2018. "Bypassing the Enemy: Distributive Politics, Credit Claiming, and Nonstate Organizations in Brazil." *Comparative Political Studies* 51(3):304–340.
- Callen, Michael, Saad Gulzar and Arman Rezaee. 2020. "Can Political Alignment be Costly?" *Journal of Politics* 82(2):612–626.
- Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell and Rocío Titiunik. 2019. "Regression Discontinuity Designs Using Covariates." *Review of Economics and Statistics* 101(3):442–451.
- Calonico, Sebastian, Matias D. Cattaneo and Rocío Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82(6):2295–2326.
- Carozzi, Felipe and Luca Repetto. 2016. "Sending the Pork Home: Birth Town Bias in Transfers to Italian Municipalities." *Journal of Public Economics* 134:42–52.
- Cattaneo, Matias D., Michael Jansson and Xinwei Ma. 2018. "Manipulation Testing based on Density Discontinuity." *Stata Journal* 18(1):234–261.
- Cattaneo, Matias D., Michael Jansson and Xinwei Ma. 2020. "Simple Local Polynomial Density Estimators." *Journal of the American Statistical Association* 115(531):1449–1455.
- Cattaneo, Matias D., Nicolas Idrobo and Rocío Titiunik. 2019. *A Practical Introduction to Regression Discontinuity Designs: Part I*. Cambridge: Cambridge University Press.



- Cavalcanti, Francisco, Gianmarco Daniele and Sergio Galletta. 2018. "Popularity Shocks and Political Selection." *Journal of Public Economics* 165:201–216.
- Chandra, Kanchan. 2004. *Why Ethnic Parties Succeed: Patronage and Ethnic Head Counts in India*. New York: Cambridge University Press.
- Charron, Nicholas and Andreas Bågenholm. 2016. "Ideology, Party Systems and Corruption Voting in European Democracies." *Electoral Studies* 41:35–49.
- Chong, Alberto, Ana De La O, Dean Karlan and Leonard Wantchekon. 2015. "Does Corruption Information Inspire the Fight or Quash the Hope? A Field Experiment in Mexico on Voter Turnout." *Journal of Politics* 29(1):55–71.
- Christenson, Dino P., Douglas L. Kriner and Andrew Reeves. 2017. "All the President's Senators: Presidential Copartisans and the Allocation of Federal Grants." *Legislative Studies Quarterly* 42(2):269–294.
- Chubb, Judith. 1982. *Patronage, Power and Poverty in Southern Italy: A Tale of Two Cities*. Cambridge: Cambridge University Press.
- Cinelli, Carlos and Chad Hazlett. 2020. "Making Sense of Sensitivity: Extending Omitted Variable Bias." *Journal of the Royal Statistical Society. Series B: Statistical Methodology* 82(1):39–67.
- Corvalan, Alejandro, Paulo Cox and Rodrigo Osorio. 2018. "Indirect Political Budget Cycles: Evidence from Chilean Municipalities." *Journal of Development Economics* 133(December 2017):1–14.
- de Janvry, Alain, Frederico Finan and Elisabeth Sadoulet. 2012. "Local Electoral Incentives and Decentralized Program Performance." *Review of Economics and Statistics* 94(3):672–685.
- De La O, Ana and Jonathan A. Rodden. 2008. "Does Religion Distract the Poor? Income and Issue Voting Around the World." *Comparative Political Studies* 41(4/5):437–476.

- Del Mar Martínez Rosón, María. 2016. “Yo prefiero al corrupto: El perfil de los ciudadanos que eligen políticos deshonestos pero competentes.” *Revista Espanola de Investigaciones Sociologicas* 153(153):77–94.
- Dell, Melissa. 2015. “Trafficking Networks and the Mexican Drug War.” *American Economic Review* 105(6):1738–1779.
- Drazen, Allan and Marcela Eslava. 2010. “Electoral Manipulation via Voter-friendly Spending: Theory and Evidence.” *Journal of Development Economics* 92(1):39–52.
- Dunning, Thad, Guy Grossman, Macartan Humphreys, Susan D. Hyde, Craig McIntosh and Gareth Nellis, eds. 2019. *Information, Accountability, and Cumulative Learning: Lessons from Metaketa I*. Cambridge: Cambridge University Press.
- Ferejohn, John. 1986. “Incumbent Performance and Electoral Control.” *Public Choice* 50(1/3):5–25.
- Fergusson, Leopoldo, Pablo Querubín, Nelson Ruiz-Guarin and Juan F. Vargas. 2020. “The Real Winner’s Curse.” *American Journal of Political Science* .
- Fernández-Vázquez, Pablo, Pablo Barberá and Gonzalo Rivero. 2016. “Rooting Out Corruption or Rooting For Corruption? The Heterogeneous Electoral Consequences of Scandals.” *Political Science Research and Methods* 4(02):379–397.
- Ferraz, Claudio and Frederico Finan. 2008. “Exposing Corrupt Politicians: The Effects of Brazil’s Publicly Released Audits on Electoral Outcomes.” *Quarterly Journal of Economics* 123(2):703–745.
- Ferraz, Claudio and Frederico Finan. 2011. “Electoral Accountability and Corruption: Evidence from the Audits of Local Governments.” *American Economic Review* 101(4):1274–1311.
- Finan, Frederico and Laura Schechter. 2012. “Vote-Buying and Reciprocity.” *Econometrica* 80(2):863–881.

- Findley, Michael G., Daniel L. Nielson and Jason C. Sharman. 2014. *Global Shell Games: Experiments in Transnational Relations, Crime, and Terrorism*. New York: Cambridge University Press.
- Fisman, Raymond, Florian Schulz and Vikrant Vig. 2014. "The Private Returns to Public Office." *Journal of Political Economy* 122(4):806–862.
- Fisman, Raymond and Miriam A. Golden. 2017. *Corruption: What Everyone Needs to Know*. Oxford: Oxford University Press.
- Fourinaies, Alexander and Hande Mutlu-Eren. 2015. "English Bacon: Copartisan Bias in Intergovernmental Grant Allocation in England." *Journal of Politics* 77(3):805–817.
- Fox, Jonathan A. 2015. "Social Accountability: What Does the Evidence Really Say?" *World Development* 72:346–361.
- Frey, Anderson. 2019. "Cash Transfers, Clientelism, and Political Enfranchisement: Evidence from Brazil." *Journal of Public Economics* 176:1–17.
- Gans-Morse, Jordan, Sebastián L. Mazzuca and Simeon Nichter. 2014. "Varieties of Clientelism: Machine Politics during Elections." *American Journal of Political Science* 58(2):415–432.
- Gelman, Andrew and Guido W. Imbens. 2019. "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs." *Journal of Business and Economic Statistics* 37(3):447–456.
- Gerring, John and Strom C. Thacker. 2004. "Political Institutions and Corruption: The Role of Unitarism and Parliamentarism." *British Journal of Political Science* 34(2004):295–330.
- Gershenkron, Alexander. 1962. *Economic Backwardness in Historical Perspective*. New York: Frederick A. Praeger.

- Golden, Miriam A. and Paasha Mahdavi. 2015. The Institutional Components of Political Corruption. In *Handbook of Comparative Political Institutions*, ed. Jennifer Gandhi and Ruben Ruiz-Rufino. New York: Routledge chapter 28, pp. 404–420.
- Gonzalez-Ocantos, Ezequiel, Chad Kiewiet de Jonge and David W. Nickerson. 2014. “The Conditionality of Vote-Buying Norms: Experimental Evidence from Latin America.” *American Journal of Political Science* 58(1):197–211.
- González, Pablo. 2014. Guatemala. In *Handbook of Central American Governance*, ed. Diego Sánchez-Ancochea and Salvador Martí i Puig. London and New York: Routledge chapter 24, pp. 400–419.
- Greene, Kenneth F. 2007. *Why Dominant Parties Lose: Mexico’s Democratization in Comparative Perspective*. New York: Cambridge University Press.
- Greene, Kenneth F. 2010. “The Political Economy of Authoritarian Single-Party Dominance.” *Comparative Political Studies* 43(7):807–834.
- Guo, Gang. 2009. “China’s Local Political Budget Cycles.” *American Journal of Political Science* 53(3):621–632.
- Hicken, Allen. 2011. “Clientelism.” *Annual Review of Political Science* 14(1):289–310.
- Hidalgo, F. Daniel and Simeon Nichter. 2016. “Voter Buying: Shaping the Electorate through Clientelism.” *American Journal of Political Science* 6:436–455.
- Hill, Andrew J. and Daniel B. Jones. 2017. “Does Partisan Affiliation Impact the Distribution of Spending? Evidence from State Governments’ Expenditures on Education.” *Journal of Economic Behavior and Organization* 143:58–77.
- Hollyer, James R. 2018. Measuring Governance: Objective and Perceptions-Based Governance Indicators. In *Governance Indicators: Approaches, Progress, Promise*, ed. Helmut Anheier, Matthias Haber and Mark Andreas Kayser. Oxford: Oxford University Press chapter 5, pp. 103–133.

Inglehart, Ronald and Christian Welzel. 2005. *Modernization, Cultural Change, and Democracy: The Human Development Sequence*. New York: Cambridge University Press.

Instituto Nacional de Estadística de Guatemala. 2014. Caracterización República de Guatemala - INE. Technical report Guatemala City: .

**URL:** <http://www.ine.gob.gt/sistema/uploads/2014/02/26/L5pNHMXzxy5FFWmk9NHCrK9x7E5Qqv>

Jensen, Peter Sandholt and Mogens K. Justesen. 2014. “Poverty and Vote Buying: Survey-Based Evidence from Africa.” *Electoral Studies* 33:220–232.

Kauder, Björn, Niklas Potrafke and Markus Reischmann. 2016. “Do Politicians Reward Core Supporters? Evidence from a Discretionary Grant Program.” *European Journal of Political Economy* 45:39–56.

Keefer, Philip. 2004. “What Does Political Economy Tell Us about Economic Development - and Vice-Versa?” *Annual Review of Political Science* 7(1):247–272.

Keefer, Philip. 2007a. “Clientelism, Credibility, and the Policy Choices of Young Democracies.” *American Journal of Political Science* 51(4):804–821.

Keefer, Philip. 2007b. The Poor Performance of Poor Democracies. In *Oxford Handbook of Comparative Politics*, ed. Carles Boix and Susan C. Stokes. Oxford: chapter 36, pp. 886–909.

Keefer, Philip and Razvan Vlaicu. 2008. “Democracy, Credibility, and Clientelism.” *Journal of Law, Economics, and Organization* 24(2):371–406.

Keefer, Philip and Stuti Khemani. 2005. “Democracy, Public Expenditures, and the Poor: Understanding Political Incentives for Providing Public Services.” *World Bank Research Observer* 20(1):1–27.

Kitschelt, Herbert and Daniel M. Kselman. 2013. “Economic Development, Democratic Experience, and Political Parties’ Linkage Strategies.” *Comparative Political Studies* 46(11):1453–1484.

- Kitschelt, Herbert and Steven I. Wilkinson. 2007. Citizen-Politician Linkages: An Introduction. In *Patrons, Clients, and Policies: Patterns of Democratic Accountability and Political Competition*, ed. Herbert Kitschelt and Steven I. Wilkinson. New York: Cambridge University Press chapter 1, pp. 1–49.
- Klašnja, Marko and Rocío Titiunik. 2017. “The Incumbency Curse: Weak Parties, Term Limits, and Unfulfilled Accountability.” *American Political Science Review* 111(1):129–148.
- Kosack, Stephen and Archon Fung. 2014. “Does Transparency Improve Governance?” *Annual Review of Political Science* 17(1):65–87.
- Kriner, Douglas L. and Andrew Reeves. 2012. “The Influence of Federal Spending on Presidential Elections.” *American Political Science Review* 106(2):348–366.
- Kriner, Douglas L. and Andrew Reeves. 2015. “Presidential Particularism and Divide-the-Dollar Politics.” *American Political Science Review* 109(1):155–171.
- Kunicová, Jana and Susan Rose-Ackerman. 2005. “Electoral Rules and Constitutional Structures as Constraints on Corruption.” *British Journal of Political Science* 35(May):573–606.
- Kurtz, Marcus J. and Andrew Schrank. 2007. “Growth and Governance: Models, Measures, and Mechanisms.” *Journal of Politics* 69(2):538–554.
- Labonne, Julien. 2016. “Local Political Business Cycles: Evidence from Philippine Municipalities.” *Journal of Development Economics* 121:56–62.
- Lara, Bernardo E. and Sergio M. Toro. 2019. “Tactical Distribution in Local Funding: The Value of an Aligned Mayor.” *European Journal of Political Economy* 56(July 2018):74–89.
- Larreguy, Horacio, John Marshall and James M. Snyder. 2020. “Publicizing Malfeasance: When Media Facilitates Electoral Accountability in Mexico.” *Economic Journal*.

- Larreguy, Horacio, John Marshall and Pablo Querubín. 2016. "Parties, Brokers, and Voter Mobilization: How Turnout Buying Depends Upon the Party's Capacity to Monitor Brokers." *American Political Science Review* 110(1):160–179.
- Lee, David S. 2008. "Randomized Experiments from Non-Random Selection in U.S. House Elections." *Journal of Econometrics* 142(2):675–697.
- Leight, Jessica, Dana Foarta, Rohini Pande and Laura Ralston. 2020. "Value for Money? Vote-Buying and Politician Accountability." *Journal of Public Economics* 190.
- Lerner, Daniel. 1958. *The Passing of Traditional Society: Modernizing the Middle East*. New York: The Free Press.
- Levitsky, Steven and Daniel Ziblatt. 2018. *How Democracies Die*. New York: Crown.
- Lieberman, Evan S., Daniel N. Posner and Lily L. Tsai. 2014. "Does Information Lead to More Active Citizenship? Evidence from an Education Intervention in Rural Kenya." *World Development* 60:69–83.
- Lipset, Seymour Martin. 1959. "Some Social Requisites of Democracy: Economic Development and Political Legitimacy." *American Political Science Review* 53(1):69–105.
- Lipset, Seymour Martin. 1960. *Political Man: The Social Bases of Politics*. Baltimore, Maryland: Johns Hopkins University Press.
- Livert, Felipe, Xabier Gainza and Jose Acuña. 2019. "Paving the Electoral Way: Urban Infrastructure, Partisan Politics and Civic Engagement." *World Development* 124:104628.
- Lü, Xiaobo. 2015. "Intergovernmental Transfers and Local Education Provision: Evaluating China's 8-7 National Plan for Poverty Reduction." *China Economic Review* 33:200–211.
- Magaloni, Beatriz, Alberto Díaz-Cayeros and Federico Estévez. 2007. Clientelism and Portfolio Diversification: A Model of Electoral Investment with Applications to Mexico. In

- Patrons, Clients, and Policies: Patterns of Democratic Accountability and Political Competition*, ed. Herbert Kitschelt and Steven I. Wilkinson. New York: Cambridge University Press chapter 8, pp. 182–205.
- Malkin, Elisabeth. 2019. “Guatemala Expels U.N.-Backed Anti-Corruption Panel, Claiming Overreach.”
- URL:** <https://www.nytimes.com/2019/01/07/world/americas/guatemala-corruption-commission-united-nations.html>
- Manacorda, Marco, Edward Miguel and Andrea Vigorito. 2011. “Government Transfers and Political Support.” *American Economic Journal: Applied Economics* 3(3):1–28.
- Manzetti, Luigi and Carole J. Wilson. 2007. “Why Do Corrupt Governments Maintain Public Support?” *Comparative Political Studies* 40(8):949–970.
- McCrary, Justin. 2008. “Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test.” *Journal of Econometrics* 142(2):698–714.
- Meilán, Xabier. 2016. “The 2015 General Elections in Guatemala.” *Electoral Studies* 43:181–184.
- Meyersson, Erik. 2014. “Islamic Rule and the Empowerment of the Poor and Pious.” *Econometrica* 82(1):229–269.
- Muñoz, Paula. 2019. *Buying Audiences: Clientelism and Electoral Campaigns*. Cambridge: Cambridge University Press.
- Muñoz, Jordi, Eva Anduiza and Aina Gallego. 2016. “Why do Voters Forgive Corrupt Mayors? Implicit Exchange, Credibility of Information and Clean Alternatives.” *Local Government Studies* 42(4):598–615.
- Nichter, Simeon. 2008. “Vote Buying or Turnout Buying? Machine Politics and the Secret Ballot.” *American Political Science Review* 102(1):19–31.



- Nichter, Simeon and Michael Peress. 2017. "Request Fulfilling: When Citizens Ask for Clientelist Benefits." *Comparative Political Studies* 50(8):1086–1117.
- Niehaus, Paul and Sandip Sukhtankar. 2013. "Corruption Dynamics: The Golden Goose Effect." *American Economic Journal: Economic Policy* 5(4):230–269.
- Nikolova, Elena and Nikolay Marinov. 2017. "Do Public Fund Windfalls Increase Corruption? Evidence From a Natural Disaster." *Comparative Political Studies* 50(11):1455–1488.
- Olken, Benjamin A. and Rohini Pande. 2012. "Corruption in Developing Countries." *Annual Review of Economics* 4(1):479–509.
- Pande, Rohini. 2011. "Can Informed Voters Enforce Better Governance? Experiments in Low-Income Democracies." *Annual Review of Economics* 3(1):215–237.
- Pavão, Nara. 2018. "Corruption as the Only Option: The Limits to Electoral Accountability." *Journal of Politics* 80(3):996–1010.
- Pereira, Carlos, J Rennó and David Samuels. 2011. Corruption, Campaign Finance and Reelection. In *Corruption and Democracy in Brazil: The Struggle for Accountability*, ed. Timothy J. Power and Matthew M. Taylor. South Bend, Indiana: Notre Dame Press pp. 80–99.
- Pereira, Carlos and Marcus André Melo. 2015. "Reelecting Corrupt Incumbents in Exchange for Public Goods: Rouba mas faz in Brazil." *Latin American Research Review* 50(4):88–115.
- Querubín, Pablo and James M. Snyder. 2013. "The Control of Politicians in Normal Times and Times of Crisis: Wealth Accumulation by U.S. Congressmen, 1850–1880." *Quarterly Journal of Political Science* 8(4):409–450.
- Robinson, James A. and Thierry Verdier. 2013. "The Political Economy of Clientelism." *Scandinavian Journal of Economics* 115(2):260–291.

- Rose-Ackerman, Susan and Bonnie Palifka. 2016. *Corruption and Government: Causes, Consequences, and Reform*. Second ed. New York: Cambridge University Press.
- Rostow, Walter Whitman. 1960. *The Stages of Economic Growth: A Non-Communist Manifesto*. Cambridge: Cambridge University Press.
- Sachs, Jeffrey D. 2005. *The End of Poverty: Economic Possibilities for Our Time*. New York: Penguin Books.
- Sandberg, Johan and Engel Tally. 2015. "Politicisation of Conditional Cash Transfers: The Case of Guatemala." *Development Policy Review* 33(4):503–522.
- Schneider, Christina J. 2010. "Fighting with One Hand Tied Behind the Back: Political Budget Cycles in the West German States." *Public Choice* 142(1-2):125–150.
- Schwindt-Bayer, Leslie A. and Margit Tavits. 2016. *Clarity of Responsibility, Accountability, and Corruption*. New York: Cambridge University Press.
- Scott, James C. 1972. "Patron-Client Politics and Political Change in Southeast Asia." *American Political Science Review* 66(1):91–113.
- Solaz, Hector, Catherine E. De Vries and Roosmarijn A. de Geus. 2019. "In-Group Loyalty and the Punishment of Corruption." *Comparative Political Studies* 52(6):896–926.
- Solé-Ollé, Albert and Pilar Sorribas-Navarro. 2008. "The Effects of Partisan Alignment on the Allocation of Intergovernmental Transfers. Differences-in-Differences Estimates for Spain." *Journal of Public Economics* 92(12):2302–2319.
- Søreide, Tina. 2014. *Drivers of Corruption: A Brief Review*. Washington, DC: World Bank.
- Stokes, Susan C. 2005. "Perverse Accountability: A Formal Model of Machine Politics with Evidence from Argentina." *American Political Science Review* 99(3):315–325.

- Stokes, Susan C., Thad Dunning, Marcelo Nazareno and Valeria Brusco. 2013. *Brokers, Voters, and Clientelism: The Puzzle of Distributive Politics*. New York: Cambridge University Press.
- Szwarcberg, Mariela. 2015. *Mobilizing Poor Voters*. New York: Cambridge University Press.
- The Economist. 2019. “An Attack on Corruption Sleuths in Guatemala is also Aimed at Judges.”.
- Timmons, Jeffrey F. and Daniel Broidy. 2013. “The Political Economy of Municipal Transfers: Evidence from Mexico.” *Publius* 43(4):551–579.
- Treisman, Daniel. 2007. “What Have We Learned About the Causes of Corruption from Ten Years of Cross-National Empirical Research?” *Annual Review of Political Science* 10(1):211–244.
- Treisman, Daniel and Vladimir Gimpelson. 2001. “Political Business Cycles and Russian Elections.” *British Journal of Political Science* 31:225–246.
- Trejo, Guillermo and Camilo Nieto-Matis. 2019. “Containing Large-Scale Criminal Violence through Internationalized Prosecution: How the CICIG Contributed to the Reduction of Guatemala’s Murder Rate.”.
- Veiga, Linda Gonçalves and Francisco José Veiga. 2007. “Political Business Cycles at the Municipal Level.” *Public Choice* 131(1-2):45–64.
- Veiga, Linda Gonçalves and Francisco José Veiga. 2013. “Intergovernmental Fiscal Transfers as Pork Barrel.” *Public Choice* 155(3-4):335–353.
- Veiga, Linda Gonçalves and Maria Manuel Pinho. 2007. “The Political Economy of Intergovernmental Grants: Evidence from a Maturing Democracy.” *Public Choice* 133(3-4):457–477.

- Velasco Rivera, Carlos. 2020. "Loyalty or Incentives? How Party Alignment Affects Bureaucratic Performance." *Journal of Politics* 82(4):1287–1304.
- Vuković, Vuk. 2020. "Corruption and Re-election: How Much Can Politicians Steal Before Getting Punished?" *Journal of Comparative Economics* 48:124–143.
- Weitz-Shapiro, Rebecca. 2012. "What Wins Votes: Why Some Politicians Opt Out of Clientelism." *American Journal of Political Science* 56(3):568–583.
- Weitz-Shapiro, Rebecca. 2014. *Curbing Clientelism in Argentina: Politics, Poverty, and Social Policy*. New York: Cambridge University Press.
- Winters, Matthew S. and Rebecca Weitz-Shapiro. 2013. "Lacking Information or Condoning Corruption: When Will Voters Support Corrupt Politicians?" *Comparative Politics* 45(4):418–436.
- World Bank. 2017. "World Development Indicators.".  
**URL:** <http://data.worldbank.org/data-catalog/world-development-indicators>
- World Bank. 2019. "BOOST Initiative.".  
**URL:** <http://boost.worldbank.org/>
- Zamboni, Yves and Stephan Litschig. 2018. "Audit Risk and Rent Extraction: Evidence from a Randomized Evaluation in Brazil." *Journal of Development Economics* 134(April):133–149.
- Zechmeister, Elizabeth J. and Daniel Zizumbo-Colunga. 2013. "The Varying Political Toll of Concerns About Corruption in Good Versus Bad Economic Times." *Comparative Political Studies* 46(10):1190–1218.