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

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Abstract

Party alignment entails politicians sharing the same party at higher and lower levels of government, giving aligned politicians greater access to the spoils of the bureaucracy. Does the political-institutional configuration of party alignment thus necessarily lead to more corruption? Given that party alignment also signals clarity of political responsibility for corruption to voters, we theorize that party alignment can actually yield lower levels of corruption if two conditions are met. Using a regression discontinuity design and novel corruption data from Guatemalan municipal audit reports, we show that aligned politicians are less likely to engage in corruption if there is both significant electoral competition and voters' poverty levels are low or decreasing. The results of our study document how the reduction of corruption through modernization forces such as decreasing poverty takes place through political institutions.

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Keywords: corruption, poverty, political institutions

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The practice of misusing entrusted power or public office for private gain has a familiar name: corruption.[1] 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; Ferraz and Finan, 2011; Golden and Mahdavi, 2015).

In this study, we examine how corruption levels depend on the political-institutional configuration of party alignment: that is, when politicians' parties match at higher and lower levels of government. Examples of party alignment include when a governor or mayor share the same political party as the president. Irrespective of its specific manifestation, party alignment is an institutional configuration that facilitates clarity of political responsibility: that is, " 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).

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.[2] Under what conditions, then,

[1] For more on definitions of corruption, see, for example, Søreide (2014) and Rose-Ackerman and Palifka (2016).

[2] There is evidence of political budget cycles and favoritism in intergovernmental transfer allocation in at least the following countries: Argentina (Garofalo, Lema and Streb, 2020); 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 (Fouirnaies and Mutlu-Eren, 2015); Germany (Kauder, Potrafke and Reischmann, 2016); Ghana (Banful, 2011*a*,*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; Berry, Burden and Howell, 2010; 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).

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

Using a Barro (1973)-Ferejohn (1986) political agency model that notably draws from Magaloni, Díaz-Cayeros and Estévez (2007) and Brollo and Nannicini (2012), we provide a theory to explain when aligned politicians engage in less corruption. To that end, the clarity of responsibility that alignment facilitates does not necessarily lower corruption, 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.

Both voter demand and politicians' supply constraints explain why alignment can only reduce corruption under lower poverty and high electoral competition. The role of poverty primarily depends on voter demand pressures. Because politicians' abilities to deliver on their policy promises of public goods provision are generally not very credible in poorer countries,[3] voters in such contexts have high levels of demand for clientelistic handouts.[4] Given that aligned politicians have greater supply-side access to government spoils,[5] and voters trade-off the value of a corrupt politician against the clientelistic benefits that the politician can bring,[6] the clarity of responsibility from alignment is not sufficient to reduce corruption. By contrast, a decrease in poverty reduces the extent to which voter discount the future and demand clientelistic handouts,[7] thereby reducing aligned politicians' incentives

[3] See, for example, Keefer (2004, 2007*a*,*b*), Keefer and Khemani (2005), and Keefer and Vlaicu (2008).

[4] As Lyne (2008) explains, in such contexts voters are trapped in an N-person person prisoner's dilemma, so it is generally not within voters' incentives—including wealthier voters' incentives—to defect and vote for non-clientelist politicians unless economic/structural conditions change. Furthermore, as Nichter and Peress (2017) and Nichter (2018) show, voter demand better explains patterns of clientelism than the canonical, supply-side models of turnout-buying (Nichter, 2008) and vote-buying (Stokes, 2005) drawn from analysis of the same Argentina data.

[5] See, for example, Brollo and Nannicini (2012) and Curto-Grau, Solé-Ollé and Sorribas-Navarro (2018).

[6] See, for example, Manzetti and Wilson (2007), Chang and Kerr (2017), Leight et al. (2020), Botero et al. (2021), and Bøttkjær and Justesen (2021).

[7] See, for example, Kitschelt and Wilkinson (2007), Lyne (2008), and Stokes et al. (2013, Chapter 6).

to engage in corruption as well. Specifically, the decrease in poverty and lower demand for clientelism also reduces aligned politicians' incentives to extract government spoils in service of their ultimate goal: reelection.

On the subject of elections, electoral competition amplifies the effects of better economic circumstances on aligned politicians' corruption levels. Winning elections by small margins, for example, signals to aligned 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 as private citizens (e.g., Fisman, Schulz and Vig, 2014), reelection prospects drive aligned 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, parties have an incentive to discourage corruption from their aligned members especially 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. However, these same incentives are not present when unaligned politicians barely win their positions in poorer electorates. The lack of clarity of responsibility means that unaligned politicians can deflect blame on the opposition and not suffer the same reelection consequences when they misappropriate resources to meet voters' increased demand for clientelistic spending. In any case, unaligned politicians lack of resources vis-à-vis aligned politicians means that they mostly need to compete on valence issues, which are generally less compelling in a context of poverty.

To support our theory that stresses how party alignment's conditional effect on corruption depends on both political competition and voters' economic circumstances, we examine new 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 myriad protests and widespread international media coverage of the CICIG expulsion, as well the CICIG's contribution to the removal of former President Otto Pérez Molina on corruption charges in 2014, underscores the relevance of corruption for Guatemala's political discourse and democratic stability more broadly.

Unlike the corruption perceptions data that dominate the literature, our data correspond to actual measures of corruption that we draw from audit reports produced by the Guatemala's Comptroller General (*Contraloría General de Cuentas*). Given the possibility for manipulation of the audit data, we subject them to multiple tests, each time finding that there do no appear to be relevant biases. Additionally, because the audit data are subnational, they do not exhibit level of analysis problems that plague many corruption studies (see Gingerich, 2013).

To operationalize whether a municipality is performing better economically, we specifically compare municipalities with low and high poverty levels (i.e., those above and below the median poverty level); municipalities with increased and decreased poverty rates relative to the previous census; and all municipalities—i.e., not subsetting by poverty. To causally identify the effects of alignment in the different samples, we exploit a series of close-election regression discontinuity designs.

We find that alignment yields 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 fewer infractions in the decreased-poverty sample, and 6.09 fewer infractions in the low-poverty sample. Numerous robustness checks show similar patterns, including when we employ Calonico et al.'s (2019) new method to control for the influence of covariates in the regression discontinuity estimates.

In most cases, alignment reduces corruption in municipalities with low or decreasing extreme poverty as well, suggesting that the theory has broad reach. Consistent with our theory, none of these results travel to municipalities in the high-poverty or poverty-increasing samples. Analysis of the full sample (i.e., not splitting the sample according to poverty levels or changes) provides results that are similarly consistent with our theory. Notably, all specifications in the full sample are 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 institutional and modernization approaches to corruption dovetail.[8] 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). Along these lines, the political-institutional configuration of party alignment only reduces corruption if politicians are susceptible to significant political competition and poverty is lower. We find the same patterns when examining both the effects of short-term poverty changes and longer-term poverty levels, and the poverty and corruption data are not endogenous (see Appendix M). Accordingly, our robust results challenge previous literature suggesting that a strong economy allows politicians to get away with corruption (e.g., Manzetti and Wilson, 2007; Klašnja and Tucker, 2013; Zechmeister and Zizumbo-Colunga, 2013; Schleiter and Tavits, 2018). That is not always the case, as our audit-based corruption data from Guatemala show.

1. Theoretical Model

1.1. Model Setup

Building on previous models from Magaloni, Díaz-Cayeros and Estévez (2007) and Brollo and Nannicini (2012), we develop a theoretical model to explain how party alignment

[8] 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). Within the corruption literature, the modernization approach "views corruption as a product of poverty" (Fisman and Golden, 2017, 15).

affects corruption. To best capture the trade-off between alignment producing both clarity of responsibility and resource advantages that fuel corruption, we employ a two-period political agency setup following the Barro (1973)-Ferejohn (1986) model (see Gehlbach, 2021, 162). In our setup, local-level politicians (agents) seek to maximize rent extraction, but they only do so in a manner that allows them to be competitive for reelection in the next electoral period. More specifically, local-level politicians maximizes rent extraction in the current electoral term, taking into account the cost of a potential corruption scandal that may affect reelection prospects as well as discounted expected income in the next term. Voter (principal) satisfaction with the local-level politician is the mechanism underpinning politicians' rent extraction decisions. Accordingly, our consideration of voter welfare and the reelection motive captures the essence of related models from Bracco et al. (2015) and Brollo et al. (2013), which follow the agency-effort setup of Besley (2006).[9]

To proceed, let us first consider local-level politician i's maximization problem. 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:

$$b_i = g_i + r_i \quad [10] \tag{1}$$

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:

$$r = c + p, \text{ where } c = \gamma r \quad [11] \tag{2}$$

[9] Note: Bracco et al. (2015) and Brollo et al. (2013) focus on taxes and transfers, whereas we focus on rents.

[10] 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.

[11] 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.

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 ($c = 0$), as explained above, voters trade-off the value of corrupt politicians against the clientelistic benefits that they can bring (e.g., Manzetti and Wilson, 2007; Chang and Kerr, 2017; Leight et al., 2020; Botero et al., 2021; Bøttkjær and Justesen, 2021). Accordingly, devoting all rents toward p would entail political suicide for politician i's reelection chances and thus hurt future potential rent extraction levels as well.

Given the possibility of reelection and how it drives politician behavior,[12] our Barro-Ferejohn setup draws from Ferraz and Finan (2011) and Niehaus and Sukhtankar (2013), who 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, $r_{i,2}$:

$$r_i = r_{i,1} + r_{i,2} \text{ [13]} \tag{3}$$

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

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

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

[12] See, for example, Barro (1973), Ferejohn (1986), Ferraz and Finan (2011), and de Janvry, Finan and Sadoulet (2012).

[13] 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.

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),[14] such that $t(\cdot)$ is a positive function, $t' > 0$, and $t'' < 0$; and β_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 $W(\gamma r_{1,i})$.[15]

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

The model incorporates two independent channels through which clarity of responsibility affects citizens' satisfaction levels with local-level politician i. The first channel focuses on the direct effects of clarity of responsibility, and the second channel pinpoints how clarity of responsibility interacts with poverty to condition support for corrupt politicians and clientelism. Because citizens' levels of satisfaction with politician i affect his/her reelection probability (π_i), citizens' levels of satisfaction also impact politician i's incentives to extract rents (r) for corrupt (p) and clientelistic (c) purposes.

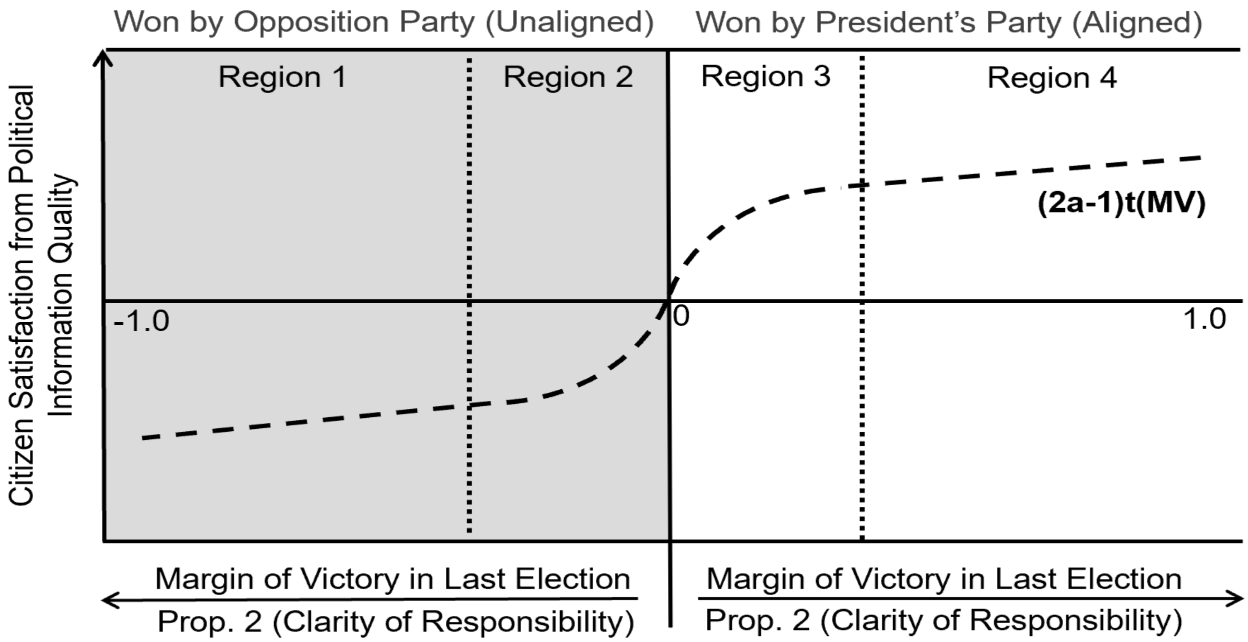
1.2.1. Channel 1: The Direct Effects of Clarity of Responsibility

With respect to the first channel, 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). We capture the joint dependency and its ability to be positive when aligned or negative when unaligned with $(2a-1)t(MV)$, which we take from Brollo and Nannicini (2012, 745) and Curto-Grau, Solé-Ollé and Sorribas-Navarro (2018, 382). Overall,

[14] We assume that MV is exogenous. While there certainly can be strategic voting, the paper's focus on alignment means that voters not only need to be able to predict the election of the local-level politician but also the executive. In practice, this would be very difficult for even an informed electorate. Thus, we believe that treating MV as exogenous is theoretically justifiable.

[15] 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



the $(2a-1)t(MV)$ term reflects citizens' levels of satisfaction via the quality of political information that they receive. As depicted in Figure 1, these levels of information quality are highest in Region 4, change precipitously as MV approaches zero, and are lowest in Region 1. Underpinning these patterns are how parties' campaign incentives vary with levels of MV. As Appendix D describes in further detail, Regions 2 and 3 are generally most desirable for campaigns to invest given levels of MV, and alignment ensures an easier-to-interpret message for citizens given the incumbent's ability and incentive to shape information flows.

Because alignment is a manifestation of single-party control of government, it plays an independent role on politicians' incentives for corruption 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.[16] By contrast, citizens' abilities to make such snap judgments are not as robust under divided government (Schwindt-Bayer and Tavits, 2016, 18; Appendix D). That is particularly the case

[16] There is a large literature on clarity of responsibility, particularly regarding its effects on economic voting (e.g., Powell and Whitten, 1993; Powell, 2000). Tavits (2007) extended this literature, showing how clarity of responsibility affects corruption as well, notably because corruption affects citizens' levels of happiness (Tavits, 2008).

in poor areas: they tend to suffer from political market imperfections, such as voters lacking information about politician performance,[17] identity voting,[18] and politicians' inability to make credible promises to voters (Keefer, 2004, 2007*a*,*b*; Keefer and Khemani, 2005; Keefer and Vlaicu, 2008).

1.2.2. Channel 2: Poverty and Voter Demands for Clientelism

The second channel through which clarity of responsibility affects s_i relates to a primary consequence of political market imperfections: the extent to which citizens value corrupt politicians and clientelism.[19] A large literature establishes that lower poverty leads voters to discount clientelistic benefits more with respect to policy-based, programmatic benefits.[20] Citizens also discount other benefits that corrupt politicians may bring in a similar manner,[21] and we posit that clarity of responsibility amplifies these discounting patterns.

We account for the *additional* discounting brought about by low or decreased poverty

[17] See, for example, Pande (2011), Banerjee et al. (2014), and Lieberman, Posner and Tsai (2014).

[18] See, for example, Chandra (2004) and De La O and Rodden (2008).

[19] 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 (e.g., 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.

[20] 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 González-Ocantos, Kiewiet de Jonge and Nickerson (2014), Jensen and Justesen (2014), Szwarcberg (2015), and Muños (2019, 228-229) have provided quantitative confirmation as well.

[21] 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; Chang and Kerr, 2017; Solaz, De Vries and de Geus, 2019; Leight et al., 2020; Bøttkjær and Justesen, 2021). That is particularly the case when voters are poor and less educated (Keefer, 2007*a*; 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).

on $W(\gamma r_{i1})$ through β_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 decreased.

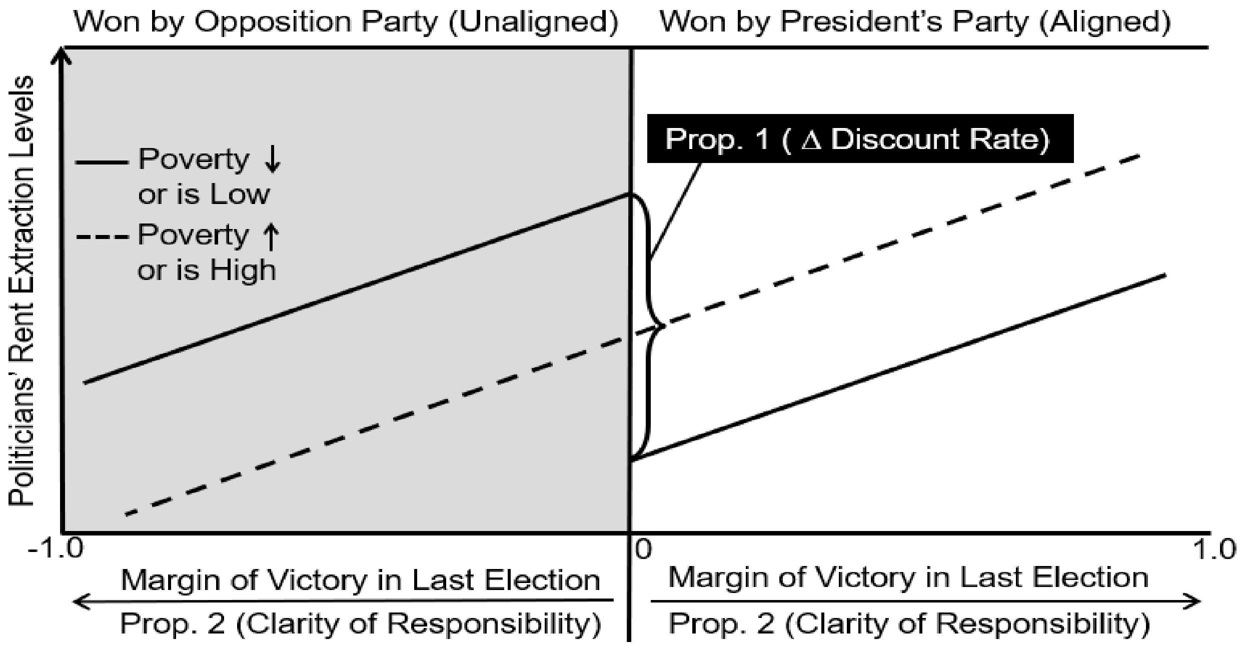
Especially given information's mixed record in fostering political accountability in poor environments,[22] it is crucial to understand how clarity of responsibility fosters different discount rates of corruption-related benefits. Per Schwindt-Bayer and Tavits (2016) and Figure 1, alignment makes identifying clarity of responsibility easier. Accordingly, we suggest that a magnifies the penalization imposed by low or reduced poverty ($\beta_i \in (0,1)$) on the 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 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

[22] See, for example, Keefer (2004, 2007*a*,*b*), Kosack and Fung (2014), Chong et al. (2015), Fox (2015), Dunning et al. (2019).

Figure 2: Graphic Presentation of Propositions 1 and 2



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:

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

Proposition 1: *Optimal rents for aligned politicians are less than rents for unaligned politicians at the cutoff (i.e., the margin of victory is zero) when the electorate's economic circumstances are good or have improved.*

The differing discounting rates for aligned and unaligned electorates drives Proposition 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 the margin of victory 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 line in Figure 2 and proof in 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 line in Figure 2 captures such a scenario, and the proof in Appendix C provides the relevant derivation.

Proposition 2: *Optimal rents for aligned politicians increase with respect to the margin of victory, while they decrease with respect to the margin of victory 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 quality of information mechanism described in Section 1.2.1 and Appendix D. Unaligned local-level politicians, in turn, react by reducing their optimal rent-seeking behavior in a manner consistent with the margin of victory. On that score, it may seem counter-intuitive for unaligned politicians with large margins of victory to not capture higher levels of rents given the lack of clarity of responsibility from alignment. However, it is necessary to recall that the lack of alignment necessarily entails lower levels of potential rents to capture (e.g., Brollo and Nannicini, 2012). Consequently, unaligned politicians have no choice but to compete more on valence appeals, making corruption-related scandals more electorally costly. By extension, unaligned politicians only capture more rents when their margin of victory is low, the political information environment is noisy (see Section 1.2.1 and Appendix D), and it is thus easier to get away with corruption.

Consistent with the clientelism and decentralization literatures (e.g., Greene, 2010; Brollo and Nannicini, 2012; Carozzi and Repetto, 2016), the opposite effect takes place in the aligned localities. Because the clarity of responsibility from alignment positively affects citizens' satisfaction with local-level politician i (see Figure 1), it provides aligned politicians with more opportunity for rent extraction as the margin of victory increases (see Figure 2). Although politicians may be tempted to extract more rents as $MV \rightarrow 0$ and re-election may appear less likely, per Niehaus and Sukhtankar (2013) and Equation (3), most politicians' rent extraction decisions entail a multi-term calculation. In both developing and wealthier countries, the rents (and other benefits) from public office are so high that politicians seek to retain them (Eggers and Hainmueller, 2009; Querubín and Snyder, 2013; Fisman, Schulz and Vig, 2014). Accordingly, our model is consistent with Ferraz and Finan (2011), who show both theoretically and empirically that politicians' re-election incentives are strong and mostly temper politicians' incentives to extract rents in the very short-term.

Proof: See Appendix C.

1.4. Summary of the Theoretical Results

A significant strand of the corruption literature argues that clarity of responsibility reduces corruption (e.g., Schwindt-Bayer and Tavits, 2016). However, another strand of the literature suggests that corruption increases with higher budgetary allocations (Brollo et al., 2013), where the latter often depend on the party alignment of the local-level politician (Bracco et al., 2015). Our model integrates both perspectives and shows that party alignment, a prominent manifestation of clarity of responsibility, only has a conditional effect on corruption. More specifically, party alignment only reduces corruption under both lower poverty and higher electoral competition. Lower poverty means that the greater clientelistic resources that aligned politicians can share are less valuable to voters, and higher electoral competition makes a potential corruption scandal more costly for politicians. In the next section, we explain our research design to test the model's predictions using unique, objective data on corruption from Guatemala.

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. In 1996, the country emerged from a devastating, 36-year civil war and since then, Guatemala has registered some democratic advances but maintains significant authoritarian enclaves and some institutional challenges (González, 2014).

Corruption, clientelism, and organized crime present particularly onerous challenges for Guatemala. The country's 2006-2019 partnership with the United Nations' International Commission Against Impunity (CICIG) helped dismantle some powerful drug-trafficking networks and some high-level corruption (Fisman and Golden, 2017; Trejo and Nieto-Matis, 2019). Notably, CICIG investigations helped lead to the indictment and removal from office of former President Otto Pérez Molina in 2015. 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, the country is one of the very few in the world where we can 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 Local Average Treatment Effect (LATE), as:

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

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 τ through a local polynomial regression following Cattaneo, Idrobo and Titiunik (2019, 70):

$$\begin{aligned}
&r_i = \alpha + f(MV_i) + \tau D_i + Z_i' \rho + \eta_i \\
&\text{where } f(MV_i) = \sum_{k=1}^{p} \beta_k MV_i^p + \sum_{k=1}^{p} \gamma_k D_i \cdot MV_i^p
\end{aligned} \tag{7}$$

where α is the intercept, η 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. Consistent with this paper's attempt to better understand the relationship between corruption and modernization theory, the poverty data specifically refer to the percent of people below the income-based 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, 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. Finally, 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.

As we show in Tables B5 and B6, the low/high poverty samples vary in terms of whether they contain municipalities with increased or decreased poverty rates from one census to the next. The same is true when we analyze these samples by alignment status in Tables B7 and

B8. Therefore, the low/high poverty measure and poverty-increasing/decreasing measure capture similar but distinct aspects of economic modernization.

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, 2007*a*,*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.[23] Accordingly, we provide a relevant analysis of these missing 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

[23] 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.

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. (2021).

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) density 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.[24]

[24] 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.

2.5. Corruption Data

The corruption data for this study come from Guatemala's Comptroller General (*Contraloría General de Cuentas*), which ranks very highly according to the only international index of supreme audit institutions (SAIs) from the World Bank (Gurazada et al., 2021). In its inaugural 2021 report, the World Bank ranked the SAIs from 118 countries on the basis of ten indicators: constitutional framework; appointment process transparency for the SAI head; financial autonomy; audits types; operational autonomy; staffing; mandate to decide on audit scope; access to records and information; and audit report rights and obligations. Each SAI then receives a final 0-10 score, ranging from 10 (only South Africa and Seychelles) to 2.5 (only Chad). Guatemala's score of 8.5 gives it an effective place of 4/18. For comparison with the audit data used in previous studies (e.g., Ferraz and Finan, 2008; Larreguy, Marshall and Snyder, 2020), Guatemala's SAI ranks just behind those from Brazil and Mexico (final score: 9.0; effective place; 3/18).

While the World Bank SAI index is very helpful for understanding numerous aspects of audit independence, it does not provide much insight into a key concern: that the distribution of audits may be biased against political rivals (Denly, 2020). On that score, each year the Comptroller General audits circa 317 of Guatemala's 340 municipalities based on risk-centered decisions outlined in its Annual Plan (*Plan Anual de Auditoría*). Although the chance for partisan bias is low given the high audit intensity, we still test for that possibility in Appendix L.9. Using both a close-election regression discontinuity design and count models for all of the samples examined in this study (see Section 2.3), we find that unaligned municipalities are not more likely to be audited than their aligned counterparts. Accordingly, there are no concerns regarding partisanship and the audit distribution.

For each audited municipality from 2004-present, the Comptroller General publishes on its website: the number of overall infractions committed (*sancciones*), and the amount of stolen or misappropriated money in the local currency (Quetzales) associated with these infractions. Both of these variables serve as our study's dependent variables and correspond most closely with bureaucratic corruption. As Fisman and Golden (2017, 41) explain, bureaucratic corruption takes place because "politicians permit it or fail to exercise adequate oversight to prevent it, all too often because they themselves are benefiting financially and politically." In the case of these infractions-based measures in Guatemala, they encompasses both what Brollo et al. (2013, 1774) call "broad corruption" and "narrow corruption".[25] 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. Appendix B provides relevant descriptive statistics and maps.

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. Because more populous municipalities likely have more resources, which makes corruption more feasible, we use covariate data on population from Guatemala's National Statistics Institute. To similarly control for changes in resource availability, which may be linked to alignment status or transfer levels, we include data on public goods spending from the Guatemalan Ministry of Finance.[26] Corruption also has prominent relationships with reelection and inequality (Alesina and Angeletos, 2005; Ferraz and Finan, 2011; Vuković, 2020), so we include relevant data from the Guatemalan Electoral Institute and Guatemalan National Statistics Institute. Tables B3 and B4 presents descriptive statistics of all covariate data by party alignment status.

[25] *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).

[26] These public goods data are publicly available through the World Bank's (2019) BOOST Initiative. 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

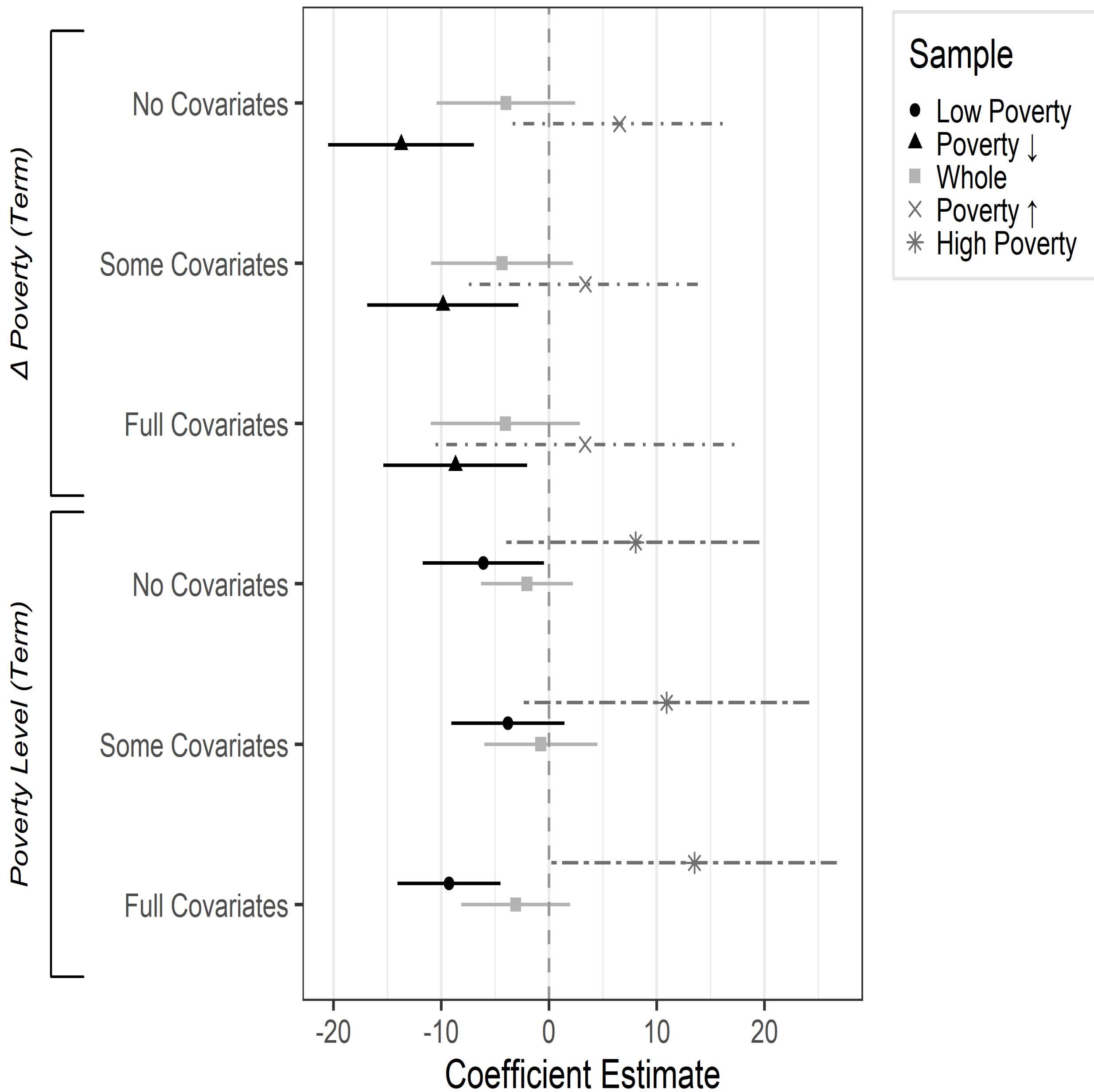
Figure 3 presents the main results for the infractions dependent variable by electoral term. We show the term-wise results for the (log) amounts of stolen/misappropriated money dependent variable in Figure 4. The figures corresponding to the year-wise results for the same dependent variables, Figures A.1 and A.2, can be found in Appendix A. 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 in Figure 3, aligned municipalities commit an average of 13.73 fewer infractions in the poverty-reducing sample and 6.09 fewer infractions in the low-poverty sample. In Appendix E, we undertake the extraordinary falsification test of adding fixed effects to our regression discontinuity estimates and find similar patterns as well. All of the results for the log amounts of stolen/misappropriated money in Figure 4 remain consistent, too.

Controlling for the influence of covariates within the automatically-derived, data-driven bandwidth in line with Calonico et al. (2019) also 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, and P.1, we also find similar results. Given that myriad 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.

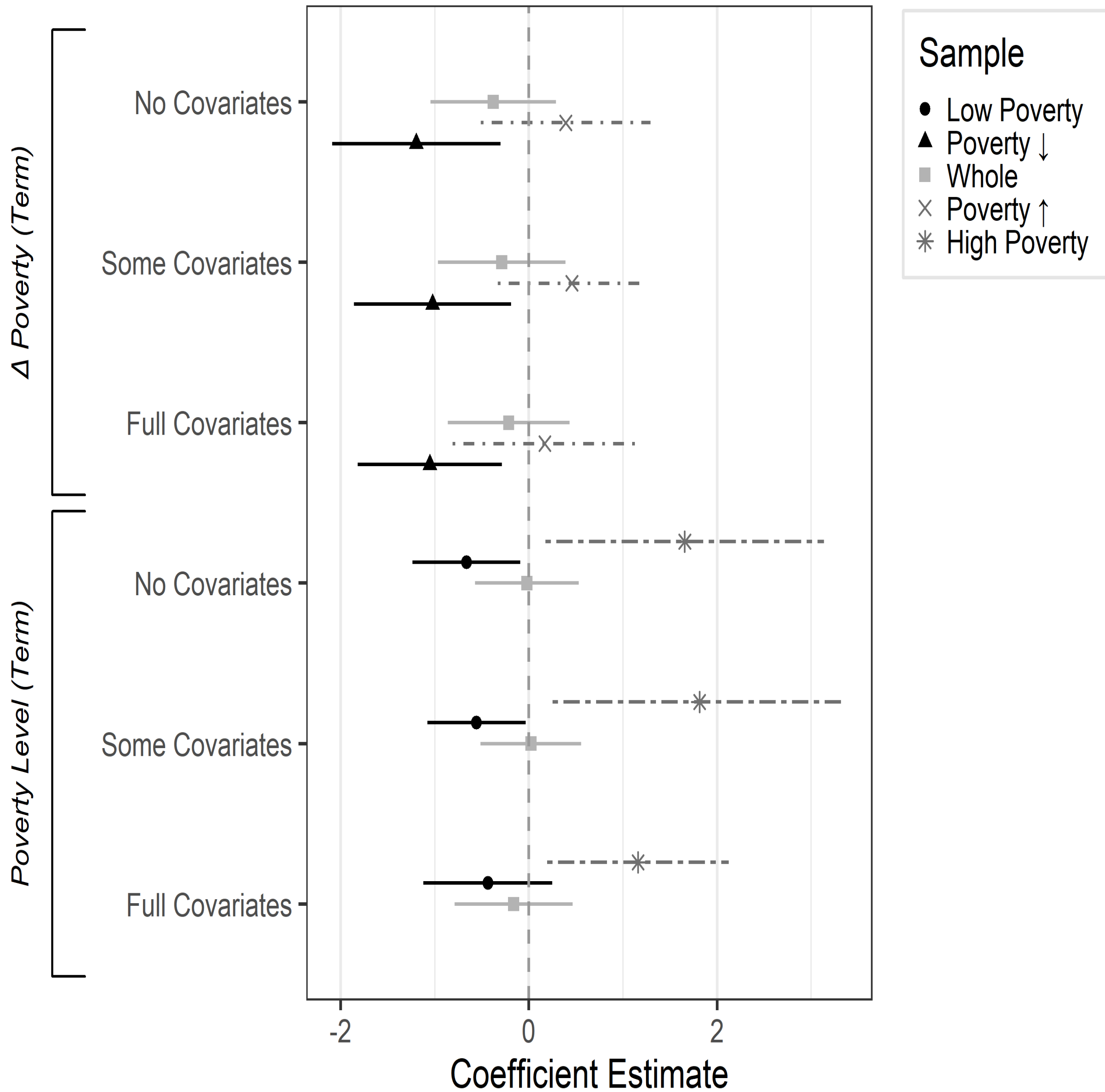
The effects of alignment on reducing corruption in the poverty-reducing sample are more pronounced within the final two years of the electoral term. Tables I13 and I14 show

Figure 3: Infraction Count by Term for Aligned Municipalities



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. Per Section 2.3, the poverty levels analyses correspond to 2004-2015, and the poverty change analyses correspond to 2010-2015. "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 4: Stolen/Misappropriated Money by Term for Aligned Municipalities (Log)



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. Per Section 2.3, the poverty levels analyses correspond to 2004-2015, and the poverty change analyses correspond to 2010-2015. "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.

the results for the last two years. When compared to the results from the first two years in Tables J21 and J22, 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 our Barro (1973)-Ferejohn (1986) model as well as related empirical work from 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; Curto-Grau, Solé-Ollé and Sorribas-Navarro, 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 Figures 3 and 4). 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 of stolen/misappropriated money 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), all of the aforementioned Figures and Appendix K show 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 second-degree polynomial fits, aligned municipalities commit an average 7.1 fewer infractions in the poverty-reducing sample and 4.2 fewer infractions in the low-poverty sample in each term. Results are a bit weaker for the log amounts of stolen/misappropriated money, as less specifications are statistically significant. Nevertheless, the results with the log amounts 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.[27] The lack of alignments for the 2016-2019 period limits the ability of the results in the previous sections to travel to other instances of party system instability. By the same token, the shock of electing a populist outsider and its consequent effects on alignment allows us to credibly

[27] New presidents in Guatemala take power in January, and the relevant elections take place late in the previous year.

Table 1: Infractions and Stolen/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.

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 money increased significantly after the election of Morales (see Table 1). Nevertheless, the (quasi) natural experiment of Morales' election is probably not sufficient for these descriptive statistics in Table 1 to be interpreted own 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 of stolen/misappropriated money 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

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

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.786***	0.748***	0.443***	0.787***	0.573***	0.487***
	(0.022)	(0.022)	(0.046)	(0.021)	(0.036)	(0.049)
Poverty Reduced		-0.071**	-0.073**			
		(0.035)	(0.036)			
Population (log)					1.571***	-0.337
					(0.209)	(0.301)
Re-elected Mayor					0.008	0.002
					(0.034)	(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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

configuration and oversee municipalities that commit more corruption.

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 of stolen/misappropriated money 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 of 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 of stolen/misappropriated money 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 (Placebo Tests)

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 amounts of misappropriated money in Appendix T through linear regression as well. The effect of alignment is only statistically distinguishable from zero when controlling for

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

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	-0.065	-0.061	-0.073	0.028	0.039	0.014
	(0.045)	(0.048)	(0.048)	(0.056)	(0.065)	(0.065)
Poverty Reduction		-0.019	-0.017			
		(0.049)	(0.049)			
Log Population					2.813^{***}	-1.017
					(0.500)	(0.998)
Reelected Mayor					0.064	0.065
					(0.066)	(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.

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 that we conduct here do not show causal relationships, but they provide support for the existence of a close election mechanism, which Propositions 1 and 2 buttress.

5. External Validity

As Findley, Kikuta and Denly (2021) explain, "external validity captures the extent to which inferences drawn from a given study's sample apply to a broader population [generalizability] or other target populations [transportability]." The previous sections have demonstrated the power of the mechanisms, and the regression discontinuity estimates underpinning the main results of this study are credible from the perspective of causal inference. However, the main regression discontinuity results only apply to a subset of Guatemalan municipalities without further analysis.

Some scholars have proposed methods to ensure that regression discontinuity results extend beyond the as good as random neighborhood around the cutoff (e.g., Angrist and Rokkanen, 2015; Dong and Lewbel, 2015; Wing and Bello-Gomez, 2018), but such analyses are not applicable for the present study. First, we do not theorize for such a possibility, so ex-post analysis along such lines would be purely exploratory. Second, both our theory and empirics, including results from relevant placebo tests, suggest that the results do not hold outside of close elections. Accordingly, we focus our generalizability analysis on whether the results hold for different subsets of the close-elections samples, and whether the close-election samples are representative of the rest of Guatemalan municipalities.

As we show in Appendix L.10, we find the same patterns as the main analysis when we restrict the sample to municipalities that the Comptroller General audited in all four years of each respective electoral term. The same is true when we examine the average number of infractions and (log) amounts of stolen/missing money per electoral term, taking into account the number of times a municipality was audited in each term (see Appendix L.11). That is accurate for both the low-poverty and poverty-decreasing samples within each subset.

To examine whether the municipalities within the close-election bandwidth are fundamentally different than the rest, we turn to balance tests of the pre-treatment covariates in the close- and noncompetitive election samples. Balance tests are unnecessary for internal validity purposes, notably because balance relates to the sample, and frequentist statistical inference assumes that the sample broadly represents the population of interest (Gill, 1999; Ho et al., 2007; Imai, King and Stuart, 2008; Mutz, Pemantle and Pham, 2019).[28] For external validity purposes, though, we need to test that assumption, particularly in regression discontinuity designs that only examine part of the overall sample. As Table 4 shows, all pre-treatment covariates show similar distributions in both the close- and noncompetitive election samples, as denoted by the clustered, robust p-values that we calculate per Hansen and Bowers (2008). We exclude the reelection variable because it is post-treatment to the

[28]More generally, frequentist statistical inference is based on the idea of a sampling distribution, for which the data are presumed to be random samples of the population (Gill, 1999).

Table 4: Balance Tests for Close-Election and Noncompetitive-Election Samples

	(1) Close Election (MV $\leq$ 10%)		(2) Noncompetitive Election (MV > 10%)		T-test P-value
Variable	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	(1)-(2)
Panel A: Whole Sample (Poverty-Change Analysis) [2010-2015]					
Population (log)	387	10.230	279	10.305	0.287
	[274]	(0.054)	[220]	(0.068)	
Inequality (Gini)	367	25.453	265	25.438	0.968
	[263]	(0.307)	[217]	(0.338)	
Public Goods p/c (log)	387	7.210	279	7.209	0.996
	[274]	(0.044)	[220]	(0.105)	
Panel B: Poverty-Reducing Sample (Poverty-Change Analysis) [2010-2015]					
Population (log)	164	10.312	120	10.155	0.063*
	[115]	(0.073)	[93]	(0.075)	
Inequality (Gini)	164	25.584	120	25.467	0.829
	[115]	(0.410)	[93]	(0.500)	
Public Goods p/c (log)	164	7.025	120	7.136	0.514
	[115]	(0.065)	[93]	(0.166)	
Panel C: Poverty-Increasing Sample (Poverty-Change Analysis) [2010-2015]					
Population (log)	189	10.173	121	10.292	0.272
	[134]	(0.080)	[100]	(0.103)	
Inequality (Gini)	189	25.653	121	26.055	0.475
	[134]	(0.464)	[100]	(0.486)	
Public Goods p/c (log)	189	7.296	121	7.214	0.644
	[134]	(0.058)	[100]	(0.174)	
Panel D: Whole Sample (Poverty-Level Analysis) [2004-2015]					
Population (log)	709	10.164	623	10.118	0.350
	[316]	(0.052)	[308]	(0.056)	
Inequality (Gini)	689	22.919	605	22.564	0.275
	[315]	(0.255)	[306]	(0.298)	
Public Goods p/c (log)	387	7.573	279	7.653	0.148
	[274]	(0.043)	[220]	(0.047)	
Panel E: Low-Poverty Sample (Poverty-Level Analysis) [2004-2015]					
Population (log)	333	10.160	314	10.085	0.272
	[186]	(0.074)	[178]	(0.074)	
Inequality (Gini)	333	23.628	314	23.405	0.602
	[186]	(0.322)	[178]	(0.395)	
Public Goods p/c (log)	150	7.755	123	7.860	0.175
	[115]	(0.065)	[104]	(0.063)	
Panel F: High-Poverty Sample (Poverty-Level Analysis) [2004-2015]					
Population (log)	376	10.167	309	10.152	0.829
	[200]	(0.062)	[184]	(0.073)	
Inequality (Gini)	356	22.255	291	21.658	0.212
	[182]	(0.370)	[170]	(0.431)	
Public Goods p/c (log)	237	7.458	156	7.489	0.650
	[182]	(0.049)	[133]	(0.061)	

Note: Standard errors clustered by municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

election—or what Angrist and Pischke (2008) call a “bad control”.[29] Overall, the results from the balance and other generalizability tests suggests that the data are as-if randomly sampled from the broader population of interest (see Findley, Kikuta and Denly, 2021).

Finally, making transportability inferences is more challenging, because we do not have data for other countries. Nevertheless, the study’s robust results enable us to conjecture that they 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 (Ferraz and Finan, 2008; Bobonis, Cámara Fuertes and Schwabe, 2016). Finally, because citizens preferences’ do not immediately respond to reductions in poverty or increasing income (Treisman, 2020), the poverty change data need to have mid-to-long term intervals for corruption levels to possibly change. In other words, it is unlikely that small yearly changes in poverty will have the same effects on corruption that we have documented throughout this paper with mid-to-longer term intervals.

6. 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 party alignment’s conditional effects on corruption is dependent on poverty and electoral competition, we demonstrate that such a theory is no longer “a long way off”. Overall, our 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 focuses on aligned politicians.

We find causal support for our theory using close-election regression discontinuity de-

[29] For more on which variables to include in balance tests, see Dunning (2012, 239-242).

signs that measure corruption both through audit infractions and the (log) amounts of misappropriated/stolen money associated with those infractions. Analysis of both dependent variables demonstrate strong support for our theory, though 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 very similar, albeit somewhat weaker, when party alignment dovetails with significant electoral competition and extreme poverty reduction. From measurement and external validity perspectives, our paper undertakes 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 in the literature.[30]

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 alignment to decrease corruption through modernization forces such as poverty reduction. 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. What helps temper these pressures are competitive elections, which suggests that they have potential policy relevance as well.

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

[30] 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).

inferences about each subgroup independently. Regression discontinuity designs cannot incorporate interactions, and Guatemala (and most developing countries) only measure local-level poverty intermittently, so subgroup analysis was our only means to test our hypothesis. Nevertheless, our overall results suggest that modernization and political-institutional forces combine to place subnational units within a polity on different starting points, leading to different corruption paths.

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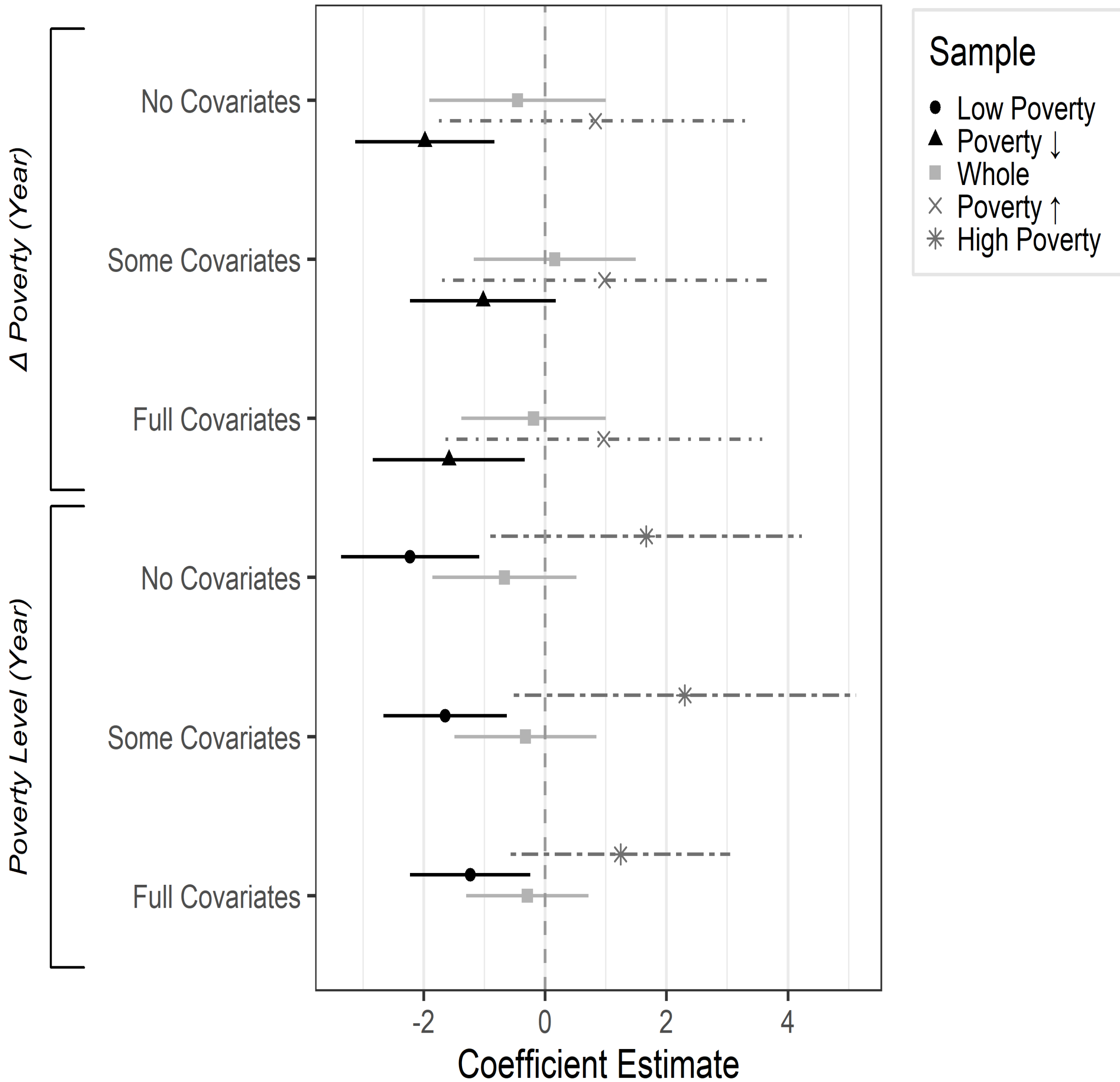
Appendices

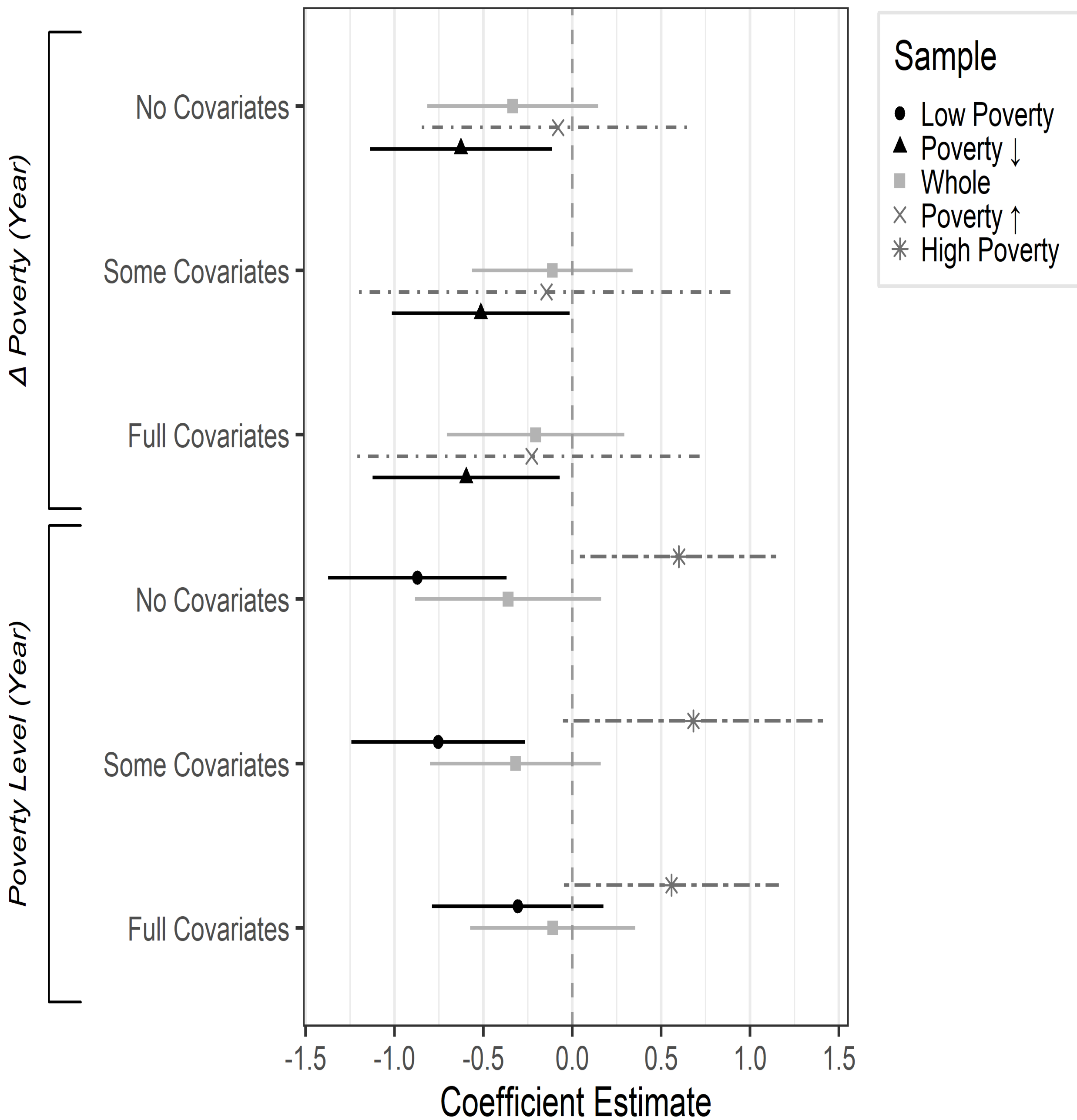
A. Additional Coefficient Plots for Year-Wise Results

Figure A.1: Infraction Count by Year for Aligned Municipalities



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. Per Section 2.3, the poverty levels analyses correspond to 2004-2015, and the poverty change analyses correspond to 2010-2015. "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: Stolen/Misappropriated Money by Year for Aligned Municipalities (Log)



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. Per Section 2.3, the poverty levels analyses correspond to 2004-2015, and the poverty change analyses correspond to 2010-2015. "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 and Maps

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

Panel A: Infractions (Year Viewpoint)	Increase Unaligned		Increase Aligned		Decrease Unaligned		Decrease Aligned	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Number of Infractions: All Years	9.453	1,107	6.376	348	8.704	1,043	5.443	271
Log Amount of Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: Last year of Term	11.83	198	11.89	111	11.84	192	11.61	91
Panel B: Infractions (Electoral Term)	**Increase Unaligned**		**Increase Aligned**		**Decrease Unaligned**		**Decrease Aligned**	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Number of Infractions: All Years	29.56	354	19.99	111	27.10	335	16.21	91
Log Amount of Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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) VARIABLES	High Unaligned Mean	N	High Aligned Mean	N	Low Unaligned Mean	N	Low Aligned Mean	N
Number of Infractions: All Years	6.854	1,393	5.574	432	8.139	1,265	5.638	475
Log Amount of Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: Last year of Term	11.83	198	11.76	96	11.83	192	11.77	106

Panel B: Infractions (Electoral Term) VARIABLES	High Unaligned Mean	N	High Aligned Mean	N	Low Unaligned Mean	N	Low Aligned Mean	N
Number of Infractions: All Years	24.60	468	20.80	118	28.86	409	19.35	136
Log Amount of Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Stolen/Misappropriated Money: 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 Unaligned		Increase Aligned		Decrease Unaligned		Decrease 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 coefficiant	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 Unaligned		Increase Aligned		Decrease Unaligned		Decrease 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 coefficiant	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		High Aligned		Low Unaligned		Low Aligned	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
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 coefficiant	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		High Aligned		Low Unaligned		Low Aligned	
VARIABLES	mean	N	mean	N	mean	N	mean	N
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 coefficiant	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.

Table B5: Wealth Distribution by Poverty Sub-Sample in 2002

Poverty-Reducing Sample			Poverty-Increasing Sample		
Poverty Level	Frequency	%	Poverty Level	Frequency	%
Below Median	41	28.87	Below Median	93	60
Above Median	101	71.13	Above Median	62	40
Total	142	100	Total	155	100

Note: "Frequency" refers to the number of municipalities that had poverty levels below/above the median poverty level in the poverty mapping for the 2002 census, which serves as a proxy for the wealth distribution of the municipalities. "Poverty-Reducing Sample" refers to the sub-sample of municipalities where poverty decreased from 2002 to 2011, while "Poverty-Increasing Sample" refers to the sub-sample of municipalities where poverty increased between the two poverty measurements.

Table B6: Wealth Distribution by Poverty Sub-Sample in 2011

Poverty-Reducing Sample			Poverty-Increasing Sample		
Poverty Level	Frequency	%	Poverty Level	Frequency	%
Below Median	91	64.08	Below Median	59	38.06
Above Median	51	35.92	Above Median	96	61.94
Total	142	100	Total	155	100

Note: "Frequency" refers to the number of municipalities that had poverty levels below/above the median poverty level in the poverty mapping for the 2011 census, which serves as a proxy for the wealth distribution of the municipalities. "Poverty-Reducing Sample" refers to the sub-sample of municipalities where poverty decreased from 2002 to 2011, while "Poverty-Increasing Sample" refers to the sub-sample of municipalities where poverty increased between the two poverty measurements.

Table B7: Wealth Distribution by Alignment and Poverty Sub-Samples in 2002

Municipalities Above Median Poverty Level				Municipalities Below Median Poverty Level			
Sample	Alignment	Freq.	%	Sample	Alignment	Freq.	%
Poverty-Reducing	Unaligned	71	70.3	Poverty-Reducing	Unaligned	25	60.98
Poverty-Reducing	Aligned	30	29.7	Poverty-Reducing	Aligned	16	39.02
	Total	101	100		Total	41	100
Poverty-Increasing	Unaligned	39	62.90	Poverty-Increasing	Unaligned	68	73.12
Poverty-Increasing	Aligned	23	37.10	Poverty-Increasing	Aligned	25	26.88
	Total	62	100		Total	93	100

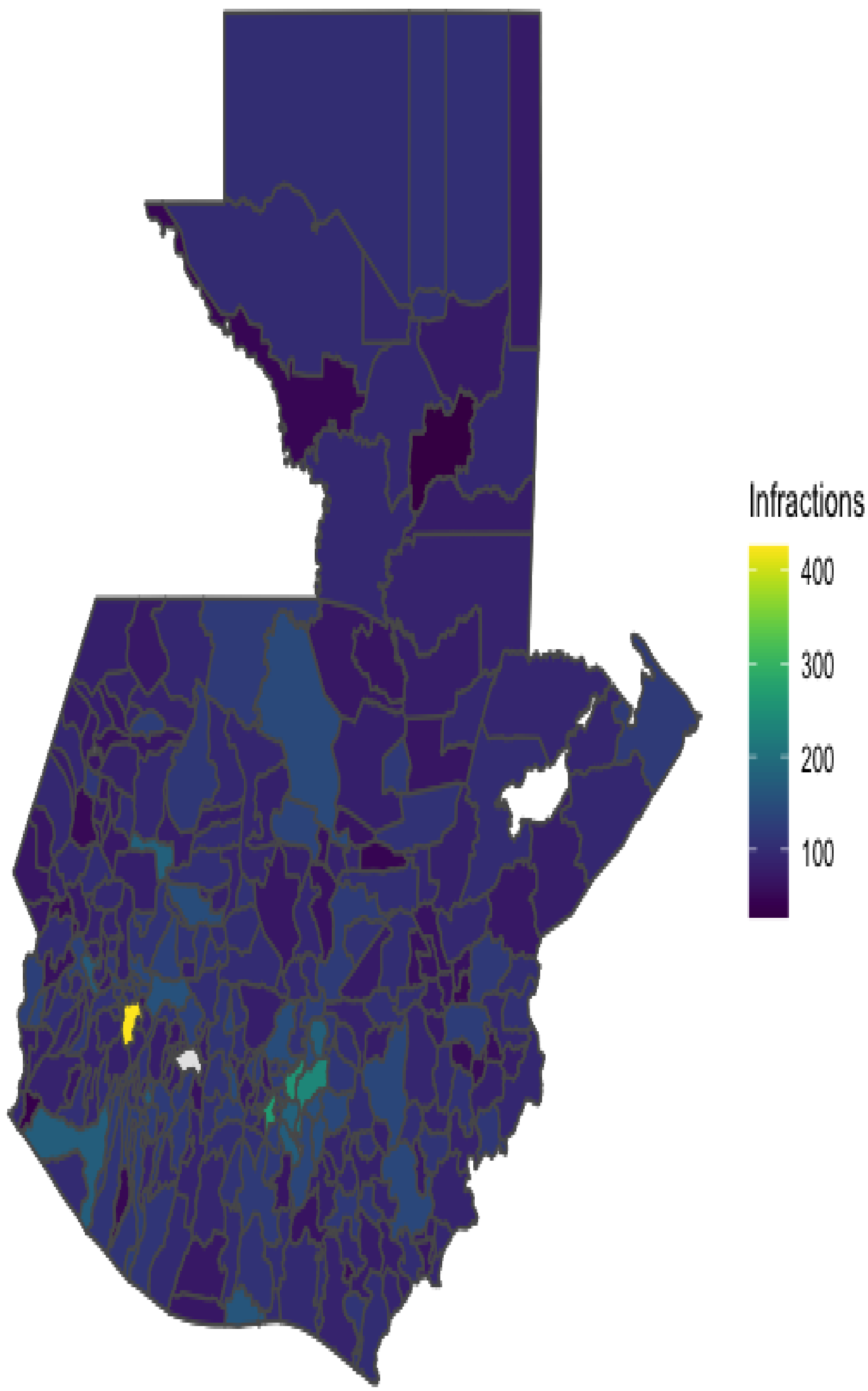
Note: "Municipalities Above Median Poverty Level" refers to the municipalities that have poverty levels above the national medial poverty levels in the poverty mapping for the 2002 census, while "Municipalities Below Median Poverty Level" refer to the municipalities below the national median poverty level. "Reducing" refers to the sub-sample of municipalities where poverty decreased from 2002 to 2011, while "Increasing" refers to the sub-sample of municipalities where poverty increased during the same period. "Freq." is the number of municipalities by alignment status in the Poverty-Increasing/Decreasing Samples.

Table B8: Wealth Distribution by Alignment and Poverty Sub-Samples in 2011

Municipalities Above Median Poverty Level				Municipalities Below Median Poverty Level			
Sample	Alignment	Freq.	%	Sample	Alignment	Freq.	%
Poverty-Reducing	Unaligned	37	72.55	Poverty-Reducing	Unaligned	59	64.84
Poverty-Reducing	Aligned	14	27.45	Poverty-Reducing	Aligned	32	35.16
	Total	51	100		Total	91	100
Poverty-Increasing	Unaligned	64	66.67	Poverty-Increasing	Unaligned	43	72.88
Poverty-Increasing	Aligned	32	33.33	Poverty-Increasing	Aligned	16	27.12
	Total	96	100		Total	59	100

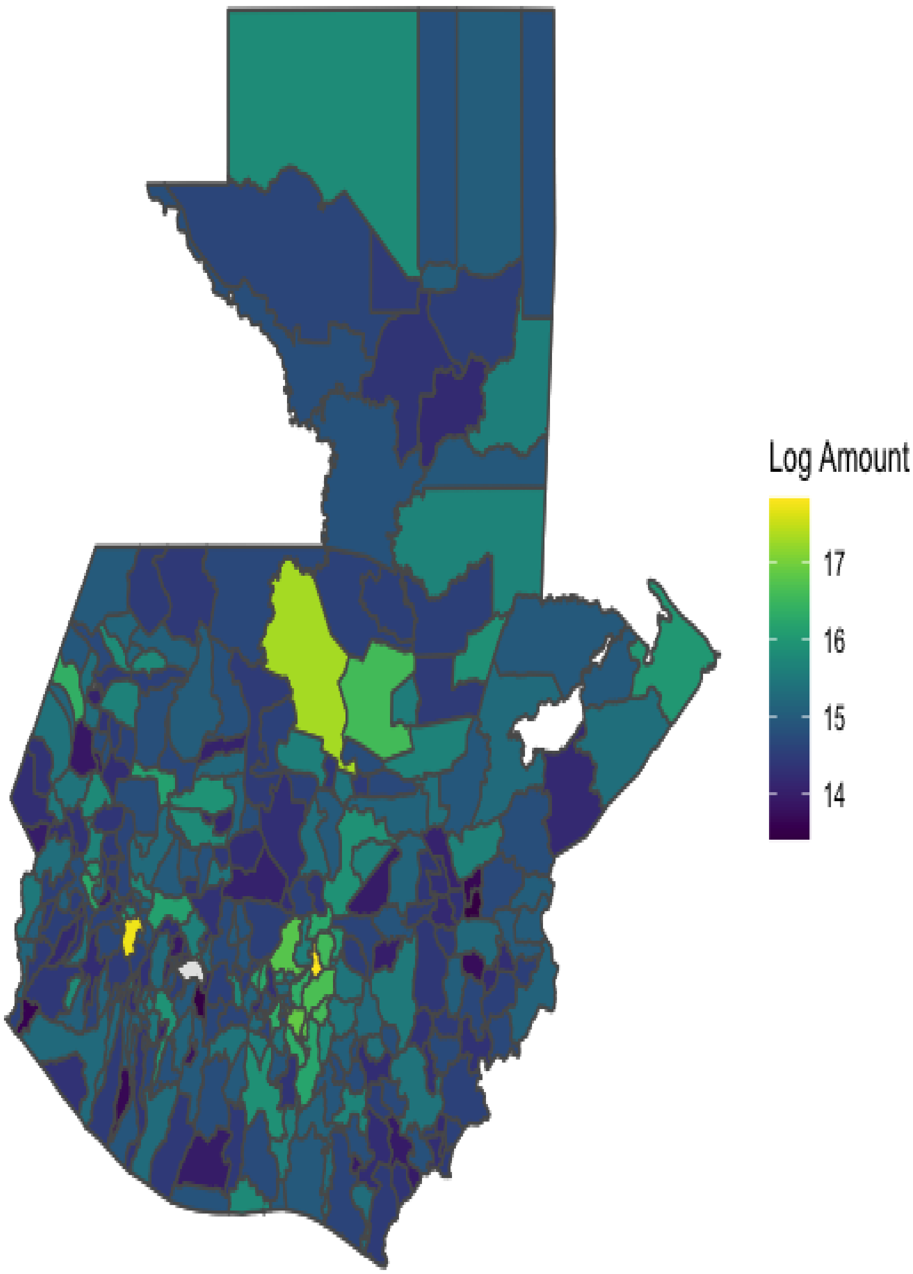
Note: "Municipalities Above Median Poverty Level" refers to the municipalities that have poverty levels above the national medial poverty levels in the poverty mapping for the 2011 census, while "Municipalities Above Median Poverty Level" refer to the municipalities below the national median poverty level. "Poverty- Reducing" refers to the sub-sample of municipalities where poverty decreased from 2002 to 2011, while "Poverty-Increasing" refers to sub-sample of municipalities where poverty increased during the same period. "Freq." is the number of municipalities by alignment status in the Poverty Increasing/Decreasing Samples.

Figure B.3: Total Infractions by Municipality, 2004-2019



Note: The three white areas are lakes.

Figure B.4: Total Misappropriated/Stolen Money by Municipality, 2004-2019



Note: The three white areas are lakes.

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):

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

Accordingly, we can rewrite the maximization problem as follows:

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

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

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

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

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

Now from the assumption on $t(\cdot)$, we know that as $MV \to 0$, $t(MV) \to 0$ since $t(\cdot)$ increases with respect to MV. Thus, as $MV \to 0$, Equation (11) can be written as:

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

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

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

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

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

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 $z, k > 0$.[31]

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

[31] 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 maximization of the second-order condition. By extension, therefore:

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

When $a = 1$:

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

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 $a = 0$:

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

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.[32] 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 $MV \to 0$—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.

[32] 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.[33] 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.

[33] 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 E9: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.649***	-1.985***	-0.802	-1.025	-0.938*	-1.587**
	(0.549)	(0.698)	(0.560)	(0.731)	(0.564)	(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**	-1.453*	-0.522	-0.727	-0.712	-1.365*
	(0.582)	(0.761)	(0.591)	(0.759)	(0.608)	(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 E10: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-10.98***	-13.73***	-7.952**	-9.853**	-7.339**	-8.692**
	(2.996)	(4.113)	(3.358)	(4.261)	(3.245)	(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*	-5.131*	-1.614	-2.859	-2.344	-4.842
	(1.982)	(2.717)	(2.118)	(2.820)	(2.252)	(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 E11: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.773***	-0.627**	-0.564**	-0.515*	-0.660**	-0.598*
	(0.216)	(0.311)	(0.256)	(0.304)	(0.264)	(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***	-0.587*	-0.488*	-0.454	-0.574**	-0.588*
	(0.213)	(0.310)	(0.263)	(0.312)	(0.267)	(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 E12: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.240***	-1.197**	-1.080***	-1.024**	-1.016***	-1.053**
	(0.427)	(0.544)	(0.389)	(0.508)	(0.371)	(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**	-0.762	-0.645*	-0.564	-0.730*	-0.697
	(0.369)	(0.470)	(0.381)	(0.483)	(0.374)	(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.

E.2. When Poverty is Low

Table E13: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.991***	-2.225***	-1.588***	-1.646***	-1.158**	-1.235**
	(0.616)	(0.692)	(0.541)	(0.618)	(0.494)	(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***	-1.988***	-1.289**	-1.223*	-1.043**	-1.011*
	(0.619)	(0.707)	(0.545)	(0.626)	(0.487)	(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 E14: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-5.810**	-6.086*	-4.698*	-3.821	-7.646***	-9.284***
	(2.826)	(3.422)	(2.517)	(3.188)	(2.589)	(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**	-6.486**	-5.332***	-5.963***	-5.948**	-6.227**
	(2.393)	(2.727)	(1.958)	(2.262)	(2.398)	(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 E15: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.735***	-0.872***	-0.632***	-0.754**	-0.243	-0.307
	(0.265)	(0.305)	(0.244)	(0.297)	(0.244)	(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***	-0.776***	-0.554**	-0.596**	-0.334	-0.392
	(0.246)	(0.300)	(0.223)	(0.285)	(0.236)	(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 E16: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.562*	-0.665*	-0.506*	-0.557*	-0.295	-0.437
	(0.299)	(0.348)	(0.264)	(0.317)	(0.291)	(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**	-0.678*	-0.596**	-0.616*	-0.344	-0.465
	(0.300)	(0.353)	(0.274)	(0.334)	(0.287)	(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.

F. When Extreme Poverty is Low/Decreasing

F.1. When Extreme Poverty Decreases

Table F17: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.489***	-1.837**	-1.152*	-1.255	-1.278**	-1.939**
	(0.557)	(0.724)	(0.612)	(0.763)	(0.648)	(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**	-1.604**	-0.992	-1.060	-1.147*	-1.524*
	(0.585)	(0.756)	(0.632)	(0.781)	(0.666)	(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 F18: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-6.891**	-7.099**	-7.733***	-7.943**	-9.355***	-10.34***
	(2.959)	(3.526)	(2.867)	(3.342)	(3.010)	(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*	-4.751*	-3.573	-4.167	-4.639**	-5.992**
	(2.040)	(2.713)	(2.258)	(2.827)	(2.362)	(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 F19: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.454*	-0.487	-0.315	-0.285	-0.363	-0.340
	(0.237)	(0.308)	(0.257)	(0.317)	(0.268)	(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*	-0.475	-0.294	-0.267	-0.321	-0.396
	(0.238)	(0.308)	(0.259)	(0.321)	(0.266)	(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 F20: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.710*	-0.764	-0.618*	-0.695	-0.692**	-0.934**
	(0.397)	(0.488)	(0.354)	(0.492)	(0.333)	(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	-0.565	-0.367	-0.367	-0.509	-0.636
	(0.358)	(0.429)	(0.328)	(0.465)	(0.321)	(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.

F.2. When Extreme Poverty is Low

Table F21: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.844***	-2.016***	-1.264**	-1.328**	-0.812	-0.885
	(0.656)	(0.701)	(0.564)	(0.620)	(0.583)	(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***	-1.914***	-1.049*	-1.052*	-0.732	-0.665
	(0.651)	(0.702)	(0.552)	(0.623)	(0.553)	(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 F22: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-3.968	-4.172	-2.367	-2.598	-7.362***	-9.000***
	(3.127)	(3.617)	(2.945)	(3.622)	(2.815)	(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*	-5.305*	-4.175**	-4.469*	-6.156**	-7.063**
	(2.541)	(2.781)	(2.073)	(2.310)	(2.476)	(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 F23: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.466*	-0.632*	-0.303	-0.388	-0.0730	-0.115
	(0.270)	(0.347)	(0.247)	(0.331)	(0.265)	(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*	-0.546*	-0.171	-0.206	-0.0458	-0.0920
	(0.249)	(0.323)	(0.238)	(0.292)	(0.261)	(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 F24: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.334	0.503	0.671	0.835	-0.208	-0.408
	(0.701)	(0.848)	(0.801)	(0.932)	(0.339)	(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	0.457	0.615	0.763	-0.275	-0.440
	(0.694)	(0.830)	(0.809)	(0.923)	(0.325)	(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.

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	0.824	0.519	0.975	0.547	0.970
	(0.942)	(1.566)	(1.030)	(1.627)	(1.081)	(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	0.521	0.547	0.919	0.593	0.960
	(0.983)	(1.549)	(1.090)	(1.599)	(1.089)	(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	6.547	1.230	3.371	-2.501	3.318
	(3.797)	(6.035)	(4.063)	(6.561)	(4.456)	(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	1.881	1.701	3.016	0.936	2.655
	(3.180)	(4.584)	(3.540)	(5.265)	(3.735)	(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	-0.0806	0.255	-0.146	0.265	-0.229
	(0.302)	(0.466)	(0.315)	(0.641)	(0.315)	(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	0.176	0.0991	-0.190	0.0759	-0.221
	(0.285)	(0.401)	(0.355)	(0.632)	(0.376)	(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	0.391	0.413	0.455	0.301	0.169
	(0.371)	(0.548)	(0.398)	(0.478)	(0.389)	(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	-0.129	0.555	0.502	0.600*	0.530
	(0.329)	(0.651)	(0.346)	(0.448)	(0.334)	(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	1.664	1.486	2.301	0.829	1.241
	(1.060)	(1.561)	(1.195)	(1.715)	(0.771)	(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	1.438	1.389	2.185	0.605	1.742
	(0.912)	(1.377)	(1.030)	(1.573)	(0.651)	(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	8.026	7.291	10.90	7.115	13.48*
	(4.747)	(7.297)	(5.527)	(8.057)	(5.269)	(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	5.662	6.039	8.339	4.323	10.78
	(3.425)	(5.127)	(3.968)	(5.740)	(3.645)	(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	0.599*	0.543*	0.680	0.413	0.557
	(0.311)	(0.338)	(0.310)	(0.446)	(0.257)	(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*	0.559	0.548**	0.682*	0.345	0.440
	(0.293)	(0.353)	(0.277)	(0.412)	(0.228)	(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*	1.656*	1.408*	1.814*	0.562	1.159**
	(0.726)	(0.900)	(0.800)	(0.950)	(0.408)	(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*	1.532*	1.365*	1.705*	0.511	1.023*
	(0.690)	(0.891)	(0.749)	(0.917)	(0.349)	(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 H9: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.913	1.191	0.920	2.295	0.652	1.576
	(0.967)	(1.772)	(1.068)	(2.156)	(1.276)	(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	3.247	1.366	4.537	0.806	6.185
	(4.751)	(6.555)	(4.782)	(7.647)	(4.067)	(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 H10: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.157	-0.129	0.105	-0.0200	0.0987	-0.0855
	(0.344)	(0.700)	(0.360)	(0.731)	(0.366)	(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	0.237	0.210	0.116	0.139	0.139
	(0.441)	(0.550)	(0.461)	(0.702)	(0.456)	(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 H11: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.735	0.986	1.120	1.792	0.381	1.263
	(1.042)	(1.318)	(1.211)	(1.749)	(0.696)	(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	7.773	6.297	10.44	4.910	9.364
	(4.866)	(7.202)	(5.683)	(8.168)	(5.036)	(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 H12: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.157	0.0686	0.131	0.0852	0.243	0.305
	(0.267)	(0.353)	(0.295)	(0.331)	(0.241)	(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	0.320	0.236	0.310	0.372	0.895
	(0.350)	(0.488)	(0.424)	(0.509)	(0.412)	(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 I13: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.925**	-2.466**	-1.133	-1.546	-1.173	-2.057*
	(0.806)	(1.068)	(0.804)	(1.092)	(0.850)	(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**	-5.039**	-2.335	-3.189	-2.390	-3.532
	(1.634)	(2.158)	(1.650)	(2.196)	(1.917)	(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 I14: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.225***	-1.296***	-0.997***	-1.139**	-1.011***	-1.157**
	(0.373)	(0.449)	(0.350)	(0.453)	(0.360)	(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***	-1.197**	-0.997**	-1.051**	-0.923**	-1.014**
	(0.423)	(0.512)	(0.410)	(0.514)	(0.386)	(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 I15: 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*	-1.719	-1.173	-1.009	0.243	0.316
	(0.910)	(1.094)	(0.919)	(1.102)	(0.930)	(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	-3.091	-3.364*	-3.277	-4.214**	-4.307**
	(1.846)	(2.187)	(1.825)	(2.144)	(1.864)	(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 I16: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.861***	-1.009***	-0.816**	-0.942**	-0.433	-0.603
	(0.318)	(0.356)	(0.331)	(0.395)	(0.355)	(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	-0.699	-0.531	-0.607	-0.684*	-0.839*
	(0.358)	(0.428)	(0.354)	(0.456)	(0.355)	(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 I17: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.479**	-1.910*	-1.278	-1.519	-1.111	-2.093*
	(0.722)	(1.000)	(0.844)	(1.103)	(0.885)	(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	-3.239	-2.588	-3.158	-3.525*	-4.544*
	(1.609)	(2.106)	(1.700)	(2.227)	(1.930)	(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 I18: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.908**	-1.048**	-0.595*	-0.779*	-0.564*	-0.710*
	(0.376)	(0.435)	(0.344)	(0.453)	(0.342)	(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**	-0.917*	-0.564	-0.742	-0.572	-0.802
	(0.404)	(0.472)	(0.392)	(0.505)	(0.351)	(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 I19: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.018**	-2.550**	-1.062	-1.245	0.132	-0.0489
	(0.905)	(1.205)	(0.866)	(1.179)	(0.834)	(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*	-4.337*	-3.196*	-3.937*	-5.568***	-5.743***
	(1.885)	(2.497)	(1.740)	(2.303)	(1.805)	(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 I20: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.717**	-0.826**	-0.597**	-0.654*	-0.375	-0.500
	(0.313)	(0.353)	(0.304)	(0.392)	(0.384)	(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	-0.711	-0.507	-0.512	-0.678*	-0.826
	(0.401)	(0.443)	(0.393)	(0.479)	(0.411)	(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 J21: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.970	-0.958	-0.195	-0.195	-0.604	-1.144
	(0.752)	(1.023)	(0.823)	(1.022)	(0.914)	(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	-1.945	-0.637	-0.415	-1.373	-2.358
	(1.503)	(2.092)	(1.577)	(2.094)	(1.747)	(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 J22: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.146	0.289	0.299	0.379	0.179	0.132
	(0.307)	(0.354)	(0.299)	(0.346)	(0.362)	(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	0.165	0.136	0.179	0.0784	-0.0730
	(0.390)	(0.447)	(0.374)	(0.435)	(0.478)	(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 J23: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.470***	-2.940***	-2.052**	-2.329**	-1.880**	-2.213**
	(0.805)	(0.936)	(0.804)	(0.944)	(0.751)	(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**	-5.180**	-4.085**	-4.699**	-3.672**	-4.944**
	(1.794)	(2.127)	(1.624)	(1.930)	(1.623)	(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 J24: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.274	-0.405	-0.149	-0.184	-0.102	-0.182
	(0.328)	(0.393)	(0.309)	(0.348)	(0.307)	(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	-0.233	-0.225	-0.272	-0.156	-0.302
	(0.350)	(0.434)	(0.321)	(0.384)	(0.318)	(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 J25: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.331*	-1.425	-0.791	-0.835	-0.891	-1.331
	(0.770)	(0.971)	(0.807)	(0.997)	(0.836)	(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*	-2.872	-1.601	-1.667	-1.925	-2.818
	(1.543)	(1.957)	(1.610)	(2.030)	(1.675)	(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 J26: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.172	0.264	0.169	0.271	0.231	0.114
	(0.351)	(0.392)	(0.342)	(0.397)	(0.353)	(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	0.166	-0.157	0.143	-0.000151	-0.0117
	(0.384)	(0.471)	(0.355)	(0.473)	(0.386)	(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 J27: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.033**	-2.577**	-1.392	-1.607	-1.426*	-1.741*
	(0.931)	(1.075)	(0.941)	(1.124)	(0.863)	(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	-4.235*	-2.789	-3.225	-2.105	-3.603
	(2.056)	(2.404)	(1.890)	(2.266)	(1.882)	(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 J28: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.135	-0.323	0.137	0.0483	0.185	0.0978
	(0.376)	(0.437)	(0.361)	(0.428)	(0.360)	(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	-0.192	-0.0704	-0.211	0.102	-0.190
	(0.400)	(0.491)	(0.362)	(0.416)	(0.361)	(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 K29: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0346	-0.455	0.110	0.158	-0.132	-0.193
	(0.581)	(0.885)	(0.532)	(0.812)	(0.634)	(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	-0.143	0.370	0.338	0.0368	0.231
	(0.624)	(0.868)	(0.578)	(0.713)	(0.654)	(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 K30: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.232	-3.996	-2.892	-4.365	-5.220*	-4.038
	(2.695)	(3.911)	(2.848)	(3.998)	(3.141)	(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	-0.196	1.232	1.225	-0.758	0.0322
	(1.954)	(2.696)	(1.989)	(2.425)	(2.234)	(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 K31: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.173	-0.336	-0.0737	-0.113	-0.0910	-0.207
	(0.217)	(0.292)	(0.210)	(0.275)	(0.211)	(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	-0.272	-0.0318	-0.0187	-0.0378	-0.0905
	(0.221)	(0.290)	(0.213)	(0.255)	(0.207)	(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 K32: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0541	-0.379	0.0199	-0.288	-0.203	-0.215
	(0.247)	(0.405)	(0.247)	(0.412)	(0.287)	(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	-0.200	0.161	0.0577	0.0716	0.0626
	(0.253)	(0.366)	(0.244)	(0.353)	(0.256)	(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 K33: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.326	-0.671	-0.233	-0.324	-0.296	-0.294
	(0.476)	(0.722)	(0.523)	(0.712)	(0.448)	(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	-0.527	0.128	-0.0374	-0.202	-0.222
	(0.438)	(0.718)	(0.457)	(0.692)	(0.444)	(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 K34: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.323	-2.044	-1.105	-0.758	-2.631	-3.089
	(2.158)	(2.585)	(2.360)	(3.186)	(2.323)	(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	-1.540	0.349	-0.526	-1.718	-2.156
	(1.626)	(2.637)	(1.683)	(2.545)	(2.383)	(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 K35: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.144	-0.362	-0.0799	-0.320	-0.0297	-0.111
	(0.193)	(0.318)	(0.182)	(0.292)	(0.190)	(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	-0.277	-0.0630	-0.202	-0.0232	-0.0849
	(0.194)	(0.307)	(0.185)	(0.268)	(0.186)	(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 K36: RDD Estimates for Infraction Amount (log) by Term

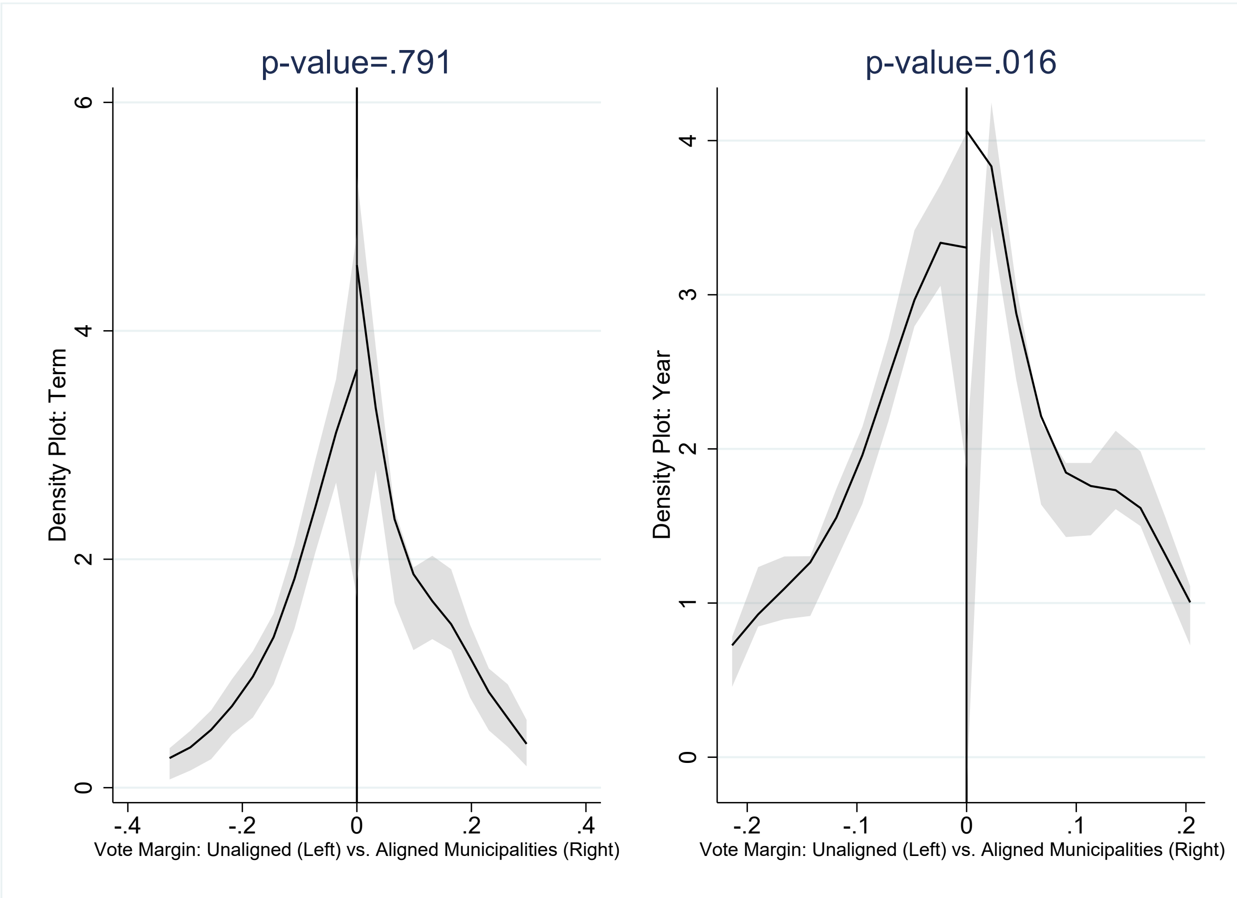
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.00483	-0.0220	-0.0127	0.0211	-0.0485	-0.162
	(0.230)	(0.335)	(0.241)	(0.325)	(0.255)	(0.381)
Observations	567	567	522	522	373	373
Effective observations	[193,174]	[202,197]	[167,152]	[193,188]	[108,102]	[119,119]
Covariates	None	None	Some	Some	All	All
p-value	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	-0.0456	0.0177	0.0136	-0.0244	-0.136
	(0.231)	(0.343)	(0.238)	(0.332)	(0.257)	(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

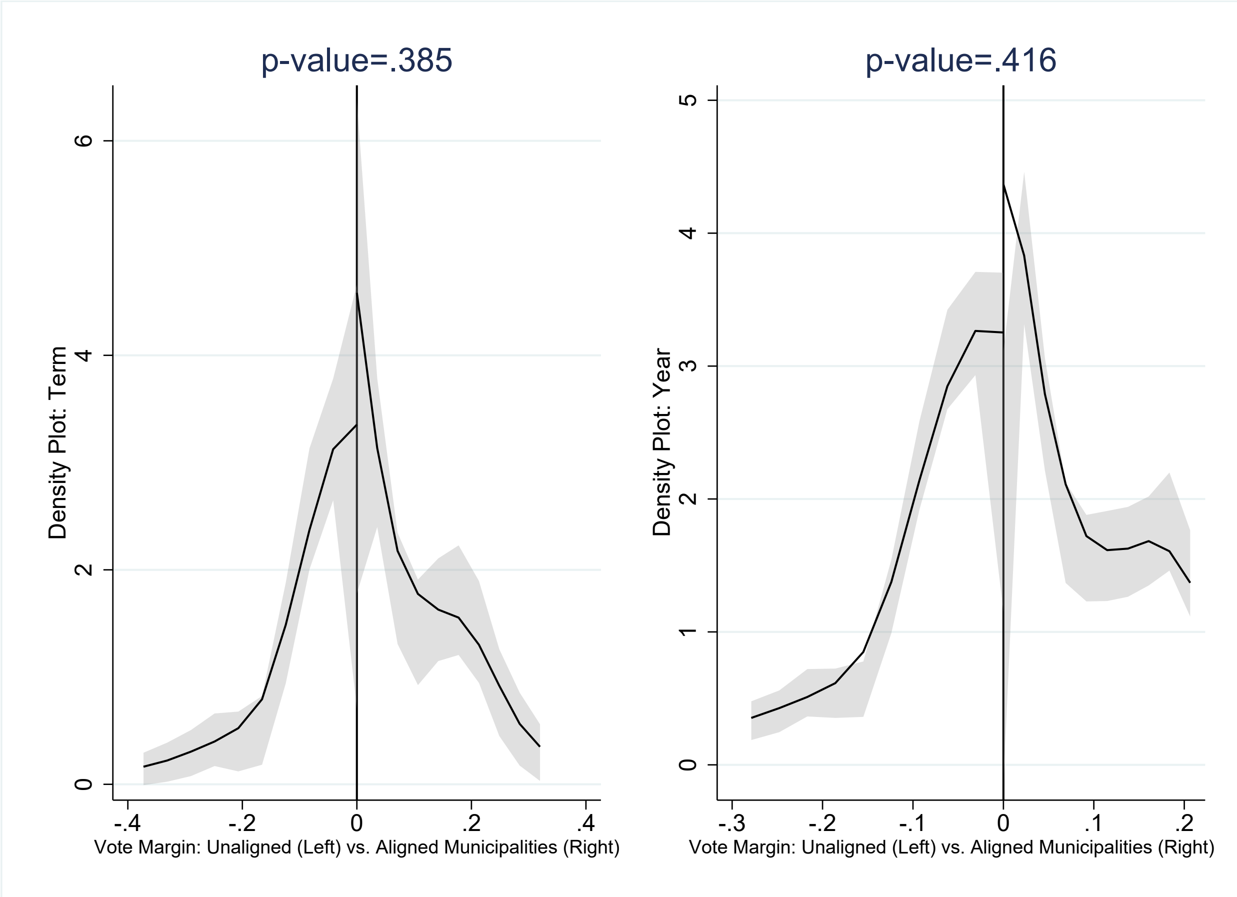
L.1. Density Plots for Poverty High/Low Sample

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



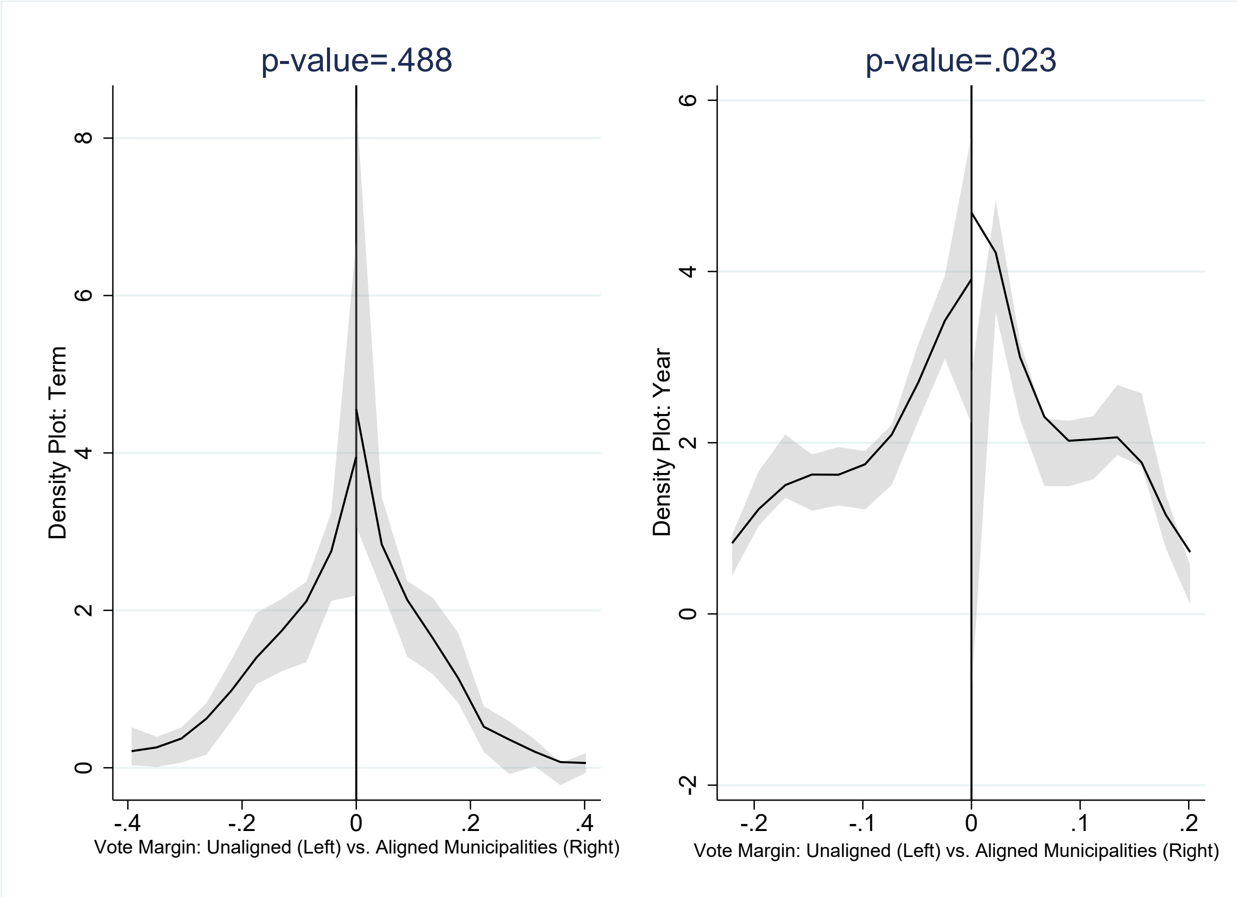
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. The electoral 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.

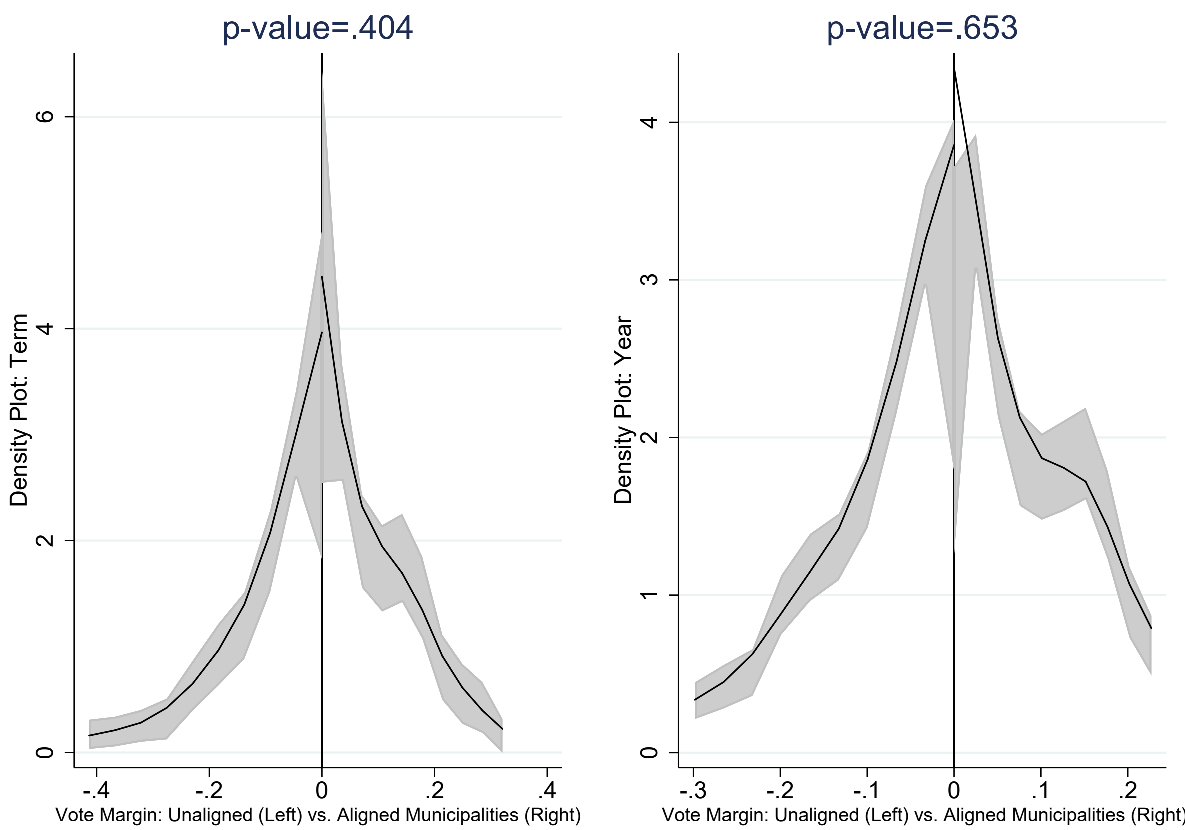
Figure L.3: RDD Density Plots for Infraction Count and Amount (High-Poverty Sample)



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.

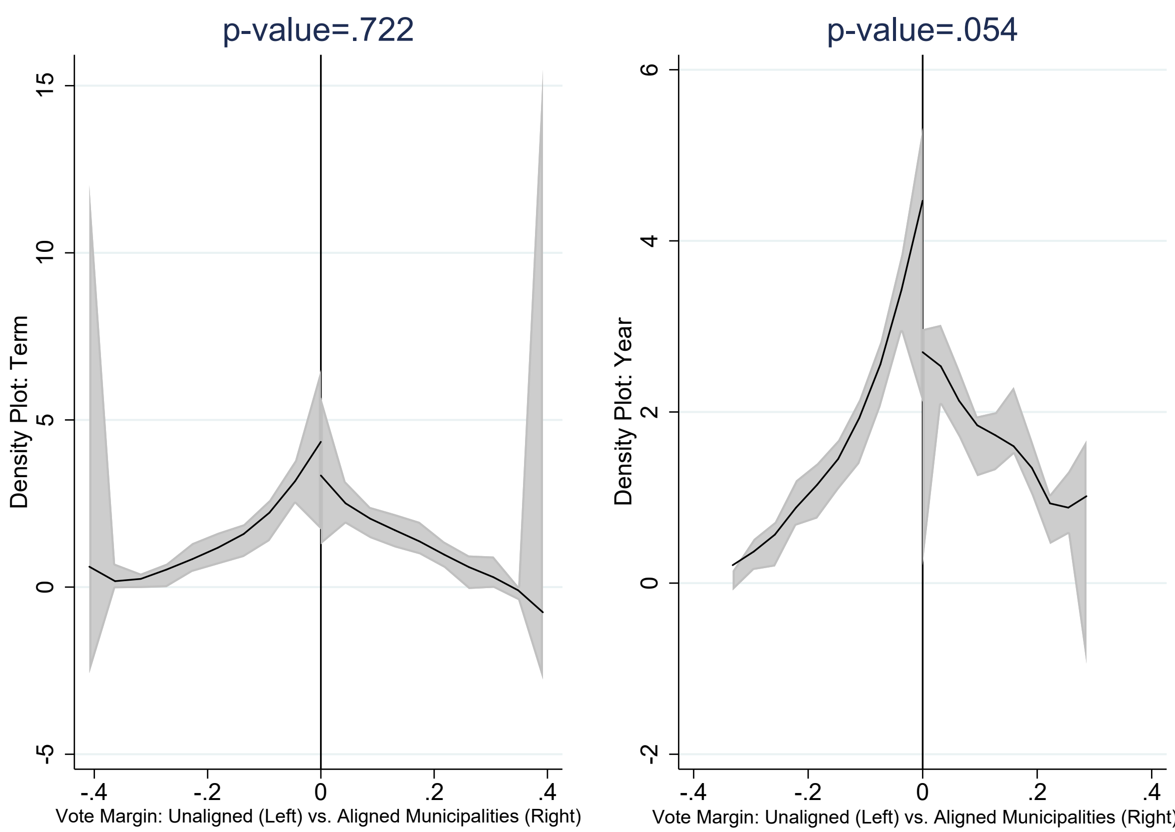
L.2. Density Plots for Poverty Increasing/Decreasing Sample: 2010-2015 (Main Results)

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



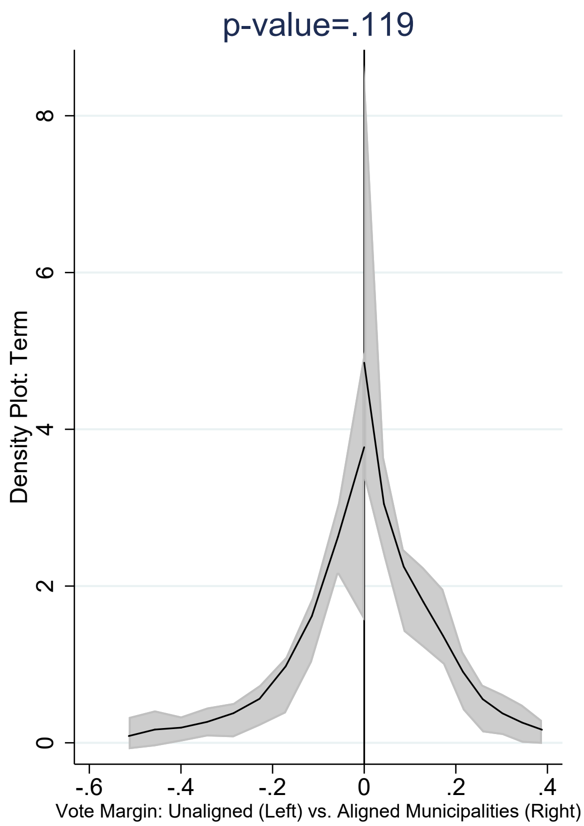
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither 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.

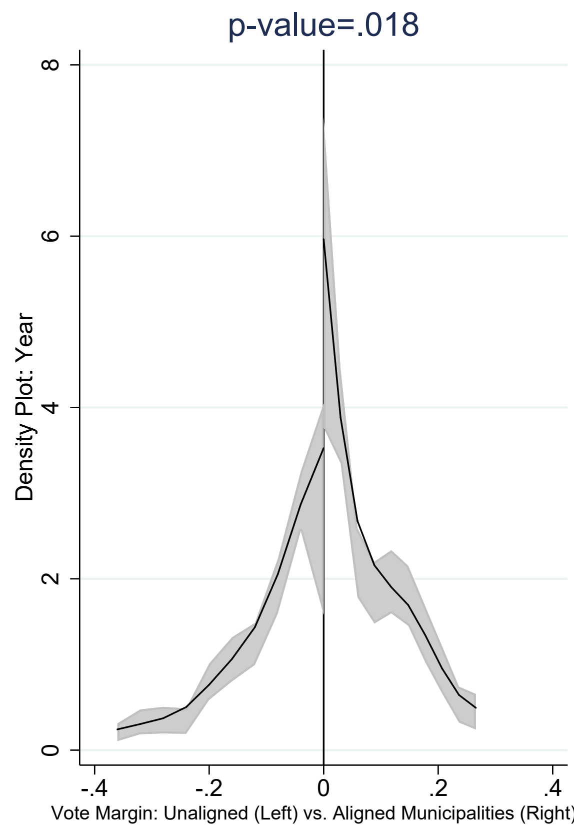
Figure L.5: RDD Density Plots for Infraction Count and Amount (Poverty-Decreasing Sample)



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)

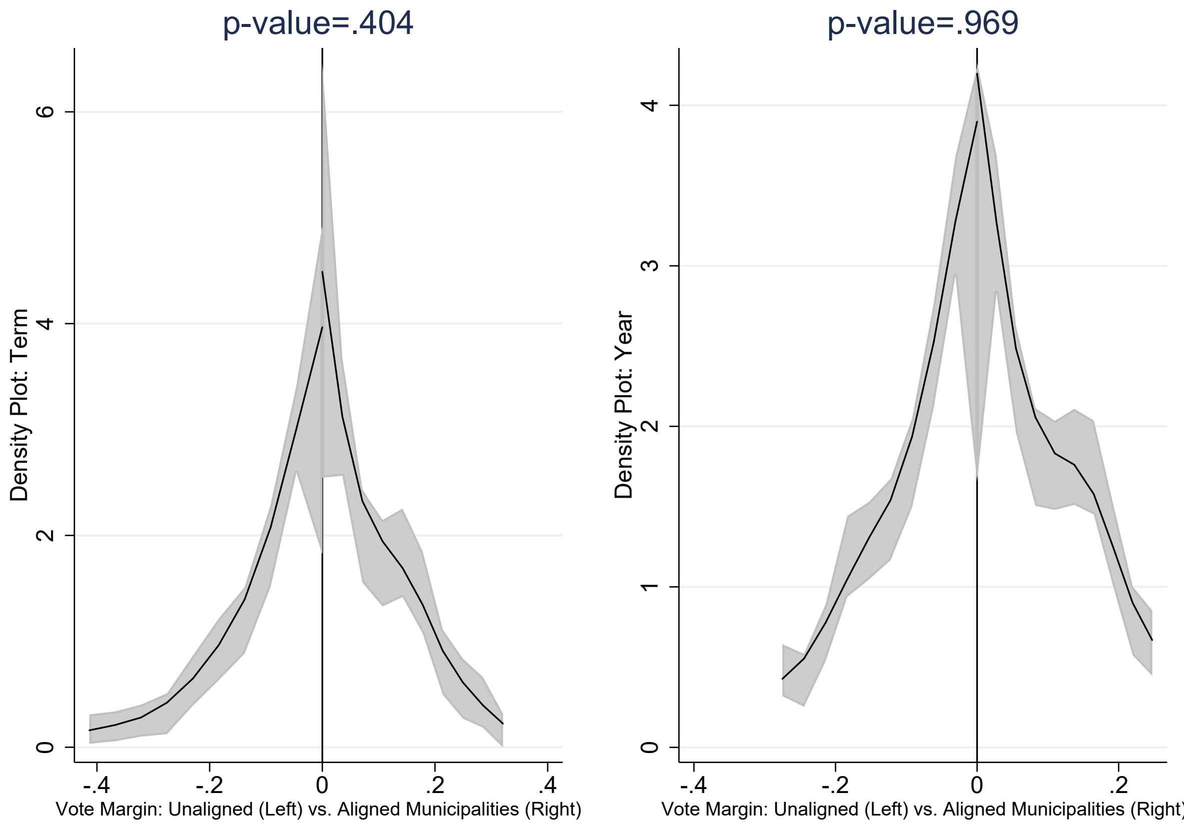




Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. The electoral term 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.

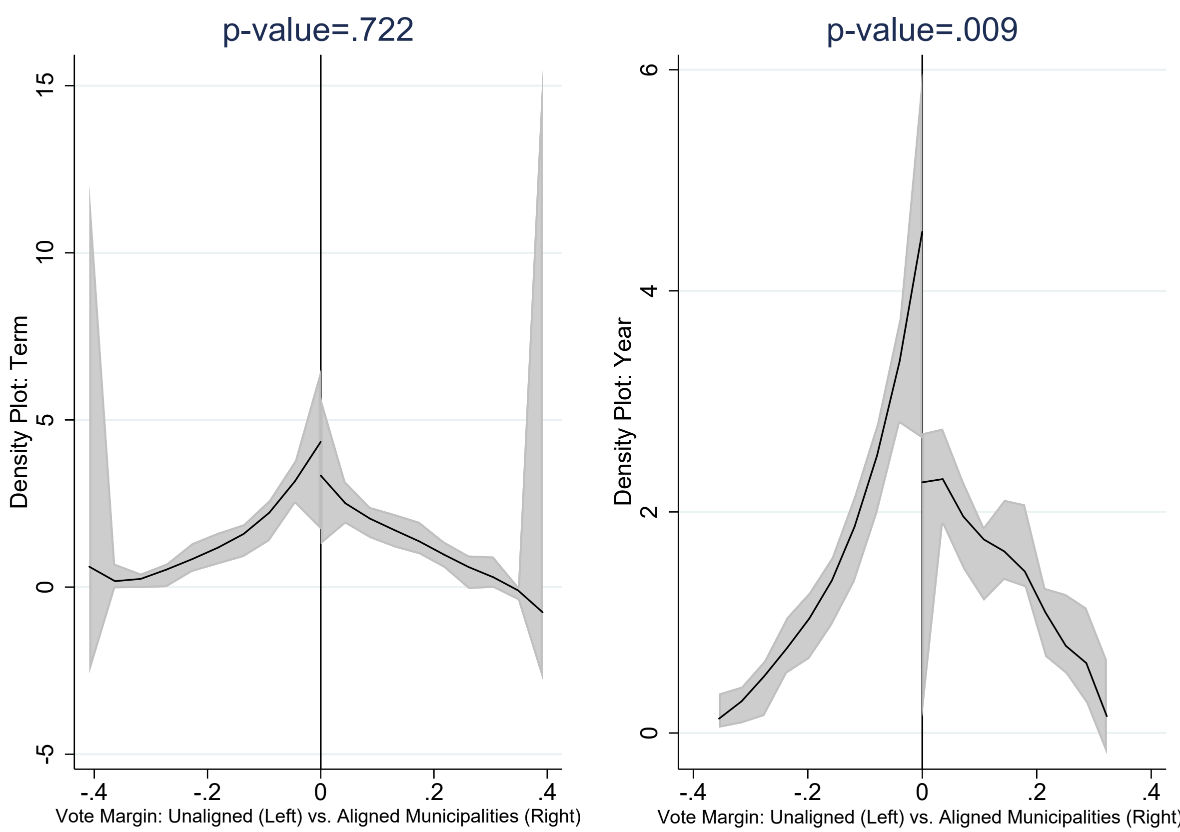
L.3. Density Plots for Poverty Increasing/Decreasing Sample: 2011-2015

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



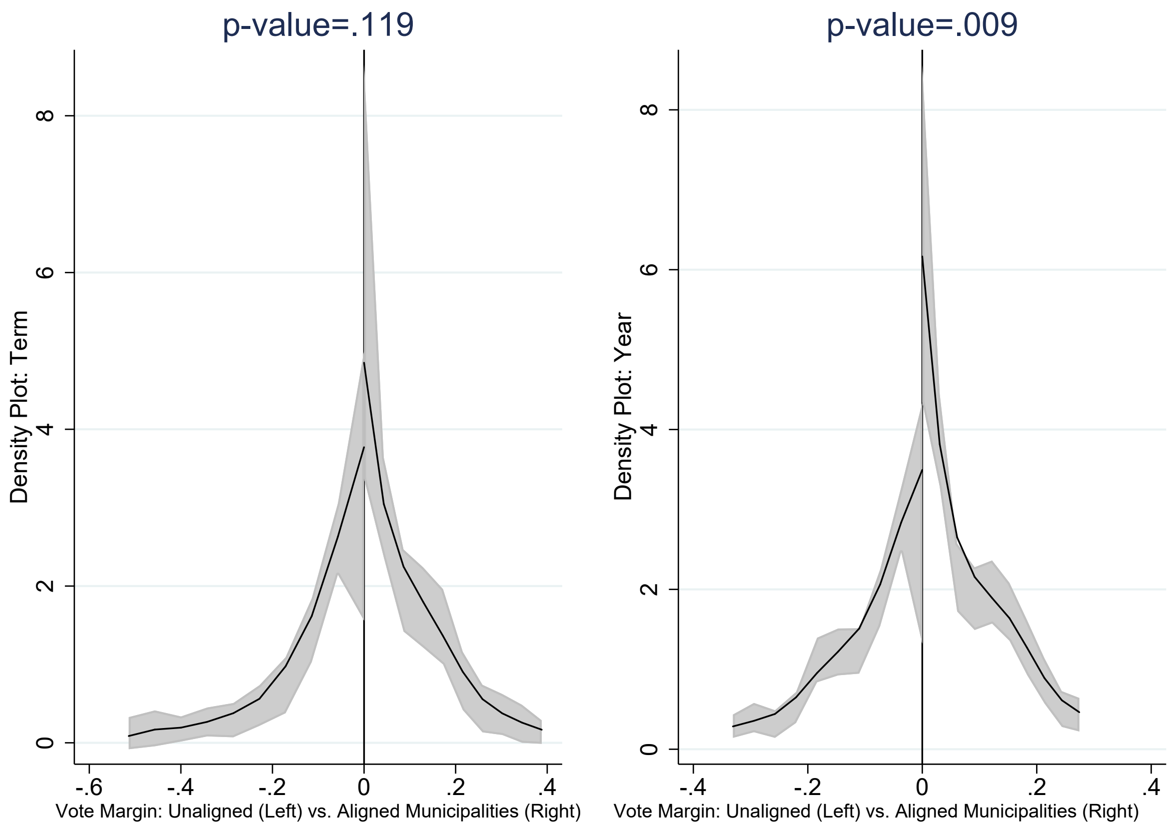
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither 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.

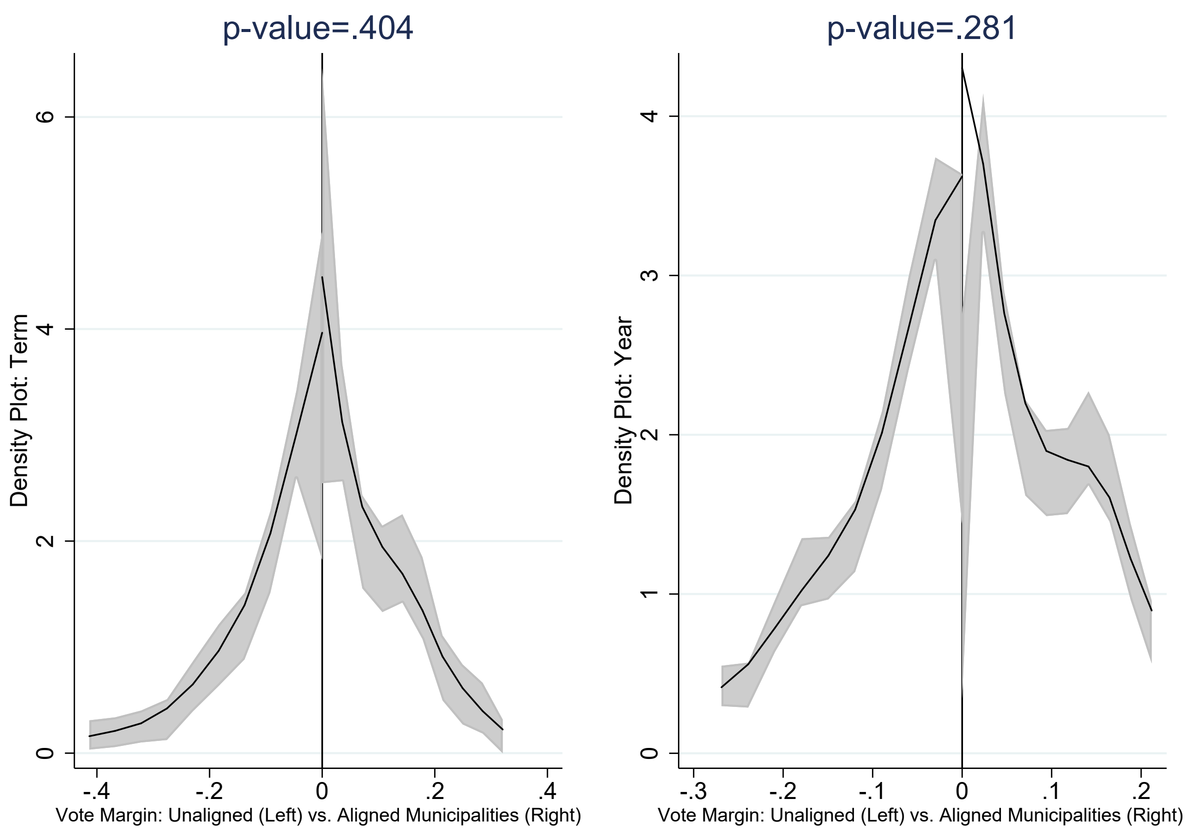
Figure L.9: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



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 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—i.e., with the cutoff being the margin of victory is zero.

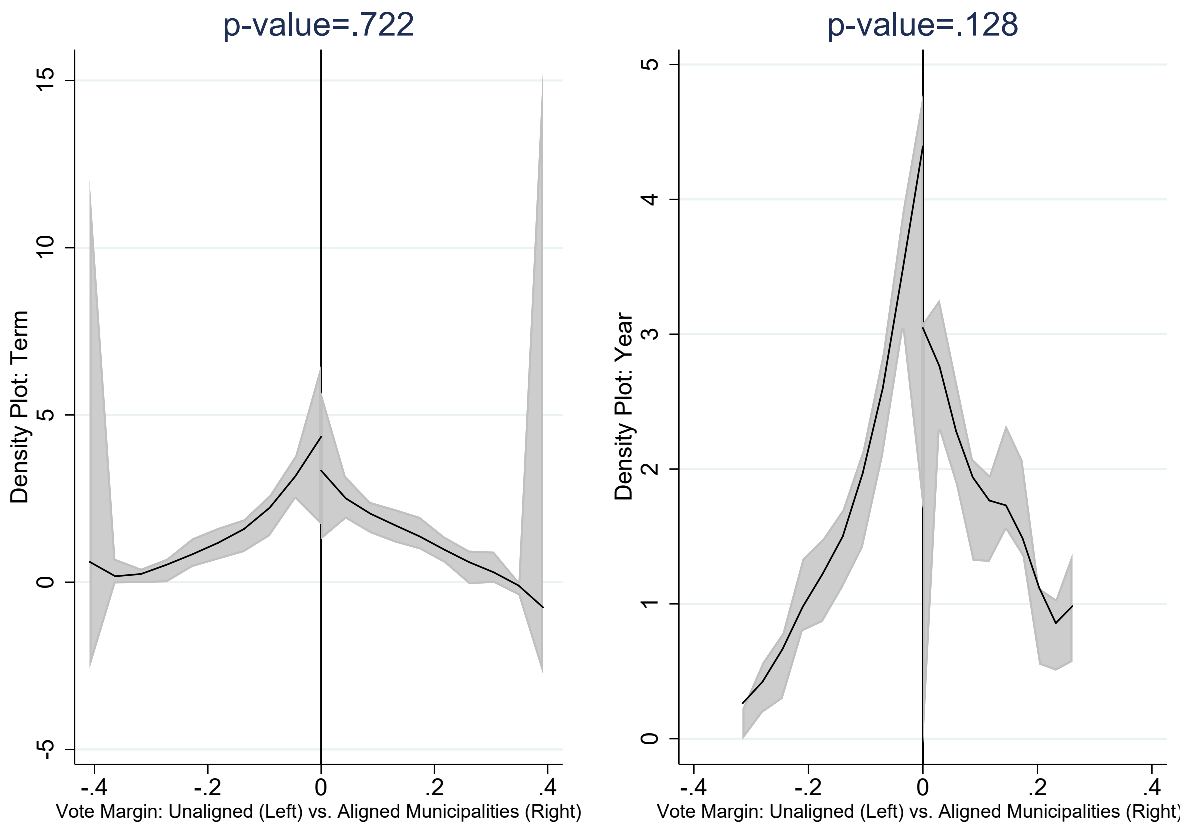
L.4. Density Plots for Poverty Increasing/Decreasing Sample: 2009-2015

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



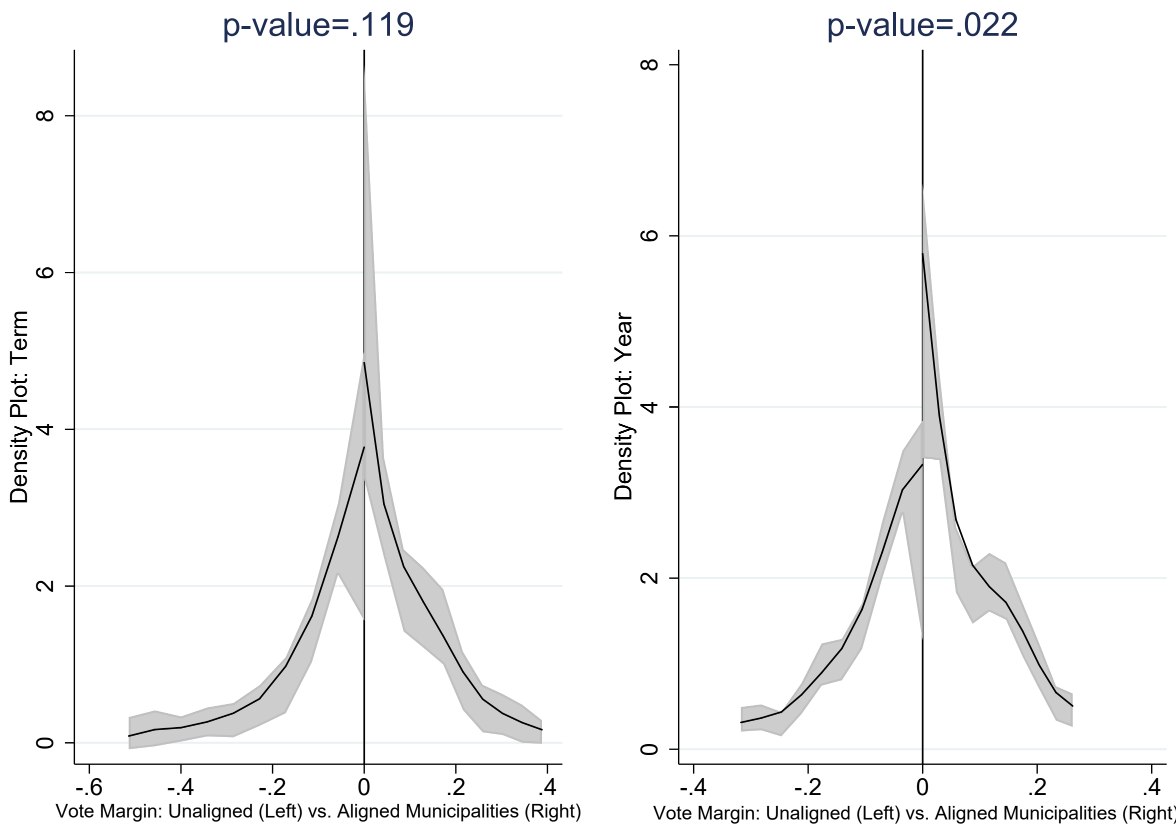
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither 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.

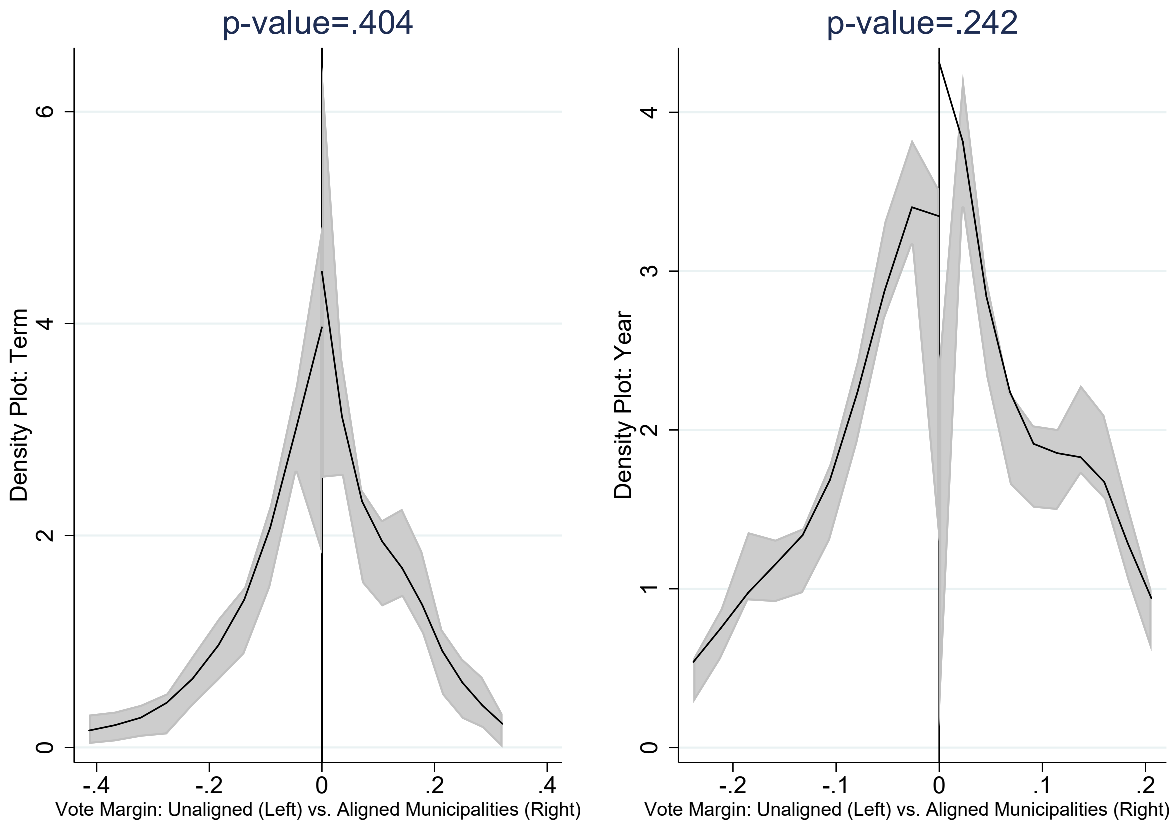
Figure L.12: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



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.

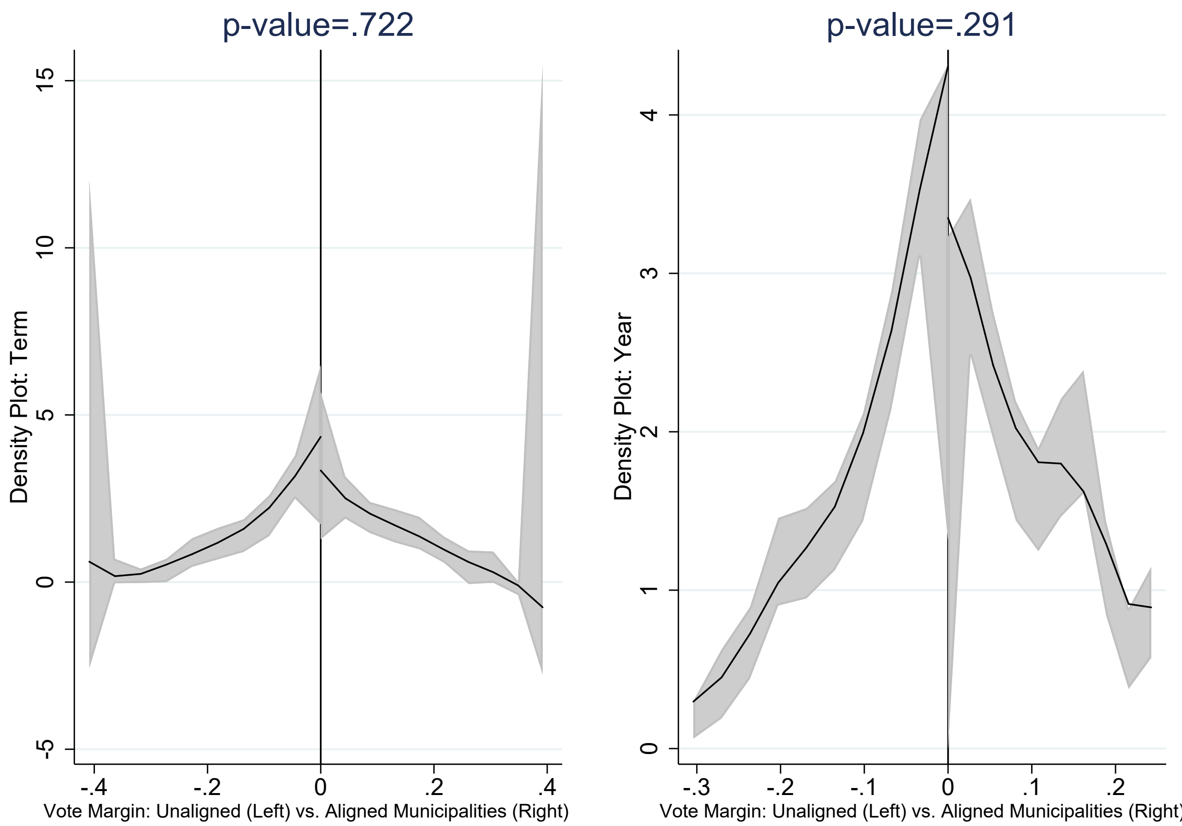
L.5. Density Plots for Poverty Increasing/Decreasing Sample: 2008-2015

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



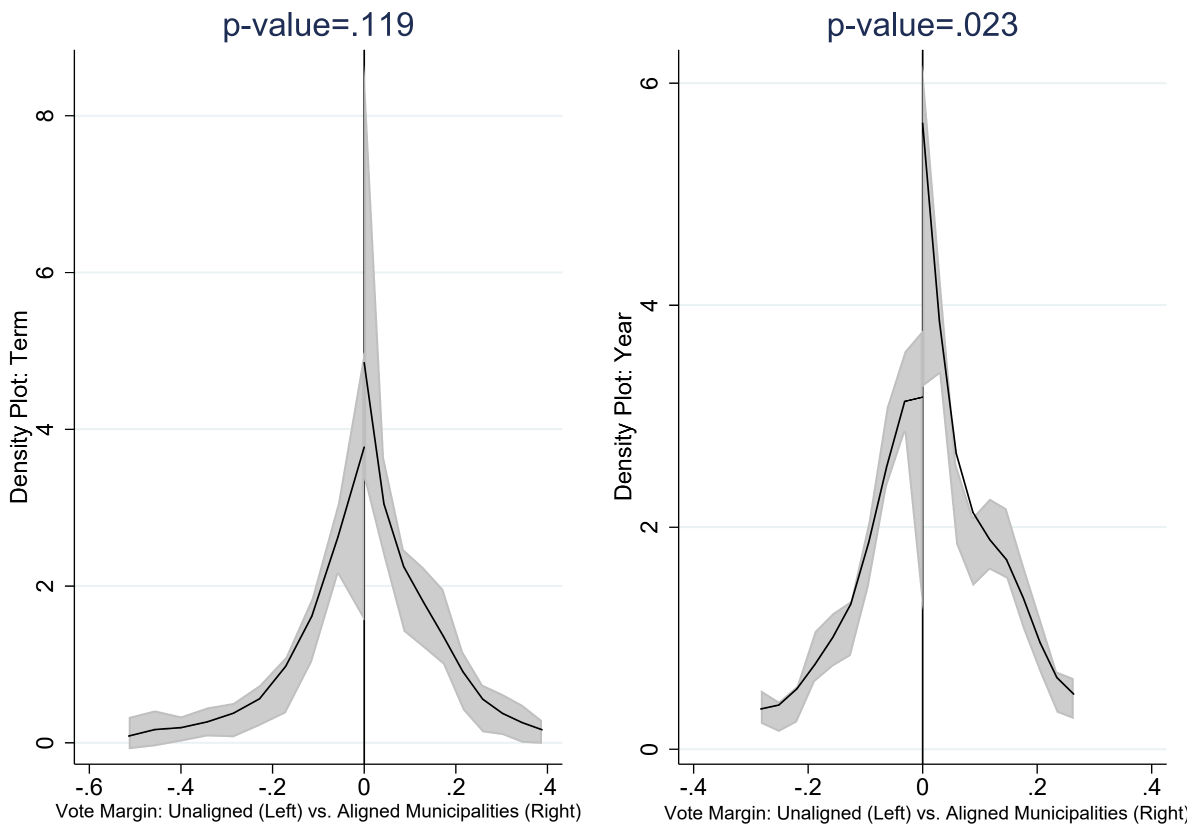
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither 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.

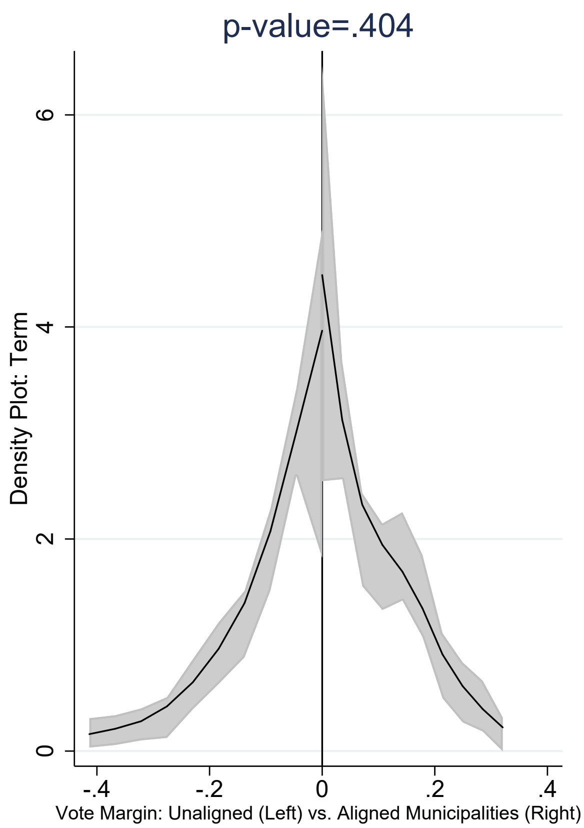
Figure L.15: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)

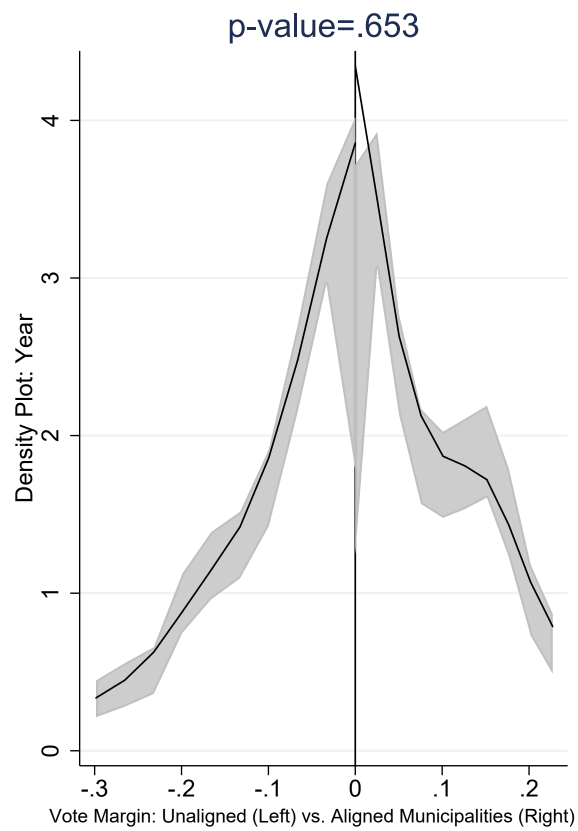


Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. The electoral 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

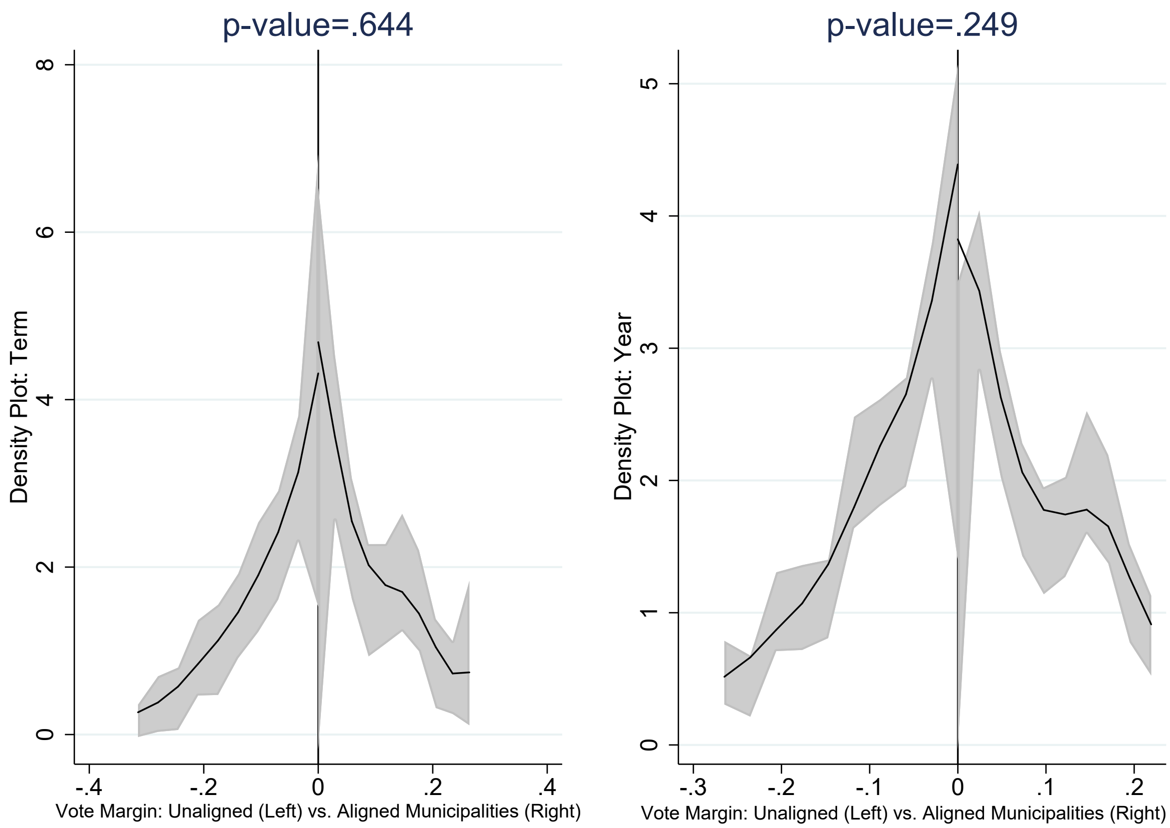
Figure L.16: RDD Density Plots for Infraction Count and Amount (Whole Sample)





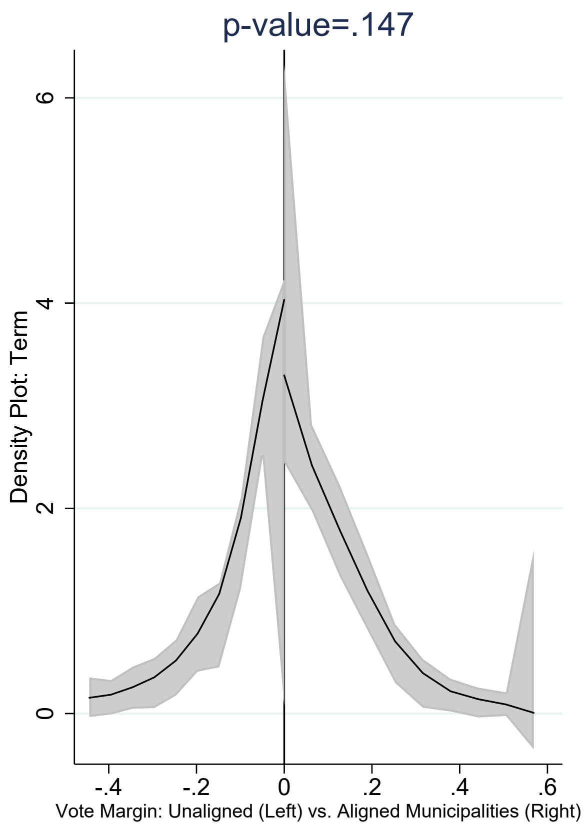
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither 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.

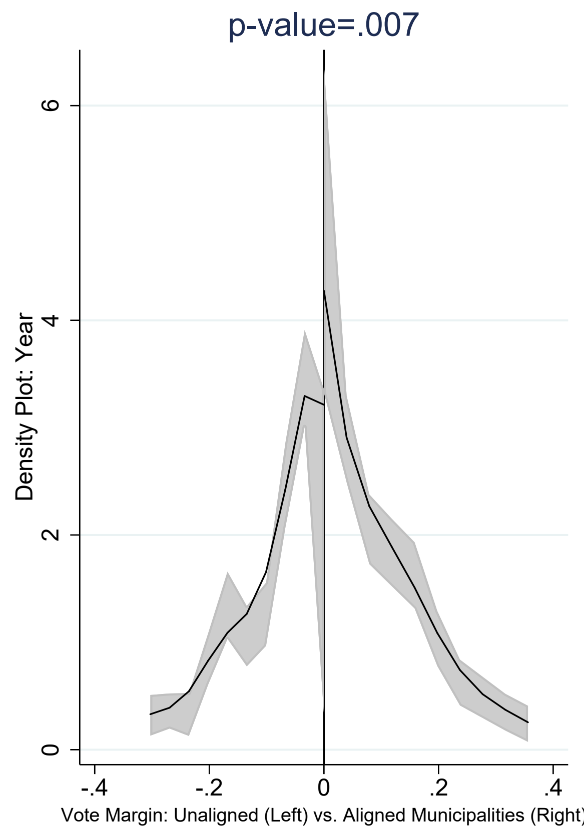
Figure L.17: RDD Density Plots for Infraction Count and Amount (Extreme Poverty-Decreasing Sample)



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***	-14.04***	-8.391**	-10.76**	-9.023***	-9.964**
	(3.076)	(4.137)	(3.264)	(4.199)	(3.359)	(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***	-2.009***	-0.855	-1.202*	-1.224**	-2.020***
	(0.543)	(0.699)	(0.539)	(0.703)	(0.587)	(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***	-1.192**	-1.081***	-1.024**	-1.017***	-1.031**
	(0.418)	(0.542)	(0.385)	(0.508)	(0.349)	(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***	-0.451*	-0.364	-0.342	-0.508**	-0.430
	(0.200)	(0.259)	(0.225)	(0.254)	(0.230)	(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*	-5.548	-4.403*	-3.244	-6.989***	-8.569***
	(2.831)	(3.556)	(2.568)	(3.281)	(2.490)	(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***	-2.135***	-1.448***	-1.530**	-1.064**	-1.137*
	(0.603)	(0.691)	(0.525)	(0.599)	(0.492)	(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*	-0.664*	-0.525**	-0.556*	-0.306	-0.442
	(0.296)	(0.349)	(0.261)	(0.317)	(0.287)	(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**	-0.732**	-0.499**	-0.603**	-0.143	-0.118
	(0.241)	(0.287)	(0.229)	(0.281)	(0.223)	(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	-9.964	-2.710	-13.20***	0.322	4.946
	(5.234)	(6.869)	(5.727)	(4.559)	(7.178)	(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	-1.037*	-0.791	-0.688	-2.054	0.837
	(0.517)	(0.614)	(0.604)	(0.657)	(2.228)	(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	8.435	2.210	-1.888	1.996	11.49***
	(4.211)	(5.933)	(5.145)	(5.337)	(7.231)	(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**	-0.876	0.336	-0.778	0.0217	0.521
	(0.376)	(0.561)	(0.466)	(0.789)	(0.577)	(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	0	0.0251	-0	0	-0.00990
	(6.42e-09)	(9.83e-09)	(0.0188)	(5.69e-09)	(1.73e-09)	(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	0.0969	0.0685	0.0965	0.0711	0.0936
	(0.0700)	(0.0971)	(0.0699)	(0.0972)	(0.0699)	(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	0.0528	0.00824	0.00643	0.00239	0.00643*
	(0.0338)	(0.0439)	(0.00685)	(0.00406)	(0.00193)	(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	0.159	-0.0155	-0.0251	0	0
	(0.111)	(0.150)	(0.0114)	(0.0169)	(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	0.00677	0.0458	-0.0120	-0.0180	-0.0365
	(0.0869)	(0.118)	(0.0863)	(0.119)	(0.0502)	(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	0.110	0.0474	0.0596	-0.00646	0.000883
	(0.0651)	(0.0722)	(0.0458)	(0.0541)	(0.0233)	(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.

L.10. RDD Estimates for Municipalities with no Missing Audits in a Term

L.10.1. For Poverty Low/High Sample

Table L13: RDD Estimates for Infraction Count and Amount (log) by Term: Low Poverty Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-6.805**	-7.250**	-4.977*	-4.062	-8.685***	-10.29***
	(2.880)	(3.362)	(2.580)	(3.239)	(2.579)	(2.884)
Observations	279	279	263	263	190	190
Effective Observations	[87,75]	[105,108]	[87,75]	[93,87]	[45,42]	[67,62]
Covariates	None	None	Some	Some	All	All
p-value	0.0181	0.0311	0.0537	0.210	0.000759	0.000358
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.111	0.185	0.120	0.145	0.0747	0.136
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.601**	-0.691*	-0.546**	-0.568*	-0.354	-0.505
	(0.302)	(0.358)	(0.263)	(0.319)	(0.287)	(0.406)
Observations	279	279	263	263	190	190
Effective Observations	[82,73]	[99,88]	[81,71]	[93,84]	[46,44]	[62,54]
Covariates	None	None	Some	Some	All	All
p-value	0.0468	0.0532	0.0381	0.0753	0.219	0.214
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.139	0.109	0.140	0.0805	0.117

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 for 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. 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 L14: RDD Estimates for Infraction Count and Amount (log) by Term: High Poverty Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	4.771	7.751	7.377	11.49	8.957*	15.27*
	(4.698)	(7.372)	(5.423)	(8.219)	(5.290)	(8.153)
Observations	249	249	222	222	175	175
Effective Observations	[62,60]	[77,74]	[49,49]	[64,62]	[39,40]	[46,50]
Covariates	None	None	Some	Some	All	All
p-value	0.310	0.293	0.174	0.162	0.0904	0.0611
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0934	0.122	0.0833	0.113	0.0789	0.104
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.741*	1.001*	0.982**	1.269**	0.706**	0.951**
	(0.433)	(0.579)	(0.439)	(0.551)	(0.359)	(0.471)
Observations	249	249	222	222	175	175
Effective Observations	[62,60]	[77,75]	[49,50]	[66,64]	[44,50]	[51,57]
Covariates	None	None	Some	Some	All	All
p-value	0.0874	0.0838	0.0254	0.0212	0.0490	0.0438
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0926	0.124	0.0848	0.117	0.0983	0.130 height

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 for 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. 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 L15: RDD Estimates for Infraction Count and Amount (log) by Term: Whole Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.595	-2.275	-0.993	-0.611	-2.431	-3.004
	(2.257)	(2.666)	(2.389)	(3.060)	(2.397)	(3.190)
Observations	554	554	511	511	365	365
Effective Observations	[181,164]	[229,232]	[162,145]	[193,193]	[104,100]	[124,139]
Covariates	None	None	Some	Some	All	All
p-value	0.480	0.394	0.678	0.842	0.310	0.346
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.123	0.206	0.118	0.172	0.104	0.154
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.0257	-0.0578	-0.00773	0.00205	-0.0393	-0.181
	(0.217)	(0.340)	(0.245)	(0.324)	(0.246)	(0.378)
Observations	554	554	511	511	365	365
Effective Observations	[196,186]	[196,187]	[157,141]	[182,181]	[101,100]	[114,114]
Covariates	None	None	Some	Some	All	All
p-value	0.906	0.865	0.975	0.995	0.873	0.632
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.147	0.149	0.113	0.156	0.100	0.123

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 for 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. 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.10.2. For Poverty Decreasing/Increasing Sample

Table L16: RDD Estimates for Infraction Count and Amount (log) by Term: Poverty Decreasing Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-10.47***	-11.88***	-7.475**	-9.517**	-7.106**	-8.573**
	(2.901)	(3.380)	(3.000)	(3.976)	(3.181)	(4.046)
Observations	191	191	175	175	175	175
Effective Observations	[62,48]	[73,69]	[53,43]	[64,46]	[45,34]	[57,43]
Covariates	None	None	Some	Some	All	All
p-value	0.000305	0.000439	0.0127	0.0167	0.0255	0.0341
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.105	0.160	0.0992	0.132	0.0807	0.107
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.232***	-1.197**	-1.080***	-1.029**	-1.006***	-1.043**
	(0.417)	(0.546)	(0.388)	(0.512)	(0.359)	(0.465)
Observations	191	191	175	175	175	175
Effective Observations	[49,39]	[56,43]	[45,34]	[48,36]	[52,38]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.00315	0.0282	0.00541	0.0444	0.00505	0.0249
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0761	0.0937	0.0794	0.0883	0.0929	0.0922

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 for 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. 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 L17: RDD Estimates for Infraction Count and Amount (log) by Term: Poverty Increasing Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	3.711	5.885	1.914	4.417	-1.893	1.605
	(3.905)	(6.069)	(3.806)	(6.699)	(4.518)	(8.688)
Observations	193	193	172	172	172	172
Effective Observations	[52,60]	[56,76]	[45,64]	[45,69]	[43,55]	[43,55]
Covariates	None	None	Some	Some	All	All
p-value	0.342	0.332	0.615	0.510	0.675	0.853
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.113	0.137	0.122	0.131	0.104	0.106
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.479	0.136	0.109	-0.0331	-0.0270	0.218
	(0.388)	(0.633)	(0.453)	(0.611)	(0.414)	(0.484)
Observations	193	193	172	172	172	172
Effective Observations	[52,58]	[55,73]	[41,52]	[47,75]	[43,55]	[57,87]
Covariates	None	None	Some	Some	All	All
p-value	0.217	0.830	0.810	0.957	0.948	0.652
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.107	0.131	0.0939	0.147	0.106	0.189

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 for 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. 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 L18: RDD Estimates for Infraction Count and Amount (log) by Term: Whole Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.966	-4.793	-2.962	-4.506	-5.248	-3.998
	(2.773)	(3.964)	(2.862)	(4.091)	(3.197)	(4.392)
Observations	433	433	393	393	367	367
Effective Observations	[128,116]	[145,139]	[116,108]	[132,132]	[98,93]	[117,117]
Covariates	None	None	Some	Some	All	All
p-value	0.285	0.227	0.301	0.271	0.101	0.363
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.105	0.134	0.109	0.139	0.0930	0.128
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0990	-0.494	-0.0395	-0.396	-0.270	-0.315
	(0.245)	(0.411)	(0.245)	(0.421)	(0.287)	(0.394)
Observations	433	433	393	393	367	367
Effective Observations	[149,146]	[143,133]	[133,138]	[128,125]	[105,101]	[122,131]
Covariates	None	None	Some	Some	All	All
p-value	0.685	0.229	0.872	0.347	0.348	0.424
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.142	0.128	0.145	0.130	0.106	0.144

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 for 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. 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.11. RDD Estimates for Average Infractions per Audit in a Term

L.11.1. For Poverty Low/High Sample

Table L19: RDD Estimates for Infraction Count and Amount (log) by Term: Low Poverty Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.880***	-2.178***	-1.665***	-1.846***	-1.911***	-2.306***
	(0.625)	(0.711)	(0.552)	(0.630)	(0.647)	(0.726)
Observations	284	284	267	267	192	192
Effective Observations	[73,65]	[99,89]	[71,65]	[94,85]	[46,42]	[67,63]
Covariates	None	None	Some	Some	All	All
p-value	0.00264	0.00220	0.00255	0.00338	0.00315	0.00148
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0817	0.133	0.0899	0.139	0.0742	0.134
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.628**	-0.770**	-0.597**	-0.705**	-0.296	-0.439
	(0.303)	(0.349)	(0.288)	(0.340)	(0.290)	(0.415)
Observations	284	284	267	267	192	192
Effective Observations	[78,65]	[95,84]	[73,65]	[88,81]	[47,44]	[62,52]
Covariates	None	None	Some	Some	All	All
p-value	0.0385	0.0275	0.0386	0.0384	0.308	0.290
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0886	0.124	0.0911	0.125	0.0778	0.114

Note: Standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Panel A shows results for average infraction count per audit in a term, while Panel B shows results for average log infraction amount per audit. 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. 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 L20: RDD Estimates for Infraction Count and Amount (log) by Term: High Poverty Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	1.126	1.791	1.780	2.606	1.694	3.320
	(1.050)	(1.570)	(1.225)	(1.756)	(1.319)	(2.054)
Observations	257	257	229	229	181	181
Effective Observations	[60,59]	[78,74]	[50,49]	[63,64]	[39,41]	[46,52]
Covariates	None	None	Some	Some	All	All
p-value	0.283	0.254	0.146	0.138	0.199	0.106
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0886	0.120	0.0797	0.112	0.0799	0.104
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.712	1.032	0.931**	1.343**	0.559	1.167**
	(0.444)	(0.628)	(0.466)	(0.617)	(0.395)	(0.577)
Observations	257	257	229	229	181	181
Effective Observations	[64,63]	[79,77]	[51,52]	[63,64]	[47,53]	[47,53]
Covariates	None	None	Some	Some	All	All
p-value	0.109	0.100	0.0456	0.0297	0.157	0.0430
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0954	0.124	0.0874	0.112	0.109	0.109

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows results for average infraction count per audit in a term, while Panel B shows results for average log infraction amount per audit. 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. 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 L21: RDD Estimates for Infraction Count and Amount (log) by Term: Whole Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.414	-0.636	-0.322	-0.377	-0.700	-0.869
	(0.515)	(0.713)	(0.551)	(0.704)	(0.585)	(0.762)
Observations	567	567	522	522	373	373
Effective Observations	[170,150]	[200,182]	[146,135]	[184,181]	[106,102]	[129,146]
Covariates	None	None	Some	Some	All	All
p-value	0.422	0.373	0.559	0.593	0.231	0.254
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.109	0.142	0.103	0.153	0.105	0.160
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.00214	-0.117	-0.00496	-0.0504	-0.0478	-0.163
	(0.228)	(0.352)	(0.237)	(0.340)	(0.253)	(0.377)
Observations	567	567	522	522	373	373
Effective Observations	[193,172]	[198,179]	[167,152]	[183,176]	[106,102]	[119,118]
Covariates	None	None	Some	Some	All	All
p-value	0.992	0.740	0.983	0.882	0.850	0.665
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.132	0.141	0.122	0.147	0.106	0.128

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows results for average infraction count per audit in a term, while Panel B shows results for average log infraction amount per audit. 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. 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.11.2. For Poverty Decreasing/Increasing Sample

Table L22: RDD Estimates for Infraction Count and Amount (log) by Term: Poverty Decreasing Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.582***	-2.133***	-0.928	-1.155	-0.918	-1.581**
	(0.552)	(0.737)	(0.577)	(0.765)	(0.632)	(0.797)
Observations	195	195	179	179	179	179
Effective Observations	[64,51]	[61,49]	[56,43]	[57,44]	[50,38]	[47,35]
Covariates	None	None	Some	Some	All	All
p-value	0.00417	0.00379	0.108	0.131	0.146	0.0473
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.104	0.103	0.106	0.0898	0.0861
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.861**	-0.807*	-0.786**	-0.698	-0.812**	-0.760
	(0.367)	(0.470)	(0.374)	(0.465)	(0.372)	(0.467)
Observations	195	195	179	179	179	179
Effective Observations	[50,39]	[57,47]	[44,33]	[52,40]	[45,34]	[53,43]
Covariates	None	None	Some	Some	All	All
p-value	0.0190	0.0862	0.0356	0.134	0.0289	0.103
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0810	0.0975	0.0750	0.0952	0.0808	0.0987

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows results for average infraction count per audit in a term, while Panel B shows results for average log infraction amount per audit. 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. 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 L23: RDD Estimates for Infraction Count and Amount (log) by Term: Poverty Increasing Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.437	0.341	0.416	0.675	-0.150	0.595
	(0.740)	(1.151)	(0.776)	(1.302)	(0.930)	(1.367)
Observations	196	196	174	174	174	174
Effective Observations	[62,81]	[67,84]	[46,64]	[47,73]	[44,55]	[46,72]
Covariates	None	None	Some	Some	All	All
p-value	0.555	0.767	0.592	0.604	0.872	0.663
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.155	0.164	0.121	0.138	0.104	0.134
Panel A	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.458	-0.0541	0.541*	0.448	0.538*	0.458
	(0.336)	(0.631)	(0.319)	(0.440)	(0.312)	(0.423)
Observations	196	196	174	174	174	174
Effective Observations	[57,70]	[57,67]	[47,73]	[58,88]	[47,74]	[58,88]
Covariates	None	None	Some	Some	All	All
p-value	0.172	0.932	0.0900	0.308	0.0854	0.279
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.124	0.122	0.138	0.189	0.142	0.197

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows results for average infraction count per audit in a term, while Panel B shows results for average log infraction amount per audit. 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. 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 L24: RDD Estimates for Infraction Count and Amount (log) by Term: Whole Sample

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0877	-0.709	0.0681	-0.197	-0.521	-0.385
	(0.479)	(0.793)	(0.514)	(0.748)	(0.606)	(0.816)
Observations	441	441	399	399	373	373
Effective Observations	[158,165]	[150,143]	[134,133]	[139,150]	[103,101]	[123,129]
Covariates	None	None	Some	Some	All	All
p-value	0.855	0.372	0.895	0.792	0.390	0.637
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.160	0.137	0.140	0.157	0.101	0.141
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.112	-0.297	0.0404	-0.0874	-0.00504	-0.0338
	(0.262)	(0.373)	(0.254)	(0.366)	(0.254)	(0.358)
Observations	441	441	399	399	373	373
Effective Observations	[127,117]	[146,135]	[120,113]	[134,133]	[109,107]	[125,135]
Covariates	None	None	Some	Some	All	All
p-value	0.670	0.426	0.873	0.811	0.984	0.925
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.129	0.114	0.139	0.114	0.147

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows results for average infraction count per audit in a term, while Panel B shows results for average log infraction amount per audit. 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. 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	0.0100	0.0432
	(0.0546)	(0.0544)	(0.0470)
Population (log)		3.609	11.62
		(14.55)	(16.32)
Public Good Spending per capita (log)		0.216**	0.261***
		(0.0876)	(0.0966)
Constant	72.60***	34.39	-47.96
	(0.642)	(148.5)	(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. Columnn (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummmy 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	0.253	0.154
	(0.344)	(0.371)	(0.418)
Population (log)		3.198	9.758
		(14.65)	(16.57)
Public Good Spending per capita (log)		0.147	0.219
		(0.178)	(0.177)
Constant	69.33***	36.17	-30.27
	(3.961)	(149.2)	(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. Columnn (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummmy 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	0.0509	0.0677
	(0.0807)	(0.0808)	(0.0759)
Population (log)		4.819	0.272
		(13.14)	(14.16)
Public Good Spending per capita (log)		-0.138**	-0.0990
		(0.0669)	(0.0701)
Constant	66.52***	18.16	62.78
	(0.923)	(133.9)	(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. Columnn (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummmy 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	0.238	0.117
	(0.179)	(0.182)	(0.179)
Population (log)		4.373	-0.381
		(13.16)	(14.27)
Public Good Spending per capita (log)		-0.267**	-0.189
		(0.113)	(0.156)
Constant	64.47***	21.19	68.93
	(1.933)	(134.0)	(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. Columnn (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummmy 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	0.0377	0.0531
	(0.0623)	(0.0408)	(0.0387)
Population (log)		0.177	4.154
		(11.14)	(12.28)
Public Good Spending per capita (log)		-3.733	-5.189*
		(2.739)	(2.886)
Constant	64.33***	98.84	69.19
	(0.753)	(121.3)	(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. Columnn (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummmy 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	0.524	0.386
	(0.317)	(0.442)	(0.518)
Population (log)		-0.645	2.496
		(11.18)	(12.50)
Public Good Spending per capita (log)		-3.857	-5.084*
		(2.721)	(2.981)
Constant	68.95***	102.1	81.01
	(3.939)	(121.3)	(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. Columnn (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummmy 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	0.143	0.163
	(0.109)	(0.138)	(0.145)
log_pop		7.300	0.541
		(18.38)	(20.78)
Public Good Spending per capita (log)		-0.304	-0.291
		(0.216)	(0.277)
Constant	65.31***	-7.204	59.25
	(0.647)	(186.7)	(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. Columnn (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummmy 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	0.418**	0.325
	(0.116)	(0.208)	(0.221)
log_pop		7.138	0.0528
		(18.36)	(20.89)
Public Good Spending per capita (log)		-0.385	-0.376
		(0.238)	(0.325)
Constant	64.84***	-9.130	61.68
	(1.331)	(186.7)	(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. Columnn (3) also adds additional electoral covariates, including dummy for mayor being reelected, number of valid votes cast in last election, dummmy 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	0.00276	0.0116
	(0.0296)	(0.0296)	(0.0288)
Constant	-0.0447	-0.0491	-0.218
	(0.525)	(0.525)	(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, dummmy for mayor being aligned with national party and dummy for mayor's gender.

Table M10: Term-wise Regression of Residuals on Log Amounts of Stolen/Misappropriated Money

	(1)	(2)	(3)
Infraction Amount (log)	0.200	0.163	0.0993
	(0.264)	(0.263)	(0.287)
Constant	-2.411	-1.973	-1.205
	(3.192)	(3.170)	(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, dummmy 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	0.0434	0.0564
	(0.0689)	(0.0688)	(0.0659)
Constant	-0.252	-0.271	-0.358
	(0.418)	(0.418)	(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, dummmy 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	0.206	0.0988
	(0.165)	(0.164)	(0.160)
Constant	-2.119	-2.223	-1.075
	(1.775)	(1.766)	(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, dummmy 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	0.0308	0.0431
	(0.0441)	(0.0370)	(0.0340)
Constant	-0.619	-0.718	-1.027
	(0.808)	(0.862)	(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, dummmy for mayor being aligned with national party and dummy for mayor's gender.

Table M14: Term-wise Regression of Residuals on Log Amounts of Stolen/Misappropriated Money

	(1)	(2)	(3)
Infraction Amount (log)	-0.284	0.516	0.374
	(0.290)	(0.426)	(0.484)
Constant	3.706	-6.800	-4.932
	(3.776)	(5.612)	(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, dummmy 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	0.141	0.159
	(0.104)	(0.136)	(0.146)
Constant	-0.476	-0.790	-0.907
	(0.611)	(0.748)	(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, dummmy 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	0.396**	0.299
	(0.189)	(0.197)	(0.206)
Constant	-3.485	-4.485**	-3.402
	(2.153)	(2.227)	(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, dummmy 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**	-1.458*	-0.536	-0.544	-0.788	-1.209
	(0.614)	(0.777)	(0.600)	(0.794)	(0.609)	(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*	-1.192	-0.337	-0.345	-0.597	-1.088
	(0.664)	(0.844)	(0.645)	(0.833)	(0.682)	(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***	-15.62***	-9.410**	-11.76**	-5.226*	-6.466*
	(3.767)	(5.100)	(4.142)	(5.457)	(3.146)	(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	-3.570	-0.873	-1.569	-1.190	-3.025
	(2.053)	(2.724)	(2.141)	(2.905)	(2.257)	(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**	-0.476	-0.465*	-0.387	-0.535*	-0.454
	(0.248)	(0.329)	(0.269)	(0.333)	(0.275)	(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***	-0.526	-0.485*	-0.472	-0.543**	-0.530
	(0.243)	(0.325)	(0.270)	(0.325)	(0.276)	(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***	-1.306*	-1.215***	-1.200*	-0.861**	-0.975*
	(0.502)	(0.678)	(0.426)	(0.619)	(0.395)	(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**	-0.558	-0.597	-0.481	-0.687*	-0.619
	(0.370)	(0.544)	(0.389)	(0.566)	(0.383)	(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	1.155	0.450	1.251	0.497	1.174
	(1.062)	(1.927)	(1.389)	(2.235)	(1.381)	(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	0.899	0.466	1.182	0.536	0.975
	(1.084)	(1.908)	(1.395)	(2.224)	(1.387)	(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	8.191	0.791	2.865	-3.459	-1.699
	(4.621)	(6.961)	(5.001)	(7.448)	(4.541)	(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	1.534	1.182	2.385	0.327	1.961
	(3.039)	(4.616)	(3.710)	(5.300)	(3.846)	(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	0.0581	0.177	-0.472	0.193	-0.476
	(0.347)	(0.459)	(0.367)	(0.670)	(0.366)	(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	-0.0624	0.189	-0.477	0.215	-0.480
	(0.348)	(0.489)	(0.366)	(0.671)	(0.365)	(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	0.236	0.0207	-0.183	-0.0501	-0.00795
	(0.533)	(0.679)	(0.554)	(0.713)	(0.448)	(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	-0.603	0.585	-0.394	0.621*	-0.441
	(0.426)	(0.714)	(0.371)	(0.703)	(0.370)	(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***	-2.038***	-0.836	-1.049	-0.877	-1.495**
	(0.566)	(0.647)	(0.558)	(0.705)	(0.535)	(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**	-1.294*	-0.480	-0.613	-0.593	-1.082
	(0.614)	(0.673)	(0.605)	(0.774)	(0.577)	(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***	-11.25***	-5.849***	-7.433**	-6.417***	-8.011**
	(2.315)	(3.058)	(2.216)	(3.096)	(2.259)	(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**	-5.611**	-2.363	-2.769	-2.725	-5.190*
	(2.216)	(2.735)	(2.180)	(2.965)	(2.131)	(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***	-0.510	-0.321	-0.313	-0.409	-0.384
	(0.243)	(0.322)	(0.269)	(0.320)	(0.279)	(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**	-0.475	-0.221	-0.244	-0.276	-0.298
	(0.244)	(0.329)	(0.289)	(0.347)	(0.285)	(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*	-0.600	-0.468	-0.349	-0.575	-0.451
	(0.394)	(0.508)	(0.444)	(0.513)	(0.428)	(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*	-0.452	-0.222	-0.138	-0.364	-0.280
	(0.388)	(0.517)	(0.487)	(0.567)	(0.475)	(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	0.363	0.192	0.426	0.230	0.457
	(0.868)	(1.326)	(0.997)	(1.466)	(1.026)	(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	0.0928	0.228	0.368	0.238	0.407
	(0.939)	(1.300)	(1.025)	(1.445)	(1.047)	(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	4.093	0.953	1.501	-1.100	1.156
	(2.876)	(5.435)	(3.204)	(5.748)	(3.988)	(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	1.268	0.804	1.247	0.0901	1.012
	(3.432)	(4.723)	(3.726)	(5.148)	(3.937)	(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	-0.0951	0.0979	-0.367	0.119	-0.262
	(0.258)	(0.416)	(0.287)	(0.455)	(0.286)	(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	-0.378	-0.0480	-0.443	-0.0878	-0.379
	(0.274)	(0.449)	(0.325)	(0.484)	(0.357)	(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	0.180	0.388	-0.00515	0.387	-0.000242
	(0.302)	(0.517)	(0.311)	(0.520)	(0.310)	(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	-0.188	0.305	-0.0460	0.320	-0.0877
	(0.322)	(0.634)	(0.352)	(0.544)	(0.348)	(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***	-2.114***	-1.157**	-1.394**	-1.045*	-1.756**
	(0.548)	(0.616)	(0.523)	(0.701)	(0.538)	(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**	-1.578**	-0.802	-0.890	-0.826	-1.362*
	(0.559)	(0.689)	(0.546)	(0.736)	(0.560)	(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***	-9.698***	-4.888**	-6.034**	-5.559***	-7.524***
	(2.207)	(2.703)	(2.121)	(2.862)	(2.141)	(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**	-6.714**	-3.531	-3.921	-3.666	-6.608**
	(2.253)	(2.844)	(2.211)	(2.990)	(2.306)	(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**	-0.423	-0.332	-0.299	-0.362	-0.329
	(0.233)	(0.314)	(0.266)	(0.316)	(0.280)	(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**	-0.393	-0.233	-0.241	-0.267	-0.274
	(0.232)	(0.320)	(0.282)	(0.332)	(0.284)	(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*	-0.326	-0.394	-0.248	-0.445	-0.326
	(0.348)	(0.472)	(0.429)	(0.499)	(0.413)	(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*	-0.442	-0.354	-0.270	-0.409	-0.319
	(0.349)	(0.462)	(0.439)	(0.499)	(0.429)	(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	0.279	0.137	0.324	0.168	0.374
	(0.800)	(1.227)	(0.915)	(1.346)	(0.954)	(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	-0.0117	0.178	0.264	0.159	0.284
	(0.876)	(1.201)	(0.971)	(1.312)	(0.988)	(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	2.333	0.535	1.155	0.0550	-0.424
	(3.324)	(5.142)	(3.647)	(5.401)	(3.905)	(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	0.949	0.697	0.998	-0.148	-1.052
	(3.622)	(4.934)	(3.863)	(5.280)	(4.086)	(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	-0.136	0.112	-0.258	0.0997	0.0636
	(0.244)	(0.405)	(0.275)	(0.405)	(0.285)	(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	-0.427	-0.0590	-0.330	-0.0601	0.0592
	(0.274)	(0.453)	(0.348)	(0.408)	(0.342)	(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	0.111	0.387	0.0247	0.368	-0.00815
	(0.310)	(0.509)	(0.326)	(0.509)	(0.327)	(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	-0.0330	0.338	0.0141	0.284	-0.0905
	(0.316)	(0.566)	(0.349)	(0.517)	(0.357)	(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	19	45
	(57.78%)	(42.22%)	(100.00%)
Not-Aligned	32	65	97
	(32.99%)	(67.01%)	(100.00%)
Panel B	**Mayor Not Corrupt**	**Mayor Corrupt**	**Total**
Aligned	25	38	63
	(39.68%)	(60.32%)	(100.00%)
Not-Aligned	46	46	92
	(50.00%)	(50.00%)	(100.00%)
Panel C	**Mayor Not Corrupt**	**Mayor Corrupt**	**Total**
Aligned	54	65	119
	(45.38%)	(54.62%)	(100.00%)
Not-Aligned	90	124	214
	(42.06%)	(57.94%)	(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	14	45
	(68.89%)	(31.11%)	(100.00%)
Not-Aligned	43	54	97
	(44.33%)	(55.67%)	(100.00%)
Panel B	**Mayor Not Corrupt**	**Mayor Corrupt**	**Total**
Aligned	29	34	63
	(46.03%)	(53.97%)	(100.00%)
Not-Aligned	48	44	92
	(52.17%)	(47.83%)	(100.00%)
Panel C	**Mayor Not Corrupt**	**Mayor Corrupt**	**Total**
Aligned	65	54	119
	(54.62%)	(45.38%)	(100.00%)
Not-Aligned	101	113	214
	(47.20%)	(52.80%)	(100.00%)

Note: "Mayor Not Corrupt" and "Mayor Corrupt" are defined as the count of municipalities with the log amount of stolen/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	21	46
	(54.35%)	(45.65%)	(100.00%)
Not-Aligned	44	52	96
	(45.83%)	(54.17%)	(100.00%)
Panel B	**Mayor Not Corrupt**	**Mayor Corrupt**	**Total**
Aligned	26	22	48
	(54.17%)	(45.83%)	(100.00%)
Not-Aligned	47	60	107
	(43.93%)	(56.07%)	(100.00%)
Panel C	**Mayor Not Corrupt**	**Mayor Corrupt**	**Total**
Aligned	54	50	104
	(51.92%)	(48.08%)	(100.00%)
Not-Aligned	107	121	228
	(46.93%)	(53.07%)	(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	18	46
	(60.87%)	(39.13%)	(100.00%)
Not-Aligned	46	50	96
	(47.92%)	(52.08%)	(100.00%)

	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	22	26	48
	(45.83%)	(54.17%)	(100.00%)
Not-Aligned	54	53	107
	(50.47%)	(49.53%)	(100.00%)

	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	52	52	104
	(50.00%)	(50.00%)	(100.00%)
Not-Aligned	112	116	228
	(49.12%)	(50.88%)	(100.00%)

Note: 'Mayor Not Corrupt" and "Mayor Corrupt" are defined as the count of municipalities with the log amount of stolen/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	69.51
	(21.46)	(16.87)
Whole Sample (including missing 2011)	63.87	65.84
	(21.46)	(20.21)
Municipalities Both in 2002 & 2011	67.34	69.51
	(18.91)	(16.87)
Municipalities Only in 2002	33.59	NA
	(18.55)	NA
Poverty-Reducing Sample	76.12	64.72
	(13.25)	(15.90)
Poverty-Increasing Sample	59.30	73.75
	(19.76)	(16.67)
Low-Poverty Sample	46.30	59.41
	(15.67)	(15.97)
High-Poverty Sample	81.34	77.61
	(7.82)	(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, 2007*a*), 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 & 2011 Waves

Sample	Mean Total Poverty-2002 (%)	Mean Total Poverty-2011 (%)
Whole Sample	19.79	20.84
	(14.27)	(15.47)
Whole Sample (including missing 2011)	19.79	19.28
	(14.27)	(15.51)
Municipalities Both in 2002 & 2011	21.42	20.84
	(14.01)	(15.47)
Municipalities Only in 2002	5.59	NA
	(6.61)	NA
Poverty-Reducing Sample	26.99	13.92
	(13.66)	(8.49)
Poverty-Increasing Sample	15.33	28.31
	(11.69)	(17.78)
Low-Poverty Sample	8.22	15.66
	(4.50)	(11.08)
High-Poverty Sample	31.29	25.09
	(10.96)	(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***	0.788***	0.468***	0.787***	0.573***	0.487***
	(0.022)	(0.022)	(0.043)	(0.021)	(0.036)	(0.049)
Low Poverty		0.003	0.012			
		(0.037)	(0.037)			
Population (log)					1.571***	-0.337
					(0.209)	(0.301)
Re-elected Mayor					0.008	0.002
					(0.034)	(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***	0.788***	0.469***	0.777***	0.568***	0.497***
	(0.022)	(0.022)	(0.043)	(0.020)	(0.032)	(0.050)
Low Poverty		0.014	0.024			
		(0.035)	(0.036)			
Population (log)					1.493***	-0.286
					(0.192)	(0.290)
Re-elected Mayor					0.019	0.001
					(0.031)	(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***	0.261***	0.352***	0.270***	-0.055	0.107
	(0.081)	(0.081)	(0.134)	(0.079)	(0.115)	(0.280)
Low Poverty		0.077	0.091			
		(0.074)	(0.074)			
Population (log)					2.472***	0.816
					(0.488)	(0.841)
Re-elected Mayor					0.020	0.010
					(0.094)	(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***	0.747***	0.442***	0.777***	0.568***	0.497***
	(0.022)	(0.022)	(0.046)	(0.020)	(0.032)	(0.050)
Poverty Reduced		-0.060*	-0.063*			
		(0.034)	(0.035)			
Population (log)					1.493***	-0.286
					(0.192)	(0.290)
Re-elected Mayor					0.019	0.001
					(0.031)	(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***	0.218**	0.254*	0.270***	-0.055	0.107
	(0.081)	(0.086)	(0.148)	(0.079)	(0.115)	(0.280)
Poverty Reduced		-0.051	-0.054			
		(0.077)	(0.077)			
Population (log)					2.472***	0.816
					(0.488)	(0.841)
Re-elected Mayor					0.020	0.010
					(0.094)	(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	-0.005	-0.036	0.016	0.047	-0.014
	(0.039)	(0.041)	(0.042)	(0.047)	(0.051)	(0.053)
Low Poverty		0.055	0.078**			
		(0.039)	(0.039)			
Log Population					0.213***	0.106*
					(0.072)	(0.062)
Reelected Mayor					0.032	0.038
					(0.047)	(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	-0.005	-0.036	0.016	0.047	-0.014
	(0.039)	(0.041)	(0.042)	(0.047)	(0.051)	(0.053)
Low Poverty		0.055	0.078^{**}			
		(0.039)	(0.039)			
Log Population					0.213^{***}	0.106^{*}
					(0.072)	(0.062)
Reelected Mayor					0.032	0.038
					(0.047)	(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	-0.164	-0.174	-0.159	-0.163	-0.099
	(0.118)	(0.122)	(0.121)	(0.120)	(0.124)	(0.137)
Low Poverty		0.190^{**}	0.206^{**}			
		(0.094)	(0.094)			
Log Population					0.445	1.989
					(0.606)	(1.638)
Reelected Mayor					0.173	0.281^{**}
					(0.114)	(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	-0.061	-0.073	0.030	0.040	0.014
	(0.045)	(0.048)	(0.047)	(0.056)	(0.065)	(0.065)
Poverty Reduction		-0.019	-0.019			
		(0.049)	(0.048)			
Log Population					2.719***	-1.017
					(0.479)	(0.998)
Reelected Mayor					0.065	0.065
					(0.066)	(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**	-0.267**	-0.279**	-0.049	-0.036	-0.038
	(0.125)	(0.132)	(0.133)	(0.132)	(0.162)	(0.155)
Poverty Reduction		-0.042	-0.043			
		(0.103)	(0.103)			
Log Population					5.704***	2.581
					(1.316)	(2.389)
Reelected Mayor					0.265	0.279*
					(0.174)	(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