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

Michael Denly† Akshat Gautam‡

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Abstract

We show that once reductions in poverty decrease voter need for clientelism, it ultimately reduces corruption through political selection. Our theoretical and empirical framework focuses on party alignment—i.e., when local-level politicians share the same party as the executive. Aligned politicians generally enjoy resource advantages due to their affiliation with the executive, but we show that close elections discipline aligned politicians to engage in less corruption after voters' economic circumstances improve. For identification, we rely on close-election regression discontinuity designs that analyze the number of audit violations committed and the amount of money misappropriated in Guatemalan municipalities. The results of our study help document how reductions in poverty decrease corruption through modernization, and how political selection and party system stability are central to the process.

JEL codes: D72, D73, F63

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†PhD Candidate, Department of Government, University of Texas at Austin, mdenly@utexas.edu. +1 202-766-3605.

‡Research Associate, akshat.gautam@kc.frb.org. +1 512 228-4223. The views expressed here are those of the author and do not necessarily represent those of the Federal Reserve Bank of Kansas City or the Federal Reserve System. Previously, Akshat was an undergraduate Research Affiliate at University of Texas at Austin's Innovations for Peace and Development.

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The practice of misusing entrusted power or public office for private gain has a familiar name: corruption.[1] Especially but not exclusively in developing countries, corruption is associated with lower development outcomes across numerous sectors of the economy,[2] and politics is often at the center of corrupt transactions. Although recent work on corruption relies more on credible audit and procurement data than problematic perception-based measures,[3] scholarship still focuses more on identifying corruption than *reducing* it.[4]

In this paper, we use objective, time-varying, subnational political and corruption data to show how economic development and institutions interact to reduce corruption through political selection. Our theoretical framework and identification strategy focuses on the institutional configuration of political party alignment: that is, when the president or prime minister's party in power has the same party in lower-level government entities.[5]

On the one hand, the decentralization, political budget cycles, and clientelism literatures are clear that party alignment is an institutional configuration that facilitates resource-related, bureaucratic advantages in both developed and developing countries.[6] On the other

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

[2] See, for example, Reinikka and Svensson (2004) and Ferraz, Finan and Moreira (2012) regarding education; Olken (2007) regarding infrastructure; and Fisman (2001) and Faccio (2006) regarding finance.

[3] For work using objective subnational data, see, for example, Ferraz and Finan (2008) on exposing corrupt politicians through the dissemination of audit results near elections; Gingerich (2013*b*) on ballot structure and party-directed corruption; Broms, Dahlström and Fazekas (2019) on public procurement outcomes and political competition; and Boas, Hidalgo and Melo (2019) on sanctioning corrupt politicians. The literature that criticizes perception-based measures of corruption is extensive, but some of the most prominent critiques include Kurtz and Schrank (2007*a*,*b*), Andersson and Heywood (2009), Olken (2009), Langbein and Knack (2010), Thomas (2010), Gingerich (2013*a*), Bersch and Botero (2014), and Gisselquist (2014).

[4] Notable exceptions include Ferraz and Finan (2008, 2011) and Bobonis, Cámara Fuertes and Schwabe (2016).

[5] de Remes (1999) calls party alignment "juxtaposed government", but we will use the term alignment given that it is more common in the literature.

[6] For a summary of how clientelism is fueled by "politicized public resources", see Greene (2007, 2010). Regarding decentralization, there is documented evidence of "budget-cycles" and favoritism in intergovernmental transfer allocation in at least the following countries: Brazil (Brollo and Nannicini, 2012); 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; Kriner and Reeves, 2012, 2015; Christenson, Kriner and Reeves, 2017;

hand, party alignment serves as an indicator of the larger phenomenon of clarity of responsibility for misgovernance. Its basic premise is that clarity of responsibility is high under alignment. By extension, corruption is more prevalent under divided government, because politicians take advantage of the fact that voters have trouble assigning blame under such institutional circumstances (Schwindt-Bayer and Tavits, 2016).

A primary objective of this paper is to reconcile the aforementioned contrasting predictions of alignment on levels of corruption. To that end, we put forth a simple model. We theorize that in democracies with lower levels of economic development that facilitate clientelistic citizen-politician linkages,[7] clarity of responsibility does not necessarily lower corruption or reduce the supply of corrupt politicians. In such contexts, voters only punish aligned politicians and the latter will only reduce their corruption levels if two conditions are met. First, the relevant politician must live in an area where economic circumstances have recently improved or poverty has declined. Second, the politician must have just barely won his/her position in a close election.

When poverty declines, voters tend to rely less on clientelistic exchanges to meet basic needs and, in turn, vote more on the basis of programmatic (policy-based) appeals.[8] By reducing the need for "request-fulfilling",[9] we argue that reducing poverty leads to less voter tolerance of corrupt politicians as well, yielding a different landscape for political selection.[10] By contrast, under comparatively more difficult economic circumstances, voters are more supportive of aligned politicians because of their access and willingness to share the spoils of the bureaucracy for electoral gain. In such environments, clientelistic linkages are typically more compelling for voters because informational environments can be weak, and politicians'

Hill and Jones, 2017); Uruguay (Manacorda, Miguel and Vigorito, 2011); and West Germany (Schneider, 2010).

[7] For a review of citizen-politician linkages, see Kitschelt (2000) and Kitschelt and Wilkinson (2007).

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

[9] Request-fulfilling entails "citizens demand[ing] clientelistic benefits" (Nichter and Peress, 2017).

[10] For an excellent review of the literature on political selection, see Dal Bó and Finan (2018).

policy promises are not credible.[11]

Winning in a close election amplifies the effects of poverty reduction on politicians' corruption levels. Such a situation signals to politicians that they have less room to capture rents if they wish to gain re-election in the electoral term—and obtain rents in the future. Given that politicians in most countries earn more in office than out of office,[12] reelection prospects drive politicians to temper their corruption levels if their close-election win gives them less ability to extract rents.

Under *both* improved economic circumstances and credible electoral competition, clarity of responsibility then becomes salient. More specifically, clarity of responsibility motivates the aligned politicians to reduce their rent-seeking behavior relative to unaligned politicians—even though aligned politicians enjoy greater resource advantages. Overall, our theory aims to depict how politics, political institutions, and economic development interact to reduce corruption through modernization.[13]

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

To obtain objective measures for corruption, we follow some pioneering recent work on Brazil, Mexico, Romania, and Bulgaria,[14] and rely on measures of municipal-level infractions and spending misapppropriations captured in audit reports. Our political data constitute

[11] See, for example, Keefer (2004, 2007*a*,*b*), Keefer and Khemani (2005), Keefer and Vlaicu (2008), and De La O and Rodden (2008).

[12] See, for example, Eggers and Hainmueller (2009) and Fisman, Schulz and Vig (2014).

[13] 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).

[14] See, for example, Ferraz and Finan (2008, 2011), Brollo et al. (2013), Zamboni and Litschig (2018), Klašnja (2015), Nikolova and Marinov (2017), Chong et al. (2015), and Larreguy, Marshall and Snyder (2019).

the electoral results of municipal elections. The poverty data come from the results of the 2002 and 2011 censuses of Guatemala.

To operationalize whether a municipality is performing better economically, we specifically compare municipalities that increased and decreased their poverty rates relative to the previous census. To causally identify the effects of alignment in both the increased and decreased poverty samples, we exploit a series of close-election regression discontinuity designs. To accommodate the concept of alignment, we modify Lee's (2008) framework for the incumbency advantage along the lines of Brollo and Nannicini (2012).

Under numerous specifications, we consistently find that alignment yielded a significant decrease in both of our measures of corruption in the municipalities with decreased poverty. For example, in our base specification for infractions, aligned municipalities commit an average of 11.5 less infractions in our electoral term analysis model. To ensure our results are robust, we use Calonico et al.'s (2019) new method to consider covariates in our regression discontinuity analyses. When controlling for reelection (Ferraz and Finan, 2011; Vuković, 2020), log public goods spending per capita, log population, and inequality (Gini coefficient), results are mostly unchanged for the poverty-reducing sample.

In some but not all cases, alignment reduces corruption in municipalities that reduced levels of extreme poverty relative to the previous census as well, suggesting that the theory has broad reach. None of these results travel to municipalities in which the poverty rate increased from 2002 to 2011. When analyzing the full sample (i.e. not splitting the sample according to poverty increases or decreases), the results under all specifications are also statistically insignificant, suggesting the limits of current understanding of clarity of responsibility theory (see Schwindt-Bayer and Tavits, 2016).

The one drawback of current results is that after the 2015 election, there are no aligned mayors in the sample. The drawback is a function of the fact that the Guatemalan people elected a populist outsider, Jimmy Morales, as president in 2015 (see Meilán, 2016). What we can draw from these results is a scope condition for our theory: it will be more difficult for the theory to be applicable in countries with very unstable party system (see also, Schleiter and Voznaya, 2018).

At the broadest possible level, the results of this study help scholars better understand the causes of democratization and the extent to which modernization processes play a role. Daron Acemoglu, James Robinson, and their co-authors, for example, suggest that there is no direct evidence for the most prominent manifestations of modernization theory: that both increasing income and education lead to democratization (e.g. Acemoglu et al., 2005, 2008, 2009; Acemoglu and Robinson, 2018; Acemoglu et al., 2019).[15] We, of course, do not dispute these very comprehensive studies.[16] Nevertheless, our empirical results based on close-election data suggest a *potential* consequence of income-based modernization: the reduction of corruption, which fuels democratization (Treisman, 2000, 2007; Lagunes, 2012).

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

1. Theoretical Framework

1.1. Model Setup

We provide a simple theoretical framework to understand the mechanisms through which poverty reduction or modernization decreases corruption through political selection. Our framework focuses on party alignment for a simple reason: aligned politicians have more

[15] For more on modernization theory, see, for example, Rostow (1960), Przeworski and Limongi (1997), Boix and Stokes (2003), and Acemoglu and Robinson (2018).

[16] Note that Chen, Chernozhukov and Fernández-Val (2019) showed the results of Acemoglu et al. (2019) are even stronger than the latter suggest in their article.

possibilities than politicians from other parties to capture the spoils of the bureaucracy for both clientelistic and corrupt purposes (Greene, 2010; Brollo and Nannicini, 2012; Corvalan, Cox and Osorio, 2018; Velasco Rivera, 2020).

To better understand the advantages of alignment and how they are crucial to reduce corruption through political selection and modernization, let us first consider local-level politician i's maximization problem. Consistent with Magaloni, Díaz-Cayeros and Estévez (2007),[17] 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:

$$b_i = g_i + r_i \quad [18] \tag{1}$$

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:

$$r = c + p, \qquad \text{where } c = \gamma r \quad [19] \tag{2}$$

Under Equation (2), we assume that c increases with r, meaning that local-level politician i devotes at least some portion of her rents toward clientelism. Although the politician may prefer to keep all of the rents for personal gain ($c = 0$), doing so would drastically hurt reelection prospects and thus future potential rent extraction levels as well. Given the possibility of reelection and how it drives politician behavior,[20] we distinguish between local-level politician i's favored levels of rent extraction in the current electoral period, $r_{i,1}$, as well as

[17] See also Díaz-Cayeros, Estévez and Magaloni (2016).

[18] 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.

[19] Because we cannot directly observe the distinction between c and p, we introduce $\gamma \in (0, 1)$, which denotes the fraction of rents used for clientelistic purposes. See Appendix A for the calculation of the maximization problem.

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

a potential future one, $r_{i,2}$:

$$r_i = r_{i,1} + r_{i,2}\text{[21]} \tag{3}$$

Since 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 corresponds to party alignment, which takes a value of 1 if local-level politician i is aligned or 0 otherwise; $t(\cdot)$ captures citizens' satisfaction from clarity of responsibility, measured by local-level politician i's margin of victory in the last election (MV), such that $t(\cdot)$ is a positive function,[22] $t' > 0$, and $t'' < 0$; and β_i represents that effect of reducing poverty on citizens' pre-existing discount rates of clientelistic and other benefits that corrupt politicians may bring through $W(\gamma r_{1,i})$.[23]

[21] 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.
[22] Mathematically, $t(0) = 0$.
[23] 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, $r_{i,1}$.

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

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

The first channel focuses on the direct effects of clarity of responsibility on citizens' overall level of satisfaction with local-level politician i in a given area, s_i. As we show in Appendix B, 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 this joint dependency and its ability to be positive or negative with the term $(2a-1)t(MV)$.[24] By definition, MV in combination with party affiliation determines a, but the latter serves an independent role as well. When local-level politician i shares the same party as the executive, citizens can easily discern which politician(s), party, or governing coalition is responsible for corruption or effective government. By contrast, citizens' abilities to make such snap judgments are not as robust under divided government (Schwindt-Bayer and Tavits, 2016; Appendix B). That is particularly the case in poor areas that suffer from political market imperfections, such as voters lacking information about politician performance,[25] identity voting,[26] and politicians' inability to make credible promises to voters (Keefer, 2004, 2007*a*,*b*; Keefer and Khemani, 2005; Keefer and Vlaicu, 2008).

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.[27] A large literature establishes that reducing poverty leads voters

[24] The $(2a-1)$ term merely signals that clarity of responsibility can be positive or negative. When the politicians is aligned (a = 1), then $2(1) - 1 * t(MV)$ must be positive. When the politician is unaligned (a = 0), then $(2(0) - 1) * t(MV)$ must be negative.

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

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

[27] Clientelism entails the the contingent distribution of material and non-material goods and services in

to discount clientelistic benefits more with respect to policy-based, programmatic benefits.[28] Citizens discount other benefits that corrupt politicians may bring in a similar manner,[29] and we posit that clarity of responsibility amplifies these discounting patterns.

We account for the *additional* discounting brought about by poverty reduction or economic improvement on $W(\gamma r_{i1})$ through β_i. In electorates where poverty has reduced $\beta_i \in (0,1)$, and $\beta_i = 1$ in electorates where poverty remains the same or has increased. In other words, citizens' *a priori* discount rate of $W(\gamma r_{i1})$ remains unchanged in our model except under the scenario in which poverty has reduced.

Especially given information's mixed record in fostering political accountability in poor environments,[30] it is crucial to understand how clarity of responsibility fosters different discount rates of corruption-related benefits. Per Schwindt-Bayer and Tavits (2016) and Appendix B, alignment makes identifying clarity of responsibility easier. Accordingly, we suggest that a magnifies the penalization imposed by a reduction in poverty ($\beta_i \in (0,1)$) on the pre-existing discount rate such that: $\beta^{1+a} = \beta^{1+1} \implies \beta^2 < \beta^1$. In words, alignment leads to even higher discount rate for clientelistic and other benefits than the unaligned

exchange for political support. There are many varieties of clientelism, including vote-buying, (e.g. Auyero, 1999; Stokes, 2005; Finan and Schechter, 2012; Hidalgo and Nichter, 2016); turnout buying (e.g. Nichter, 2008; Larreguy, Marshall and Querubín, 2016); abstention-buying (e.g. Gans-Morse, Mazzuca and Nichter, 2014); double persuasion (e.g. Gans-Morse, Mazzuca and Nichter, 2014); and patronage (Robinson and Verdier, 2013). In making our argument, we make no distinction between the different forms of clientelism; our argument applies to the phenomenon as a whole.

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

[29] Here, we are referring to the trade-off hypothesis, commonly known through the Portuguese expression "*rouba mas faz*" [he steals but gets things done]. In short, voters trade-off the value of a corrupt politician against the clientelistic benefits and other benefits (e.g. ideology) the politician can bring (Magaloni, Díaz-Cayeros and Estévez, 2007; Manzetti and Wilson, 2007; Pereira, Rennó and Samuels, 2011; Winters and Weitz-Shapiro, 2013; Pereira and Melo, 2015; Muñoz, Anduiza and Gallego, 2016; Solaz, De Vries and de Geus, 2019). 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).

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

case due to clarity of responsibility. Given that $\beta = 1$ when poverty remains unchanged or increases, the effects of clarity of responsibility do not travel beyond the poverty-reducing 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).[31] We specify that $x_{i,2} < r_{i,2}$ because politicians in countries with relatively high levels of corruption and clientelism can earn more in office than as a private citizen (e.g., Fisman, Schulz and Vig, 2014). Given Equations (3) and (4), the maximization problem for local-level politician i can be represented as:

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

Appendix A solves the maximization problem in Equation (5), from which we proffer the following two propositions:

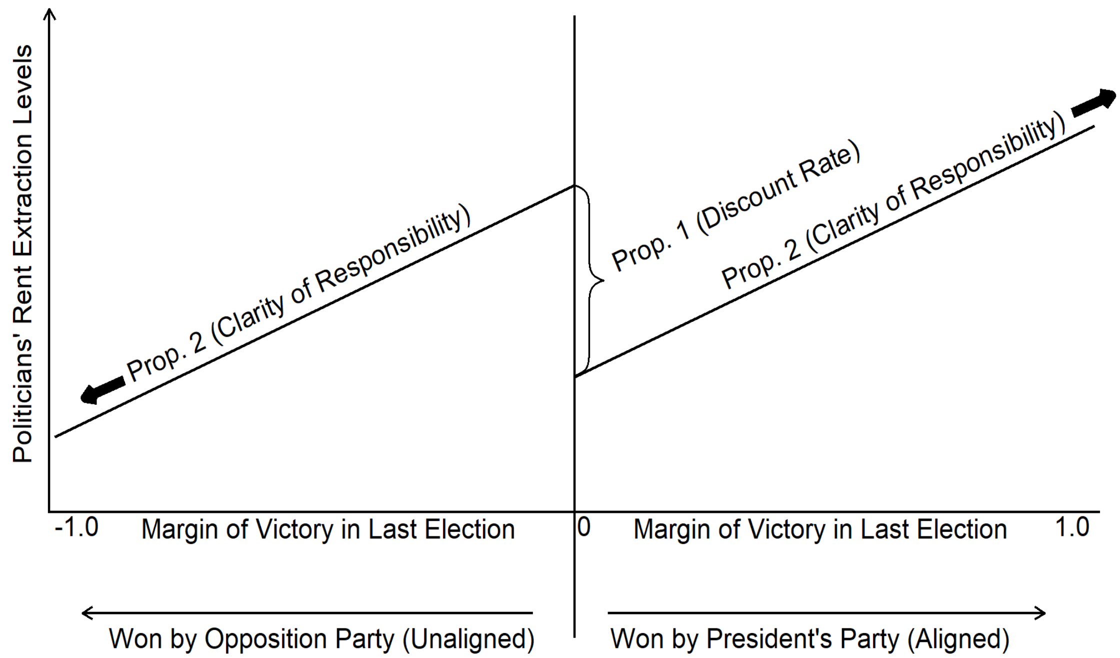
Proposition 1: *Optimal rents for aligned politicians are less than rents for unaligned politicians right at the cutoff when the electorate's economic circumstances have improved.*

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 $MV \to 0$, or right at the cutoff, this difference in

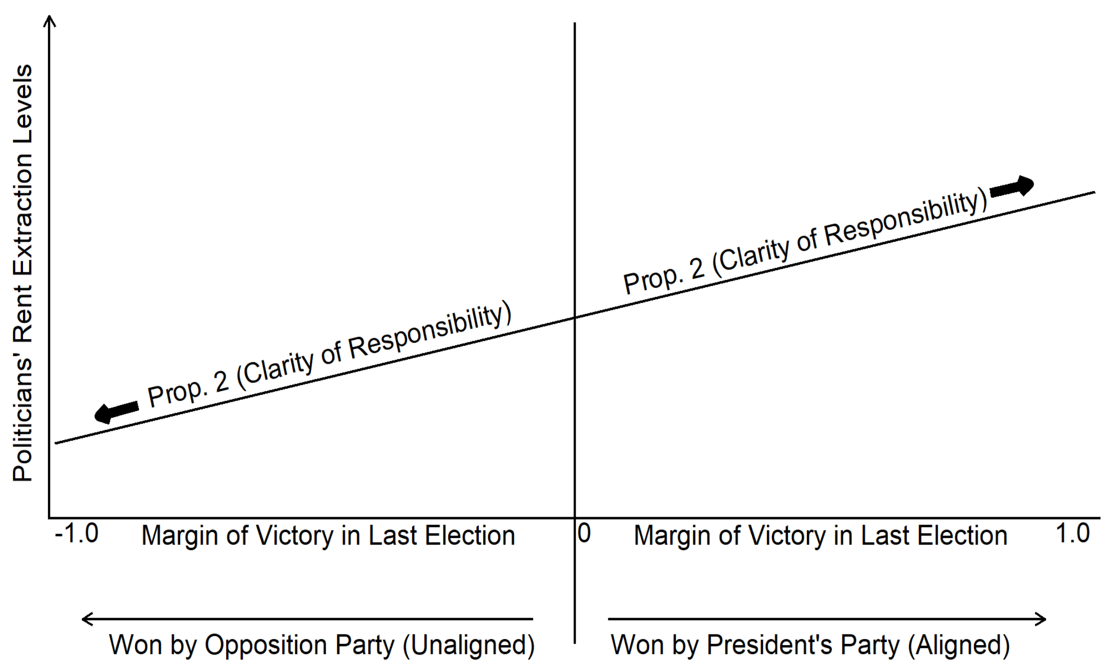
[31] For more on how politicians trade-off rents in the current period compared to those in a future period, see Niehaus and Sukhtankar (2013) on the "golden goose effect."

Figure 1: Graphic Presentation of Propositions 1 and 2

(a) When Poverty Decreases



(b) When Poverty Increases



discount rate results in a discontinuity between the optimal rents extracted, where aligned politicians extract less than the unaligned politicians.

Proof: See Appendix A.

Corollary 1 also shows the case when economic circumstances worsen or remain the same in a given electorate. In such a case, since citizens do not discount any differently in either the aligned or the non-aligned electorates, there does not exist any discontinuity at the cutoff.

Proof: See Appendix A.

Proposition 2: *Optimal rents for aligned politicians increase with respect to the level of margin of victory, while they decrease with respect to the level of 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 varying means described in Appendix B. Unaligned local-level politicians, in turn, react by reducing their optimal rent-seeking behavior in a manner consistent with the levels of MV (see Figure 1). The opposite effect takes place in the aligned municipalities. Since the clarity of responsibility from alignment positively affects citizens' satisfaction with their local-level politician, it provides aligned politicians with additional opportunity for rent extraction as MV increases. Accordingly, our model is consistent with previous literature underscoring that party alignment fuels greater levels of rent extraction and resource availability (Greene, 2010; Brollo and Nannicini, 2012; Carozzi and Repetto, 2016; Corvalan, Cox and Osorio, 2018; Lara and Toro, 2019).

Proof: See Appendix A.

2. Research Design

2.1. Institutional Context for Guatemala

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

Corruption, clientelism, and organized crime present particularly onerous challenges for Guatemala. The country's 2006-2019 partnership with the United Nations' International Commission Against Impunity (CICIG) helped uncover some high-level corruption and dismantle some powerful drug-trafficking networks (Fisman and Golden, 2017; Trejo and Nieto-Matis, 2019). Nevertheless, the country still ranks 144/180 on Transparency International's (2018) Corruption Perceptions Index, part of the reason for which is likely due to clientelistic pressures. For example, vote buying is a concern in social programs, and CICIG investigations have revealed significant use of state resources in the financing of party campaigns (Sandberg and Tally, 2015; Meilán, 2016).

General elections for both the national and municipal levels take place concurrently every four years. For departments, which comprise administrative level-2 units akin to a state or province, the president appoints governors from his or her same political party. Accordingly, Guatemala does not have political variation at the department level. Although it may not be ideal for democracy, it is a boon for our identification strategy, which exploits municipal-presidential political party alignments.

2.2. Identification Strategy

To identify the causal effects of alignment on corruption in each of our samples, we employ a series of sharp electoral regression discontinuity designs. To accommodate the concept of alignment, we modify Lee's (2008) seminal framework for the incumbency advantage along the lines of Brollo and Nannicini (2012). Specifically, we estimate the following Average Treatment Effect (ATE):

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

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).

We take a number of steps to ensure the robustness of the results. First, to guard against the risk of functional form misspecification and bias-variance trade-offs, we follow Gelman and Imbens (2019) and estimate our results with first- and second-order polynomial fits. Second, we employ Calonico et al.'s (2019) new method to consider how adding covariates to our regression discontinuity analyses may alter the results. Third, we cluster our standard errors at the municipality level. Fourth, we 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 comes from Guatemala's National Statistics Institute (INE, *Instituto Nacional de Estadística*) poverty maps. The data specifically refer to the percent of people below the poverty and extreme poverty lines.

As with most countries in the world, Guatemala does not measure municipal-level poverty rates on a yearly basis. Instead, the country only measures municipal-level poverty rates for the whole country during each census. The latest two years for which poverty map/census data are available are 2002 and 2011. From our 331-municipality cross-section in the panel, poverty data are missing from 32 urban municipalities in 2011.[32] Accordingly, we provide a relevant analysis of these missing data in Appendix Q, and conclude that these missing data do not suggest any potential biases.

Given the inability of regression discontinuity designs to accommodate interactions,[33] we use the aforementioned poverty and extreme poverty data to divide our sample into the following groups: poverty-increasing, poverty-decreasing, extreme poverty-decreasing, and extreme poverty-increasing municipalities. 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 rates changes.

We provide the aforementioned estimates by poverty or extreme poverty group for the years 2010-2015 (main analysis), 2011-2015 (Appendix M), 2009-2015 (Appendix N), and 2008-2015 (Appendix O). 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

[32] 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.

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

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).

Following Ferraz and Finan (2008) and Bobonis, Cámara Fuertes and Schwabe (2016), the final two years of an electoral term are also the most significant in terms of corruption. We thus include estimates for the final two years of each electoral term in Appendix G, and Appendix H provides the estimates for the final year of each electoral term.

2.4. Electoral Data

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

Given that the TSE's funding and capacity are limited (Meilán, 2016), we take additional steps to ensure that the data are not marred by electoral fraud and are suitable for analysis, etc. In Appendices K.1, K.2, K.3, K.4, and K.5, we run McCrary (2008) density tests corresponding to our running variable for all of the different samples in the main analyses and appendices. The running variable, margin of victory, passes 10/10 of the tests for the whole sample; 9/10 of the tests, including those for our main models, in the poverty-reducing and extreme-poverty reducing sample; and 5/10 of the tests for the poverty-increasing and extreme poverty-increasing samples. All tests corresponding to the original electoral term data pass the respective tests. The failing tests only correspond to year-wise perspectives of the electoral data.[34]

2.5. Corruption Data

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

[34] 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.

Each year, the National Audit Office audits circa 320 of Guatemala's 340 municipalities. As shown in Appendix K.8, municipalities with aligned party mayors are definitely not more likely to be audited than municipalities with non-aligned party mayors. That is accurate for all of the samples that we examine in this study (see Section 2.3). Accordingly, there are no concerns regarding the partisan distribution of audits.

For each audited municipality from 2004-present, the National Audit Office publishes on its website: the number of overall infractions committed (*sancciones*), and the amount of money in the local currency (Quetzales) associated with these infractions. Both of these variables serve as our study's dependent variables and correspond most closely with bureaucratic corruption. It encompasses both what Brollo et al. (2013, 1774) call "broad corruption" and "narrow corruption".[35] For comparability purposes, we first deflate the money version of the infractions variable and then take its log. We do not transform the number of infractions committed variable.

Table 1 presents descriptive statistics for the infractions variable. We disaggregate the data according to whether they correspond to poverty-increasing/poverty-decreasing and aligned/unaligned municipalities.

2.6. Other Data

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

[35] Broad corruption refers to "irregularities that could also be interpreted as bad administration as rather than as overt corruption." *Narrow corruption* refers to "severe irregularitie that are also more visible to the voters" (Brollo et al., 2013, 1774).

Table 1: Descriptive Statistics of Infraction Variables

Panel A: Infractions (Year Viewpoint) VARIABLES	Increase Unaligned Mean	N	Increase Aligned Mean	N	Decrease Unaligned Mean	N	Decrease Aligned Mean	N
Number of Infractions: All Years	8.807	969	6.376	348	8.183	920	5.472	271
Log Amount of Infractions: All Years	10.76	824	10.77	347	10.73	788	10.56	270
Number of Infractions: First 2 Years of Term	6.00	184	6.286	126	5.985	194	5.233	90
Log Amount of Infractions: First 2 Years of Term	10.57	183	10.66	125	10.61	193	10.27	89
Number of Infractions: Last 2 Years of Term	6.071	395	6.428	222	6.433	383	5.591	181
Log Amount of Infractions: Last 2 Years of Term	10.89	395	10.83	222	10.92	382	10.71	181
Number of Infractions: Final Year of Term	6.894	198	7.387	111	7.370	192	6.242	91
Log Amount of Infractions: Final Year of Term	11.19	198	11.24	111	11.19	191	10.98	91

Panel B: Infractions (Electoral Term) VARIABLES	Increase Unaligned Mean	N	Increase Aligned Mean	N	Decrease Unaligned Mean	N	Decrease Aligned Mean	N
Number of Infractions: All Years	24.11	354	19.99	111	22.47	335	16.30	91
Log Amount of Infractions: All Years	11.66	354	12.23	111	11.54	335	11.84	91
Number of Infractions: First 2 Years of Term	12.00	92	12.77	62	12.09	96	10.47	45
Log Amount of Infractions: First 2 Years of Term	11.44	92	11.63	62	11.58	96	11.08	45
Number of Infractions: Last 2 Years of Term	12.05	199	12.86	111	12.83	192	11.12	91
Log Amount of Infractions: Last 2 Years of Term	11.75	199	11.79	111	11.82	192	11.56	91
Number of Infractions: Final Year of Term	6.894	198	7.387	111	7.408	191	6.242	91
Log Amount of Infractions: Final Year of Term	11.19	198	11.24	111	11.19	191	10.98	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.

Initiative.[36] Table 2 presents descriptive statistics of these covariate data.

3. Results

3.1. Corruption Results Disaggregated by Poverty

Figure 2 provides optimal data-driven regression discontinuity plots of our main results for corruption in the poverty-reducing samples. We present these plots using Calonico, Cattaneo and Titiunik’s (2015) evenly-spaced variance method and second-degree polyno-

[36] 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; urban community service.

Figure 2: RDD plots for Infraction Count and Amount (Poverty Reduction)

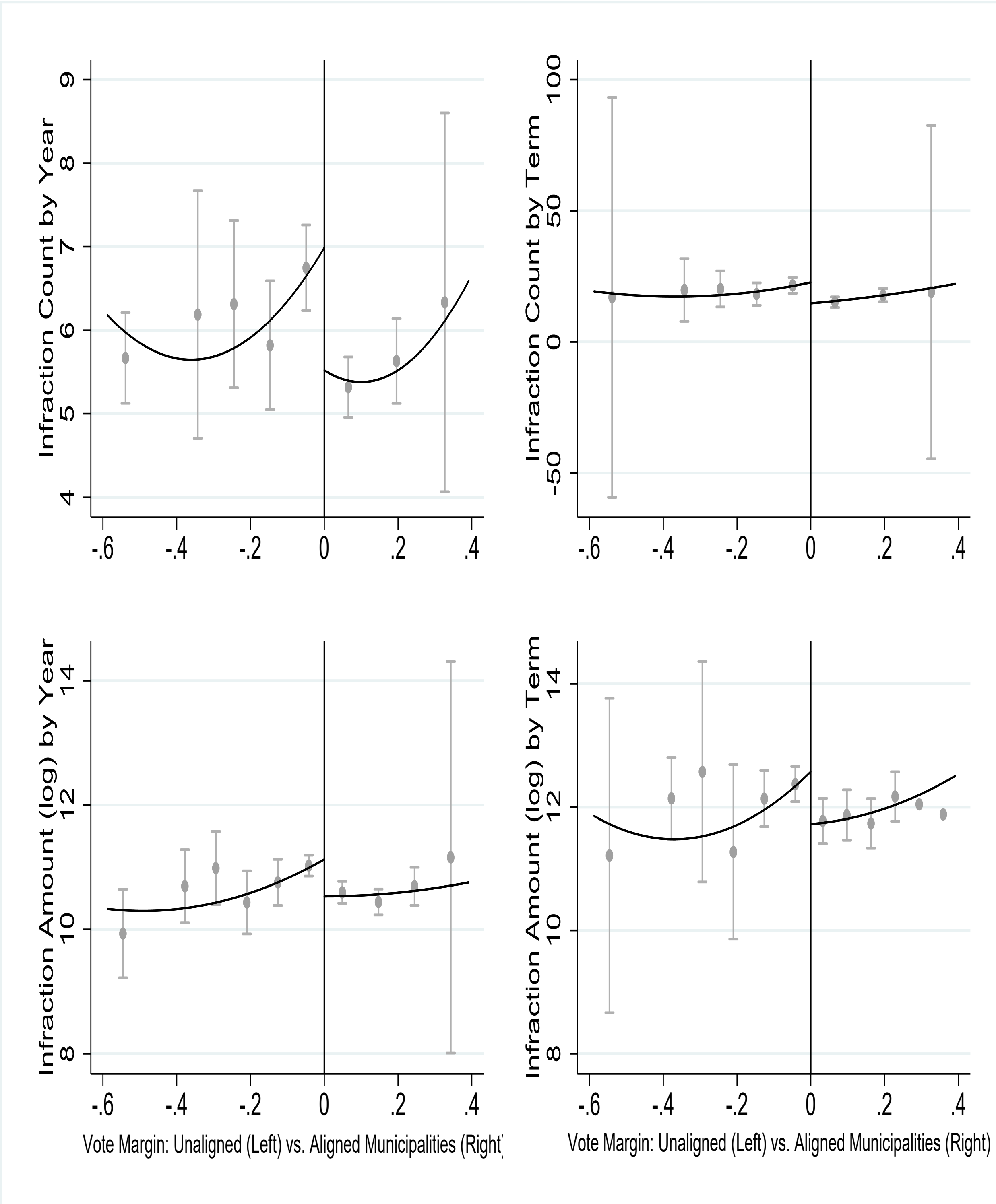


Table 2: Descriptive Statistics of Covariates

Panel A VARIABLES	Increase Unaligned Mean	N	Increase Aligned Mean	N	Decrease Unaligned Mean	N	Decrease Aligned Mean	N
Percentage of Mayor Reelected	0.307	1,005	0.217	332	0.331	968	0.0945	254
Extreme Poverty Rate	24.94	1,047	25.35	348	16.52	1,006	15.53	272
Gini coefficient	24.87	1,047	25.29	348	24.93	1,006	23.94	272
Total Poverty Rate	72.54	1,047	70.96	348	66.23	1,006	65.09	272
Log Population	10.28	1,047	10.22	348	10.33	1,006	10.12	272
Log Public Goods Spending (per capita)	5.790	582	5.518	348	5.512	580	5.744	272

Panel B VARIABLES	Increase Unaligned mean	N	Increase Aligned mean	N	Decrease Unaligned mean	N	Decrease Aligned mean	N
Percentage of Mayor Reelected	0.306	333	0.214	103	0.320	316	0.122	82
Extreme Poverty Rate	26.13	354	27.91	111	19.13	335	19.83	91
Gini coefficient	25.56	354	26.17	111	25.56	335	25.26	91
Total Poverty Rate	73.87	354	73.37	111	68.44	335	68.84	91
Log Population	10.27	354	10.23	111	10.33	335	10.10	91
Log Public Goods Spending (per capita)	6.673	199	6.556	111	6.351	193	6.625	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.

mial fits following Gelman and Imbens (2019). For comprehensiveness, we estimate these results using both the number of municipal-level infractions committed and the log amounts associated with those infractions as the dependent variables.

The results are similar for both yearly and electoral term data: party alignment consistently yields less corruption in the poverty-reducing sample. In Appendix C, Tables C1 and C2 present the results for the infractions dependent variable, and Tables C3 and C4 present the results for the infraction amounts dependent variables. For all of these tables, Panel A provides the results as typically presented in the literature (without fixed effects), whereas Panel B adds fixed effects in line with Frey (2019). The results are not only statistically significant but substantively significant as well. For example, in our base specification, aligned municipalities commit as average of 11.5 infraction less in each term or 1.5 infractions each year.

Controlling for the influence of covariates within the data-driven bandwidth in line with

Calonico et al. (2019) does not alter the interpretation of our results. In Tables K1 and K2 of Appendix K, we further show that these results are not due to outliers. When we change the samples to encompass different years in Appendices M.1, N.1, O.1, we also find similar results. Similar to Brollo and Nannicini (2012), we fail only one of the ten placebo tests that we run in Appendix K.7, for which we test the effects of alignment at varying cutoffs. Given that tests reveal that poverty is not empirically endogenous to corruption (See Appendix L), the results for the 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 G1 and G2 in Appendix G.1 show that the results for the last two years, and Tables H1 and Tables H2 in Appendix H.1 present the results for the final year before the election. When compared to the results from the first two years in Tables I1 and I2 in Appendix I, it is clear that the final two years are mostly driving the decrease in corruption in the poverty-reducing sample. Overall, these results are consistent with Ferraz and Finan (2008) and Bobonis, Cámara Fuertes and Schwabe (2016), who find that audits in Brazil and Puerto Rico are most effective at reducing corruption closer to elections. More broadly, the results of our analysis are consistent with Barro (1973), Ferejohn (1986), Ferraz and Finan (2011), and de Janvry, Finan and Sadoulet (2012): elections help discipline politicians. In our case, that applies even to aligned politicians, who generally enjoy resource advantages relative to non-aligned politicians (e.g. Brollo and Nannicini, 2012; Carozzi and Repetto, 2016; Corvalan, Cox and Osorio, 2018; Lara and Toro, 2019).

As predicted by our theory, alignment only reduces corruption in the poverty-reducing sample. Appendix E disaggregates results for the sample in which poverty increased from one census to next. As Tables E1, E2, E3, and E4 show, results generally shift in the opposite direction: municipalities in which poverty increased from one census to the next experienced an increase in corruption—again, measured by infractions the log amounts associated with those infractions. Theoretically, it is logical that poorer voters may be more forgiving of mayors' corruption, as long as the mayors share their rents with voters through clientelistic or other means means (Fernández-Vázquez, Barberá and Rivero, 2016). However, the year-wise specifications for the poverty-increasing sample fail the McCrary (2008) density tests in Appendices K.1, K.2, K.3, and K.4, 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 M.2, N.2, and O.2. Accordingly, we caution against interpreting the poverty-increasing sample results as definitive evidence of higher poverty facilitating aligned mayors to extract higher levels of rents.

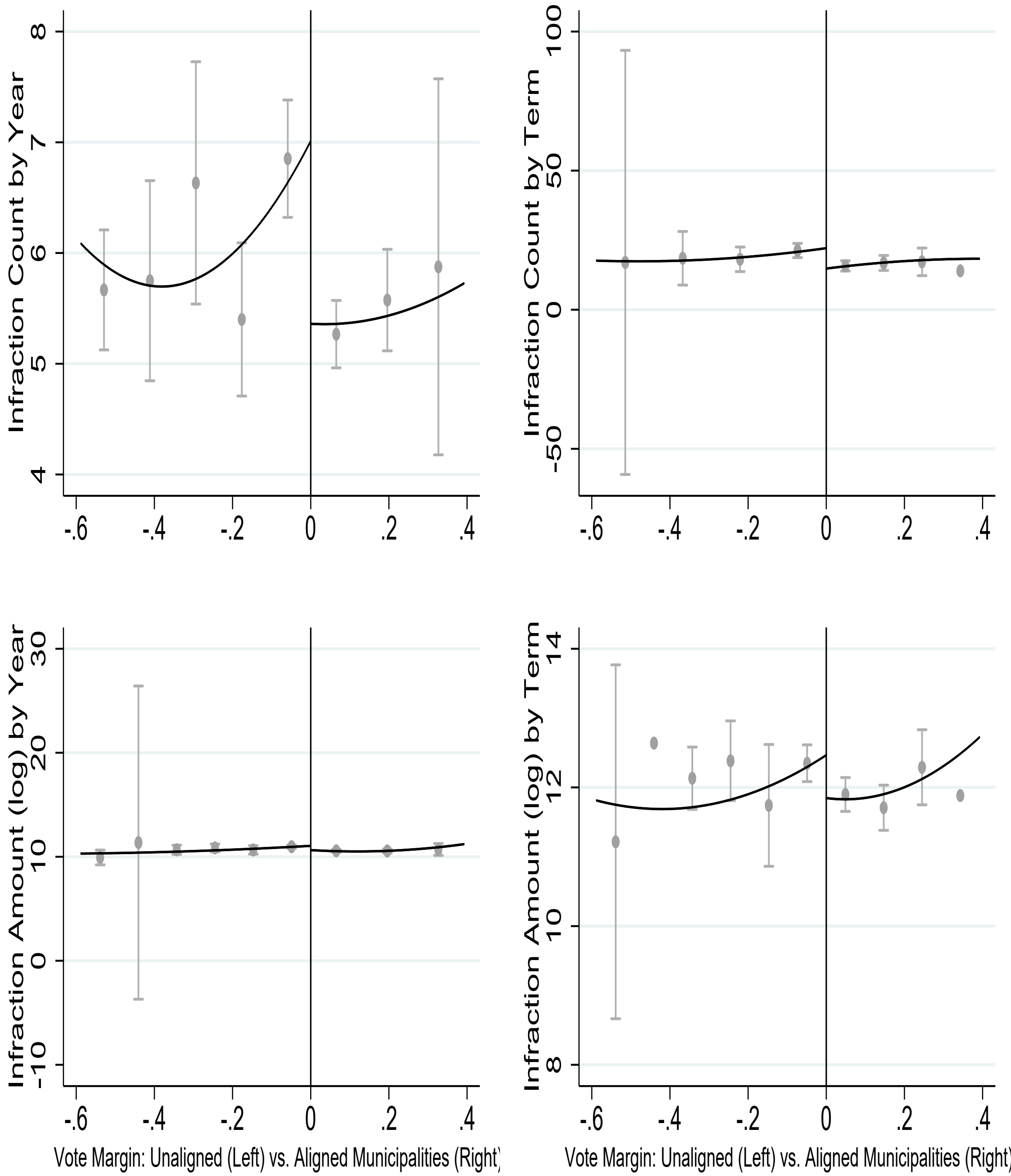
For purposes of comparison with current predictions of clarity of responsibility theory (see Schwindt-Bayer and Tavits, 2016), Tables J1, J2, J3, and J4 in Appendix J show the results for the whole sample—i.e., when not disaggregating by poverty. Overall, these tables show very inconsistent findings. 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 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 whether improving economic conditions can reduce corruption from aligned politicians, we also examine the extent to which reducing extreme poverty from one census to the next yields similar results as those of the poverty-reducing sample. Figure 3 shows the main results for extreme poverty reduction using Calonico, Cattaneo and Titiunik's (2015) evenly-spaced variance method and second-degree polynomial fits following Gelman and Imbens (2019). In all specifications, alignment reduces corruption when extreme poverty declines as well.

Tables D1, D2, D3, and D4 in Appendix D present the detailed results. In our base

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



specification, aligned municipalities commit an average 6.8 less infractions each term or 1.5 less infractions each year. Results are a bit weaker for the log amounts, as not all specifications are statistically significant. Nevertheless, the results with the log amount as the dependent variable are still suggestive of the same overall pattern: reductions in extreme poverty yields a situation in which aligned politicians reduce their overall corruption levels.

As with the previous subsection, the same results do not hold for the sample in which extreme poverty increased from one census to the next. We present the detailed results for the poverty-increasing sample in Tables F1 and F2 in Appendix F. In 21/24 specifications entailing counts of the number of infractions and the amounts associated with those infractions, the coefficient for alignment is positive, indicating that alignment yields an increase in corruption. However, similar to the previous section on the poverty-increasing sample, none of the results are statistically significant for the extreme poverty-increasing sample, and the year-wise specification does not pass the McCrary (2008) density test (see Appendix K.5).

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 B). 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.

Table 3: Infractions and Misappropriated Money Amounts by Alignment Shares and Electoral Term

Term	Years Coded	Municipalities Aligned	Infractions Mean	Amount Mean	Log Amount Mean
2004-2007	2007	22%	4.86	181,967.5 Q	10.51
2008-2011	2008-2011	31%	20.77	442,884.8 Q	12.38
2012-2015	2012-2015	36%	26.10	449,274.2 Q	12.56
2016-2019	2016-2018	0	31.74	568,759.2 Q	12.60

Note: All amounts adjusted for inflation in the local currency, Quetzales.

Table 4: Number of Infractions Committed (2007-2018)

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.781***	0.739***	0.629***	0.779***	0.564***	0.650***
	(0.027)	(0.028)	(0.046)	(0.026)	(0.036)	(0.046)
Poverty Reduced		-0.068*	-0.069*			
		(0.036)	(0.036)			
Population (log)					1.605***	-0.346
					(0.196)	(0.320)
Re-elected Mayor					0.028	0.019
					(0.033)	(0.031)
Observations	3821	3384	3384	3821	3478	3478
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; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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.[37] The lack of alignments for the 2016-2019 period limits the ability of the results in the previous sections to travel to other instances party system instability. By the same token, the shock of electing a populist outsider and its consequent effects on alignment allows us to credibly

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

identify the power of the alignment mechanism and thus support the results presented in Sections 3.1 and 3.2.

Both the mean number of municipal-level infractions and amount of misappropriated money increased significantly after the election of Morales (See Table 3). The results from Sections 3.1 and 3.2 show that alignment signals clarity of responsibility strongest after voters' economic circumstances improve. Nevertheless, the overall lack of alignments for all configurations of economic circumstances for the 2016-2019 electoral term still suggests that corruption should rise. More specifically, the lack of alignments means that mayors can blame poor outcomes on the president and vice-versa, and voters have more difficulty assigning blame, leading to less accountability for mayors and more corruption overall.

In all likelihood, the (quasi) natural experiment of Morales' election is not sufficient for the descriptive statistics presented in Table 3 to be interpreted own their own. We therefore supplement these descriptive statistics with the regression analyses presented in Table 4 as well as Tables R1 and R2 in Appendix R. Each regression contains the main covariates used in our regression discontinuity analyses throughout the paper as well as the poverty reduction indicator 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. 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 stronger for the number of infractions than the log amounts associated with those infractions, but the overwhelming evidence points to increased corruption following the election of populist outsider Jimmy 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 to the lack of alignments, mayors take advantage of the institutional 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 both our 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 L.2. Since endogeneity entails a correlation between the independent variable (in our case, alignment) and the error term, we first directly test for such a relationship using two-stage regression analysis. In the first stage, we separately run a regression of poverty on each of corruption variables: the number of infractions committed and the log amounts associated with those infractions. In the second stage, we regress the residuals from the first-stage equation on each corruption variable. In all instances, the results suggest no overall relationship and R-squared values that are essentially 0, indicating that there is no endogeneity between poverty and corruption. Since the lack of enodgeneity is so critical to our results, we undertake a second set of regression analyses as well. More specifically, in Appendix L.1 we test whether corruption predicts poverty in a conventional linear regression. Using numerous specifications for both the year-wise and term-wise results, we find no empirical support for the proposition that poverty predicts corruption.

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

Consider, for example, Panel A of Table P1 in Appendix P.1, 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 P2, 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 P provides even more tables and relevant analysis.

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

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

In this subsection, we subject Proposition 1 to further scrutiny by conducting placebo tests that examine corruption activity of aligned and unaligned politicians away from the cutoff. In our regression discontinuity models, Calonico, Cattaneo and Titiunik's (2014) algorithm usually resulted in data-driven bandwidths for MV at around 10% on either side of the cutoff. Accordingly, in order to avoid duplication of previous work, in this section we analyze the data in which $MV > 10\%$. 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, then analysis of data away from the cutoff should not exhibit the same pattern as those in earlier sections. Per Proposition 2, levels of rent extraction between aligned and unaligned municipalities should be statistically indistinguishable from zero (or null) as MV increases away from the cutoff.

Table 5: Infractions: How Close Elections Matter (2010-2015)

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	-0.064	-0.060	-0.073	0.029	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					2.814***	-1.016
					(0.501)	(0.997)
Reelected Mayor					0.064	0.065
					(0.066)	(0.064)
Observations	1259	1124	1124	1259	1177	1177
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.

Table 5 and Table S1 in Appendix S present the main results from the analysis of infractions outside the close election bandwidth. Under myriad negative binomial and poisson model specifications, alignment does not correlate with the number of infractions committed in less competitive elections. Results are similar when analyzing the log amount of misappropriated in Table S2 in Appendix S as well. The effect of alignment is only statistically distinguishable from zero when without municipal fixed effects. After adding the municipal fixed effects and control variables, the effect of alignment quickly becomes null. In short, the placebo tests we have conducted here do not show a causal relationship, but they provide support for the existence of a close election mechanism, which Propositions 1 and 2 buttress.

5. Conclusion

In a recent review, eminent corruption scholars Golden and Mahdavi (2015, 414) suggest that "[t]o understand variations in the frequency of bureaucratic corruption requires a theory of electoral incentives governing strategies of bureaucratic slippage, something that is a long way off". By showing how economic development and political institutions interact with electoral incentives to produce different levels of bureaucratic corruption, we demonstrated that such a theory is no longer "a long way off". More specifically, our above analysis shows that the combination of party alignment, significant electoral competition, and poverty reduction decreases corruption.

We find causal support for our theory using regression discontinuity designs that measure corruption both through audit infractions and the (log) amounts of money associated with those infractions. Of these two dependent variables, results are marginally stronger for infractions than log amounts. That pattern is likely due to the greater electoral risk associated with stealing large amounts of money vis-à-vis committing lots of small infractions that are less visible to voters. Our results are similar, albeit somewhat weaker, when party alignment dovetails with significant electoral competition and extreme poverty reduction. From a measurement perspective, our paper undertakes numerous checks that scholars can follow to credibly analyze corruption outside a context with randomized audits like Brazil, which has heretofore served as the main country examined in the literature.[38]

For our above theory to hold, it is necessary to have some form of party system stability.

[38] 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).

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. Local-level politicians, in turn, take advantage of these institutional circumstances to oversee more corrupt governments at the local level. The loss of alignments makes it easier for local-level politicians to blame the executive for misgovernance and vice-versa, thereby making clarity of responsibility more difficult to discern.

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

Poverty is not endogenous to corruption in our models (see Appendix L), but the subgroup analyses that we performed throughout the paper only allowed us to make causal inferences about each subgroup independently. Regression discontinuity designs cannot 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. Despite these limitations, our regression discontinuity analyses allow us to make causal inferences about each poverty subgroup independently. In turn, our overall results suggest that modernization forces place subnational units within a polity on different corruption paths—and, potentially, democratic paths as well.

Appendix

A. Theoretical Derivation

Proposition 1 *Optimal rent levels for aligned politicians are less than rents levels for unaligned politicians at the cutoff when the electorate's economic circumstances have improved.*

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

$$\begin{aligned} &\max_{r_{i,1}} U(r_{i,1}) + \pi(s_i)U(r_{i,2}) + [1-\pi(s_i)]U(x_{i,2}) \\ &\qquad \text{where } s_i = W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a-1)t(MV) \end{aligned} \tag{7}$$

Accordingly, we can rewrite the maximization problem as follows:

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

The corresponding First-Order Condition (F.O.C.) for Equation (8) is:

$$\begin{aligned} 0 = & \; U'(r_{i,1}) + U(r_{i,2})\pi'(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a-1)t(MV))[-W'(1-r_{i,1}) \\ & + \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] - U(x_{i,2})\pi'(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) \\ & + (2a-1)t(MV))[-W'(1-r_{i,1}) + \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] \end{aligned} \tag{9}$$

Collecting like terms and bringing them to the other side, Equation (9) can be rewritten as:

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

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 (10) can be written as:

$$U'(r_{i,1}) = [U(r_{i,2}) - U(x_{i,2})]\pi'(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}))[W'(1 - r_{i,1}) - \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] \tag{11}$$

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

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

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

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

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 $z, k > 0$.[39]

Corollary 1: *Optimal rents extraction levels for aligned and unaligned politicians do not differ at the cutoff if economic circumstances remain at the status quo or worsen.*

This proof follows from replacing $\beta_i = 1$ in Equation (10) 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 the level of margin of victory, while they decrease with respect to the margin of victory for the unaligned politicians.*

The proof of this Proposition follows a similar structure as Brollo and Nannicini (2012, Proof of Proposition 2). Per Equation (9), the first-order condition is $g(r_{i,1}, MV) = 0$, so by implicit differentiation $\partial r_{i,1}/\partial MV = -(\partial g/\partial MV)/(\partial g/\partial r_{i,1})$, where $\partial g/\partial r_{i,1} < 0$ due to

[39] The result follows from similar structural implications as derived in Brollo and Nannicini (2012, Proof of Proposition 1).

the maximisation second order condition. By extension, therefore:

$$\begin{aligned}\partial g/\partial MV =&[U(r_{i,2}) - U(x_{i,2})]\pi'_{MV}(W(1 - r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1})+ \\ &(2a - 1)t(MV))[W'(1 - r_{i,1}) - \gamma\beta_i^{1+a}W'(\gamma r_{i,1})][(2a - 1)t'(MV)]\end{aligned} \tag{14}$$

When $a = 1$:

$$\begin{aligned}\partial g/\partial MV =&[U(r_{i,2}) - U(x_{i,2})]\pi'_{MV}(W(1 - r_{i,1}) + \beta_i^2W(\gamma r_{i,1})+ \\ &t(MV))[W'(1 - r_{i,1}) - \gamma\beta_i^2W'(\gamma r_{i,1})]t'(MV) > 0\end{aligned} \tag{15}$$

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 $a = 0$:

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

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

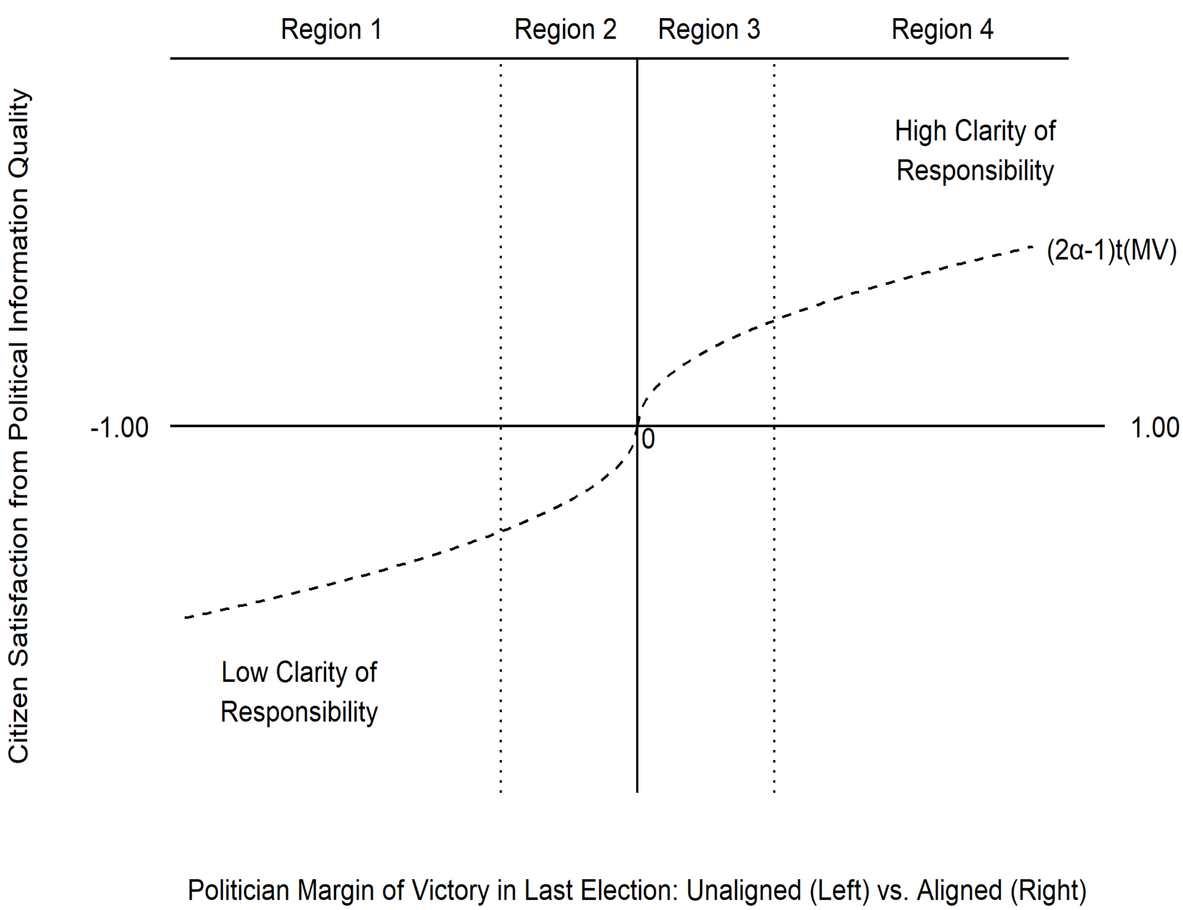
B. Party Alignment's Effects on Clarity of Responsibility and Citizen Satisfaction

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 B.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.[40] 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 B.1. Again, electoral competition is the primary driver of these information flows and intensifies with smaller margins of victory for the incumbent.

[40] 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.

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



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.[41] 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.

[41] This relative lack of clarity and information leads to higher dissatisfaction in Region 1 than the information-overload encountered in Region 2

C. When Poverty Decreases

Table C1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.508***	-2.092***	-0.786	-1.162	-1.027*	-1.691**
	(0.534)	(0.703)	(0.564)	(0.744)	(0.592)	(0.765)
Observations	601	601	569	569	569	569
Effective observations	[206,147]	[192,139]	[170,112]	[170,118]	[150,102]	[154,104]
Covariates	None	None	Some	Some	All	All
p-value	0.00473	0.00293	0.163	0.118	0.0826	0.0270
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.121	0.103	0.0935	0.0959	0.0816	0.0850
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.170**	-1.532**	-0.498	-0.834	-0.742	-1.435*
	(0.580)	(0.759)	(0.608)	(0.759)	(0.627)	(0.774)
Observations	601	601	569	569	569	569
Effective observations	[190,138]	[198,139]	[158,106]	[174,128]	[150,104]	[154,104]
Covariates	None	None	Some	Some	All	All
p-value	0.0436	0.0436	0.413	0.272	0.237	0.0637
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.107	0.0876	0.0977	0.0819	0.0868

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 C2: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-11.46***	-14.07***	-8.176**	-10.12**	-7.859**	-8.948**
	(3.075)	(4.204)	(3.637)	(4.327)	(3.423)	(4.143)
Observations	195	195	179	179	179	179
Effective observations	[54,43]	[62,49]	[44,32]	[57,45]	[44,32]	[57,44]
Covariates	None	None	Some	Some	All	All
p-value	0.000194	0.000819	0.0246	0.0194	0.0217	0.0308
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0901	0.104	0.0737	0.111	0.0726	0.106
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-3.607*	-5.402**	-1.615	-3.220	-2.493	-5.052*
	(1.883)	(2.731)	(2.166)	(2.825)	(2.295)	(2.953)
Observations	195	195	179	179	179	179
Effective observations	[62,49]	[61,49]	[46,35]	[53,42]	[45,34]	[47,35]
Covariates	None	None	Some	Some	All	All
p-value	0.0554	0.0479	0.456	0.254	0.278	0.0871
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.107	0.104	0.0831	0.0968	0.0762	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 C3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.752***	-0.620**	-0.566**	-0.518*	-0.655**	-0.594*
	(0.211)	(0.309)	(0.254)	(0.304)	(0.263)	(0.320)
Observations	598	598	566	566	566	566
Effective observations	[221,151]	[182,138]	[144,98]	[170,118]	[146,102]	[188,129]
Covariates	None	None	Some	Some	All	All
p-value	0.000369	0.0452	0.0260	0.0883	0.0127	0.0639
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.129	0.0980	0.0737	0.0946	0.0786	0.111
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.727***	-0.584*	-0.496*	-0.459	-0.574**	-0.591*
	(0.214)	(0.309)	(0.261)	(0.312)	(0.266)	(0.321)
Observations	598	598	566	566	566	566
Effective observations	[208,147]	[182,138]	[144,94]	[170,118]	[144,98]	[170,118]
Covariates	None	None	Some	Some	All	All
p-value	0.000684	0.0587	0.0573	0.140	0.0306	0.0656
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.122	0.0981	0.0717	0.0953	0.0740	0.0956

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 C4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.234***	-1.196**	-1.074***	-1.026**	-1.009***	-1.056**
	(0.426)	(0.543)	(0.385)	(0.507)	(0.369)	(0.466)
Observations	195	195	179	179	179	179
Effective observations	[48,37]	[56,45]	[45,34]	[51,38]	[47,35]	[51,38]
Covariates	None	None	Some	Some	All	All
p-value	0.00377	0.0275	0.00527	0.0429	0.00627	0.0235
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0737	0.0947	0.0802	0.0906	0.0870	0.0909
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.816**	-0.759	-0.644*	-0.568	-0.722*	-0.700
	(0.370)	(0.468)	(0.377)	(0.482)	(0.371)	(0.484)
Observations	195	195	179	179	179	179
Effective observations	[49,39]	[57,48]	[45,34]	[52,40]	[47,35]	[53,43]
Covariates	None	None	Some	Some	All	All
p-value	0.0274	0.105	0.0877	0.239	0.0518	0.148
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0792	0.0979	0.0778	0.0959	0.0863	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.

D. When Extreme Poverty Decreases

Table D1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.487***	-1.966***	-1.193*	-1.368*	-1.344**	-2.044**
	(0.559)	(0.731)	(0.631)	(0.771)	(0.653)	(0.811)
Observations	670	670	625	625	625	625
Effective observations	[191,162]	[203,172]	[144,132]	[196,161]	[140,130]	[172,144]
Covariates	None	None	Some	Some	All	All
p-value	0.00786	0.00717	0.0588	0.0758	0.0396	0.0117
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0952	0.102	0.0825	0.107	0.0770	0.0930
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.291**	-1.717**	-1.023	-1.155	-1.191*	-1.606**
	(0.588)	(0.759)	(0.648)	(0.786)	(0.673)	(0.809)
Observations	670	670	625	625	625	625
Effective observations	[191,162]	[213,173]	[144,132]	[196,161]	[140,130]	[188,160]
Covariates	None	None	Some	Some	All	All
p-value	0.0280	0.0236	0.114	0.142	0.0769	0.0471
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0946	0.105	0.0819	0.110	0.0767	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 D2: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-6.890**	-6.661*	-8.121***	-8.497**	-9.876***	-10.66***
	(2.984)	(3.428)	(3.045)	(3.498)	(3.144)	(3.908)
Observations	217	217	194	194	194	194
Effective Observations	[60,58]	[81,83]	[44,44]	[68,62]	[41,41]	[58,54]
Covariates	None	None	Some	Some	All	All
p-value	0.0209	0.0520	0.00765	0.0151	0.00168	0.00638
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0962	0.167	0.0858	0.140	0.0734	0.108
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-3.612*	-5.132*	-3.687	-4.472	-4.792**	-6.266**
	(2.037)	(2.733)	(2.295)	(2.847)	(2.392)	(2.919)
Observations	217	217	194	194	194	194
Effective observations	[59,54]	[64,60]	[42,43]	[58,54]	[41,42]	[56,53]
Covariates	None	None	Some	Some	All	All
p-value	0.0762	0.0604	0.108	0.116	0.0451	0.0319
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0915	0.103	0.0794	0.108	0.0750	0.102

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 D3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.447*	-0.483	-0.315	-0.287	-0.360	-0.341
	(0.241)	(0.308)	(0.257)	(0.317)	(0.267)	(0.334)
Observations	667	667	622	622	622	622
Effective observations	[187,156]	[195,172]	[144,132]	[184,160]	[144,132]	[196,161]
Covariates	None	None	Some	Some	All	All
p-value	0.0631	0.116	0.220	0.365	0.177	0.308
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0916	0.0996	0.0816	0.100	0.0844	0.108
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.437*	-0.477	-0.297	-0.272	-0.321	-0.395
	(0.243)	(0.307)	(0.258)	(0.320)	(0.264)	(0.326)
Observations	667	667	622	622	622	622
Effective observations	[183,152]	[195,172]	[144,130]	[184,160]	[140,130]	[176,158]
Covariates	None	None	Some	Some	All	All
p-value	0.0720	0.121	0.251	0.396	0.225	0.227
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0905	0.0993	0.0813	0.100	0.0800	0.0961

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 D4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.710*	-0.766	-0.612*	-0.698	-0.691**	-0.937**
	(0.395)	(0.486)	(0.353)	(0.490)	(0.329)	(0.470)
Observations	217	217	194	194	194	194
Effective observations	[49,46]	[60,56]	[51,48]	[51,48]	[54,53]	[44,44]
Covariates	None	None	Some	Some	All	All
p-value	0.0726	0.115	0.0829	0.154	0.0358	0.0461
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0714	0.0957	0.0918	0.0927	0.100	0.0848
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.551	-0.567	-0.364	-0.371	-0.505	-0.640
	(0.358)	(0.428)	(0.327)	(0.464)	(0.318)	(0.462)
Observations	217	217	194	194	194	194
Effective observations	[49,46]	[61,59]	[53,52]	[53,53]	[56,53]	[52,50]
Covariates	None	None	Some	Some	All	All
p-value	0.123	0.186	0.266	0.424	0.112	0.166
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0723	0.0997	0.0967	0.0996	0.102	0.0955

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. When Poverty Increases

Table E1: 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	0.545	0.965
	(0.942)	(1.566)	(1.030)	(1.627)	(1.082)	(1.585)
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	0.615	0.543
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	0.590	0.956
	(0.983)	(1.549)	(1.090)	(1.599)	(1.090)	(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	0.588	0.540
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 E2: 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	-2.481	3.347
	(3.797)	(6.035)	(4.063)	(6.561)	(4.466)	(8.440)
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
p-value	0.266	0.278	0.762	0.607	0.578	0.692
Order of Polynomial	1	2	1	2	1	2
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	1.448	1.881	1.701	3.016	0.943	2.634
	(3.180)	(4.584)	(3.540)	(5.265)	(3.740)	(5.100)
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
p-value	0.649	0.682	0.631	0.567	0.801	0.606
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.109	0.144	0.0958	0.128	0.0921	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 E3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.303	0.193	0.255	-0.150	0.264	-0.213
	(0.301)	(0.402)	(0.315)	(0.638)	(0.314)	(0.575)
Observations	603	603	560	560	560	560
Effective observations	[158,212]	[196,274]	[131,184]	[131,176]	[131,182]	[137,192]
Covariates	None	None	Some	Some	All	All
p-value	0.314	0.631	0.418	0.814	0.400	0.711
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.121	0.173	0.114	0.107	0.113	0.118
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.349	0.220	0.0925	-0.191	0.0660	-0.238
	(0.283)	(0.390)	(0.358)	(0.629)	(0.382)	(0.608)
Observations	603	603	560	560	560	560
Effective observations	[164,238]	[206,282]	[125,164]	[129,176]	[115,158]	[131,182]
Covariates	None	None	Some	Some	All	All
p-value	0.218	0.572	0.796	0.762	0.863	0.695
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.141	0.184	0.0933	0.103	0.0869	0.111

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

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.600	0.386	0.418	0.462	0.301	0.172
	(0.369)	(0.548)	(0.395)	(0.468)	(0.388)	(0.591)
Observations	196	196	174	174	174	174
Effective observations	[55,61]	[60,79]	[44,58]	[59,88]	[45,60]	[48,75]
Covariates	None	None	Some	Some	All	All
p-value	0.104	0.481	0.290	0.324	0.438	0.772
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.147	0.114	0.204	0.116	0.150
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.516	-0.128	0.554	0.494	0.599*	0.530
	(0.329)	(0.651)	(0.346)	(0.438)	(0.334)	(0.449)
Observations	196	196	174	174	174	174
Effective observations	[57,74]	[57,64]	[46,69]	[59,88]	[46,73]	[58,86]
Covariates	None	None	Some	Some	All	All
p-value	0.117	0.844	0.109	0.259	0.0728	0.238
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.133	0.118	0.131	0.204	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.

F. When Extreme Poverty Increases

Table F1: 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	0.656	1.567
	(0.967)	(1.772)	(1.068)	(2.156)	(1.274)	(1.891)
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	0.607	0.407
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	1.946	6.492
	(4.751)	(6.555)	(4.782)	(7.647)	(3.698)	(9.507)
Observations	174	174	159	159	159	159
Effective observations	[49,47]	[54,67]	[43,46]	[45,56]	[47,63]	[43,47]
Covariates	None	None	Some	Some	All	All
p-value	0.528	0.620	0.775	0.553	0.599	0.495
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.105	0.152	0.110	0.128	0.149	0.112

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 F2: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.179	-0.132	0.108	-0.0204	0.103	-0.0827
	(0.335)	(0.694)	(0.359)	(0.727)	(0.365)	(0.665)
Observations	534	534	504	504	504	504
Effective observations	[141,186]	[135,154]	[123,152]	[123,144]	[123,150]	[123,150]
Covariates	None	None	Some	Some	All	All
p-value	0.592	0.850	0.763	0.978	0.779	0.901
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.129	0.112	0.113	0.100	0.112	0.113
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.323	0.239	0.213	0.119	0.140	0.137
	(0.438)	(0.548)	(0.458)	(0.700)	(0.454)	(0.764)
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
p-value	0.461	0.664	0.642	0.865	0.757	0.858
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.121	0.191	0.117	0.142	0.119	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.

G. Last Two Years of the Electoral Term

G.1. When Poverty Decreases

Table G1: RDD Estimates for Infraction Count by Year and Term (Final 2 Years of Term)

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.945**	-2.643**	-1.156	-1.742	-1.405	-2.250*
	(0.825)	(1.082)	(0.866)	(1.112)	(0.923)	(1.248)
Observations	389	389	357	357	357	357
Effective observations	[112,90]	[124,97]	[96,72]	[112,87]	[92,68]	[94,70]
Covariates	None	None	Some	Some	All	All
p-value	0.0184	0.0146	0.182	0.117	0.128	0.0715
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0952	0.108	0.0875	0.103	0.0810	0.0870
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-4.107**	-5.470**	-2.584	-3.540	-2.591	-3.781
	(1.720)	(2.208)	(1.795)	(2.229)	(1.967)	(2.481)
Observations	194	194	178	178	178	178
Effective observations	[53,41]	[62,49]	[45,34]	[57,44]	[44,33]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.0169	0.0132	0.150	0.112	0.188	0.128
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0887	0.105	0.0793	0.105	0.0749	0.0911

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 G2: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.226***	-1.295***	-0.989***	-1.140**	-1.005***	-1.158**
	(0.374)	(0.449)	(0.349)	(0.452)	(0.357)	(0.460)
Observations	388	388	356	356	356	356
Effective observations	[96,72]	[118,96]	[104,76]	[110,86]	[106,86]	[120,93]
Covariates	None	None	Some	Some	All	All
p-value	0.00104	0.00392	0.00457	0.0117	0.00487	0.0118
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0725	0.102	0.0919	0.102	0.0978	0.124
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B						
RD Estimate	-1.164***	-1.195**	-0.989**	-1.054**	-0.912**	-1.016**
	(0.425)	(0.511)	(0.410)	(0.514)	(0.384)	(0.512)
Observations	194	194	178	178	178	178
Effective observations	[48,37]	[62,49]	[46,34]	[56,43]	[53,43]	[59,46]
Covariates	None	None	Some	Some	All	All
p-value	0.00611	0.0194	0.0158	0.0401	0.0176	0.0474
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0742	0.105	0.0810	0.103	0.0996	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.

G.2. When Extreme Poverty Decreases

Table G3: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.503*	-2.201**	-1.291	-1.735	-1.323	-2.303*
	(0.773)	(1.019)	(0.847)	(1.119)	(0.940)	(1.238)
Observations	432	432	387	387	387	387
Effective observations	[117,108]	[133,119]	[102,96]	[116,107]	[84,86]	[94,90]
Covariates	None	None	Some	Some	All	All
p-value	0.0519	0.0309	0.128	0.121	0.159	0.0628
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0937	0.105	0.0924	0.106	0.0768	0.0886
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-2.564	-3.693*	-2.765	-3.558	-3.696*	-4.846**
	(1.614)	(2.144)	(1.794)	(2.261)	(1.950)	(2.424)
Observations	216	216	193	193	193	193
Effective observations	[59,54]	[67,60]	[44,44]	[58,54]	[41,43]	[53,53]
Covariates	None	None	Some	Some	All	All
p-value	0.112	0.0849	0.123	0.116	0.0580	0.0456
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0932	0.105	0.0851	0.105	0.0754	0.0987

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.913**	-1.050**	-0.583*	-0.778*	-0.549	-0.705*
	(0.376)	(0.435)	(0.341)	(0.453)	(0.337)	(0.419)
Observations	431	431	386	386	386	386
Effective observations	[89,86]	[121,118]	[104,100]	[116,107]	[114,107]	[138,135]
Covariates	None	None	Some	Some	All	All
p-value	0.0152	0.0157	0.0871	0.0861	0.104	0.0926
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0691	0.0994	0.0953	0.110	0.104	0.148
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.846**	-0.925*	-0.551	-0.748	-0.560	-0.807
	(0.405)	(0.472)	(0.385)	(0.505)	(0.347)	(0.505)
Observations	216	216	193	193	193	193
Effective observations	[47,44]	[65,60]	[50,48]	[58,54]	[58,54]	[60,55]
Covariates	None	None	Some	Some	All	All
p-value	0.0368	0.0501	0.152	0.138	0.106	0.110
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0702	0.104	0.0907	0.108	0.111	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.

H. Final Year in Electoral Term

H.1. When Poverty Decreases

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.351**	-2.999**	-1.705	-1.810	-1.713	-1.827
	(1.011)	(1.439)	(1.094)	(1.530)	(1.189)	(1.596)
Observations	195	195	179	179	179	179
Effective observations	[67,53]	[65,52]	[52,42]	[57,44]	[52,40]	[57,44]
Covariates	None	None	Some	Some	All	All
p-value	0.0200	0.0372	0.119	0.237	0.150	0.253
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.126	0.122	0.0966	0.109	0.0945	0.110
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-2.359**	-3.003**	-1.733	-1.839	-1.447	-1.393
	(1.012)	(1.437)	(1.098)	(1.525)	(1.322)	(1.631)
Observations	194	194	178	178	178	178
Effective observations	[67,53]	[65,52]	[52,40]	[57,44]	[45,34]	[57,44]
Covariates	None	None	Some	Some	All	All
p-value	0.0197	0.0367	0.115	0.228	0.273	0.393
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.125	0.122	0.0957	0.110	0.0792	0.109

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A provides estimates by year, and Panel B provides estimates 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 H2: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.147**	-1.119*	-0.894**	-1.010*	-0.902**	-1.048*
	(0.460)	(0.603)	(0.438)	(0.603)	(0.411)	(0.576)
Observations	194	194	178	178	178	178
Effectiveness observations	[49,39]	[56,45]	[51,38]	[53,42]	[56,44]	[53,43]
Covariates	None	None	Some	Some	All	All
p-value	0.0127	0.0638	0.0411	0.0938	0.0283	0.0687
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0776	0.0952	0.0908	0.0974	0.104	0.0983
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.147**	-1.119*	-0.894**	-1.010*	-0.821*	-0.974*
	(0.460)	(0.603)	(0.438)	(0.603)	(0.425)	(0.591)
Observations	194	194	178	178	178	178
Effective observations	[49,39]	[56,45]	[51,38]	[53,42]	[56,44]	[55,43]
Covariates	None	None	Some	Some	All	All
p-value	0.0127	0.0638	0.0412	0.0938	0.0533	0.0995
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0776	0.0952	0.0907	0.0975	0.103	0.101

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 Decreases

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.990*	-1.942	-1.441	-1.560	-1.356	-1.643
	(1.029)	(1.228)	(1.053)	(1.531)	(1.158)	(1.545)
Observations	217	217	194	194	194	194
Effective observations	[56,51]	[78,69]	[58,54]	[58,54]	[44,44]	[58,54]
Covariates	None	None	Some	Some	All	All
p-value	0.0533	0.114	0.171	0.308	0.242	0.288
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0897	0.141	0.105	0.111	0.0863	0.108
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.991*	-1.917	-1.446	-1.567	-1.839	-1.919
	(1.029)	(1.220)	(1.055)	(1.529)	(1.231)	(1.594)
Observations	216	216	193	193	193	193
Effective observations	[56,52]	[79,71]	[58,54]	[58,55]	[43,44]	[59,55]
Covariates	None	None	Some	Some	All	All
p-value	0.0531	0.116	0.170	0.305	0.135	0.229
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0898	0.142	0.105	0.111	0.0835	0.113

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.863**	-0.936*	-0.453	-0.675	-0.309	-0.641
	(0.437)	(0.546)	(0.436)	(0.579)	(0.383)	(0.573)
Observations	216	216	193	193	193	193
Effective observations	[49,47]	[60,55]	[46,45]	[56,53]	[58,54]	[57,54]
Covariates	None	None	Some	Some	All	All
p-value	0.0484	0.0861	0.299	0.244	0.420	0.264
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0727	0.0944	0.0883	0.103	0.109	0.104
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.863**	-0.936*	-0.452	-0.675	-0.439	-0.748
	(0.437)	(0.546)	(0.435)	(0.579)	(0.391)	(0.570)
Observations	216	216	193	193	193	193
Effective observations	[49,47]	[60,55]	[46,45]	[56,53]	[58,54]	[58,54]
Covariates	None	None	Some	Some	All	All
p-value	0.0484	0.0861	0.299	0.244	0.262	0.189
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0727	0.0944	0.0884	0.103	0.108	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 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. First Two Years

I.1. When Poverty Decreases

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.970	-0.958	-0.195	-0.195	-0.606	-1.147
	(0.752)	(1.023)	(0.823)	(1.022)	(0.913)	(1.204)
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	0.507	0.341
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	-1.372	-2.357
	(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
p-value	0.189	0.352	0.686	0.843	0.432	0.337
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 I2: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.147	0.288	0.300	0.380	0.167	0.131
	(0.307)	(0.354)	(0.299)	(0.346)	(0.361)	(0.416)
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	0.633	0.416	0.316	0.273	0.645	0.752
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0609	0.0866	0.0601	0.0864	0.0593	0.0860
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.0210	0.167	0.139	0.183	0.0848	-0.0701
	(0.391)	(0.448)	(0.375)	(0.436)	(0.479)	(0.557)
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
p-value	0.957	0.709	0.711	0.676	0.859	0.900
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0612	0.0893	0.0611	0.0910	0.0578	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.

I.2. When Extreme Poverty Decreases

Table I3: 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	-0.898	-1.336
	(0.770)	(0.971)	(0.807)	(0.997)	(0.835)	(1.049)
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	0.282	0.203
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	-1.923	-2.816
	(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
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 I4: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.172	0.264	0.166	0.271	0.230	0.114
	(0.351)	(0.393)	(0.341)	(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	0.624	0.502	0.627	0.495	0.516	0.786
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0649	0.0915	0.0716	0.0869	0.0696	0.0868
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.0279	0.167	-0.137	0.145	0.00202	-0.00694
	(0.385)	(0.472)	(0.360)	(0.473)	(0.387)	(0.524)
Observations	118	118	118	118	118	118
Effective observations	[28,22]	[35,24]	[31,22]	[35,24]	[28,22]	[33,23]
Covariates	None	None	Some	Some	All	All
p-value	0.942	0.723	0.704	0.759	0.996	0.989
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0766	0.0935	0.0880	0.0922	0.0779	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. Results for the Whole Sample (i.e. When Poverty Is Not Considered)

Table J1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0621	-0.489	0.163	0.0966	-0.133	-0.0911
	(0.602)	(0.893)	(0.541)	(0.853)	(0.644)	(0.789)
Observations	1,357	1,357	1,275	1,275	1,151	1,151
Effective observations	[429,407]	[467,461]	[464,477]	[464,479]	[340,331]	[453,479]
Covariates	None	None	Some	Some	All	All
p-value	0.918	0.584	0.763	0.910	0.836	0.908
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.124	0.147	0.163	0.165	0.117	0.187
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.0444	-0.170	0.419	0.362	0.0352	0.198
	(0.632)	(0.874)	(0.593)	(0.724)	(0.665)	(0.805)
Observations	1,357	1,357	1,275	1,275	1,151	1,151
Effective observations	[415,371]	[467,467]	[420,403]	[532,551]	[327,314]	[435,475]
Covariates	None	None	Some	Some	All	All
p-value	0.944	0.846	0.480	0.617	0.958	0.806
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.116	0.152	0.134	0.212	0.111	0.181

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 J2: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.133	-4.067	-2.852	-4.456	-5.290*	-4.460
	(2.677)	(3.932)	(2.841)	(4.019)	(3.171)	(4.021)
Observations	440	440	398	398	372	372
Effective observations	[133,120]	[148,141]	[117,108]	[134,133]	[99,93]	[123,127]
Covariates	None	None	Some	Some	All	All
p-value	0.426	0.301	0.315	0.268	0.0953	0.267
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.112	0.135	0.108	0.140	0.0926	0.140
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.258	-0.278	1.231	1.418	-0.728	-0.0200
	(1.974)	(2.718)	(2.012)	(2.527)	(2.242)	(2.994)
Observations	440	440	398	398	372	372
Effective observations	[133,120]	[153,154]	[124,118]	[156,164]	[101,101]	[124,132]
Covariates	None	None	Some	Some	All	All
p-value	0.896	0.919	0.541	0.575	0.746	0.995
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.113	0.150	0.120	0.182	0.0999	0.144

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 J3: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.172	-0.336	-0.0728	-0.168	-0.0966	-0.213
	(0.218)	(0.292)	(0.211)	(0.288)	(0.213)	(0.304)
Observations	1,352	1,352	1,270	1,270	1,146	1,146
Effective observations	[394,353]	[459,431]	[389,361]	[432,445]	[334,322]	[375,389]
Covariates	None	None	Some	Some	All	All
p-value	0.429	0.249	0.730	0.560	0.651	0.484
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.106	0.141	0.118	0.152	0.115	0.142
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.138	-0.278	-0.0338	-0.0161	-0.0416	-0.0985
	(0.221)	(0.290)	(0.214)	(0.261)	(0.209)	(0.292)
Observations	1,352	1,352	1,270	1,270	1,146	1,146
Effective observations	[386,352]	[461,439]	[373,349]	[502,509]	[339,331]	[379,407]
Covariates	None	None	Some	Some	All	All
p-value	0.531	0.339	0.874	0.951	0.842	0.736
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.142	0.114	0.182	0.116	0.149

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

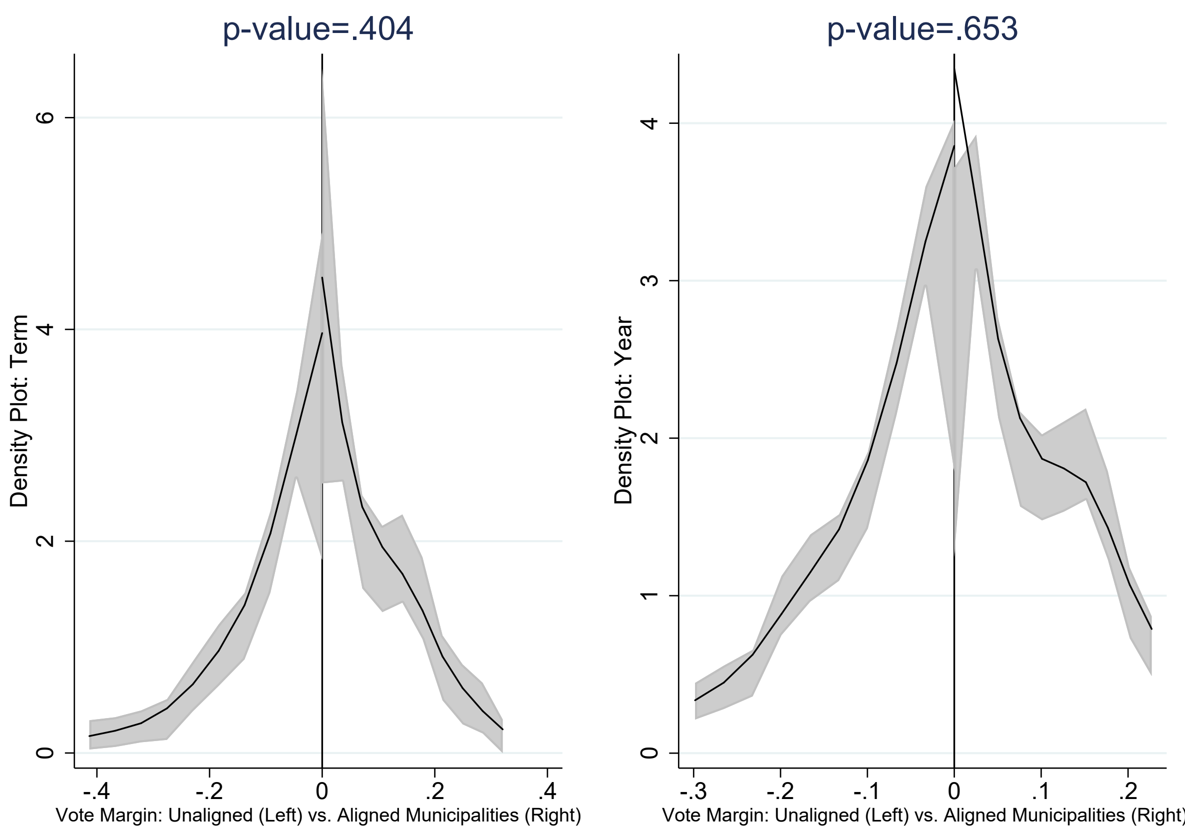
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0559	-0.377	0.0278	-0.286	-0.198	-0.215
	(0.248)	(0.405)	(0.244)	(0.411)	(0.286)	(0.398)
Observations	440	440	398	398	372	372
Effective observations	[148,142]	[146,136]	[136,142]	[132,132]	[108,103]	[124,132]
Covariates	None	None	Some	Some	All	All
p-value	0.822	0.351	0.909	0.487	0.489	0.588
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.136	0.130	0.149	0.135	0.109	0.145
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.0186	-0.201	0.162	0.0562	0.0677	0.0596
	(0.253)	(0.367)	(0.244)	(0.353)	(0.257)	(0.359)
Observations	440	440	398	398	372	372
Effective observations	[132,120]	[146,136]	[126,124]	[136,142]	[109,107]	[125,135]
Covariates	None	None	Some	Some	All	All
p-value	0.942	0.585	0.508	0.874	0.793	0.868
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.111	0.130	0.124	0.148	0.113	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. RDD Robustness Checks

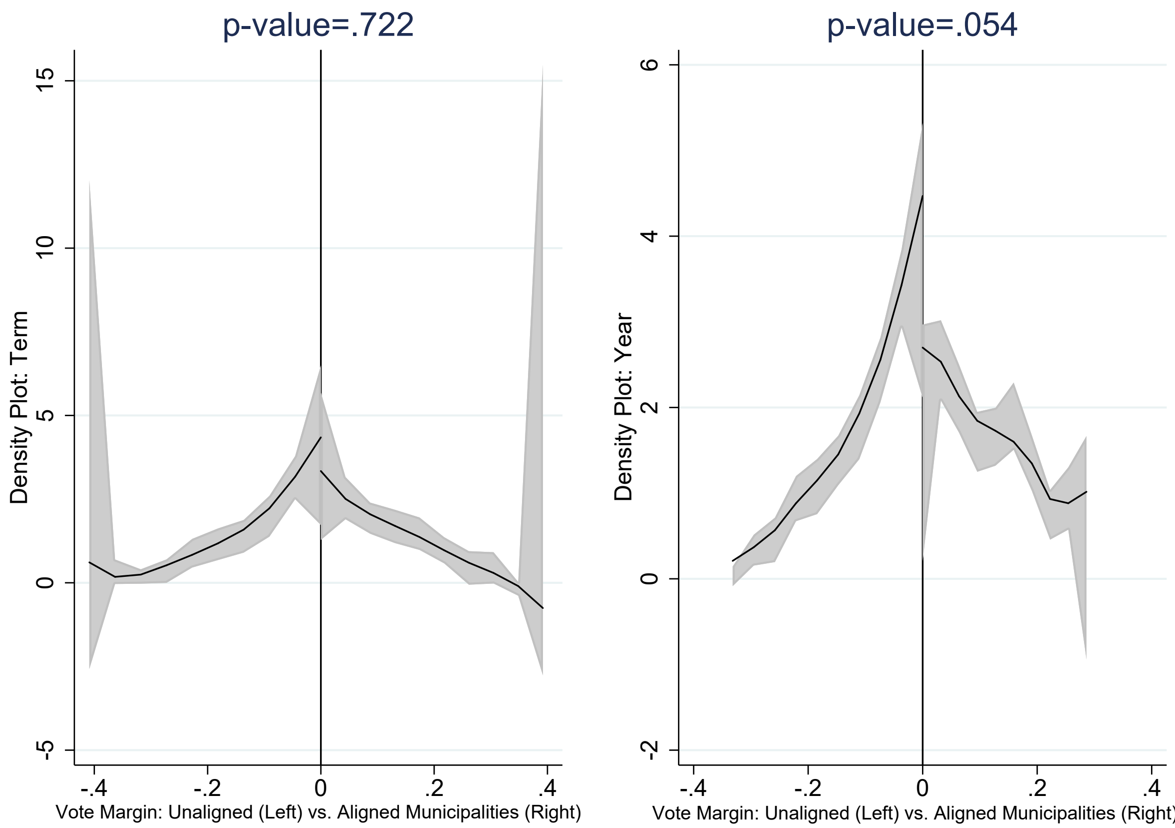
K.1. Density Plots for 2010-2015: Year and Term

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



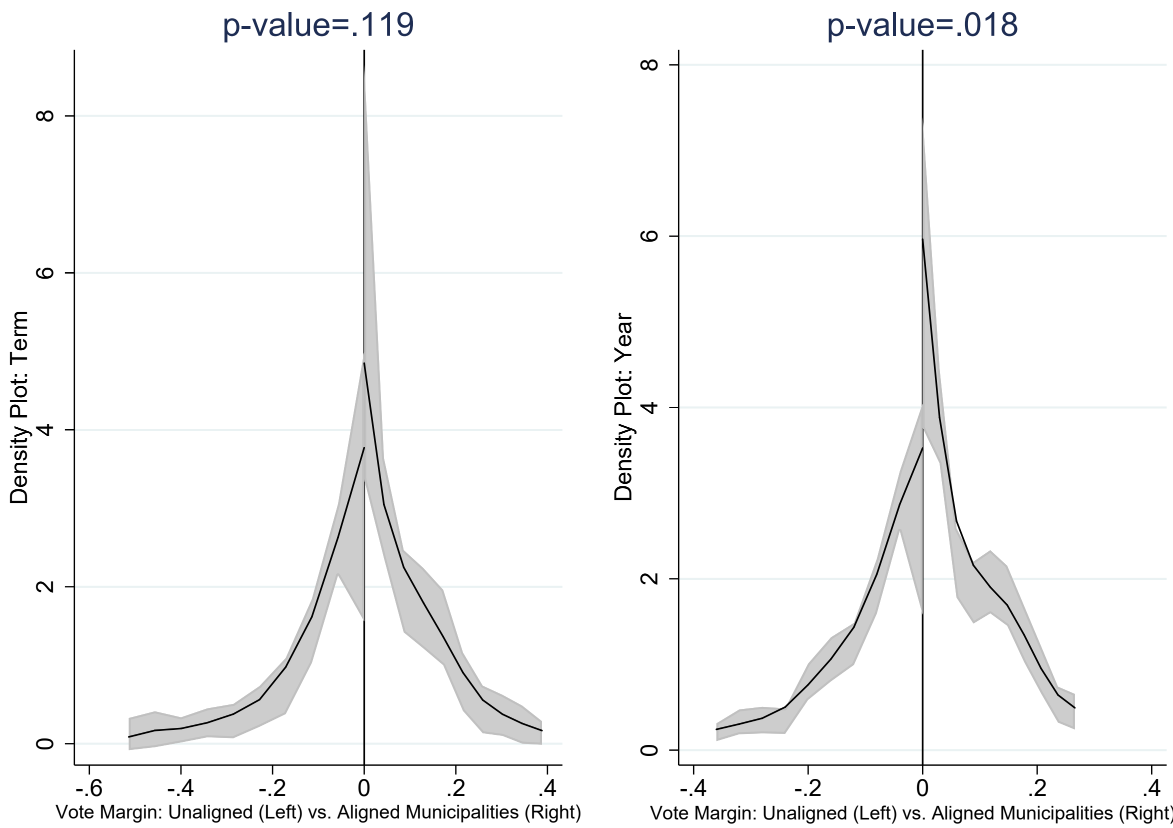
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither 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 K.2: 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.

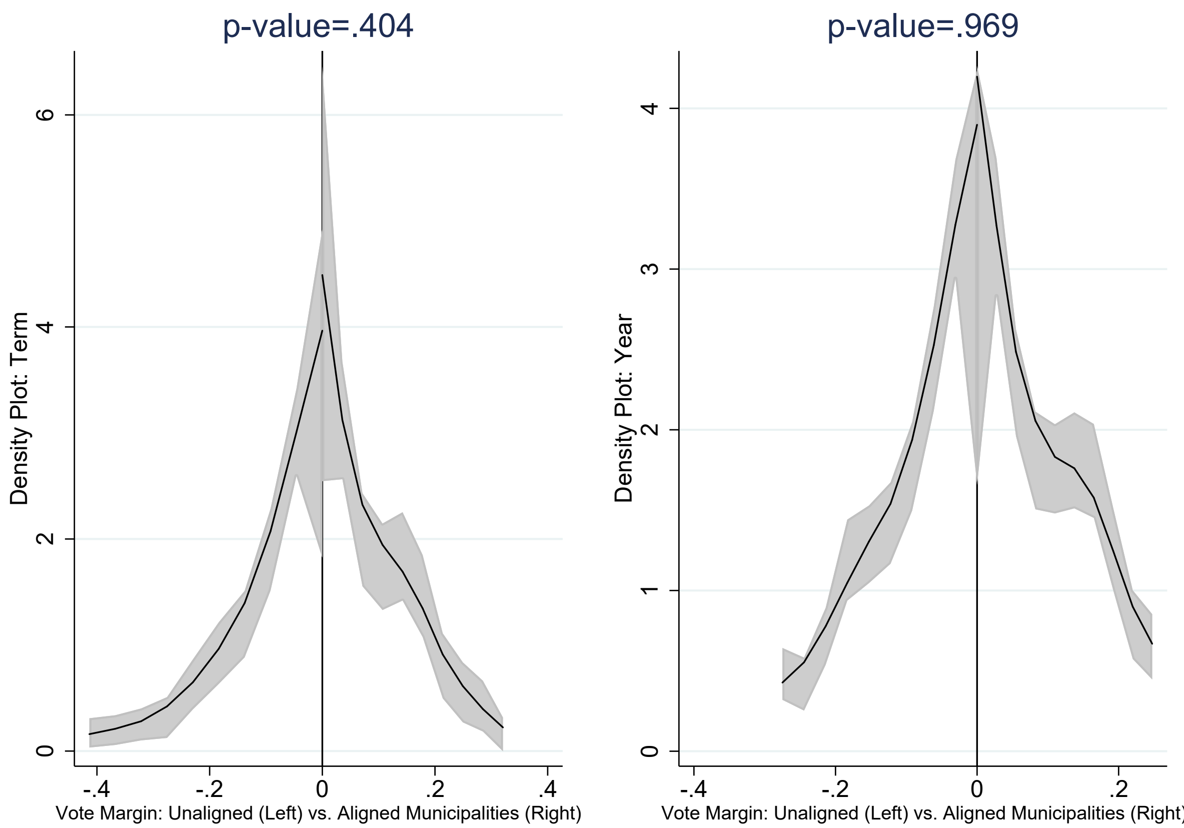
Figure K.3: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



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.

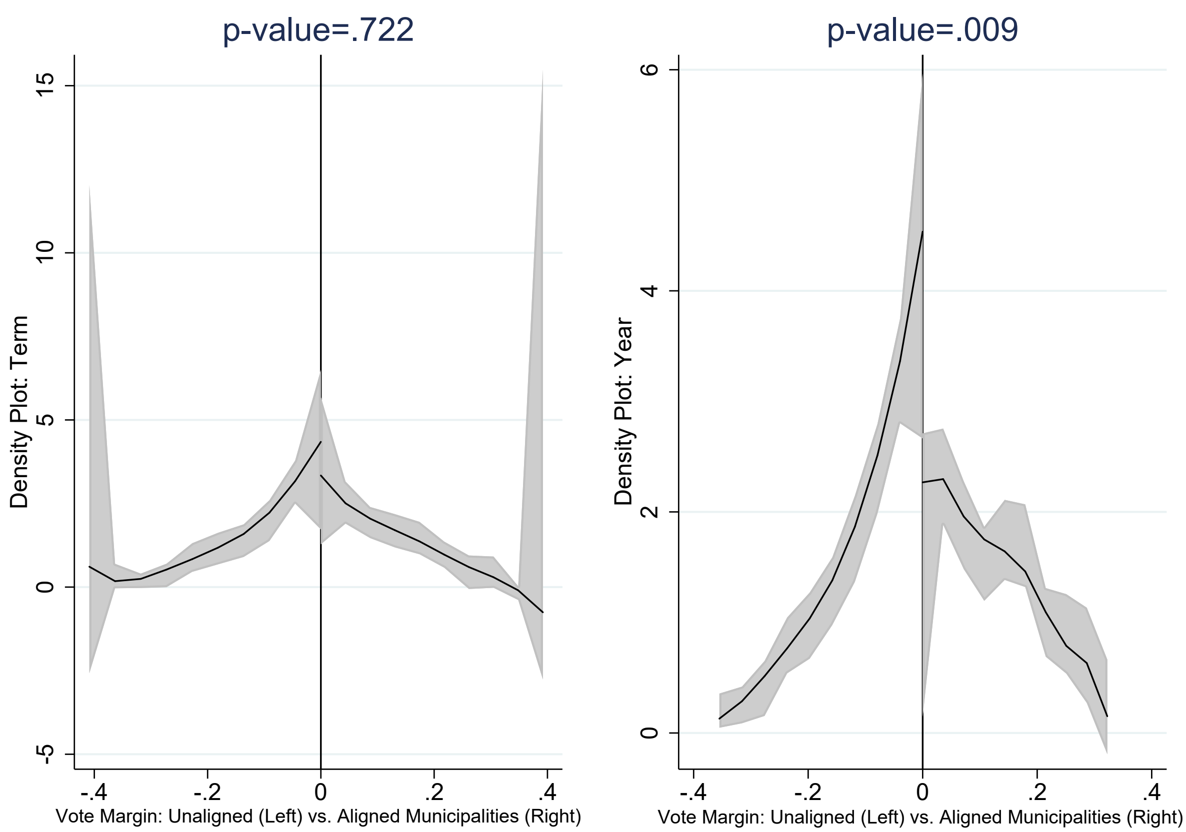
K.2. Density Plots for 2011-2015: Year and Term

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



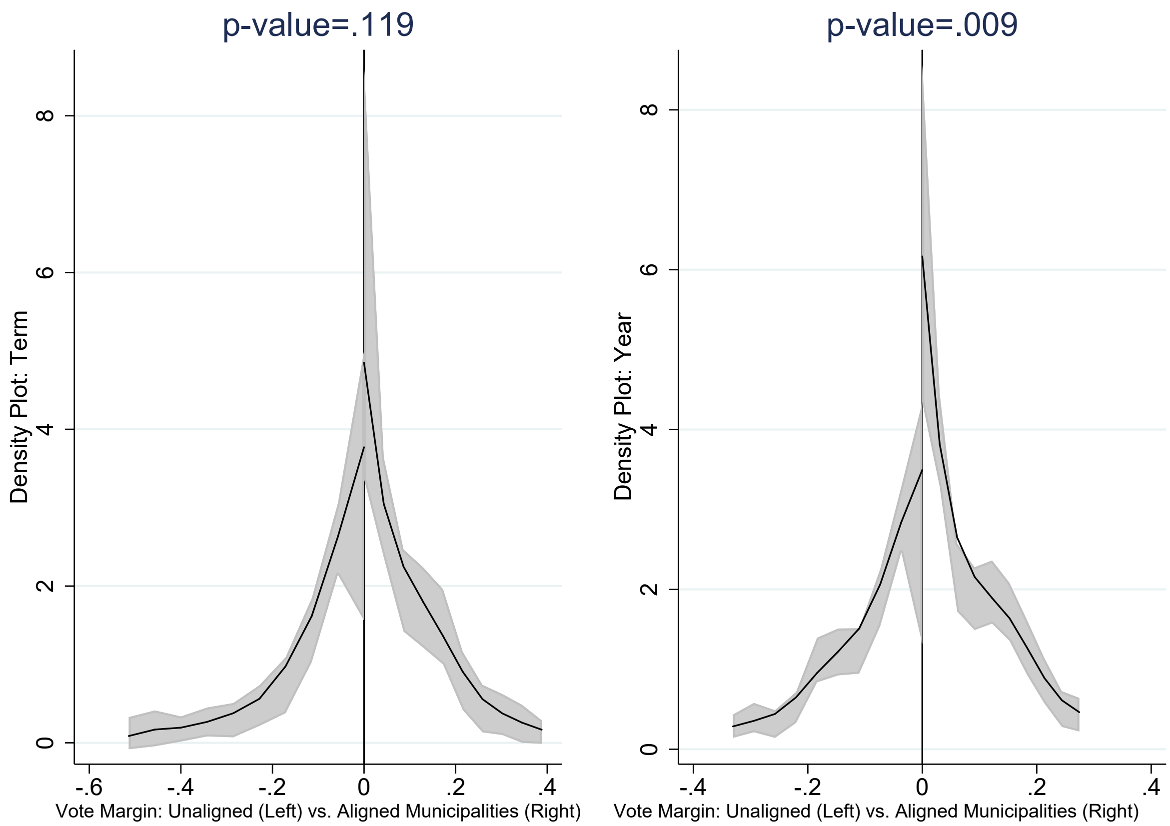
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither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

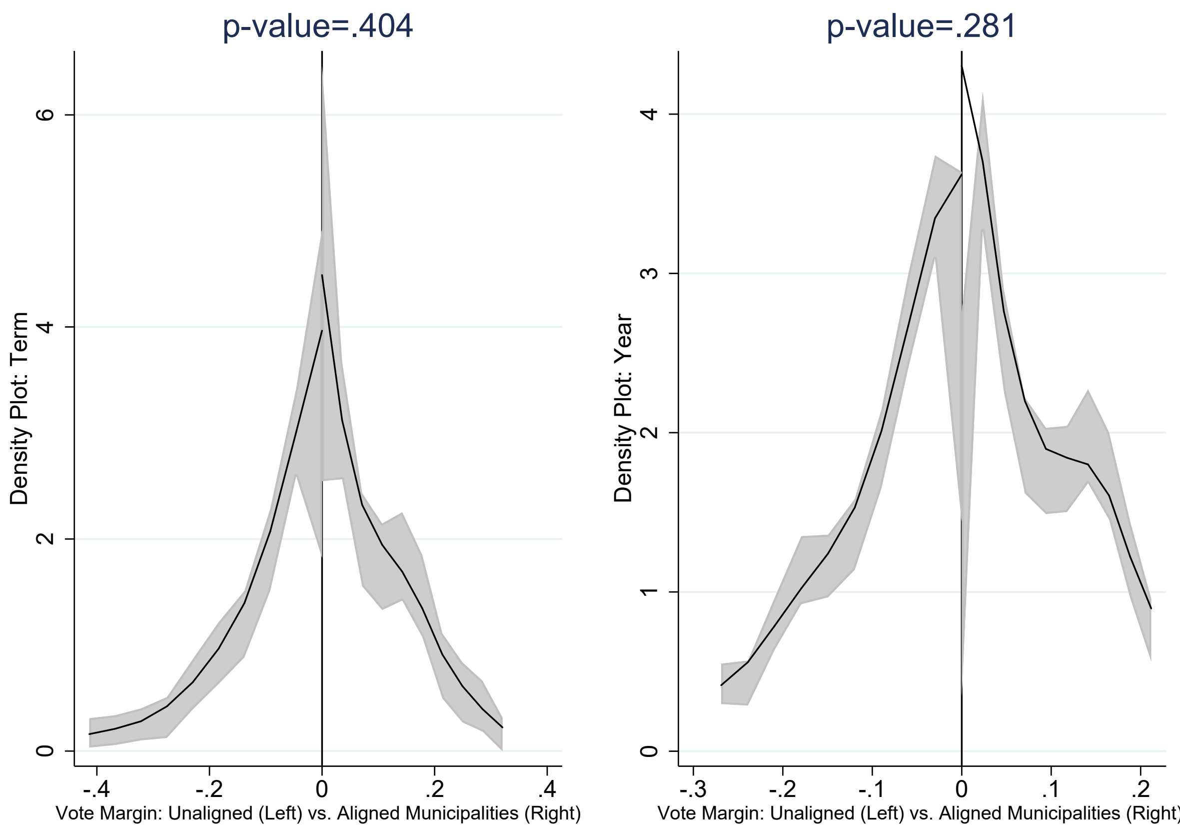
Figure K.6: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



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. 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.

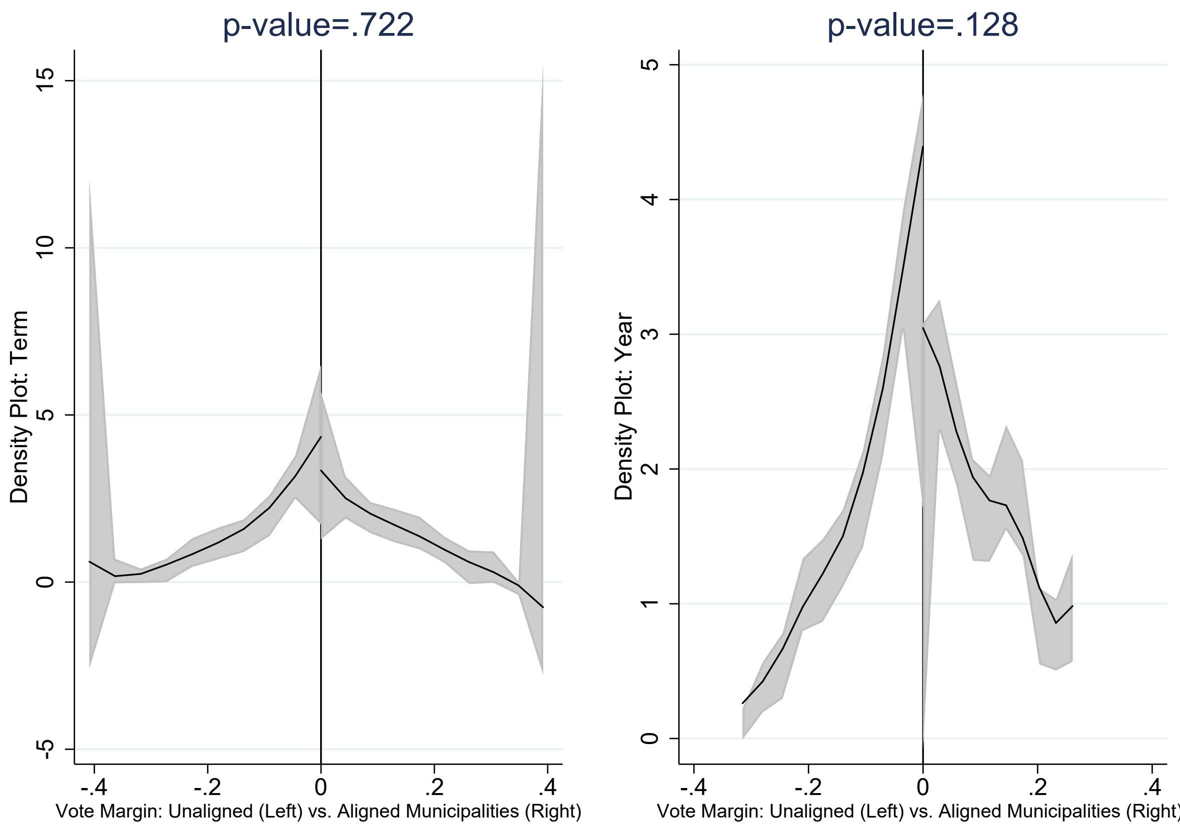
K.3. Density Plots for 2009-2015: Year and Term

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



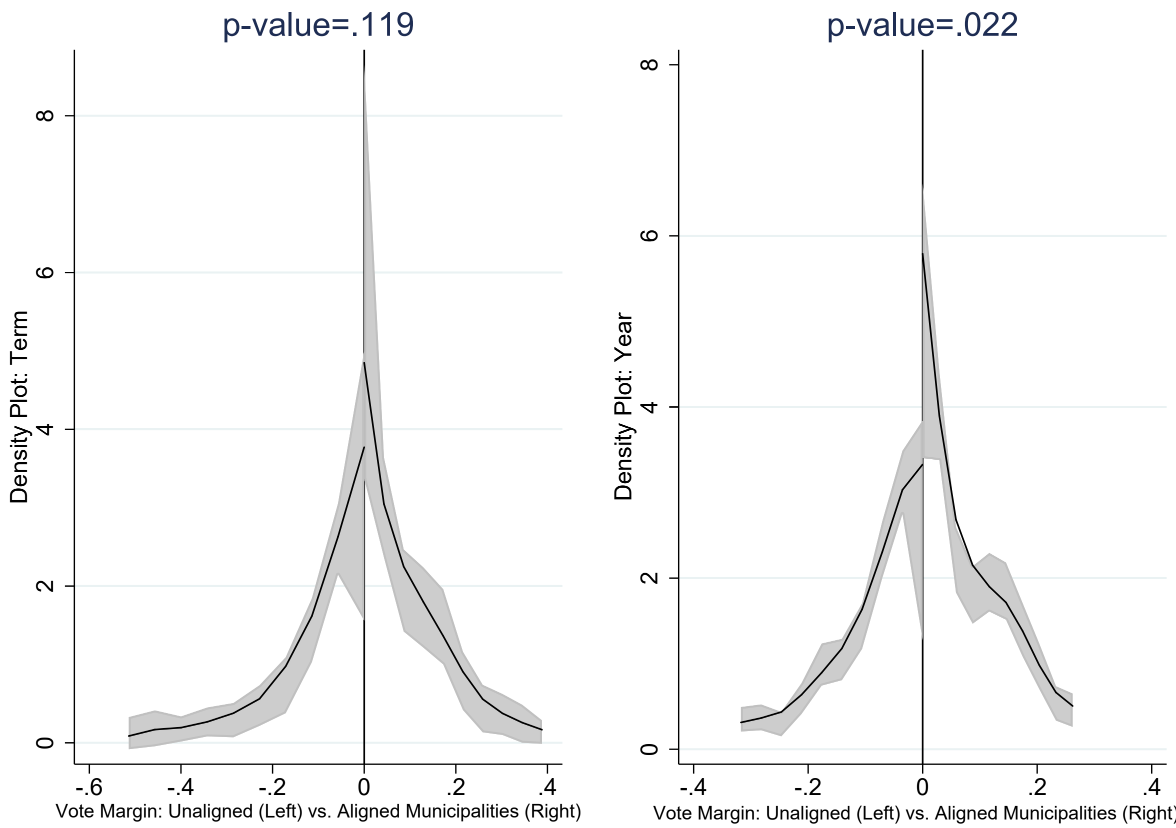
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither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

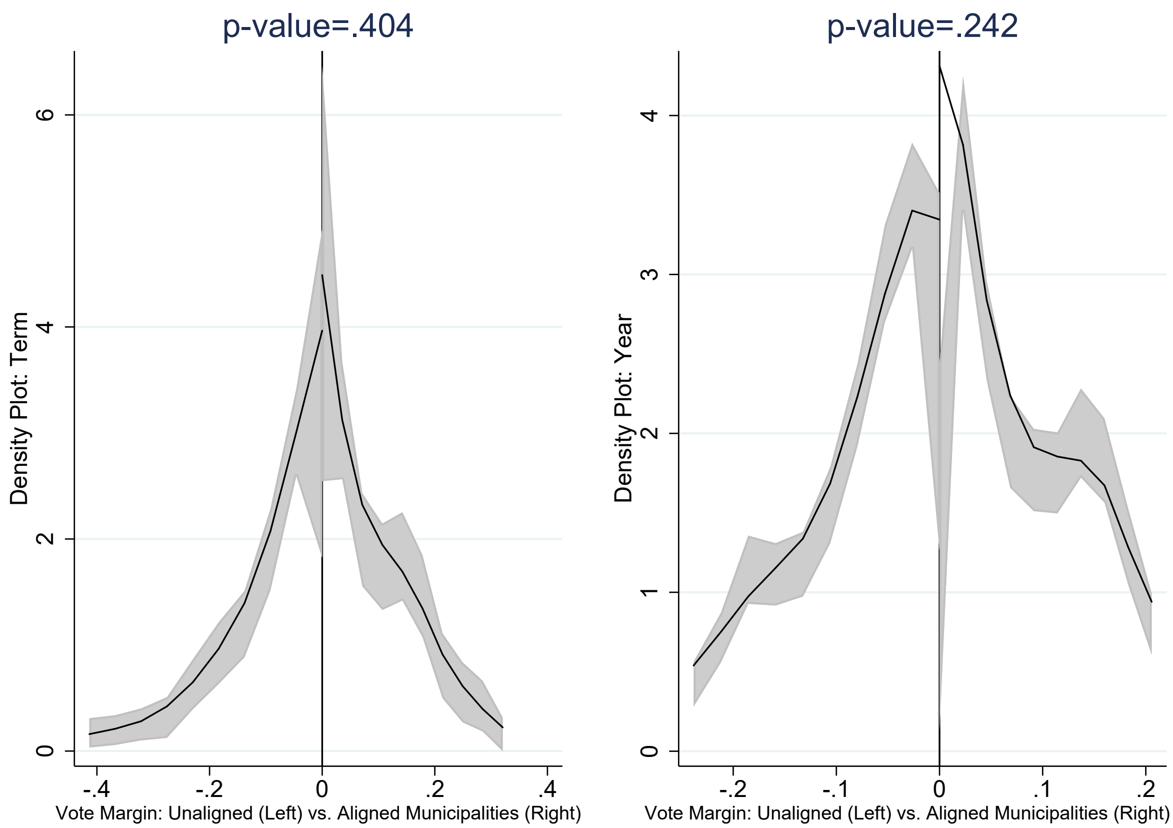
Figure K.9: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



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 confidence intervals (shaded gray areas) on both sides of the cutoff—i.e., with the cutoff being the margin of victory is zero.

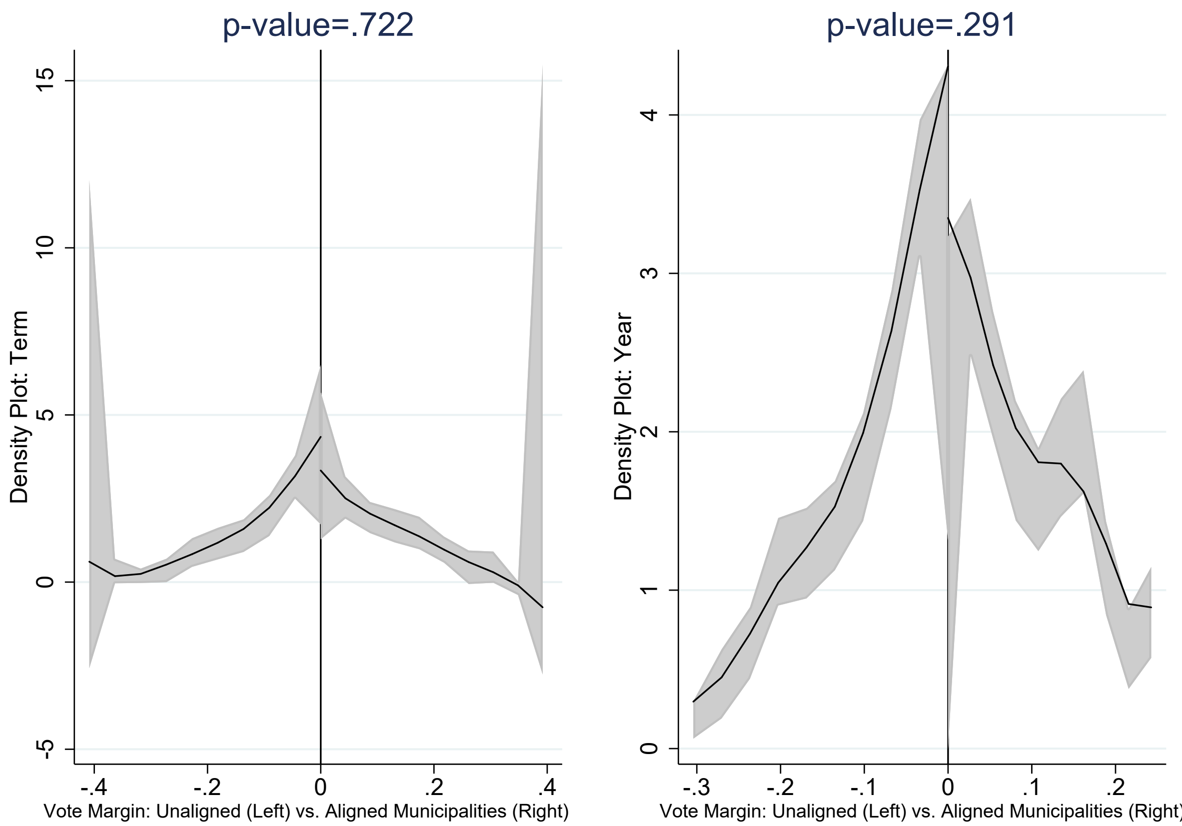
K.4. Density Plots for 2008-2015: Year and Term

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



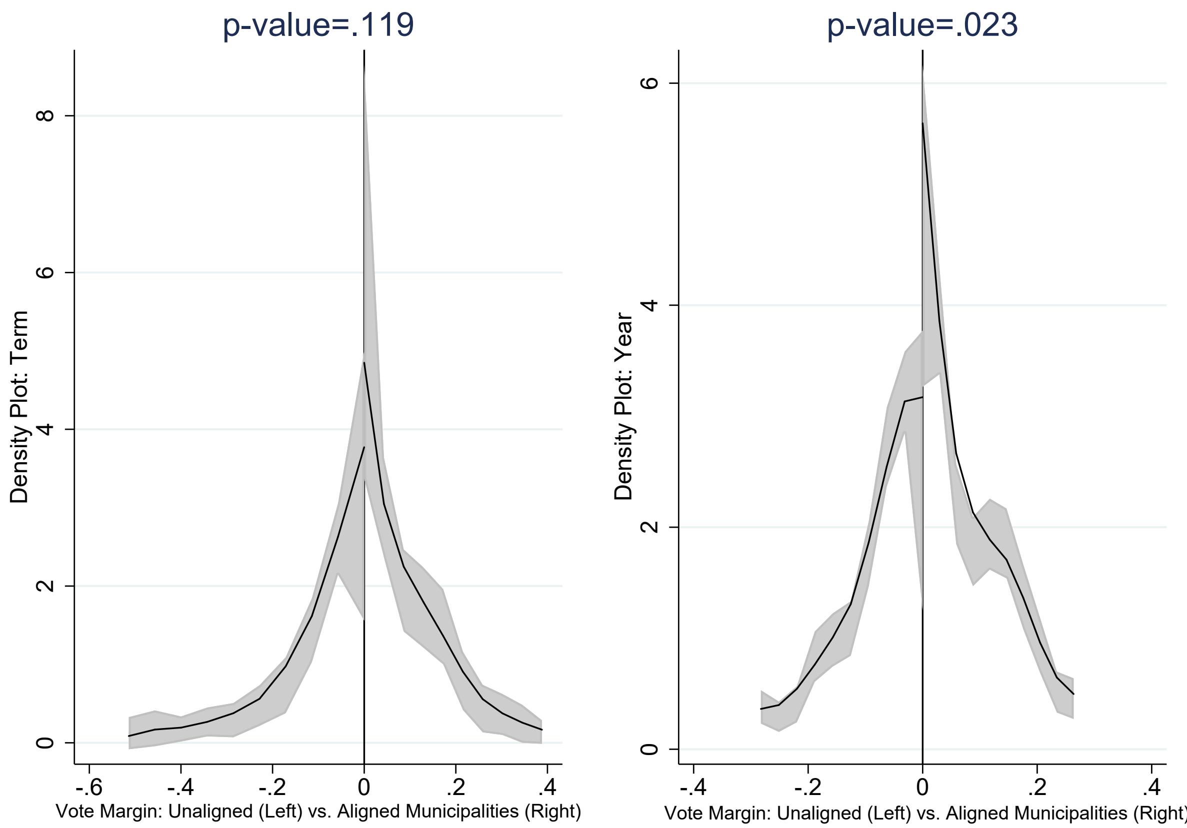
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither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

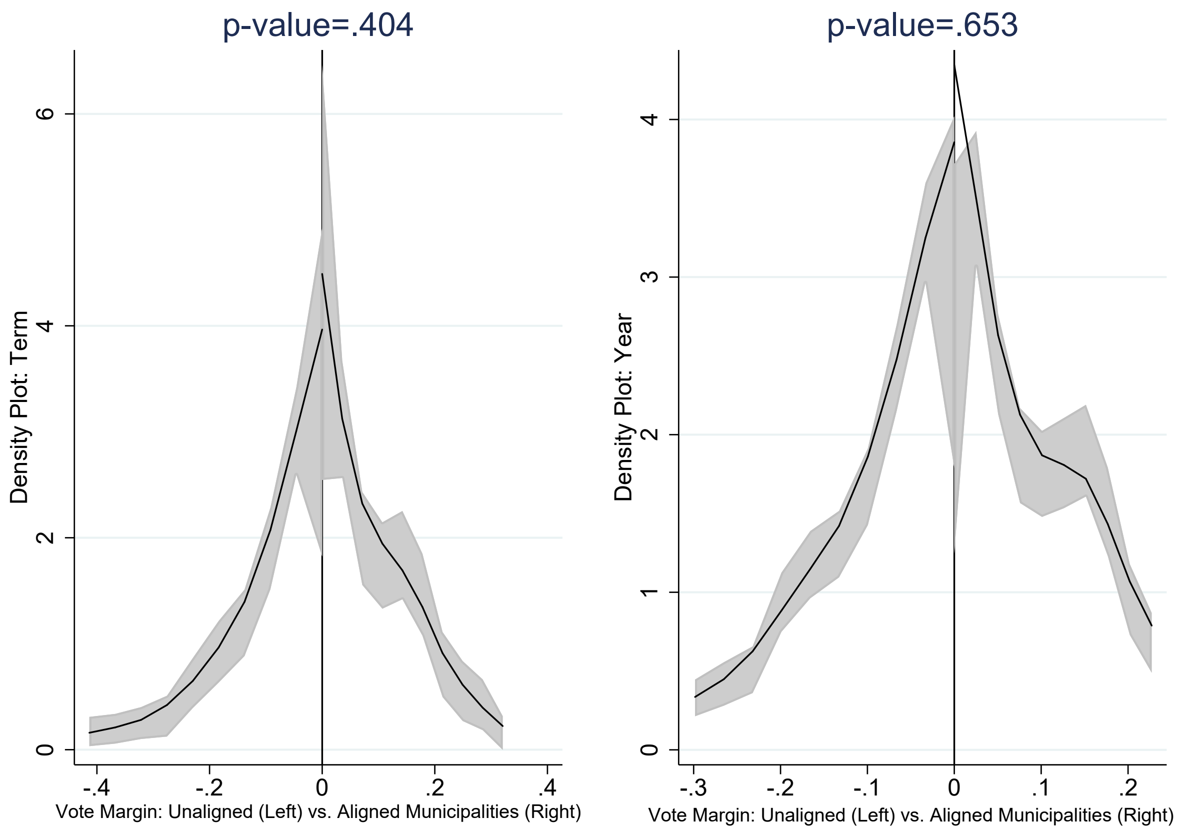
Figure K.12: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)



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 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.

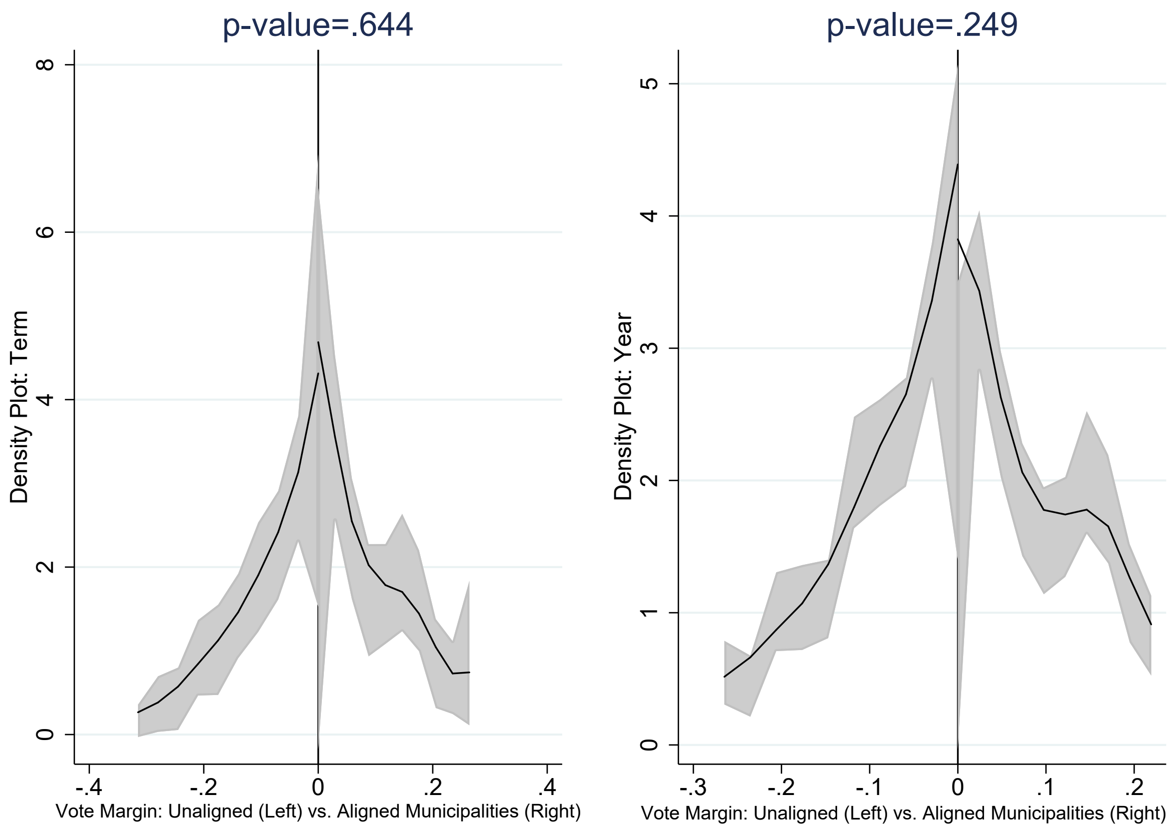
K.5. Extreme Poverty Density Plots for 2010-2015: Year and Term

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



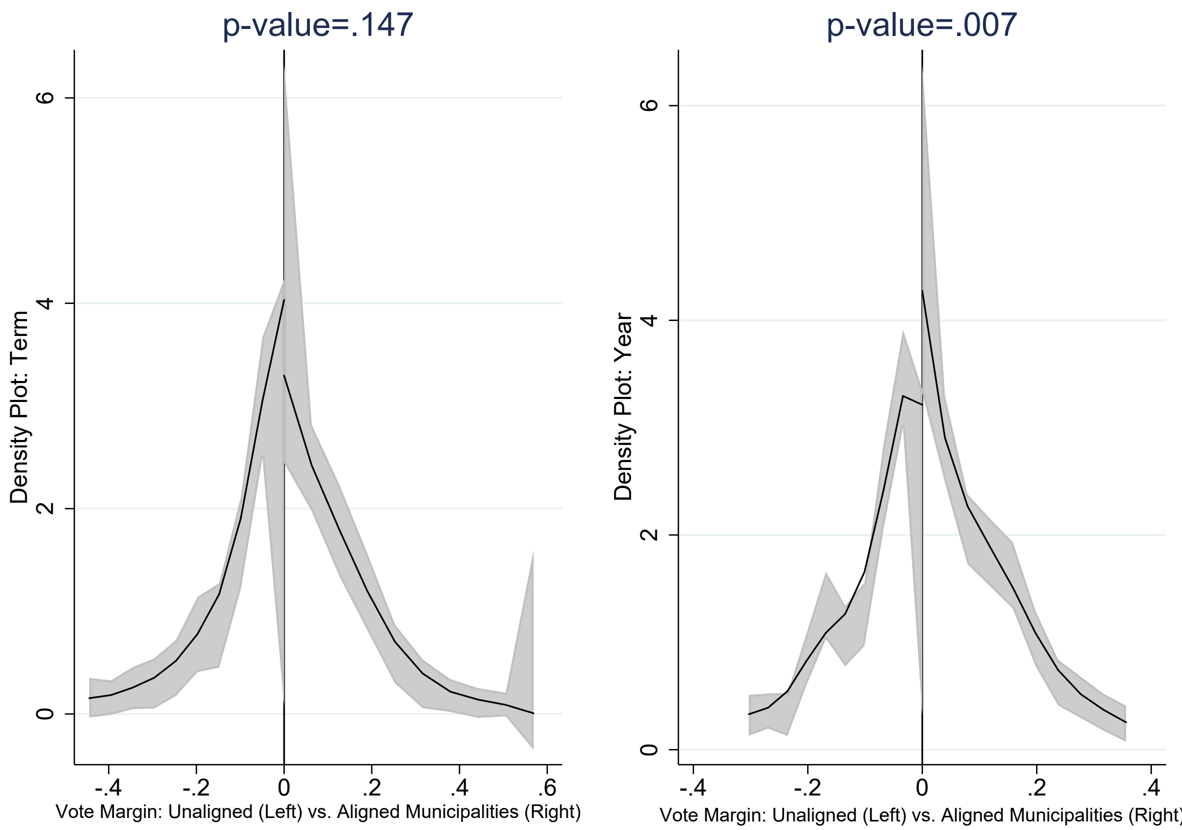
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. Neither 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 K.14: 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 K.15: 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.

K.6. RDD Estimates Eliminating Outliers

Table K1: RDD Estimates for Infraction Count by Term and Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-6.603**	-11.18***	-6.364**	-8.121**	-6.545**	-7.549**
	(2.624)	(3.697)	(2.962)	(3.594)	(3.094)	(3.831)
Observations	182	182	167	167	167	167
Effective Observations	[65,54]	[57,50]	[46,38]	[59,48]	[39,34]	[51,43]
Covariates	None	None	Some	Some	All	All
p-value	0.0118	0.00250	0.0317	0.0238	0.0344	0.0488
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.138	0.118	0.0911	0.137	0.0753	0.111
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.580***	-2.074***	-0.973*	-1.303*	-1.337**	-1.964***
	(0.509)	(0.696)	(0.571)	(0.715)	(0.599)	(0.718)
Observations	591	591	559	559	559	559
Effective observations	[198,143]	[195,139]	[148,104]	[179,128]	[142,98]	[156,106]
Covariates	None	None	Some	Some	All	All
p-value	0.00189	0.00289	0.0886	0.0686	0.0257	0.00623
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.112	0.107	0.0841	0.102	0.0744	0.0882

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.To avoid excessive ommissions, Term results are winsorized at top/bottom 10% level, while Year results are winsorized at top/bottom 5%. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that Calonico, Cattaneo and Titiunik's (2014) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per Gelman and Imbens (2019), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table K2: RDD Estimates for Infraction Amount (log) by Term and Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.832***	-0.809**	-0.721***	-0.675*	-0.677***	-0.671**
	(0.310)	(0.390)	(0.234)	(0.367)	(0.237)	(0.333)
Observations	177	177	163	163	163	163
Effective Observations	[44,35]	[57,45]	[53,40]	[47,34]	[53,40]	[46,34]
Covariates	None	None	Some	Some	All	All
p-value	0.00729	0.0384	0.00207	0.0660	0.00424	0.0442
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0762	0.105	0.109	0.0902	0.106	0.0901
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.618***	-0.474	-0.396	-0.361	-0.505*	-0.432
	(0.220)	(0.310)	(0.262)	(0.308)	(0.267)	(0.321)
Observations	585	585	555	555	555	555
Effective observations	[188,136]	[178,134]	[130,76]	[172,124]	[138,84]	[186,131]
Covariates	None	None	Some	Some	All	All
p-value	0.00503	0.127	0.130	0.241	0.0589	0.178
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.0974	0.0663	0.0970	0.0706	0.111

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.To avoid excessive ommissions, Term results are winsorized at top/bottom 10% level, while Year results are winsorized at top/bottom 5%. 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.7. RDD Estimates at Varying Cutoffs (Placebo Tests)

Table K3: RDD Estimates for Infraction Count and Amount (log) by Term

Panel A	(-5%)	(5%)	(-10%)	(10%)	(-15%)	(15%)
RD Estimate	1.627	-0.156	-1.364	-11.33***	2.513	4.428
	(4.443)	(4.276)	(4.809)	(3.959)	(5.739)	(4.614)
Observations	195	195	195	195	195	195
Effective Observations	[36,66]	[44,24]	[30,58]	[28,22]	[17,24]	[23,16]
p-value	0.714	0.971	0.777	0.00423	0.661	0.337
Order of polynomial	1	1	1	1	1	1
Bandwidth	0.102	0.0745	0.103	0.0627	0.0716	0.0633
Panel B	**(-5%)**	**(5%)**	**(-10%)**	**(10%)**	**(-15%)**	**(15%)**
RD Estimate	0.571	-0.578	-0.879*	-0.819	-0.656	0.658
	(0.420)	(0.448)	(0.510)	(0.578)	(1.044)	(0.638)
Observations	195	195	195	195	195	195
Effective Observations	[36,62]	[27,14]	[29,49]	[34,25]	[16,19]	[14,15]
p-value	0.174	0.197	0.0849	0.157	0.530	0.302
Order of polynomial	1	1	1	1	1	1
Bandwidth	0.0957	0.0427	0.0895	0.0735	0.0608	0.0465

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.

K.8. RDD Estimates for Number of Audits in a Term

Table K4: 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 K5: 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 K6: 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. Potential Endogeneity between Poverty and Corruption

L.1. Regression of Poverty Rate on Corruption

Table L1: 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 L2: 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 L3: 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 L4: 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.

L.2. Two-Stage Regression of Residuals on Corruption

Table L5: 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 L6: Term-wise Regression of Residuals on Log Amount of Infraction

	(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 L7: 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 L8: 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. Results for 2011-2015

M.1. Results When Poverty Decreases

Table M1: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.416**	-1.534**	-0.521	-0.698	-0.754	-1.282
	(0.607)	(0.773)	(0.605)	(0.798)	(0.631)	(0.826)
Observations	513	513	497	497	497	497
Effective observations	[167,111]	[173,112]	[151,92]	[151,97]	[133,86]	[141,87]
Covariates	None	None	Some	Some	All	All
p-value	0.0196	0.0472	0.389	0.382	0.232	0.121
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.110	0.0913	0.0956	0.0847	0.0872
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.117*	-1.227	-0.315	-0.483	-0.558	-1.155
	(0.655)	(0.845)	(0.662)	(0.837)	(0.702)	(0.876)
Observations	513	513	497	497	497	497
Effective observations	[159,111]	[177,116]	[137,86]	[155,106]	[133,86]	[143,87]
Covariates	None	None	Some	Some	All	All
p-value	0.0880	0.147	0.634	0.564	0.427	0.187
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0984	0.112	0.0858	0.0982	0.0832	0.0889

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 M2: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-12.92***	-13.83***	-9.753**	-12.02**	-5.808*	-6.697*
	(3.888)	(4.346)	(4.428)	(5.515)	(3.256)	(3.805)
Observations	195	195	179	179	179	179
Effective observations	[56,43]	[74,70]	[45,34]	[59,46]	[45,34]	[57,44]
Covariates	None	None	Some	Some	All	All
p-value	0.000889	0.00146	0.0276	0.0294	0.0744	0.0784
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0930	0.160	0.0784	0.115	0.0761	0.109

Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.910	-3.715	-0.811	-1.882	-1.178	-3.209
	(2.013)	(2.747)	(2.206)	(2.933)	(2.317)	(3.082)
Observations	195	195	179	179	179	179
Effective observations	[56,43]	[62,49]	[46,35]	[55,43]	[45,34]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.148	0.176	0.713	0.521	0.611	0.298
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0929	0.110	0.0839	0.101	0.0804	0.0934

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 M3: 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
	(0.246)	(0.330)	(0.269)	(0.334)	(0.275)	(0.340)
Observations	510	510	494	494	494	494
Effective observations	[167,111]	[159,110]	[129,85]	[163,106]	[129,85]	[176,112]
Covariates	None	None	Some	Some	All	All
p-value	0.00894	0.151	0.0842	0.247	0.0554	0.177
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.0968	0.0771	0.101	0.0808	0.115
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.688***	-0.531	-0.487*	-0.472	-0.540*	-0.532
	(0.242)	(0.324)	(0.269)	(0.325)	(0.275)	(0.335)
Observations	510	510	494	494	494	494
Effective observations	[168,111]	[155,102]	[128,85]	[151,97]	[129,85]	[155,106]
Covariates	None	None	Some	Some	All	All
p-value	0.00451	0.102	0.0702	0.146	0.0500	0.112
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.0945	0.0752	0.0955	0.0765	0.0982

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 M4: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.485***	-1.309*	-1.206***	-1.204*	-0.854**	-0.979*
	(0.506)	(0.678)	(0.422)	(0.619)	(0.394)	(0.505)
Observations	195	195	179	179	179	179
Effective observations	[49,39]	[51,40]	[52,38]	[47,35]	[56,44]	[47,35]
Covariates	None	None	Some	Some	All	All
p-value	0.00336	0.0537	0.00426	0.0517	0.0300	0.0523
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0762	0.0862	0.0941	0.0864	0.103	0.0848
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.784**	-0.558	-0.618	-0.484	-0.684*	-0.622
	(0.368)	(0.543)	(0.381)	(0.565)	(0.380)	(0.563)
Observations	195	195	179	179	179	179
Effective observations	[57,48]	[56,43]	[54,43]	[52,38]	[57,44]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.0332	0.304	0.105	0.391	0.0713	0.270
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0989	0.0910	0.100	0.0934	0.105	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.

M.2. Results When Poverty Increases

Table M5: 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	0.496	1.137
	(1.062)	(1.927)	(1.389)	(2.235)	(1.381)	(2.104)
Observations	517	517	495	495	495	495
Effective observations	[120,163]	[126,193]	[92,137]	[114,164]	[96,137]	[115,181]
Covariates	None	None	Some	Some	All	All
p-value	0.499	0.549	0.746	0.576	0.720	0.589
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.114	0.124	0.0856	0.116	0.0863	0.121
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.600	0.899	0.466	1.182	0.534	0.972
	(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	0.700	0.622
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 M6: 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	-3.478	-1.646
	(4.621)	(6.961)	(5.001)	(7.448)	(4.558)	(7.965)
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	0.445	0.836
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.104	0.132	0.101	0.133	0.0953	0.109
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	1.258	1.534	1.182	2.385	0.330	1.942
	(3.039)	(4.616)	(3.710)	(5.300)	(3.846)	(5.142)
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	0.706
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 M7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.249	0.0912	0.118	-0.469	0.160	-0.474
	(0.347)	(0.453)	(0.379)	(0.668)	(0.372)	(0.668)
Observations	515	515	493	493	493	493
Effective observations	[123,167]	[162,231]	[108,154]	[109,155]	[108,154]	[109,155]
Covariates	None	None	Some	Some	All	All
p-value	0.473	0.840	0.755	0.482	0.668	0.478
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.169	0.0985	0.110	0.101	0.110
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.250	0.0845	0.160	-0.474	0.194	-0.433
	(0.347)	(0.455)	(0.371)	(0.668)	(0.369)	(0.649)
Observations	515	515	493	493	493	493
Effective observations	[123,167]	[162,227]	[108,154]	[109,155]	[109,154]	[113,164]
Covariates	None	None	Some	Some	All	All
p-value	0.471	0.852	0.666	0.478	0.599	0.505
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.116	0.168	0.101	0.110	0.104	0.116

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 M8: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.459	0.230	0.0219	-0.183	-0.0449	-0.0931
	(0.531)	(0.679)	(0.551)	(0.711)	(0.447)	(0.601)
Observations	196	196	174	174	174	174
Effective observations	[48,52]	[57,75]	[38,50]	[46,73]	[44,55]	[51,79]
Covariates	None	None	Some	Some	All	All
p-value	0.387	0.735	0.968	0.797	0.920	0.877
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0820	0.134	0.0855	0.136	0.106	0.158
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.220	-0.602	0.600*	-0.393	0.624*	-0.441
	(0.425)	(0.714)	(0.361)	(0.703)	(0.367)	(0.696)
Observations	196	196	174	174	174	174
Effective observations	[53,56]	[53,58]	[48,75]	[44,58]	[47,75]	[44,57]
Covariates	None	None	Some	Some	All	All
p-value	0.605	0.399	0.0962	0.576	0.0893	0.527
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0962	0.103	0.151	0.114	0.144	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.

N. Results for 2009-2015

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	-1.806***	-2.082***	-0.883*	-1.155	-0.906*	-1.600**
	(0.550)	(0.651)	(0.528)	(0.711)	(0.520)	(0.753)
Observations	687	687	639	639	639	639
Effective observations	[201,153]	[257,185]	[189,146]	[201,149]	[197,149]	[175,124]
Covariates	None	None	Some	Some	All	All
p-value	0.00103	0.00139	0.0944	0.104	0.0813	0.0337
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0949	0.137	0.0966	0.101	0.100	0.0872
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.248**	-1.479**	-0.512	-0.720	-0.592	-1.141
	(0.593)	(0.732)	(0.581)	(0.777)	(0.566)	(0.773)
Observations	687	687	639	639	639	639
Effective observations	[201,153]	[253,179]	[189,138]	[204,150]	[193,149]	[189,131]
Covariates	None	None	Some	Some	All	All
p-value	0.0353	0.0433	0.378	0.354	0.296	0.140
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0955	0.132	0.0954	0.103	0.0991	0.0916

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	-7.782***	-10.59***	-5.993***	-7.722**	-6.305***	-8.213**
	(2.228)	(2.712)	(2.246)	(3.140)	(2.292)	(3.216)
Observations	195	195	179	179	179	179
Effective observations	[73,67]	[73,68]	[57,44]	[57,44]	[53,43]	[57,44]
Covariates	None	None	Some	Some	All	All
p-value	0.000478	9.51e-05	0.00762	0.0139	0.00595	0.0107
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.154	0.155	0.108	0.109	0.0992	0.108
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-4.731**	-5.838**	-2.425	-3.029	-2.580	-5.379*
	(2.152)	(2.853)	(2.138)	(2.983)	(2.139)	(2.857)
Observations	195	195	179	179	179	179
Effective observations	[57,48]	[64,52]	[55,43]	[57,44]	[52,38]	[50,38]
Covariates	None	None	Some	Some	All	All
p-value	0.0279	0.0407	0.257	0.310	0.228	0.0597
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0989	0.121	0.102	0.105	0.0940	0.0898
height						

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	-0.627***	-0.508	-0.326	-0.318	-0.408	-0.385
	(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]	[189,138]
Covariates	None	None	Some	Some	All	All
p-value	0.00993	0.115	0.227	0.319	0.144	0.251
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.100	0.0929	0.0646	0.0895	0.0687	0.0945
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.604**	-0.476	-0.229	-0.252	-0.283	-0.304
	(0.243)	(0.328)	(0.288)	(0.345)	(0.285)	(0.343)
Observations	684	684	636	636	636	636
Effective observations	[216,164]	[201,146]	[146,89]	[175,124]	[146,89]	[181,124]
Covariates	None	None	Some	Some	All	All
p-value	0.0131	0.146	0.426	0.466	0.321	0.376
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.0915	0.0611	0.0880	0.0636	0.0889

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	-0.725*	-0.598	-0.469	-0.352	-0.569	-0.452
	(0.393)	(0.508)	(0.442)	(0.513)	(0.426)	(0.495)
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	0.0654	0.238	0.290	0.493	0.182	0.361
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0965	0.0953	0.0737	0.0956	0.0804	0.0964
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.634	-0.454	-0.227	-0.144	-0.363	-0.283
	(0.388)	(0.517)	(0.486)	(0.566)	(0.472)	(0.548)
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	0.103	0.380	0.641	0.799	0.443	0.605
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.0942	0.0684	0.0950	0.0736	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.

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	0.364	0.363	0.192	0.426	0.228	0.454
	(0.868)	(1.326)	(0.997)	(1.466)	(1.027)	(1.437)
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	0.825	0.752
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	0.235	0.403
	(0.939)	(1.300)	(1.025)	(1.445)	(1.048)	(1.427)
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	0.822	0.778
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 N6: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	3.580	4.093	0.953	1.501	-1.033	1.100
	(2.712)	(5.435)	(3.204)	(5.748)	(3.976)	(5.892)
Observations	196	196	174	174	174	174
Effective observations	[67,86]	[59,78]	[46,64]	[46,72]	[43,55]	[46,72]
Covariates	None	None	Some	Some	All	All
p-value	0.187	0.451	0.766	0.794	0.795	0.852
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.168	0.143	0.122	0.135	0.103	0.135
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	1.225	1.268	0.804	1.247	0.0937	0.992
	(3.432)	(4.723)	(3.726)	(5.148)	(3.938)	(5.134)
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	0.981	0.847
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 N7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.290	-0.118	0.0974	-0.350	0.114	-0.165
	(0.255)	(0.419)	(0.286)	(0.448)	(0.286)	(0.416)
Observations	690	690	626	626	626	626
Effective observations	[202,280]	[202,280]	[153,205]	[160,254]	[153,205]	[168,268]
Covariates	None	None	Some	Some	All	All
p-value	0.257	0.778	0.734	0.434	0.690	0.691
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.149	0.146	0.112	0.133	0.112	0.149
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.242	-0.380	-0.0608	-0.422	-0.0868	-0.366
	(0.274)	(0.450)	(0.334)	(0.471)	(0.354)	(0.451)
Observations	690	690	626	626	626	626
Effective observations	[191,266]	[191,260]	[143,183]	[160,241]	[131,175]	[160,254]
Covariates	None	None	Some	Some	All	All
p-value	0.377	0.399	0.855	0.371	0.806	0.417
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.134	0.132	0.0909	0.125	0.0837	0.133

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	0.472	0.173	0.389	-0.00623	0.388	-0.00379
	(0.301)	(0.517)	(0.310)	(0.521)	(0.309)	(0.516)
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	0.116	0.737	0.211	0.990	0.209	0.994
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.149	0.141	0.143	0.150	0.143	0.150
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.383	-0.188	0.305	-0.0465	0.320	-0.0877
	(0.322)	(0.634)	(0.352)	(0.545)	(0.348)	(0.545)
Observations	196	196	174	174	174	174
Effective observations	[57,74]	[57,63]	[46,64]	[47,75]	[46,64]	[47,73]
Covariates	None	None	Some	Some	All	All
p-value	0.234	0.767	0.386	0.932	0.358	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.

O. Results for 2008-2015

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	-1.940***	-2.376***	-1.111**	-1.522**	-1.010*	-1.855**
	(0.539)	(0.681)	(0.505)	(0.710)	(0.545)	(0.758)
Observations	776	776	712	712	712	712
Effective observations	[228,191]	[256,201]	[236,181]	[224,173]	[224,173]	[200,143]
Covariates	None	None	Some	Some	All	All
p-value	0.000320	0.000486	0.0278	0.0320	0.0637	0.0144
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0991	0.119	0.117	0.103	0.104	0.0895
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.342**	-1.653**	-0.795	-0.980	-0.731	-1.423*
	(0.556)	(0.715)	(0.538)	(0.733)	(0.582)	(0.767)
Observations	776	776	712	712	712	712
Effective observations	[236,191]	[260,209]	[228,173]	[228,173]	[220,171]	[208,151]
Covariates	None	None	Some	Some	All	All
p-value	0.0157	0.0208	0.139	0.181	0.209	0.0636
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.124	0.111	0.107	0.102	0.0931

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	-7.206***	-9.733***	-5.011**	-6.383**	-5.367**	-7.713***
	(2.143)	(2.804)	(2.209)	(2.882)	(2.202)	(2.858)
Observations	195	195	179	179	179	179
Effective observations	[69,53]	[62,50]	[55,43]	[57,44]	[52,40]	[55,43]
Covariates	None	None	Some	Some	All	All
p-value	0.000771	0.000518	0.0233	0.0267	0.0148	0.00697
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.128	0.111	0.102	0.106	0.0950	0.101
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-5.558**	-6.874**	-3.313	-4.150	-3.666	-6.837**
	(2.245)	(2.918)	(2.297)	(2.991)	(2.409)	(2.788)
Observations	195	195	179	179	179	179
Effective observations	[62,49]	[65,52]	[56,43]	[57,44]	[47,35]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.0133	0.0185	0.149	0.165	0.128	0.0142
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.105	0.121	0.103	0.109	0.0868	0.0920

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	-0.542**	-0.416	-0.334	-0.301	-0.361	-0.328
	(0.233)	(0.314)	(0.266)	(0.315)	(0.280)	(0.334)
Observations	773	773	709	709	709	709
Effective observations	[236,191]	[220,171]	[160,103]	[200,143]	[160,103]	[208,151]
Covariates	None	None	Some	Some	All	All
p-value	0.0201	0.185	0.209	0.341	0.197	0.326
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.0903	0.0633	0.0894	0.0646	0.0924
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.532**	-0.396	-0.241	-0.247	-0.271	-0.279
	(0.232)	(0.319)	(0.281)	(0.332)	(0.283)	(0.339)
Observations	773	773	709	709	709	709
Effective observations	[244,193]	[208,163]	[156,103]	[188,139]	[160,103]	[192,143]
Covariates	None	None	Some	Some	All	All
p-value	0.0219	0.215	0.392	0.456	0.340	0.411
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.104	0.0878	0.0594	0.0860	0.0612	0.0880

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	-0.602*	-0.321	-0.393	-0.246	-0.440	-0.328
	(0.334)	(0.471)	(0.428)	(0.500)	(0.412)	(0.474)
Observations	195	195	179	179	179	179
Effective observations	[65,53]	[57,48]	[44,32]	[54,43]	[45,34]	[52,42]
Covariates	None	None	Some	Some	All	All
p-value	0.0712	0.495	0.359	0.623	0.286	0.489
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.123	0.0995	0.0726	0.100	0.0766	0.0961
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.631*	-0.442	-0.357	-0.275	-0.409	-0.323
	(0.351)	(0.461)	(0.438)	(0.498)	(0.427)	(0.478)
Observations	195	195	179	179	179	179
Effective observations	[62,49]	[56,45]	[40,28]	[52,40]	[44,29]	[52,38]
Covariates	None	None	Some	Some	All	All
p-value	0.0717	0.338	0.414	0.580	0.338	0.500
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.105	0.0945	0.0688	0.0949	0.0710	0.0932

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	0.283	0.279	0.137	0.324	0.165	0.370
	(0.800)	(1.227)	(0.915)	(1.346)	(0.955)	(1.336)
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	0.862	0.782
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	0.155	0.280
	(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	0.875	0.830
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 O6: 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	0.0939	-0.371
	(3.324)	(5.142)	(3.647)	(5.401)	(3.895)	(5.600)
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	0.981	0.947
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.124	0.150	0.0988	0.135	0.0944	0.139
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.854	0.949	0.697	0.998	-0.146	-1.050
	(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	0.852
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 O7: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.306	-0.154	0.106	-0.209	0.0882	0.119
	(0.228)	(0.407)	(0.275)	(0.399)	(0.288)	(0.354)
Observations	779	779	693	693	693	693
Effective observations	[265,343]	[233,307]	[175,228]	[187,295]	[175,220]	[231,339]
Covariates	None	None	Some	Some	All	All
p-value	0.180	0.705	0.700	0.601	0.759	0.738
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.167	0.139	0.111	0.143	0.105	0.186
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.193	-0.426	-0.0610	-0.295	-0.0599	0.106
	(0.273)	(0.455)	(0.344)	(0.403)	(0.340)	(0.354)
Observations	779	779	693	693	693	693
Effective observations	[225,267]	[225,267]	[151,196]	[183,291]	[151,196]	[231,339]
Covariates	None	None	Some	Some	All	All
p-value	0.480	0.350	0.859	0.464	0.860	0.764
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.122	0.122	0.0811	0.137	0.0821	0.184

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	0.448	0.104	0.383	0.0201	0.366	-0.00664
	(0.310)	(0.510)	(0.327)	(0.510)	(0.328)	(0.510)
Observations	196	196	174	174	174	174
Effective observations	[57,70]	[57,73]	[46,64]	[47,73]	[46,61]	[47,73]
Covariates	None	None	Some	Some	All	All
p-value	0.149	0.839	0.241	0.969	0.265	0.990
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.125	0.132	0.121	0.138	0.120	0.139
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.411	-0.0326	0.338	0.0136	0.285	-0.0914
	(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	0.193	0.954	0.333	0.979	0.426	0.863
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.124	0.121	0.111	0.138	0.106	0.134

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

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

Table P1: 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 P2: 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 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.

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

Table P3: 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 P4: 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 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.

Q. Poverty Rates For Different Samples

Table Q1: 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)
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)

Note: Standard deviations are in parentheses. Total poverty rates are from the 2002 and 2011 census. "Municipalities only in 2002" (row 3) refer to the 32 municipalities that had data in the 2002 census only.

Table Q2: 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)
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)

Note: Standard deviations are in parentheses. Extreme poverty rates are from the 2002 and 2011 census. "Municipalities only in 2002" (row 3) refer to the 32 municipalities that had data in the 2002 census only.

As shown in the above tables, the 32 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. By the same token, the poverty-reducing and extreme poverty-reducing samples generally have higher poverty and extreme poverty rates than the poverty-increasing and extreme poverty-increasing samples. Additionally, the literatures on poverty traps (e.g. Sachs, 2005), clientelism (Scott, 1972; Keefer, 2007*a*, e.g.), and modernization itself (e.g. Lerner, 1958; 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 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.

R. Additional Results for Morales Term Regressions

Table R1: Number of Infractions Committed (2007-2018)

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.781***	0.738***	0.627***	0.762***	0.552***	0.661***
	(0.027)	(0.027)	(0.045)	(0.025)	(0.033)	(0.045)
Poverty Reduced		-0.059*	-0.062*			
		(0.034)	(0.034)			
Population (log)					1.518***	-0.216
					(0.181)	(0.300)
Re-elected Mayor					0.037	0.019
					(0.030)	(0.030)
Observations	3821	3384	3384	3821	3478	3478
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 R2: Log Amounts of Misappropriated Funds (2007-2018)

	(1)	(2)	(3)	(4)	(5)	(6)
Morales Term	0.183*	0.141	0.422***	0.184*	0.023	0.363
	(0.096)	(0.101)	(0.148)	(0.095)	(0.118)	(0.288)
Poverty Reduced		-0.082	-0.084			
		(0.075)	(0.075)			
Population (log)					1.286***	0.355
					(0.459)	(0.948)
Re-elected Mayor					0.071	0.054
					(0.093)	(0.094)
Observations	3816	3379	3379	3816	3473	3473
R^2	0.002	0.002	0.035	0.002	0.005	0.041
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. Additional Regressions Regarding the Close Election Mechanism

Table S1: Infractions: How Much Do Close Elections Matter (2010-2015)?

	(1)	(2)	(3)	(4)	(5)
Alignment	-0.064	-0.061	-0.073	0.030	0.040
	(0.045)	(0.048)	(0.047)	(0.056)	(0.065)
Poverty Reduction		-0.019	-0.019		
		(0.049)	(0.048)		
Log Population					2.720***
					(0.479)
Reelected Mayor					0.065
					(0.066)
Observations	1259	1124	1124	1259	1177
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 S2: Log Amounts of Misappropriated Funds (2007-2018)

	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	-0.261**	-0.263**	-0.274**	-0.048	-0.038	-0.039
	(0.121)	(0.127)	(0.128)	(0.132)	(0.161)	(0.155)
Poverty Reduction		-0.044	-0.044			
		(0.101)	(0.101)			
Log Population					5.411***	2.529
					(1.295)	(2.383)
Reelected Mayor					0.263	0.277*
					(0.173)	(0.160)
Observations	1255	1120	1120	1255	1173	1076
R^2	0.007	0.008	0.078	0.000	0.037	0.192

Note: Linear regression models; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors clustered by municipality in parentheses

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