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

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Abstract

We show that once reductions in poverty decrease voter need for clientelism, it ultimately reduces corruption through political selection. After reductions in poverty open up the possibility of more programmatic (non-clientelistic) voting, voters seek to attribute blame for their previous economic circumstances. In their search for clarity of responsibility, voters default toward the easiest indicator: party alignment between subnational and national levels of government. Such dynamics reduce the bureaucratic advantages of alignment, and aligned politicians respond by reducing their corruption levels. To provide empirical tests for our theory, we employ a series of close-election regression discontinuity designs on mayoral races in Guatemala. We find broad empirical support for our theory when analyzing the number of audit violations committed, the amount of money misappropriated, and the reelection rates of aligned politicians. The results of our study help document how reductions in poverty decrease corruption through modernization, and how politics is central to the process.

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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 manifests in many deleterious forms, and politics is often at the center of corrupt transactions. For example, politicians and bureaucrats in Mexico and Colombia are infamous for accepting bribes from drug cartels, who fuel violence to such an extent that it lowers life expectancy (Dal Bó, Dal Bó and Di Tella, 2006; Aburto et al., 2016). In India, politicians who narrowly win public office quickly accumulate 3-5% more assets than second-place candidates, providing yet another example of how politics often facilitates egregious corruption (Fisman, Schulz and Vig, 2014).

Two of the most prominent remedies to corruption include improving institutional quality and increasing levels of economic development.[2] Although these are theoretically compelling explanations for corruption and its mitigation, extant literature suffers from three major drawbacks. First, the majority of the literature relies on corruption perceptions, not empirical measures of corruption.[3] Second, limited existing work uses objective subnational data,[4] and most of the literature that uses such data focuses almost exclusively only Brazil, making it difficult to disentangle the precise set of institutions and/or economic remedies that reduce corruption more broadly. Third, even less existing work shows how economic development and institutions interact over time with politics to produce different levels of corruption.[5] This third drawback is particularly significant given that corruption is mostly a political phenomenon.

[1] For more on the definition of corruption, see, for example, Treisman (2000, 2007), Rose-Ackerman and Palifka (2016) and Søreide (2014).

[2] For more on how institutional quality affects levels of corruption, see, for example, Shleifer and Vishny (1993), Persson, Tabellini and Trebbi (2003), Lederman, Loayza and Soares (2005), Aidt and Dutta (2008), Aidt (2009), Dreher, Kotsogiannis and McCorriston (2009), and Ferraz and Finan (2011). For more on how economic development reduces corruption, see, for example, Mauro (1995), La Porta et al. (1999), and Treisman (2000, 2007).

[3] 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*), Langbein and Knack (2010), Thomas (2010), Gingerich (2013*a*), Bersch and Botero (2014), and Gisselquist (2014).

[4] For notable exceptions, see 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.

[5] See Pereira, Melo and Figueiredo (2009), Ferraz and Finan (2011), Brollo et al. (2013), and Klašnja (2015).

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 focuses on political party alignment between national and subnational governments, an institutional configuration that facilitates significant resource advantages (e.g. Solé-Ollé and Sorribas-Navarro, 2008; Greene, 2010; Brollo and Nannicini, 2012).[6] In simpler terms, this paper examines the consequences for corruption when the president or prime minister's party in power has the same party in lower-level government entities.

On the one hand, the decentralization and clientelism literatures are clear that party alignment is an institutional configuration that facilitates resource-related bureaucratic advantages in both developed and developing countries.[7] On the other 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

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

[7] 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); Ghana (Banful, 2011*a*,*b*); Guatemala (Sandberg and Tally, 2015); India (Velasco Rivera, 2020); Italy (Alesina and Paradisi, 2017); Mexico (Timmons and Broidy, 2013); Philippines (Labonne, 2016); Pakistan (Callen, Gulzar and Rezaee, 2020); Portugal (Veiga and Veiga, 2007; Veiga and Pinho, 2007; Aidt, Veiga and Veiga, 2011; Veiga and Veiga, 2013); Russia (Treisman and Gimpelson, 2001); Spain (Solé-Ollé and Sorribas-Navarro, 2008); USA (Ansolabehere, Snyder and Ting, 2003; Kriner and Reeves, 2012, 2015; Christenson, Kriner and Reeves, 2017; Hill and Jones, 2017); Uruguay (Manacorda, Miguel and Vigorito, 2011); and West Germany (Schneider, 2010).

clientelistic citizen-politician linkages,[8] 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 after reductions in poverty open up the possibility of more programmatic (non-clientelistic) voting.

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.[9] By reducing the need for "request-fulfilling",[10] we argue that reducing poverty leads voters be less tolerant of corrupt politicians as well, yielding a different landscape for political selection.[11] By contrast, under comparatively more difficult economic circumstances, voters are more supportive of aligned politicians because of their access to the spoils of the bureaucracy. With these resources, aligned politicians can buy the support of the masses, who in turn will be more likely to forgive corrupt politicians as long as part of the money is redistributed back to them in form of clientelistic transfers or discretionary spending. In such environments, clientelistic linkages can be more compelling for voters because informational environments can be weak and politicians' programmatic policy promises are typically not very credible.[12] Because non-aligned politicians do not have as large of a resource pool at their disposal, non-aligned politicians have no choice but to rely on valence appeals, which are less compelling in a context of poverty. 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

[8] For a review of citizen-politician linkages, see Kitschelt (2000) and Kitschelt and Wilkinson (2007).
[9] 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).
[10] Request-fulfilling entails "citizens demand[ing] clientelistic benefits" (Nichter and Peress, 2017).
[11] For an excellent review of the literature on political selection, see Dal Bó and Finan (2018).
[12] See, for example, Keefer (2004, 2007*a*,*b*), Keefer and Khemani (2005), Keefer and Vlaicu (2008), and De La O and Rodden (2008).
[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).

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 misapppropriation derived from audit reports. Our political data constitute the electoral results of municipal elections. The poverty data come from the results of the 2002 and 2011 censuses of Guatemala.

To operationalize whether a municipality is performing better economically, we specifically compare municipalities 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. In some but not all cases, the result is similar for 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). In future analyses, we aim to examine whether aligned politicians who are relatively corrupt are less likely to run for reelection in future

[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).

electoral terms.[15]

The one drawback of current results regarding corruption 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 systems.

Given that electoral incentives mechanistically underpin politicians' incentives to engage in corruption (e.g. Ferraz and Finan, 2011), we complement of our corruption analyses with related ones pertaining to reelection. In preliminary analyses using alignment as the running variable in close-election regression discontinuity designs, we find that voters are significantly less likely to reelect aligned politicians in the decreased poverty sample. The result provides support for our proposition that decreasing poverty leads voters to become less tolerant of aligned politicians, despite their greater access to the spoils of the bureaucracy. By the same token, the result contrasts existing explanations of economic voting in wealthier countries with stronger programmatic citizen-politician linkages (e.g. Rudolph, 2003; Soroka, Stecula and Wlezien, 2015; Schleiter and Tavits, 2018). Our results thus provide a new perspective and mechanism—alignment—from which to understand economic voting and the incumbency disadvantage in countries with stronger clientelistic citizen-politician linkages.[16]

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,

[15] Nikolova and Marinov (2017) find that Bulgarian politicians who steal higher proportions of natural disaster reconstruction funds are less likely to run for reelection. However, we do not know of any studies that find or investigate a similar result specifically for aligned politicians.

[16] For related studies, see, for example, Keefer (2007*a*), Hanusch and Keefer (2014), Klašnja (2015, 2016), Klašnja and Titiunik (2017), and Dettman, Pepinsky and Pierskalla (2017).

2009; Acemoglu and Robinson, 2018; Acemoglu et al., 2019).[17] We, of course, do not dispute these very comprehensive studies. Nevertheless, our empirical results based on close-election data suggest a potential alternative path to income-based modernization: that is, through the reduction of corruption. After all, our results show that aligned parties with resource advantages reduce corruption and lose close elections more frequently in areas where poverty has reduced.

The paper proceeds as follows. Section 1 provides a theoretical framework to understand how the combination of reducing poverty and alignment yield decreased levels of corruption. Section 2 constitutes the Research Design, which introduces readers to the data, institutional context, and identification strategy underpinning this paper. Section 3 provides the main results for this paper.

1. Theoretical Framework

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 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 the local-level politician i's maximisation problem. Consistent with Magaloni, Díaz-Cayeros and Estévez (2007),[18] local-level politician i's personal budget constraint, b_i, comprises spending on public

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

[18] The theoretical framework in Magaloni, Díaz-Cayeros and Estévez (2007) also forms the basis of Díaz-Cayeros, Estévez and Magaloni (2016).

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

$$b_i = g_i + r_i \tag{1}$$[19]

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:

$$r = c + p, \qquad \text{where } c = \gamma r \tag{2}$$[20]

Under Equation (2), we assume that c increases with r, meaning that the local-level politician 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,[21] we distinguish between local-level politician i's favored levels of rent extraction in the current electoral period, $r_{i,1}$, as well as a potential future one, $r_{i,2}$:

$$r_i = r_{i,1} + r_{i,2} \tag{3}$$[22]

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

[19] 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.

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

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

[22] 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.

define for the current period as follows:

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

where $W(\cdot)$ is the satisfaction that the electorate derives 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; and β represents the electorate's discount rate of clientelistic benefits under a decrease in poverty through $\beta_i \in (0, 1)$, making the discount rate under alignment for such electorates:[23]

$$\beta^{1+a} = \beta^{1+1} \implies \beta^2 < \beta^1 \tag{5}$$

Our theory depends on β_i. In line with the conventional wisdom of the clientelism literature, we assume that reducing poverty leads voters to discount clientelism more and fairer, policy-based programmatic spending less (Kitschelt and Wilkinson, 2007; Stokes et al., 2013; Gonzalez-Ocantos, Kiewiet de Jonge and Nickerson, 2014). Consistent with Schwindt-Bayer and Tavits (2016), alignment signals clarity of responsibility for misgovernance, thereby yielding an even higher discount rate for clientelistic benefits than poverty reduction alone.

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).[24] 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,

[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, $r_{i,1}$.

[24] 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."

2014). Given Equation (3), the maximization problem for local-level politician's i can be represented as:

$$\max_{r_{i,1}} \; U(r_{i,1}) + \pi(s)\; U(r_{i,2}) + (1 - \pi(s))\; U(x_{i,2}) \tag{6}$$

Appendix A solves the maximization problem in Equation (6) for both the aligned and non-aligned local-level government entities. According to the solution of the maximization problem, the electorate starts highly discounting the clientelistic benefits associated with local-level politician i having higher levels of rents after a reduction in poverty. Alignment entails an even higher discount rate on aligned politician's clientelistic activities, yielding repercussions for her reelection probabilities and future expected rents. The combination of poverty reduction, alignment, and changes in political selection thus lead to a discontinuity in corruption activity between aligned and unaligned local-level politicians.

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 an administrative level akin to a state or province, the president appoints governors from his or her same political party. Accordingly, Guatemala does not have political variation at the department level.

2.2. Identification Strategy

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):

$$C_{it}^{(unaligned)} = f(C_{it}^{(unaligned)}) + \rho^{(unaligned)} D_{it} + \eta_{it}^{(unaligned)} \tag{7}$$
$$\text{where } f(C_{it}^{(unaligned)}) = \alpha^{(unaligned)} + \sum_{k=1}^{p} \beta_j^{(unaligned)} X_{it}^{p} + \rho^{(unaligned)} D_{it} + \eta_{it}^{(unaligned)}$$

$$C_{it}^{(aligned)} = f(C_{it}^{(aligned)}) + \rho^{(aligned)} D_{it} + \eta_{it}^{(aligned)} \tag{8}$$
$$\text{where } f(C_{it}^{(aligned)}) = \alpha^{(aligned)} + \sum_{k=1}^{p} \beta_j^{(aligned)} X_{it}^{p} + \rho^{(aligned)} D_{it} + \eta_{it}^{(aligned)}$$

where C_{it} reflects the amount of corruption in municipality i at time t after a close election; the running variable, X_i, is the margin of victory for the aligned party in the most recent election; D_{it} is an indicator for whether municipality i is aligned at time t; α is an intercept; p reflects the order of polynomial fit; and η_{it} is an error term. Given the role of poverty in our theory and the inability of the typical regression discontinuity design to accomodate interactions, we run the above regression discontinuity analyses under three different samples: (a) the whole sample; (b) municipalities in which poverty decreased between the two latest censuses; and (c) municipalities in which poverty increased between the latest two censuses.[25]

We take a number of steps to ensure the robustness of 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, to assuage readers that we do not intentionally choose bandwidths that favor our theory, we apply an automatically derived, optimal bandwidth following Calonico, Cattaneo and Titiunik (2014). Third, we employ Calonico et al.'s (2019) new method to consider how adding covariates to our regression discontinuity analyses may alter the results. Fourth, we cluster our standard errors at the municipality level. Fifth, 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

The municipality-level poverty data in this paper come from Guatemala's National Statistics Institute (INE, *Instituto Nacional de Estadística*). The country uses the United Nations Economic Commission for Latin America (CEPAL) methodology to measure poverty. Accordingly, the INE measures poverty based on the number of people with at least one major unmet basic need, and extreme poverty encompasses people with more than one basic need.

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 census data

[25] Recent papers by Carril et al (2017) and Hsu and Shen (2019) propose new methods to undertake subgroup analysis and assess treatment effect heterogeneity for regression discontinuity designs. However, neither of these two papers use bias-corrected regression discontinuity inference (see Calonico, Cattaneo and Titiunik, 2014), so we do not use these methods in this paper.

are available are 2002 and 2011. We use thus use the poverty and extreme poverty data from these years to divide our sample into poverty-increasing, poverty decreasing, extreme poverty-decreasing, and extreme poverty-increasing municipalities.

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 candidates; (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 party mayor. If neither the first- nor second-place candidate is from the aligned party, we exclude it from the analysis. Such a strategy 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 Appendix J, we run a McCrary (2008) density test on our running variable. From both a yearly and electoral perspectives, it passes the test.

2.5. Corruption Data

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

Each year, the National Audit Office audits circa 320 of Guatemala's 340 municipalities. As shown in Appendix X, municipalities with aligned party mayors are more likely to be audited than municipalities with non-aligned party mayors,. Accordingly, there are no concerns regarding the partisan distribution of audits.

For each audited municipality from 2004 to the present, the National Audit Office publishes on its website: (1) the number of overall infractions committed (*sancciones*), including the amount of money in the local currency (Quetzales) associated with these infractions; (2) the number charge reports (*informes de cargo*) filed, including the amount of lost money associated with such charges; and (3) the number formal legal complaints (*denuncias*) issues, as well as the amount of money associated with these complaints. Both formal charge reports and formal legal complaints are rather rare. Given that our study aims to uncover the sources of bureaucratic corruption, we focus our analysis on the number of infractions committed and the amount of money associated with those infractions. For comparability purposes, we first deflate the money version of the infractions variable and then take its natural log. We do not transform the infractions variable.

Table 1: Descriptive Statistics of Infraction Variables

Panel A: Infractions (Year Viewpoint) VARIABLES	Decrease Unaligned Mean	N	Decrease Aligned Mean	N	Increase Unaligned Mean	N	Increase 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	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	Decrease Unaligned Mean	N	Decrease Aligned Mean	N	Increase Unaligned Mean	N	Increase 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	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.

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

Table 2: Descriptive Statistics of Covariates

Panel A VARIABLES	Decrease Unaligned Mean	N	Decrease Aligned Mean	N	Increase Unaligned Mean	N	Increase Aligned Mean	N
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
Percentage of Mayor Reelected	0.307	1,005	0.217	332	0.331	968	0.0945	254
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	Decrease Unaligned mean	N	Decrease Aligned mean	N	Increase Unaligned mean	N	Increase 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.

of Finance, which made its data publicly available through the World Bank's (2019) BOOST Initiative.[26] Table 2 presents descriptive statistics of these covariate data.

3. Results

3.1. Corruption Results Disaggregated by Poverty

Figure 3 provides optimal data-driven regression discontinuity plots of our main results for corruption in the poverty-reducing samples, using Calonico, Cattaneo and Titiunik's (2015) evenly-spaced variance method. For comprehensiveness, we estimate these results using both the number of infractions committed and the log amounts associated with those

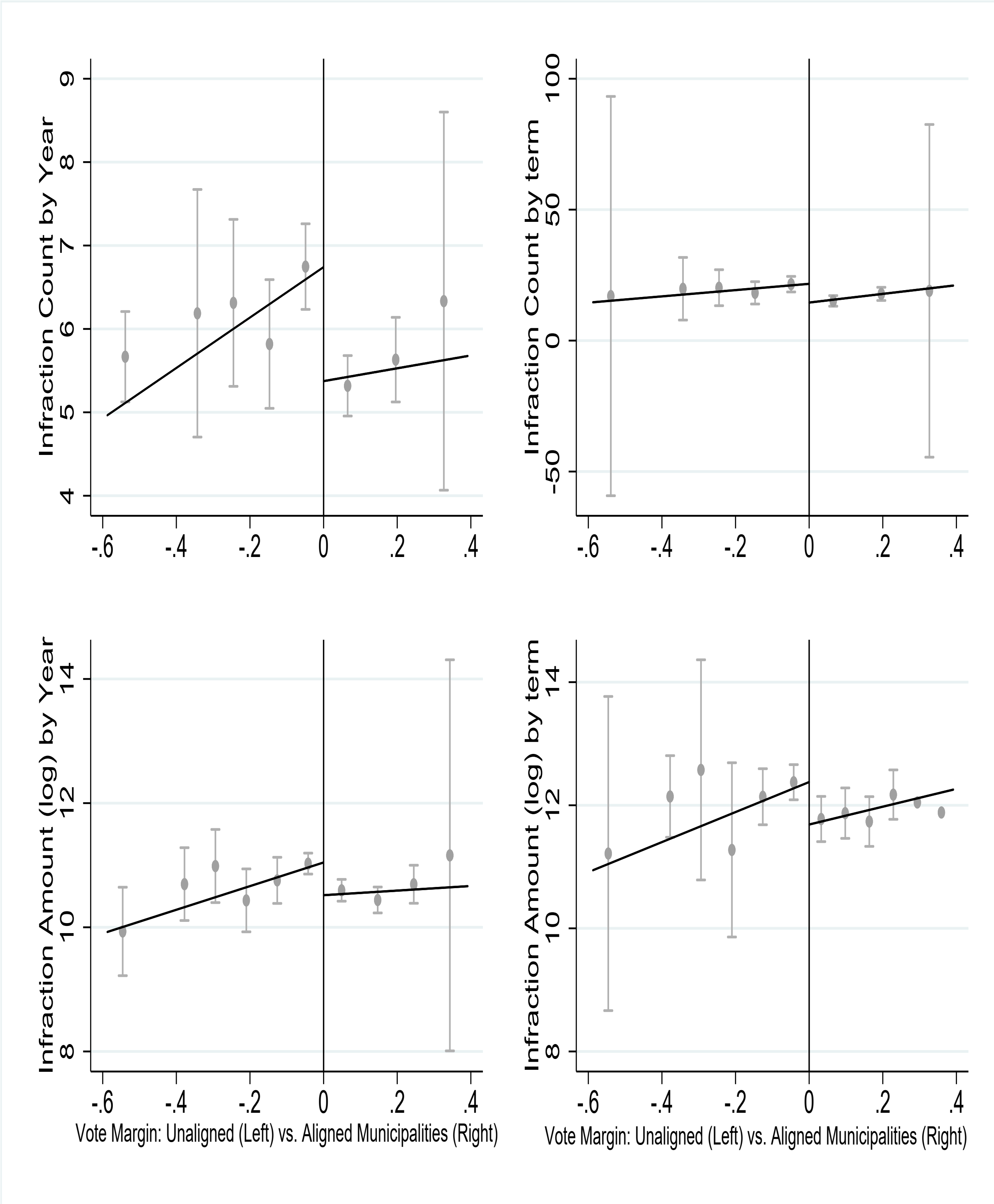
[26] The data aggregate spending on the following categories: Care and natural disaster management; defense and homeland security; defense; education; environmental protection; health; internal security; public order and safety; social protection; sports, culture, recreation, and religion; urban community service.

infractions as the dependent variables, and the results are similar for both yearly and electoral term data. In Appendix B, Tables 4 and 5 present the results for the infractions dependent variable, and Tables 6 and 7 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). All specifications, including those that control for the influence of covariates, suggest the same overall relationship for these poverty-reducing samples: party alignment yields less corruption. In Tables 34 and 35 of Appendix J, we further show that these results are not due to outliers.

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 18 and 19 in Appendix F.1 show that the results for the last two years, and Tables 22 and Tables 23 in Appendix G.1 present the results for the final year before the election. When compared to the results from the first two years in Tables 26 and 27 in Appendix H, it is clear to see that the final two years are mostly driving the overall reduction in corruption in the poverty-reducing sample. Overall, these results are consistent with Ferraz and Finan (2008) and Bobonis, 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 moderate poor behavior from aligned politicians, who generally enjoy a resource advantage relative to non-aligned politicians.

As predicted by our theory, alignment only reduces corruption in the poverty-reducing samples. Appendix D disaggregates results for the sample in which poverty increased from one census to next. As Tables 12, 13, 14, and 15 show, results are very inconsistent in the poverty-increasing sample. Tables 30, 31, 32, and 33 in Appendix I show similarly inconsistent results for the sample that is not disaggregated according to poverty.

Figure 1: RDD plots for Infraction Count and Amount



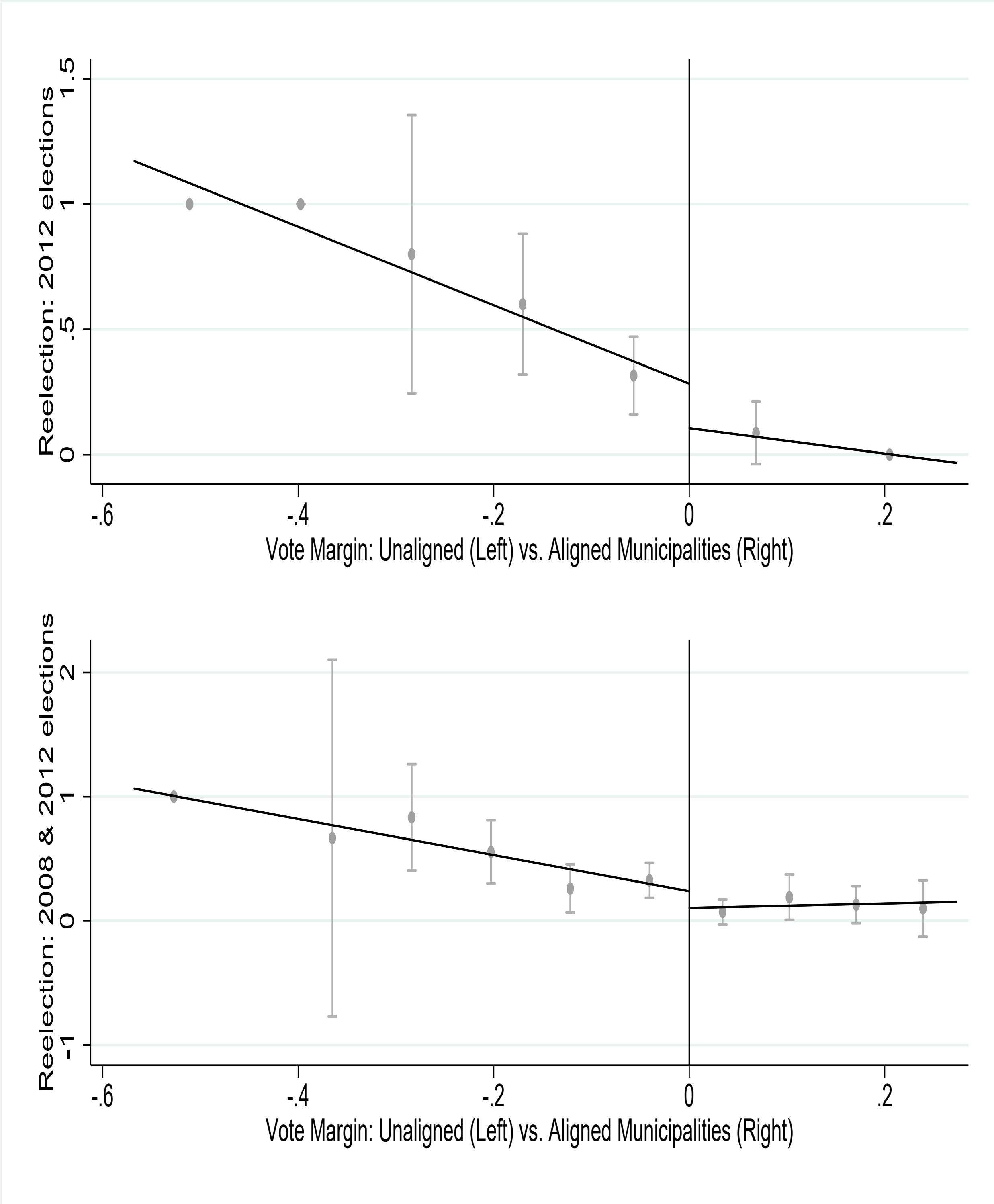
3.2. Corruption Results Disaggregated by Extreme Poverty

To assess the extent to which improving economic conditions can reduce corruption from aligned politicians, we also examine the extent to which reducing extreme poverty yields similar results as those of the poverty-reducing sample. Tables 8 and 9 in Appendix C present the main results. Overall, they are weaker than those of the previous section but still suggestive of an overall relationship. The coefficients are negative throughout, and results are statistically significant for many of the specifications. Nevertheless, it is necessary to note that the confidence intervals are generally wider and the p-values are marginally larger. As with the previous subsection, Tables 16 and 17 in Appendix E show that same relationship does not hold for the sample in which extreme poverty increased.

3.3. Reelection Results

Figure 2 presents the main results for the reelection dependent variable.

Figure 2: RDD plots for Reelection



3.4. RDD Results for Reelection

Table 3: RDD Estimate for Reelection

Panel A: Including 2012 Elections

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.316	-0.334	-0.357*	-0.403*	-0.419**	-0.528**
	(0.197)	(0.228)	(0.208)	(0.232)	(0.203)	(0.219)
Observations	106	106	106	106	106	106
Effective observations	[39,22]	[45,33]	[38,22]	[45,27]	[35,21]	[45,26]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.109	0.143	0.0861	0.0822	0.0385	0.0160
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.118	0.174	0.113	0.149	0.100	0.145

Panel B: Including 2008 and 2012 Elections

Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.286*	-0.326*	-0.408***	-0.417**	-0.420***	-0.431***
	(0.155)	(0.171)	(0.152)	(0.167)	(0.146)	(0.167)
Observations	179	179	179	179	179	179
Effective observations	[46,35]	[68,56]	[46,35]	[68,56]	[46,35]	[66,49]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0648	0.0562	0.00705	0.0124	0.00402	0.00967
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0837	0.148	0.0822	0.148	0.0826	0.136

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. 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.

Appendix

A. Theoretical Derivation

We solve for the following maximisation problem for the local-level politician:

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

Accordingly, we can rewrite the maximization problem as follows:

$$\max_{r_{i,1}} U(r_{i,1}) + \pi(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}))U(r_{i,2}) + [1-\pi(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}))]U(x_{i,2}) \tag{10}$$

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

$$\begin{aligned} 0 =& U'(r_{i,1}) + U(r_{i,2})\pi'(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}))[-W'(1-r_{i,1}) + \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] \\ &- U(x_{i,2})\pi'(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}))[-W'(1-r_{i,1}) + \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] \end{aligned} \tag{11}$$

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

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

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

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

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

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

where $\overline{r_{i,1}}$ and $\underline{r_{i,1}}$ are the optimal rent for the aligned and unaligned mayors, respectively. Accordingly, it follows that $\overline{r_{i,1}} = r_{i,1}* - z < r_{i,1}* < r_{i,1}* + k = \underline{r_{i,1}}$ where $z, k > 0$.[27] ■

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

B. When Poverty Decreases

Table 4: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.626***	-2.228***	-0.855	-1.204	-1.209*	-1.627**
	(0.553)	(0.724)	(0.608)	(0.799)	(0.630)	(0.782)
Observations	601	601	569	569	569	569
Effective observations	[186,138]	[178,136]	[146,102]	[154,104]	[140,86]	[146,102]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00329	0.00209	0.159	0.132	0.0549	0.0375
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.100	0.0960	0.0765	0.0852	0.0705	0.0769
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.166*	-1.693**	-0.563	-0.963	-0.895	-1.421*
	(0.596)	(0.768)	(0.629)	(0.798)	(0.648)	(0.792)
Observations	601	601	569	569	569	569
Effective observations	[182,136]	[182,138]	[146,102]	[154,104]	[144,98]	[146,102]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0505	0.0275	0.371	0.228	0.167	0.0726
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0967	0.0981	0.0762	0.0870	0.0733	0.0790

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 5: 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
Conventional 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
Conventional 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 6: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.766***	-0.684**	-0.529**	-0.539*	-0.663**	-0.685**
	(0.218)	(0.327)	(0.268)	(0.318)	(0.275)	(0.322)
Observations	598	598	566	566	566	566
Effective observations	[206,145]	[158,114]	[132,76]	[150,104]	[132,82]	[170,126]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.000438	0.0367	0.0480	0.0899	0.0158	0.0333
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.117	0.0844	0.0625	0.0839	0.0686	0.0964
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.737***	-0.652**	-0.462*	-0.493	-0.561**	-0.621*
	(0.219)	(0.326)	(0.273)	(0.324)	(0.276)	(0.332)
Observations	598	598	566	566	566	566
Effective Observations	[206,145]	[158,114]	[132,76]	[150,104]	[132,76]	[150,104]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.000748	0.0454	0.0905	0.128	0.0419	0.0618
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.0845	0.0616	0.0844	0.0645	0.0848

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 7: 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
Conventional 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
Conventional 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.

C. When Extreme Poverty Decreases

C.1. RDD Tables

Table 8: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.537***	-2.224***	-1.292*	-1.540*	-1.525**	-2.207**
	(0.576)	(0.770)	(0.670)	(0.808)	(0.689)	(0.870)
Observations	670	670	625	625	625	625
Effective observations	[179,152]	[187,156]	[138,122]	[172,144]	[128,110]	[138,126]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00765	0.00387	0.0540	0.0567	0.0270	0.0112
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0903	0.0912	0.0723	0.0936	0.0691	0.0735
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.333**	-1.982**	-1.120*	-1.302	-1.370*	-2.074**
	(0.604)	(0.777)	(0.678)	(0.805)	(0.703)	(0.871)
Observations	670	670	625	625	625	625
Effective Observations	[179,152]	[191,162]	[138,126]	[180,158]	[134,114]	[140,130]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0274	0.0107	0.0983	0.106	0.0514	0.0172
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0903	0.0944	0.0732	0.0972	0.0702	0.0772

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 9: 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
Conventional 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
Conventional 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 10: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.452*	-0.650**	-0.313	-0.412	-0.389	-0.555
	(0.255)	(0.321)	(0.273)	(0.333)	(0.281)	(0.341)
Observations	667	667	622	622	622	622
Effectiveness observations	[155,142]	[155,142]	[132,114]	[140,130]	[138,122]	[152,134]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0763	0.0425	0.252	0.216	0.166	0.104
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0794	0.0781	0.0699	0.0783	0.0716	0.0882
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.441*	-0.639**	-0.290	-0.400	-0.336	-0.490
	(0.255)	(0.320)	(0.275)	(0.335)	(0.281)	(0.340)
Observations	667	667	622	622	622	622
Effective observations	[155,142]	[155,142]	[128,110]	[140,130]	[128,106]	[140,130]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0833	0.0463	0.292	0.232	0.233	0.149
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0793	0.0783	0.0688	0.0788	0.0666	0.0773

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.

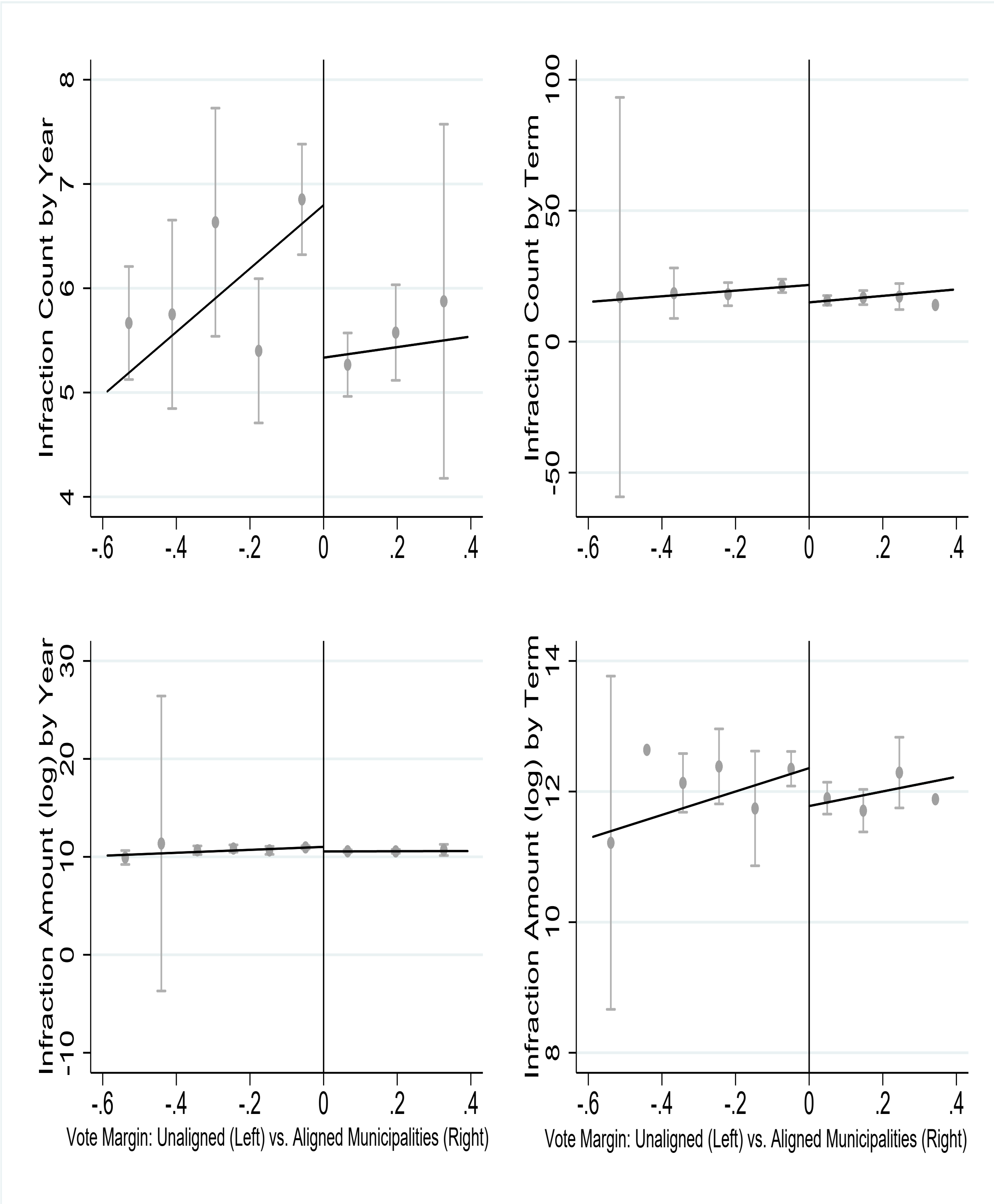
Table 11: RDD Estimates for Infraction Amount (log) by Term

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
Conventional 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
Conventional 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.

C.2. RDD Plots

Figure 3: RDD plots for Infraction Count and Amount



D. When Poverty Increases

Table 12: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.748	0.430	1.049	0.379	0.540	0.525
	(0.946)	(1.321)	(0.758)	(1.322)	(0.949)	(1.166)
Observations	756	756	706	706	582	582
Effective observations	[201,216]	[235,290]	[204,274]	[220,286]	[139,185]	[193,278]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.429	0.745	0.166	0.774	0.569	0.652
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.109	0.154	0.145	0.159	0.111	0.187
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	1.004	0.389	0.844	0.926	0.591	0.673
	(0.817)	(1.327)	(0.912)	(1.092)	(1.066)	(1.215)
Observations	756	756	706	706	582	582
Effective observations	[227,272]	[235,290]	[190,214]	[254,324]	[135,171]	[189,274]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.219	0.769	0.355	0.397	0.579	0.580
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.139	0.153	0.114	0.192	0.0968	0.180

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 13: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	3.348	2.544	-0.210	-1.162	-2.342	1.740
	(4.026)	(5.221)	(4.534)	(5.747)	(4.700)	(6.485)
Observations	246	246	220	220	194	194
Effective observations	[67,68]	[83,94]	[54,59]	[68,86]	[46,55]	[55,71]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.406	0.626	0.963	0.840	0.618	0.789
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0997	0.157	0.0897	0.149	0.0914	0.127
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	3.472	1.676	2.558	2.775	0.868	1.744
	(2.372)	(4.106)	(2.955)	(3.758)	(3.451)	(4.648)
Observations	246	246	220	220	194	194
Effective observations	[87,96]	[83,93]	[62,68]	[79,96]	[48,55]	[56,77]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.143	0.683	0.387	0.460	0.801	0.708
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.163	0.156	0.115	0.180	0.0946	0.138

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.252	-0.212	0.286	-0.215	0.315	-0.118
	(0.312)	(0.473)	(0.298)	(0.479)	(0.302)	(0.500)
Observations	754	754	704	704	580	580
Effective observations	[212,232]	[220,268]	[199,248]	[203,262]	[146,210]	[146,226]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.419	0.654	0.336	0.653	0.297	0.814
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.117	0.134	0.128	0.138	0.121	0.131
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.287	-0.242	0.360	0.147	0.264	0.268
	(0.301)	(0.474)	(0.276)	(0.422)	(0.323)	(0.419)
Observations	754	754	704	704	580	580
Effective observations	[220,246]	[220,264]	[207,274]	[235,292]	[138,181]	[182,265]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.342	0.610	0.192	0.728	0.414	0.523
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.122	0.132	0.147	0.168	0.110	0.172

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.590	0.282	0.319	0.0640	0.215	0.280
	(0.360)	(0.587)	(0.421)	(0.625)	(0.419)	(0.600)
Observations	246	246	220	220	194	194
Effective observations	[76,75]	[77,86]	[57,64]	[66,83]	[49,58]	[57,79]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.101	0.631	0.448	0.918	0.608	0.640
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.120	0.135	0.102	0.136	0.105	0.146
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.605*	0.139	0.526	0.210	0.501	0.345
	(0.321)	(0.593)	(0.348)	(0.556)	(0.369)	(0.567)
Observations	246	246	220	220	194	194
Effective observations	[77,83]	[77,81]	[65,74]	[67,83]	[52,62]	[56,78]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0599	0.814	0.130	0.706	0.174	0.543
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.131	0.124	0.122	0.137	0.115	0.143

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 Extreme Poverty Increases

Table 16: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	1.120	0.800	1.472	1.212	0.686	1.844
	(1.085)	(1.455)	(0.908)	(1.247)	(1.340)	(2.157)
Observations	687	687	650	650	526	526
Effective observations	[186,182]	[226,254]	[192,220]	[246,280]	[127,133]	[136,166]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.302	0.582	0.105	0.331	0.609	0.393
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.110	0.159	0.133	0.192	0.0899	0.116
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	2.112	0.565	0.370	-0.851	-1.533	0.411
	(4.888)	(6.320)	(5.187)	(6.618)	(5.349)	(6.711)
Observations	224	224	205	205	179	179
Effective observations	[62,54]	[73,77]	[56,50]	[66,73]	[48,45]	[55,65]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.666	0.929	0.943	0.898	0.774	0.951
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0919	0.144	0.0883	0.144	0.0930	0.142

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.169	-0.0756	0.139	0.164	0.126	-0.142
	(0.354)	(0.501)	(0.354)	(0.411)	(0.353)	(0.557)
Observations	685	685	648	648	524	524
Effective observations	[195,200]	[213,242]	[181,188]	[251,280]	[137,168]	[138,190]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.633	0.880	0.695	0.690	0.720	0.799
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.117	0.151	0.116	0.203	0.119	0.132
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.360	0.0738	0.229	0.0170	0.156	0.298
	(0.446)	(0.702)	(0.489)	(0.692)	(0.488)	(0.579)
Observations	224	224	205	205	179	179
Effective observations	[65,60]	[71,72]	[57,54]	[66,71]	[49,48]	[64,77]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.420	0.916	0.639	0.980	0.750	0.607
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.113	0.132	0.0989	0.138	0.106	0.179

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.

F. Last Two Years of the Electoral Term

F.1. When Poverty Decreases (Final 2 Years of Term)

Table 18: RDD Estimates for Infraction Count by Year and Term (Final 2 Years of Term)

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-2.179**	-2.796**	-1.327	-1.773	-1.639*	-2.219*
	(0.874)	(1.114)	(0.928)	(1.136)	(0.979)	(1.261)
Observations	389	389	357	357	357	357
Effective observations	[100,80]	[118,96]	[88,64]	[106,86]	[88,62]	[92,70]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0127	0.0121	0.153	0.119	0.0943	0.0786
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0824	0.101	0.0735	0.0978	0.0717	0.0834
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
Conventional 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 19: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.253***	-1.415***	-1.016***	-1.208***	-1.004***	-1.162**
	(0.384)	(0.466)	(0.363)	(0.460)	(0.375)	(0.468)
Observations	388	388	356	356	356	356
Effective observations	[88,66]	[112,86]	[92,70]	[104,76]	[100,72]	[118,91]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00110	0.00240	0.00514	0.00867	0.00739	0.0129
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0684	0.0910	0.0827	0.0939	0.0888	0.116

	(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
Conventional 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.

F.2. When Extreme Poverty Decreases

Table 20: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.594**	-2.381**	-1.469	-1.908*	-1.494	-2.275*
	(0.800)	(1.042)	(0.928)	(1.159)	(0.998)	(1.304)
Observations	432	432	387	387	387	387
Effective observations	[109,102]	[121,116]	[84,86]	[104,100]	[76,74]	[82,80]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0463	0.0223	0.113	0.0997	0.134	0.0810
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0887	0.0972	0.0763	0.0955	0.0680	0.0721
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
Conventional 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 21: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.949**	-1.211***	-0.614*	-0.869*	-0.579	-0.766*
	(0.382)	(0.445)	(0.362)	(0.459)	(0.358)	(0.438)
Observations	431	431	386	386	386	386
Effective observations	[89,82]	[111,102]	[88,88]	[106,106]	[102,96]	[132,115]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0130	0.00646	0.0900	0.0584	0.106	0.0805
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0642	0.0895	0.0861	0.0978	0.0938	0.132
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
Conventional 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.

G. Final Year in Electoral Term

G.1. When Poverty Decreases

Table 22: 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
Conventional 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	-2.351**	-2.999**	-1.730	-1.837	-1.326	-1.393
	(1.011)	(1.439)	(1.094)	(1.530)	(1.277)	(1.631)
Observations	195	195	179	179	179	179
Effective observations	[67,53]	[65,52]	[53,42]	[57,44]	[50,36]	[57,44]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0200	0.0372	0.114	0.230	0.299	0.393
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.126	0.122	0.0966	0.109	0.0892	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 23: 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
Conventional 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	-1.163**	-1.115*	-0.903**	-1.001*	-0.818*	-0.999*
	(0.471)	(0.602)	(0.416)	(0.600)	(0.418)	(0.593)
Observations	195	195	179	179	179	179
Effective observations	[48,39]	[56,45]	[53,43]	[53,43]	[57,44]	[53,42]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0134	0.0639	0.0298	0.0953	0.0502	0.0920
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0752	0.0959	0.0998	0.0989	0.108	0.0975

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.2. When Extreme Poverty Decreases

Table 24: 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
Conventional 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	-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
Conventional 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 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 25: 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
Conventional 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	-0.876**	-0.928*	-0.435	-0.673	-0.468	-0.766
	(0.445)	(0.541)	(0.415)	(0.579)	(0.414)	(0.573)
Observations	217	217	194	194	194	194
Effective observations	[47,44]	[60,58]	[52,49]	[56,54]	[53,52]	[56,53]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0489	0.0866	0.295	0.245	0.258	0.181
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0708	0.0962	0.0944	0.103	0.0970	0.103

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.

H. First Two Years

H.1. When Poverty Decreases

Table 26: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.964	-0.956	-0.193	-0.201	-0.632	-1.219
	(0.754)	(1.024)	(0.824)	(1.024)	(0.928)	(1.213)
Observations	212	212	212	212	212	212
Effective observations	[72,42]	[74,42]	[62,34]	[70,42]	[66,36]	[70,42]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.201	0.351	0.815	0.845	0.496	0.315
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.105	0.0894	0.100	0.0927	0.100
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-6.659***	-6.364**	-5.179*	-6.792*	-4.115*	-5.665
	(2.432)	(2.717)	(2.714)	(3.770)	(2.475)	(3.637)
Observations	195	195	179	179	179	179
Effective observations	[62,49]	[86,76]	[52,40]	[59,46]	[55,43]	[57,44]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00618	0.0192	0.0564	0.0716	0.0964	0.119
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.106	0.192	0.0955	0.117	0.101	0.108

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.155	0.290	0.306	0.367	0.174	0.131
	(0.308)	(0.352)	(0.299)	(0.344)	(0.362)	(0.415)
Observations	210	210	210	210	210	210
Effective observations	[50,24]	[62,34]	[50,24]	[62,34]	[50,24]	[60,34]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.616	0.411	0.307	0.287	0.631	0.753
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0596	0.0887	0.0597	0.0893	0.0589	0.0861
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-5.829**	-6.524**	-4.512**	-5.180	-3.502	-3.581
	(2.350)	(3.309)	(2.193)	(3.638)	(2.174)	(3.552)
Observations	195	195	179	179	179	179
Effective Observations	[54,41]	[63,51]	[57,44]	[53,42]	[55,43]	[51,38]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0131	0.0486	0.0396	0.154	0.107	0.313
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0896	0.115	0.106	0.0971	0.101	0.0904

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 28: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.332*	-1.450	-0.785	-0.866	-0.911	-1.277
	(0.772)	(0.959)	(0.812)	(0.987)	(0.844)	(1.043)
Observations	238	238	238	238	238	238
Effective observations	[58,44]	[84,56]	[58,44]	[84,56]	[56,44]	[80,54]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0843	0.131	0.334	0.380	0.280	0.221
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0824	0.118	0.0811	0.116	0.0801	0.110
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-4.175*	-4.895	-5.005**	-6.061*	-5.512**	-6.228*
	(2.235)	(3.362)	(2.278)	(3.295)	(2.320)	(3.200)
Observations	217	217	194	194	194	194
Effective observations	[67,60]	[71,64]	[55,53]	[60,55]	[51,48]	[60,55]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0617	0.145	0.0280	0.0658	0.0175	0.0516
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.110	0.121	0.102	0.119	0.0928	0.116

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.187	0.267	0.204	0.272	0.234	0.109
	(0.355)	(0.390)	(0.353)	(0.396)	(0.362)	(0.418)
Observations	236	236	236	236	236	236
Effective observations	[52,34]	[70,48]	[52,34]	[64,44]	[52,34]	[64,44]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.598	0.495	0.563	0.492	0.518	0.795
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0609	0.0939	0.0651	0.0894	0.0641	0.0889
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-2.570	-2.617	-3.314*	-3.900	-3.676**	-3.804
	(1.832)	(3.183)	(1.839)	(3.008)	(1.763)	(2.997)
Observations	217	217	194	194	194	194
Effective observations	[75,65]	[64,59]	[60,55]	[55,53]	[58,54]	[53,52]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.161	0.411	0.0715	0.195	0.0371	0.204
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.130	0.102	0.119	0.102	0.111	0.0976

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. Results for the Whole Sample (i.e. When Poverty is not Considered)

Table 30: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.117	-0.506	0.192	0.0415	-0.165	-0.0762
	(0.621)	(0.900)	(0.598)	(0.880)	(0.668)	(0.886)
Observations	1,357	1,357	1,275	1,275	1,151	1,151
Effective observations	[421,379]	[467,453]	[420,401]	[446,473]	[327,310]	[395,441]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.850	0.574	0.748	0.962	0.804	0.932
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.119	0.146	0.133	0.158	0.110	0.160
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.0229	-0.179	0.388	0.490	0.0425	-0.0280
	(0.639)	(0.877)	(0.634)	(0.807)	(0.690)	(0.715)
Observations	1,357	1,357	1,275	1,275	1,151	1,151
Effective observations	[403,365]	[467,463]	[392,373]	[486,505]	[325,308]	[474,517]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.971	0.838	0.541	0.544	0.951	0.969
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.113	0.151	0.121	0.178	0.104	0.218

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 31: 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
Conventional 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
Conventional 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 32: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.177	-0.334	-0.0991	-0.181	-0.126	-0.237
	(0.220)	(0.291)	(0.217)	(0.290)	(0.226)	(0.311)
Observations	1,352	1,352	1,270	1,270	1,146	1,146
Effective observations	[388,353]	[459,435]	[369,337]	[432,439]	[318,307]	[365,377]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.422	0.251	0.649	0.534	0.578	0.445
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.141	0.110	0.149	0.102	0.137
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.143	-0.280	-0.0533	-0.0233	-0.0648	-0.100
	(0.225)	(0.291)	(0.222)	(0.270)	(0.218)	(0.293)
Observations	1,352	1,352	1,270	1,270	1,146	1,146
Effective observations	[376,352]	[459,439]	[365,335]	[470,487]	[326,308]	[379,407]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.526	0.335	0.811	0.931	0.766	0.732
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0994	0.141	0.104	0.171	0.106	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 33: RDD Estimates for Infraction Amount (log) by Term

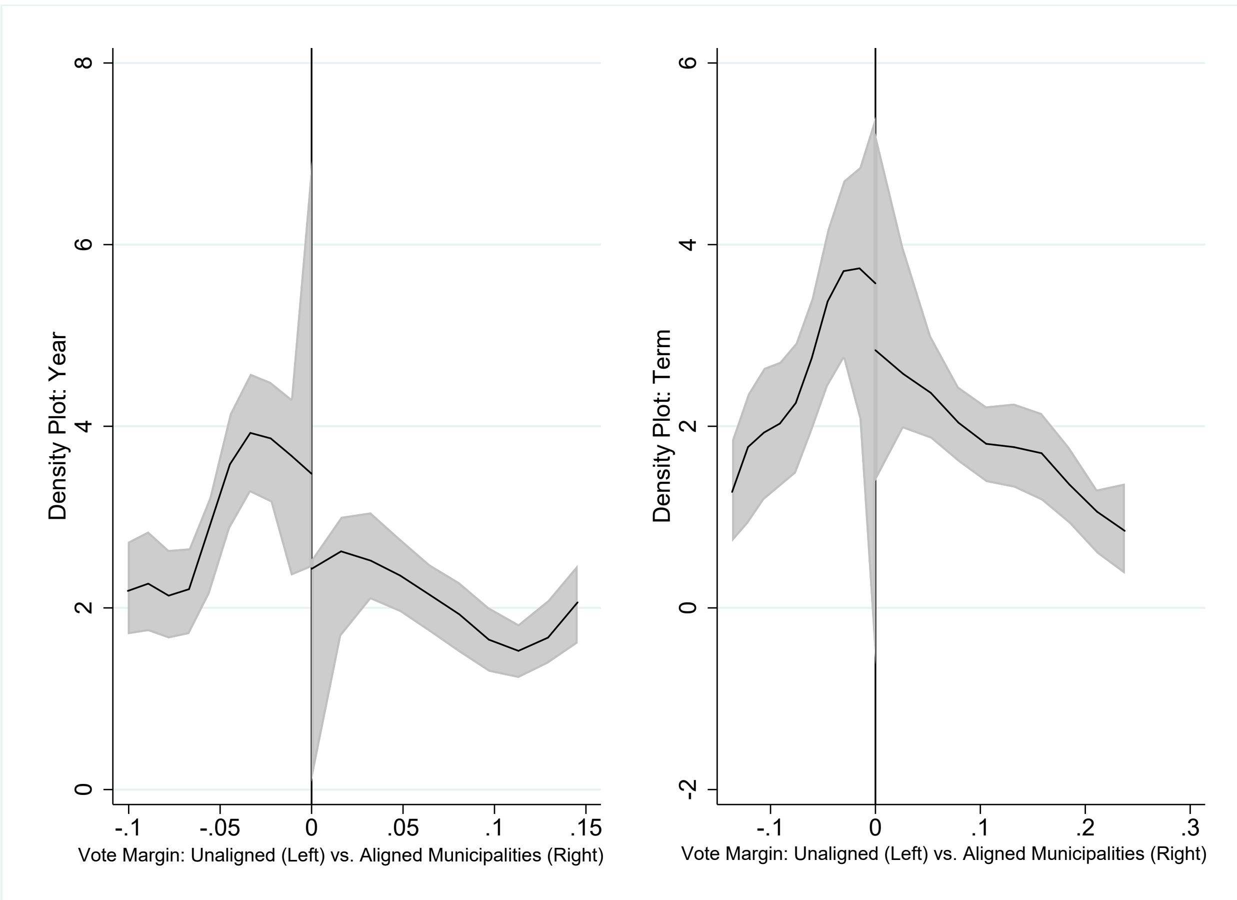
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.0559	-0.377	0.0278	-0.286	-0.198	-0.215
	(0.248)	(0.405)	(0.244)	(0.411)	(0.286)	(0.398)
Observations	440	440	398	398	372	372
Effective observations	[148,142]	[146,136]	[136,142]	[132,132]	[108,103]	[124,132]
Covariates	None	None	Some	Some	All	All
Conventional 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
Conventional 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.

J. RDD Robustness Checks

J.1. Density Plots: Year and Term

Figure 4: RDD Density Plots for Infraction Count and Amount



J.2. RDD Estimates Eliminating Outliers

Table 34: 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
Conventional 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	-1.633***	-2.187***	-1.023*	-1.361*	-1.452**	-1.957***
	(0.526)	(0.713)	(0.612)	(0.734)	(0.621)	(0.732)
Observations	591	591	559	559	559	559
Effective Observations	[189,138]	[179,138]	[142,98]	[167,118]	[131,82]	[144,102]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00190	0.00217	0.0945	0.0636	0.0193	0.00754
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.102	0.0982	0.0733	0.0949	0.0669	0.0808

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 35: 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
Conventional 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	-0.577**	-0.547*	-0.377	-0.414	-0.502*	-0.498
	(0.226)	(0.327)	(0.270)	(0.323)	(0.274)	(0.324)
Observations	585	585	555	555	555	555
Effective Observations	[178,134]	[158,112]	[126,74]	[152,102]	[130,74]	[172,126]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0107	0.0942	0.163	0.201	0.0670	0.124
Order of polynomial	1	2	1	2	1	2
Bandwidth	0.0970	0.0867	0.0598	0.0857	0.0647	0.0979

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.

J.3. RDD Estimates at Varying Cutoffs

Table 36: 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]
Conventional 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]
Conventional 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.

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