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. Our theoretical and empirical framework focuses on party alignment—i.e., when local-level politicians share the same party as the executive. Aligned politicians generally enjoy resource advantages due to their affiliation with the executive, but we show that close elections discipline aligned politicians to engage in less corruption after voters' economic circumstances improve. For identification, we rely on close-election regression discontinuity designs that analyze the number of audit violations committed and the amount of money misappropriated in Guatemalan municipalities. The results of our study help document how reductions in poverty decrease corruption through modernization, and how 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 is associated with lower development outcomes across numerous sectors of the economy,[2] and politics is often at the center of corrupt transactions. Although recent work on political corruption relies more on credible audit data than problematic perception-based measures,[3] scholarship still focuses more on identifying corruption than *reducing* it.[4]

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

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

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

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

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

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

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

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

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

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

When poverty declines, voters tend to rely less on clientelistic exchanges to meet basic needs and, in turn, vote more on the basis of programmatic (policy-based) appeals.[8] By reducing the need for "request-fulfilling",[9] we argue that reducing poverty leads to less voter tolerance of corrupt politicians as well, yielding a different landscape for political selection.[10] By contrast, under comparatively more difficult economic circumstances, voters are more supportive of aligned politicians because of their access to the spoils of the bureaucracy. With these resources, aligned politicians can buy voters' support. For their part, voters will be more likely to forgive corrupt politicians, as long as politicians redistribute part of the money back in the form of clientelistic transfers or discretionary spending. In such

2010).

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

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

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

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

environments, clientelistic linkages can be more compelling for voters because informational environments can be weak, and politicians' policy promises are typically not very credible.[11] Because non-aligned politicians do not have as large of a resource pool at their disposal, non-aligned politicians must rely more on valence appeals, which are less compelling in a context of poverty.

Winning in a close election amplifies the effects of poverty reduction on politicians' corruption levels. Such a situation signals to politicians that they have less room to capture rents if they wish to gain re-election in the electoral term—and obtain rents in the future. Given that politicians in most countries earn more in office than out of office,[12] reelection prospects drive politicians to temper their corruption levels if their close-election win gives them less ability to extract rents. Overall, our theory aims to depict how politics, political institutions, and economic development interact to reduce corruption through modernization.[13]

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

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

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

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

[13] By "modernization", we are referring to the prediction of modernization theory that economic growth or education leads to democratization (see Acemoglu and Robinson, 2018, 26).

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

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

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

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

In some but not all cases, alignment reduces corruption in municipalities that reduced levels of extreme poverty relative to the previous census as well, suggesting that the theory has broad reach. None of these results travel to municipalities in which the poverty rate increased from 2002 to 2011. When analyzing the full sample (i.e. not splitting the sample according to poverty increases or decreases), the results under all specifications are also statistically insignificant, suggesting the limits of current understanding of clarity of responsibility theory (see Schwindt-Bayer and Tavits, 2016). In future analyses, we aim to examine whether aligned politicians who are relatively corrupt are less likely to run for reelection in future electoral terms.[15]

The one drawback of current results is that after the 2015 election, there are no aligned

[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. Similarly, Cavalcanti, Daniele and Galletta (2018) find that disclosing audit reports regarding malfeasant mayors in Brazil results in different types of candidates running for office, though they find no effect for aligned mayors.

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.

At the broadest possible level, the results of this study help scholars better understand the causes of democratization and the extent to which modernization processes play a role. Daron Acemoglu, James Robinson, and their co-authors, for example, suggest that there is no direct evidence for the most prominent manifestations of modernization theory: that both increasing income and education lead to democratization (e.g. Acemoglu et al., 2005, 2008, 2009; Acemoglu and Robinson, 2018; Acemoglu et al., 2019).[16] We, of course, do not dispute these very comprehensive studies.[17] Nevertheless, our empirical results based on close-election data suggest a potential consequence of income-based modernization: the reduction of corruption. Although the shifts in poverty in our empirical results are not *de facto* exogenous to corruption, we find no evidence of empirical endogeneity (see Appendix K). Accordingly, in a similar way that Przeworski et al. (2000) suggest that democracies do not to backslide after reaching certain income levels, we interpret poverty reduction as placing subnational units within a polity on different corruption paths. To be clear, since poverty in our models relates to sampling, not inference, our conclusion regarding corruption, modernization, and democratization pertains to external validity, not internal validity.

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 the data, institutional context, and identification strategy underpinning this paper. Section 3 provides the main results for this paper. We supplement these results with an analysis of the poverty and alignment mechanisms in Section 4. Section 5 concludes.

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

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

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

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

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

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

Under Equation (2), we assume that c increases with r, meaning that the local-level

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

[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.

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 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]

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

[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.
[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, $r_{i,1}$.

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, 25; Stokes et al., 2013, Chapter 6).[24] 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).[25] 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 (Eggers and Hainmueller, 2009; Fisman, Schulz and Vig, 2014). Given Equations (3) and (4), the maximization problem for local-level politician i can be represented as:

$$\begin{aligned} &\max_{r_{i,1}} \; U(r_{i,1}) + \pi(s_{i,1}) \; U(r_{i,2}) + (1 - \pi(s_{i,1})) \; U(x_{i,2}), \\ &\text{where } s_{i,1} = \; W(g_{i,1}) + \beta_i^{1+a} \; W(\gamma r_{i,1}) \end{aligned} \tag{6}$$

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. When combined poverty reduction is combined with alignment, it further increases voter discount rates on aligned politicians' clientelistic activities, yielding repercussions for her reelection probabilities and future expected rents. The combination of poverty reduction, alignment,

[24] For further large-N, empirical evidence regarding the relationship between clientelism and poverty, refer Gonzalez-Ocantos, Kiewiet de Jonge and Nickerson (2014), Jensen and Justesen (2014), Szwarcberg (2015), and Muños (2019, 228-229). For qualitative evidence, refer to Chubb (1982) and Auyero (1999, 2000).

[25] 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."

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 administrative level 2 units akin to a state or province, the president appoints governors from his or her same political party. Accordingly, unlike many countries in Latin America, Guatemala does not have political variation at the department level. That is a boon for our identification strategy, which exploits municipal-presidential political party alignment.

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/unaligned 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.

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 and Samples for Estimation

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 when people have more than one unmet 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 are available are 2002 and 2011. Given the inability of regression discontinuity designs to accommodate interactions,[26] we use these poverty and extreme poverty data from these years to divide our sample into the following groups: poverty-increasing, poverty-decreasing, extreme poverty-decreasing, and extreme poverty-increasing municipalities. For comparison with the macro-level predictions of Schwindt-Bayer and Tavits (2016), we also provide estimations using the whole sample—i.e., not dividing the sample by the poverty rates changes.

We provide the aforementioned estimates by poverty or extreme poverty group both for the years 2010-2015 and 2011-2015. Given that a new electoral term started in 2012, both groups of estimates encompass more than one electoral term. To facilitate the analysis with 2010, we backdate the 2011 poverty rate measure by one year to cover 2010. This

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

is justifiable because census poverty measurements for 2011 took place between 2008-2011 (Instituto Nacional de Estadística de Guatemala, 2014), it is unlikely that estimates fluctuate from 2010 to 2011, and it is improbable that most citizens are aware or respond to INE's poverty rate announcements. Policy commitments and information are generally not very credible or abundant in a context of poverty like Guatemala, but people generally have a sense of whether their economic conditions are improving (Banerjee and Duflo, 2007, 2011; Keefer, 2004, 2007*a*,*b*; Keefer and Khemani, 2005; Keefer and Vlaicu, 2008). Following Ferraz and Finan (2008) and Bobonis, Fuertes and Schwabe (2016), the final two years of an electoral term are also the most significant in terms of corruption. The 2010-2015 sample, which encompasses the final two years of two electoral terms, thus represents the best possible sample for our analysis. In any case, as Appendix L shows, the results for 2011-2015 period show broadly similar patterns of those we highlight in the rest of the paper with the 2010-2015 sample.

2.4. Electoral Data

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

Given that the TSE's funding and capacity are limited (Meilán, 2016), we take additional steps to ensure that the data are not marred by electoral fraud and are suitable for analysis, etc. In Appendix J.1, we run a McCrary (2008) density test on our running variable. From both a yearly and electoral term perspectives, it passes the test in the whole sample as well as the poverty-decreasing and poverty-increasing samples.

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 J.4, municipalities with aligned party mayors are definitely not more likely to be audited than municipalities with non-aligned party mayors. That is accurate for all of the samples that we examine in this study (see Section 2.3). Accordingly, there are no concerns regarding the partisan distribution of audits.

For each audited municipality from 2004 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 of 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 log. We do not transform the number of infractions committed variable.

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

2.6. Other Data

Although most sharp regression discontinuity analyses typically assume that treatment assignment is as good as random within the data-driven bandwidth, we use Calonico et al.'s (2019) method to control for the influence of covariates within the bandwidth. We take covariate data on population and inequality (Gini Coefficient) from Guatemala's National Statistics Institute. We include data on public goods spending from the Guatemalan Ministry of Finance, which made its data publicly available through the World Bank's (2019) BOOST Initiative.[27] Table 2 presents descriptive statistics of these covariate data.

[27] 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.

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	6.00	184	6.286	126	5.985	194	5.233	90
Log Amount of Infractions: First 2 Years of Term	10.57	183	10.66	125	10.61	193	10.27	89
Number of Infractions: Last 2 Years of Term	6.071	395	6.428	222	6.433	383	5.591	181
Log Amount of Infractions: Last 2 Years of Term	10.89	395	10.83	222	10.92	382	10.71	181
Number of Infractions: Final Year of Term	6.894	198	7.387	111	7.370	192	6.242	91
Log Amount of Infractions: Final Year of Term	11.19	198	11.24	111	11.19	191	10.98	91

Panel B: Infractions (Electoral Term) VARIABLES	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	12.00	92	12.77	62	12.09	96	10.47	45
Log Amount of Infractions: First 2 Years of Term	11.44	92	11.63	62	11.58	96	11.08	45
Number of Infractions: Last 2 Years of Term	12.05	199	12.86	111	12.83	192	11.12	91
Log Amount of Infractions: Last 2 Years of Term	11.75	199	11.79	111	11.82	192	11.56	91
Number of Infractions: Final Year of Term	6.894	198	7.387	111	7.408	191	6.242	91
Log Amount of Infractions: Final Year of Term	11.19	198	11.24	111	11.19	191	10.98	91

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

3. Results

3.1. Corruption Results Disaggregated by Poverty

Figure 1 provides optimal data-driven regression discontinuity plots of our main results for corruption in the poverty-reducing samples. We present these plots using Calonico, Cattaneo and Titiunik's (2015) evenly-spaced variance method and second-degree polynomial fits following Gelman and Imbens (2019). For comprehensiveness, we estimate these results using both the number of municipal-level infractions committed and the log amounts associated with those infractions as the dependent variables.

The results are similar for both yearly and electoral term data: party alignment con-

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

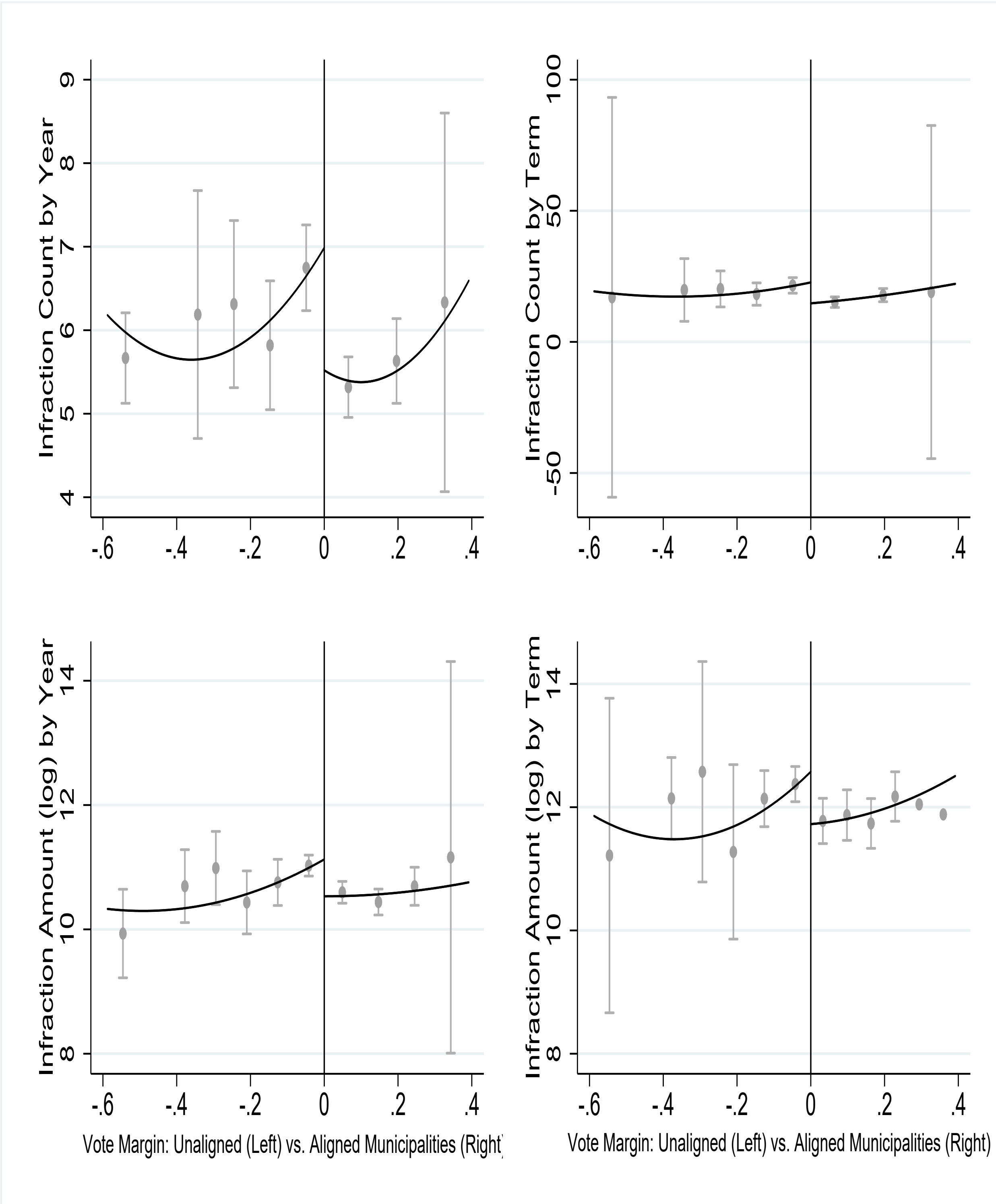


Table 2: Descriptive Statistics of Covariates

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.

sistently yields less corruption in the poverty-reducing sample. In Appendix B, Tables 6 and 7 present the results for the infractions dependent variable, and Tables 8 and 9 present the results for the infraction amounts dependent variables. For all of these tables, Panel A provides the results as typically presented in the literature (without fixed effects), whereas Panel B adds fixed effects in line with Frey (2019). The results are not only statistically significant but substantively significant as well. For example, in our base specification, aligned municipalities commit as average of 11.5 infraction less in each term or 1.5 infractions each year.

Controlling for the influence of covariates within the data-driven bandwidth in line with Calonico et al. (2019) does not alter the interpretation of our results. In Tables 36 and 37 of Appendix J, we further show that these results are not due to outliers. Similar to Brollo and Nannicini (2012), we fail only one of the ten placebo tests that we run in Appendix J.3, for which we test the effects of alignment at varying cutoffs. Given that tests reveal that poverty is not empirically endogenous to corruption (See Appendix K), the results for the poverty-reducing sample are robust.

The effects of alignment on reducing corruption in the poverty-reducing sample are more pronounced within the final two years of the electoral term. Tables 20 and 21 in Appendix F.1 show that the results for the last two years, and Tables 24 and Tables 25 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 28 and 29 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. In our case, that applies even to aligned politicians, who generally enjoy a resource advantage relative to non-aligned politicians (e.g. Brollo and Nannicini, 2012; Carozzi and Repetto, 2016; Corvalan, Cox and Osorio, 2018; Lara and Toro, 2019).

As predicted by our theory, alignment only reduces corruption in the poverty-reducing sample. Appendix D disaggregates results for the sample in which poverty increased from one census to next. As Tables 14, 15, 16, and 17 show, results generally shift in the opposite direction: municipalities in which poverty increased from one census to the next experienced an increase in corruption—again, measured by infractions the log amounts associated with those infractions. Theoretically, it is logical that poorer voters may be more forgiving of mayors' corruption, as long as the mayors share their rents with voters through clientelistic means. However, in none of the specifications for the poverty-increasing sample are results statistically significant, so we caution against in interpreting these results as definitive evidence of poverty facilitating aligned mayors to extract higher levels of rents.

For purposes of comparison with current predictions of clarity of responsibility theory (see Schwindt-Bayer and Tavits, 2016), Tables 32, 33, 34, and 35 in Appendix I show the results for the whole sample—i.e., when not disaggregating by poverty. Overall, these tables show very inconsistent findings. Sometimes, alignment yields less corruption; other times, it leads to more corruption. In all instances, though, none of the results are statistically significant. We thus interpret the results as evidence of the fact that alignment both provides resource advantages and increases clarity of responsibility. When not disaggregating the sample by poverty, these countervailing effects often cancel each other out, which is what the data show here.

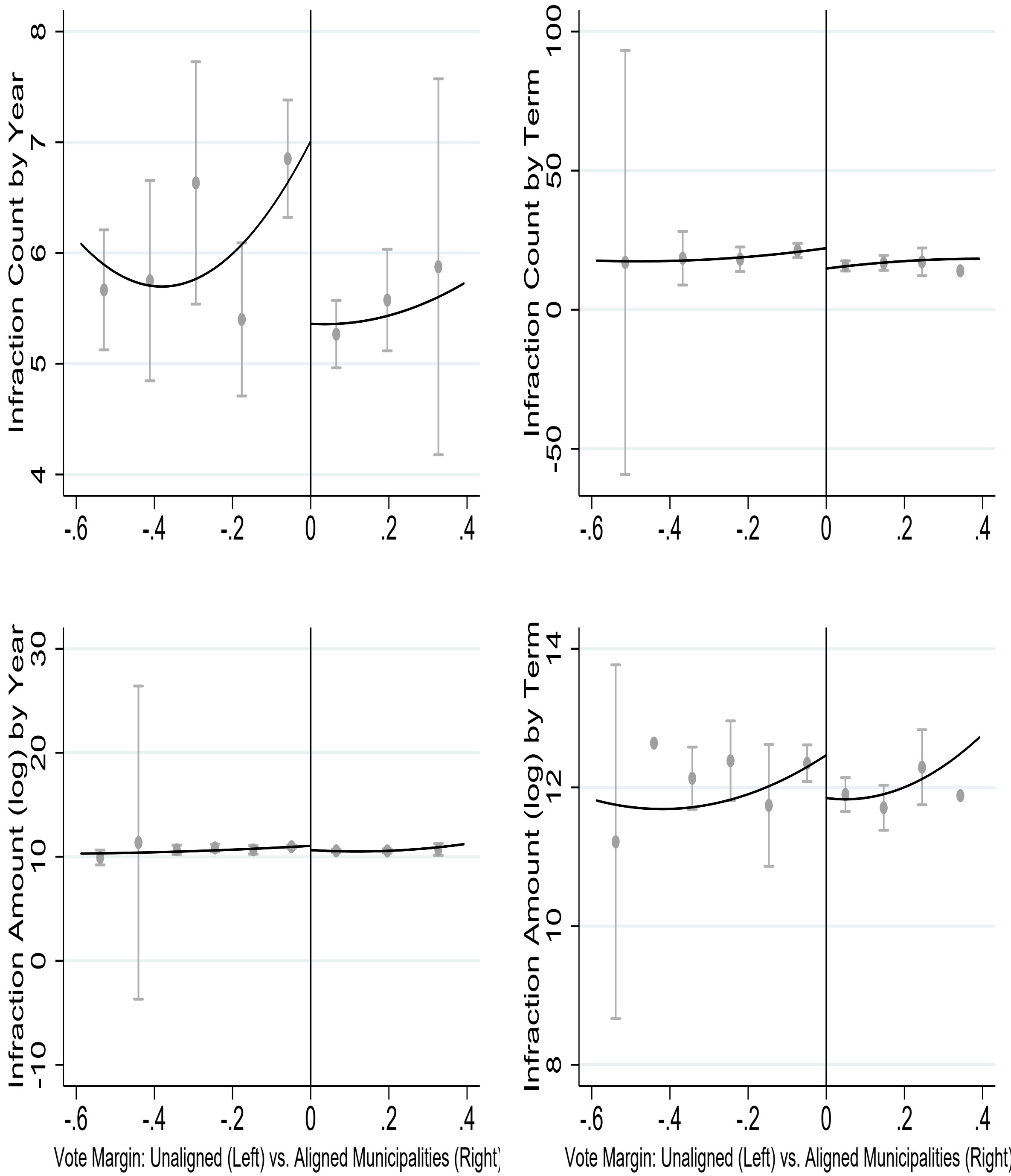
3.2. Corruption Results Disaggregated by Extreme Poverty

To further assess whether improving economic conditions can reduce corruption from aligned politicians, we also examine the extent to which reducing extreme poverty from one census to the next yields similar results as those of the poverty-reducing sample. Figure 2 shows the main results for extreme poverty reduction using Calonico, Cattaneo and Titiunik's (2015) evenly-spaced variance method and second-degree polynomial fits following Gelman and Imbens (2019). In all specifications, alignment reduces corruption when extreme poverty declines as well.

Tables 10, 11, 12, and 13 in Appendix C present the detailed results. In our base specification, aligned municipalities commit an average 6.8 less infractions each term or 1.5 less infractions each year. Results are a bit weaker for the log amounts, as not all specifications are statistically significant at conventional levels. Nevertheless, the results with the log amount as the dependent variable are still suggestive of the same overall pattern: reductions in extreme poverty make politicians reduce their overall corruption levels.

As with the previous subsection, the same results do not hold for the sample in which extreme poverty increased from one census to the next. We present the detailed results for the poverty-increasing sample in Tables 18 and 19 in Appendix E. In 21/24 specifications entailing counts of the number of infractions and the amounts associated with those infrac-

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



tions, the coefficient for alignment is positive, indicating that alignment yields an increase in corruption. However, similar to the previous for the poverty-increasing sample, none of the results are statistically significant for the sample in which extreme poverty increased.

4. Analysis of the Poverty and Alignment Mechanisms

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

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

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

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

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

In Guatemala's October 25, 2015 run-off election, the people a elected populist outsider, Jimmy Morales, as president. Since not a single candidate from Morales' party, National Convergence Front (FCN), won a mayoral race during the same general election, it ensured that there were no mayoral-presidential party alignments for the 2016-2019 period.[28] The

[28] Similar to the United States, elections in Guatemala take place late in the previous year, and the president

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

	Dependent Variable: Number of Infractions				
	(1)	(2)	(3)	(4)	(5)
Morales' Term	0.610***	0.472***	-0.242***	0.128***	0.137***
	(0.029)	(0.028)	(0.045)	(0.028)	(0.039)
Poverty Reduced		-0.079**	9.411***	-0.095***	-0.956
		(0.033)	(0.535)	(0.034)	(0.642)
Population (log)		0.139***	5.414***	0.119***	-0.200
		(0.026)	(0.291)	(0.026)	(0.348)
Re-elected Mayor		0.028	-0.015	0.063**	0.031
		(0.042)	(0.046)	(0.030)	(0.032)
Observations	1352	1071	1071	1071	1071
Term Fixed Effects	No	No	Yes	No	Yes
Year Fixed Effects	No	No	No	Yes	Yes

Standard errors clustered by municipality in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Negative binomial model following Allison and Waterman (2002)

Table 5: Log Amounts of Misappropriated Funds by Electoral Term (2007-2018)

	Dependent Variable: Misappropriated Funds (log)				
	(1)	(2)	(3)	(4)	(5)
Morales Term	0.781***	0.544***	-0.479***	1.807***	1.764***
	(0.105)	(0.106)	(0.131)	(0.182)	(0.365)
Poverty Reduced		-0.198**		-0.198**	
		(0.100)		(0.100)	
Population (log)		0.252***	6.863***	0.216***	0.405
		(0.063)	(0.928)	(0.062)	(1.564)
Re-elected Mayor		0.022	-0.050	0.084	0.020
		(0.117)	(0.126)	(0.104)	(0.116)
Observations	1352	1071	1071	1071	1071
R^2			0.127		0.259
Term Fixed Effects	No	No	Yes	No	Yes
Year Fixed Effects	No	No	No	Yes	Yes

Linear regression with standard errors clustered by municipality in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

takes power in January of the following year.

lack of alignments for the 2016-2019 period limits the ability of the results in the previous sections to travel to other instances party system instability. By the same token, the shock of electing a populist outsider and its consequent effects on alignment allows us to credibly identify the power of the alignment mechanism and thus support the results presented in Sections 3.1 and 3.2.

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

In all likelihood, the (quasi) natural experiment of Morales' election is not sufficient for the descriptive statistics presented in Table 3 to be interpreted own their own. We therefore supplement these descriptive statistics with the regression analyses presented in Tables 4 and 5. Each regression contains the main covariates used in our regression discontinuity analyses throughout the paper as well as the poverty reduction indicator used to construct our samples. We exclude the alignment variable because it is collinear with the Morales Term variable, which serves as our main independent variable for the analysis.

Consistent with our expectations, the Morales Term variable is mostly positive and highly statistically significant throughout. The number of infractions and the log amounts associated with those infractions increased following the election of populist outsider Jimmy Morales, so party system instability fuels corruption. Since the party system instability makes it more difficult for citizens to discern who is responsible for misgovernance due to the lack of alignments, mayors take advantage of the institutional configuration and oversee municipalities that commit more corruption.

4.2. Analysis of the Poverty Mechanism

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

If poverty is endogenous to corruption in our empirical analysis, it not appropriate to interpret the results presented in Sections 3.1 and 3.2 as causal. We therefore test for endogeneity between poverty and corruption in Appendix K.2. Since endogeneity entails a correlation between the independent variable (in our case, alignment) and the error term, we first directly test for such a relationship using two-stage regression analysis. In the first stage, we separately run a regression of poverty on each of corruption variables: the number of infractions committed and the log amounts associated with those infractions. In the second stage, we regress the residuals from the first-stage equation on each corruption variable. In all instances, the results suggest no overall relationship and R-squared values that are essentially 0, indicating that there is no endogeneity between poverty and corruption. Since the lack of enodgeneity is so critical to our results, we undertake a second set of regression analyses as well. More specifically, in Appendix K.1 we test whether corruption predicts poverty in a conventional linear regression. Using numerous specifications for both the year-wise and term-wise results, we find no empirical support for the proposition that poverty predicts corruption.

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

Consider, for example, Table 58 in Appendix M.1, which presents the number of infractions committed in the poverty-reducing sample. Under such circumstances, approximately 58% of aligned mayors are less corrupt than the median, whereas 42% are more corrupt than the median. For unaligned mayors in the poverty-reducing sample, the results present the opposite pattern: 67% of mayors are more corrupt than the median, and 32% of mayors are less corrupt than the median. We can find results that similarly confirm our theory in Table 60, which presents the distribution of amounts in the poverty-decreasing sample (see Appendix M.2). When the mayor is aligned, 69% of mayors are less corrupt than the median, whereas 31% of mayors are less corrupt than the median. For unaligned mayors the pattern again flips: 56% of mayors are more corrupt than the median, and 44% of mayors are less corrupt than the median. Overall, the combination of poverty and alignment is what contributes to differential municipal-level corruption patterns.

5. Conclusion

The above analysis shows that the combination of party alignment and poverty reduction decreases politicians' levels of corruption. Our results are similar, though a somewhat weaker, when party alignment dovetails with extreme poverty reduction. To credibly measure corruption, we used measures of municipal-level infractions and the (log) amounts of misappropriated money associated with those infractions. For inference, we rely on a regression discontinuity design, involving close mayoral races in Guatemala. Overall, from a measurement perspective, our paper undertakes a suite of checks that future scholars may follow to credibly analyze corruption outside a context with randomized audits like Brazil.[29]

Broadly, our paper helps scholars better understand when different economic circumstances and political/institutional configurations yield different levels of corruption. To

[29] See, for example, Ferraz and Finan (2008, 2011), Avis, Ferraz and Finan (2018), Cavalcanti, Daniele and Galletta (2018), and Zamboni and Litschig (2018).

disentangle the relevant mechanisms theoretically, we focused on the circumstances under which political party alignment produced more corruption or more clarity of responsibility for misgovernance. In the process, we showed how poverty mediates the relationship between alignment and corruption. In other words, we showed that clarity of responsibility for misgovernance becomes more salient to voters in a developing country context only after economic circumstances improves.

Appendix

A. Theoretical Derivation

We solve for the following problem for the local-level politician in as in Equation (6):

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

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 $z, k > 0$.[30] ■

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

B. When Poverty Decreases

Table 6: RDD Estimates for Infraction Count by Year

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

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

Table 7: 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 8: RDD Estimates for Infraction Amount (log) by Year

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

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

Table 9: 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 10: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.487***	-1.966***	-1.193*	-1.368*	-1.344**	-2.044**
	(0.559)	(0.731)	(0.631)	(0.771)	(0.653)	(0.811)
Observations	670	670	625	625	625	625
Effective observations	[191,162]	[203,172]	[144,132]	[196,161]	[140,130]	[172,144]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00786	0.00717	0.0588	0.0758	0.0396	0.0117
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0952	0.102	0.0825	0.107	0.0770	0.0930
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.291**	-1.717**	-1.023	-1.155	-1.191*	-1.606**
	(0.588)	(0.759)	(0.648)	(0.786)	(0.673)	(0.809)
Observations	670	670	625	625	625	625
Effective observations	[191,162]	[213,173]	[144,132]	[196,161]	[140,130]	[188,160]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0280	0.0236	0.114	0.142	0.0769	0.0471
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0946	0.105	0.0819	0.110	0.0767	0.101

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

Table 11: 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 12: RDD Estimates for Infraction Amount (log) by Year

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

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

Table 13: 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.

D. When Poverty Increases

Table 14: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.570	0.824	0.519	0.975	0.545	0.965
	(0.942)	(1.566)	(1.030)	(1.627)	(1.082)	(1.585)
Observations	605	605	562	562	562	562
Effective observations	[159,198]	[159,234]	[130,176]	[138,222]	[130,168]	[138,228]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.545	0.599	0.614	0.549	0.615	0.543
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.118	0.133	0.101	0.131	0.0969	0.135
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.415	0.521	0.547	0.919	0.590	0.956
	(0.983)	(1.549)	(1.090)	(1.599)	(1.090)	(1.560)
Observations	605	605	562	562	562	562
Effective observations	[155,194]	[159,236]	[130,164]	[138,224]	[130,168]	[138,230]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.673	0.737	0.616	0.565	0.588	0.540
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.135	0.0955	0.131	0.0965	0.136

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

Table 15: RDD Estimates for Infraction Count by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	4.224	6.547	1.230	3.371	-2.481	3.347
	(3.797)	(6.035)	(4.063)	(6.561)	(4.466)	(8.440)
Observations	196	196	174	174	174	174
Effective observations	[55,62]	[57,76]	[44,56]	[46,71]	[44,55]	[44,57]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.266	0.278	0.762	0.607	0.578	0.692
Order of Polynomial	1	2	1	2	1	2
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	1.448	1.881	1.701	3.016	0.943	2.634
	(3.180)	(4.584)	(3.540)	(5.265)	(3.740)	(5.100)
Observations	196	196	174	174	174	174
Effective observations	[54,59]	[59,79]	[43,53]	[46,67]	[41,52]	[46,71]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.649	0.682	0.631	0.567	0.801	0.606
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.109	0.144	0.0958	0.128	0.0921	0.133

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

Table 16: RDD Estimates for Infraction Amount (log) by Year

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

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

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.600	0.386	0.418	0.462	0.301	0.172
	(0.369)	(0.548)	(0.395)	(0.468)	(0.388)	(0.591)
Observations	196	196	174	174	174	174
Effective observations	[55,61]	[60,79]	[44,58]	[59,88]	[45,60]	[48,75]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.104	0.481	0.290	0.324	0.438	0.772
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.115	0.147	0.114	0.204	0.116	0.150
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.516	-0.128	0.554	0.494	0.599*	0.530
	(0.329)	(0.651)	(0.346)	(0.438)	(0.334)	(0.449)
Observations	196	196	174	174	174	174
Effective observations	[57,74]	[57,64]	[46,69]	[59,88]	[46,73]	[58,86]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.117	0.844	0.109	0.259	0.0728	0.238
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.133	0.118	0.131	0.204	0.137	0.188

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

E. When Extreme Poverty Increases

Table 18: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.913	1.191	0.920	2.295	0.656	1.567
	(0.967)	(1.772)	(1.068)	(2.156)	(1.274)	(1.891)
Observations	536	536	506	506	506	506
Effective observations	[148,196]	[142,192]	[128,158]	[128,158]	[124,144]	[130,184]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.345	0.501	0.389	0.287	0.607	0.407
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.140	0.135	0.115	0.116	0.0995	0.129
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	2.999	3.247	1.366	4.537	1.946	6.492
	(4.751)	(6.555)	(4.782)	(7.647)	(3.698)	(9.507)
Observations	174	174	159	159	159	159
Effective observations	[49,47]	[54,67]	[43,46]	[45,56]	[47,63]	[43,47]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.528	0.620	0.775	0.553	0.599	0.495
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.105	0.152	0.110	0.128	0.149	0.112

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

Table 19: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.179	-0.132	0.108	-0.0204	0.103	-0.0827
	(0.335)	(0.694)	(0.359)	(0.727)	(0.365)	(0.665)
Observations	534	534	504	504	504	504
Effective observations	[141,186]	[135,154]	[123,152]	[123,144]	[123,150]	[123,150]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.592	0.850	0.763	0.978	0.779	0.901
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.129	0.112	0.113	0.100	0.112	0.113
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.323	0.239	0.213	0.119	0.140	0.137
	(0.438)	(0.548)	(0.458)	(0.700)	(0.454)	(0.764)
Observations	174	174	159	159	159	159
Effective observations	[51,55]	[64,80]	[44,51]	[46,61]	[45,52]	[45,57]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.461	0.664	0.642	0.865	0.757	0.858
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.121	0.191	0.117	0.142	0.119	0.129

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

F. Last Two Years of the Electoral Term

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

Table 20: RDD Estimates for Infraction Count by Year and Term (Final 2 Years of Term)

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.945**	-2.643**	-1.156	-1.742	-1.405	-2.250*
	(0.825)	(1.082)	(0.866)	(1.112)	(0.923)	(1.248)
Observations	389	389	357	357	357	357
Effective observations	[112,90]	[124,97]	[96,72]	[112,87]	[92,68]	[94,70]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0184	0.0146	0.182	0.117	0.128	0.0715
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0952	0.108	0.0875	0.103	0.0810	0.0870
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-4.107**	-5.470**	-2.584	-3.540	-2.591	-3.781
	(1.720)	(2.208)	(1.795)	(2.229)	(1.967)	(2.481)
Observations	194	194	178	178	178	178
Effective observations	[53,41]	[62,49]	[45,34]	[57,44]	[44,33]	[52,38]
Covariates	None	None	Some	Some	All	All
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 21: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.226***	-1.295***	-0.989***	-1.140**	-1.005***	-1.158**
	(0.374)	(0.449)	(0.349)	(0.452)	(0.357)	(0.460)
Observations	388	388	356	356	356	356
Effective observations	[96,72]	[118,96]	[104,76]	[110,86]	[106,86]	[120,93]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00104	0.00392	0.00457	0.0117	0.00487	0.0118
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0725	0.102	0.0919	0.102	0.0978	0.124
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B						
RD Estimate	-1.164***	-1.195**	-0.989**	-1.054**	-0.912**	-1.016**
	(0.425)	(0.511)	(0.410)	(0.514)	(0.384)	(0.512)
Observations	194	194	178	178	178	178
Effective observations	[48,37]	[62,49]	[46,34]	[56,43]	[53,43]	[59,46]
Covariates	None	None	Some	Some	All	All
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 22: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.503*	-2.201**	-1.291	-1.735	-1.323	-2.303*
	(0.773)	(1.019)	(0.847)	(1.119)	(0.940)	(1.238)
Observations	432	432	387	387	387	387
Effective observations	[117,108]	[133,119]	[102,96]	[116,107]	[84,86]	[94,90]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0519	0.0309	0.128	0.121	0.159	0.0628
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0937	0.105	0.0924	0.106	0.0768	0.0886
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-2.564	-3.693*	-2.765	-3.558	-3.696*	-4.846**
	(1.614)	(2.144)	(1.794)	(2.261)	(1.950)	(2.424)
Observations	216	216	193	193	193	193
Effective observations	[59,54]	[67,60]	[44,44]	[58,54]	[41,43]	[53,53]
Covariates	None	None	Some	Some	All	All
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 23: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.913**	-1.050**	-0.583*	-0.778*	-0.549	-0.705*
	(0.376)	(0.435)	(0.341)	(0.453)	(0.337)	(0.419)
Observations	431	431	386	386	386	386
Effective observations	[89,86]	[121,118]	[104,100]	[116,107]	[114,107]	[138,135]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0152	0.0157	0.0871	0.0861	0.104	0.0926
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0691	0.0994	0.0953	0.110	0.104	0.148
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.846**	-0.925*	-0.551	-0.748	-0.560	-0.807
	(0.405)	(0.472)	(0.385)	(0.505)	(0.347)	(0.505)
Observations	216	216	193	193	193	193
Effective observations	[47,44]	[65,60]	[50,48]	[58,54]	[58,54]	[60,55]
Covariates	None	None	Some	Some	All	All
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 24: 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	-2.359**	-3.003**	-1.733	-1.839	-1.447	-1.393
	(1.012)	(1.437)	(1.098)	(1.525)	(1.322)	(1.631)
Observations	194	194	178	178	178	178
Effective observations	[67,53]	[65,52]	[52,40]	[57,44]	[45,34]	[57,44]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0197	0.0367	0.115	0.228	0.273	0.393
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.125	0.122	0.0957	0.110	0.0792	0.109

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

Table 25: 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	-1.147**	-1.119*	-0.894**	-1.010*	-0.821*	-0.974*
	(0.460)	(0.603)	(0.438)	(0.603)	(0.425)	(0.591)
Observations	194	194	178	178	178	178
Effective observations	[49,39]	[56,45]	[51,38]	[53,42]	[56,44]	[55,43]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0127	0.0638	0.0412	0.0938	0.0533	0.0995
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0776	0.0952	0.0907	0.0975	0.103	0.101

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

G.2. When Extreme Poverty Decreases

Table 26: 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	-1.991*	-1.917	-1.446	-1.567	-1.839	-1.919
	(1.029)	(1.220)	(1.055)	(1.529)	(1.231)	(1.594)
Observations	216	216	193	193	193	193
Effective observations	[56,52]	[79,71]	[58,54]	[58,55]	[43,44]	[59,55]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0531	0.116	0.170	0.305	0.135	0.229
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0898	0.142	0.105	0.111	0.0835	0.113

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

Table 27: 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	-0.863**	-0.936*	-0.452	-0.675	-0.439	-0.748
	(0.437)	(0.546)	(0.435)	(0.579)	(0.391)	(0.570)
Observations	216	216	193	193	193	193
Effective observations	[49,47]	[60,55]	[46,45]	[56,53]	[58,54]	[58,54]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0484	0.0861	0.299	0.244	0.262	0.189
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0727	0.0944	0.0884	0.103	0.108	0.105

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

H. First Two Years

H.1. When Poverty Decreases

Table 28: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.970	-0.958	-0.195	-0.195	-0.606	-1.147
	(0.752)	(1.023)	(0.823)	(1.022)	(0.913)	(1.204)
Observations	212	212	212	212	212	212
Effective observations	[72,42]	[74,42]	[62,36]	[72,42]	[66,38]	[72,42]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.197	0.349	0.813	0.848	0.507	0.341
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.105	0.0900	0.101	0.0952	0.102
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-1.972	-1.945	-0.637	-0.415	-1.372	-2.357
	(1.503)	(2.092)	(1.577)	(2.094)	(1.747)	(2.454)
Observations	105	105	105	105	105	105
Effective observations	[37,21]	[36,21]	[36,21]	[34,21]	[35,21]	[36,21]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.189	0.352	0.686	0.843	0.432	0.337
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.109	0.103	0.103	0.0998	0.100	0.102

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

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

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.147	0.288	0.300	0.380	0.167	0.131
	(0.307)	(0.354)	(0.299)	(0.346)	(0.361)	(0.416)
Observations	210	210	210	210	210	210
Effective observations	[52,24]	[60,34]	[50,24]	[60,34]	[50,24]	[60,34]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.633	0.416	0.316	0.273	0.645	0.752
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0609	0.0866	0.0601	0.0864	0.0593	0.0860
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.0210	0.167	0.139	0.183	0.0848	-0.0701
	(0.391)	(0.448)	(0.375)	(0.436)	(0.479)	(0.557)
Observations	105	105	105	105	105	105
Effective observations	[26,12]	[31,17]	[26,12]	[33,18]	[25,12]	[30,17]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.957	0.709	0.711	0.676	0.859	0.900
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0612	0.0893	0.0611	0.0910	0.0578	0.0868

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

H.2. When Extreme Poverty Decreases

Table 30: RDD Estimates for Infraction Count by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.331*	-1.425	-0.791	-0.835	-0.898	-1.336
	(0.770)	(0.971)	(0.807)	(0.997)	(0.835)	(1.049)
Observations	238	238	238	238	238	238
Effective observations	[58,44]	[82,56]	[58,44]	[82,56]	[58,44]	[80,54]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0837	0.142	0.327	0.402	0.282	0.203
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0830	0.114	0.0824	0.113	0.0818	0.106
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-2.661*	-2.872	-1.601	-1.667	-1.923	-2.816
	(1.543)	(1.957)	(1.610)	(2.030)	(1.675)	(2.160)
Observations	118	118	118	118	118	118
Effective observations	[29,22]	[42,28]	[29,22]	[41,28]	[29,22]	[40,27]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0847	0.142	0.320	0.411	0.251	0.192
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0842	0.115	0.0845	0.112	0.0840	0.105

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

Table 31: RDD Estimates for Infraction Amount (log) by Year and Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.172	0.264	0.166	0.271	0.230	0.114
	(0.351)	(0.393)	(0.341)	(0.397)	(0.353)	(0.420)
Observations	236	236	236	236	236	236
Effective observations	[52,34]	[70,48]	[56,42]	[60,44]	[54,36]	[60,44]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.624	0.502	0.627	0.495	0.516	0.786
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0649	0.0915	0.0716	0.0869	0.0696	0.0868
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD_Estimate	-0.0279	0.167	-0.137	0.145	0.00202	-0.00694
	(0.385)	(0.472)	(0.360)	(0.473)	(0.387)	(0.524)
Observations	118	118	118	118	118	118
Effective observations	[28,22]	[35,24]	[31,22]	[35,24]	[28,22]	[33,23]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.942	0.723	0.704	0.759	0.996	0.989
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0766	0.0935	0.0880	0.0922	0.0779	0.0902

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

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

Table 32: RDD Estimates for Infraction Count by Year

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

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

Table 33: 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 34: RDD Estimates for Infraction Amount (log) by Year

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

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

Table 35: 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 3: RDD Density Plots for Infraction Count and Amount (Poverty-Decreasing Sample)

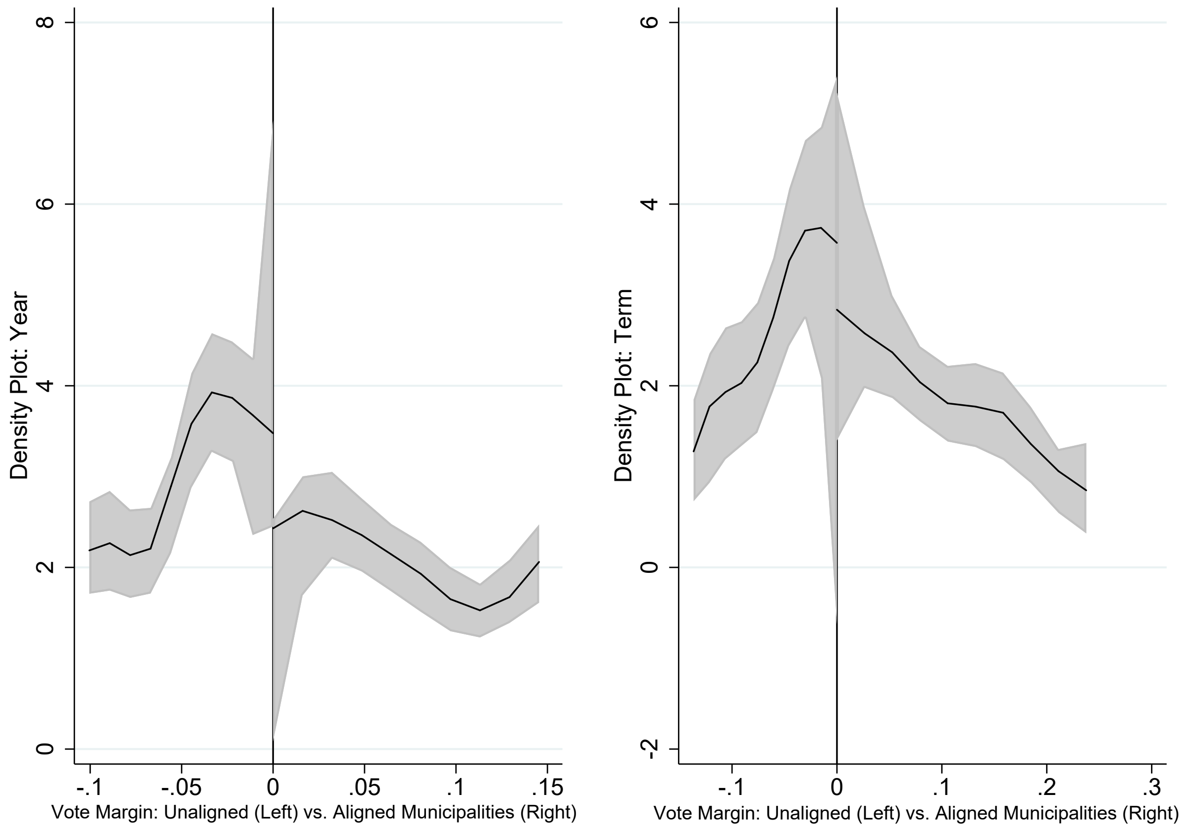


Figure 4: RDD Density Plots for Infraction Count and Amount (Poverty-Increasing Sample)

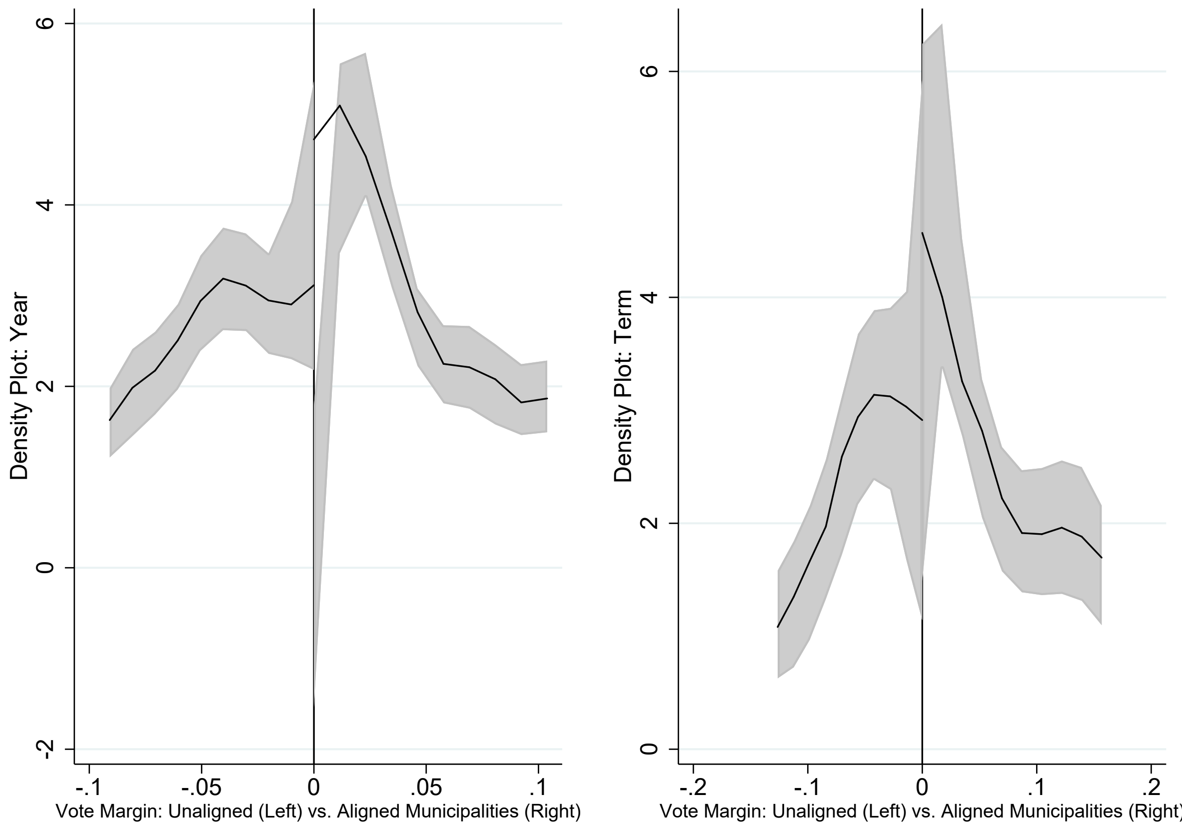
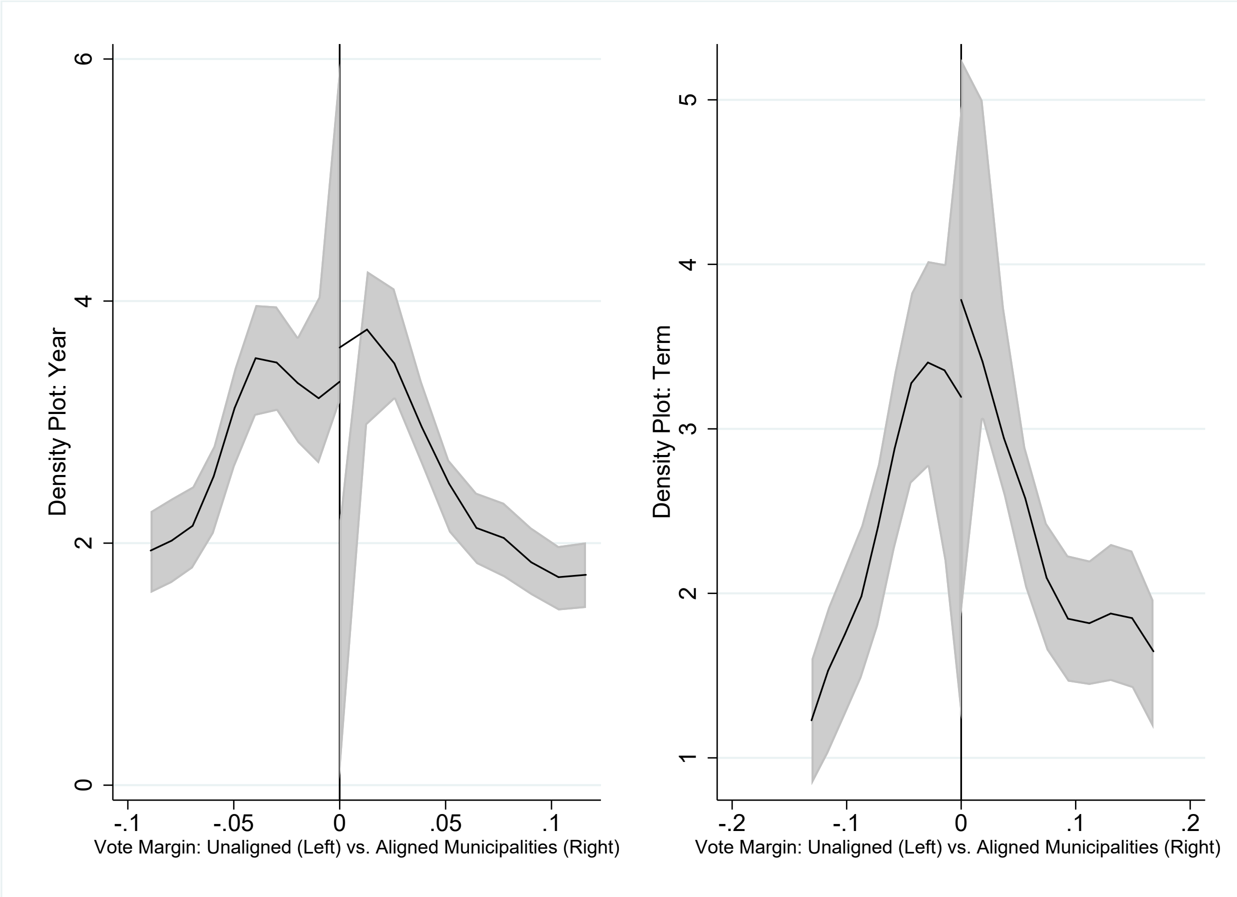


Figure 5: RDD Density Plots for Infraction Count and Amount (Whole Sample)



J.2. RDD Estimates Eliminating Outliers

Table 36: 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	-1.580***	-2.074***	-0.973*	-1.303*	-1.337**	-1.964***
	(0.509)	(0.696)	(0.571)	(0.715)	(0.599)	(0.718)
Observations	591	591	559	559	559	559
Effective observations	[198,143]	[195,139]	[148,104]	[179,128]	[142,98]	[156,106]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00189	0.00289	0.0886	0.0686	0.0257	0.00623
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.112	0.107	0.0841	0.102	0.0744	0.0882

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows results for term, while Panel B shows results year.To avoid excessive ommissions, Term results are winsorized at top/bottom 10% level, while Year results are winsorized at top/bottom 5%. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that Calonico, Cattaneo and Titiunik's (2014) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per Gelman and Imbens (2019), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

Table 37: 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	-0.618***	-0.474	-0.396	-0.361	-0.505*	-0.432
	(0.220)	(0.310)	(0.262)	(0.308)	(0.267)	(0.321)
Observations	585	585	555	555	555	555
Effective observations	[188,136]	[178,134]	[130,76]	[172,124]	[138,84]	[186,131]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00503	0.127	0.130	0.241	0.0589	0.178
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.0974	0.0663	0.0970	0.0706	0.111

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows results for term, while Panel B shows results year.To avoid excessive ommissions, Term results are winsorized at top/bottom 10% level, while Year results are winsorized at top/bottom 5%. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that Calonico, Cattaneo and Titiunik's (2014) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those preceding the comma on the left side of the cutoff, and observations after the comma corresponding to those on the right of the cutoff. Per Gelman and Imbens (2019), estimations only rely on polynomials of the first and second order. Columns 1 and 2 do not use any controls. Columns 3 and 4 use population (log) and a reelection dummy as controls. Columns 5 and 6 use population (log), reelection dummy, Gini coefficient, and log public goods spending (per capita) as controls.

J.3. RDD Estimates at Varying Cutoffs

Table 38: 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.

J.4. RDD Estimates for Number of Audits in a Term

Table 39: RDD Estimates for Poverty Decreasing Sample

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

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Variable of interest is the number of times a municipality gets audited in the term. All specifications use standard errors clustered by municipality. Bandwidth corresponds to the margin of victory on each side of the cutoff that Calonico, Cattaneo and Titiunik's (2014) data-driven algorithm deems to be a close election. Effective observations correspond to the observations that fall within the data-driven bandwidth—with those 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.

Table 40: RDD Estimates for Poverty Increasing Sample

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

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

Table 41: RDD Estimates for Whole Sample

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

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

K. Potential Endogeneity between Poverty and Corruption

K.1. Regression of Poverty Rate on Corruption

Table 42: Term-wise Regression of Poverty Rate on Count of Infraction

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

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

Table 43: Term-wise Regression of Poverty Rate on Amount of Infraction

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

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

Table 44: Year-wise Regression of Poverty Rate on Count of Infraction

	(1)	(2)	(3)
Infraction Count	0.0465	0.0509	0.0677
	(0.0807)	(0.0808)	(0.0759)
Population (log)		4.819	0.272
		(13.14)	(14.16)
Public Good Spending per capita (log)		-0.138**	-0.0990
		(0.0669)	(0.0701)
Constant	66.52***	18.16	62.78
	(0.923)	(133.9)	(144.2)
Observations	1,819	1,819	1,694
R-squared	0.016	0.016	0.027
Number of Municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Year FE	YES	Yes	Yes
Electoral Controls			Yes

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.

Table 45: Year-wise Regression of Poverty Rate on Amount of Infraction

	(1)	(2)	(3)
Infraction Amount (log)	0.217	0.238	0.117
	(0.179)	(0.182)	(0.179)
Population (log)		4.373	-0.381
		(13.16)	(14.27)
Public Good Spending per capita (log)		-0.267**	-0.189
		(0.113)	(0.156)
Constant	64.47***	21.19	68.93
	(1.933)	(134.0)	(145.2)
Observations	1,814	1,814	1,689
R-squared	0.017	0.018	0.027
Number of Municipalities	333	333	327
Municipality FE	Yes	Yes	Yes
Year FE	YES	Yes	Yes
Electoral Controls			Yes

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

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

Table 46: Year-wise Regression of Residuals on Count of Infraction

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

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

Table 47: Year-wise Regression of Residuals on Log Amount of Infraction

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

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

Table 48: Year-wise Regression of Residuals on Count of Infraction

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

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

Table 49: Year-wise Regression of Residuals on Amount of Infraction

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

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

L. RDD Results for Only 2011-2015

L.1. When Poverty Decreases

Table 50: RDD Estimates for Infraction Count by Year

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

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

Table 51: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-12.92***	-13.83***	-9.753**	-12.02**	-5.808*	-6.697*
	(3.888)	(4.346)	(4.428)	(5.515)	(3.256)	(3.805)
Observations	195	195	179	179	179	179
Effective observations	[56,43]	[74,70]	[45,34]	[59,46]	[45,34]	[57,44]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.000889	0.00146	0.0276	0.0294	0.0744	0.0784
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0930	0.160	0.0784	0.115	0.0761	0.109
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-2.910	-3.715	-0.811	-1.882	-1.178	-3.209
	(2.013)	(2.747)	(2.206)	(2.933)	(2.317)	(3.082)
Observations	195	195	179	179	179	179
Effective observations	[56,43]	[62,49]	[46,35]	[55,43]	[45,34]	[52,38]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.148	0.176	0.713	0.521	0.611	0.298
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0929	0.110	0.0839	0.101	0.0804	0.0934

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

Table 52: RDD Estimates for Infraction Amount (log) by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.643***	-0.473	-0.465*	-0.387	-0.527*	-0.459
	(0.246)	(0.330)	(0.269)	(0.334)	(0.275)	(0.340)
Observations	510	510	494	494	494	494
Effective observations	[167,111]	[159,110]	[129,85]	[163,106]	[129,85]	[176,112]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00894	0.151	0.0842	0.247	0.0554	0.177
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.101	0.0968	0.0771	0.101	0.0808	0.115
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.688***	-0.531	-0.487*	-0.472	-0.540*	-0.532
	(0.242)	(0.324)	(0.269)	(0.325)	(0.275)	(0.335)
Observations	510	510	494	494	494	494
Effective observations	[168,111]	[155,102]	[128,85]	[151,97]	[129,85]	[155,106]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00451	0.102	0.0702	0.146	0.0500	0.112
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.103	0.0945	0.0752	0.0955	0.0765	0.0982

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

Table 53: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-1.485***	-1.309*	-1.206***	-1.204*	-0.854**	-0.979*
	(0.506)	(0.678)	(0.422)	(0.619)	(0.394)	(0.505)
Observations	195	195	179	179	179	179
Effective observations	[49,39]	[51,40]	[52,38]	[47,35]	[56,44]	[47,35]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.00336	0.0537	0.00426	0.0517	0.0300	0.0523
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0762	0.0862	0.0941	0.0864	0.103	0.0848
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	-0.784**	-0.558	-0.618	-0.484	-0.684*	-0.622
	(0.368)	(0.543)	(0.381)	(0.565)	(0.380)	(0.563)
Observations	195	195	179	179	179	179
Effective observations	[57,48]	[56,43]	[54,43]	[52,38]	[57,44]	[52,38]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.0332	0.304	0.105	0.391	0.0713	0.270
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0989	0.0910	0.100	0.0934	0.105	0.0943

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

L.2. When Poverty Increases

Table 54: RDD Estimates for Infraction Count by Year

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.717	1.155	0.450	1.251	0.496	1.137
	(1.062)	(1.927)	(1.389)	(2.235)	(1.381)	(2.104)
Observations	517	517	495	495	495	495
Effective observations	[120,163]	[126,193]	[92,137]	[114,164]	[96,137]	[115,181]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.499	0.549	0.746	0.576	0.720	0.589
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.114	0.124	0.0856	0.116	0.0863	0.121
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.600	0.899	0.466	1.182	0.534	0.972
	(1.084)	(1.908)	(1.395)	(2.224)	(1.387)	(1.971)
Observations	517	517	495	495	495	495
Effective observations	[120,162]	[126,193]	[92,137]	[114,168]	[96,137]	[115,190]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.580	0.638	0.738	0.595	0.700	0.622
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.113	0.125	0.0852	0.116	0.0860	0.125

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

Table 55: RDD Estimates for Infraction Count by Electoral Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	5.162	8.191	0.791	2.865	-3.478	-1.646
	(4.621)	(6.961)	(5.001)	(7.448)	(4.558)	(7.965)
Observations	196	196	174	174	174	174
Effective observations	[54,58]	[57,73]	[43,55]	[46,71]	[43,52]	[44,56]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.264	0.239	0.874	0.700	0.445	0.836
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.104	0.132	0.101	0.133	0.0953	0.109
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	1.258	1.534	1.182	2.385	0.330	1.942
	(3.039)	(4.616)	(3.710)	(5.300)	(3.846)	(5.142)
Observations	196	196	174	174	174	174
Effective observations	[54,60]	[59,77]	[41,52]	[46,67]	[40,50]	[46,71]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.679	0.740	0.750	0.653	0.932	0.706
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.111	0.139	0.0913	0.128	0.0891	0.132

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

Table 56: RDD Estimates for Infraction Amount (log) by Year

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

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

Table 57: RDD Estimates for Infraction Amount (log) by Term

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	0.459	0.230	0.0219	-0.183	-0.0449	-0.0931
	(0.531)	(0.679)	(0.551)	(0.711)	(0.447)	(0.601)
Observations	196	196	174	174	174	174
Effective observations	[48,52]	[57,75]	[38,50]	[46,73]	[44,55]	[51,79]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.387	0.735	0.968	0.797	0.920	0.877
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0820	0.134	0.0855	0.136	0.106	0.158
Panel B	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
RD Estimate	0.220	-0.602	0.600*	-0.393	0.624*	-0.441
	(0.425)	(0.714)	(0.361)	(0.703)	(0.367)	(0.696)
Observations	196	196	174	174	174	174
Effective observations	[53,56]	[53,58]	[48,75]	[44,58]	[47,75]	[44,57]
Covariates	None	None	Some	Some	All	All
Conventional p-value	0.605	0.399	0.0962	0.576	0.0893	0.527
Order of Polynomial	1	2	1	2	1	2
Bandwidth	0.0962	0.103	0.151	0.114	0.144	0.111

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

M. Corruption Poverty Reducing/ Increasing Samples

M.1. Corrupt Mayors defined by Count of Infraction (Term 2012-2015)

Table 58: Poverty Reducing Sample (Term 2012-2015)

	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	26	19	45
	(57.78)	(42.22)	(100.00)
Not-Aligned	32	65	97
	(32.99)	(67.01)	(100.00)

Table 59: Poverty Increasing Sample (Term 2012-2015)

	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	25	38	63
	(39.68)	(60.32)	(100.00)
Not-Aligned	46	46	92
	(50.00)	(50.00)	(100.00)

M.2. Corrupt Mayor defined by Amount (log) of Infraction (Term 2012-2015)

Table 60: Poverty Reducing Sample

	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	31	14	45
	(68.89)	(31.11)	(100.00)
Not-Aligned	42	54	97
	(44.33)	(55.67)	(100.00)

Table 61: Poverty Increasing Sample (Term 2012-2015)

	Mayor Not Corrupt	Mayor Corrupt	Total
Aligned	29	34	63
	(46.03)	(53.97)	(100.00)
Not-Aligned	48	44	92
	(52.17)	(47.83)	(100.00)

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