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

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Abstract

Party alignment entails politicians sharing the same party at higher and lower levels of government, giving aligned subnational units greater access to discretionary resources. Does the political-institutional configuration of party alignment thus necessarily increase subnational corruption? Given that party alignment can also clarify political responsibility for voters, we theorize that it can instead reduce corruption under two conditions: high electoral competition and lower poverty. We examine the empirical implications of the theory using a close-election regression discontinuity design and novel corruption data from Guatemalan municipal audit reports. We find that municipalities barely electing aligned rather than unaligned mayors exhibit lower corruption when poverty is low or decreasing. The results suggest that the reduction of corruption through modernization forces takes place through political institutions.

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

In this study, we examine how corruption levels depend on whether subnational units have party alignment. It corresponds to when politicians' parties match at higher and lower levels of government. Examples include when a governor or mayor share the same political party as the president. Irrespective of its specific manifestation, party alignment facilitates clarity of political responsibility for misgovernance.[2]

Party alignment, however, does not only facilitate clarity of responsibility. For example, party alignment yields greater access to discretionary resources, which incites clientelism and unfair party competition (Greene, 2007, 2010). Similarly, the decentralization literature shows that party alignment fuels politically-motivated subnational spending and budget cycles in both rich and poor countries (e.g., de Haan and Klomp, 2013; Klomp and de Haan, 2013; Lago, Lago-Peñas and Martinez-Vazquez, 2024). Under what conditions, then, do subnational units with the resource advantages from party alignment engage in less corruption?

Using a political agency model that notably draws from Magaloni, Díaz-Cayeros and Estévez (2007) and Brollo and Nannicini (2012), we provide an interactive theory to explain when subnational units with aligned politicians engage in less corruption. To that end, the clarity of responsibility that alignment facilitates does not necessarily yield less corruption, but it may do so under two conditions. First, aligned politicians must represent subnational units where levels of poverty are low or have recently declined. These better economic

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

[2]Schwindt-Bayer and Tavits (2016, 1) define clarity of responsibility as "institutional and partisan arrangements that make it easy for voters to monitor their representatives, identify those responsible for undesirable outcomes, and hold them accountable by voting them out of office."

circumstances yield less voter demand for corrupt politicians and the clientelistic handouts that they bring, thereby fostering a transition to programmatic politics.[3] Second, aligned politicians must be susceptible to significant electoral competition, having won their position by a small margin of victory in the most recent election.

Both voter demand and politicians' supply constraints explain why subnational alignment is more likely to reduce corruption where poverty is lower and electoral competition is higher. Poverty matters primarily through voter demand. Because politicians' policy promises of public goods provision are generally not very credible in poorer countries, voters demand clientelistic handouts (Keefer, 2007*a*,*b*; Nichter, 2018). Subnational units with aligned politicians have greater supply-side access to discretionary resources to meet those demands, making clarity of responsibility from alignment insufficient to reduce corruption (Manzetti and Wilson, 2007; Brollo and Nannicini, 2012; Chang and Kerr, 2017; Leight et al., 2020; Curto-Grau, Solé-Ollé and Sorribas-Navarro, 2018; Bøttkjær and Justesen, 2021). When poverty declines, though, voters demand fewer clientelistic handouts and are less likely to tolerate corruption in exchange for clientelistic benefits (Kitschelt and Wilkinson, 2007; Lyne, 2008; Stokes et al., 2013). Under such settings, aligned politicians thus have less incentive to divert public resources in pursuit of their ultimate goal, reelection, and subnational units respond accordingly.

Electoral competition amplifies these patterns. Winning elections by small margins signals to aligned politicians that they have less ability to capture rents if they hope to win reelection. Politicians are sensitive to these constraints because, in most countries, they can earn more in office than as private citizens (Fisman, Schulz and Vig, 2014; Szakonyi, 2023). Parties face similar incentives: close races heighten voter attention, increase clarity of responsibility, and raise the cost of corruption scandals, encouraging parties to discipline aligned incumbents. These same incentives, however, are weaker for subnational units with

[3] By programmatic politics, we mean that the distribution of public resources has public rules, they are followed, and they are not targeted at a particular group or area (Hicken, 2011, 296; Stokes et al., 2013, 7). In this setting, politics operates more on the basis of ideas, policy, and party platforms than *ad-hoc* resource distribution.

unaligned politicians. Lower clarity of responsibility allows them to deflect blame on the opposition and not suffer the same reelection consequences when they misappropriate resources to meet voter demands for clientelistic spending. Nevertheless, lack of alignment yields fewer resources, forcing unaligned politicians to compete more on valence issues, which are often less compelling to voters in a context of poverty.

To support our theory that stresses how party alignment's conditional effect on corruption depends on both political competition and voters' economic circumstances, we examine new municipality-level data from Guatemala. The country is not only relatively poor and has a long history of clientelism and corruption but also, in 2019, expelled its United Nations-backed anti-corruption body, the International Commission Against Impunity (CICIG) (González, 2014; Sandberg and Tally, 2015; The Economist, 2019; Malkin, 2019). The myriad protests and international coverage CICIG's expulsion, as well the removal of former President Otto Pérez Molina on corruption charges, underscores the relevance of corruption for Guatemala's political discourse and democratic stability more broadly.

Unlike the corruption perceptions data that dominate the literature, our data correspond to actual measures of corruption that we draw from audit reports produced by the Guatemala's Comptroller General (*Contraloría General de Cuentas*). We subject these audit data to multiple validation tests and find no evidence of meaningful bias. Because the data are subnational, they also avoid the level-of-analysis problems that affect many cross-national studies (see Gingerich, 2013).

To operationalize whether a municipality is performing better economically, we specifically compare municipalities with low and high poverty levels (i.e., those above and below the median poverty level); municipalities with increased and decreased poverty rates relative to the previous census; and all municipalities—i.e., not subsetting by poverty. To estimate the causal effect of municipalities barely electing aligned rather than unaligned mayors, we exploit a series of close-election regression discontinuity designs. While they do not identify the effects of leader characteristics (Marshall, 2024), they do identify municipal-level effects

(Bertoli and Hazlett, 2025).

We find that municipalities with aligned mayors exhibit a significant decrease in both of our measures of corruption when poverty is lower or decreasing. In our base specification for infractions committed in each electoral term, aligned municipalities commit an average of 10.98 fewer infractions in the decreased-poverty sample, and 6.09 fewer infractions in the low-poverty sample. Numerous robustness checks show similar patterns, including with the log amount regressions.

In most cases, municipalities barely electing aligned rather than unaligned mayors exhibit lower corruption in the low- or decreasing-extreme-poverty samples as well, suggesting that the theory has broad reach. Consistent with our theory, none of these results travel to municipalities in the high-poverty or poverty-increasing samples. Analysis of the full sample (i.e., not splitting the sample according to poverty levels or changes) provides results that are similarly consistent with our theory. Notably, all specifications in the full sample are substantively and statistically insignificant, suggesting some scope conditions to clarity of responsibility theory.

At the broadest possible level, the results of this study help scholars re-evaluate how the institutional and modernization approaches to corruption dovetail.[4] As Fisman and Golden (2017, 15-16) explain, previous research has not found much empirical support for the modernization approach in poor countries. We would argue that is the case because poverty cannot be analyzed in isolation from the institutions that cause it (Acemoglu, Johnson and Robinson, 2005). Along these lines, municipalities barely electing aligned rather than unaligned mayors exhibit lower corruption only when political competition is high and poverty is lower. We find the same patterns when examining both the effects of short-term poverty changes and longer-term poverty levels, and the poverty and corruption data are not endogenous (see Appendix L). Accordingly, our robust results speak to previous litera-

[4]By "modernization", we are referring to the prediction of modernization theory that economic growth or education leads to democratization (see Acemoglu and Robinson, 2018, 26). Within the corruption literature, the modernization approach "views corruption as a product of poverty" (Fisman and Golden, 2017, 15).

ture suggesting that a strong economy allows politicians to get away with corruption (e.g., Manzetti and Wilson, 2007; Klašnja and Tucker, 2013; Zechmeister and Zizumbo-Colunga, 2013; Schleiter and Tavits, 2018). Under the right electoral conditions, our results suggest that a better economy can also discipline politicians with greater access to discretionary resources.

1. Theoretical Model

1.1. Model Setup

Building on Magaloni, Díaz-Cayeros and Estévez (2007) and Brollo and Nannicini (2012), we develop a two-period political agency model to explain how party alignment affects subnational corruption in a developing country setting (Barro, 1973; Ferejohn, 1986). The model clarifies how local-level politicians (agents) extract rents in the current term, accounting for future reelection consequences from voters (principals) and expected future rents. The central trade-off is that party alignment can both increase clarity of responsibility and resources that fuel corruption. To isolate that trade-off, we abstract away from politician types, effort, and a voter reservation utility (see Besley, 2006).

We represent local-level politician i's budget constraint as:

$$b_i = g_i + r_i \tag{1}$$

where b_i is the total budget,[5] g_i denotes spending on public goods and services, r_i denotes total rents. The latter consists of both money set aside for clientelism, c, and the personal benefits of public office (corruption), p:

$$r = c + p \tag{2}$$

[5]Without a loss of generality, we assume b is exogenous and normalized to 1. We recognize that b could decrease as a result of corruption and/or clientelism in previous periods, but we assume exogeneity for simplicity purposes.

where $c = \gamma r$ and $\gamma \in (0, 1)$, capturing the unobserved share of rents devoted to clientelism. Because voters trade-off the value of corrupt politicians against the clientelistic benefits that they can bring, $c = 0$ is infeasible given politicians' reelection goals (Manzetti and Wilson, 2007; Chang and Kerr, 2017; Leight et al., 2020; Botero et al., 2021).

Following Ferraz and Finan (2011) and Niehaus and Sukhtankar (2013), we distinguish between rents in the current period, $r_{i,1}$, and expected rents in a future period, $r_{i,2}$, where:

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

We represent local-level politician i's chance of gaining reelection with π, where $\pi' > 0, \pi'_{MV} > 0$ and $\pi'' < 0$, and MV corresponds to politician i's margin of victory in the last election.[6] Re-election, π, is also dependent on constituents' levels of satisfaction with the local-level politician, s_i, which we define for the current period as follows:

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

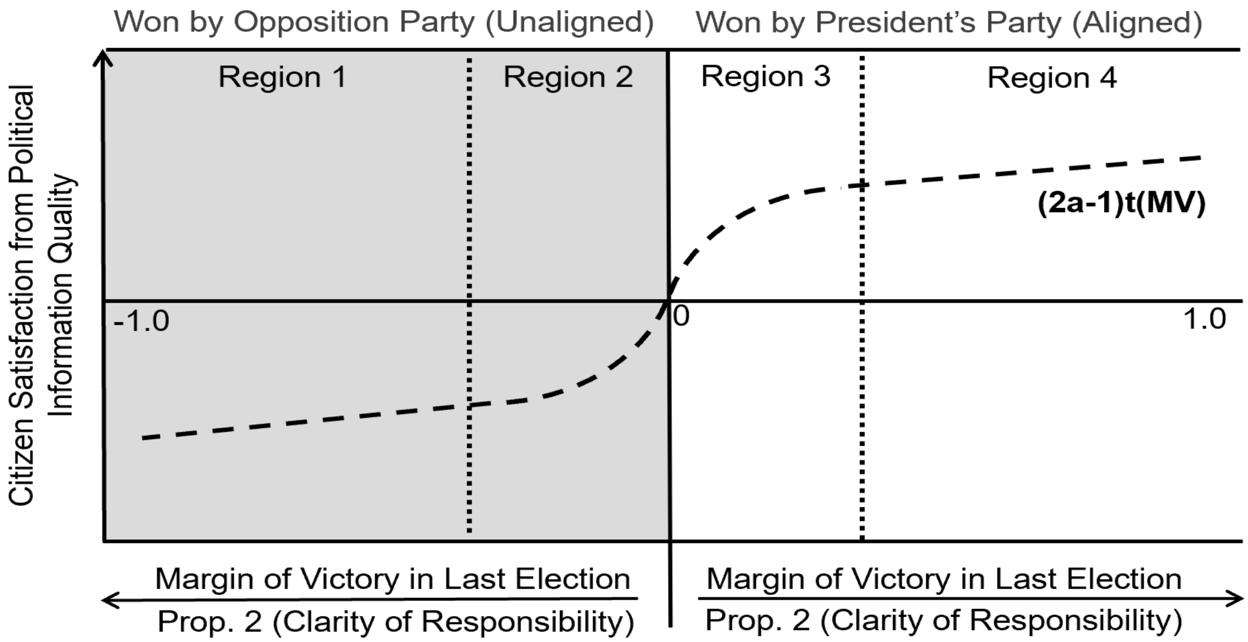
where $W(\cdot)$ increasing and concave; a is an indicator for party alignment; $t(\cdot)$ is an increasing and concave function capturing citizens' satisfaction from clarity of responsibility given MV; and β_i represents that effect of low or decreased poverty on citizens' pre-existing discount rates of clientelistic benefits that corrupt politicians may bring.

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

The model incorporates two independent channels through which clarity of responsibility affects voter satisfaction levels and, in turn, politicians' rent extraction levels.

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

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



Following Brollo and Nannicini (2012, 745) and Curto-Grau, Solé-Ollé and Sorribas-Navarro (2018, 782), we capture the direct information channel with $(2a-1)t(MV)$ and depict it in Figure 1. Under alignment ($a = 1$) in Regions 3 and 4, clarity of responsibility is highest and increases with MV, enabling voters to make more snap judgments. By contrast, under divided government ($a = 0$) in Regions 1 and 2, voters have more difficulty with responsibility attribution. Regions 2 and 3 contain the most movement in $(2a-1)t(MV)$ because lower absolute MV attracts more party attention, candidate challenges, and political information in the run-up to the next election. Identity voting, politicians' inability to make credible policy promises to voters, and other political market imperfections amplify these patterns (e.g., Chandra, 2004; Keefer, 2007a,b; Pande, 2011).

The second channel concerns voter demand for clientelistic benefits from corrupt politicians (Nichter, 2018; Pavão, 2018). A large literature suggests that voters discount clientelistic benefits more relative to more programmatic ones when poverty is less of a concern (e.g., Stokes et al., 2013, Chapter 6). Our model accounts for the *additional*, individual-level discounting from low or decreased poverty on $W(\gamma r_{i1})$ through $\beta_i \in (0, 1)$. Given the clarity of responsibility from alignment (see Figure 1), we also posit that a magnifies the penalization imposed by β on $W(\gamma r_{i1})$, such that $\beta^{1+a} = \beta^{1+1} \implies \beta^2 < \beta^1$. When poverty is higher ($\beta = 1$), these effects dissipate, as $\beta^2 = 1$. Similar to Adida et al. (2020), our model thus offers a potential resolution to information's mixed record in fostering political accountability in poor environments (Keefer, 2007*a*,*b*; Pande, 2011; Chong et al., 2015; Dunning et al., 2019).

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

We introduce U(·), such that $U^{'} > 0$ and $U^{''} < 0$, to represent the full utility function that politician i aims to maximize:

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

Consistent with Brollo and Nannicini (2012), Equation (5) adds private income that politician i can earn out of office in a future period, $x_{i,2}$. We also specify that $x_{i,2} < r_{i,2}$, because politicians in high-corruption areas can generally earn more in office than as a private citizen (Fisman, Schulz and Vig, 2014; Szakonyi, 2023).

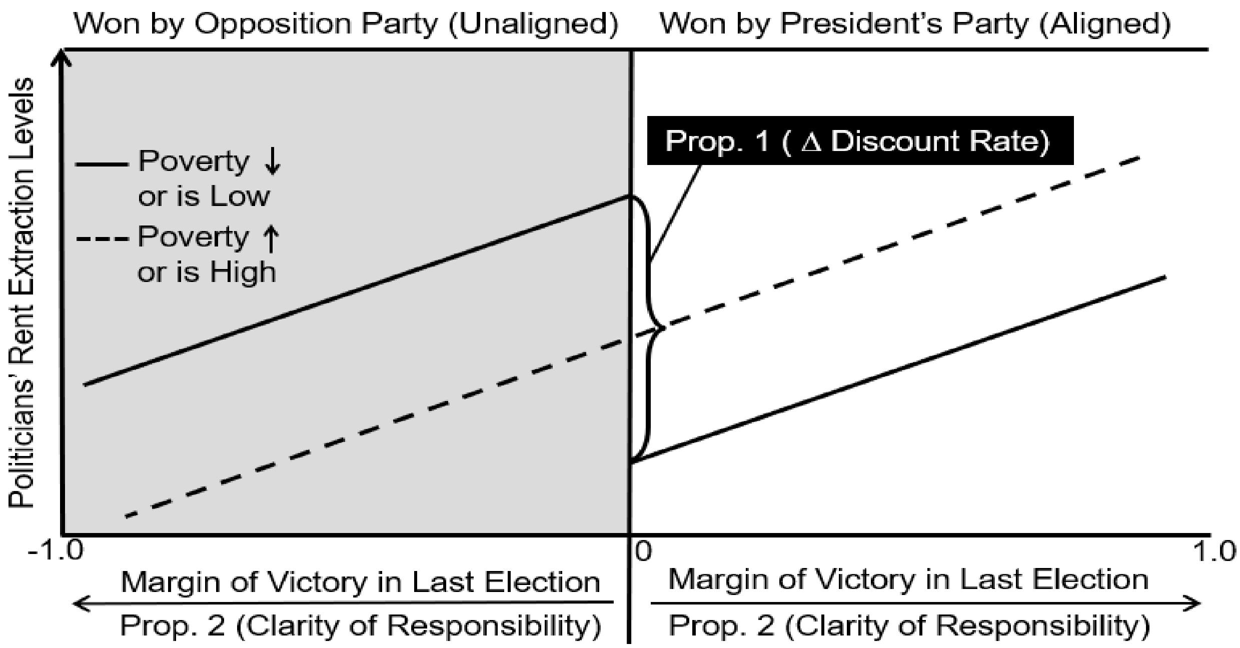
Proposition 1: *Optimal rents for aligned politicians are less than rents for unaligned politicians at the cutoff ($MV = 0$) under better economic circumstances.*

Under lower or declining poverty ($\beta < 1$), aligned voters' higher discounting ($\beta^2 < \beta^1$) reduces their politicians' reelection probabilities relative to unaligned ones, resulting in a discontinuity in rent extraction. See Figure 2 and the proof in Appendix C.

Proposition 2: *Rent extraction increases as aligned incumbents' seats become electorally safer, whereas rent extraction decreases as unaligned incumbents' seats become electorally safer.*

Figure 1 summarizes the logic, which does not depend on poverty. Aligned politi-

Figure 2: Graphical Presentation of Propositions 1 and 2



cians in safe seats (Region 4) have clarity of responsibility but face low accountability due to (semi-)guaranteed reelection, enabling them to extract more rents. As races become competitive (Region 3), parties discipline aligned incumbents more, and politicians fear for their reelection, prompting them to decrease rent extraction. For unaligned politicians, the pattern reverses. In safe seats (Region 1), citizens receive little political information, but unaligned politicians have fewer resources to extract and must compete more on valence appeals, limiting rent extraction. In competitive races (Region 2), electoral competition increases political information, obscures responsibility attribution, and makes clientelism more desirable, thereby increasing rent extraction. See Appendix C for the proof.

1.4. Summary of the Theoretical Results

A significant strand of the corruption literature argues that clarity of responsibility reduces corruption (e.g., Schwindt-Bayer and Tavits, 2016). However, another strand of the literature suggests that corruption increases with higher budgetary allocations (Brollo et al., 2013), where the latter often depend on the party alignment of the local-level politician

(Carozzi and Repetto, 2016). Our model integrates both perspectives and shows that party alignment, a prominent manifestation of clarity of responsibility, only has a conditional effect on subnational corruption. More specifically, party alignment only reduces corruption under both lower poverty and higher electoral competition. Lower poverty means that the greater clientelistic resources that aligned politicians can share are less valuable to voters, and higher electoral competition makes a potential corruption scandal more costly for politicians. In the next section, we explain our research design to test the model's predictions using unique, objective data on corruption from Guatemala.

2. Research Design

2.1. Institutional Context for Guatemala

Guatemala is a poor Central American country with a population of roughly 18 million people, of which 59% live in poverty and 23% live in extreme poverty (World Bank, 2017). Like many countries in the region, Guatemala officially has a presidential democracy. In 1996, the country emerged from a 36-year civil war. Since then, Guatemala has registered some democratic advances but maintains significant authoritarian enclaves and some institutional challenges (González, 2014).

Corruption, clientelism, and organized crime present particularly onerous challenges for Guatemala (Romero, 2025). The country's 2006-2019 partnership with the United Nations' International Commission Against Impunity (CICIG) helped dismantle some powerful drug-trafficking networks and some high-level corruption (Trejo and Nieto-Matiz, 2023). Notably, CICIG investigations helped lead to the indictment and removal from office of former President Otto Pérez Molina in 2015. Nevertheless, the country 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 revealed significant use of state resources in financing party campaigns

(Sandberg and Tally, 2015; Meilán, 2016).

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

2.2. Identification Strategy

Although the lack of political variation at the department level may not be ideal for democracy, it is useful for our empirical strategy. Specifically, Guatemala allows for a unique setting to study the consequences of municipalities barely electing aligned rather than unaligned mayors. We do so using a series of close-election regression discontinuity designs. Consistent with Marshall (2024), they do not identify causal effect of party alignment as a mayoral attribute. Instead, following Bertoli and Hazlett (2025), we estimate the Local Average Treatment Effect (LATE) for municipalities of electing an aligned rather than unaligned mayor.

Formally, we denote $D_{it} = 1$ if municipality i elects an aligned mayor in period t or 0 otherwise. For the corruption dependent variable, $r_{it}(1)$ captures the potential outcome for municipality i at period t under the aligned mayor, and $r_{it}(0)$ denotes the potential outcome for the unaligned mayor. The municipality-level causal effect is therefore:

$$\tau_{it} = r_{it}(1) - r_{it}(0) \tag{6}$$

Our parameter of interest is the LATE for municipalities at the cutoff:

$$\tau = \mathbb{E}\left[r_{it}(1) - r_{it}(0) \mid MV_{it} = 0\right] \tag{7}$$

where MV_{it} is the margin of victory for the aligned or unaligned mayor in the most recent election relevant for period t. Consistent with Figure 2, positive values of MV_{it} indicate that the aligned candidate won, negative values indicate that the unaligned candidate won, and the cutoff is $c = 0$. Under the continuity assumptions underpinning sharp regression discontinuity designs, we identify the difference in conditional expectations at the cutoff:

$$\tau = \lim_{m \downarrow 0} \mathbb{E}\left[r_{it} \mid MV_{it} = m\right] - \lim_{m \uparrow 0} \mathbb{E}\left[r_{it} \mid MV_{it} = m\right] \tag{8}$$

where m denotes the values of the running variable as MV_{it}. For observations with $MV_{it} \in (-h, h)$, we estimate τ using local polynomial regression:

$$r_{it} = \alpha + \tau D_{it} + \sum_{k=1}^{p} \beta_k MV_{it}^k + \sum_{k=1}^{p} \gamma_k \left(D_{it} \times MV_{it}^k\right) + Z_{it}'\rho + \eta_{it} \tag{9}$$

where h is Calonico, Cattaneo and Titiunik's (2014) data-driven optimal bandwidth; Z_{it} is a vector of covariates for robustness in some specifications, following Calonico et al. (2019); k indexes the polynomial terms; $p \in \{1, 2\}$ denotes the order of the polynomial (Gelman and Imbens, 2019); and η_{it} is the error term. We report municipality-clustered standard errors per Bartalotti and Brummet (2017), and we also report versions with municipality and year fixed effects as robustness checks following Frey (2019).

2.3. Poverty Data and Samples for Estimation

The municipality-level poverty data in this paper come from Guatemala's National Statistics Institute (INE, *Instituto Nacional de Estadística*) poverty maps from the 2002 and 2011 censuses. Consistent with this paper's attempt to better understand the relationship between corruption and modernization theory, the poverty data specifically refer to the percent of people below the income-based poverty and extreme poverty lines.

Given the absence of annual municipal poverty data and the difficulty of incorporating these interactions into a regression discontinuity design, we estimate results separately for low-poverty, high-poverty, poverty-increasing, poverty-decreasing, extreme-poverty-increasing, and extreme-poverty-decreasing municipalities. We define low- and high-poverty municipalities relative to the median. As Figures B.2 and B.3 show, the level- and change-based measures overlap but are not identical, capturing related but distinct dimensions of economic modernization. For comparison with the macro-level predictions of Schwindt-Bayer and Tavits (2016), we also estimate models using the full sample—i.e., not splitting the sample by poverty.

For analyses by poverty level, the sample covers 2004-2015. For analyses by poverty or extreme-poverty change, the main sample covers 2010-2015, and Appendix M contains additional analyses for 2008-2015, 2009-2015, and 2011-2015. In specifications extending before 2011, we backdate the 2011 poverty measure by up to three years given that Instituto Nacional de Estadística de Guatemala (2014) collected the data from 2008 to 2011, and municipal poverty rates are unlikely to vary sharply from year to year. From the 331 municipalities in the panel, poverty data are missing for 32 urban municipalities in 2011. Appendix N examines these missing data and finds no evidence of meaningful bias.

2.4. Electoral Data

Municipal electoral data come from Guatemala's Supreme Electoral Tribunal (TSE, *Tribunal Supremo Electoral*), which publishes a *Memoria Electoral* after each election. We compute valid votes by subtracting spoiled ballots from total votes, then calculate the valid vote shares of the winner and runner-up. The margin of victory is the winner's valid vote share minus that of the second-place candidate, and we exclude instances where neither of the top two candidates is aligned.

Because concerns about electoral manipulation are especially relevant for regression discontinuity designs, Appendix P reports McCrary density tests for the running variable across the main and appendix samples, following Cattaneo, Jansson and Ma (2018). All tests pass for the original term-level electoral data. Failures arise only in some year-wise replications of the same election results, which do not reflect the original term-wise distributions, so they provide some indication of robustness.

2.5. Corruption Data

Unlike a large portion of the literature, which relies on perceptions data that can have measurement challenges (e.g., Kurtz and Schrank, 2007), we use objective data from audits. Our data specifically derive from Guatemala's Comptroller General (*Contraloría General de Cuentas*), which ranks very highly according to the only international index of supreme audit institutions (SAIs) from the World Bank (Gurazada et al., 2021). In its inaugural 2021 report, the World Bank ranked the SAIs from 118 countries on the basis of ten indicators: constitutional framework; appointment process transparency for the SAI head; financial autonomy; audits types; operational autonomy; staffing; mandate to decide on audit scope; access to records and information; and audit report rights and obligations. Each SAI then receives a final 0-10 score, ranging from 10 (only South Africa and Seychelles) to 2.5 (only Chad). Guatemala's score of 8.5 gives it an effective place of 4/18. For comparison with the audit data used in previous studies (e.g., Ferraz and Finan, 2008; Larreguy, Marshall and Snyder, 2020), Guatemala's SAI ranks just behind those from Brazil and Mexico (scores: 9/10; effective places: 3/18).

While the World Bank SAI index is very helpful for understanding numerous aspects of audit independence, it does not provide much insight into a key concern: that the distribution of audits may be biased against political rivals. In practice, the Comptroller General audits approximately 317 of Guatemala's 340 municipalities each year under its Annual Audit Plan, which leaves limited scope for selectively targeting rivals. We nonetheless test for partisan audit allocation in Figure K.3 using a close-election regression discontinuity design. Consistent with Denly (2020), we find no evidence that unaligned municipalities are more likely to be audited than aligned municipalities.

For each audited municipality from 2004-present, the Comptroller General publishes on its website: the number of overall infractions committed (*sancciones*), and the amount of stolen or misappropriated money in the local currency (Quetzales) associated with these infractions. Both of these variables serve as our study's dependent variables and correspond most closely with bureaucratic corruption. As Fisman and Golden (2017, 41) explain, bureaucratic corruption takes place because "politicians permit it or fail to exercise adequate oversight to prevent it, all too often because they themselves are benefiting financially and politically." In the case of these infractions-based measures in Guatemala, they encompasses both what Brollo et al. (2013, 1774) call "broad corruption" and "narrow corruption".[7] For comparability purposes, we first deflate the money version of the infractions variable and then take its log. We do not transform the number of infractions committed variable. Appendix B provides relevant descriptive statistics and maps.

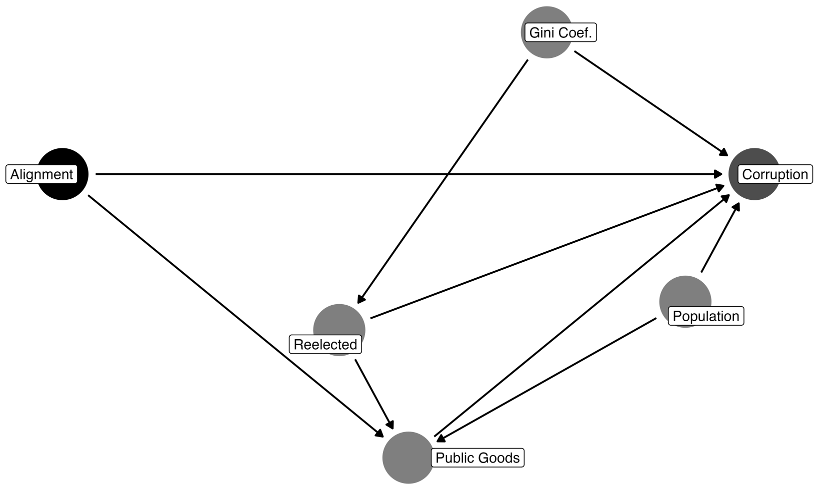
2.6. DAG-Based Adjustment for Robustness

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 for robustness. Because more populous municipalities likely have more resources, which makes corruption more feasible, we use covariate data on population from Guatemala's National Statistics Institute. Corruption also has prominent relationships with reelection and inequality (Alesina and Angeletos, 2005; Ferraz and Finan, 2011), so we include relevant data from the Guatemalan Electoral Institute and Guatemalan National Statistics Institute. Initially, we collected data on public goods spending from the World Bank (2019), but the Directed Acyclic Graph (DAG) in Figure 3 suggests that public goods enter as an estimand-shifting mediator,[8] so we exclude it from the robustness analysis. Table B2 presents descriptive statistics of all covariate data by party alignment status.

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

[8] As we underscore earlier in paper, a large literature suggests that alignment increases resource availability, which makes corruption more feasible. To ensure that we maintain the average treatment effect estimand, we thus exclude public goods from the analysis.

Figure 3: DAG for Adjustment Set Determination



Note: Poverty and alignment are uncorrelated, so the omission of a poverty node from DAG is inconsequential for adjustment. No variable directly affects alignment, because it is a simultaneous function of both presidential and mayor elections, which are difficult to predict or shape. That is also why the minimal adjustment set is the bivariate RD estimate. With the exception of the public goods mediator, all other variables enter as precision-oriented controls in the canonical adjustment set for robustness.

3. Results

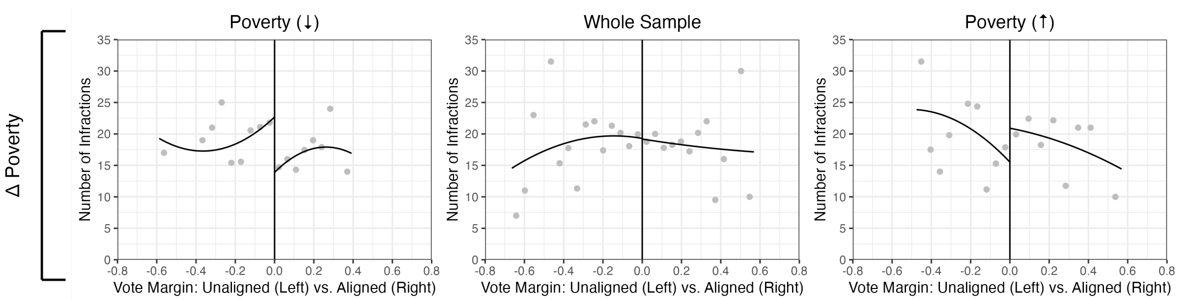
3.1. Corruption Results Disaggregated by Poverty

Figures 4a and 4b present the main results for the infractions dependent variable by electoral term. We show the term-wise results for the (log) amounts of stolen/misappropriated money dependent variable in Figures 4c and 4d. Appendix A contains the year-wise results for the same dependent variables, and Appendices E, F, and I contain full tables, including the robustness tests with fixed effects, different polynomial orders, and covariates.

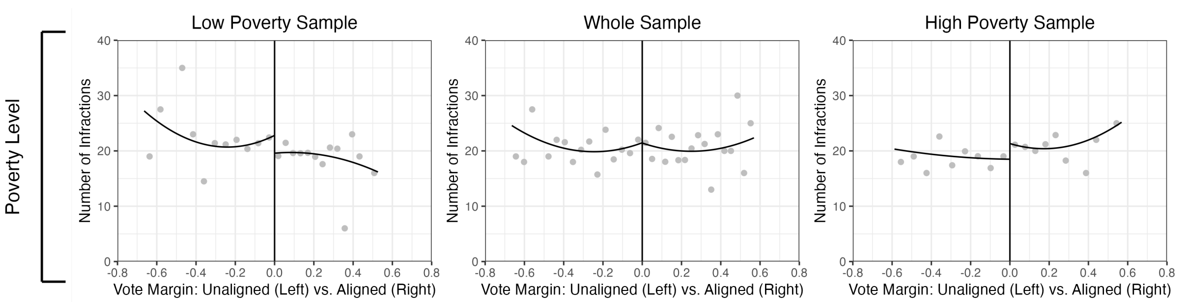
Overall, the results are similar for both yearly and electoral term data: municipalities with aligned mayors exhibit less corruption in the low-poverty and poverty-reducing samples. The results for these samples are not only statistically significant but substantively significant as well. For example, in our base term specification without fixed effects (Figures 4a-4b), aligned municipalities commit an average of 10.98 fewer infractions in the poverty-reducing sample and 5.81 fewer infractions in the low-poverty sample (see Tables E1-E2). These tables also include the extraordinary falsification test of adding fixed effects to our regression

Figure 4: Main Results by Term

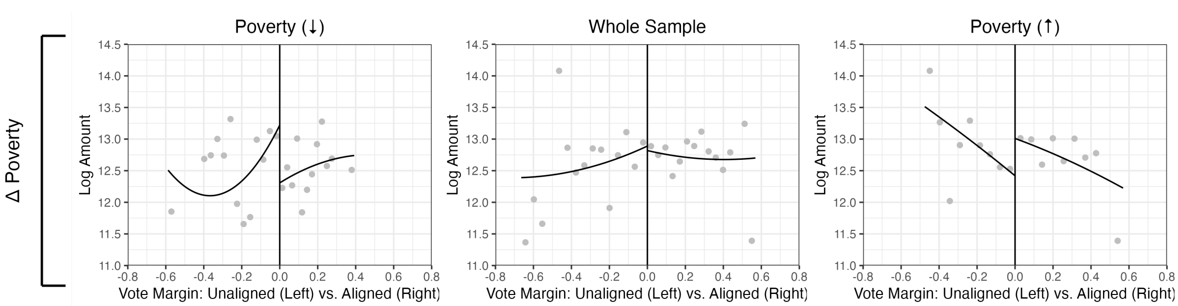
(a) Infraction Count (Poverty Changes)



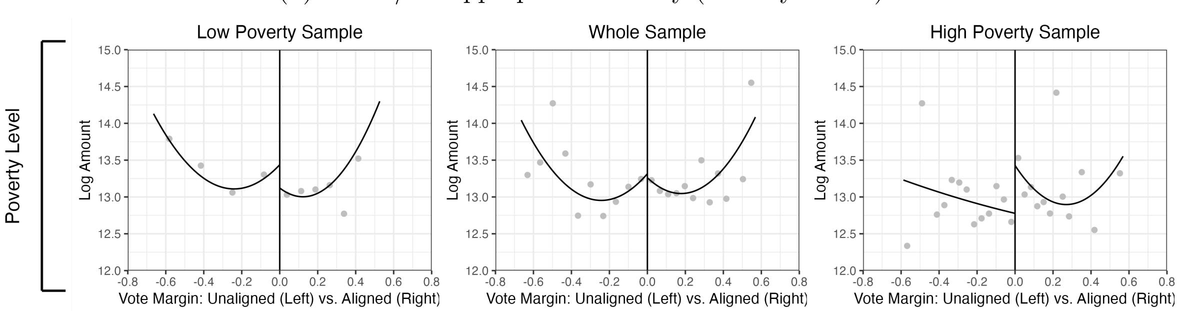
(b) Infraction Count (Poverty Levels)



(c) Stolen/Misappropriated Money (Poverty Changes)



(d) Stolen/Misappropriated Money (Poverty Levels)



Note: The above plots reflect 95% confidence intervals with second-order polynomial fits in line with Gelman and Imbens (2019). In Appendices E, F, and I, we present corresponding tables and additional figures using (i) local linear fits with and without covariates; (ii) second-order fits with and without covariates; and (iii) estimates with and without fixed effects. Appendix M also considers robustness to different time periods.

discontinuity estimates, and we find similar patterns as well. All of the results for the log amounts of stolen/misappropriated money in Figures 4c-4d remain consistent, too.

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

Poverty-reducing, aligned municipalities also reduce their corruption levels most consistently within the final two years of the electoral term. Appendix G shows the results for the last two years. When compared to the results from the first two years in Appendix H, it is clear that the final two years of each electoral term are mostly driving the decrease in corruption in the low-poverty and poverty-reducing samples. Overall, these results are consistent with Ferraz and Finan (2008, 2011) and Bobonis, Cámara Fuertes and Schwabe (2016), who show that elections discipline politicians. In our case, that applies even to municipalities with aligned politicians, which generally enjoy resource advantages relative to municipalities with unaligned politicians (e.g., Brollo and Nannicini, 2012; Carozzi and Repetto, 2016; Corvalan, Cox and Osorio, 2018; Curto-Grau, Solé-Ollé and Sorribas-Navarro, 2018).

As predicted by our theory, only aligned municipalities in the poverty-reducing and low-poverty samples consistently reduce their corruption levels (see Tables E1-E2 and Figure 4). Appendix F disaggregates results for the samples in which poverty is high or increased from one census to next. These results generally shift in the opposite direction from the low-poverty and poverty-reducing samples (see Figure 4). In these instances, there is an uptick in corruption—again, measured by infractions or the log amounts of stolen/misappropriated money associated with those infractions (see Tables F1-F2). Theoretically, it is logical that poorer voters may be more forgiving of corruption, as long as the mayors share their rents with voters through clientelistic or other means (Fernández-Vázquez, Barberá and Rivero, 2016). However, the year-wise specifications for the poverty-increasing sample fail the McCrary (2008) density tests in Appendix P, and none of the specifications for the poverty-increasing sample have statistically significant results. The same is true for when we alter the sample to encompass different years in Appendix M. Accordingly, we caution against interpreting the results from the high-poverty and poverty-increasing samples as definitive evidence.

For purposes of comparison with current predictions of clarity of responsibility theory (see Schwindt-Bayer and Tavits, 2016), all of the aforementioned Figures and Appendix I show the results for the whole sample—i.e., when not disaggregating by poverty. Overall, these findings from the whole sample are inconsistent. Sometimes, aligned municipalities have less corruption; other times, they have more. In all instances, though, none of the results are statistically significant (see Tables I1-I2). We thus interpret the whole sample results as evidence of the fact that alignment both provides resource advantages and increases clarity of responsibility. When not disaggregating the sample by poverty, these countervailing effects often cancel each other out.

3.2. Corruption Results Disaggregated by Extreme Poverty

To further assess the extent to which better economic conditions can reduce corruption in aligned municipalities, we also examine the extent to which low or decreasing extreme poverty yields similar results as those of low or decreasing poverty. In all specifications, which we detail in Figure F.1, aligned municipalities exhibit reduced corruption when extreme poverty is low or declines. Results are slightly weaker for the log amounts of stolen/misappropriated money, as less specifications are statistically significant. Nevertheless, the results with the log amounts as the dependent variable are still suggestive of the same overall pattern.

As above, the same results do not hold for the high-extreme-poverty or increasing-extreme-poverty samples (see Appendix D). In nearly all specifications entailing counts of the number of infractions and the (log) amounts associated with those infractions, the coefficient for alignment is positive, indicating that aligned municipalities exhibit an increase in corruption. However, similar to the results for the high-poverty and the poverty-increasing samples, none of the results are statistically significant for the high-extreme-poverty or increasing-extreme-poverty samples, and the year-wise specification does not pass the McCrary (2008) density test (see Appendix P.3).

4. Mechanism Analyses

4.1. Clarity of Responsibility Mechanism

A premise of the above results is that an important component of electing aligned rather than unaligned mayors is greater clarity of responsibility for misgovernance, and that politicians are aware and take mitigating measures. Although Schwindt-Bayer and Tavits (2016) provide strong evidence for this mechanism, it is useful to reaffirm it for Guatemala. We do so with an analysis of municipal corruption levels before and after Guatemala experienced an alignment and party system shock in 2016.

Guatemala's October 25, 2015 presidential run-off brought the populist outsider Jimmy Morales to office. Because no mayor from Morales' party, the National Convergence Front (FCN), won in the same election, there were no mayoral-presidential alignments for 2016-2019.[9] This abrupt disappearance of alignments enables a useful test of whether alignment helps structure accountability.

Both the mean number of municipal-level infractions and amount of misappropriated money increased significantly after the election of Morales (see Table 1). Nevertheless, the (quasi) natural experiment of Morales' election is probably not sufficient for these descriptive statistics in Table 1 to be interpreted own their own. We therefore supplement these

[9]New presidents in Guatemala take office in January, and relevant elections occur late in the previous year.

Table 1: Infractions and Stolen/Misappropriated Money Amounts by Alignment Shares

Term	Municipalities Aligned %	Infractions Mean	Amount Mean	Log Amount Mean
2008-2011 (Colom)	31%	5.33	213,325 Q	11.3
2012-2015 (Molina/Maldonado)	36%	6.56	216,441 Q	11.5
2016-2019 (Morales)	0%	13.1	427,646 Q	11.7

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

descriptive statistics with the regression analyses presented in Table 2 and the additional dotwhisker plots in Appendix J. Each regression contains the main, time-varying covariates used in our regression discontinuity analyses throughout the paper as well as the poverty indicators used to construct our samples. We exclude the alignment variable because it is collinear with the Morales Term variable, which serves as our main independent variable for the analysis. Given that infractions is a count variable, we estimate those respective regressions with Poisson and negative binomial models, and the log amounts regressions are estimated with linear regression.

Across all specifications, the Morales Term coefficient is generally positive and highly statistically significant. The results are somewhat stronger for the number of infractions than for logged amounts, but the overall pattern is consistent: corruption increased after the disappearance of mayoral-presidential alignments. This evidence is consistent with the argument that alignment helps clarify responsibility for misgovernance and thereby constrains municipal corruption.

4.2. Poverty Mechanism

To further assess the poverty mechanism behind the results in Sections 3.1 and 3.2, we perform two sets of additional tests. First, we examine whether the results reflect reverse causality from corruption to poverty. Second, we analyze how poverty and alignment jointly structure the behavior of relatively more and less corrupt mayors.

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

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

Note: Poisson regression model, since infractions are a count variable.
Standard errors clustered by municipality in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix L provides several tests of whether corruption predicts poverty, including residualized regressions and ones that regress poverty on corruption. Across numerous specifications, we find no meaningful relationship between poverty and corruption using using year-wise or term-wise data. These results are consistent with the idea that the poverty patterns in our main analyses do not reflect reverse causality from corruption.

Appendix O then presents descriptive evidence on how municipality alignment conditions corruption across low-poverty, high-poverty, poverty-reducing, and poverty-increasing municipalities. To do so, we classify municipalities as relatively more or less corrupt using the median number of infractions and the median logged amount of misappropriated funds. Although these are coarse splits, they provide a simple way to illustrate the mechanism. The patterns are consistent with the theory: in lower-poverty and poverty-reducing municipalities, aligned municipalities are more likely than unaligned municipalities to fall below the corruption median, whereas the opposite pattern is more common in poorer and poverty-increasing settings.

4.3. Close Elections Mechanism (Placebo Tests)

To further assess Proposition 1, we examine whether the municipality-level alignment-corruption relationship appears away from the regression discontinuity cutoff. Because Calonico, Cattaneo and Titiunik's (2014) data-driven bandwidths in the main RD analyses are typically about 10 percentage points on either side of the cutoff, we restrict this placebo exercise to observations where $-10\% < MV > 10\%$). While these results do not yield the same causal interpretation as the RD results (Bertoli and Hazlett, 2025), they still have abductive value in ascertaining whether patterns are specific to close elections (see Spirling and Stewart, 2025).

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

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

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

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

5. Discussion and External Validity

Given recent work on regression discontinuity designs involving politician characteristics (Marshall, 2024; Bertoli and Hazlett, 2025), it is useful to clarify what the results imply. Crucially, they do *not* correspond to a politician-level estimand. Instead, the results identify the effects of municipalities barely electing aligned politicians compared to unaligned ones, not the effects of alignment by itself. As such, the treatment is bundled but arguably more policy-relevant. Aligned municipalities differ from unaligned, opposition municipalities in terms of resource advantages, economic outcomes, party fragility, and numerous other characteristics (Bertoli and Hazlett, 2025, 394). Absorbing these "nuisance" characteristics into the treatment thus renders the alignment treatment more meaningful.[10]

Another consideration is external validity (Findley, Kikuta and Denly, 2021), and regression discontinuity designs tend to score lower on this criterion (Cattaneo, Idrobo and Titiunik, 2019, 14). While some scholars have proposed methods extending results beyond the as-good-as-random neighborhood around the cutoff (e.g., Angrist and Rokkanen, 2015), they are not applicable for the present study. First, we do not theorize for such a possibility, so any ex-post analysis would be purely exploratory. Second, both our theory and empirics, including results from relevant placebo tests, suggest that the results do not hold outside of close elections. Accordingly, we focus our generalizability analysis on whether results hold for different sample subsets and their representativeness relative to the rest of Guatemalan municipalities.

As we show in Figure K.4, we find the similar patterns as the main analysis when we

[10]See Kikuta (2026, 120-121) for a similar discussion of gender-based RDs.

restrict the sample to municipalities that the Comptroller General audited in all four years of each respective electoral term. The same is true when we examine the average number of infractions and (log) amounts of stolen/missing money per electoral term, taking into account the number of times a municipality was audited in each term (see Figure K.5).

Table 4: Balance Diagnostics for Close-Election and Uncompetitive-Election Samples

Variable	Absolute (Standardized) Mean Difference	Variance Ratio
Panel A: Whole Sample (Poverty-Change Analysis) [2010–2015]		
Reelected Mayor	0.108	
Population (log)	0.083	1.209
Inequality (Gini)	0.004	1.001
Panel B: Poverty-Reducing Sample (Poverty-Change Analysis) [2010–2015]		
Reelected Mayor	0.228	
Population (log)	0.216	0.916
Inequality (Gini)	0.027	1.107
Panel C: Poverty-Increasing Sample (Poverty-Change Analysis) [2010–2015]		
Reelected Mayor	0.098	
Population (log)	0.130	1.182
Inequality (Gini)	0.083	0.862
Panel D: Whole Sample (Poverty-Level Analysis) [2004–2015]		
Reelected Mayor	0.187	
Population (log)	0.003	1.227
Inequality (Gini)	0.030	1.004
Panel E: Low-Poverty Sample (Poverty-Level Analysis) [2004–2015]		
Reelected Mayor	0.331	
Population (log)	0.049	1.206
Inequality (Gini)	0.041	1.069
Panel F: High-Poverty Sample (Poverty-Level Analysis) [2004–2015]		
Reelected Mayor	0.036	
Population (log)	0.098	1.288
Inequality (Gini)	0.048	0.941

To examine the representativeness of the close-election sample, we turn to balance tests of the pre-treatment covariates in the close- and uncompetitive-election samples. As Table 4 shows, population and inequality do not pose representativeness challenges. Across the various samples, their standardized mean differences tend to be lower than 0.1, and their variance ratios are within the 0.8-1.2 range.[11] By contrast, the non-standardized mean differences for the binary re-election variable tends to be higher than 0.1, suggesting potential

[11] Per Imai, King and Stuart (2008), we do not conduct significance testing for these inferences.

generalization difficulties for samples with different numbers of reelected mayors.

Given the bundled nature of our treatment and our lack of data for other countries, our ability to make transportability inferences is limited. Nevertheless, per the Morales results in Section 4.1, we can conjecture that the results unlikely to hold without a stable party system. Additionally, because citizens preferences' do not immediately respond to reductions in poverty or increasing income (Treisman, 2020), our poverty change results are subject to having similarly mid-to-long term intervals for corruption levels to respond accordingly.

6. Conclusion

Explaining variation in bureaucratic corruption requires understanding how electoral incentives shape bureaucratic slippage (Golden and Mahdavi, 2015, 414). We attempt to better understand these phenomena by proffering a simple political agency model. Then, we examine its empirical implications using novel, audit-based corruption data from Guatemala and a series of close-election regression discontinuity designs.

We find that municipalities barely electing aligned mayors exhibit reduced corruption levels when those municipalities have lower or declining poverty. Our results are robust to both infraction- and amount-based measures of corruption. From measurement and external validity perspectives, our paper undertakes checks that scholars can follow to credibly analyze corruption outside a context with randomized audits like Brazil, which has heretofore served as the main country in the literature.[12] More broadly, our paper helps show how economic modernization and political-institutional forces combine to place subnational units on different corruption paths.

[12]See, for example, Ferraz and Finan (2008, 2011), Brollo et al. (2013), and Zamboni and Litschig (2018).

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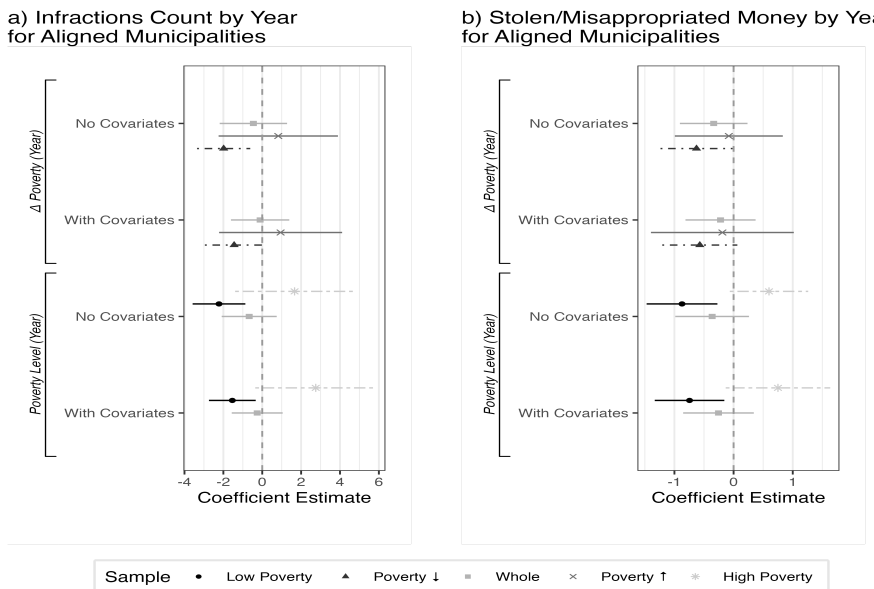
Appendices

A. Additional Coefficient Plots for Year-Wise Results

Figure A.1: Year-Specific Results

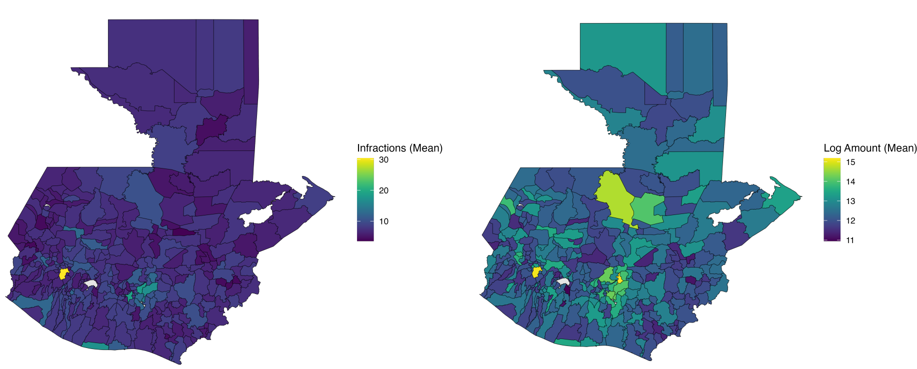


Note: The above estimates are second-order polynomial fits in line with Gelman and Imbens (2019), with standard errors clustered by municipality and confidence intervals at the 95% level. Per Section 2.3, the poverty levels analyses correspond to 2004-2015, and the poverty change analyses correspond to 2010-2015. "With covariates" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini). Full tables corresponding to the above Figure can be found in Appendices E and I.

B. Descriptive Statistics and Maps

Figure B.1: Infractions and Stolen/Misappropriated Money by Municipality, 2004-2019

(a) Mean Infractions (b) Mean Misappropriated/Stolen Money



Note: All geographic units are municipalities. The three white areas are lakes.

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

Panel A: Infractions (Year Viewpoint)	Increase Unaligned		Increase Aligned		Decrease Unaligned		Decrease Aligned	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Number of Infractions: All Years	9.453	1,107	6.376	348	8.704	1,043	5.443	271
Log Amount of Stolen/Misappropriated Money: All Years	11.53	1,106	11.40	347	11.50	1,041	11.20	270
Number of Infractions: First 2 years of Term	6	184	6.286	126	5.985	194	5.233	90
Log Amount of Stolen/Misappropriated Money: First 2 years of Term	11.21	183	11.29	125	11.24	193	10.91	89
Number of Infractions: Last 2 years of Term	6.071	395	6.428	222	6.438	384	5.547	181
Log Amount of Stolen/Misappropriated Money: Last 2 years of Term	11.53	395	11.47	222	11.56	383	11.34	181
Number of Infractions: Last year of Term	6.894	198	7.387	111	7.373	193	6.154	91
Log Amount of Stolen/Misappropriated Money: Last year of Term	11.83	198	11.89	111	11.84	192	11.61	91
Panel B: Infractions (Electoral Term)	**Increase Unaligned**		**Increase Aligned**		**Decrease Unaligned**		**Decrease Aligned**	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Number of Infractions: All Years	29.56	354	19.99	111	27.10	335	16.21	91
Log Amount of Stolen/Misappropriated Money: All Years	13.14	354	12.87	111	13.12	335	12.48	91
f Number of Infractions: First 2 years of Term	12	92	12.77	62	12.09	96	10.47	45
Log Amount of Stolen/Misappropriated Money: First 2 years of Term	12.08	92	12.27	62	12.21	96	11.72	45
Number of Infractions: Last 2 years of Term	12.05	199	12.86	111	12.88	192	11.03	91
Log Amount of Stolen/Misappropriated Money: Last 2 years of Term	12.39	199	12.43	111	12.47	192	12.20	91
Number of Infractions: Last year of Term	6.894	198	7.387	111	7.411	192	6.154	91
Log Amount of Stolen/Misappropriated Money: Last year of Term	11.83	198	11.89	111	11.84	192	11.61	91
Panel C: Infractions (Year Viewpoint)	**High Unaligned**		**High Aligned**		**Low Unaligned**		**Low Aligned**	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Number of Infractions: All Years	6.854	1,393	5.574	432	8.139	1,265	5.638	475
Log Amount of Stolen/Misappropriated Money: All Years	11.32	1,390	11.34	431	11.62	1,265	11.34	474
Number of Infractions: First 2 years of Term	5.438	416	5.441	204	6.063	365	5.513	197
Log Amount of Stolen/Misappropriated Money: First 2 years of Term	11.23	414	11.24	203	11.38	365	11.18	196
Number of Infractions: Last 2 years of Term	5.882	407	5.944	198	6.656	372	6.117	205
Log Amount of Stolen/Misappropriated Money: Last 2 years of Term	11.50	406	11.34	198	11.59	372	11.48	205
Number of Infractions: Last year of Term	6.754	199	6.698	96	7.521	192	6.953	106
Log Amount of Stolen/Misappropriated Money: Last year of Term	11.83	198	11.76	96	11.83	192	11.77	106
Panel D: Infractions (Electoral Term)	**High Unaligned**		**High Aligned**		**Low Unaligned**		**Low Aligned**	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Number of Infractions: All Years	24.60	468	20.80	118	28.86	409	19.35	136
Log Amount of Stolen/Misappropriated Money: All Years	13.13	468	13.14	118	13.37	409	12.94	136
Number of Infractions: First 2 years of Term	10.88	208	10.88	102	12.09	183	10.97	99
Log Amount of Stolen/Misappropriated Money: First 2 years of Term	12.16	208	12.25	102	12.32	183	12.06	99
Number of Infractions: Last 2 years of Term	11.76	208	11.87	103	13.24	183	12.20	99
Log Amount of Stolen/Misappropriated Money: Last 2 years of Term	12.40	208	12.35	103	12.46	183	12.30	99
Number of Infractions: Last year of Term	6.716	208	6.689	103	7.643	182	6.980	99
Log Amount of Stolen/Misappropriated Money: Last year of Term	11.80	208	11.77	103	11.87	182	11.76	99

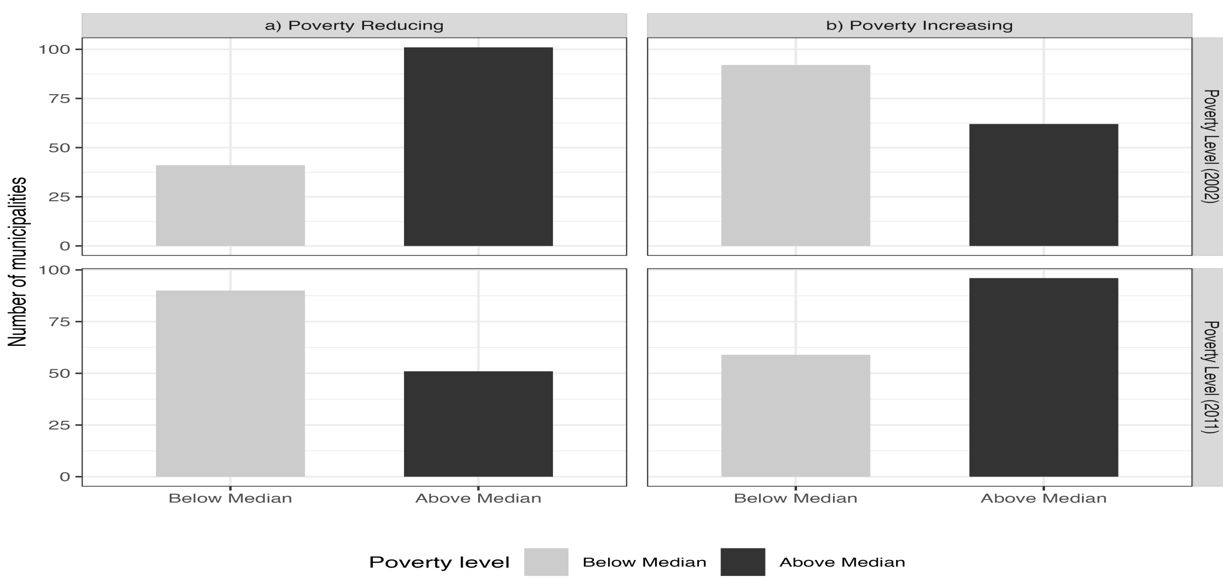
Note: Panel A shows results by years, while the Panel B shows results by electoral term. “Decrease” refers to the sample of municipalities where poverty had decreased between 2002 and 2011, while “Increase” refers to the sample where poverty increased between 2002 and 2011. All amounts are expressed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

Table B2: Descriptive Statistics of Covariates (Poverty Increasing/Decreasing Sample)/(Poverty High/Low Sample)

Panel A: Year Viewpoint	Increase Unaligned		Increase Aligned		Decrease Unaligned		Decrease Aligned	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Percentage of Mayor Reelected	0.305	1,160	0.217	332	0.326	1,110	0.0945	254
Extreme Poverty Rate	25.11	1,202	25.35	348	16.32	1,148	15.53	272
Gini coefficiant	24.95	1,202	25.29	348	24.99	1,148	23.94	272
Total Poverty Rate	72.70	1,202	70.96	348	66.05	1,148	65.09	272
Log Population	10.29	1,202	10.22	348	10.34	1,148	10.12	272
Panel B: Electoral Term	Increase Unaligned		Increase Aligned		Decrease Unaligned		Decrease Aligned	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Percentage of Mayor Reelected	0.306	333	0.214	103	0.320	316	0.122	82
Extreme Poverty Rate	26.13	354	27.91	111	19.13	335	19.83	91
Gini coefficiant	25.56	354	26.17	111	25.56	335	25.26	91
Total Poverty Rate	73.87	354	73.37	111	68.44	335	68.84	91
Log Population	10.27	354	10.23	111	10.34	335	10.10	91
Panel C: Year Viewpoint	High Unaligned		High Aligned		Low Unaligned		Low Aligned	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Percentage of Mayor Reelected	0.270	1,387	0.159	390	0.365	1,286	0.231	450
Extreme Poverty Rate	30.22	1,493	31.36	435	10.73	1,342	10.62	477
Gini coefficiant	22.17	1,493	21.43	435	25.04	1,342	25.15	477
Total Poverty Rate	82.25	1,493	82.72	435	54.01	1,342	53.16	477
Log Population	10.31	1,493	10.15	435	10.21	1,342	10.14	477
Panel D: Electoral Term	High Unaligned		High Aligned		Low Unaligned		Low Aligned	
VARIABLES	Mean	N	Mean	N	Mean	N	Mean	N
Percentage of Mayor Reelected	0.275	440	0.179	106	0.359	396	0.248	129
Extreme Poverty Rate	31.61	468	33.66	118	12.09	409	12.54	136
Gini coefficiant	22.92	468	23.42	118	25.83	409	26.48	136
Total Poverty Rate	82.84	468	83.78	118	56.51	409	55.45	136
Log Population	10.31	468	10.15	118	10.20	409	10.12	136

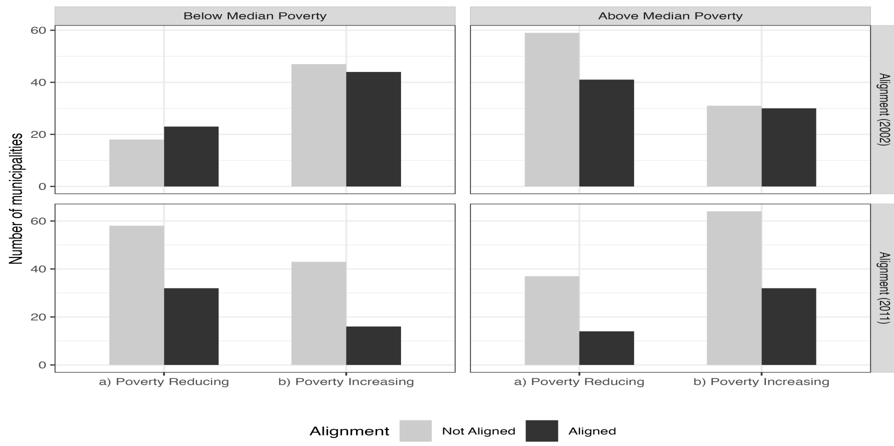
Note: Panels A/C are year-viewpoint; Panels B/D are electoral-term. "Decrease" refers to municipalities where poverty decreased between 2002 and 2011; "Increase" refers to municipalities where poverty increased between 2002 and 2011.

Figure B.2: Wealth Distribution by Poverty Sub-Sample in 2002 and 2011



Note: "Poverty-Reducing Sample" refers to the sub-sample of municipalities where poverty decreased from 2002 to 2011, while "Poverty-Increasing Sample" refers to the sub-sample of municipalities where poverty increased between the two poverty measurements.

Figure B.3: Wealth Distribution by Alignment and Poverty Sub-Samples in 2002 and 2011



Note: "Municipalities Above Median Poverty" refers to the municipalities that have poverty levels above the national medial poverty levels in the poverty mapping for the 2002 census, while "Municipalities Below Median Poverty" refer to the municipalities below the national median poverty level. "Reducing" refers to the municipalities where poverty decreased from 2002 to 2011, while "Increasing" refers to the municipalities where poverty increased during the same period.

C. Theoretical Derivation

Proof of Proposition 1. Given Equation (5), we can rewrite the maximization problem as follows:

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

The corresponding First-Order Condition (F.O.C.) for Equation (10) is:

$$\begin{aligned}0 = U'(r_{i,1}) + U(r_{i,2})\pi'(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a-1)t(MV))[-W'(1-r_{i,1}) \\ + \gamma\beta_i^{1+a}W'(\gamma r_{i,1})] - U(x_{i,2})\pi'(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) \\ + (2a-1)t(MV))[-W'(1-r_{i,1}) + \gamma\beta_i^{1+a}W'(\gamma r_{i,1})]\end{aligned} \tag{11}$$

Collecting like terms and bringing them to the other side, Equation (11) can be rewritten as:

$$\begin{aligned}U'(r_{i,1}) =& [U(r_{i,2}) - U(x_{i,2})]\pi'(W(1-r_{i,1}) + \beta_i^{1+a}W(\gamma r_{i,1}) + (2a-1)t(MV))[W'(1-r_{i,1}) - \\ & \gamma\beta_i^{1+a}W'(\gamma r_{i,1})]\end{aligned} \tag{12}$$

Now from the assumption on $t(\cdot)$, we know that as $MV \to 0$, $t(MV) \to 0$ since $t(\cdot)$ increases with respect to MV. Thus, as $MV \to 0$, Equation (12) can be written as:

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

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

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

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

$$U'(\underline{r_{i,1}}) = [U(r_{i,2}) - U(x_{i,2})]\pi'(W(1-\underline{r_{i,1}}) + \beta_i W(\gamma\underline{r_{i,1}}))[W'(1-\underline{r_{i,1}}) - \gamma\beta_i W'(\gamma\underline{r_{i,1}})] \tag{15}$$

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 $z, k > 0$.[13] □

Proof of Proposition 2. The proof of this Proposition follows a similar structure as Brollo and Nannicini (2012, Proof of Proposition 2). Per Equation (11), we define the first-order condition as $g(r_{i,1}, MV) = 0$, so by implicit differentiation $\partial r_{i,1}/\partial MV = -(\partial g/\partial MV)/(\partial g/\partial r_{i,1})$, where $\partial g/\partial r_{i,1} < 0$ due to the maximization of the second-order condition. By extension, therefore:

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

When $a = 1$:

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

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

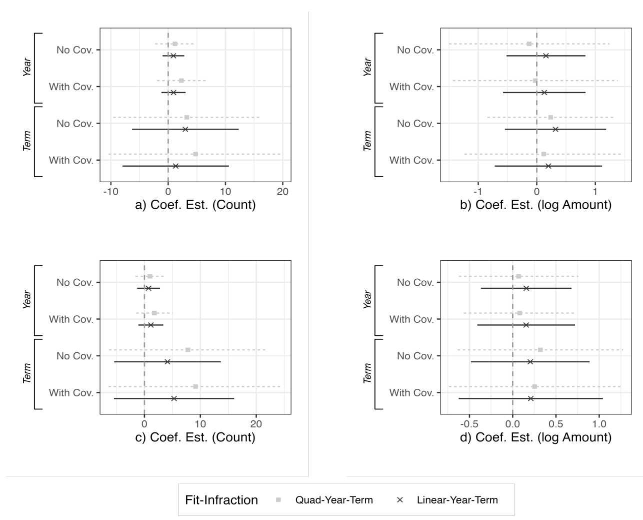
When $a = 0$:

$$\begin{aligned}\partial g/\partial MV =& -[U(r_{i,2}) - U(x_{i,2})]\pi'_{MV}(W(1 - r_{i,1}) + \beta_i W(\gamma r_{i,1}) \\ & - t(MV))[W'(1 - r_{i,1}) - \gamma\beta_i W'(\gamma r_{i,1})]t'(MV) < 0\end{aligned} \tag{18}$$

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

D. Extreme Poverty High/Extreme Poverty ↑ Samples

Figure D.1: Extreme Poverty High/Extreme Poverty ↑ Samples



Note: Panels A and B correspond to the extreme poverty increasing sample. Panels C and D correspond to the high extreme poverty sample. All specifications use standard errors clustered by municipality. Whiskers depict 95% confidence intervals. "No Cov." specifications do not use any controls. "With covariates" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

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

E. Low Poverty/Poverty ↓ Sample

Table E1: RDD Estimates for Infraction Count/Amount (log) by Term (Poverty ↓)

Panel A	(1)	(2)	(3)	(4)	Panel C	(1)	(2)	(3)	(4)
RD Estimate	-10.982***	-13.728***	-7.964**	-9.901**	RD Estimate	-1.240***	-1.197**	-1.119***	-1.070**
	(2.996)	(4.113)	(3.333)	(4.184)		(0.427)	(0.544)	(0.389)	(0.510)
Observations	195	195	179	179	Observations	195	195	179	179
Eff. obs.	[56,45]	[62,49]	[46,35]	[59,46]	Eff. obs.	[48,37]	[56,43]	[45,34]	[52,38]
Covariates	None	None	All	All	Covariates	None	None	All	All
p-value	0.000	0.001	0.017	0.018	p-value	0.004	0.028	0.004	0.036
Order	1	2	1	2	Order	1	2	1	2
Bandwidth	0.0958	0.1059	0.0827	0.1199	Bandwidth	0.0739	0.0942	0.0798	0.0915

Panel B	(1)	(2)	(3)	(4)	Panel D	(1)	(2)	(3)	(4)
RD Estimate	-3.805*	-5.131*	-2.119	-4.508	RD Estimate	-0.819**	-0.762	-0.709*	-0.633
	(1.982)	(2.717)	(2.244)	(3.031)		(0.369)	(0.470)	(0.392)	(0.505)
Observations	195	195	179	179	Observations	195	195	179	179
Eff. obs.	[57,47]	[62,49]	[46,35]	[50,36]	Eff. obs.	[49,39]	[57,47]	[45,34]	[55,43]
Covariates	None	None	All	All	Covariates	None	None	All	All
p-value	0.055	0.059	0.345	0.137	p-value	0.027	0.105	0.070	0.210
Order	1	2	1	2	Order	1	2	1	2
Bandwidth	0.0967	0.1060	0.0830	0.0895	Bandwidth	0.0795	0.0971	0.0798	0.1010

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows estimates for infraction count by electoral term without term fixed effects, while Panel B shows estimates with term fixed effects. Panel C shows estimates for infraction amount by electoral term without term fixed effects, while Panel D shows estimates 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), reelection dummy, and Gini coefficient as controls.

Figure E.1: RD Plots for Table E1 Panels A and C (Poverty ↓ Sample)

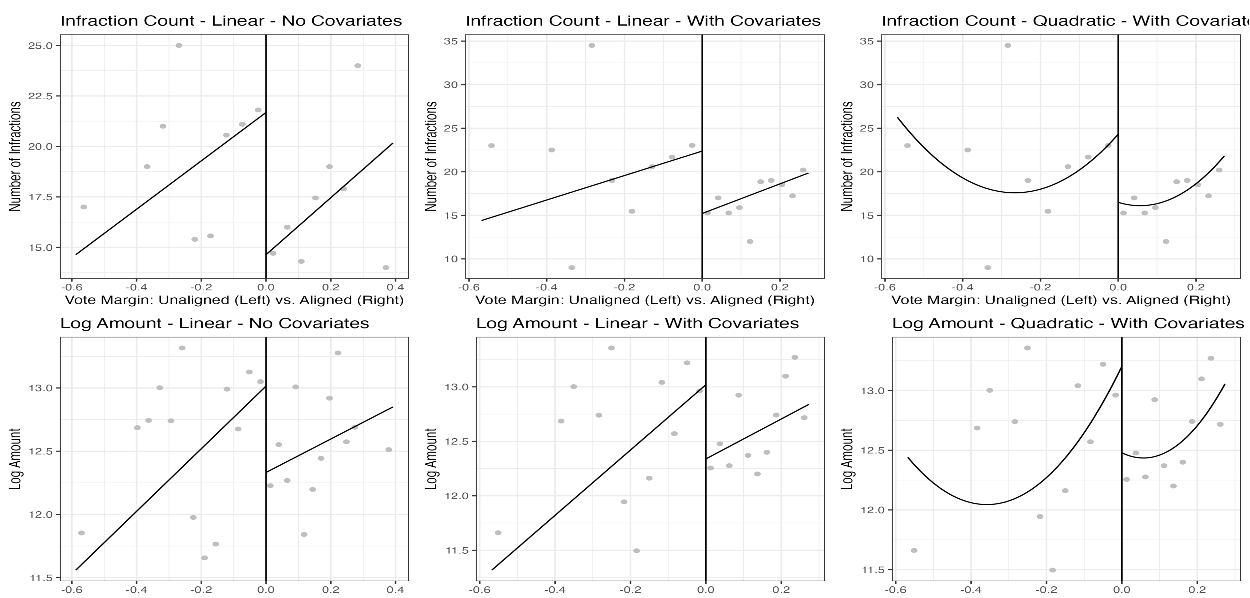


Figure E.2: RD Plots for Table E1 Panels B and D (Poverty ↓ Sample)

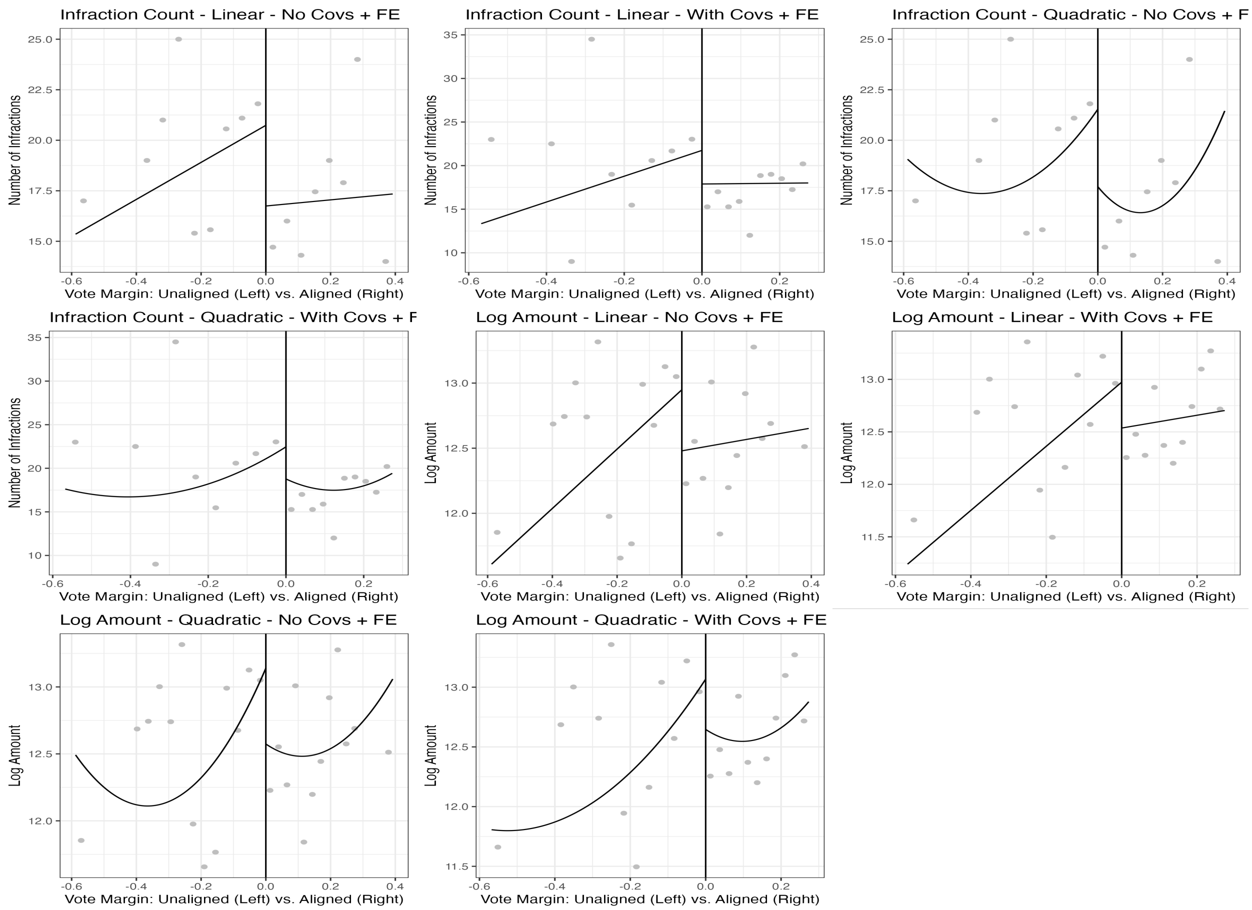


Figure E.3: RD Plots for Table E2 Panels A and C (Low-Poverty Sample)

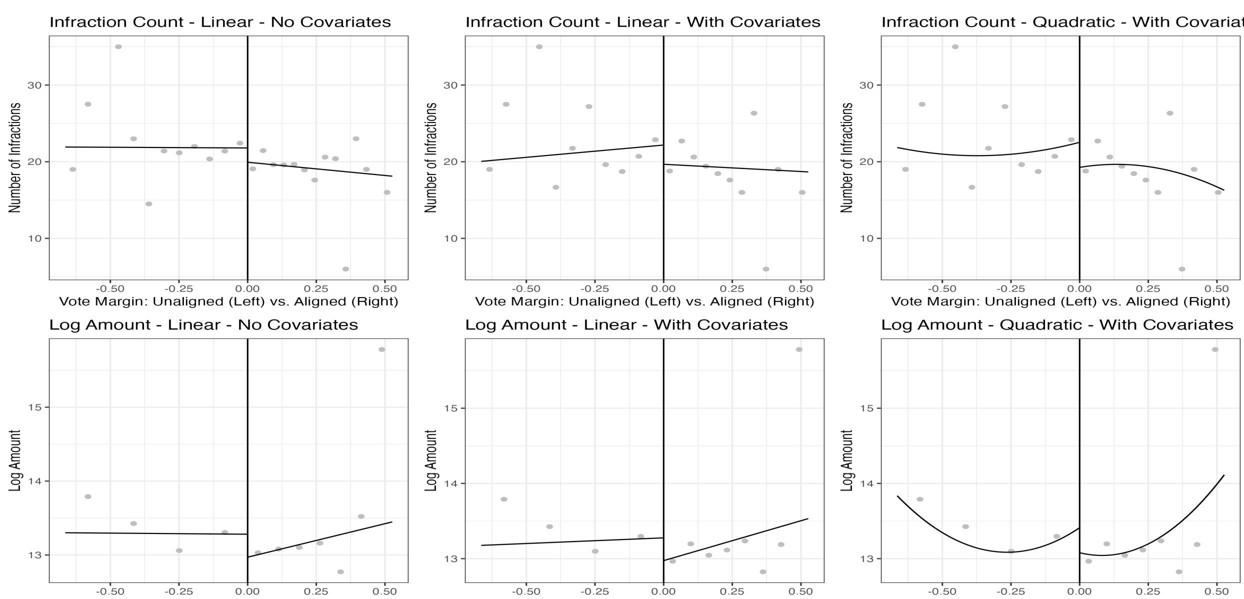


Table E2: RDD Estimates for Infraction Count/Amount (log) by Term (Low Poverty)

Panel A	(1)	(2)	(3)	(4)
RD Estimate	-5.810**	-6.086*	-4.781**	-5.215*
	(2.826)	(3.422)	(2.318)	(2.802)
Observations	284	284	267	267
Eff. obs.	[90,75]	[105,100]	[88,79]	[95,92]
Covariates	None	None	All	All
p-value	0.040	0.075	0.039	0.063
Order	1	2	1	2
Bandwidth	0.1110	0.1706	0.1227	0.1549

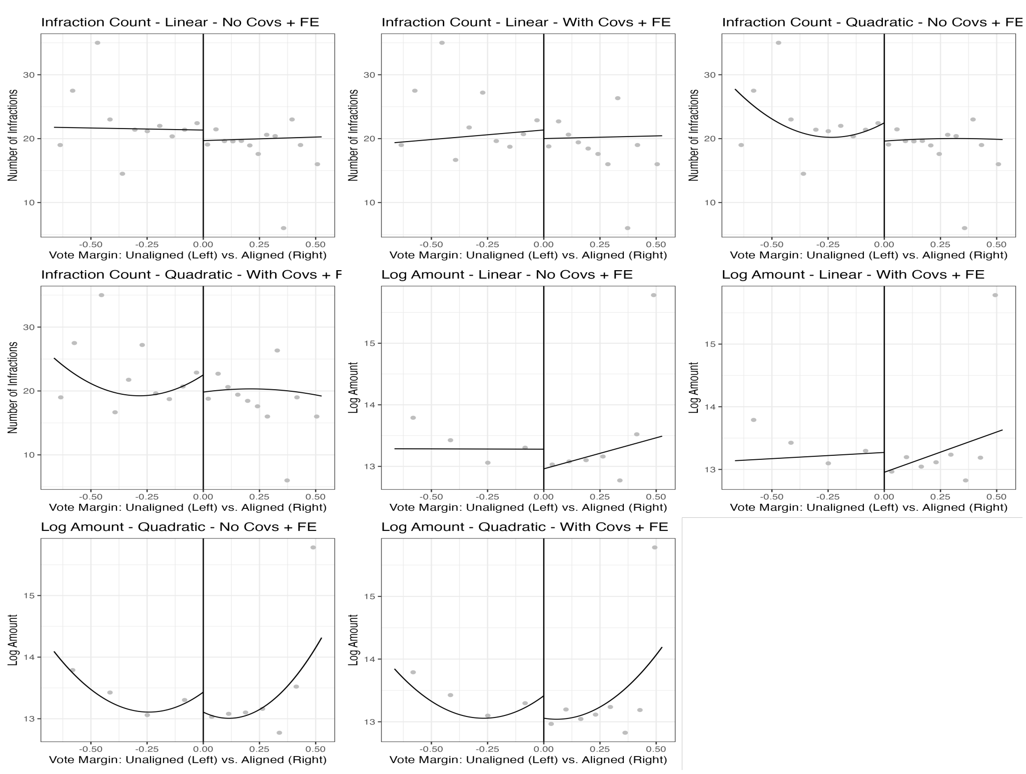
Panel B	(1)	(2)	(3)	(4)
RD Estimate	-5.568**	-6.486**	-5.293***	-5.790**
	(2.393)	(2.727)	(1.958)	(2.259)
Observations	284	284	267	267
Eff. obs.	[75,65]	[101,90]	[69,62]	[94,88]
Covariates	None	None	All	All
p-value	0.020	0.017	0.007	0.010
Order	1	2	1	2
Bandwidth	0.0849	0.1411	0.0862	0.1452

Panel C	(1)	(2)	(3)	(4)
RD Estimate	-0.562*	-0.665*	-0.505*	-0.530*
	(0.299)	(0.348)	(0.261)	(0.317)
Observations	284	284	267	267
Eff. obs.	[83,73]	[101,89]	[82,70]	[94,87]
Covariates	None	None	All	All
p-value	0.060	0.056	0.053	0.095
Order	1	2	1	2
Bandwidth	0.1005	0.1401	0.1065	0.1423

Panel D	(1)	(2)	(3)	(4)
RD Estimate	-0.592**	-0.678*	-0.585**	-0.629*
	(0.300)	(0.353)	(0.271)	(0.329)
Observations	284	284	267	267
Eff. obs.	[80,68]	[95,84]	[71,65]	[88,81]
Covariates	None	None	All	All
p-value	0.049	0.055	0.031	0.056
Order	1	2	1	2
Bandwidth	0.0914	0.1249	0.0902	0.1260

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows estimates for infraction count by electoral term without term fixed effects, while Panel B shows estimates with term fixed effects. Panel C shows estimates for infraction amount by electoral term without term fixed effects, while Panel D shows estimates 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), reelection dummy, and Gini coefficient as controls.

Figure E.4: RD Plots for Table E2 Panels B and D (Low-Poverty Sample)



F. Low Extreme Poverty/Extreme Poverty ↓ and High Poverty/Poverty ↑ Samples

Figure F.1: Low Extreme Poverty/Extreme Poverty ↓ Sample

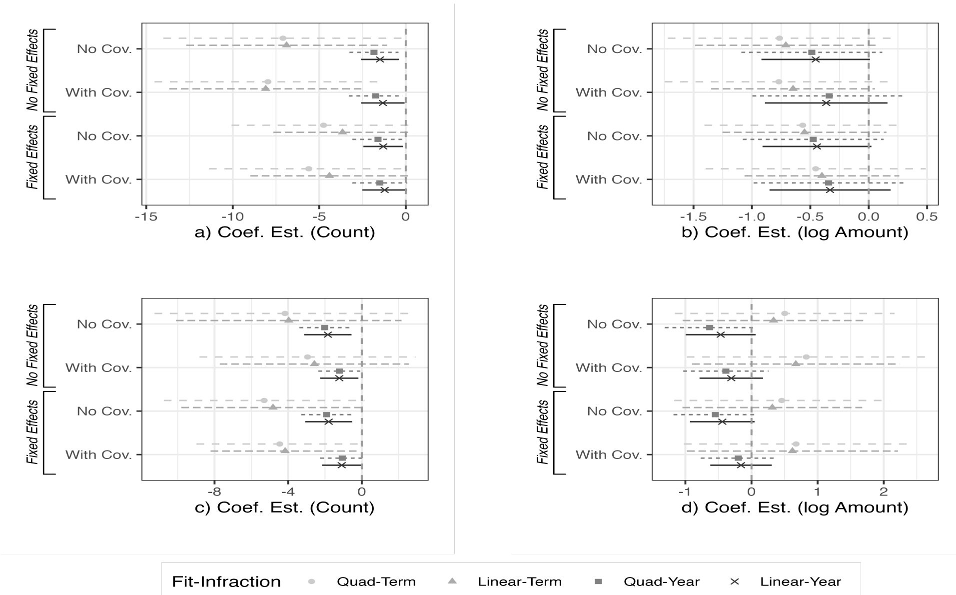


Table F1: RDD Estimates for Infraction Count/Amount (log) by Term (Poverty ↑ Sample)

Panel A	(1)	(2)	(3)	(4)
RD Estimate	4.224	6.547	1.254	3.433
	(3.797)	(6.035)	(4.063)	(6.582)
Observations	196	196	174	174
Eff. obs.	[55,62]	[57,76]	[44,56]	[46,70]
Covariates	None	None	All	All
p-value	0.266	0.278	0.758	0.602
Order	1	2	1	2
Bandwidth	0.1153	0.1359	0.1095	0.1319

Panel B	(1)	(2)	(3)	(4)
RD Estimate	1.448	1.881	1.767	2.932
	(3.180)	(4.584)	(3.533)	(5.159)
Observations	196	196	174	174
Eff. obs.	[54,59]	[59,79]	[43,52]	[46,69]
Covariates	None	None	All	All
p-value	0.649	0.682	0.617	0.570
Order	1	2	1	2
Bandwidth	0.1094	0.1442	0.0952	0.1302

Panel C	(1)	(2)	(3)	(4)
RD Estimate	0.602	0.391	0.405	0.455
	(0.371)	(0.548)	(0.400)	(0.477)
Observations	196	196	174	174
Eff. obs.	[55,61]	[60,79]	[44,57]	[58,88]
Covariates	None	None	All	All
p-value	0.104	0.476	0.311	0.340
Order	1	2	1	2
Bandwidth	0.1144	0.1471	0.1109	0.1955

Panel D	(1)	(2)	(3)	(4)
RD Estimate	0.516	-0.129	0.580*	0.506
	(0.329)	(0.651)	(0.332)	(0.450)
Observations	196	196	174	174
Eff. obs.	[57,74]	[57,63]	[47,73]	[58,86]
Covariates	None	None	All	All
p-value	0.117	0.843	0.081	0.261
Order	1	2	1	2
Bandwidth	0.1329	0.1179	0.1385	0.1882

Table F2: RDD Estimates for Infraction Count and Amount (log) by Term (High Poverty Sample)

Panel A	(1)	(2)	(3)	(4)
RD Estimate	5.120	8.026	10.911*	13.819*
	(4.747)	(7.297)	(5.622)	(7.276)
Observations	258	258	230	230
Eff. obs.	[64,61]	[80,76]	[48,44]	[63,62]
Covariates	None	None	All	All
p-value	0.281	0.271	0.052	0.058
Order	1	2	1	2
Bandwidth	0.0924	0.1223	0.0677	0.1065

Panel B	(1)	(2)	(3)	(4)
RD Estimate	4.130	5.662	7.681**	10.552*
	(3.425)	(5.127)	(3.874)	(5.453)
Observations	258	258	230	230
Eff. obs.	[63,61]	[80,77]	[50,49]	[63,62]
Covariates	None	None	All	All
p-value	0.228	0.269	0.047	0.053
Order	1	2	1	2
Bandwidth	0.0919	0.1231	0.0777	0.1083

Panel C	(1)	(2)	(3)	(4)
RD Estimate	1.266*	1.656*	1.669**	2.121**
	(0.726)	(0.900)	(0.812)	(0.968)
Observations	258	258	230	230
Eff. obs.	[68,66]	[84,80]	[54,52]	[72,69]
Covariates	None	None	All	All
p-value	0.081	0.066	0.040	0.028
Order	1	2	1	2
Bandwidth	0.1007	0.1349	0.0895	0.1262

Panel D	(1)	(2)	(3)	(4)
RD Estimate	1.203*	1.532*	1.556**	1.921**
	(0.690)	(0.891)	(0.772)	(0.964)
Observations	258	258	230	230
Eff. obs.	[69,68]	[87,82]	[56,54]	[73,70]
Covariates	None	None	All	All
p-value	0.081	0.086	0.044	0.046
Order	1	2	1	2
Bandwidth	0.1037	0.1373	0.0940	0.1317

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows estimates for infraction count by electoral term without term fixed effects, while Panel B shows estimates with term fixed effects. Panel C shows estimates for infraction amount by electoral term without term fixed effects, while Panel D shows estimates 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.

Figure F.2: RD Plots for Table F1 Panels A and C (Poverty-Increasing Sample)

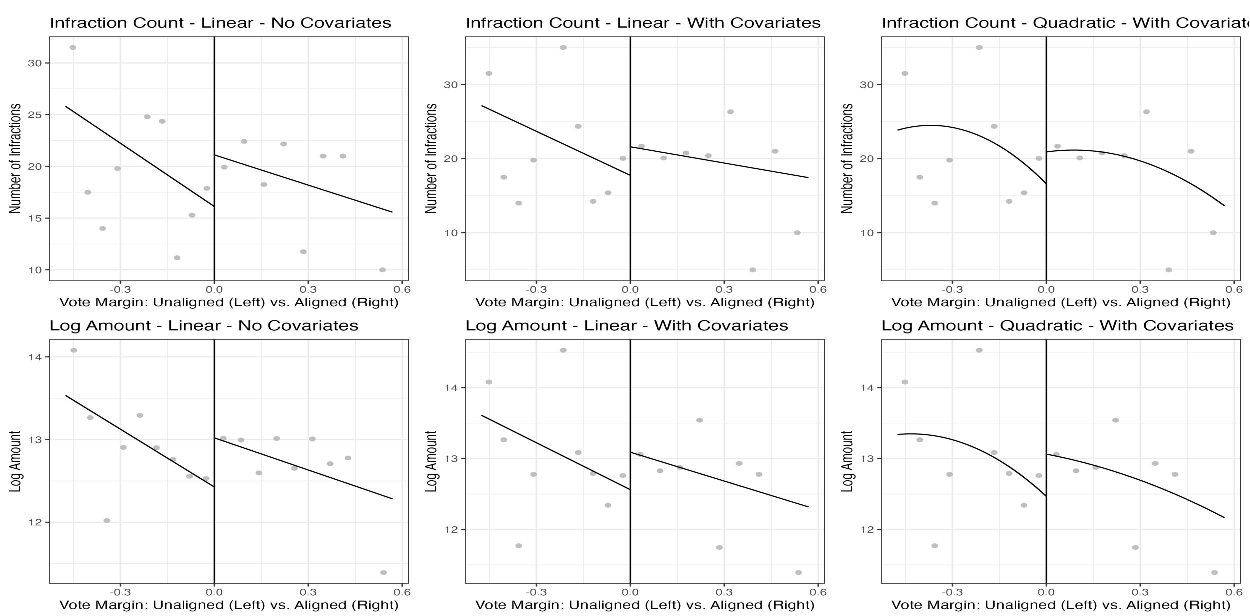


Figure F.3: RD Plots for Table F1 Panels B and D (Poverty-Increasing Sample)

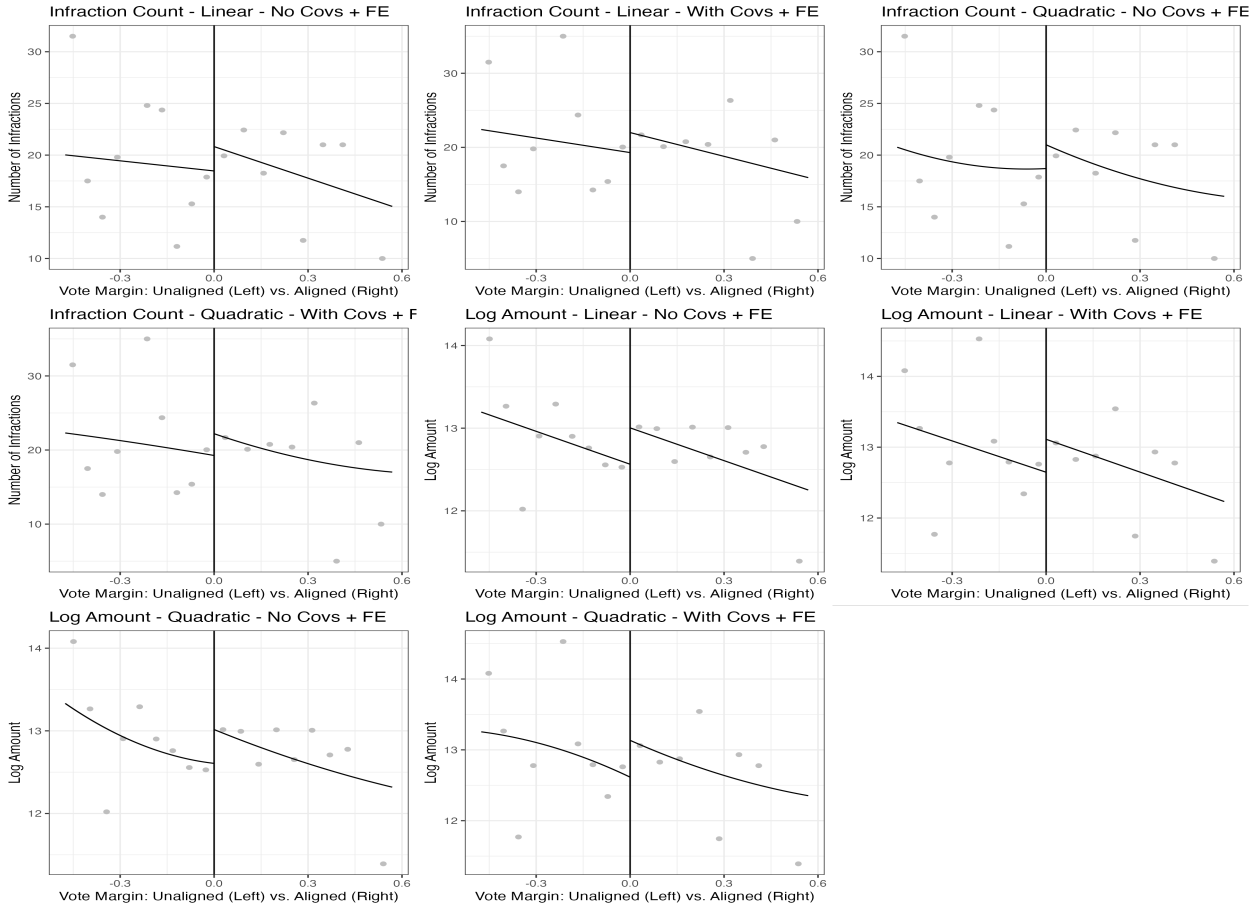


Figure F.4: RD Plots for Table F2 Panels A and C (High-Poverty Sample)

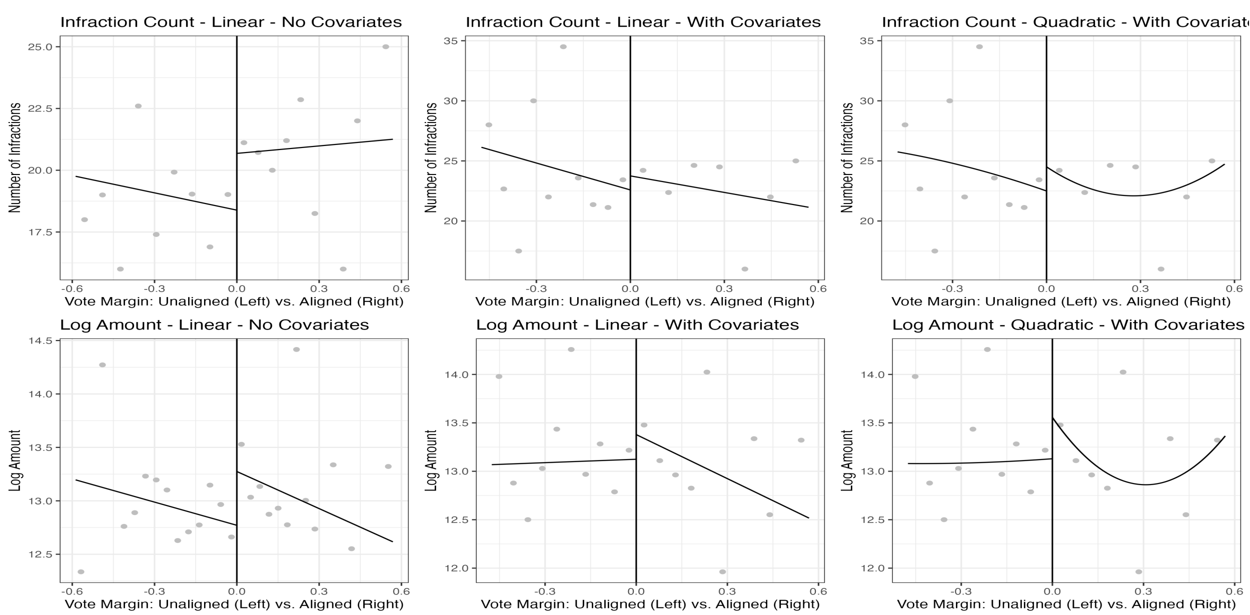
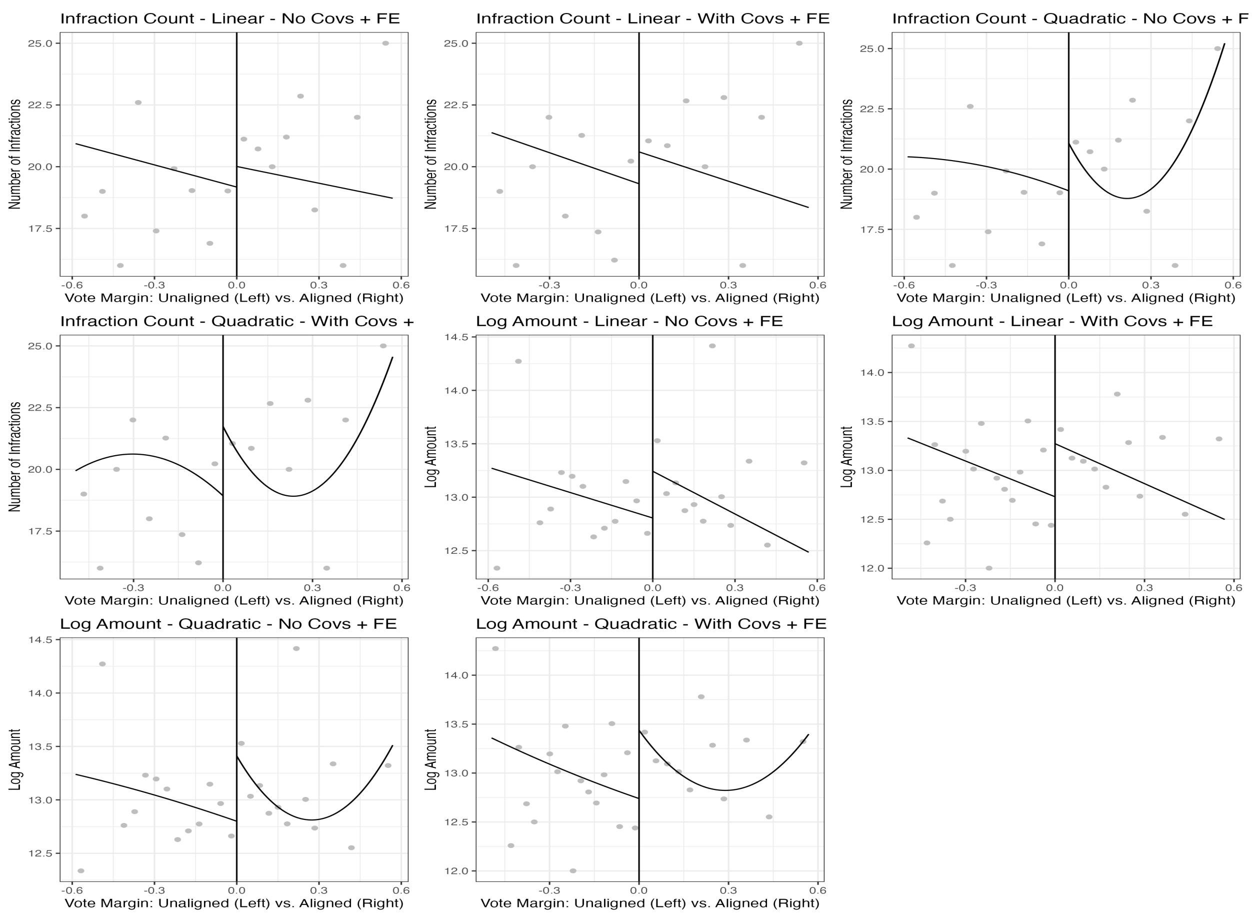


Figure F.5: RD Plots for Table F2 Panels B and D (High-Poverty Sample)



G. Last Two Years of the Electoral Term

Figure G.1: Last Two Years: Low Poverty/Poverty ↓ Samples

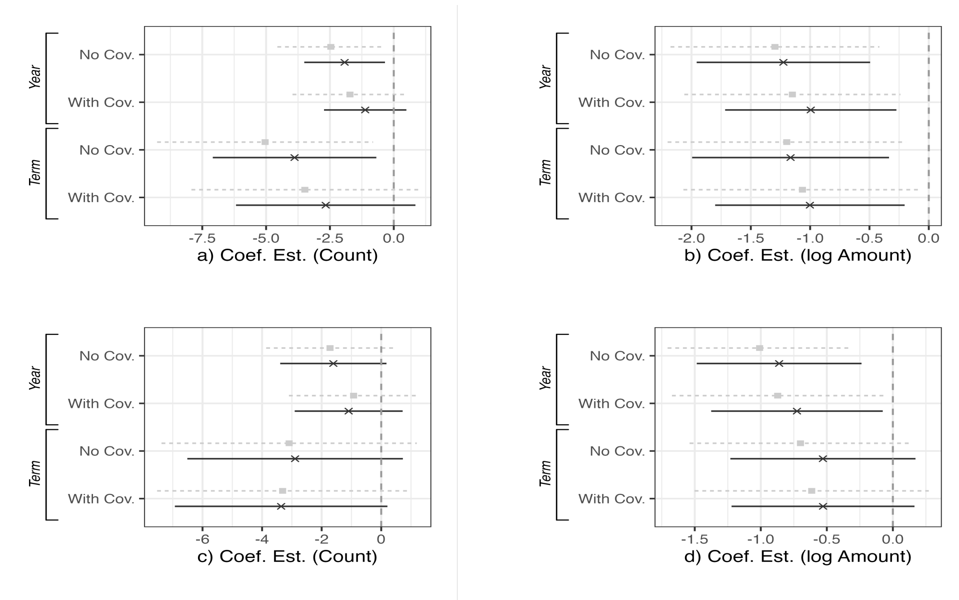
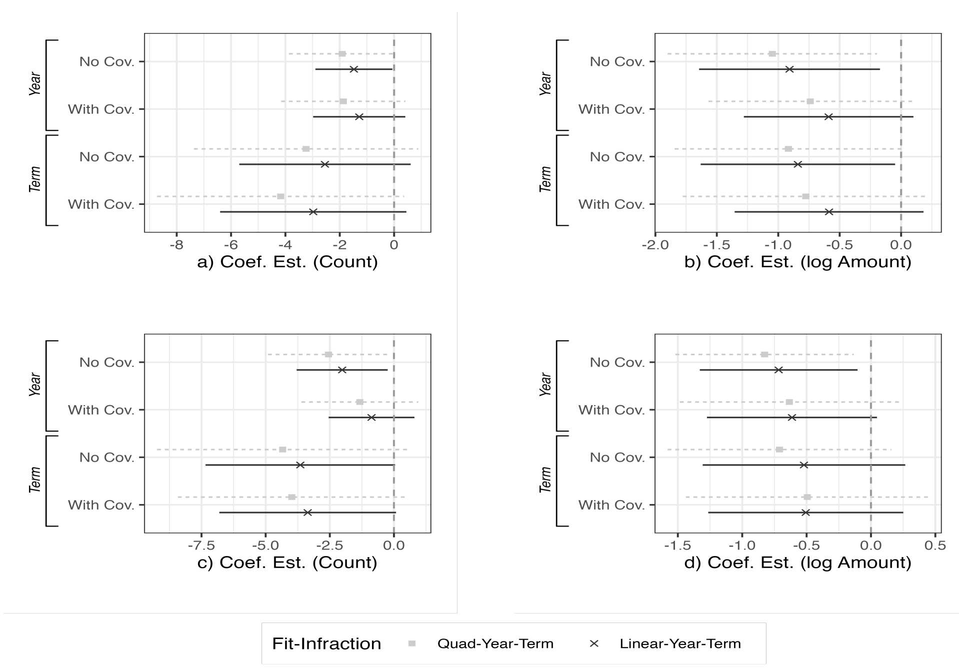


Figure G.2: Last Two Years: Low Extreme Poverty/Extreme Poverty ↓ Samples



Note: Panels A and B correspond to the poverty decreasing/increasing samples. Panels C and D correspond to the low/high poverty samples. All specifications use standard errors clustered by municipality. Whiskers depict 95% confidence intervals. “With covariates” uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

H. First Two Years

Figure H.1: First Two Years: Low Poverty/Poverty ↓ Samples

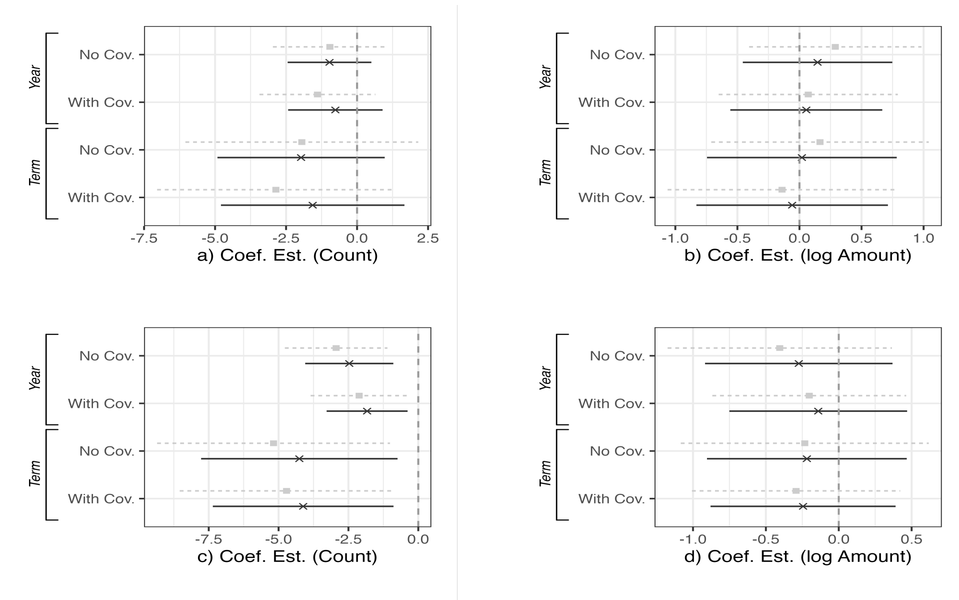
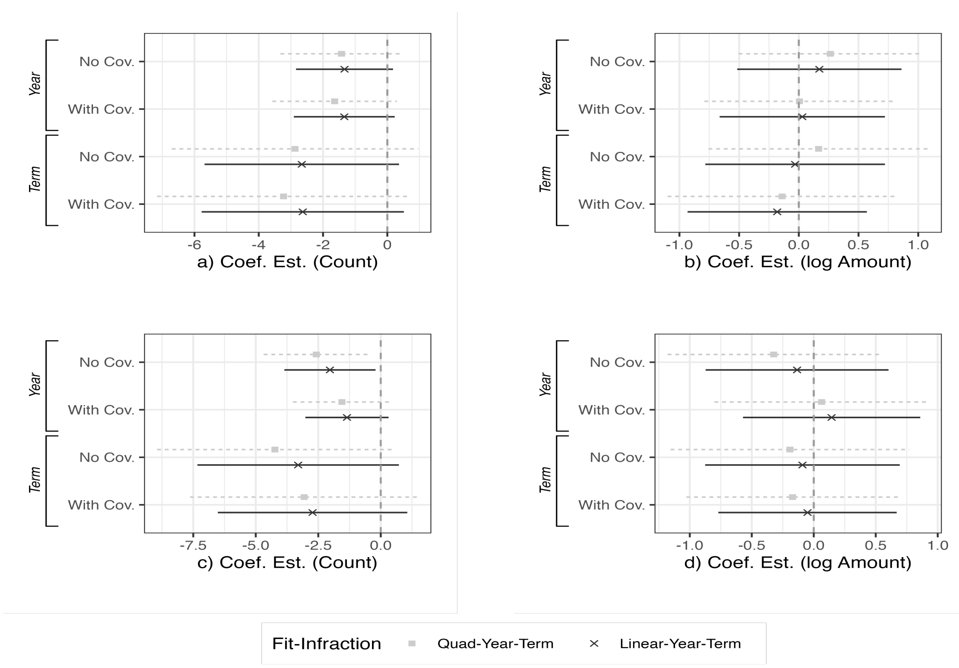


Figure H.2: First Two Years: Low Extreme Poverty/Extreme Poverty ↓ Samples



Note: Panels A and B correspond to the poverty decreasing/increasing samples. Panels C and D correspond to the low/high poverty samples. All specifications use standard errors clustered by municipality. Whiskers depict 95% confidence intervals. "No Cov." specifications do not use any controls. "With covariates" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

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

Table I1: RDD Estimates for Infraction Count and Amount (log) by Term (Whole Sample)

Panel A	(1)	(2)	(3)	(4)	Panel B	(1)	(2)	(3)	(4)
RD Estimate	-2.232	-3.996	-3.345	-4.084	RD Estimate	0.276	-0.196	-0.039	0.424
	(2.695)	(3.911)	(2.742)	(3.814)		(1.954)	(2.696)	(2.090)	(2.783)
Observations	440	440	372	372	Observations	440	440	372	372
Eff. obs.	[132,119]	[148,142]	[109,105]	[126,138]	Eff. obs.	[135,122]	[154,157]	[108,103]	[128,145]
Covariates	None	None	All	All	Covariates	None	None	All	All
p-value	0.408	0.307	0.223	0.284	p-value	0.888	0.942	0.985	0.879
Order	1	2	1	2	Order	1	2	1	2
Bandwidth	0.1107	0.1357	0.1123	0.1522	Bandwidth	0.1145	0.1523	0.1091	0.1573
Panel C	(1)	(2)	(3)	(4)	Panel D	(1)	(2)	(3)	(4)
RD Estimate	-0.054	-0.379	0.027	-0.206	RD Estimate	-0.020	-0.200	0.142	0.076
	(0.247)	(0.405)	(0.251)	(0.418)		(0.253)	(0.366)	(0.251)	(0.361)
Observations	440	440	372	372	Observations	440	440	372	372
Eff. obs.	[150,144]	[146,136]	[124,130]	[121,126]	Eff. obs.	[133,120]	[147,136]	[113,111]	[125,132]
Covariates	None	None	All	All	Covariates	None	None	All	All
p-value	0.827	0.350	0.915	0.622	p-value	0.937	0.585	0.570	0.832
Order	1	2	1	2	Order	1	2	1	2
Bandwidth	0.1380	0.1301	0.1429	0.1368	Bandwidth	0.1119	0.1307	0.1191	0.1461

Table I2: RDD Estimates for Infraction Count and Amount (log) by Term (Whole Sample; Levels)

Panel A	(1)	(2)	(3)	(4)	Panel B	(1)	(2)	(3)	(4)
RD Estimate	-1.323	-2.044	-1.337	-0.501	RD Estimate	-0.082	-1.540	-0.450	-0.790
	(2.158)	(2.585)	(2.249)	(3.230)		(1.626)	(2.637)	(1.599)	(2.458)
Observations	567	567	496	496	Observations	567	567	496	496
Eff. obs.	[191,169]	[236,238]	[156,145]	[172,166]	Eff. obs.	[198,178]	[198,178]	[169,159]	[175,178]
Covariates	None	None	All	All	Covariates	None	None	All	All
p-value	0.540	0.429	0.552	0.877	p-value	0.960	0.559	0.778	0.748
Order	1	2	1	2	Order	1	2	1	2
Bandwidth	0.1309	0.2121	0.1206	0.1462	Bandwidth	0.1401	0.1397	0.1403	0.1565
Panel C	(1)	(2)	(3)	(4)	Panel D	(1)	(2)	(3)	(4)
RD Estimate	-0.005	-0.022	0.007	0.046	RD Estimate	0.012	-0.046	0.015	0.031
	(0.230)	(0.335)	(0.249)	(0.298)		(0.231)	(0.343)	(0.242)	(0.327)
Observations	567	567	496	496	Observations	567	567	496	496
Eff. obs.	[193,174]	[202,197]	[151,137]	[195,197]	Eff. obs.	[189,169]	[201,186]	[153,139]	[174,177]
Covariates	None	None	All	All	Covariates	None	None	All	All
p-value	0.983	0.948	0.979	0.876	p-value	0.959	0.894	0.950	0.924
Order	1	2	1	2	Order	1	2	1	2
Bandwidth	0.1340	0.1541	0.1134	0.1859	Bandwidth	0.1301	0.1465	0.1157	0.1549

Note: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Panel A shows estimates for infraction count by electoral term without term fixed effects, while Panel B shows estimates with term fixed effects. Panel C shows estimates for infraction amount (log) by electoral term without term fixed effects, while Panel D shows estimates 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), reelection dummy, and Gini coefficient as controls.

Figure I.1: RD Plots for Table I1 Panel A and C (Poverty Change Sample)

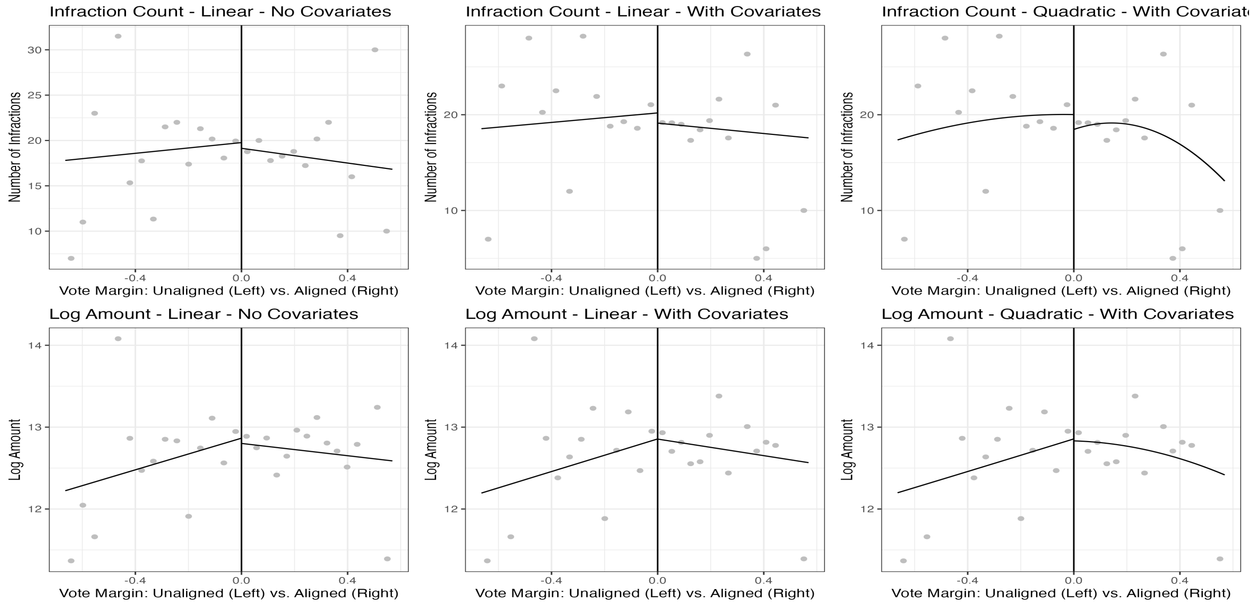


Figure I.2: RD Plots for Table I1 Panel B and D (Poverty Change Sample)

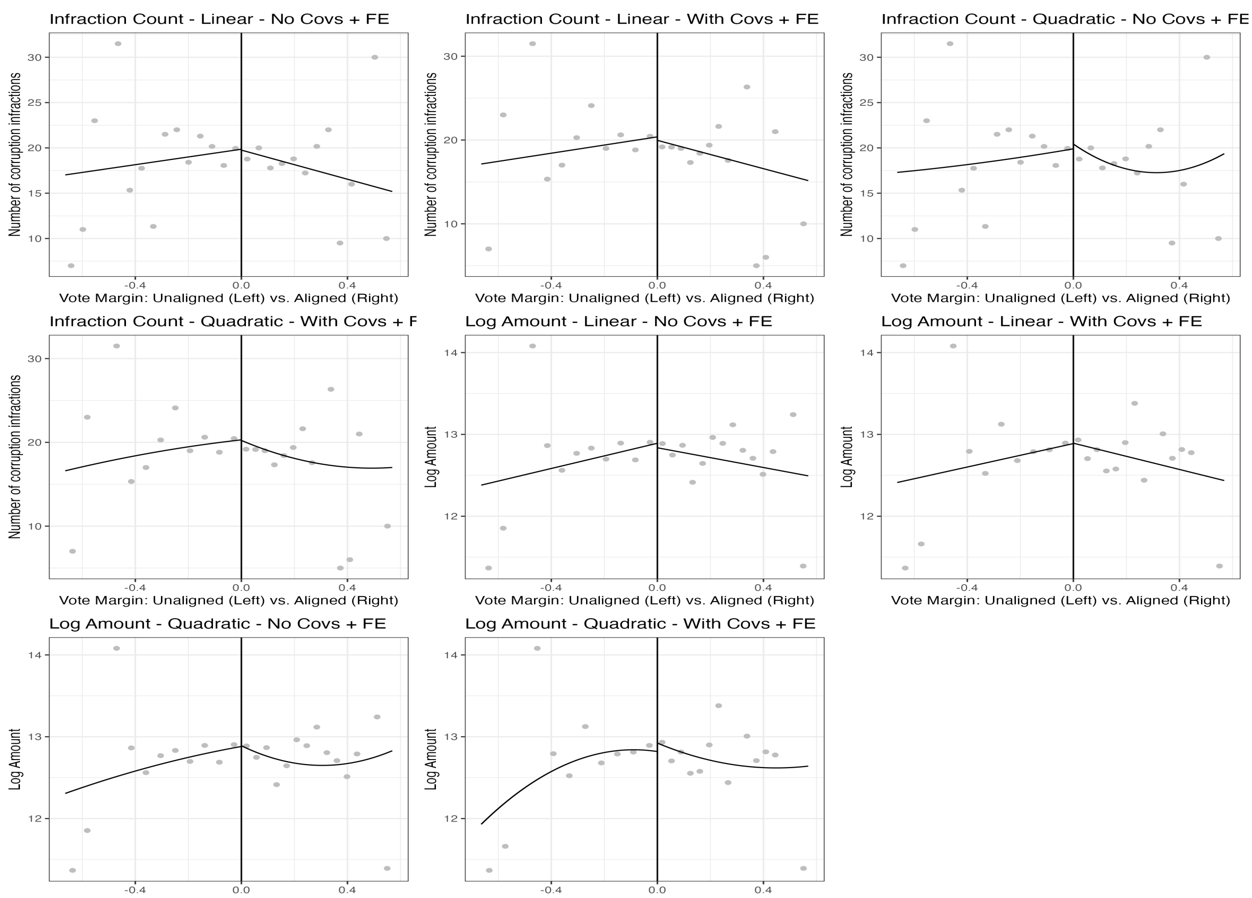


Figure I.3: RD Plots for Table I2 Panel A and C (Poverty High/Low Sample)

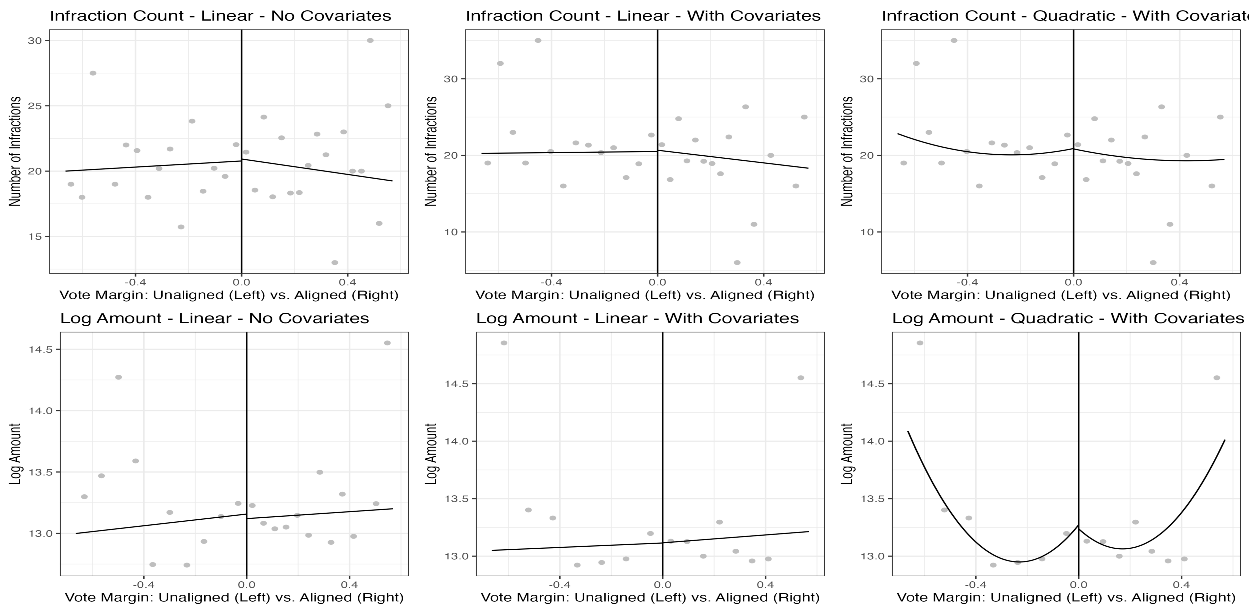
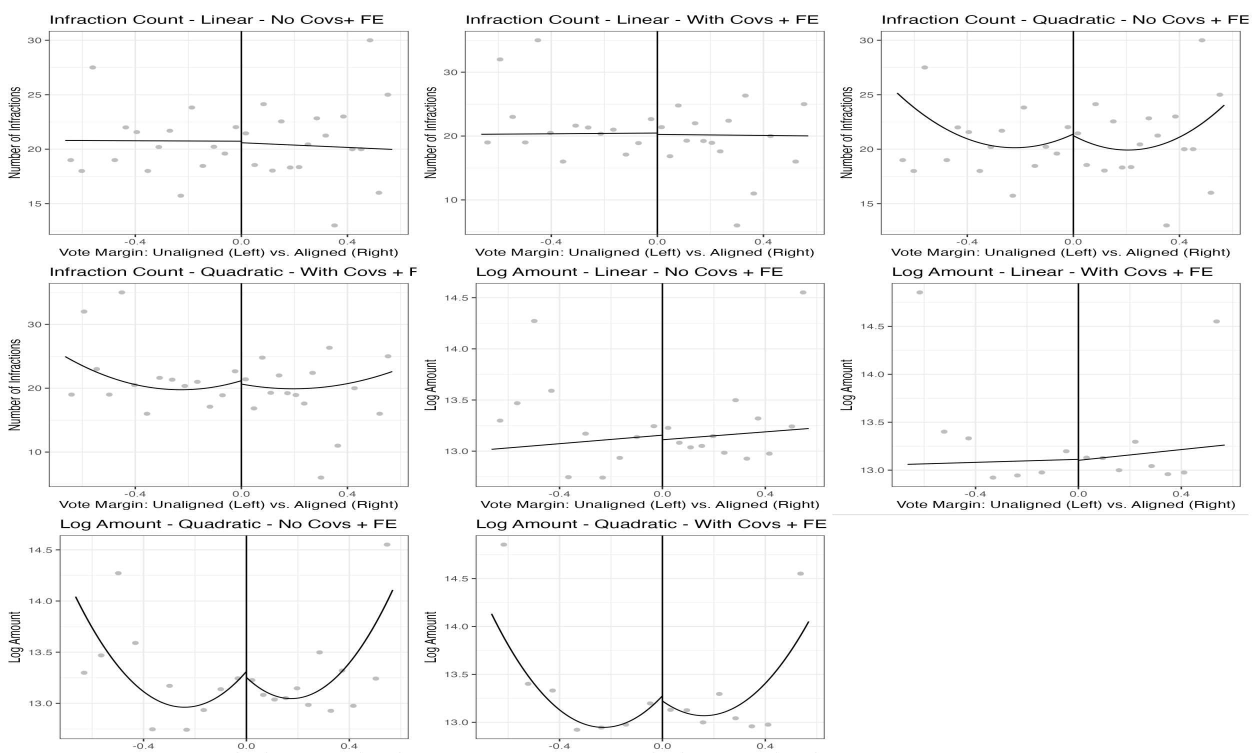
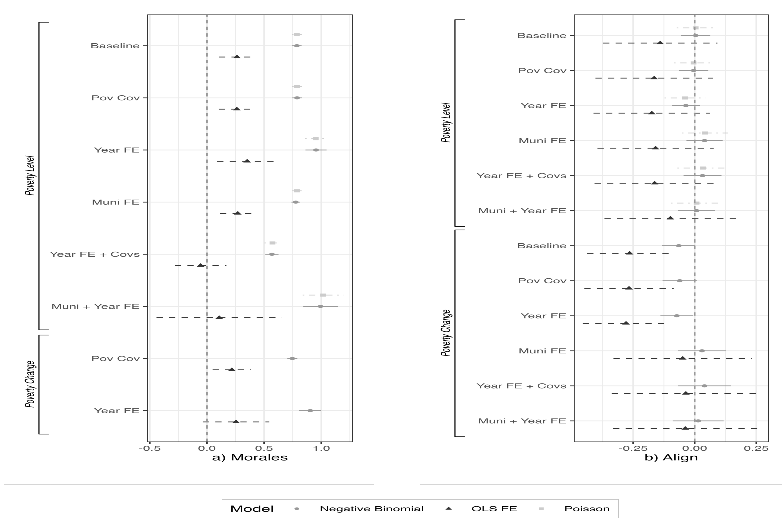


Figure I.4: RD Plots for Table I2 Panel B and D (Poverty High/Low Sample)



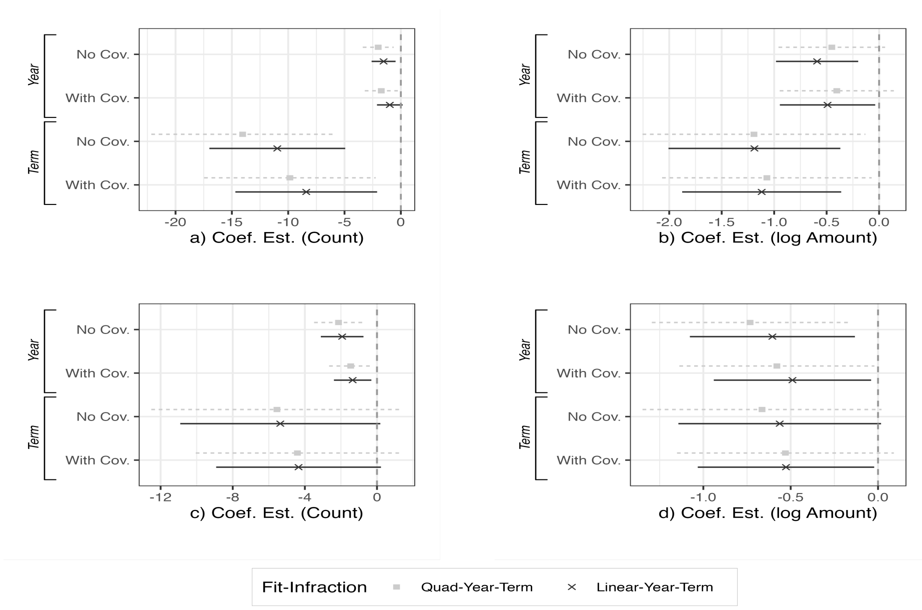
J. Additional Regression Results



Note: Panel A shows number of infractions committed during Morales' term (2008-2019). "Poverty Level" and "poverty change" brackets denote the inclusion of poverty level and poverty change covariates. Panel B shows number of infractions committed after close elections by alignment. "Pov cov" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

K. RDD Robustness Checks: Term and Year

Figure K.1: Low Poverty/Poverty ↓ Samples: No Outliers



Note: Panels A and B correspond to the poverty decreasing sample. Panels C and D correspond to the low poverty sample. All specifications use standard errors clustered by municipality. Whiskers depict 95% confidence intervals. "No Cov." specifications do not use any controls. "With covariates" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

Figure K.2: Placebo Tests: Low Poverty/Poverty ↓ Samples

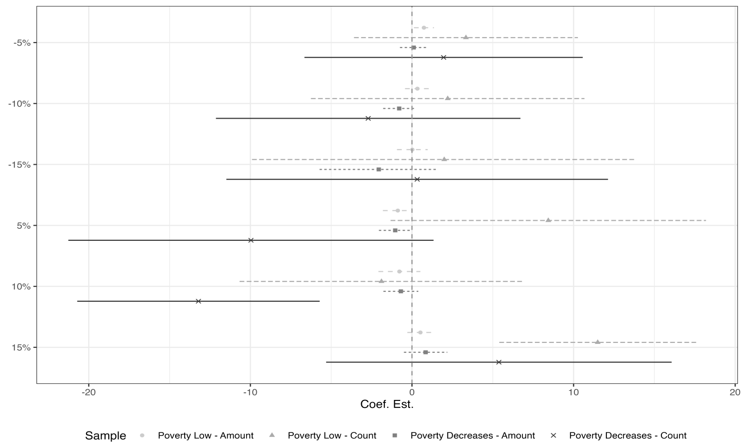


Figure K.3: RDD Estimates for Number of Audits in a Term

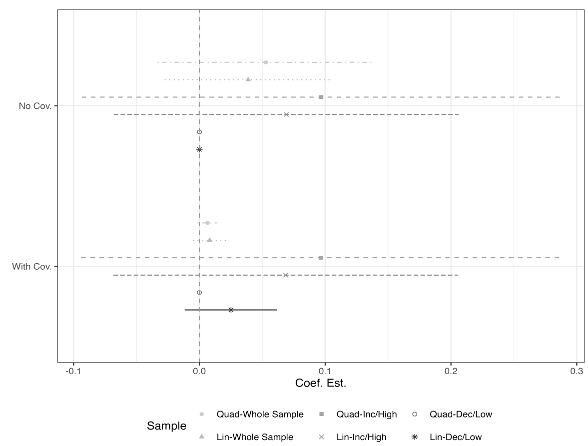
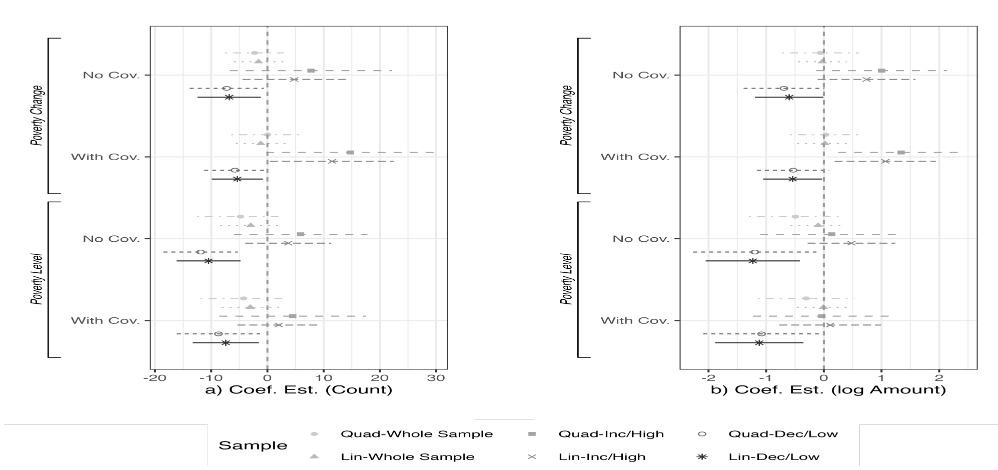
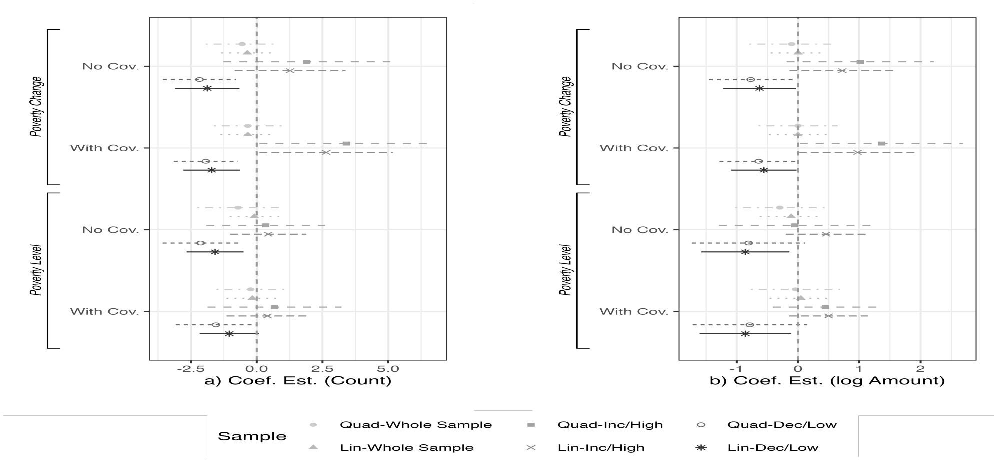


Figure K.4: RDD Estimates for Municipalities with no Missing Audits in a Term



Note: Both panels correspond to the poverty change and poverty levels samples. All specifications use standard errors clustered by municipality. Whiskers depict 95% confidence intervals. "No Cov." specifications do not use any controls. "With covariates" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

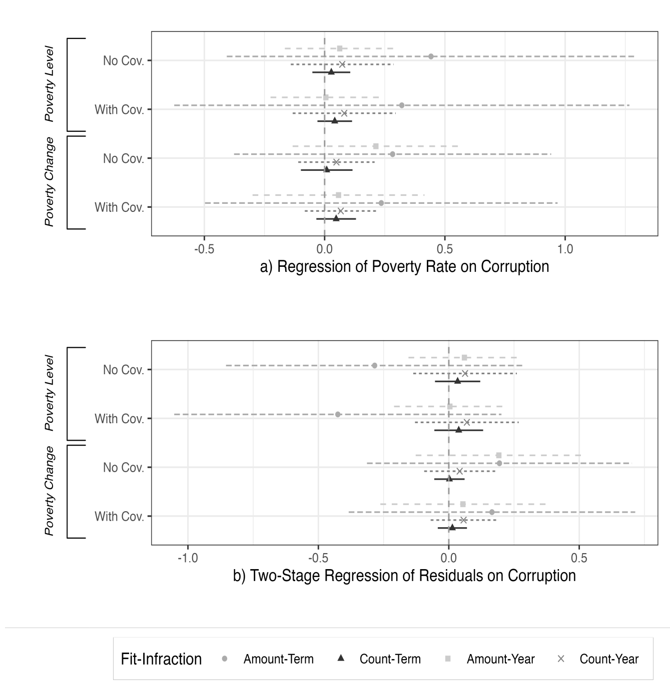
Figure K.5: RDD Estimates for Average Infractions per Audit in a Term



Note: Both panels correspond to the poverty change and poverty levels samples. All specifications use standard errors clustered by municipality. Whiskers depict 95% confidence intervals. "No Cov." specifications do not use any controls. "With covariates" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

L. Testing Poverty/Corruption Endogeneity

Figure L.1: Regression Estimates



Note: All specifications use standard errors clustered by municipality. In Panel A, the dependent variable is the average total poverty rate in the municipality in the given term. Panel B shows the second stage regression result of residuals on infraction count. Residuals from the first stage are obtained by regressing average total poverty in a term on covariates. All specifications use baseline Term and Municipality fixed-effects and cluster standard errors by municipality. Whiskers depict 95% confidence intervals. "No Cov." specifications do not use any controls. "With covariates" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

M. Results for 2008-2015/2009-2015/2011-2015

Figure M.1: Results for 2008-2015

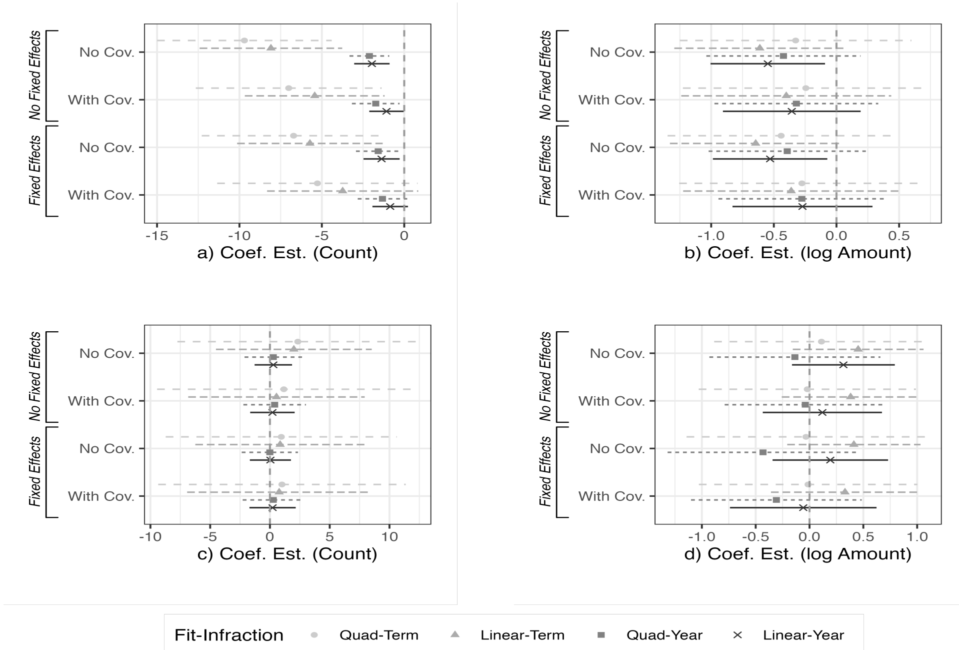
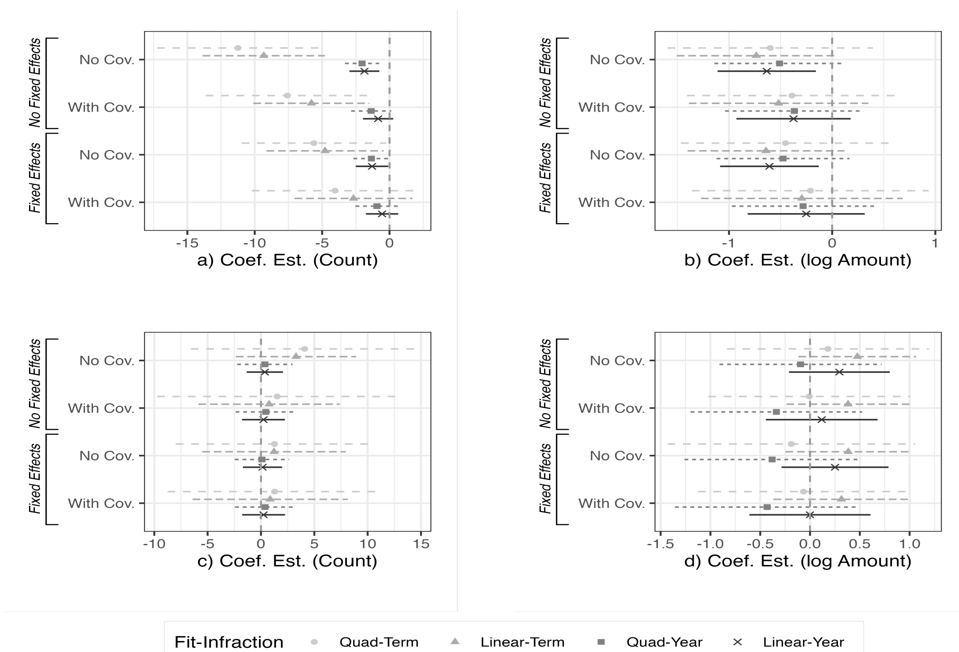
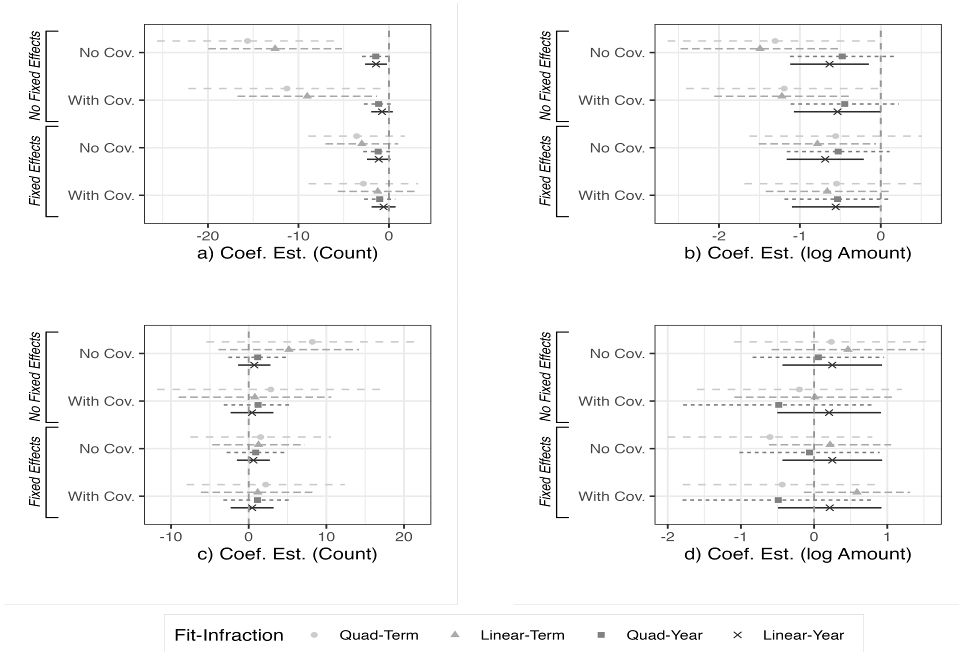


Figure M.2: Results for 2009-2015



Note: Panels A and B correspond to the poverty-decreasing sample. Panels C and D correspond to the poverty-increasing sample. All specifications use standard errors clustered by municipality. Whiskers depict 95% confidence intervals. "No Cov." specifications do not use any controls. "With covariates" uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

Figure M.3: Results for 2011-2015



Note: Panels A and B correspond to the poverty decreasing sample. Panels C and D correspond to the poverty increasing sample. All specifications use standard errors clustered by municipality. Whiskers depict 95% confidence intervals. “No Cov.” specifications do not use any controls. “With covariates” uses the canonical adjustment set identified by the DAG in Figure 3–(log) population, a mayor re-election indicator, and inequality (Gini).

N. Poverty Rates For Different Samples

Table N1: Total & Extreme Poverty Rates (2002 & 2011) Across Samples

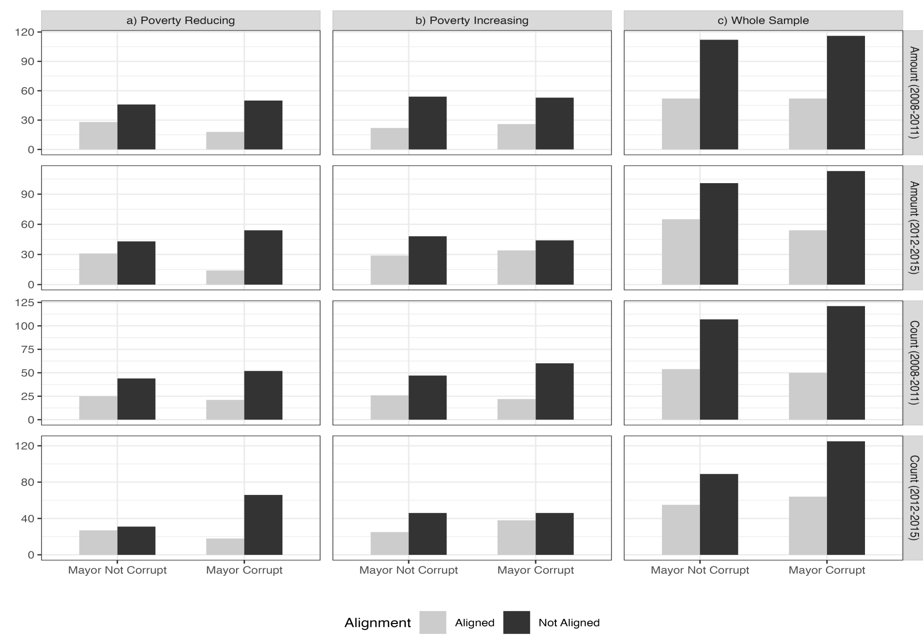
Sample	Mean Total Poverty		Mean Extreme Poverty	
	2002 (Total)	2011 (Total)	2002 (Extreme)	2011 (Extreme)
Whole Sample	63.87	69.51	19.79	20.84
	(21.46)	(16.87)	(14.27)	(15.47)
Whole Sample (including missing 2011)	63.87	65.84	19.79	19.28
	(21.46)	(20.21)	(14.27)	(15.51)
Municipalities Both in 2002 & 2011	67.34	69.51	21.42	20.84
	(18.91)	(16.87)	(14.01)	(15.47)
Municipalities Only in 2002	33.59	NA	5.59	NA
	(18.55)		(6.61)	
Poverty-Reducing Sample	76.12	64.72	26.99	13.92
	(13.25)	(15.90)	(13.66)	(8.49)
Poverty-Increasing Sample	59.30	73.75	15.33	28.31
	(19.76)	(16.67)	(11.69)	(17.78)
Low-Poverty Sample	46.30	59.41	8.22	15.66
	(15.67)	(15.97)	(4.50)	(11.08)
High-Poverty Sample	81.34	77.61	31.29	25.09
	(7.82)	(12.72)	(10.96)	(17.21)

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

As shown in Table N1, the 34 urban municipalities for which there are only poverty and extreme poverty data in 2002 exhibit less poverty and extreme poverty than the 299 other municipalities in the whole sample. Additionally, the literatures on poverty traps (e.g., Sachs, 2005), clientelism (e.g., Keefer, 2007*a*), and modernization itself (e.g., Lipset, 1959) indicate that more rural areas are less likely to undergo modernization processes. In short, the results that we find in this article based on more rural areas are less likely from a theoretical perspective. Accordingly, we conjecture that the inclusion of the missing poverty data from the less-poor, urban municipalities would, if anything, reinforce our results. In all likelihood, though, the missing data would not change much of anything. First, if the data actually existed (and they do not according to email communication Guatemala's National Statistical Office), the data would be divided between the low-poverty and high-poverty sample, or the poverty-increasing sample and the poverty-decreasing sample. Second, the data in each sample would be further attenuated based on whether Calonico, Cattaneo and Titiunik's (2014) algorithm for regression discontinuity analysis classified the municipality-year as having a close election. In technical terms, the observation would have to be an "effective observation", and the likelihood of any particular observation being an effective observation is circa 50-60% in our models. Therefore, adding the missing the observations would likely only add a minimal number of observations to each sample, thereby making the missing data rather insignificant from a statistical power perspective.

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

Figure O.1: Dichotomous Corruption Results

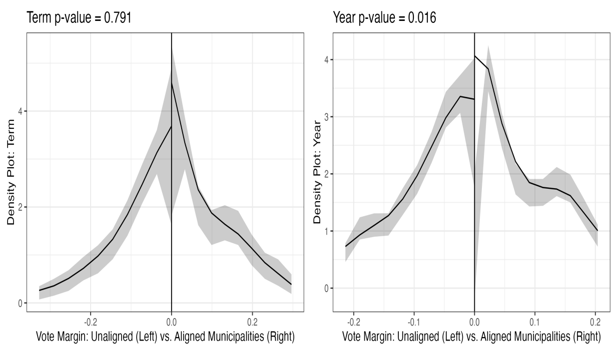


Note: For the "Count (2008-2011)" and "Count (2012-2015)" rows, "Mayor Not Corrupt" and "Mayor Corrupt" are defined as the count of municipalities with the total number of infractions being above/below the median by term. For the "Amount (2008-2011)" and "Amount (2012-2015)" rows, "Mayor Not Corrupt" and "Mayor Corrupt" are defined as the count of municipalities with the log amount of stolen/misappropriated money associated with audit infractions being above/below the median by term. All specifications use standard errors clustered by municipality. Whiskers depict 90% confidence intervals. Column A reports the results by alignment status for the poverty-decreasing sample, Column B presents results by alignment status for the poverty-increasing sample, and Column C provides the same results but for the whole sample.

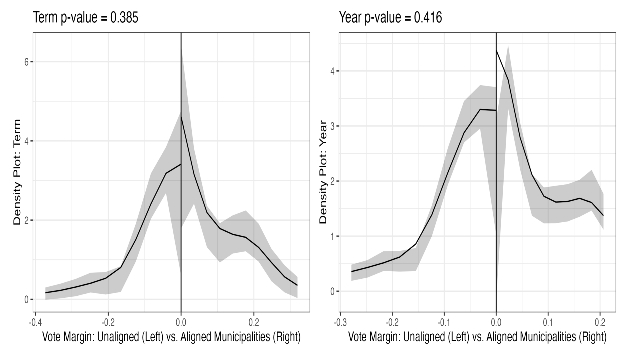
P. Density Plots

Figure P.1: RDD Density Plots for Infraction Count and Amount

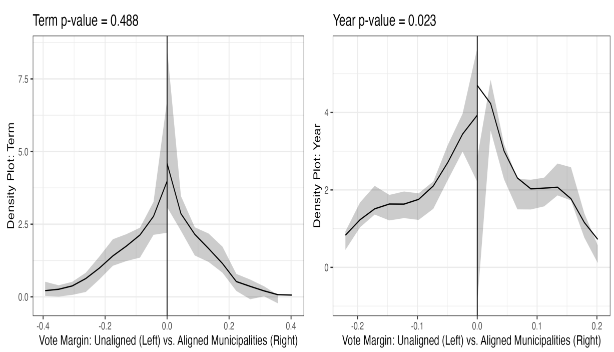
(a) (Whole Sample)



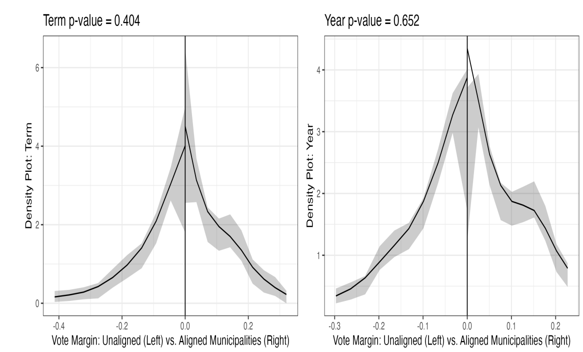
(b) (Low-Poverty Sample)



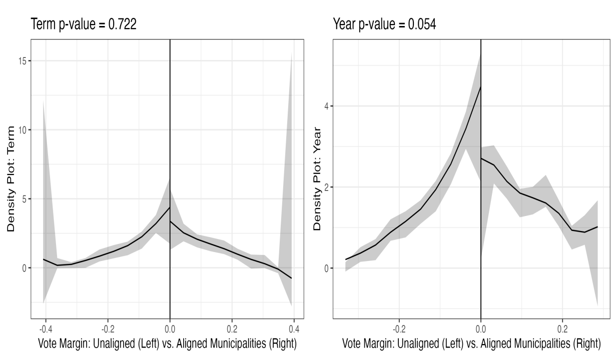
(c) (High-Poverty Sample)



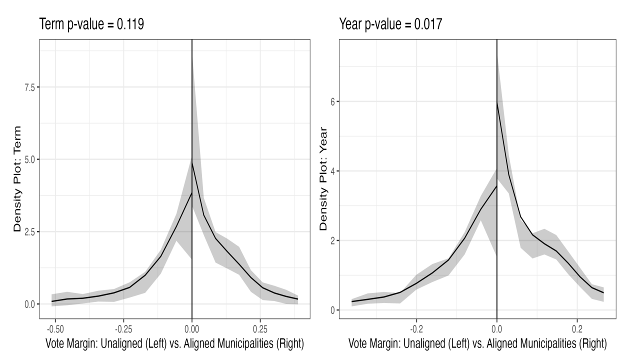
(d) Increasing/Decreasing Sample: 2010-2015 (Main Results)



(e) (Poverty-Decreasing Sample)



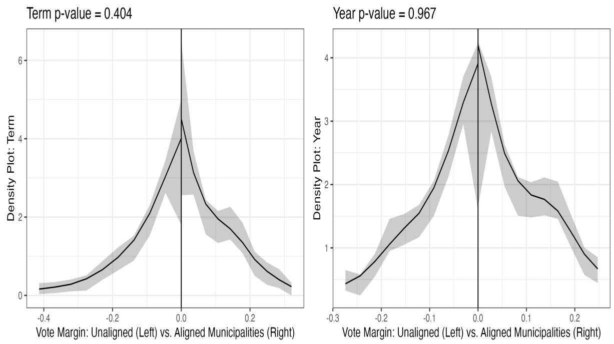
(f) (Poverty-Increasing Sample)



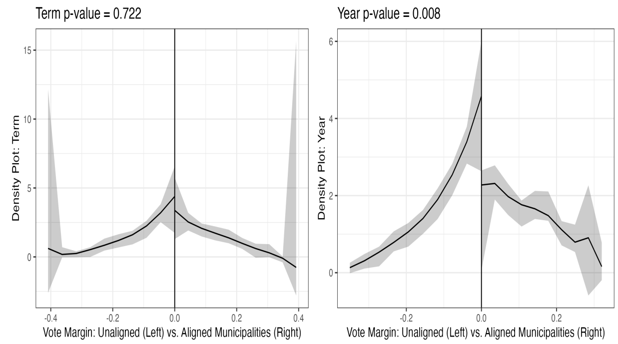
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. The electoral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

Figure P.2: RDD Density Plots for Infraction Count and Amount (Subsamples)

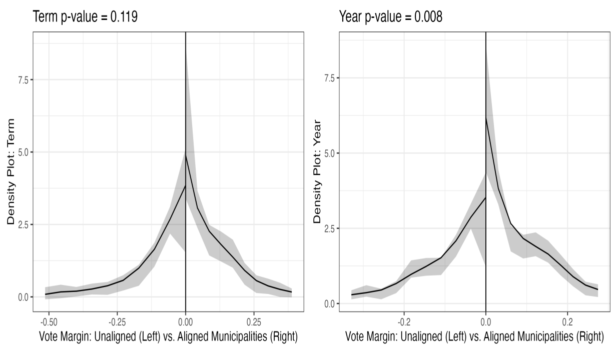
(a) (Whole Increasing/Decreasing Sample: 2011-2015)



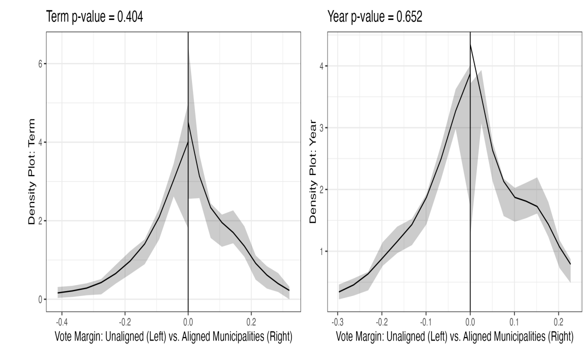
(b) (Poverty-Decreasing Sample: 2011-2015)



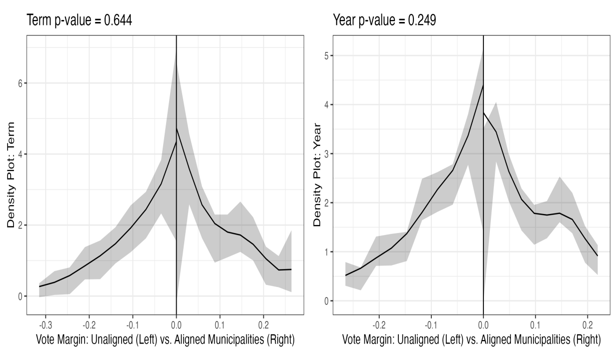
(c) (Poverty-Increasing Sample: 2011-2015)



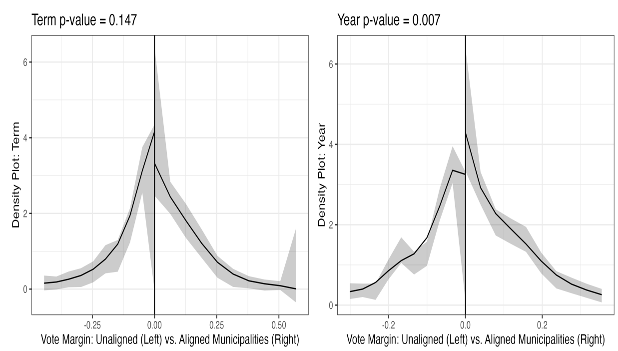
(d) (Whole Increasing/Decreasing Sample: 2009-2015)



(e) (Poverty-Decreasing Sample: 2009-2015)



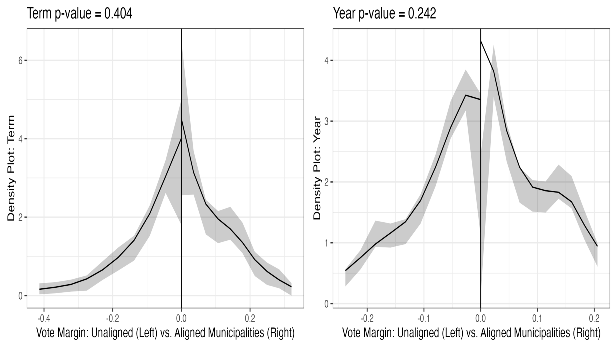
(f) (Poverty-Increasing Sample: 2009-2015)



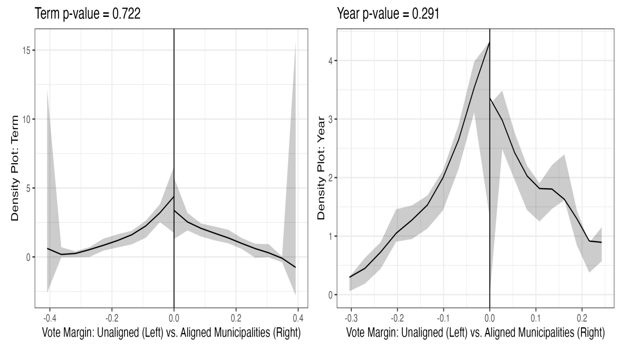
Note: "Term" refers to the margin of victory for mayors in each electoral term. "Year" refers to the same margin of victory variable but corresponding to a year-wise perspective. Following Cattaneo, Jansson and Ma (2018), all McCrary (2008) density tests are fit with second-order polynomials. The electoral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

Figure P.3: RDD Density Plots for Infraction Count and Amount (Subsamples Cont.)

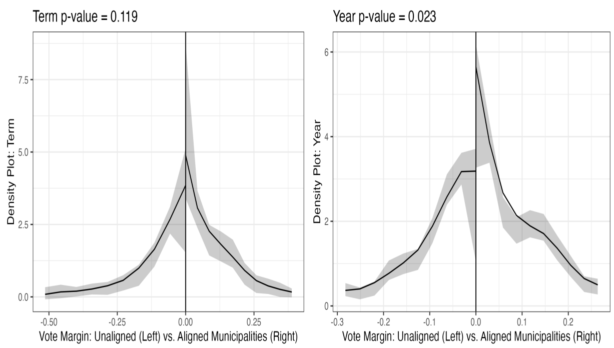
(a) (Whole Increasing/Decreasing Sample: 2008-2015)



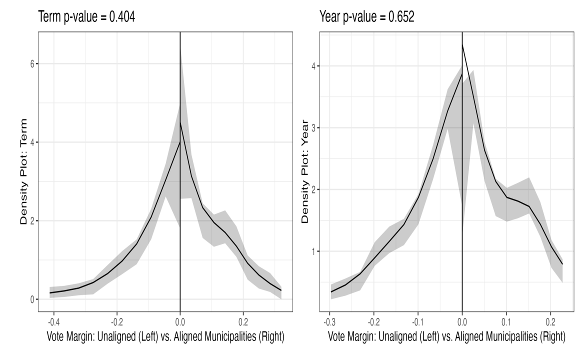
(b) (Poverty-Decreasing Sample: 2008-2015)



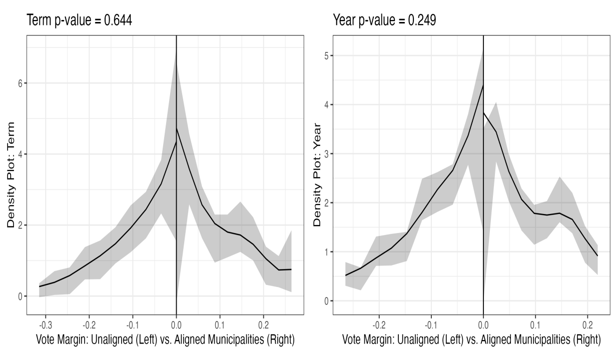
(c) (Poverty-Increasing Sample: 2008-2015)



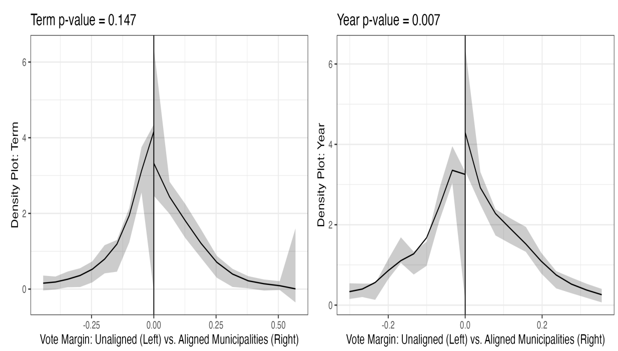
(d) (Whole Extreme Poverty Sample 2010-2015: Year and Term)



(e) (Extreme Poverty-Decreasing Sample)



(f) (Extreme Poverty-Increasing Sample)



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