IDEOLOGY AND CORRUPTION

Paula López-Villalba (University of Michigan)
Christian Ruzzier (Universidad de San Andrés)

Primera versión: Febrero 2026
Esta versión: Marzo 2026

Documento de Trabajo Nº 178
Departamento de Economía
Universidad de San Andrés

Vito Dumas 284, B1644BID, Victoria,
San Fernando, Buenos Aires, Argentina
Teléfono +54 11 7078 0400
Email: economia@udesa.edu.ar

Ideology and Corruption

PAULA A. LÓPEZ-VILLALBA† and CHRISTIAN A. RUZZIER‡
† *Department of Economics, University of Michigan, 238 Lorch Hall, 611 Tappan Ave., Ann Arbor, MI 48109-1220, USA*
(e-mail: plopezv@umich.edu)
‡ (Corresponding author) *Department of Economics, Universidad de San Andres, Vito Dumas 284, B1644BID Victoria, Buenos Aires, Argentina*
(e-mail: cruzzier@udesa.edu.ar) (phone: +54 11 7078 0400 ext 4570)

March 12, 2026

Abstract

The most corrupt countries are developing or transition countries, have low income levels, and are closed economies. More interesting, corruption is significantly positively correlated with left-wing governments in a simple cross section of countries – conditional on GDP per capita, income inequality, dominant religion, a recent history of war, and a history of communist rule. Correlation is not causality, however. In this paper, we analyze the direct relevance of government ideology for the extent of corruption, which is a question that has received little attention in the literature so far. Exploiting close electoral races across the world in the post-WWII period in a regression discontinuity design, we compare the levels of corruption in countries where the left wins and loses elections by a small margin, and find that electing a left-wing government has a significant and positive effect on corruption. We also link our results to extended government intervention in the economy and lower-quality bureaucracies under the left.

Keywords: corruption, ideology, partisan effects, close elections.

JEL codes: D72, D73, D78, H11, K42.

1. Introduction

Corruption (the misuse of public office for private gain) costs at least 3-5% of world GDP each year (Svensson, 2005; Bonnano et al., 2025), and constitutes a major impediment to economic development (see, e.g., Mauro, 1995; and Olken and Pande, 2012). Non-surprisingly, efforts to understand and explain corruption have increased sharply in recent years – especially since the late 1990s, with the increasing quality and availability of data (Jain, 2001; Brown et al., 2011; Dimant and Tosato, 2018). In looking for the common characteristics of corrupt countries, the literature has identified several robust correlations (Svensson, 2005). Corruption is negatively associated with, for instance, income, education, Protestant religion, common law, deregulation, media diffusion, democracy, economic freedom, and international trade – to name a few (see the survey by Dimant and Tosato, 2018, for references and other determinants).

More interesting for our purposes in this paper, corruption appears to be positively correlated with the presence of a left-wing government – even after controlling for GDP per capita, income inequality, and dummies for the dominant religion, a recent history of war, and a history of communist rule (Svensson, 2005; Di Tella and MacCulloch, 2009). Needless to say, correlation does not imply causation. The identification problem in generating unbiased estimates of the effect of electing a government of a certain ideology on corruption is challenging because of omitted variables (e.g., the preferences of the voters), and reverse causality (for instance, if increases in corruption in a country precede increases in left-wing voting, as found by Di Tella and MacCulloch, 2009).

This paper estimates the causal effect of electing a government with a left-wing ideology on overall corruption. To deal with endogeneity issues, we compare countries where the left wins and loses elections by a small margin. We employ a regression-discontinuity (RD) design on an original dataset of worldwide national general (parliamentary and presidential) elections in the post-WWII period (1945–2018) from Girardi (2020). We obtain measures of corruption from the Varieties of Democracy (V-Dem) Database (from 1789 to the present).

We first show that, on average, countries under left-wing governments experience more systemic corruption in the four years after the election – a 13.2% increase in the index compared to the mean of non-left governments. The result is economically significant, akin

to moving from a country like the United Kingdom to a country like Greece, or from Argentina to Cote d'Ivoire, in terms of overall corruption. The effect of having a left-wing government materializes gradually over time, and can be seen both in the bureaucracy (petty corruption), and in the executive, legislative, and judicial branches of the state (grand corruption). All the effects are robust to specifying different bandwidths and bandwidth selection criteria, and different polynomial orders in the RD design. They survive the inclusion of country-specific trends in corruption, and the elimination of observations very close to the cutoff.

Higher corruption may be the outcome of left-wing governments implementing different policies. For instance, left-leaning parties in power tend to favor more intrusive rules and regulations – which give public officials the power to demand and collect bribes (Shleifer and Vishny, 1993; Svensson, 2003); larger public sectors – which may imply extended opportunities for bribery and other forms of corruption (De Haan and Sturm, 1994); and direct control of the means of production (Ades and Di Tella, 1997; McCarthy, 1997; Tanzi, 2000). We do not find evidence that left-wing parties in power run larger governments (measured with spending or revenues as a share of GDP), but they do seem to intervene more through state ownership of the means of production, especially in countries with weaker checks and balances. We also find a large but imprecisely estimated effect of left-wing governments on a measure of regulation, though the very small sample size limits the interpretation of this result.

Even with no partisan effect on policies, outcomes may be different because different people (more or less capable, more or less corrupt) execute those policies – and the higher corruption we observe under leftist governments may be due to such governments hiring people that, e.g., are more dishonest. Individual characteristics are not observed, but we find that the left in power runs bureaucracies of lower quality, with less meritocratic recruitment and less impartial administration, which is consistent with our results on public sector, or petty, corruption. We also produce evidence that the executive is less constrained by the legislature and the judiciary under left-wing governments – consistent with our results on grand corruption.

Empirical evidence on the effect of the ideology of the ruling political party on a country's corruption is surprisingly sparse and remains somewhat inconclusive. Svensson (2005) notes that many of the most corrupt countries (at the time of his study) were governed by left-wing parties – a correlation also reported in Di Tella and MacCulloch (2009) for a simple cross section of countries, using Beck et al.'s (2001) measure of government ideology. Hessami (2011) provides an empirical analysis for 106 countries over the 1984–2008 period that finds a positive association between corruption and *right*-wing governments – a link that the author attributes to right-wing politicians maintaining closer ties to representatives of the private sector, which breeds mutual trust and reciprocity, and makes corruption more likely to occur. Bellido et al. (2021) find a similar relationship between government ideology and the level of perceived corruption, using a panel data of OECD countries covering the years 1996–2015, but only in the period before the Great Recession.

To the best of our knowledge, ours is the first paper to provide causal evidence of the impact of electing a government of a certain ideology on corruption. Besides the large literature on the determinants of corruption, we contribute more generally to the literature on partisan effects. Most research on partisanship has tended to focus on macroeconomic outcomes, like growth, unemployment, inflation or investment (see, e.g., Lange and Garrett, 1985; Hibbs, 1987; Alesina and Rosenthal, 1995; Scruggs, 2001; Potrafke, 2017); or on fiscal and economic policies, such as government spending, allocation of funds, government size, taxation, land use regulation, local public goods provision, and contracting out of public services (see. e.g., Garand, 1988; Besley and Case, 1995, 2003; Dilger, 1998; Caplan, 2001; Reed, 2006; Solé-Ollé and Viladecans-Marsal, 2013; Beland and Oolomi, 2017; Alonso and Andrews, 2020).[1] Corruption has been rarely considered – perhaps due to the difficulties in constructing RD designs at the country level, and in obtaining long series of comparable corruption data.

2. Data

[1] There is also a literature that uses RD designs to look at partisan effects at the local level, following pioneering work by Pettersson-Lidbom (2008) and Ferreira and Gyourko (2009).

To investigate whether corruption is affected by the ideology of the government, we combine information from different databases. Election results, the classification of the political system, and the ideological stance of parties and candidates come from Girardi (2020), who provides an original dataset of worldwide national general (parliamentary and presidential) elections in the post-WWII period (1945–2018).[2] Following Girardi (2020), we exclude (i) presidential elections in which the president is elected by parliament or an electoral college, (ii) presidential elections in purely parliamentary systems, and (iii) parliamentary elections occurring in the same month of a presidential election under a presidential system.

Using this data, we construct our treatment indicator: whether the elected government is left-leaning. In our RD design, the running variable determining assignment to treatment is the (center-)left's margin of victory/loss. For presidential elections, this is defined as the vote share of the left-wing candidate minus that of the non-left candidate. In parliamentary elections, the running variable is computed as twice the difference between the share of parliamentary seats won by center-left parties and 50 percent (Girardi, 2020).

Our primary outcome of interest is corruption, measured using the political corruption index (v2x_corr) from the Varieties of Democracy (V-Dem) Database (Coppedge et al., 2024a).[3] This index is coded from the answers to the question "How pervasive is political corruption?", and ranges from 0 (low) to 1 (high); for details on variable construction, see Coppedge et al. (2024b). We compute the level of corruption after an election taking place in country i and year t as the average level of the political corruption index in the four years following the election $(t+1, \dots, t+4)$.[4]

The merged dataset comprises 1,025 elections (374 presidential and 651 parliamentary) from 116 countries. This sample includes all observations for which the running variable, the

[2] Girardi, Daniele. Replication Data for: Partisan shocks and financial markets: evidence from close national elections. Nashville, TN: American Economic Association [publisher], 2020. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2020-09-23. https://doi.org/10.3886/E115008V1.

[3] One of the largest data collection efforts in the social sciences, the V-Dem Database provides annual measurements for more than 600 indicators across all countries from 1789 to the present. Available at: https://www.v-dem.net. We use version 14 of the database.

[4] Four years is the modal distance between elections of the same type in a country. Our results are robust to using three and five years to compute the average, as shown in Table A.1 in the Appendix.

treatment indicator, and the political corruption index are available. Table 2.1 reports summary statistics for our estimation sample.

In supplemental analyses, we decompose the aggregate corruption index into public sector corruption (v2x_pubcorr) and regime corruption (v2xnp_regcorr), both also ranging from 0 (low) to 1 (high).[5] While the political corruption index captures systemic corruption at the country level, public sector corruption serves as proxy for petty (or bureaucratic or administrative) corruption. In contrast, regime corruption encompasses corruption within the higher echelons of the state—specifically within the executive, legislative, and judicial branches (grand corruption).

We also collect data on several predetermined country characteristics from various sources. In selecting our data sources, we seek to maximize the number of observations for each covariate while maintaining consistency across sources. Among the characteristics considered, many have been repeatedly associated with corruption in the literature (e.g., Ades and Di Tella, 1999; Treisman, 2000; Svensson, 2005), like income, urbanization, education, democracy, inequality, international trade, and freedom of expression (see Dimant and Tosato, 2018, and the references therein).

From the V-Dem Database, we obtain a measure of the quality of democracy, computed as the simple average of the deliberative, egalitarian, liberal, participatory, and electoral democracy indices (all ranging from 0 to 1).[6] We also use V-Dem's indicator for freedom of expression (v2x_freexp) as another measure of institutional quality. We measure education as the average years of education among citizens older than 15 (e_peaveduc), and we proxy inequality through the equal distribution of resources index (v2xeg_eqdr), which measures the levels of inequality in the distribution of resources – both tangible and intangible – in society. Finally, we take population figures also from V-Dem (e_pop).

[5] Both indices are also from the V-Dem Database. The public sector corruption index is coded from the answers to "To what extent do public sector employees grant favors in exchange for bribes, kickbacks, or other material inducements, and how often do they steal, embezzle, or misappropriate public funds or other state resources for personal or family use?", and the regime corruption index is built from the answers to "To what extent do political actors use political office for private or political gain?".

[6] See Marx et al. (2025). The indices used are v2x_polyarchy, v2x_libdem, v2x_partipdem, v2x_delibdem, and v2x_egaldem. We treat an observation as missing if at least one of the components has missing values. If we computed the average of the non-missing components, nothing would change.

From the Penn World Tables (Feenstra et al., 2015), we extract data on labor force participation, and trade intensity (computed as the sum of export and import penetration).[7] To measure income, we follow Marx et al. (2025) and use real GDP at constant national prices (RGDPNA), obtained from national accounts data for each country, and GPD per capita (RGDPNA divided by population) – both from the Penn World Tables. Finally, data on urbanization (share of population in urban agglomerations) come from the World Development Indicators.

[Table 2.1 about here]

3. Empirical strategy

3.1. Regression discontinuity design

Simply regressing our measure of corruption on an indicator of government ideology will typically fail to identify the causal effect of interest. Voters' preferences are not observed, and may influence both the electoral outcomes and the prevalence of corruption. The incidence of corruption may also affect voters' preferences, and thus electoral outcomes. To deal with such endogeneity concerns, we employ a regression-discontinuity design (RDD; Imbens and Lemieux, 2008; Lee and Lemieux, 2010; Cattaneo et al., 2020, 2024), by comparing countries where the left wins and loses elections by a small margin. Put differently, we take advantage of the discontinuous change in the ideology of the ruling party, given by its victory in a close election, to identify the local average treatment effect (LATE) of countries electing a left-wing government (vs. a non-left-wing one) on corruption.

Let $LeftWin_{it} = 1[MV_{it} \geq 0]$ determine the assignment of country i to treatment (left-wing government) and control (non-left-wing government) groups. $1[.]$ is the indicator function. MV_{it} is our running variable: the margin of victory/loss of the left in country i in election year t. As explained before, in presidential elections, the margin of victory/loss is defined as the vote share of the left-wing candidate minus the vote share of the non-left candidate. MV_{it} then takes positive values if the left wins the election, and negative if the left loses. In parliamentary elections, on the other hand, the running variable is computed as twice

[7] We use version 10.0 of the Penn World Tables.

the difference between the share of parliamentary seats won by center-left parties and 50 percent. In these elections, MV_{it} takes positive values if the left controls parliament after the election, and negative if the left does not.

For presidential elections we assume perfect compliance, so that an electoral win of the left always results in a left-leaning government ($LeftGvt_{it} = LeftWin_{it}$), and we have a sharp RD design. In parliamentary elections, however, an electoral win of the left (a majority of seats) does not always lead to a left-leaning government—put differently, there is imperfect compliance with the assigned treatment, giving rise to a fuzzy RD design (so that $LeftGvt_{it} \neq LeftWin_{it}$ for some observations). The probability of a left-wing government stills jumps discretely at the zero cutoff, $MV_{it} = 0$, but no longer from zero to one as in presidential elections.

To estimate the causal effect of interest we run the following RD specification:

$$LeftGvt_{it} = \beta * LeftWin_{it} + f(MV_{it}) + \mu_t + \lambda_i + \epsilon_{it}$$

$$Corruption_{it} = \delta * LeftWin_{it} + g(MV_{it}) + \mu_t + \lambda_i + \varepsilon_{it} \qquad (1)$$

where $LeftGvt_{it}$ is equal to one if a left government is formed after the election, $Corruption_{it}$ is a measure of the average level of corruption in the four years following election t in country i, and ϵ_{it} and ε_{it} are error terms. $f(MV_{it})$ and $g(MV_{it})$ are potentially non-linear functions of the margin of victory that we approximate through a non-parametric local linear regression, following current best practice (Cattaneo et al., 2020, 2024).[8] We restrict the sample to elections with MV_{it} in the interval $[-h, +h]$, and compute the optimal bandwidth h to minimize the mean squared error (MSE) of the point estimator, using the data-driven selection algorithm of Calonico et al. (2014).

μ_t are year fixed effects, and λ_i are country fixed effects, to account for large cross-country differences in corruption and common time shocks, and to increase the precision of our estimates (see, e.g., Hoxby, 2000; and Pettersson-Lidbom, 2008).[9] As suggested by Lee

[8] To implement the local polynomial approach, we use Stata command `rdrobust`, set the polynomial order to 1 in our preferred specification, and check the sensitivity of our results to this choice in section 5.2. In all regressions we use a triangular kernel.

[9] Given the slow-moving and latent nature of the V-Dem corruption index, we focus on corruption levels with country and year fixed effects, rather than short-run changes, to preserve a meaningful estimand and avoid amplifying measurement noise.

and Lemieux (2010), we begin by regressing $Corruption_{it}$ on country and year dummies, and then use the residuals from this regression as our dependent variable in the RD estimation. As in Gouvea and Girardi (2021), we use the full sample in the first-step regression (i.e., including observations for which the margin of victory is not available).[10] We cluster standard errors by country in all of our regressions.

β in the first equation in (1) captures first-stage effects: the effect of assigning the treatment (a left win) on the probability of receiving the treatment (having a left-wing government). In the second equation, δ captures intention-to-treat effects: the effects of treatment assignment on corruption. We are interested in δ/β, the fuzzy RD parameter capturing the LATE of countries electing a left-wing government on corruption. We estimate equation (1) using the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths.

We start by analyzing the first-stage relationship between a left electoral win and the formation of a left-wing government in parliamentary elections. Figure 3.1 shows an RD plot of this relationship, where the estimated probability of a left-wing government jumps from around 0.40 to around 0.83 at the cutoff. The RD estimate of this first-stage effect is 42.1 percentage points, a result consistent with the discrete jump observed in the figure, and highly statistically significant (p-value = 0.001)—suggesting a strong first stage.[11]

[Figure 3.1 about here]

3.2. Validation

As usual in empirical work, we argue in favor of the credibility of our RD design by exploiting the two testable implications of the identification assumption proposed by Lee (2008): (i) that individuals have imprecise control over the running variable; and (ii) that the treatment is locally randomized at the cutoff (see also Canay and Kamat, 2018).

For the RD design to be valid, implication (i) requires that political candidates and parties cannot manipulate the electoral outcomes. If this is true, the number of observations just above the cutoff must be similar to the number of observations just below the cutoff – i.e.,

[10] Using the same sample as in the RD estimation does not change our results, though we (naturally) face some very marginal loss of significance due to reduced efficiency.
[11] The corresponding figure for the whole sample of elections is 56.9 percentage points, with a p-value of 0.000.

the probability density function of the running variable should be continuous. We formally check for non-random sorting around the cutoff through a manipulation test of density discontinuity based on local polynomial density estimation methods (Cattaneo et al., 2018).[12] The value of the test statistic is -0.591 and the associated p-value is 0.555, meaning that we cannot reject the null hypothesis of no manipulation of the density at the cutoff. The result can also be seen graphically in Figure 3.2, which plots a kernel density function for the margin of victory, and also shows no evidence of non-random sorting of countries. The density estimates for treated and control countries align closely at the cutoff, and the 95% confidence intervals (shaded areas) display a substantial overlap. We also fail to find evidence of manipulation in the subsamples of presidential (p-value = 0.602) and parliamentary elections (p-value = 0.675).

[Figure 3.2 about here]

For our estimator to identify the average causal effect of having a left-wing government on corruption, implication (ii) requires that countries on both sides of the cutoff be comparable in terms of both observed and unobserved characteristics (Hahn et al., 2001). This implies that the distribution of all our observed covariates must be continuous at the cutoff. To further validate our RD design, we proceed to check whether our predetermined covariates display a discontinuity at the cutoff. We also consider lagged values of our outcome of interest (measured in pre-election years $t-1$, $t-2$, and $t-3$), the value of the running variable at the previous election, the value of the treatment variable at the previous election, and the time elapsed since the last treatment.

Panel A in Table 3.1 shows the results of local linear regressions like (1), but using each covariate as the outcome variable (with its optimally chosen bandwidth), and focusing on the intention-to-treat effects (that is, the effects of $LeftWin_{it}$ on the covariates).[13] Most of the RD point estimates in Table 3.1 tend to be small and all but one of the 95% robust confidence intervals contain zero. We report p-values in column [3]. The only statistically significant

[12] Given the goal of the density test, the relevant test compares the number of observations with $LeftWin_{it} = 1$ and $LeftWin_{it} = 0$ near the cutoff, not those with $LeftGvt_{it} = 1$ and $LeftGvt_{it} = 0$ (Cattaneo et al., 2024).
[13] We follow Cattaneo et al. (2020) and use the coverage-error (CER)-optimal bandwidth, rather than the MSE-optimal bandwidth, for falsification purposes because we are mostly interested in inference and the point estimates are of no particular interest in this case. Using MSE-optimal bandwidths would not change any of the empirical conclusions.

effects appear in income per capita (at the 5% level), and education and time elapsed since last treatment (at the 10% level).

[Table 3.1 about here]

The large number of covariates, however, raises the problem of multiple testing. To minimize the risk of spurious rejections of the null hypothesis of continuity, we also use the Simes (1986) implementation of the Benjamini and Hochberg (1995) False Discovery Rate (FDR) adjustment, as recommended by Anderson (2008). We report the corresponding q-values in column [4]. We find no evidence of discontinuity at the cutoff in our pre-election covariates when adjusting for multiple hypothesis testing in this manner.

Rather than contenting ourselves with checking continuity of the means of individual covariates at the cutoff, we verify also whether the *distribution* of our large set of pre-election covariates is continuous at the cutoff using the permutation test proposed by Canay and Kamat (2018), which is more powerful against some alternatives.[14] Panel B of Table 3.1 reports the p-values for the joint tests of the continuity of the 16-dimensional vector of covariates in the full sample of elections, and in the subsamples of presidential and parliamentary elections. As can be readily seen, we fail to reject the null hypothesis of continuity of the conditional distributions of the covariates at the cutoff in every case. Therefore, variables that may affect a country's corruption after the election seem then to be identical on both sides of the RD threshold.

4. Ideology and corruption: main results

Table 4.1 presents our main estimates of the impact of electing a left-wing government on corruption at the country level (equation (1)). As explained in previous sections, our main outcome is the average level of corruption in the four years following an election (*Corruption*). The findings indicate that, on average, countries under left-wing governments experience more systemic corruption. The estimated coefficient in column [1] of Table 4.1 shows a statistically significant (at the 10% level) effect of left-wing governments on the

[14] We implement the test using the Stata command `rdperm` with the default options. We also found similar results when computing the test by setting the number of observations closest to the cutoff (q) to 10, 25, 50, 0.8*$\hat{q}_{rot}$, and 1.2*$\hat{q}_{rot}$ – where $\hat{q}_{rot}$ is the feasible rule-of-thumb q recommended by Canay and Kamat (2018).

outcome of interest: corruption is higher compared to countries in which a non-left government is elected – a 13.2% increase in the corruption score evaluated at the mean of the control group. The result is not only statistically, but also economically significant. The effect of electing a left-wing government is 0.12 of a standard deviation of the corruption index—akin to moving from a country like the United Kingdom to a country like Greece, or from Argentina to Cote d'Ivoire, in terms of corruption.

[Table 4.1 about here]

In columns [2] and [3] we decompose the corruption index into public sector corruption and regime corruption. As can be readily seen, the increased levels of corruption under leftist governments show up everywhere: in the bureaucracy (petty corruption), but also in the executive, legislative, and judicial branches of the state (grand corruption).[15] Appendix Table A.2 shows that corruption in the executive, in particular, is affected in the same way by our treatment. In the same table, we also report that the effect is present (although imprecisely estimated due to reduced power) both after presidential and parliamentary elections, and hence it does not appear to be specific to any given political system.

5. Falsification tests and robustness analysis

5.1. Placebo thresholds

Our identification of the effect of electing a government of a given ideology on corruption is predicated on the continuity of the regression functions for treatment and control municipalities at the cutoff in the absence of the treatment. As the presence of discontinuities away from the cutoff would cast doubt on the RD design, we perform a set of placebo tests to rule out such possibility. For our main outcome, we look for RD treatment effects at placebo cutoff points, using control and treatment units separately (Cattaneo et al., 2020). Specifically, we re-estimate equation (1) using 580 fake cutoffs between 30% and 1% below and above the true threshold (using increments of 0.1%), as in Brollo and Troiano (2016). For the estimation of the effects on placebo outcomes, we focus again on the intention-to-

15

treat effects. No systematic evidence of treatment effects should appear at these artificial cutoffs.

Figure 5.1a plots the empirical cumulative density function of the t-statistics of the 580 fake treatment effects from these regressions. The vertical lines represent the critical values for the normal distribution (±1.96). Alternatively, Figure 5.2b plots the resulting histogram of t-statistics from the estimation, and then compares it with the t-statistic obtained at the true cutoff (the vertical line). Both figures show very little evidence of systematic discontinuities in corruption at the artificial cutoffs, away from the true cutoff of 0. Most of the coefficients from these placebo tests are not statistically significant, and the t-statistic at the true cutoff is in the right tail of the distribution of placebo t-statistics—providing strong support to the credibility of our main result.[16]

[Figure 5.1a about here] [Figure 5.1b about here]

5.2. Robustness checks

Next, we analyze how sensitive the results are to the choice of bandwidth used for local polynomial estimation. Following Cattaneo et al. (2020), we do this over small ranges around the optimal bandwidth, so that point estimation is still reliable – in particular we set $h = 2h^*$ (twice the optimal bandwidth h^*), and $h = h^*/2$. We also consider alternative criteria for choosing bandwidths: MSE-optimal bandwidth with two different bandwidth sizes below and above the threshold; and CER-optimal bandwidth with a common size and with different sizes on the two sides of the threshold. As reported in Appendix Table A.3, results are robust to all these alternative bandwidth choices. Table A.4 shows regression results using different polynomial orders (1 to 4). Results are also insensitive to the choice of order.

In the specification presented in Appendix Table A.5, we additionally allow for country-specific linear trends to account for slow-moving, country-level changes in corruption unrelated to electoral outcomes.[17] The results stay the same, once again. Finally, we check the sensitivity of our results to observations very close to the cutoff through a donut-hole analysis (e.g., Bajari et al., 2011). We exclude units within ±0.1 to ±0.5% of the cutoff and

[16] Figures A.1, A.2 and A.3 replicate the analysis and conclusions for the disaggregated measures of corruption.
[17] We first residualize the outcome with respect to country and year fixed effects, and country-specific time trends, and then apply `rdrobust` to the residualized outcome.

then repeat the estimation and inference analysis using the remaining sample. All of our results survive these checks (see Appendix Table A.6).

6. Discussion

6.1. Mechanisms

One major question that our research has not answered yet is *why* left-wing governments tend to be more corrupt. Understanding which mechanisms produce these persistent differences between government ideologies is certainly important, and in this section we explore these mechanisms in more detail, in order to develop a more comprehensive explanation of the effect of government ideology on corruption that we have uncovered.

Corruption is an outcome. As such, differences in corruption may result from different policies implemented by left-wing governments, as compared to non-left ones. For instance, there is a large literature emphasizing (and documenting) ideological differences on the role (and hence size) of the government in the economy (Cameron, 1978; Cusack, 1997; Holsey and Borcherding, 1997; Kau and Rubin, 2002; Tavits, 2004; Gabel and Hix, 2005; Pickering and Rockey, 2011). A leftist stance has been repeatedly associated with increased preferences for government intervention, in the form of higher taxes and higher levels of public spending, state provision of public goods and services, redistribution, and regulation of business (Feldman and Zaller, 1992; Bean and Papadakis, 1998; Alesina and Glaeser, 2004; Corneo and Gruner, 2002; Gabel and Hix, 2005; Shaw and Shapiro, 2005; Alesina and Fuchs-Schundeln, 2007; Shapiro, 2009; Bietenbeck and Thiemann, 2023).

Excessive government intervention, on the other hand, is commonly believed to facilitate corruption (Harriss-White and White, 1996; Becker and Becker, 1997; Shleifer and Vishny, 1998; Rose-Ackerman, 1999, 2000; Tanzi, 2000; Treisman, 2000). We thus begin our exploration of mechanisms with the two most commonly used measures of government intervention, which are actually measures of government size: spending and revenues as a share of GDP.[18] A government decisions on the level of taxes or expenditures may create

[18] Expenditure data come from Our World in Data (*Government spending as share of GDP*), and tax revenue information was obtained from the World Development Indicators (*Tax revenue (% of GDP)*, indicator

more opportunities for rent-seeking (Scully, 1991; Nitzan, 1994; Goel and Nelson, 1998), and hence increase corruption. Results are reported in Table 6.1. We estimate a small, largely non-significant, negative effect of electing a left-wing government on both measures of government size.

Larger governments need not always lead to corruption, however, since a large government would be able to establish a better system of checks and balances (La Porta et al., 1999; Goel and Nelson, 2010; Bonnano et al., 2025). Actually, some of the least corrupt countries in the world (such as Denmark, Sweden, and Norway) have some of the largest governments, measured as tax revenue or public spending as a share of GDP. A recent meta-analysis of 44 articles (for 450 observations/estimations) by Bonnano et al. (2025) finds a positive relationship between government size (proxied by government spending) and corruption, but the effect tends to disappear after controlling for publication selection bias.

[Table 6.1 about here]

It has long been recognized (Goel and Nelson, 2010) that it is not a large public sector, per se, that causes corruption, but rather the specific forms in which the government intervenes in the economy. Intervention can come under many guises (Hopkin and Rodriguez-Pose, 2007): besides intervening as a 'consumer', through its decisions on the level of taxes or expenditures, the government can intervene as a 'producer', for instance via direct production through public enterprises (Ades and Di Tella, 1997; McCarthy, 1997; Tanzi, 2000); and as a 'regulator', through the imposition of rules and restrictions that create opportunities for bureaucrats to request or accept bribes (Stigler; 1971; Peltzman, 1998; Tanzi, 1998; Glaeser and Shleifer, 2003). Given the multiple facets of government intervention in the economy, it is hardly surprising that the empirical evidence on intervention and corruption is mixed (La Porta et al., 1999; Adsera et al., 2003; Gerring and Thacker, 2005; Hopkin and Rodriguez-Pose, 2007; Goel and Nelson, 2010).

We proxy government intervention as a producer through V-Dem's indicator of state ownership in the economy (v2clstown), which measures the extent to which the state owns

GC.TAX.TOTL.GD.ZS). Consistent with our treatment of corruption, we compute the average of each variable in the four years after the election.

and controls capital (including land) in the industrial, agricultural, and service sectors.[19] Notice that *lower* values of this indicator are associated with *more* intervention. Table 6.1 shows a large, negative coefficient on left-wing government, although imprecisely estimated, suggesting that countries electing left-wing governments experience more intervention in the economy that may lead to increased corruption.[20] Appendix Table A.7 shows further that the effect is significant, both statistically (at the 5% level) and economically (42% evaluated at the mean of state ownership), in countries with weaker checks and balances.

Government regulation (of business activity, for instance) is often believed to provide fertile ground for corruption (De Soto, 1989; Shleifer and Vishny, 1993; Tanzi, 1998; Rose-Ackerman, 1999; Djankov et al., 2002; Svensson, 2005; Goel and Nelson, 2010). Through the imposition of rules and restrictions, the government creates opportunities for bureaucrats to request or accept bribes (Stigler, 1971: Peltzman, 1998; Tanzi, 1998; Glaeser and Shleifer, 2003). The evidence of a positive relationship between regulation and corruption is actually much more robust than the evidence on government size (Kaufmann and Wei, 2000; Paldam, 2001; Djankov et al., 2002; Golden, 2003; Svensson, 2003; World Bank, 2004; Hopkin and Rodriguez-Pose, 2007; Duvanova, 2014; Holcombe and Boudreaux, 2015; Fisman and Golden, 2017; Amin and Soh, 2021).

It is well known that the standard measures of government size often fail to capture the impacts of government regulations (Rosen, 1995). Measuring regulation is harder than government revenues and expenses, and it is only around the turn of the century that systematic data on government regulations began to be regularly collected (see, for instance, the World Bank Doing Business database, the Index of Economic Freedom indicators, the World Bank Enterprise Surveys, the OECD Product Market Regulation (PMR) indicators, or the World Economic Forum Global Competitiveness Reports), severely limiting our sample size.

We use here the Index of Economic Freedom of the Heritage Foundation, which tracks the evolution of economic freedom around the world in 184 countries from 1995 to 2025. We proxy the level of regulation by the business freedom component of the index, which

[19] We use the average of the indicator in the four years after the election in the regressions.
[20] This is similar to the findings in Ades and Di Tella (1997). Hopkin and Rodriguez-Pose (2007), on the other hand, find no relationship between government ownership and management of enterprises and corruption.

"measures the extent to which a country's regulatory and infrastructure environments constrain the efficient operation of businesses", in terms of factors that affect the ease of starting, operating, and closing a business.[21] The score for each country is a number between 0 and 100, with 100 indicating the freest business environment (i.e., that regulation is less obtrusive). We fail to find a statistically significant effect of left-wing governments on this measure of regulation, as can be seen in Table 6.1, although it is suggestive that the effect is of the expected sign, and reaches almost 0.3 of a standard deviation of the index. Notice, however, that the very small sample size precludes further analysis of this result.

Even if governments of different ideologies implement exactly the same policies, we may still see variation in corruption across governments if the people implementing the policies are different in dimensions relevant for corrupt activities. We do not have information on the personal characteristics of the bureaucrats in each country through time, but we can obtain some (admittedly indirect) evidence by looking at the criteria through which public employees are selected, and the rigor and impartiality in how the public administration is conducted.

To measure bureaucratic quality, we follow Marx et al. (2025) and use the bureaucracy score from Besley et al. (2022), which we compute as the simple average of two V-Dem indicators: criteria for appointment decisions in the state administration (v2stcritrecadm), and rigorous and impartial public administration (v2clrspct).[22] A lower value of the score indicates that the public administration is characterized more by arbitrariness and biases (i.e., nepotism, cronyism, or discrimination), and that appointment decisions (including hiring, firing and promotion) in the public sector are more reliant on personal and political connections, as opposed to skills and merit. A lower quality of the bureaucracy can contribute to corruption indirectly. Less meritocratic recruitment and promotion, for instance, has been associated with a higher extent of corruption (e.g., Tanzi, 1998). Political patronage may imply that political support, rather than competence, determines hiring and promotion – with negative effects on the quality of the public workforce (Colonnelli et al., 2020; see Brollo et al., 2017, for a less pessimistic view). Table 6.1 shows that countries electing left-wing

[21] https://economicfreedom.heritage.org/pages/about#indexMethodology.
[22] We compute the simple average over the 4 years after the election and then normalize the resulting indicator of bureaucracy quality, so that it takes values between 0 and 1 (with a higher score indicating a higher quality).

governments are indeed characterized by bureaucracies of lower quality, which is consistent with our results in Table 4.1 (especially on public sector, or petty, corruption).[23]

We do not have information of the personal characteristics of elected officials either, but we can gauge the extent to which the executive office is constrained by the two other powers (legislature and judiciary) after an election. We compute the average of two V-Dem indices (judicial and legislative constraints on the executive – v2x_jucon and v2xlg_legcon) over the four years after the election, and investigate the impact of closely electing a left-wing government on these institutional constraints. Table 6.1 reports a negative (though marginally non-significant) coefficient, indicating that the executive is less constrained under left-wing governments – consistent with our results on grand corruption reported in Table 4.1.

6.2. Heterogeneous effects

The effect on corruption that we have identified is an average treatment effect. In this section, we investigate whether it varies depending on the degree of institutional constraints on the government (both internal and external), income levels (using OECD membership in 1973 to identify high-income economies), time period, and party policy platform. We note that our sample size and the slow-moving nature of the corruption index may constrain the statistical power of our heterogeneity analyses; nonetheless, we provide some exploratory results, which should be interpreted with caution. Table 6.2 reports the estimated effect of left-wing governments for different subsamples, with each estimate corresponding to a separate regression.[24]

[Table 6.2 about here]

Institutional constraints. We measure the level of internal constraints (the strength of checks and balances in Marx et al., 2025) using the average of two V-Dem indices (judicial and legislative constraints on the executive – v2x_jucon and v2xlg_legcon). External constraints are proxied by our measure of trade intensity. Following Marx et al. (2025), we take the value of each variable in the year before the corresponding election, compute the

[23] Appendix Table A.8 shows that the effect of electing a left-wing government appears on both components of the index of bureaucratic quality.

[24] Appendix Table A.9 reports manipulation tests for each subsample. In no case are we able to reject the null hypothesis of no manipulation of the density at the cutoff.

median among close elections (elections for which the absolute value of the running variable is under 15 percentage points), and split the sample between elections above and below the median.

Table 6.2 shows that the positive effect of left-wing governments on corruption only appears in countries with weaker internal constraints on the executive. External constraints, on the other hand, do not seem to have a similar moderating effect: point estimates are economically meaningful in both subsamples, though imprecisely estimated when constraints are low.

Income levels. As in Girardi (2018), we use OECD membership as an (invariant) proxy for high-income country. We choose 1973 to assess membership because after that year there were no new members for over 20 years (Mexico only joined in 1994). The results presented in Table 6.2 show that the increase in corruption under left-wing governments only occurs in non-high-income (or less developed) economies.[25]

Time periods. To perform this heterogeneity analysis, we split the sample in two using the median year (1995) in our sample. The effect of electing a left-wing government on corruption is always positive in Table 6.2, but appears much larger post-1995 – and is only statistically significant in that period.

Policy platforms. We obtain a measure of the left's economic interventionism from Girardi (2020), computed as the difference between the planned economy index (planeco) and the market economy index (markeco) of the country's main left party from the policy position estimates in the Manifesto Project Database (Volkens et al., 2018).[26] These data are only available for parliamentary elections, resulting in a reduced sample size, even before subsampling. To increase statistical power, we focus on the reduced-form relation (as in Girardi, 2020). Finally, we split the sample of elections according to whether economic interventionism is below or above its median value in the sample.

[25] The estimated coefficient is similar to that in Table 4.1, and there is a very marginal loss of statistical significance due to the reduced sample size (p-value = .110).

[26] The planned economy index (planeco) measures support for government intervention in the economy (regulation, planning, and control), whereas and the market economy index (markeco) measures support for free markets and a reduced role of the state.

As can be seen in Table 6.2, there is a positive (though imprecisely estimated) effect on corruption when the left is more interventionist, and (essentially) no effect when the left is more market-oriented. Given the small sample size, the evidence is hardly conclusive, but it is consistent with government intervention in the economy being a driver of the higher corruption under left-wing governments (as argued in Section 6.1).

6.3. Dynamic effects

To evaluate how the effect of electing a left-wing government evolves over time, we re-run equation (1) using corruption in year $t - \tau$ as the dependent variable, where t is the election year and $\tau \in \{-3, -2, 0, 1, 2, 3, 4\}$. Figure 6.1 reports the RD point estimates, with 90% robust confidence intervals. Estimates for $t - 2$ and $t - 3$ can be interpreted as placebo tests (see Section 3.2) – they are smaller in magnitude and non-significant, as expected. The estimated effect of left-wing governments increases in economic and statistical significance over time, and is clearly noticeable by the end of a typical term – suggesting that it takes some time for government ideology to affect corruption.

[Figure 6.1 about here]

6.4. External validity

Given the evidence in Section 5, we are confident that the RD treatment effect estimated in Section 4 is a causal effect of electing a left-wing government on corruption. However, as in any RD design, we are concerned with the external validity of this effect – put differently, since we focus on close, rather than all, elections, we would like to know whether the RD effect we identify is informative of the effect of electing a left-wing government on corruption in countries with scores away from the cutoff.

Corruption is higher in countries where the left barely wins than where it barely loses. These are countries in which the left is highly competitive, and voters' political preferences and attitudes towards corruption in these countries may be systematically different from those in countries where the left almost surely wins (or loses). Voters' preferences and attitudes are not observed, but we can at least compare the countries experiencing close elections to the rest of countries in our estimation sample, to check whether or not they are systematically different along the different observable characteristics that we have considered in Table 2.1.

Table 6.3 compares the means of the characteristics of the countries with and without close elections.[27] Following the related literature, we define as close an election with a vote margin below 10 percent.

[Table 6.3 about here]

Even though differences tend to be small, close elections in our estimation sample occur in countries with average characteristics quite different to the rest of the countries along several dimensions: countries experiencing close elections have (non-surprisingly) better democratic institutions, a slightly larger and more urban population, and are somewhat poorer. On the other hand, we find no differences in GDP, labor force participation, education, inequality or openness to trade. Countries with and without close elections tend to be comparable in terms of economic and social conditions, but they differ markedly in the political indicators considered.

The evidence in Table 6.3 thus suggests that caution should be exercised when generalizing our results to other countries. On the other hand, the closeness or competitiveness of an election does not seem to be a time-invariant characteristic of countries. For instance, conditional on having a close election, the probability of having the next election defined by a narrow margin in a given country is 39.8%. Therefore, the effect on corruption is unlikely to be due to some permanent feature of those countries that experience close elections.

7. Concluding remarks

As in Brown et al. (2011), we contend that who sits at the controls matters for corruption, and provide causal evidence from an RD design that left-controlled governments are more likely to engage in corrupt behavior than center or right-wing governments. We contend that countries under left-wing governments may experience more corruption due to the left's ideological preference for extended government intervention in the economy – and find suggestive evidence that *some* (not all) forms of intervention are indeed stronger when the

[27] The differences (and their corresponding standard errors) are computed through OLS regressions of each covariate on a dummy for close election, and country and year fixed effects.

left is in power. Besides, we show that left-wing rule is characterized by a lower quality bureaucracy and reduced checks on the executive, which may contribute to increased corruption even if there were no differences in policies across ideologies.

We caution that our measures of corruption are subjective, relying on indices that reflect perceptions and expert opinions, rather than direct, quantitative counts of actual acts of corruption. The potential biases of such measures—such as perception lags or ideological bias—are well known (Tanzi, 1998; Svensson, 2003; Lambsdorff, 2004; Treisman, 2007; Fan, Lin, and Treisman, 2009; Gutmann et al., 2020). While experience-based measures from survey data are often proposed as a superior alternative, they are generally unavailable prior to the 1990s. Furthermore, survey data are subject to their own limitations, as they rely on respondent recall and subjective assessments of official intent; consequently, they may not be as objective as commonly assumed (Bonnano et al., 2025). We believe V-Dem data are a good compromise, as they provide unrivaled longitudinal coverage, and employ sophisticated aggregation methods to mitigate individual coder bias. We nonetheless acknowledge their inherent limitations.

Whether our results generalize to subnational levels of government remains an open question for future research, and in a complementary project, we are currently extending the analysis in this paper to the local level, using objective, audit-based data from Brazilian municipalities.

Appendix

[Table A.1 about here]

[Table A.2 about here]

[Table A.3 about here]

[Table A.4 about here]

[Table A.5 about here]

[Table A.6 about here]

[Table A.7 about here]

[Table A.8 about here]

[Table A.9 about here]

References

Ades, A., & Di Tella, R. (1997b). National champions and corruption: Some unpleasant interventionist arithmetic. *Economic Journal* 107(443), 1023–1042. https://doi.org/10.1111/j.1468-0297.1997.tb00005.x

Ades, A., & Di Tella, R. (1999). Rents, competition and corruption. *American Economic Review* 89(4), 982–993. https://doi.org/10.1257/aer.89.4.982

Adsera, A., Boix, C., & Payne, M. (2003). Are you being served? Political accountability and quality of government. *The Journal of Law, Economics, and Organization* 19(2), 445–490. https://doi.org/10.1093/jleo/ewg017

Alesina, A., & Fuchs-Schundeln, N. (2007). Good-bye Lenin (or not?): The effect of communism on people's preferences. *American Economic Review* 97(4), 1507–1528. https://doi.org/10.1257/aer.97.4.1507

Alesina, A., & Glaeser, E.L. (2004). *Fighting poverty in the US and Europe: A world of difference*. Oxford University Press: New York.

Alesina, A., & Rosenthal, H. (1995). *Partisan politics, divided government, and the economy*. Cambridge University Press: Cambridge, UK.

Alonso, J.M., & Andrews, R. (2020). Political ideology and social services contracting: evidence from a regression discontinuity design. *Public Administration Review* 80(5), 743–754. https://doi.org/10.1111/puar.13177

Amin, M., & Soh, Y.C. (2021). Does greater regulatory burden lead to more corruption? Evidence using firm-level survey data for developing countries. *The World Bank Economic Review* 35(3), 1–17. https://doi.org/10.1093/wber/lhaa007

Anderson, M.L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training projects. *Journal of the American Statistical Association* 103(484), 1481–1495. https://doi.org/10.1198/016214508000000841

Bajari, P., Hong, H., Park, M., & Town, R. (2011). Regression Discontinuity Designs with an endogenous forcing variable and an application to contracting in health care. *NBER Working Paper* No. 17643, National Bureau of Economic Research, December. https://www.nber.org/papers/w17643

Bean, C., & Papadakis, E. (1998). A comparison of mass attitudes towards the welfare state in different institutional regimes, 1985–1990. *International Journal of Public Opinion* 10(3): 211–236. https://doi.org/10.1093/ijpor/10.3.211

Beck, T., Clarke, G., Keefer, P., & Walsh, P. (2001). New tools in comparative political economy: the database of political institutions. *The World Bank Economic Review* 11(1), 165–176. https://doi.org/10.1093/wber/15.1.165

Becker, G., & Becker, G.N. (1997). *The Economics of Life*. New York: McGraw-Hill.

Beland, L.P., & Oolomi, S. (2017). Party affiliation and public spending: evidence from U.S. governors. *Economic Inquiry* 55(2), 982–995. https://doi.org/10.1111/ecin.12393

Bellido, H., Olmos, L., & Román-Aso, J.A. (2021). The influence of government ideology on corruption: the impact of the Great Recession. *Economía Política* 38, 677–708. https://doi.org/10.1007/s40888-020-00212-6

Benjamini, Y., & Hochberg, Y. (1995). Controlling the false discovery rate: A practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society: Series B (Methodological)* 57(1), 289–300. https://doi.org/10.1111/j.2517-6161.1995.tb02031.x

Besley, T., & Case, A. (1995). Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits. *The Quarterly Journal of Economics* 110 (3), 769–798. https://doi.org/10.2307/2946699

Besley, T., & Case, A. (2003). Political institutions and policy choices: evidence from the United States. *Journal of Economic Literature* 41 (1), 7–73. https://doi.org/10.1257/002205103321544693

Besley, T., Burgess, R., Khan, A., & Xu, G. (2022). Bureaucracy and development. *Annual Review of Economics* 14(1), 397–424. https://doi.org/10.1146/annurev-economics-080521-011950

Bietenbeck, J., & Thiemann, P. (2023). Revisiting the effect of growing up in a recession on attitudes towards redistribution. *Journal of Applied Econometrics* 38(5), 786–794. https://doi.org/10.1002/jae.2970

Bonnano, G., Errico, L., Fiorino, N., & Ricciuti, R. (2025). The impact of government size on corruption: a meta-regression analysis. Journal of Economic Surveys 39(4), 1727–1757. https://doi.org/10.1111/joes.12672

Brollo, F., & Troiano, U. (2016). What happens when a woman wins an election? Evidence from close races in Brazil. *Journal of Development Economics* 122, 28–45. https://doi.org/10.1016/j.jdeveco.2016.04.003

Brollo, F., Forquesato, P., & Gozzi, J.C. (2017). To the victor belongs the spoils? Party membership and public sector employment in Brazil. *CAGE Online Working Paper Series* 353, Competitive Advantage in the Global Economy (CAGE). https://ideas.repec.org/p/cge/wacage/353.html

Brown, D.S., Touchton, M., & Whitford, A. (2011). Political polarization as a constraint on corruption: a cross-national comparison. *World Development* 39(9), 1516–1529. https://doi.org/10.1016/j.worlddev.2011.02.006

Calonico, S., Cattaneo, M.D., & Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica* 82(6), 2295–2326. https://doi.org/10.3982/ECTA11757

Cameron, D.R. (1978). The expansion of the public economy: a comparative analysis. *American Political Science Review* 72(4), 1243–1261. https://doi.org/10.2307/1954537

Canay, I., & Kamat, V. (2018). Approximate permutation tests and induced order statistics in the Regression Discontinuity Design. *Review of Economic Studies* 85, 1577–1608.
https://doi.org/10.1093/restud/rdx062

Caplan, B. (2001). Has Leviathan been bound? A theory of imperfectly constrained government with evidence from the states. *Southern Economic Journal* 67(4), 825–847.
https://doi.org/10.1002/j.2325-8012.2001.tb00376.x

Cattaneo, M.D., Jansson, M., & Ma, X. (2018). Manipulation testing based on density discontinuity. *The Stata Journal* 18(1), 234–261.
https://doi.org/10.1177/1536867X1801800115

Cattaneo, M.D., Idrobo, N., & Titiunik, R. (2020). A practical introduction to regression discontinuity designs: foundations. *Elements in Quantitative and Computational Methods for the Social Sciences*, Cambridge University Press.
https://doi.org/10.1017/9781108684606

Cattaneo, M.D., Idrobo, N., & Titiunik, R. (2024). A practical introduction to regression discontinuity designs: extentions. *Elements in Quantitative and Computational Methods for the Social Sciences*, Cambridge University Press.
https://doi.org/10.1017/9781009441896

Colonnelli, E., Prem, M., & Teso, E. (2020). Patronage and selection in public sector organizations. *American Economic Review* 110(10), 3071–99.
https://doi.org/10.1257/aer.20181491

Coppedge, M., Gerring, J., Knutsen, C.H., Lindberg, S.I., Teorell, J., Altman, D., Angiolillo, F., Bernhard, M., Borella, C., Cornell, A., Fish, M.S., Fox, L., Gastaldi, L., Gjerløw, H., Glynn, A., Good God, A., Grahn, S., Hicken, A., Kinzelbach, K., Krusell, J., Marquardt, K.L., McMann, K., Mechkova, V., Medzihorsky, J., Natsika, N., Neundorf, A., Paxton, P., Pemstein, D., Pernes, J., Rydén, O., von Römer, J., Seim, B., Sigman, R., Skaaning, S-E., Staton, J., Sundström, A., Tzelgov, E., Wang, Y-t., Wig, T., Wilson, S., & Ziblatt, D. (2024a). V-Dem Dataset v14. *Varieties of Democracy (V-Dem) Project*.
https://doi.org/10.23696/mcwt-fr58

Coppedge, M., Gerring, J., Knutsen, C.H., Lindberg, S.I., Teorell, J., Altman, D., Angiolillo, F., Bernhard, M., Borella, C., Cornell, A., Fish, M.S., Fox, L., Gastaldi, L.,

Gjerløw, H., Glynn, A., Good God, A., Grahn, S., Hicken, A., Kinzelbach, K.,, Marquardt, K.L., McMann, K., Mechkova, V., Neundorf, A., Paxton, P., Pemstein, D., Rydén, O., von Römer, J., Seim, B., Sigman, R., Skaaning, S-E., Staton, J., Sundström, A., Tzelgov, E., Uberti, L., Wang, Y-t., Wig, T., and Ziblatt, D. (2024b). V-Dem Codebook v14. *Varieties of Democracy (V-Dem) Project*.

Corneo, G., & Gruner, H.P. (2002). Individual preferences for political redistribution. *Journal of Public Economics* 83(1), 83–107. https://doi.org/10.1016/S0047-2727(00)00172-9

Cusack, T.R. (1997). Partisan politics and public finance: changes in public spending in the industrialized democracies, 1955–1989, *Public Choice* 91, 375–395. https://doi.org/10.1023/A:1004995814758

De Haan, J., & Sturm, J.E. (1994). Political and institutional determinants of fiscal policy in the European Community. *Public Choice* 80, 157–172. https://doi.org/10.1007/BF01047953

De Soto, H. (1989). *The Other Path*. New York: Harper and Row.

Dilger, R. (1998). Does politics matter? Partisanship's impact on state spending and taxes, 1985–95. *State and Local Government Review* 30(2), 139–144. https://doi.org/10.1177/0160323X9803000206

Dimant, E., & Tosato, G. (2018). Causes and effects of corruption: what has past decade's empirical research taught us? A survey. *Journal of Economic Surveys* 32(2), 335–356. https://doi.org/10.1111/joes.12198

Di Tella, R., & MacCulloch, R. (2009). Why doesn't capitalism flow to poor countries. *Brookings Papers on Economic Activity*, 2009, No. 1. https://www.brookings.edu/wp-content/uploads/2009/03/2009a_bpea_ditella.pdf

Djankov, S., La Porta, R., Lopez-De-Silanes, F., & Shleifer, A. (2002). The regulation of entry. *Quarterly Journal of Economics* 117(1): 1–37. https://doi.org/10.1162/003355302753399436

Duvanova, D. (2014). Economic regulations, red tape, and bureaucratic corruption in post-communist economies. *World Development* 59: 298–312. https://doi.org/10.1016/j.worlddev.2014.01.028

Fan, C. S., Lin, C., & Treisman, D. (2009). Political decentralization and corruption: Evidence from around the world. *Journal of Public Economics* 93(1–2), 14–34. https://doi.org/10.1016/j.jpubeco.2008.09.001

Feenstra, R.C., Inklaar, R., & Timmer, M.P. (2015). The next generation of the Penn World Table. *American Economic Review* 105(10), 3150–3182. https://doi.org/10.1257/aer.20130954

Feldman, S., & Zaller, J. (1992). The political culture of ambivalence: Ideological responses to the welfare state. *American Journal of Political Science* 36(1): 268–307. https://doi.org/10.2307/2111433

Ferreira, F., & Gyourko, J. (2009). Do political parties matter? evidence from U.S. cities. *The Quarterly Journal of Economics* 124(1), 399–422. https://doi.org/10.1162/qjec.2009.124.1.399

Fisman, R., & Golden, M. (2017). *Corruption: What everyone needs to know*. Oxford: Oxford University Press.

Gabel, M., & Hix, S. (2005). Reconsidering the economic effects of constitutions. Unpublished research paper.

Garand, J.C. (1988). Explaining government growth in the U.S. states. *American Political Science Review* 82(3), 837–849. https://doi.org/10.2307/1962494

Gerring, J., & Thacker, S.C. (2005). Do neo-liberal policies deter political corruption? *International Organization* 59(1), 233–254. https://doi.org/10.1017/S0020818305050083

Girardi, D. (2018). Political shocks and financial markets: regression-discontinuity evidence from national elections. Working Paper 2018-08, University of Massachusetts – Amherst. https://doi.org/10.7275/12066016

Girardi, D. (2020). Partisan shocks and financial markets: Evidence from close national elections. *American Economic Journal: Applied Economics* 12(4), 224–252. https://doi.org/10.1257/app.20190292

Glaeser, E. L., & Shleifer, A. (2003). The rise of the regulatory state. J*ournal of Economic Literature* 41(2), 401–425.

Goel, R. K., & Nelson,M. A. (1998). Corruption and government size: A disaggregated analysis. *Public Choice* 97, 107–120.
https://doi.org/10.1023/A:1004900603583

Goel, R. K., & Nelson, M. A. (2010). Causes of corruption: History, geography and government. *Journal of Policy Modeling* 32(4), 433–447.
https://doi.org/10.1016/j.jpolmod.2010.05.004

Golden, M. (2003). Electoral connections: The effects of the personal vote on political patronage, bureaucracy and legislation in postwar Italy. *British Journal of Political Science* 33(2), 189–212.

Gouvea, R., & Girardi, D. (2021). Partisanship and local fiscal policy: evidence from Brazilian cities. *Journal of Development Economics* 150, 102637.
https://doi.org/10.1016/j.jdeveco.2021.102637

Gutmann, J., Padovano, F., & Voigt, S. (2020). Perception vs. experience: Explaining differences in corruption measures using microdata. *European Journal of Political Economy* 65, 101925.
https://doi.org/10.1016/j.ejpoleco.2020.101925

Hahn, J., Todd, P., & Van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression discontinuity design. *Econometrica* 69 (1), 201–209.
https://doi.org/10.1111/1468-0262.00183

Harriss-White, B., & White, G. (1996). Corruption, liberalization and democracy: Editorial introduction. *IDS Bulletin* 27(2), 1–5.
https://doi.org/10.1111/j.1759-5436.1996.mp27002001.x

Hessami, Z. (2011). On the link between government ideology and corruption in the public sector. Paper prepared for the 2011 ISNIE Meeting at Stanford University, June.
https://extranet.sioe.org/uploads/isnie2011/hessami.pdf

Hibbs, D. (1987). *The American political economy: macroeconomics and electoral politics*. Harvard University Press: Cambridge, MA.

Holcombe, R., & Boudreaux, C. (2015). Regulation and corruption. *Public Choice* 164, 75–85.
https://doi.org/10.1007/s11127-015-0263-x

Holsey, C.M., & Borcherding, T.E. (1997). Why does government's share grow? An assessment of the recent literature on the U.S. experience. In Dennis C. Mueller (Ed.), *Perspectives on Public Choice: A Handbook* (Cambridge: Cambridge University Press, 1997).

Hopkin, J., & Rodriguez-Pose, A. (2007). 'Grabbing hand' or 'helping hand'? Corruption and the economic role of the state. *Governance* 20(2), 187–208. https://doi.org/10.1111/j.1468-0491.2007.00353.x

Hoxby, C. (2000). The effects of class size on student achievements: New evidence from population variation. *The Quarterly Journal of Economics* 115(4), 1239–1285. https://doi.org/10.1162/003355300555060

Imbens, G.W., & Lemieux, T. (2008). Regression discontinuity designs: a guide to practice. *Journal of Econometrics* 142(2), 615–635. https://doi.org/10.1016/j.jeconom.2007.05.001

Jain, A.K. (2001). Corruption: a review. *Journal of Economic Surveys* 15(1), 71–121. https://doi.org/10.1111/1467-6419.00133

Kau, J.B., & Rubin, P.H. (2002). The growth of government: sources and limits. *Public Choice* 113, 389–402. https://doi.org/10.1023/A:1020884121375

Kaufmann, D., & Wei, S.-J. (1999). Does "grease money" speed up the wheels of commerce? *NBER Working Papers* No. 7093, National Bureau of Economic Research, April. https://www.nber.org/papers/w7093

Lambsdorff, J. (2004). Background paper to the 2004 Corruption Perceptions Index. Univ. Passau. http://www.transparency.org/policy research/surveys indices/cpi/2004

Lange, P., & Garrett, G. (1985). The politics of growth: strategic interaction and economic performance in the advanced industrial democracies, 1974–1980. *The Journal of Politics* 47(3), 792–827. https://doi.org/10.2307/2131212

La Porta, R., Lopez-de-Silanes, F., Shleifer, A., & Vishny, R. (1999). The quality of government. *Journal of Law, Economics and Organization* 15(1), 222–279. https://doi.org/10.1093/jleo/15.1.222

Lee, D.S., & Lemieux, T. (2010). Regression discontinuity designs in Economics. *Journal of Economic Literature* 48(2), 281–355.
https://doi.org/10.1257/jel.48.2.281

McCarthy, P. (1997). *The crisis of the Italian state*. Basingstoke: Macmillan.

Marx, B., Pons, V., & Rollet, V. (2025). Electoral turnovers. *The Review of Economic Studies* 92(5), 3306–3339.
https://doi.org/10.1093/restud/rdae108

Mauro, P. (1995). Corruption and growth. *The Quarterly Journal of Economics* 110, 681–712.
https://doi.org/10.2307/2946696

Nitzan, S. (1994). Transfers or public good provision? A political allocation perspective. *Economics Letters* 45(4), 451–457.
https://doi.org/10.1016/0165-1765(94)90086-8

Olken, B.A., & Pande, R. (2012). Corruption in developing countries. *Annual Review of Economics* 4, 479–509.
https://doi.org/10.1146/annurev-economics-080511-110917

Paldam, M. (2001). Corruption and religion. Adding to the economic model. *Kyklos* 54, 383–414.

Peltzman, S. (1998). *Political participation and government regulation*. Chicago: University of Chicago Press.

Pettersson-Lidbom, P. (2008). Do parties matter for economic outcomes? a regression discontinuity approach. *Journal of the European Economic Association* 6(5), 1037–1056.
https://doi.org/10.1162/JEEA.2008.6.5.1037

Pickering, A., & Rockey, J. (2011). Ideology and the growth of government. *The Review of Economics and Statistics* 93(3), 907–919.
https://doi.org/10.1162/REST_a_00101

Potrafke, N. (2017). Partisan politics: the empirical evidence from OECD panel studies. *Journal of Comparative Economics* 45(4), 712–750.
https://doi.org/10.1016/j.jce.2016.12.004

Reed, W.R. (2006). Democrats, republicans, and taxes: evidence that political parties matter. *Journal of Public Economics* 90(4-5), 725–750.

https://doi.org/10.1016/j.jpubeco.2004.12.008

Rose-Ackerman, S. (1999). *Corruption and government*. Cambridge: Cambridge University Press.

Rose-Ackerman, S. (2000). Is leaner government necessarily cleaner government? In J. Tulchin & R. Esbach (eds.), *Combating corruption in Latin America*. Washington DC: Woodrow Wilson Center Press.

Rosen, H.S. (1995). *Public finance*. Chicago: Irwin.

Scruggs, L. (2001). The politics of growth revisited. *The Journal of Politics* 63(1), 120–140.

https://doi.org/10.1111/0022-3816.00061

Scully, G. (1991). Rent-seeking in US government budgets, 1900-88. *Public Choice* 70, 99–106.

https://doi.org/10.1007/BF00239349

Shapiro, R.Y. (2009). From depression to depression? Seventy-five years of public opinion toward welfare. Paper presented at the panel on "The Politics of TANF Reauthorization" at the 31st Annual Fall Research Conference of the Association of Public Policy Analysis and Management, November 5-7, 2009, Washington, D.C.

Shaw, G.M., & Shapiro, R.Y. (2005). Welfare. In Samuel J. Best and Benjamin Radcliff (eds.), *Polling America: An encyclopedia of public opinion*. Greenwood Press: Westport, Conn.

Shleifer, A., & Vishny, R. (1993). Corruption. *The Quarterly Journal of Economics* 108(3), 599–617.

https://doi.org/10.2307/2118402

Shleifer, A., & Vishny, R. (1998). *The grabbing hand. Government pathologies and their cures*. Cambridge, MA: Harvard University Press.

Simes, R.J. (1986). An improved Bonferroni procedure for multiple tests of significance. *Biometrika* 73(3), 751–754.

https://doi.org/10.1093/biomet/73.3.751

Solé-Ollé, A.; & Viladecans-Marsal, E. (2013). Do political parties matter for local land use policies? *Journal of Urban Economics* 78, 42–56.

https://doi.org/10.1016/j.jue.2013.07.003

Stigler, G. (1971). The theory of economic regulation. *Bell Journal of Economics and Management Science* 2(1), 3–21.
https://doi.org/10.2307/3003160

Svensson, J. (2003). Who must pay bribes and how much? *The Quarterly Journal of Economics* 118(1), 207–230.
https://doi.org/10.1162/00335530360535180

Svensson, J. (2005). Eight questions about corruption. *The Journal of Economic Perspectives* 19(3), 19–42.
https://doi.org/10.1257/089533005774357860

Tanzi, V. (1998). Corruption around the world: Causes, consequences, scope, and cures. *International Monetary Fund Staff Papers* 45, 559–594.
https://doi.org/10.2307/3867585

Tanzi, V. (2000). *Policies, institutions and the dark side of economics*. Cheltenham: Edward Elgar.

Tavits, M. (2004). The size of government in majoritarian and consensus democracies. *Comparative Political Studies* 37(3), 340–359.
https://doi.org/10.1177/0010414003262068

Treisman, D. (2000). The causes of corruption: A cross-national study. *Journal of Public Economics* 76(3), 399–457.
https://doi.org/10.1016/S0047-2727(99)00092-4

Treisman, D. (2007). What have we learned about the causes of corruption from ten years of cross-national empirical research? *Annual Review of Political Science* 10, 211–244.
https://doi.org/10.1146/annurev.polisci.10.081205.095418

Volkens, A., Lehmann, P., Matthieß, T., Merz, M., Regel, S., & Weßels, B. (2018). *The Manifesto Data Collection. Manifesto Project (MRG/CMP/MARPOR). Version 2018a*. Berlin: Wissenschaftszentrum Berlin für Sozialforschung (WZB).
https://doi.org/10.25522/manifesto.mpds.2018a

World Bank (2004). *Doing Business in 2004*. Oxford: Oxford University Press.

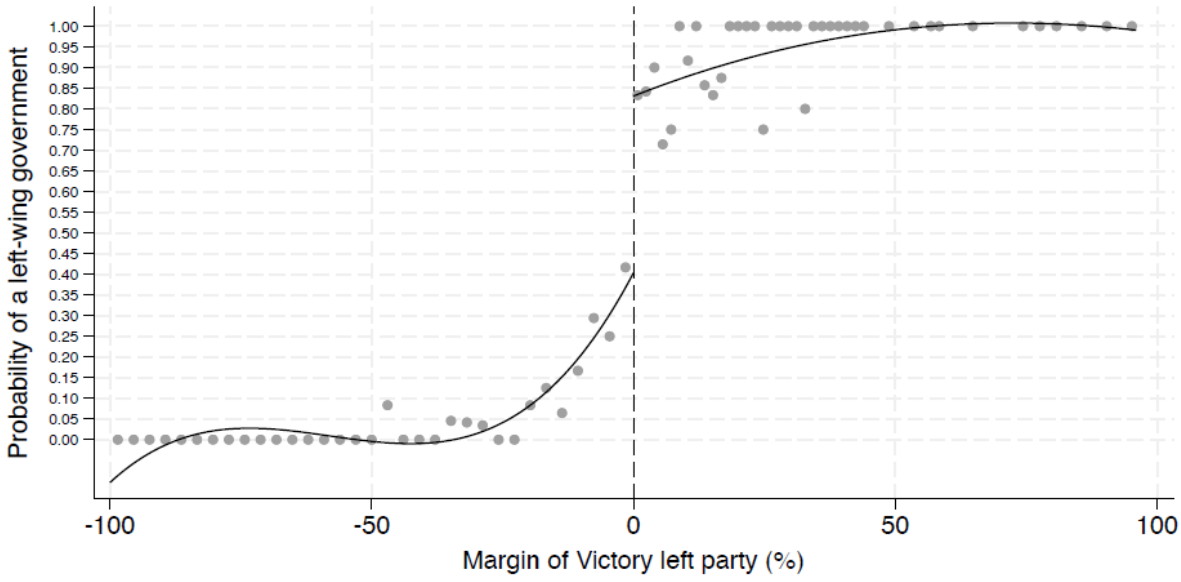


Figure 3.1. First-stage relationship between a left electoral win and the formation of a left-wing government in parliamentary elections

Notes: The figure plots the effect of left-wing parties winning a parliamentary majority on the ideology of the government formed after the election. The horizontal axis displays the margin of victory of the left as defined in the main text. Scatterplot points are binned outcome means, using the mimicking variance evenly-spaced method. The solid lines represent a third-order global polynomial fit estimated separately for treated and control observations using the original raw data. Purely presidential systems are excluded from the graph.

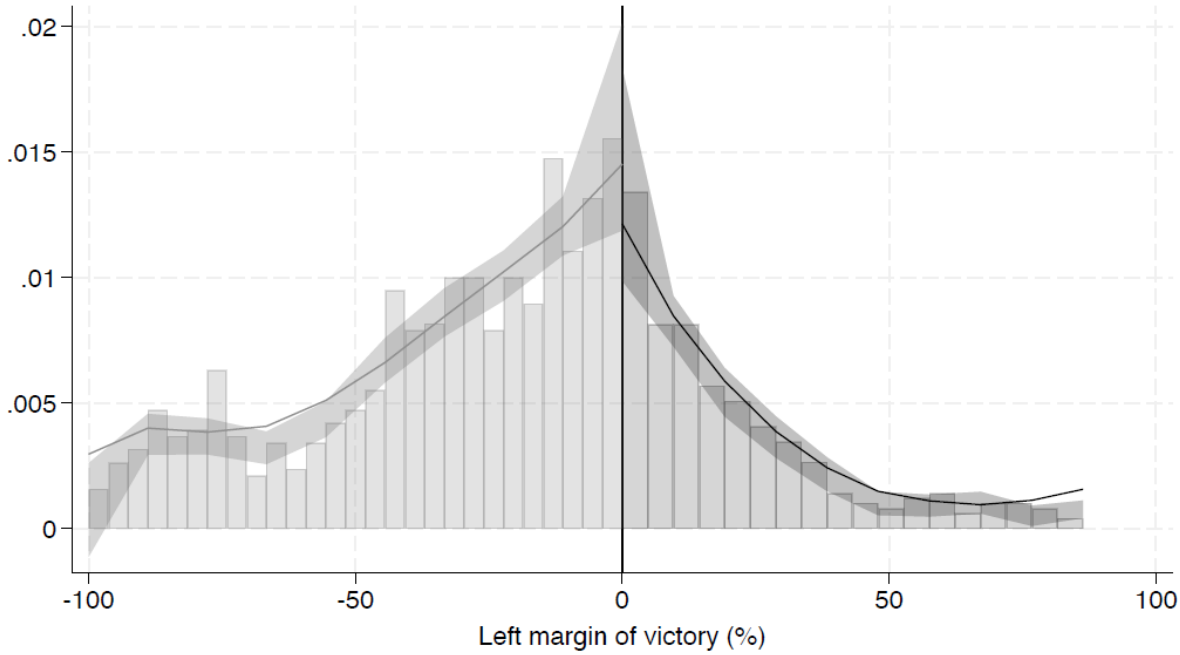
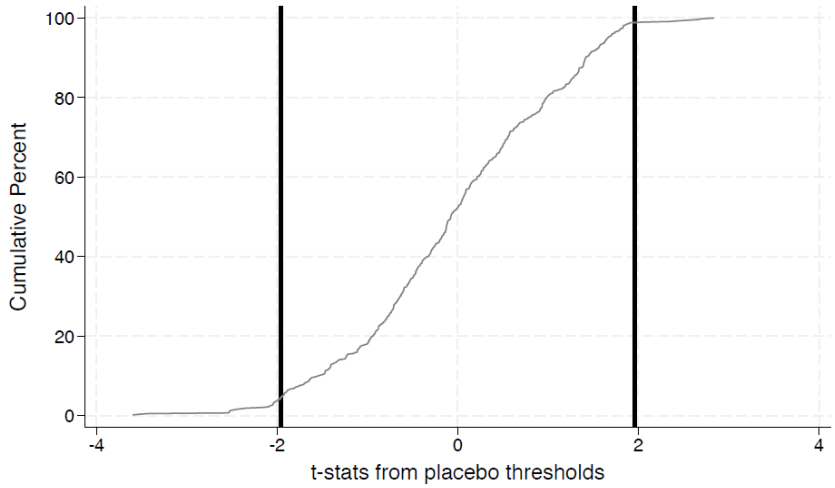
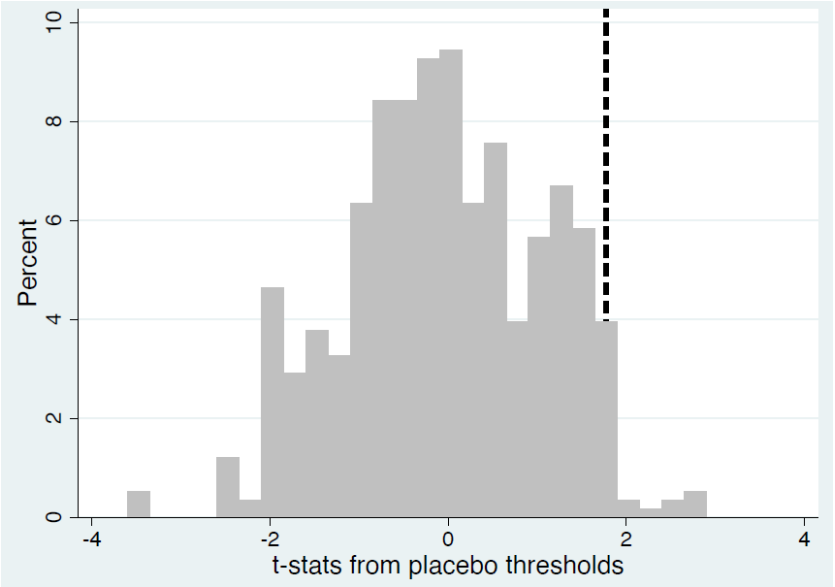


Figure 3.2. Manipulation test of density discontinuity

Notes: The figure implements a manipulation test of density discontinuity based on local polynomial density estimation methods from Cattaneo et al. (2018), using the margin of victory of the left as running variable. We plot the density of the running variable on both sides of the cutoff, for all elections in our sample. Shaded areas are 95% confidence intervals.



(a) Cumulative density function



(b) Histogram

Figure 5.1. Falsification test using placebo thresholds

Notes: The figures report the t-statistics from robust bias-corrected standard errors clustered by country, from re-estimating equation (1) using 580 fake cutoffs between 30% and 1% below and above the true threshold (using increments of 0.1%). Panel (a) plots the empirical cumulative density function of the t-statistics. The solid vertical lines represent the critical values for the normal distribution (±1.96). Panel (b) plots the resulting histogram of t-statistics. The dashed vertical line represents the t-statistic obtained at the true cutoff.

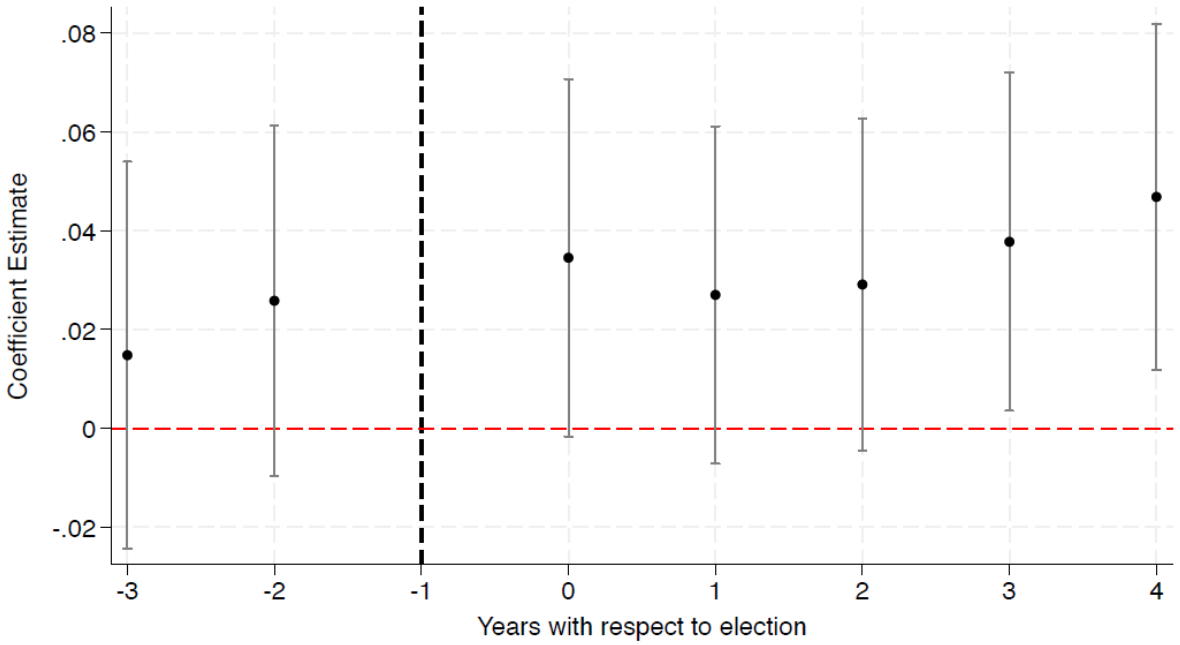


Figure 6.1. Dynamic treatment effects

Notes: The figure plots RD point estimates of treatment effects (with 90% robust confidence intervals) from re-running equation (1) using corruption in year $t - \tau$ as the dependent variable, where t is the election year and $\tau \in \{-3, -2, 0, 1, 2, 3, 4\}$. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths, include country and year fixed effects, and cluster standard errors by country.

Table 2.1. Summary statistics

	Observations	Mean	SD	Min	Max
Margin of victory of the left (%)	1,025	-14.35	41.31	-100.00	100.00
1[Elected government is left-leaning]	1,025	0.35	0.48	0.00	1.00
Corruption	1,025	0.30	0.30	0.00	0.96
Public sector corruption	1,025	0.27	0.30	0.00	0.97
Regime corruption	1,025	0.29	0.31	0.00	0.96
Quality of democracy	1,024	0.57	0.23	0.06	0.86
Freedom of expression	1,025	0.81	0.22	0.02	0.99
Education	892	8.51	2.91	0.67	13.30
Inequality	1,025	0.74	0.25	0.11	0.99
Population (in …)	1,023	2,642.25	4,507.77	6.92	34,196.10
Labor force participation	899	0.42	0.07	0.20	0.71
Trade intensity	902	0.54	0.41	0.02	2.72
Income (real GDP)	902	585,616.59	1,546,433.49	592.60	18,215,924.00
Income per capita	902	20,396.80	15,061.81	349.60	85,342.09
Urbanization (%)	673	25.38	14.08	2.99	63.72

Notes: This table reports summary statistics for our main electoral and outcome variables, and predetermined country characteristics. The data include only elections in the 1945–2018 period for which the running variable, the treatment indicator, and the political corruption index are available. The definition and source of each variable can be found in the main text.

Table 3.1. Tests of discontinuities in pre-election covariates at the cutoff

PANEL A: Continuity of the means of individual covariates

Variable	Estimate	95% CI	p-value	q-value (FDR)	CER-optimal bandwidth	No. of effective obs.
	[1]	[2]	[3]	[4]	[5]	[6]
Corruption ($t-1$)	0.018	[-0.009, 0.046]	0.181	0.579	13.782	329
Corruption ($t-2$)	0.018	[-0.008, 0.045]	0.170	0.579	14.279	341
Corruption ($t-3$)	0.011	[-0.020, 0.041]	0.503	0.764	14.531	349
Quality of democracy	-0.012	[-0.046, 0.023]	0.516	0.764	14.501	349
Freedom of expression	-0.022	[-0.073, 0.028]	0.387	0.764	15.923	370
Inequality	0.004	[-0.028, 0.038]	0.773	0.996	18.229	405
Education	-0.145	[-0.353, 0.023]	0.085*	0.453	14.273	304
Population	168.384	[-270.297, 596.462]	0.461	0.764	25.084	507
Income	29,995.89	[-8.95e+04, 1.83e+05]	0.500	0.764	14.378	319
Income per capita	-21.498	[-46.627, -1.792]	0.034**	0.448	12.749	276
Labor force participation	0.001	[-0.011, 0.013]	0.882	0.996	17.679	358
Trade intensity	0.015	[-0.098, 0.115]	0.873	0.996	17.533	358
Urbanization	0.449	[-0.839, 1.644]	0.525	0.764	19.062	291
Margin of victory at previous election	0.951	[-6.878, 8.632]	0.825	0.996	12.740	278
Treatment status at previous election	0.039	[-0.193, 0.227]	0.875	0.996	14.712	320
Years since last treatment	-2.514	[-5.736, 0.167]	0.064*	0.453	10.906	213

PANEL B: Continuity of the distribution of the set of covariates

Sample	p-value	No. of effective obs.
Full sample	0.742	26
Presidential sample	0.944	14
Parliamentary sample	0.487	17

Notes: Panel A shows the results of local linear regressions like (1), but using each covariate as the outcome variable, and focusing on the intention-to-treat effects. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, with CER-optimal bandwidths, include country and year fixed effects, and cluster standard errors by country. Except otherwise noted, all variables are measured in the year before the election. The definition of each variable can be found in the main text. Column [4] reports p-values adjusted for the False Discovery Rate. Panel B reports the p-values for the joint tests of the continuity of the 16-dimensional vector of covariates in the full sample of elections, and in the subsamples of presidential and parliamentary elections, using the permutation test proposed by Canay and Kamat (2018). * significant at 10% level. ** significant at 5% level.

Table 4.1. Effect of electing a left-wing government on corruption (fuzzy RD estimates)

	Corruption [1]	Public corruption [2]	Regime corruption [3]
LeftGvt	0.036*	0.055**	0.040*
	(0.024)	(0.029)	(0.029)
	[0.065]	[0.014]	[0.081]
MSE-optimal bandwidth	26.830	28.585	29.419
First stage (*LeftWin*)	0.569***	0.570***	0.570***
	(0.087)	(0.086)	(0.086)
	[0.000]	[0.000]	[0.000]
No. of effective obs.	537	557	570

Notes: Estimation of equation (1), using different measures of corruption. *Corruption* is the average level of the political corruption index in the four years following the election. *Public* is the average level of the public sector corruption index in the four years following the election. *Regime corruption* is the average level of the regime corruption index in the four years following the election. *LeftWin* is a dummy variable determining the assignment of a country to treatment (left-wing government) and control (non-left-wing government) groups. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths, include country and year fixed effects, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. First stage (*LeftWin*) reports the first stage in the fuzzy RD estimation. * significant at 10% level. ** significant at 5% level. *** significant at 1% level.

Table 6.1. Mechanisms

	Government spending [1]	Government revenues [2]	Government ownership [3]
LeftGvt	-3.287	-0.533	-0.150
	(3.066)	(1.764)	(0.175)
	[0.227]	[0.723]	[0.216]
MSE-optimal bandwidth	18.824	19.252	32.617
No. of effective obs.	353	286	609
	Business regulations [4]	Bureaucratic quality [5]	Constraints on the executive [6]
LeftGvt	-4.262	-0.043**	-0.058
	(3.446)	(0.022)	(0.045)
	[0.226]	[0.016]	[0.122]
MSE-optimal bandwidth	34.039	31.895	21.417
No. of effective obs.	284	586	453

Notes: Estimation of equation (1), using different outcomes. *Government spending* is the average level of government spending as share of GDP in the four years following the election. *Government revenues* is the average level of total tax revenues in the four years following the election. *Government ownership* is the average level of state ownership in the economy in the four years following the election. *Business regulations* is the average score for business freedom in the four years following the election. *Bureaucratic quality* is the simple average of two indicators: criteria for appointment decisions in the state administration, and rigorous and impartial public administration, computed over the four years following the election. *Constraints on the executive* is the simple average of two indicators: judicial and legislative constraints on the executive, computed over the four years after the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. See the main text for more on variable definitions. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths, include country and year fixed effects, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. ** significant at 5% level.

Table 6.2. Heterogeneity analysis

	Corruption [1]	Public corruption [2]	Regime corruption [3]
Institutional constraints - Internal constraints			
High			
LeftGvt	-0.008	0.009	0.005
	(0.017)	(0.021)	(0.015)
	[0.562]	[0.426]	[0.492]
MSE-optimal bandwidth	23.260	19.227	18.813
No. of effective obs.	239	215	215
Low			
LeftGvt	0.051*	0.076**	0.058*
	(0.031)	(0.039)	(0.039)
	[0.058]	[0.012]	[0.082]
MSE-optimal bandwidth	37.367	32.221	36.884
No. of effective obs.	343	310	340
Institutional constraints - External constraints			
High			
LeftGvt	0.035*	0.072**	0.044
	(0.023)	(0.037)	(0.036)
	[0.060]	[0.010]	[0.125]
MSE-optimal bandwidth	37.202	34.569	38.349
No. of effective obs.	354	342	360
Low			
LeftGvt	0.026	0.026	0.029
	(0.034)	(0.034)	(0.038)
	[0.406]	[0.326]	[0.362]
MSE-optimal bandwidth	29.375	28.942	29.443
No. of effective obs.	264	262	265
Income levels			
High income			
LeftGvt	0.008	0.003	0.004
	(0.021)	(0.018)	(0.018)
	[0.911]	[0.940]	[0.947]
MSE-optimal bandwidth	19.079	15.715	19.177
No. of effective obs.	228	196	228
Non-high income			
LeftGvt	0.041	0.091***	0.058*
	(0.032)	(0.045)	(0.045)
	[0.110]	[0.008]	[0.098]
MSE-optimal bandwidth	34.237	27.338	29.931
No. of effective obs.	286	249	259
Time periods			
Before 1995			
LeftGvt	0.016	0.037	0.020
	(0.020)	(0.031)	(0.026)
	[0.499]	[0.130]	[0.327]
MSE-optimal bandwidth	30.254	26.412	27.541
No. of effective obs.	315	286	297

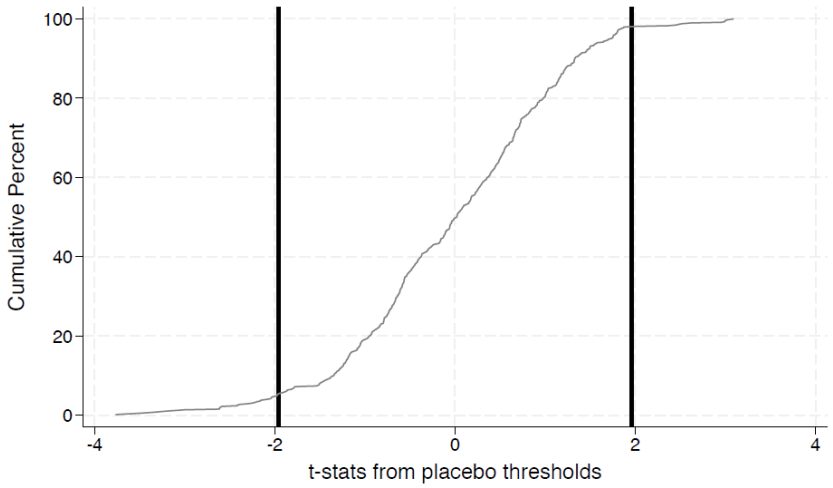
After 1995			
LeftGvt	0.074*	0.103**	0.085*
	(0.047)	(0.055)	(0.061)
	[0.058]	[0.019]	[0.090]
MSE-optimal bandwidth	25.086	23.889	25.685
No. of effective obs.	230	223	234
Policy platforms (parliamentary elections only – reduced form)			
More interventionist			
LeftWin	0.026	0.012	0.013
	(0.016)	(0.017)	(0.017)
	[0.541]	[0.707]	[0.927]
MSE-optimal bandwidth	15.226	12.579	8.871
No. of effective obs.	64	61	63
More market oriented			
LeftWin	-0.002	0.000	-0.007
	(0.017)	(0.019)	(0.025)
	[0.935]	[0.918]	[0.977]
MSE-optimal bandwidth	17.965	19.158	18.207
No. of effective obs.	129	133	133

Notes: This table reports estimated effects of electing a left-wing government for different subsamples. Each estimate corresponds to a separate regression. *Corruption* is the average level of the political corruption index in the four years following the election. *Public corruption* is the average level of the public sector corruption index in the four years following the election. *Regime corruption* is the average level of the regime corruption index in the four years following the election. *LeftWin* is a dummy variable determining the assignment of a country to treatment (left-wing government) and control (non-left-wing government) groups. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. Other variable definitions are in the main text. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths, include country and year fixed effects, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. * significant at 10% level. ** significant at 5% level. *** significant at 1% level.

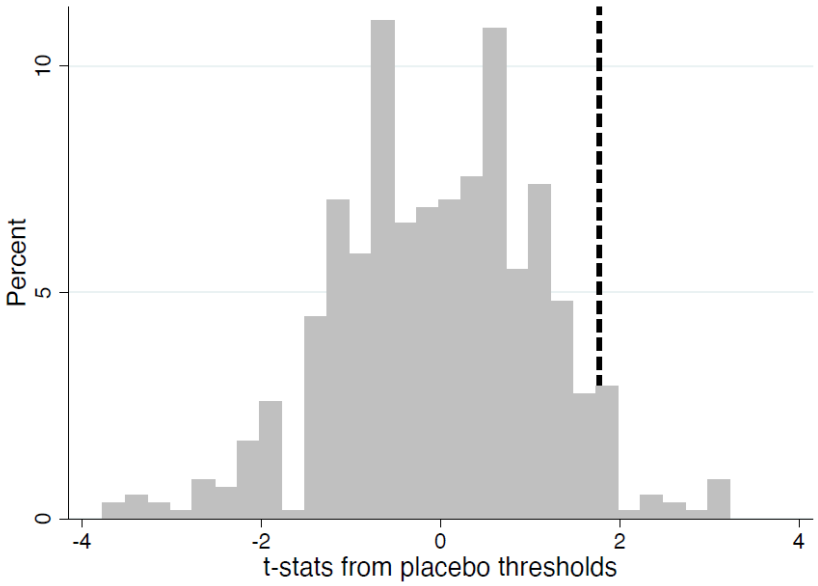
Table 6.3. Country characteristics of countries with and without close elections

Variable	Mean, close	Mean, not close	Difference	p-value	N
Quality of democracy	0.644	0.544	0.031***	0.001	1024
			(0.009)		
Freedom of expression	0.884	0.789	0.034***	0.002	1025
			(0.010)		
Inequality	0.769	0.734	0.007	0.382	1025
			(0.008)		
Education	8.743	8.429	-0.019	0.643	892
			(0.042)		
Population	2645.445	2641.207	144.541*	0.061	1023
			(76.251)		
Urbanization	24.687	25.649	0.403*	0.091	673
			(0.236)		
Income	677,000	553,000	69003.755	0.254	902
			(60136.125)		
Income per capita	20620.921	20316.469	-1284.376**	0.028	902
			(575.525)		
Labor force participation	0.422	0.414	0.002	0.624	899
			(0.003)		
Trade intensity	0.527	0.547	-0.012	0.542	902
			(0.019)		

Notes: This table reports mean values for the pre-election country characteristics for countries experiencing close elections (vote margin under 10%) and the rest of the countries in the estimation sample. It also reports the difference in the means of each variable between the two subsamples. These differences (and corresponding standard errors reported in brackets) are computed through an OLS regression of each country characteristic on an indicator for a close election, and country and year fixed effects. Standard errors are clustered at the country level. Variable definitions are in the main text. * significant at 10% level. ** significant at 5% level. *** significant at 1% level.



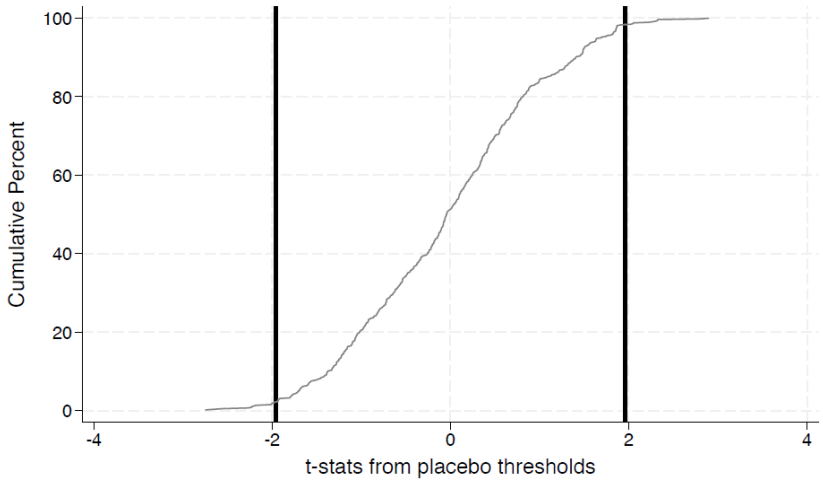
(c) Cumulative density function



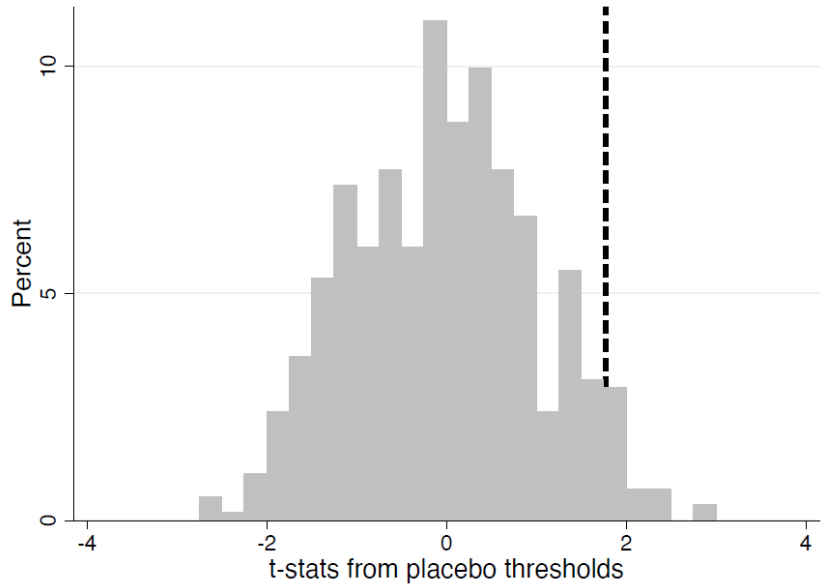
(d) Histogram

Figure A.1. Falsification test using placebo thresholds – Public corruption

Notes: The figures report the t-statistics from robust bias-corrected standard errors clustered by country, from re-estimating equation (1) using 580 fake cutoffs between 30% and 1% below and above the true threshold (using increments of 0.1%). Panel (a) plots the empirical cumulative density function of the t-statistics. The solid vertical lines represent the critical values for the normal distribution (±1.96). Panel (b) plots the resulting histogram of t-statistics. The dashed vertical line represents the t-statistic obtained at the true cutoff.



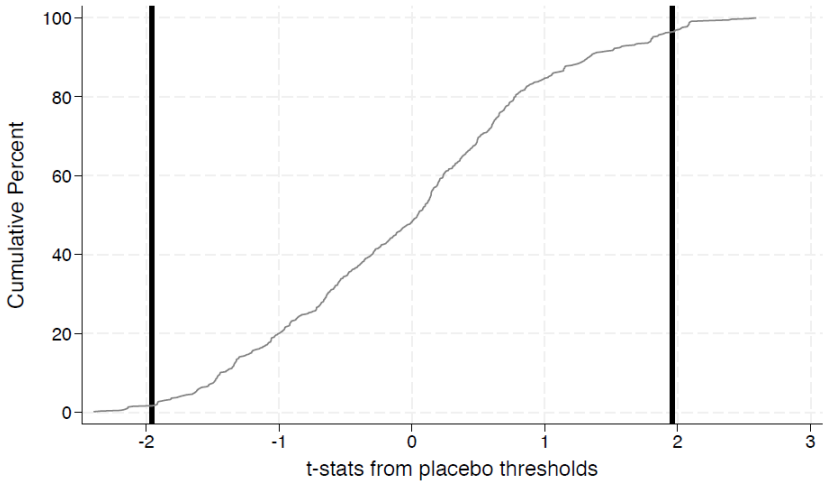
(a) Cumulative density function



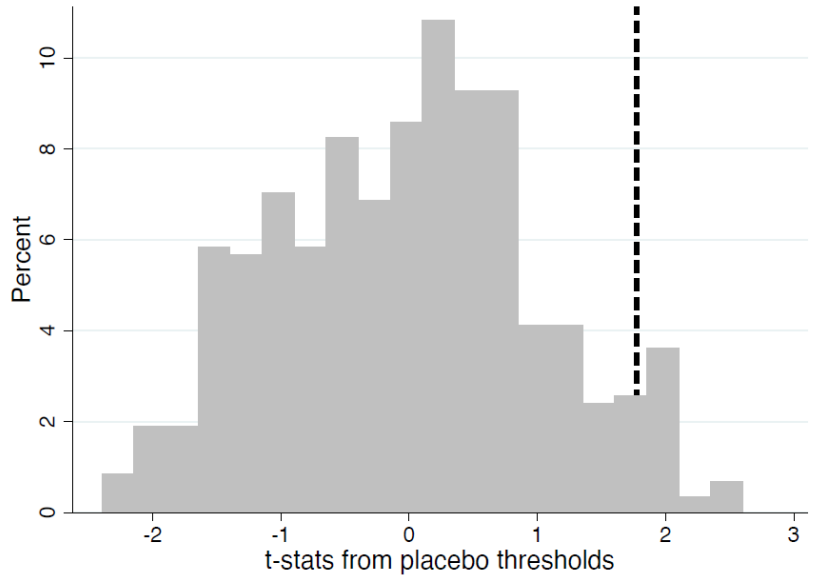
(b) Histogram

Figure A.2. Falsification test using placebo thresholds – Regime corruption

Notes: The figures report the t-statistics from robust bias-corrected standard errors clustered by country, from re-estimating equation (1) using 580 fake cutoffs between 30% and 1% below and above the true threshold (using increments of 0.1%). Panel (a) plots the empirical cumulative density function of the t-statistics. The solid vertical lines represent the critical values for the normal distribution (±1.96). Panel (b) plots the resulting histogram of t-statistics. The dashed vertical line represents the t-statistic obtained at the true cutoff.



(a) Cumulative density function



(b) Histogram

Figure A.3. Falsification test using placebo thresholds – Executive corruption

Notes: The figures report the t-statistics from robust bias-corrected standard errors clustered by country, from re-estimating equation (1) using 580 fake cutoffs between 30% and 1% below and above the true threshold (using increments of 0.1%). Panel (a) plots the empirical cumulative density function of the t-statistics. The solid vertical lines represent the critical values for the normal distribution (±1.96). Panel (b) plots the resulting histogram of t-statistics. The dashed vertical line represents the t-statistic obtained at the true cutoff.

Table A.1. Effect of electing a left-wing government on alternative computations of corruption (fuzzy RD estimates)

	Corruption [1]	Public corruption [2]	Regime corruption [3]
Panel A: Outcomes computed over the 5 years after the election			
LeftGvt	0.036*	0.054**	0.039*
	(0.023)	(0.027)	(0.028)
	[0.060]	[0.011]	[0.082]
MSE-optimal bandwidth	26.989	29.531	29.706
No. of effective obs.	540	570	570
Panel B: Outcomes computed over the 3 years after the election			
LeftGvt	0.031	0.051**	0.035
	(0.023)	(0.029)	(0.029)
	[0.109]	[0.023]	[0.128]
MSE-optimal bandwidth	26.129	27.393	28.839
No. of effective obs.	520	544	560

Notes: Estimation of equation (1), using different measures of corruption. *Corruption* is the average level of the political corruption index in the five (panel A) and three (panel B) years following the election. *Public* is the average level of the public sector corruption index in the five (panel A) and three (panel B) years following the election. *Regime corruption* is the average level of the regime corruption index in the five (panel A) and three (panel B) years following the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths, include country and year fixed effects, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. * significant at 10% level. ** significant at 5% level.

Table A.2. Effect of electing a left-wing government on executive corruption, and by political system (fuzzy RD estimates)

	Executive corruption [1]	Corruption [2]	Corruption [3]
LeftGvt	0.049*	0.029	0.065
	(0.034)	(0.028)	(0.540)
	[0.060]	[0.158]	[0.230]
Sample	All elections	Presidential	Parliamentary
MSE-optimal bandwidth	30.552	21.009	21.282
No. of effective obs.	582	150	300

Notes: Estimation of equation (1), using different measures of corruption as defined in the main text. *Executive corruption* is the average level of the executive corruption index in the four years following the election. *Corruption* is the average level of the political corruption index in the four years following the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. The regression uses the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths, includes country and year fixed effects, and clusters standard errors by country. Robust and bias-corrected standard error reported in parenthesis. The associated p-value is reported in brackets. * significant at 10% level.

Table A.3. Robustness to different bandwidth choices

	Corruption	Public corruption	Regime corruption	Executive corruption
	[1]	[2]	[3]	[4]
$\boldsymbol{h = h^*/2}$				
LeftGvt	0.049*	0.088**	0.058	0.072
	(0.031)	(0.042)	(0.043)	(0.052)
	[0.095]	[0.036]	[0.190]	[0.183]
MSE-optimal bandwidth	13.415	14.293	14.710	15.276
No. of effective obs.	325	347	350	363
$\boldsymbol{h = 2h^*}$				
LeftGvt	0.028*	0.034**	0.030*	0.033*
	(0.019)	(0.023)	(0.024)	(0.027)
	[0.065]	[0.015]	[0.080]	[0.053]
MSE-optimal bandwidth	53.660	57.170	58.838	61.104
No. of effective obs.	795	811	824	827
MSE-optimal, two different				
LeftGvt	0.035*	0.058**	0.041*	0.051*
	(0.023)	(0.029)	(0.029)	(0.034)
	[0.068]	[0.013]	[0.087]	[0.069]
No. of effective obs.	556	544	561	555
CER-optimal, common				
LeftGvt	0.040*	0.067**	0.047*	0.058*
	(0.024)	(0.030)	(0.031)	(0.036)
	[0.063]	[0.012]	[0.082]	[0.060]
CER-optimal bandwidth	20.650	22.000	22.642	23.515
No. of effective obs.	441	464	472	489
CER-optimal, two different				
LeftGvt	0.040*	0.070***	0.048*	0.060*
	(0.024)	(0.031)	(0.030)	(0.036)
	[0.064]	[0.010]	[0.083]	[0.066]
No. of effective obs.	458	447	466	460

Notes: Estimation of equation (1), using different measures of corruption. *Corruption* is the average level of the political corruption index in the four years following the election. *Public corruption* is the average level of the public sector corruption index in the four years following the election. *Regime corruption* is the average level of the regime corruption index in the four years following the election. *Executive corruption* is the average level of the executive corruption index in the four years following the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, include country and year fixed effects, and cluster standard errors by country. "$h = h^*/2$" is half the MSE-optimal bandwidth from each original regression. "$h = 2h^*$" is twice the MSE-optimal bandwidth from the original regression. "MSE-optimal, two different" means that two different bandwidth sizes are used (below and above the cutoff). "CER-optimal, common" is the CER-optimal bandwidth selector from Calonico et al. (2014) with a unique bandwidth size on both sides of the cutoff. "CER-optimal, two different" is the CER-optimal bandwidth using two different bandwidth sizes below and above the cutoff. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. * significant at 10% level. ** significant at 5% level. *** significant at the 1% level.

Table A.4: Effect of electing a left-wing government on corruption with different polynomial orders (fuzzy RD estimates)

	Corruption [1]	Public corruption [2]	Regime corruption [3]	Executive corruption [4]
Linear (order 1) – baseline				
LeftGvt	0.036*	0.055**	0.040*	0.049*
	(0.024)	(0.029)	(0.029)	(0.034)
	[0.065]	[0.014]	[0.081]	[0.060]
MSE-optimal bandwidth	26.830	28.585	29.419	30.552
No. of effective obs.	537	557	570	582
Quadratic (order 2)				
LeftGvt	0.045*	0.077**	0.056*	0.072*
	(0.027)	(0.034)	(0.037)	(0.044)
	[0.064]	[0.012]	[0.087]	[0.070]
MSE-optimal bandwidth	39.076	40.384	34.990	34.167
No. of effective obs.	681	696	644	637
Cubic (order 3)				
LeftGvt	0.055*	0.103**	0.063	0.080
	(0.029)	(0.042)	(0.041)	(0.049)
	[0.054]	[0.013]	[0.122]	[0.108]
MSE-optimal bandwidth	35.978	35.393	38.430	39.259
No. of effective obs.	650	647	672	683
Quartic (order 4)				
LeftGvt	0.057*	0.102**	0.063	0.077
	(0.030)	(0.044)	(0.042)	(0.051)
	[0.057]	[0.021]	[0.141]	[0.144]
MSE-optimal bandwidth	41.882	43.371	43.222	44.409
No. of effective obs.	708	730	727	737

Notes: Estimation of equation (1), using different measures of corruption and different polynomial orders. *Corruption* is the average level of the political corruption index in the four years following the election. *Public corruption* is the average level of the public sector corruption index in the four years following the election. *Regime corruption* is the average level of the regime corruption index in the four years following the election. *Executive corruption* is the average level of the executive corruption index in the four years following the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, include country and year fixed effects, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. * significant at 10% level. ** significant at 5% level.

Table A.5. Effect of electing a left-wing government on corruption with country-specific trends (fuzzy RD estimates)

	Corruption [1]	Public corruption [2]	Regime corruption [3]	Executive corruption [4]
LeftGvt	0.026**	0.025**	0.031**	0.031**
	(0.016)	(0.015)	(0.017)	(0.018)
	[0.043]	[0.030]	[0.026]	[0.037]
MSE-optimal bandwidth	28.228	24.370	29.941	33.534
No. of effective obs.	553	500	575	624

Notes: Estimation of equation (1), using different measures of corruption. *Corruption* is the average level of the political corruption index in the four years following the election. *Public corruption* is the average level of the public sector corruption index in the four years following the election. *Regime corruption* is the average level of the regime corruption index in the four years following the election. *Executive corruption* is the average level of the executive corruption index in the four years following the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, include country and year fixed effects, and country-specific time trends, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. ** significant at 5% level.

Table A.6. Donut-hole analysis (fuzzy RD estimates)

	Corruption [1]	Public corruption [2]	Regime corruption [3]	Executive corruption [4]
|MV| ≥ 0.10				
Left margin	0.037*	0.060**	0.043*	0.052*
	(0.025)	(0.030)	(0.031)	(0.036)
	[0.066]	[0.012]	[0.082]	[0.064]
MSE-optimal bandwidth	25.987	25.741	27.299	27.627
No. of effective obs.	512	509	539	541
|MV| ≥ 0.20				
Left margin	0.037*	0.060**	0.043*	0.052*
	(0.025)	(0.030)	(0.031)	(0.036)
	[0.066]	[0.012]	[0.082]	[0.064]
MSE-optimal bandwidth	25.987	25.741	27.299	27.627
No. of effective obs.	512	509	539	541
|MV| ≥ 0.30				
Left margin	0.040**	0.062**	0.049**	0.061**
	(0.024)	(0.031)	(0.029)	(0.034)
	[0.037]	[0.012]	[0.027]	[0.020]
MSE-optimal bandwidth	27.450	26.089	28.405	28.277
No. of effective obs.	539	513	550	547
|MV| ≥ 0.40				
Left margin	0.040**	0.062**	0.049**	0.061**
	(0.024)	(0.031)	(0.029)	(0.034)
	[0.037]	[0.012]	[0.027]	[0.020]
MSE-optimal bandwidth	27.450	26.089	28.405	28.277
No. of effective obs.	539	513	550	547
|MV| ≥ 0.50				
Left margin	0.043**	0.067***	0.053**	0.065**
	(0.025)	(0.033)	(0.030)	(0.035)
	[0.034]	[0.010]	[0.023]	[0.016]
MSE-optimal bandwidth	26.738	25.276	27.881	27.881
No. of effective obs.	527	499	539	539

Notes: Estimation of equation (1), using different measures of corruption, and excluding observations very close (within ±0.1 to ±0.5%) to the cutoff. *Corruption* is the average level of the political corruption index in the four years following the election. *Public corruption* is the average level of the public sector corruption index in the four years following the election. *Regime corruption* is the average level of the regime corruption index in the four years following the election. *Executive corruption* is the average level of the executive corruption index in the four years following the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, include country and year fixed effects, and country-specific time trends, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. * significant at 10% level. ** significant at 5% level. *** significant at 1% level.

Table A.7. Mechanisms – Heterogeneity analysis

	Government ownership
High constraints on the executive	
LeftGvt	0.241
	(0.189)
	[0.129]
MSE-optimal bandwidth	19.650
No. of effective obs.	215
Low constraints on the executive	
LeftGvt	-0.365*
	(0.244)
	[0.052]
MSE-optimal bandwidth	28.178
No. of effective obs.	279

Notes: This table reports estimated effects of electing a left-wing government for different subsamples, according to the strength of checks and balances in the country, on *Government ownership*, the average level of state ownership in the economy in the four years following the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. See the main text for more on variable and subsample definitions. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths, include country and year fixed effects, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. * significant at 10% level.

Table A.8. Mechanisms – Decomposition of the bureaucratic quality score

	Meritocratic recruitment	Rigorous and impartial public administration
	[1]	[2]
LeftGvt	-0.267**	-0.300*
	(0.165)	(0.182)
	[0.033]	[0.074]
MSE-optimal bandwidth	27.205	25.843
No. of effective obs.	536	515

Notes: Estimation of equation (1), using different outcomes. *Meritocratic recruitment* is the average level of the criteria for appointment decisions in the state administration index in the four years following the election. *Rigorous and impartial public administration* is the average level of the rigorous and impartial public administration index in the four years following the election. *LeftGvt* is a dummy variable taking value 1 if a left government is formed after the election. See the main text for more on variable definitions. All regressions use the Calonico et al. (2014) robust and bias-corrected RD estimator, with MSE-optimal bandwidths, include country and year fixed effects, and cluster standard errors by country. Robust and bias-corrected standard errors are reported in parenthesis. The associated p-values are reported in brackets. * significant at 10% level. ** significant at 5% level.

Table A.9. Heterogeneity analysis – Manipulation tests for subsamples

Subsample	N	Bandwidth (left)	Bandwidth (right)	t-stat (robust)	p-value
High internal constraints	423	41.350	33.858	-1.5466	0.1220
Low internal constraints	602	49.385	45.495	-0.3806	0.7035
High external constraints	553	30.946	25.806	-1.1636	0.2446
Low external constraints	472	46.523	62.321	-1.5420	0.1231
High income	485	22.293	16.502	0.2670	0.7894
Non-high income	540	26.975	33.385	-0.4180	0.6760
Before 1995	527	36.718	33.710	-1.3802	0.1675
After 1995	498	40.466	34.905	0.5283	0.5973
More interventionist	305	19.408	14.775	0.8004	0.4235
More market oriented	305	39.315	46.440	-1.3201	0.1868

Notes: The table reports results from manipulations tests of density discontinuity based on local polynomial density estimation methods from Cattaneo et al. (2018), using the margin of victory of the left as running variable. Details on the subsampling are in the main text.