# Does the Early Bird always Get the Worm?
## First Round Advantages and Second Round Victories in Latin America*

Adrián Lucardi
Department of Political Science
ITAM
adrian.lucardi@itam.mx

Juan Pablo Micozzi
Department of Political Science
ITAM
juan.micozzi@itam.mx

Agustín Vallejo
Hobby School of Public Affairs
University of Houston
avallejo7@uh.edu

December 2, 2022

Word count: 5778

**Abstract**

Runoff systems allow for a reversion of the first-round result: the most voted candidate in the first round may end up losing the election in the second. But do voters take advantage of this opportunity? Or does winning the first round increase the probability of winning the second? We investigate this question with data from presidential elections since 1945, as well as subnational elections in Latin America. Using a regression discontinuity design, we find that being the most voted candidate in the first round has a substantial positive effect on the probability of winning the second round in mayoral races –especially in Brazil–, but in presidential and gubernatorial elections the effect is negative, though not statistically significant at conventional levels. The positive effect in municipal races is much stronger when the top-two placed candidates are ideologically close –and thus harder to distinguish for voters– but weakens considerably and becomes insignificant when the election is polarized. We attribute these differences to the disparate informational environment prevailing in local vs. higher-level races.

**Keywords**: electoral systems – runoff – first-round advantage – bandwagon effect – regression discontinuity

---

*Lucardi and Micozzi thank the Asociación Mexicana de Cultura, A.C. for financial support. Carlos Villaseñor, Monserrat Pérez Villanueva and Uriel Luna provided invaluable research assistance. Previous versions of this paper were presented at ITAM, the 2018 LASA Annual Meeting and the 2022 International Methods Colloquium. We thank Federico Estévez, Allyson Benton, Francisco Cantú, Juan Olmeda, Eric Magar, Justin Esarey, Gustavo Novoa and two anonymous reviewers for their helpful comments. All remaining mistakes are the authors' responsibility.

The two-round (qualified) majority system –henceforth the runoff system or runoff– is one of the most popular electoral systems around the world. Used extensively in legislative elections in France as well as in some national- and state-level elections in the United States –the Democrats' control of the Senate in 2021 hinged on two runoffs in Georgia–, since the 1990s it has also been widely employed in presidential elections in Latin America, Eastern Europe and Francophone Africa. More than half of such elections have been decided in the second round and, conditional on a runoff being needed, the second-placed candidate emerged victorious around one-third of the time (see Figure 1 and Table 1). The Fujimori family in Peru is a living example of these dynamics: together, Alberto Fujimori and his daughter Keiko contested a runoff five times since 1990, twice in the first place and three times as runner-ups. He won his two runoffs (the second under credible accusations of fraud); she lost all three, two of them by less than 50,000 votes. Even in Brazil, where the first-placed candidate in the presidential election has always emerged victorious in the runoff, reversals are quite common at the subnational level: in the most recent gubernatorial elections (2022), 4 out of 12 runner-ups overturned the result in the second round.

The possibility of such *reversions* –meaning that the second-placed candidate in the first round overturns the outcome and emerges victorious in the second– is one of the most appealing features of the runoff system, as it gives voters the possibility to evaluate candidates more carefully and facilitates rejecting those who are only supported by a narrow subset of the electorate –i.e., discarding *Condorcet losers* (Bordignon, Nannicini and Tabellini 2016, though this depends on the threshold employed; see Bouton 2013). However, recent research suggests that voters may not take full advantage of this opportunity. The reason is that *rankings matter*: when given the chance, voters, party elites and/or campaign donors prefer higher-ranked candidates over lower-ranked ones, even if there is little difference between the two (Kiss and Simonovits 2014; Morton et al. 2015; Anagol and Fujiwara 2016; Hix, Hortala-Vallve and Riambau-Armet 2017; Pons and Tricaud 2018; Granzier, Pons and Tricaud 2019; Gulzar, Robinson and Ruiz 2022).

Why does this effect exist, and when does it operate? When more than two candidates may participate in the second round (Granzier, Pons and Tricaud 2019); if voters want to find a suitable challenger to run against the incumbent (Anagol and Fujiwara 2016); or when donors prefer to rally behind the winner

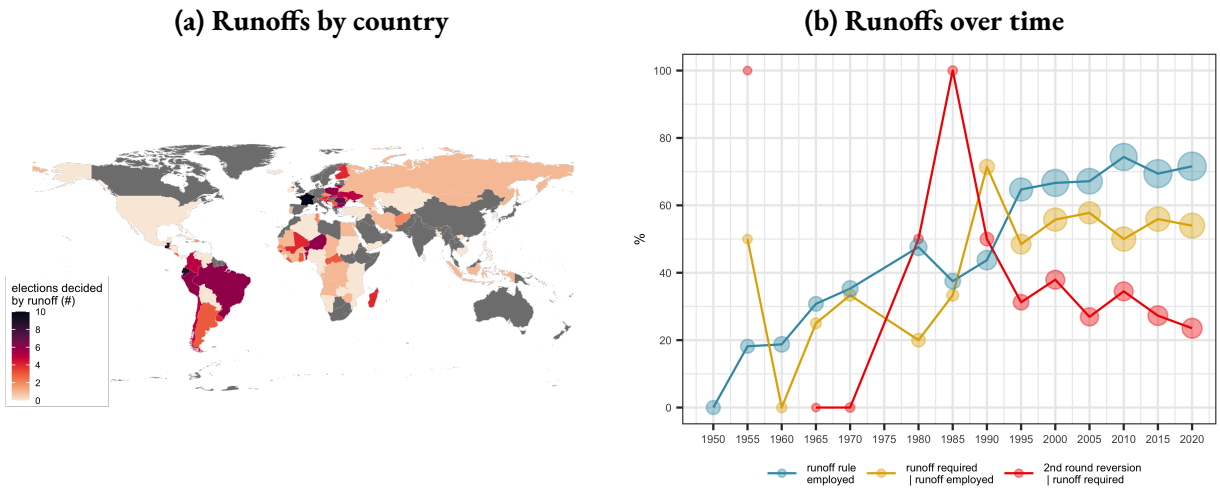



Figure 1: The runoff system in presidential elections, 1945-2020. The dots in panel (b) show averages over the previous five years, with dot sizes proportional to the number of observations.

(Gulzar, Robinson and Ruiz 2022), supporting the higher-ranked candidate may be optimal from a coordination perspective –i.e., higher-ranked candidates may become focal. However, voters may also opt for the first-ranked alternative for purely psychological reasons (Chun and Larrick 2022), giving rise to a "bandwagon" effect (Kiss and Simonovits 2014; Morton et al. 2015; Hix, Hortala-Vallve and Riambau-Armet 2017; though cf. Chatterjee and Kamal 2021).

To the extent that bandwagoning is regularly observed in high-stakes elections, it raises troubling implications for democratic representation in general and the runoff system in particular. But if it occurs only rarely, or in less relevant circumstances, the implications are less dire. Elucidating why voters rely on ranks is also important: are rankings the heuristic of choice for distinguishing between candidates, or do they constitute a last resource when there is no better way to adjudicate between them?

In a groundbreaking contribution, Granzier, Pons and Tricaud (2019) go a long way towards answering some of these questions in France and a complementary sample of 19 (mostly European) countries. Their findings reveal a substantial advantage for higher-placed first round candidates. This is partly explained by coordination dynamics on the part of parties and voters, but bandwagoning behind the better-ranked candidate is also commonplace.

While these findings are compelling and rigorously performed, the scope of their design leaves space for further inquiries. To begin with, the executive elections –mostly at the subnational level, and in Latin America– that we analyze in this paper differ from the legislative elections in France and other 18 European countries (plus Haiti) that comprise Granzier, Pons and Tricaud's (2019) sample. In France, more than two candidates may participate in the second round, allowing these authors both to compare the effect of multiple rankings –first vs. second, second vs. third, etc– and to examine (pretty common) withdrawals. This matters for interpretation, as rankings can provide a useful focal point when more than two candidates reach the second round. But in executive elections, typically only two contenders may participate in the runoff, and withdrawals are rare –just 12 (out of 663) runoff races in our sample featured one. Since two-candidate elections offer no possibility to vote strategically, our findings can be interpreted as a pure bandwagon effect. Second, Brazilian politicians who win a mayoralty by a small margin are neither more nor less likely to win the next mayoral election than the runner-up (de Magalhães 2015). As with the literature on the incumbency advantage more generally, the reasons behind this (null) result are unclear; by showing that first-placed candidates do much better than runner-ups in the second round, our results suggest that mayoral candidates begin with an initial advantage before assuming office but dilapidate it during the subsequent four years.[1] In addition, Granzier, Pons and Tricaud's (2019) finding that bandwagon effects are smaller in ideologically polarized elections may not travel to Brazil, where most parties are pretty weak (Klašnja and Titiunik 2017). Finally, by examining different levels of government –national, state and municipal– with varying levels of policy relevance and visibility among voters, we can gain leverage on the potential sources behind the bandwagoning effect in runoff elections.

Specifically, we analyze a sample of runoff elections used to elect executive authorities: presidents in 69 countries, plus governors and mayors in Argentina, Bolivia, Brazil, Chile and Mexico (see Table 1). Employing a regression discontinuity design to identify the effect of being placed first in the first round, we document three main findings. First, in line with the literature, rankings matter: finishing first in the first round has a large positive effect on both the probability of winning the election –between 11 and

[1] Though de Magalhães's sample excludes municipalities with runoff elections, and thus does not overlap with ours.

40 percentage points– and on vote shares –a 1.6-4.7 pp. increase. Second, there is heterogeneity across samples: these results are mostly driven by *municipal* elections in Brazil and Mexico. But in presidential and gubernatorial elections the effect of finishing first is negative –minus 13 pp.–, though the corresponding estimates are far from statistically significant. Finally, even in mayoral elections, the results depend on the ideological distance between the candidates. When the top two candidates are far apart ideologically, the advantage of finishing first in the first round diminishes substantially and becomes statistically insignificant; but if they are ideologically close, the first-round advantage increases to a massive 50-61 pp. Thus, while our results can thus be seen as an extension of Granzier, Pons and Tricaud's (2019), we find much larger effects, suggesting that rankings matter more when party labels convey less information about candidates' ideological positions (see also Gulzar, Robinson and Ruiz 2022).

These findings may be driven by a combination of statistical and substantive factors. Presidential elections may be different due to balance issues –second-placed candidates are more right-wing than first-placed ones–, but this does not apply to gubernatorial elections. The small number of observations included in some samples (see Table 1) means that some of our estimates are underpowered. Yet while this can explain why many estimates are statistically insignificant, in most samples we have enough power to detect an effect as large as one standard deviation of the outcome variable in the control group, and except for mayoral elections in Brazil and Mexico or subnational elections in Brazil, point estimates are much smaller than that. Indeed, when we pool all samples together, we maximize power, yet the effects are weaker and sometimes insignificant; in contrast, when we split the mayoral and subnational Brazil samples according to the ideological distance between the top two candidates, we have enough power to detect a large effect in ideologically close elections.

On the substantive side, there are reasons to expect mayoral elections to differ from presidential and gubernatorial ones. An important one is voters' awareness of, and familiarity with, the candidates. Presidential elections typically receive substantial media coverage, with candidates' personalities, vita, and stances being scrutinized for weeks, if not months. In federal countries like Argentina or Brazil, governors are strong political players, and thus voters are familiar with them (Gervasoni 2010; Samuels 2003).

In local-level elections, in contrast, voters may be much less informed about candidates, making them more likely to adopt the heuristic of simply picking the first-placed one. That said, the fact that the effect weakens considerably when candidates' ideological positions are far away from each other suggests that rankings are just one among multiple pieces of information voters consider when deciding whom to support in the second round. To put it simply, voters are more likely to rely on rankings when there is no competing and easy-to-access clue –such as ideology –to adjudicate between candidates. This interpretation is reinforced by the fact that a huge proportion of our sample consists of open seats, with no incumbent seeking reelection, and restricting the sample to open seats produces similar results. Furthermore, when we subset the Brazilian data according to the previous experience of the top two placed candidates,[2] the effect is strongest when neither candidate or both were presumed to be known based on their previous political experience –ie, there is no "experience advantage" for any candidate. When only one of them was, on the other hand, the more experienced individual does not seem to be advantaged.

## Data and Research Design

**Data.** We consider an election to be held under a runoff system if

1. The most voted candidate needs to obtain a minimum percentage of votes –typically 50%, though lower thresholds are sometimes observed– to win outright in the first round; and
2. In case no candidate is victorious in the first round, the same electorate[3] must choose between the top-$n$ contenders in a second round, where typically $n = 2$.

Our focus is on executive elections. For simplicity, we excluded the handful of presidential elections in which more than two candidates may participate in the second round. We also ruled out those elections

[2] We thank an anonymous reviewer for suggesting this analysis.

[3] This excludes cases in which the legislature, rather than citizens, decides among the top-$n$ contenders in the first round (e.g., Chile before 1973).

Table 1: Samples included in the analysis

| sample | office | period covered | number of districts | runoff rule employed | second round needed | % second round | number of reversions | % reversions |
|---|---|---|---|---|---|---|---|---|
| World | president | 1951-2020 | 69 | 352 | 182 | 51.7 | 58 | 31.9 |
| Brazil (governor) | governor | 1994-2018 | 27 | 189 | 91 | 48.1 | 27 | 29.7 |
| Brazil (mayor) | mayor | 1996-2020 | 97 | 519 | 300 | 57.8 | 76 | 25.3 |
| Argentina | governor | 1973-2021 | 24 | 51 | 30 | 58.8 | 8 | 26.7 |
| Bolivia | governor | 2010-2021 | 9 | 19 | 6 | 31.6 | 3 | 50.0 |
| Chile | governor | 2021-2021 | 16 | 16 | 13 | 81.2 | 3 | 23.1 |
| Mexico | mayor | 1997-2000 | 58 | 116 | 41 | 35.3 | 12 | 29.3 |
| Full sample | | | 300 | 1262 | 663 | 52.5 | 187 | 28.2 |

Except for the Brazil (governor), Chile and Mexico samples, the "runoff rule employed" column is not a multiple of the "number of districts" column because different districts held elections every 4, 5 or 6 years (World), they introduced a runoff rule at different moments in time (World, Argentina, Bolivia), or reached 200,000 registered voters in different election years (mayoral elections in Brazil).

(mainly in Mexico in 2003) in which a second round could be avoided if turnout in the first round was sufficiently high, as this emphasizes turnout rather than candidates' vote shares.[4]

We combined data from seven samples (see Table 1). First, a team of research assistants assembled a list of presidential elections around the world (1945-2020) and coded whether they were held under a runoff system. We then restricted the sample to minimally competitive elections (Hyde and Marinov 2012) with a V-Dem polyarchy score larger than 1/3 on a 0:1 scale (Coppedge et al. 2021). For subnational elections, we looked at Latin American cases where we know a runoff rule was employed: Argentine governors (22 provinces in 1973; 4 provinces since the 1990s); Bolivian governors since 2010; Brazilian governors and mayors in municipalities with more than 200,000 registered voters (Fujiwara 2011) since 1994; Chilean regional governors in 2021; and mayors in San Luis Potosí (Mexico) in 1997 and 2000. Appendix A1 lists the corresponding sources.

Since some of these samples are quite small and the Brazilian ones represent 59% of observations, we aggregated the data into six partially overlapping (sub)samples:

1. The *full sample* includes all observations ($N_{\text{runoff}} = 663$);

[4] Alternatively, if turnout is sufficiently low, a second round may be held even if one candidate gets more than 50% of the vote. We removed the handful of elections in which this happened from the sample.

2. The *presidential* sample only includes presidential elections ($N_{\text{runoff}} = 182$);
3. The *gubernatorial* sample includes gubernatorial elections ($N_{\text{runoff}} = 140$);
4. The *mayoral* sample includes local elections in Brazil and Mexico ($N_{\text{runoff}} = 341$);
5. The *subnational (Brazil)* sample includes all non-presidential elections in Brazil ($N_{\text{runoff}} = 391$);
6. The *subnational (non-Brazil)* sample includes all other non-presidential elections ($N_{\text{runoff}} = 90$).

Thus, we can both check whether presidential and gubernatorial elections are different, or whether the results are driven by mayoral elections in Brazil and Mexico and/or subnational elections in Brazil. To code candidates' ideological positions, we used the scores provided by Herrmann and Döring (forthcoming), who code parties' ideological positions along the Left-Right dimension by aggregating information from their Wikipedia tags. This measure maximizes coverage –we have data for the first and second-placed candidate for over 80% of second rounds (see Figure A3a), but treats parties' ideologies as time-invariant. As a robustness check, we measure candidates' ideologies with the V-Party v.1 dataset, which is based on country expert ratings that vary by (legislative) election (Lührmann et al. 2020). At 0.86, both measures are highly correlated along the Left-Right dimension (see Figure A4a), but the latter is only available for 50-70% of observations (see Figure A3b). For subnational elections, we implicitly assume that national party ideology scores hold.

**Variables.** We aggregated the data at both the election and the candidate-election levels. For the former, we recorded election dates, whether a second round was needed, and whether there was a reversion. At the candidate level, we collected data on party ID, partisan and individual incumbency status, previous elected experience (for Brazil only), the number and percentage of (valid) votes obtained in each round, first-round rank, ideology score(s) from both Wikipedia and V-Party, withdrawal from the race, and whether the candidate in question was declared the election winner. We then restricted the sample to the top-two placed candidates (in the first round), in elections that required a second round. We kept those elections in which a second round *should* have been held but was not because one of the top-two vote-getters withdrew from the race, as such withdrawals are strategic: they reflect candidates' expectations about

the second round outcome.[5] Tables A1 and A2 report the descriptive statistics measured at the election and the candidate levels, respectively.

We consider two outcomes: *winner*, a dummy that takes the value of 100 if a candidate won the election, and 0 otherwise;[6] and *vote share*$_{R2}$, the candidates' vote % in the second round.[7] For subnational elections in Brazil, we code governors and mayors as experienced if they were the sitting incumbent or had occupied an elected position as president, senator or governor in the past; mayoral candidates are also coded as experienced if they had served as federal deputies or mayors.[8]

**Identification.** We seek to determine whether a candidate enjoys an electoral advantage in the second round *solely by having finished first in the initial round*. This is problematic insofar as first-placed candidates are probably more popular and better funded than runner-ups. These differences, rather than finishing first in the initial round, may explain their subsequent victory. We thus employ a regression discontinuity (RD) design, comparing candidates who finished first or second by a small margin. This can only identify "local" effects, which strictly speaking means the effect at the point of the discontinuity, i.e. when the margin of victory between the first and second-placed candidates is exactly zero. This is not an issue in our context, as candidates who win the first round comfortably are probably more charismatic, popular, or better funded, and thus their second-round victory is in a sense "overdetermined." The more

[5] For the same reason, whenever a withdrawal resulted in a lower-ranked candidate participating in the second round, we still kept the top-two placed candidates in the sample.

[6] We use a 0/100 dummy so that effect sizes can be interpreted as percentage point changes.

[7] We excluded elections with withdrawals from the vote share analysis. The exceptions are Benin 2001, Costa Rica 2014, Sao Tome and Principe 2016 and the Brazilian municipality of Niteroi in 2004, in which a runoff was actually held, e.g. between the candidate who did not withdraw and the third- (or fourth-) placed candidate in the first round. In these cases, we only included the candidate who did not withdraw in the sample.

[8] These decisions reflect the relative frequency of these offices, and thus their visibility to voters. Brazil has over 5,500 municipalities in 27 states; thus, former mayors running for governor may not be particularly well known. Similarly, each state has just 3 senators but between 8 and 70 federal deputies, meaning that the typical deputy is not very well known across the state –but may be in the municipality in which (s)he is running (Ames 2001).

empirically interesting –and normatively problematic– question is whether candidates who are not substantially better and/or more popular enjoy an advantage simply by virtue of finishing first in the first round. Specifically, we estimate the following equation:

$$Y_{i,e} \quad = \quad \alpha + \tau_{\text{RD}} \cdot F_{i,e} + \beta \cdot \text{first round margin}_{i,e} + \gamma \cdot F_{i,e} \cdot \text{first round margin}_{i,e} + \varepsilon_{i,e}, \qquad (1)$$

where $Y_{i,e}$ is the outcome for candidate $i$ in election $e$, $F_{i,e}$ takes the value of 1 if candidate $i$ finished first in the first round of election $e$ and 0 otherwise, and *first round margin* is the percentage point distance between candidate $i$ and candidate $-i$. Following Calonico, Cattaneo and Titiunik (2014), we estimate the effect of interest non-parametrically, fitting a separate regression at each side of the cutoff point of zero. We use a triangular kernel that weights observations close to the cutoff more heavily. The estimated effect, $\hat{\tau}_{\text{RD}}$, is the difference between the predicted values of the regression approaching the cutoff from the right and from the left. For a given outcome variable and polynomial order, we choose the bandwidth that minimizes the (asymptotic) mean squared error of the estimates. Since we include exactly two observations for every election and their outcome and *first round margin* values are polar opposites, we cluster the standard errors at the election level.

# Results

**Overall effects.** We begin by showing the raw data. The mimicking-variance quantile-spaced RD plots in Figure 2 show how a candidate's margin of victory in the first round relates to her probability of winning the election. The $\hat{\tau}$ values show the difference, at the exact point of the discontinuity, between two third-order polynomials estimated separately at each side of the threshold, using all the data and weighting all observations equally. There is a sharp discontinuity in the probability of victory when *first round margin* equals zero. However, the positive 8.5 pp. result for the full sample in the top left plot represents a mixture of negative and positive effects across different samples: presidential and gubernatorial candidates who win a plurality in the first round are 13 to 18 pp. *less* likely to win the race than the runner-up, but for

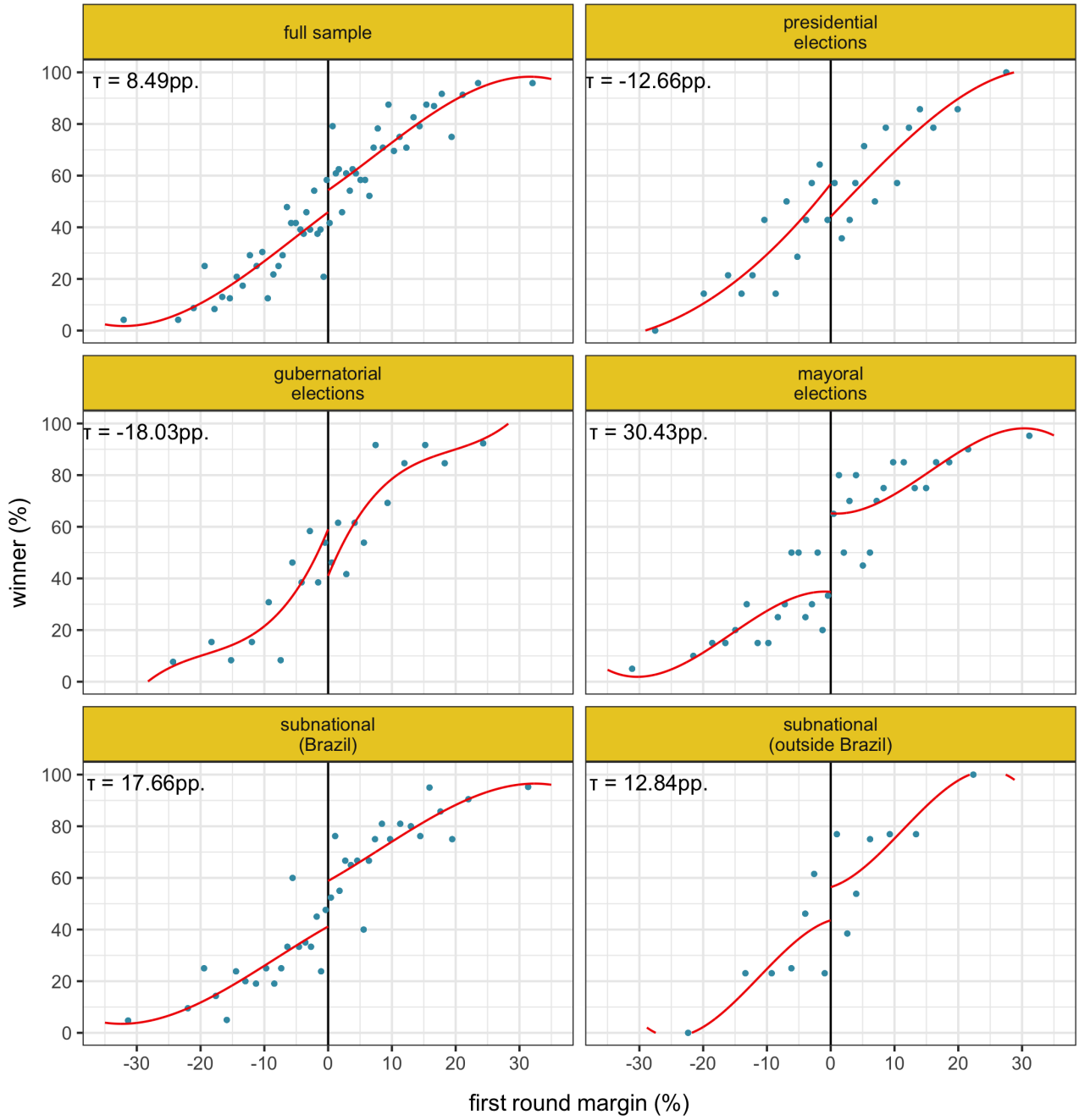



Figure 2: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the probability of winning the election. Red lines show third-order polynomials estimated separately at each side of the cutoff, using a uniform kernel.

mayoral candidates in Brazil and Mexico the effect is positive and much larger –around 30 pp. When gubernatorial and mayoral elections are taken together, the effect is positive but smaller both in Brazil and elsewhere, probably reflecting the contribution of mayoral elections. Figure A11 in the Appendix

Table 2: RD estimates of first-round advantage on second-round outcomes

| | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | 11.10 | [-2.65:27.87] | 0.11 | 10.57 | 396\|396 | 48.31 | 1.00 | 0.99 | 0.52 |
| presidential | -12.60 | [-47.70:22.62] | 0.48 | 11.94 | 116\|116 | 49.61 | 0.97 | 0.50 | 0.17 |
| gubernatorial | -12.93 | [-45.36:14.67] | 0.32 | 9.84 | 84\|84 | 49.38 | 1.00 | 0.63 | 0.22 |
| mayoral | 39.88 | [21.78:65.40] | 0.00 | 8.90 | 182\|182 | 47.71 | 1.00 | 0.86 | 1.00 |
| subnational (Brazil) | 20.96 | [2.07:44.04] | 0.03 | 11.00 | 233\|233 | 47.44 | 1.00 | 0.88 | 0.79 |
| subnational (¬ Brazil) | 17.76 | [-25.60:67.29] | 0.38 | 9.44 | 57\|57 | 49.11 | 0.83 | 0.31 | 0.19 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | |
| full sample | 1.62 | [-0.31:4.11] | 0.09 | 9.18 | 357\|358 | 7.48 | 1.00 | 1.00 | 0.53 |
| presidential | 0.11 | [-5.57:5.68] | 0.98 | 11.07 | 108\|109 | 9.15 | 0.99 | 0.62 | 0.05 |
| gubernatorial | -1.70 | [-6.23:3.07] | 0.51 | 7.98 | 74\|74 | 7.27 | 0.99 | 0.58 | 0.17 |
| mayoral | 4.73 | [2.29:8.32] | 0.00 | 7.68 | 160\|160 | 6.45 | 1.00 | 0.84 | 0.99 |
| subnational (Brazil) | 2.16 | [-0.24:5.53] | 0.07 | 8.48 | 194\|194 | 6.95 | 1.00 | 0.92 | 0.55 |
| subnational (¬ Brazil) | 3.58 | [-0.98:9.76] | 0.11 | 6.90 | 50\|50 | 6.34 | 0.91 | 0.37 | 0.45 |

Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). Observations are clustered by election.

shows similar effects on candidates' vote percentages in the second round, though the magnitudes are predictably smaller. That said, when two candidates are otherwise equivalent (on average), even a small increase in vote share can make a reasonable difference on the probability of winning.[9] Finally, in Table A3 we report the difference of means between the treatment and control groups around the 2 pp. and 5 pp. bandwidths. While these cannot be interpreted as causal estimates because the value of the outcome is increasing on *first round margin*, these numbers are generally consistent with Figures 2 and A11; the main difference is that the negative impact on presidential and gubernatorial elections is not always apparent.

Going to the actual RD estimates, Table 2 tells essentially the same story: at the point of the discontinuity, candidates who finish first in the first round experience a 11 pp. increase in their probability of winning the election overall, though this estimate falls just short of the 0.1 significance level. This is twice

[9] Nonetheless, it is theoretically possible to observe a positive coefficient on the probability of winning the election and a negative one on the second round vote share (or vice versa). This can happen if first-round winners who win the second round do so by small margins, while runner-ups who triumph do so by large margins: the former will win many elections by a small difference, while the latter will win fewer ones, but will receive more votes on average. This can explain why in the presidential election and the subnational Brazilian sample the signs get switched between Figures 2 and A11.

as large as the effects reported (for legislative elections) by Granzier, Pons and Tricaud (2019): 5.8 pp. in France and 7.6 pp. elsewhere. As in Figure 2, however, the positive full sample effect is mainly driven by municipal elections in Brazil and Mexico. In presidential and gubernatorial contests, the estimates are negative and large (−13 pp.), but with wide confidence intervals. But in mayoral elections the effect is a massive 39.9 pp. increase, which probably explains the 21 pp. effect in subnational elections in Brazil as a whole –both of which are significant at the 0.05 level. Finally, in subnational elections outside Brazil, the 17.8 pp. effect is insignificant at conventional levels –unsurprisingly given the small sample size. These findings are consistent with Figure 2 as well as with the fact that second-round reversions are more common in presidential and gubernatorial elections (31.9% and 29.3%, respectively) than in mayoral or subnational elections in Brazil (25.3% and 26.3%, respectively; see Table 1).

The second-round vote percentages reported in Table 2b tell a consistent story: the 1.6 pp. increase for the full sample is again driven by mayoral contests, where there is a larger and significant 4.7 pp. increase for the first-placed candidate. Subnational elections in Brazil and elsewhere show a positive (2.2 and 3.6 pp.) advantage, though only the former is statistically significant at the 0.10 level. In presidential elections the effect is positive but very close to zero (see fn. 9), while in gubernatorial elections it is again negative, though in both cases the estimates are far from significant. Comparing these estimates to Granzier, Pons and Tricaud's (2019) is trickier because French candidates often retire from the second round, and thus their vote shares cannot be observed. Nonetheless, their bounded analysis shows that, conditional on staying in the race, finishing first increases a candidate's vote share by between 1.3 and 4.0 pp.

To what extent does the lack of statistical power explain the insignificance of these estimates? While this represents an issue in some samples, a couple of observations are in order. First, in all samples we have at least 80% power –and usually 100%– to detect an effect as large as a standard deviation of the outcome in the control group –arguably a large effect. Second, when we find statistically significant effects –in the mayoral and subnational Brazilian samples– it is typically because the estimates themselves are large. The full sample is the largest, yet its estimates are only significant at the 0.11 level because the estimated

effect is smaller in absolute value, comprising a mixture of positive effect in some subsamples and negative estimates in others. Finally, Table A6 reports local randomization estimates for the subset of observations whose covariates are balanced around the threshold. The effect for both the mayoral and Brazilian samples are similar to those reported in Table 2, though substantially larger in magnitude, and always statistically significant. The estimates for the full sample and the gubernatorial sample get reversed in sign, though both are far from being statistically significant. We do not report estimates for the presidential and non-Brazilian samples because there were less than 10 observations at each side of the threshold.

These results are robust to a wide variety of specifications (see Appendix A4). Figure A16 shows that the results remain quite similar across a range of alternative bandwidths. Excluding elections with missing data on ideology produces somewhat stronger effects if we employ the Wikipedia scores calculated by Herrmann and Döring (forthcoming) –for which we have more observations– than if we use V-Party –substantially reducing sample sizes. However, the results are roughly similar (see Table A7). Including two observations per election guarantees that all election-level covariates –such as district characteristics, election year dynamics, runoff thresholds, first-round vote shares for all candidates, and the timing between the first and second rounds– are implicitly controlled for.[10] Nonetheless, candidates' characteristics may differ. Table A8 shows that controlling for their ideology and incumbency status does not change the results, though. Nor does employing a CER-optimal rather than a MSERD-optimal bandwidth (Table A9; see de Magalhães et al. 2020), or using a quadratic rather than a linear polynomial (Table A10).

A potentially bigger concern is that candidates' outcomes are not independent: if one wins, the other must lose, and vice versa. Similarly, their combined second-round vote percentages must add up to 100. To avoid this, RD studies typically report results for a reference party whose identity is determined beforehand. Since we lack a reference party common to all elections, for each election in our sample, we randomly coded either the first-round winner or the runner-up as the reference party and estimated the corresponding RD effect. We repeated this process 500 times. Figure A17 shows that the resulting estimates and 95% confidence intervals differ little from those reported in Table 2.

[10] For the same reason, the density of our running variable is balanced at the discontinuity by construction.

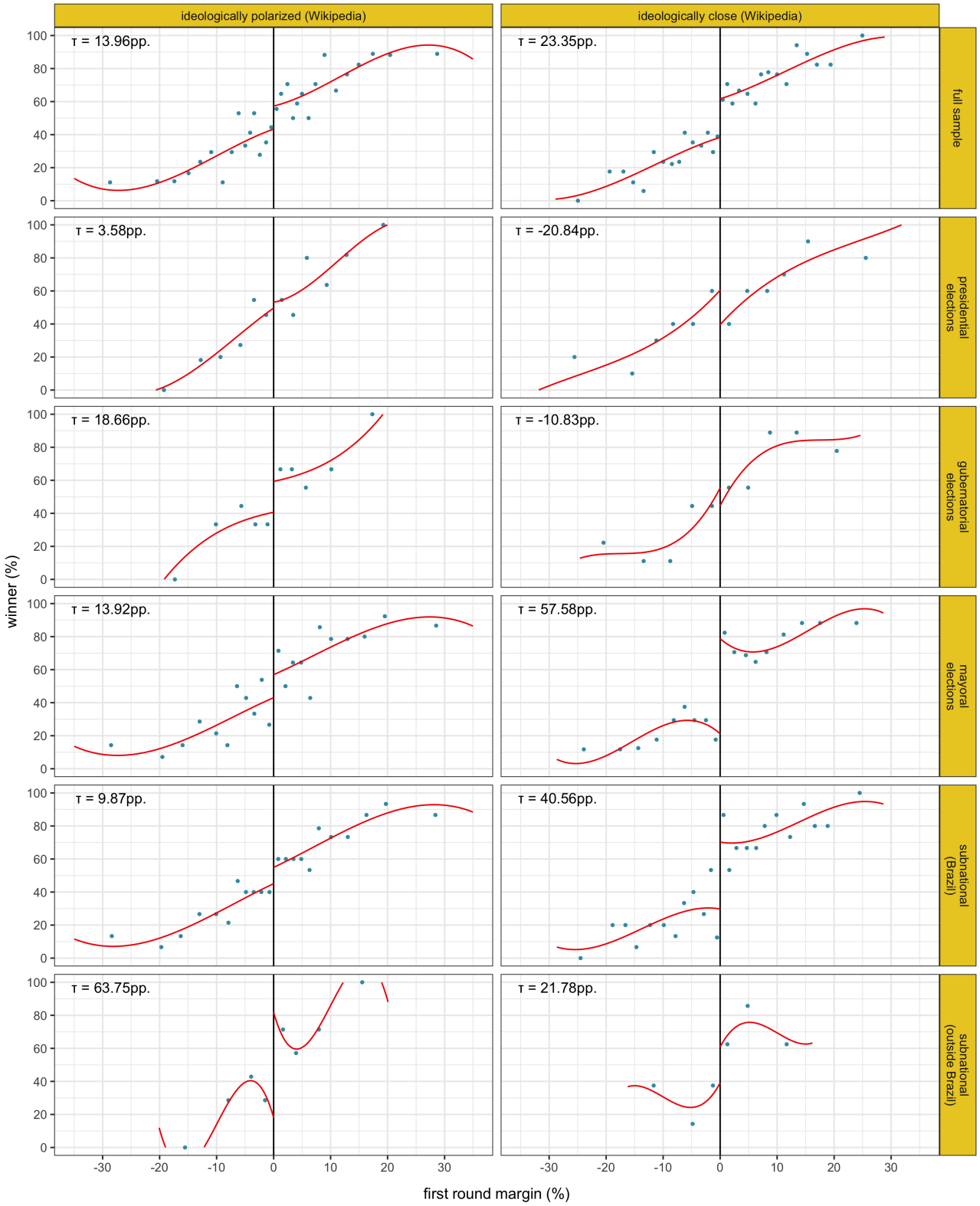



Figure 3: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the probability of winning the election. Polarized (respectively, close) elections are those in which the absolute ideological distance along the Wikipedia Left-Right dimension between the top-two vote getters in the first round was larger (smaller) than the median for each sample.

**Effect heterogeneity.** What explains this first-placement advantage? We build on the intuition that candidates' placements in the first round constitute just one piece of information voters consider when deciding whom to support in the runoff. As a decision rule, opting for the first-placed candidate is simple and does not require exerting much effort. But these advantages can be overturned if the stakes are sufficiently high to outweigh the costs of collecting more information (or paying the cost of turning out to vote; see Muñoz and Meguid 2021), or if such information is cheaply available anyway.

Such conditions are more likely to be present in three instances. First, some elections attract more attention and media coverage than others. When candidates' political and personal stances are discussed in the media over the course of weeks, voters should much more likely to down-weight candidates' placements in the first round and emphasize features like ideology, personal history, or policy stances. This may explain why presidential elections –which attract the most attention–, as well as gubernatorial contests, are different from mayoral races –about which voters tend to be the least informed.

Voters may also be more informed when they already know the candidates in question, e.g. if one of them is the incumbent or has previous experience in other positions. In this regard, it is telling that our sample contains mostly open seat races: either the incumbent is term limited, or reelection-seeking incumbents are more likely to win outright in the first round. In any case, Table A11 shows that restricting the sample to open seat races produces almost the same results as Table 2. For the Brazilian samples, we have further data on candidates' previous experience as presidents, senators, governors or (for mayoral candidates only) federal deputies or mayors. Table A12 shows that the effects of interest are much stronger than in Table 2 when neither candidate is experienced or both are –i.e., neither enjoys an "experience (dis)advantage" over the other. When a single candidate is experienced, in contrast, the estimated effect is generally negative, and not necessarily in a way that benefits the most experienced candidate. Rather, it appears that when the most experienced candidate gets a plurality in the first round, (s)he is more disadvantaged than if (s)he ends in the second place. This suggests that voters penalize candidates they already know (and do not like), though the sample sizes are too small to draw any strong conclusions.

Alternatively, voters may pay more attention when there are obvious political differences between candidates. When an election is ideologically polarized, prioritizing candidates' rankings above all else can be costly. Importantly, this may happen even with no deliberateness on the part of voters: the fact that an election is a "high stakes" may be entirely communicated to them by political elites (Cox 1997). This mechanism should operate through voters who did not vote for the top two candidates in the first round. These voters should have few incentives to pay attention in an election between candidates who initially were not their first choice. In a non-polarized runoff with hard-to-difference candidates, they would find a valuable source of information in the first-round ranking. But in a polarized second round, candidates' ideologies should receive more weight, sending first-round rankings to the background. For example, in the 2014 presidential election in Colombia, the conservative Óscar Iván Zuluaga led the incumbent Juan Manuel Santos by 31.1% to 27.4% of the first-round vote. For the second round, Zuluaga received the endorsement of the right-winger Marta Lucía Ramírez, who had finished third, while Santos got the support of Clara López Obregón and Enrique Peñalosa, who had placed fourth and fifth respectively and were located on the left of the ideological spectrum (Pachón 2014). In the second round, Santos reversed the first-round outcome and defeated Zuluaga with a vote share the sum of his, López Obregón's and Peñalosa's in the first round, while Zuluaga's vote was the sum of his and Ramírez's.

To evaluate this possibility more systematically, we split our samples in two, depending on the ideological distance between the top-two placed candidates (as measured by their Wikipedia scores) in the first round. Specifically, we coded an election as "polarized" if the absolute value of the Left-Right ideological distance between the first- and second-placed candidate was larger than the sample median;[11] otherwise, the election is (ideologically) "close." Figure 3 shows the corresponding RD plots with *winner* as the dependent variable. For the full sample, the effect of finishing first is much larger in close (23 pp.) than in polarized elections (14 pp.). The difference is much starker in the mayoral and the Brazilian samples, where an effect of 14/10 percentage points in polarized elections translates into a massive 58/41 pp. advantage if the top two candidates are ideologically similar. The results for presidential and subnational

[11] We calculated the median ideological distance separately for each sample.

elections outside Brazil go counter to expectations, but given the small sample sizes involved, it remains to be seen whether such effect sizes are statistically significant. Figure A12 shows a similar –but less stark– story for second round vote shares: despite the effects going from 2.8 pp to 1 pp in the entire sample, we see a large effect in mayoral elections (0.0 to 6.8 pp.) as well as in subnational elections in Brazil (1.0 pp to 2.8 pp) and outside (2.4 pp to 10.4 pp.). Figures A10 and A13 show that measuring polarization with V-Party data produces results consistent with these.

We report the RD estimates in Table 3. Despite the much smaller sample sizes –we first lose up to 20-30% of observations to missing values (Figure A3a), and then we split the samples in two–, we observe a large difference in point estimates between polarized and close elections in the full sample: from 19 to 26 pp., the second of which is statistically significant at the 0.01 level. Again, the effect is driven by the mayoral and subnational Brazilian samples, where an insignificant effect in polarized elections becomes a massive 61/50 pp. increase in contests featuring two ideologically close contenders. Of course, we have much less power than before, but the size of the effects is so large that all these estimates are significant at the 0.01 level. The fact that such effects are found in a setting in which parties are generally considered weak and non-ideological is remarkable, though survey data from presidential elections confirms that (some) Brazilian voters do understand the logic of strategic voting (Plutowski, Weitz-Shapiro and Winters 2021). Again, presidential and gubernatorial elections run contrary to expectations, but in neither case is the effect statistically significant.

Table 3b shows consistent results for vote percentages in the second round, though the estimates for the full sample get reversed. Again, these results are consistent with other pieces of evidence. Figure A9 shows that reversions tend to cluster around elections that are far from the 45-degree line, meaning that the first- and second-placed candidates are far apart ideologically. Tables A13 through A15 show that measuring ideological distance using V-Party results in similar, though quite underpowered, estimates.

Table 3: Heterogeneous effects: Left-Right ideological distance (Wikipedia)

| | | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | id. distance | estim. | 95% CI | *p*-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | polarized | 18.99 | [-6.01:49.94] | 0.12 | 9.82 | 155\|155 | 48.19 | 1.00 | 0.67 | 0.47 |
| | close | 26.05 | [7.42:49.03] | 0.01 | 9.49 | 141\|141 | 47.31 | 1.00 | 0.88 | 0.93 |
| presidential | polarized | -3.71 | [-59.67:50.56] | 0.87 | 10.30 | 40\|40 | 48.30 | 0.68 | 0.23 | 0.05 |
| | close | -11.57 | [-62.43:47.91] | 0.80 | 11.18 | 37\|37 | 50.52 | 0.72 | 0.25 | 0.09 |
| gubernatorial | polarized | 7.14 | [-65.34:65.21] | 1.00 | 6.63 | 26\|26 | 49.61 | 0.56 | 0.18 | 0.06 |
| | close | 4.95 | [-62.28:80.13] | 0.81 | 8.29 | 22\|22 | 49.24 | 0.48 | 0.16 | 0.05 |
| mayoral | polarized | 18.93 | [-13.02:56.75] | 0.22 | 10.39 | 93\|93 | 48.11 | 0.97 | 0.48 | 0.32 |
| | close | 60.87 | [34.73:98.26] | 0.00 | 8.59 | 80\|80 | 45.55 | 0.98 | 0.51 | 1.00 |
| subnational (Brazil) | polarized | 10.81 | [-18.85:40.42] | 0.48 | 10.74 | 102\|102 | 48.32 | 0.99 | 0.62 | 0.17 |
| | close | 50.26 | [28.50:82.09] | 0.00 | 8.11 | 87\|87 | 46.04 | 1.00 | 0.66 | 1.00 |
| subnational (¬ Brazil) | polarized | 54.68 | [-38.69:168.28] | 0.22 | 8.11 | 19\|19 | 49.56 | 0.26 | 0.10 | 0.31 |
| | close | 30.82 | [-72.66:130.66] | 0.58 | 6.46 | 15\|15 | 45.77 | 0.24 | 0.10 | 0.13 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | | |
| full sample | polarized | 4.09 | [1.93:7.07] | 0.00 | 7.00 | 125\|125 | 7.26 | 1.00 | 0.97 | 0.99 |
| | close | 1.60 | [-1.34:5.41] | 0.24 | 9.02 | 138\|138 | 6.55 | 1.00 | 0.77 | 0.26 |
| presidential | polarized | 0.32 | [-3.68:2.92] | 0.82 | 6.91 | 31\|31 | 8.69 | 1.00 | 0.95 | 0.06 |
| | close | -1.43 | [-8.85:7.33] | 0.85 | 10.77 | 37\|37 | 8.50 | 0.83 | 0.31 | 0.08 |
| gubernatorial | polarized | 3.98 | [-2.15:9.69] | 0.21 | 6.31 | 26\|26 | 6.04 | 0.81 | 0.29 | 0.46 |
| | close | -3.25 | [-9.52:5.52] | 0.60 | 8.71 | 22\|22 | 6.40 | 0.66 | 0.22 | 0.22 |
| mayoral | polarized | 1.61 | [-2.54:6.96] | 0.36 | 8.28 | 77\|77 | 7.05 | 0.98 | 0.54 | 0.16 |
| | close | 8.40 | [4.61:14.30] | 0.00 | 7.50 | 72\|72 | 5.59 | 0.89 | 0.36 | 1.00 |
| subnational (Brazil) | polarized | 3.26 | [0.18:7.67] | 0.04 | 7.23 | 76\|76 | 7.21 | 1.00 | 0.76 | 0.67 |
| | close | 3.66 | [0.06:8.78] | 0.05 | 8.69 | 90\|90 | 6.46 | 0.98 | 0.54 | 0.64 |
| subnational (¬ Brazil) | polarized | 0.67 | [-11.96:11.83] | 0.99 | 7.37 | 18\|18 | 5.88 | 0.28 | 0.11 | 0.05 |
| | close | 11.35 | [2.49:23.00] | 0.01 | 5.76 | 14\|14 | 4.81 | 0.25 | 0.10 | 0.87 |

Sharp (conventional) RD estimates, with robust CIs and *p*-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Polarized (respectively, close) elections are those in which the absolute ideological distance along the Wikipedia Left-Right dimension between the top-two vote getters in the first round was larger (smaller) than the median for each sample. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample size.

# Conclusion

We examined whether executive candidates who finish first in the first round are more likely to win the election overall. Our findings suggest a bandwagon effect in subnational races: going to a second round as the first-round winner increases the probability of emerging victorious by 40 and 21 pp. in mayoral elections in Brazil and Mexico, and subnational elections in Brazil, respectively. The results for vote shares go in a similar direction. In contrast, in presidential and gubernatorial elections, the effect of capturing the pole position in the first round is negative, though far from statistically significant. These results are consistent with those of Granzier, Pons and Tricaud (2019), and extend our knowledge of the effect of rankings on voters outside of legislative elections (mostly) in Europe to executive contests in Latin America. Additionally, our results are compelling in light of previous research on the null personal incumbency advantage enjoyed by Brazilian mayors (de Magalhães 2015): since first-placed candidates are advantaged between rounds, our findings imply that this lack of advantage four years later is probably the result of incumbents' actions while in office.

However, in contexts where voters can easily differentiate candidates –as in presidential elections, where they have abundant information– or polarized races –where they have an incentive to distinguish between candidates' policy stances– such bandwagoning logic weakens considerably. The fact that our sample comprises mostly open races, and results for Brazil are stronger when neither candidate is more experienced than the other reinforce this interpretation. Thus, our results are consistent with the claim that voters' attention to politics is limited and based on cues (Downs 1997), though it can be increased in specific circumstances, e.g.. as elections near (Le Pennec and Pons forthcoming; Marshall 2022), or when the stakes are high (Muñoz and Meguid 2021). Part of this effect is also mediated by elites' strategic behavior (Cox 1997), be it in the form of strategic withdrawals (Granzier, Pons and Tricaud 2019), increasing media coverage (Boas and Hidalgo 2011) or donors' strategic decisions, though Gulzar, Robinson and Ruiz (2022) and Granzier, Pons and Tricaud (2019) report conflicting findings in this regard.

Lastly, this study has implications regarding electoral system design and implementation. If voters take advantage of the period between rounds to gather information and make more informed choices only in high-information environments (e.g., national elections), runoff systems would be a suboptimal design where information is scarcer (i.e., in local contexts). This is especially relevant insofar as runoff elections are more expensive in terms of logistics, mobilization, and campaign contributions (Bouton et al. 2022), and may hinder governability by creating a more fragmented legislature (Pérez-Liñán 2006, though Fujiwara 2011 reports conflicting results in this regard). Yet these considerations must be weighted against the assurance that runoff elections offer against extremist candidates (Bordignon, Nannicini and Tabellini 2016), though other authors claim that these are quite modest (Bouton et al. 2022).

# References


Ames, Barry. 2001. *The Deadlock of Democracy in Brazil*. University of Michigan Press.

Anagol, Santosh and Thomas Fujiwara. 2016. "The Runner-Up Effect." *Journal of Political Economy* 124(4):927–991.

Boas, Taylor C. and F. Daniel Hidalgo. 2011. "Controlling the Airwaves: Incumbency Advantage and Community Radio in Brazil." *American Journal of Political Science* 55(4):869–885.

Bordignon, Massimo, Tommaso Nannicini and Guido Tabellini. 2016. "Moderating Political Extremism: Single Round versus Runoff Elections under Plurality Rule." *American Economic Review* 106(8):2349–2370.

Bouton, Laurent. 2013. "A Theory of Strategic Voting in Runoff Elections." *American Economic Review* 103(4):1248–1288.

Bouton, Laurent, Jorge Gallego, Aniol Llorente-Saguer and Rebecca Morton. 2022. "Run-off Elections in the Laboratory." *The Economic Journal* 132(641):106–146.

Calonico, Sebastian, Matías D. Cattaneo and Rocío Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82(6):2295–2326.

Calonico, Sebastian, Matías D. Cattaneo and Rocío Titiunik. 2015. "Optimal Data-Driven Regression Discontinuity Plots." *Journal of the American Statistical Association* 110(512):1753–1769.

Chatterjee, Somdeep and Jai Kamal. 2021. "Voting for the Underdog or Jumping on the Bandwagon? Evidence from India's Exit Poll Ban." *Public Choice* 188(3):431–453.

Chun, Jinseok S. and Richard P. Larrick. 2022. "The Power of Rank Information." *Journal of Personality and Social Psychology* 122(6):983–1003.

Coppedge, Michael, John Gerring, Carl Henrik Knutsen, Staffan I. Lindberg, Jan Teorell, Nazifa Alizada, David Altman, Michael Bernhard, Agnes Cornell, M. Steven Fish et al. 2021. "V-Dem Dataset v11.1.".
**URL:** https://www.v-dem.net/vdemds.html

Cox, Gary W. 1997. *Making Votes Count. Strategic Coordination in the World's Electoral Systems*. Cambridge University Press.

de Magalhães, Leandro M. 2015. "Incumbency Effects in a Comparative Perspective: Evidence from Brazilian Mayoral Elections." *Political Analysis* 23(1):113–126.

de Magalhães, Leandro M., Dominik Hangartner, Salomo Hirvonen, Jaakko Meriläinen, Nelson A. Ruiz and Janne Tukiainen. 2020. "How Much Should We Trust Regression Discontinuity Design Estimates? Evidence from Experimental Benchmarks of the Incumbency Advantage." Unpublished manuscript.

Downs, Anthony. 1997. *An Economic Theory of Democracy*. Harper and Brothers.

Fujiwara, Thomas. 2011. "A Regression Discontinuity Test of Strategic Voting and Duverger's Law." *Quarterly Journal of Political Science* 6(3-4):197–233.

Gervasoni, Carlos. 2010. "A Rentier Theory of Subnational Regimes: Fiscal Federalism, Democracy, and Authoritarianism in the Argentine Provinces." *World Politics* 62(3):302–340.

Granzier, Riako, Vincent Pons and Clémence Tricaud. 2019. The Large Effects of a Small Win: How Past Rankings Shape the Behavior of Voters and Candidates. Working Paper 26599 NBER.

Gulzar, Saad, Thomas S. Robinson and Nelson A. Ruiz. 2022. "How Campaigns Respond to Ballot Position: A New Mechanism for Order Effects." *The Journal of Politics* 84(2):1256–1261.

Herrmann, Michael and Holger Döring. forthcoming. "Party Positions from Wikipedia Classifications of Party Ideology." *Political Analysis* .

Hix, Simon, Rafael Hortala-Vallve and Guillem Riambau-Armet. 2017. "The Effects of District Magnitude on Voting Behavior." *The Journal of Politics* 79(1):356–361.

Hyde, Susan D. and Nikolay Marinov. 2012. "Which Elections Can Be Lost?" *Political Analysis* 20(2):191–210.

Kiss, Áron and Gábor Simonovits. 2014. "Identifying the Bandwagon Effect in Two-round Elections." *Public Choice* 160(3/4):327–344.

Klašnja, Marko and Rocío Titiunik. 2017. "The Incumbency Curse: Weak Parties, Term Limits, and Unfulfilled Accountability." *American Political Science Review* 111(1):129–148.

Le Pennec, Caroline and Vincent Pons. forthcoming. "How Do Campaigns Shape Vote Choice? Multi-Country Evidence from 62 Elections and 56 TV Debates." *Quarterly Journal of Economics* .

Lührmann, Anna, Nils Düpont, Masaaki Higashijima, Yaman Berker Kavasogly, Kyle L. Marquardt, Michael Bernhard, Holger Döring, Allen Hicken, Melis Laebens et al. 2020. "Varieties of Party Identity and Organization (V-Party) Dataset v.1.". **URL:** https://www.v-dem.net/vpartyds.html

Marshall, John. 2022. "Tuning In, Voting Out: News Consumption Cycles, Homicides, and Electoral Accountability in Mexico." Unpublished manuscript.

Morton, Rebecca B., Daniel Muller, Lionel Page and Benno Torgler. 2015. "Exit Polls, Turnout, and Bandwagon Voting: Evidence from a Natural Experiment." *European Economic Review* 77:65–81.

Muñoz, Maria Murias and Bonnie M. Meguid. 2021. "Does Party Polarization Mobilize or De-Mobilize Voters? The Answer Depends on where Voters Stand." *Electoral Studies* 70.

Pachón, Mónica. 2014. "Las Elecciones de Colombia 2014: El Tortuoso Camino para Darle un Segundo Tiempo al Proceso de Paz." *Iberoamericana* XIV(55):181–187.

Pérez-Liñán, Aníbal. 2006. "Evaluating Presidential Runoff Elections." *Electoral Studies* 25(1):129–146.

Plutowski, Luke, Rebecca Weitz-Shapiro and Matthew S. Winters. 2021. "Voter Beliefs and Strategic Voting in Two-Round Elections." *Political Research Quarterly* 74(4):852–865.

Pons, Vincent and Clémence Tricaud. 2018. "Expressive Voting and Its Cost: Evidence from Runoffs with Two or Three Candidates." *Econometrica* 86(5):1621–1649.

Samuels, David J. 2003. *Ambition, Federalism, and Legislative Politics in Brazil*. Cambridge University Press.

# Online Appendix

## "Does the Early Bird always Get the Worm?"
## (for online publication only)

(1) Section A1 lists our data sources.

(2) Section A2 presents the descriptive statistics and some additional plots.

(3) Section A3 presents the balance checks and placebo tests.

(4) Section A4 reports additional results and robustness checks.

# A1 Data Sources

**Presidential elections**

- We assembled a list of presidential elections between 1945 and 2020 from the following sources: Bormann and Golder (2013), NELDA (Hyde and Marinov 2012) and V-Dem v.11.1 (Coppedge et al. 2021). Following Hyde and Marinov (2012), we restricted the sample to elections that are minimally competitive, meaning that (a) opposition was allowed; (b) there was more than a single legal party; and (c) there was a choice of candidates in the ballot.
- To identify elections employing a runoff, a couple of research assistants employed the Nohlen handbooks, Bormann and Golder (2013), the Comparative Constitutions Project v2.0 (Elkins, Ginsburg and Melton 2014) and other country-specific sources.
- Election results come from the Nohlen handbooks, *Wikipedia*, and the African Elections Database (https://africanelections.tripod.com/).

**Argentina**

- Dirección Nacional Electoral (DINE: https://www.argentina.gob.ar/interior/dine).
- *Wikipedia*.
- Tow (N.d.).

**Bolivia**

- Órgano Electoral Plurinacional (OEP: https://www.oep.org.bo/).

**Chile**

- *Wikipedia*: https://es.wikipedia.org/wiki/Elecciones_de_gobernadores_regionales_de_Chile_de_2021, and subsequent links.

**San Luis Potosí, Mexico**

- Vázquez Salguero (2013) for a description of electoral rules.
- *Consejo Estatal Electoral y de Participación Ciudadana de San Luis Potosí* (CEEPAC: http://www.ceepacslp.org.mx/ceepac/) for election outcomes.

**Ideology scores**

- Wikipedia scores: Herrmann and Döring (forthcoming). These are calculated using an ideal point model on the basis of parties' tags in their Wikipedia pages. The resulting scores map naturally along the Left-Right dimension. By construction, each party's score is constant over time. For subnational elections, we implicitly assumed parties' national ideology scores hold.
- V-Party v.1 dataset: Lührmann et al. (2020). This data is only available for legislative elections, so we used the coding from the previous congressional race when executive and legislative elections were not concurrent. For subnational elections, we implicitly assumed parties' national ideology scores hold.
- *Left-Right score.* `v2pariglef` variable from V-Party. Country experts were asked to classify parties across a seven-point scale ranging from Far-Left to Far-Right, and then a Bayesian IRT measurement model was used to calculate parties' latent ideology scores. This variable has a mean of 0 and a standard deviation of 1 (in V-Party's entire sample; our subsamples may differ).
- *(Il)Liberalism score.* This captures the extent to which a party employs anti-elite rhetoric, exalts "the people," embraces pluralism and rejects political violence. Measured with a factor score of five variables included in V-Party: `v2paanteli`, `v2papeople`, `v2paopresp`, `v2paplur` and `v2paviol`.
- *Post-Materialism score.* Indicates whether a party advocates secularism and support for women's rights, immigrants and minorities. Measured with a factor score of `v2paminor`, `v2paimmig`, `v2palgbt`, `v2parelig` and `v2pawomlab`.
- Figure A4 shows the correlation between these four scores: both Left-Right measures are highly correlated with each other, and the Left-Right dimension is negatively correlated with the other two.

# A2 Descriptive statistics

**Descriptive statistics.** Tables A1 and A2 show the descriptive statistics for the main variables of interest, disaggregated by sample. The former reports variables measured at the election level, while the latter shows the values corresponding to the first- and second-placed candidates in the first round.

**Descriptive by subsample (I): Votes.** For the subset of elections requiring a second round, Figure A1 shows the distribution of first-round vote shares for the first three placed candidates (left) and the first vs. second and second vs. third margins (right). Figure A2 plots the relationship between the vote share(s) of the first- and second-placed candidates in the first round.

**Descriptive by subsample (II): Ideology.** Figure A3 presents the proportion of elections with non-missing data on the ideology scores of the first and second-placed candidate(s) in the first round. For both the first- and second-placed candidates in the first round, Figure A4 presents the correlation between their ideology scores. For the subset of elections in which a second round was needed, Figures A5 through A7 summarize the distribution of (i) our measures of ideological polarization –the absolute value of the difference between the ideology of the first- and second-placed candidate–; and (ii) the Wikipedia and V-Party ideology scores of the first- and second-placed candidates in the first round. Finally, Figures A8 and A9 plot the relationship between the Left-Right ideology of the first- and second-placed candidates in the first round.

**Difference-in-means for 2 pp. and 5 pp. bandwidths.** For each subsample, Table A3 presents (i) the number of observations within a 2 (or 5) percentage point bandwidth; (ii) the average values of the outcome variables for the first- and second-placed candidates in the first round; and (iii) the difference in means between the two.

**Additional RD plots.** Figure A10 visualizes the heterogeneous effects using V-Party Left-Right scores instead of the Wikipedia ideology measure. Figures A11 through A13 present the mimicking variance evenly-spaced RD plots showing the effect of first-round advantage on $\textit{vote share}_{R2}$.

Table A1: Descriptive statistics (1): Election-level characteristics

| | (a) Full sample | | | | | (b) Presidential elections | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | *N* | mean | SD | min | max | *N* | mean | SD | min | max |
| *margin (1vs2)* (0:50) | 663 | 9.81 | 7.43 | 0.03 | 39.06 | 182 | 9.96 | 7.52 | 0.22 | 32.81 |
| *margin (2vs3)* (0:25) | 663 | 13.56 | 11.52 | 0.14 | 48.64 | 182 | 11.96 | 10.18 | 0.14 | 46.58 |
| *margin (1vs2)* (R2) (0:100) | 656 | 14.40 | 13.80 | 0.01 | 100.00 | 178 | 16.54 | 17.22 | 0.01 | 100.00 |
| *reversion in second round* (0/1) | 663 | 0.28 | 0.45 | 0.00 | 1.00 | 182 | 0.32 | 0.47 | 0.00 | 1.00 |
| *number of candidates* (#) | 663 | 8.24 | 4.46 | 3.00 | 39.00 | 182 | 11.21 | 6.36 | 3.00 | 39.00 |
| *effective number of candidates* | 663 | 3.57 | 1.01 | 2.04 | 10.43 | 182 | 3.97 | 1.27 | 2.12 | 10.43 |
| \|*ideol. distance (1vs2)*\| (Left-Right, Wikipedia) | 521 | 0.95 | 0.57 | 0.00 | 2.65 | 125 | 1.16 | 0.65 | 0.00 | 2.65 |
| \|*ideol. distance (1vs2)*\| (Left-Right, V-Party) | 453 | 1.83 | 1.10 | 0.00 | 5.14 | 123 | 1.90 | 1.13 | 0.00 | 5.14 |
| \|*ideol. distance (1vs2)*\| ((Il)Liberalism) | 453 | 0.47 | 0.44 | 0.00 | 2.61 | 123 | 0.53 | 0.64 | 0.00 | 2.61 |
| \|*ideol. distance (1vs2)*\| (Post-Materialism) | 453 | 1.11 | 0.70 | 0.00 | 4.45 | 123 | 0.96 | 0.79 | 0.00 | 4.45 |
| *incumbent first-placed* (0/1) | 663 | 0.15 | 0.36 | 0.00 | 1.00 | 182 | 0.24 | 0.43 | 0.00 | 1.00 |
| *incumbent second-placed* (0/1) | 663 | 0.08 | 0.26 | 0.00 | 1.00 | 182 | 0.09 | 0.28 | 0.00 | 1.00 |
| *first-placed is experienced* (0/1) | 391 | 0.20 | 0.40 | 0.00 | 1.00 | | | | | |
| *second-placed is experienced* (0/1) | 391 | 0.13 | 0.34 | 0.00 | 1.00 | | | | | |
| *distance b/w first and second round* (days) | 656 | 27.43 | 16.12 | 7.00 | 224.00 | 178 | 27.49 | 23.93 | 7.00 | 224.00 |
| | (c) Gubernatorial elections | | | | | (d) Mayoral elections | | | | |
| *margin (1vs2)* (0:50) | 140 | 9.13 | 7.06 | 0.12 | 28.82 | 341 | 10.00 | 7.54 | 0.03 | 39.06 |
| *margin (2vs3)* (0:25) | 140 | 16.81 | 12.99 | 0.26 | 48.64 | 341 | 13.09 | 11.32 | 0.14 | 45.61 |
| *margin (1vs2)* (R2) (0:100) | 137 | 14.41 | 13.61 | 0.09 | 87.56 | 341 | 13.27 | 11.60 | 0.16 | 70.86 |
| *reversion in second round* (0/1) | 140 | 0.29 | 0.46 | 0.00 | 1.00 | 341 | 0.26 | 0.44 | 0.00 | 1.00 |
| *number of candidates* (#) | 140 | 6.65 | 3.14 | 3.00 | 28.00 | 341 | 7.30 | 2.56 | 3.00 | 16.00 |
| *effective number of candidates* | 140 | 3.18 | 0.73 | 2.04 | 5.71 | 341 | 3.51 | 0.87 | 2.11 | 9.25 |
| \|*ideol. distance (1vs2)*\| (Left-Right, Wikipedia) | 90 | 0.85 | 0.49 | 0.07 | 2.10 | 306 | 0.90 | 0.54 | 0.00 | 2.60 |
| \|*ideol. distance (1vs2)*\| (Left-Right, V-Party) | 93 | 1.79 | 0.98 | 0.07 | 3.92 | 237 | 1.81 | 1.13 | 0.00 | 5.00 |
| \|*ideol. distance (1vs2)*\| ((Il)Liberalism) | 93 | 0.43 | 0.36 | 0.00 | 1.80 | 237 | 0.46 | 0.32 | 0.00 | 1.80 |
| \|*ideol. distance (1vs2)*\| (Post-Materialism) | 93 | 1.06 | 0.63 | 0.04 | 2.61 | 237 | 1.21 | 0.67 | 0.04 | 4.10 |
| *incumbent first-placed* (0/1) | 140 | 0.09 | 0.29 | 0.00 | 1.00 | 341 | 0.13 | 0.34 | 0.00 | 1.00 |
| *incumbent second-placed* (0/1) | 140 | 0.07 | 0.26 | 0.00 | 1.00 | 341 | 0.07 | 0.26 | 0.00 | 1.00 |
| *first-placed is experienced* (0/1) | 91 | 0.12 | 0.33 | 0.00 | 1.00 | 300 | 0.22 | 0.42 | 0.00 | 1.00 |
| *second-placed is experienced* (0/1) | 91 | 0.08 | 0.27 | 0.00 | 1.00 | 300 | 0.15 | 0.35 | 0.00 | 1.00 |
| *distance b/w first and second round* (days) | 137 | 28.23 | 16.77 | 7.00 | 196.00 | 341 | 27.08 | 9.48 | 14.00 | 43.00 |
| | (e) Subnational (Brazil) | | | | | (f) Subnational (outside Brazil) | | | | |
| *margin (1vs2)* (0:50) | 391 | 10.12 | 7.52 | 0.10 | 39.06 | 90 | 8.11 | 6.68 | 0.03 | 28.82 |
| *margin (2vs3)* (0:25) | 391 | 13.50 | 11.59 | 0.14 | 48.64 | 90 | 17.09 | 13.01 | 0.37 | 45.61 |
| *margin (1vs2)* (R2) (0:100) | 391 | 13.39 | 11.46 | 0.09 | 70.86 | 87 | 14.52 | 15.16 | 0.24 | 87.56 |
| *reversion in second round* (0/1) | 391 | 0.26 | 0.44 | 0.00 | 1.00 | 90 | 0.29 | 0.46 | 0.00 | 1.00 |
| *number of candidates* (#) | 391 | 7.30 | 2.48 | 3.00 | 16.00 | 90 | 6.29 | 3.62 | 3.00 | 28.00 |
| *effective number of candidates* | 391 | 3.47 | 0.85 | 2.04 | 9.25 | 90 | 3.20 | 0.79 | 2.11 | 5.71 |
| \|*ideol. distance (1vs2)*\| (Left-Right, Wikipedia) | 345 | 0.89 | 0.54 | 0.00 | 2.60 | 51 | 0.86 | 0.46 | 0.32 | 1.74 |
| \|*ideol. distance (1vs2)*\| (Left-Right, V-Party) | 283 | 1.84 | 1.13 | 0.00 | 5.00 | 47 | 1.63 | 0.76 | 0.59 | 3.92 |
| \|*ideol. distance (1vs2)*\| ((Il)Liberalism) | 283 | 0.40 | 0.32 | 0.00 | 1.80 | 47 | 0.74 | 0.28 | 0.05 | 1.08 |
| \|*ideol. distance (1vs2)*\| (Post-Materialism) | 283 | 1.19 | 0.70 | 0.04 | 4.10 | 47 | 1.03 | 0.38 | 0.35 | 2.40 |
| *incumbent first-placed* (0/1) | 391 | 0.14 | 0.34 | 0.00 | 1.00 | 90 | 0.04 | 0.21 | 0.00 | 1.00 |
| *incumbent second-placed* (0/1) | 391 | 0.08 | 0.27 | 0.00 | 1.00 | 90 | 0.03 | 0.18 | 0.00 | 1.00 |
| *first-placed is experienced* (0/1) | 391 | 0.20 | 0.40 | 0.00 | 1.00 | | | | | |
| *second-placed is experienced* (0/1) | 391 | 0.13 | 0.34 | 0.00 | 1.00 | | | | | |
| *distance b/w first and second round* (days) | 391 | 25.40 | 8.22 | 14.00 | 43.00 | 87 | 36.41 | 19.87 | 7.00 | 196.00 |

Only elections in which a second round was needed to determine the winner are included. Unless specifically noted, all variables are measured in the first round or have a common value for both rounds.

Table A2: Descriptive statistics (11): First- and Second-placed candidates

| | top placed (first round) | | | | | runner-up (first round) | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (a) Full sample | *N* | mean | SD | min | max | *N* | mean | SD | min | max |
| *winner* (0/100) | 663 | 71.64 | 45.11 | 0.00 | 100.00 | 663 | 28.21 | 45.03 | 0.00 | 100.00 |
| *vote share* (0:50) | 663 | 39.71 | 7.01 | 18.40 | 49.98 | 663 | 29.90 | 7.46 | 10.60 | 49.30 |
| *vote share* (R2) (0:100) | 656 | 54.27 | 9.01 | 14.78 | 100.00 | 653 | 45.76 | 8.91 | 0.00 | 85.22 |
| *ideology* (Left-Right, Wikipedia) | 577 | -0.18 | 0.74 | -2.19 | 1.82 | 566 | -0.19 | 0.76 | -2.27 | 1.86 |
| *ideology* (Left-Right, V-Party) | 551 | 0.23 | 1.38 | -2.61 | 3.24 | 541 | 0.26 | 1.37 | -3.36 | 3.24 |
| *ideology* ((Il)Liberalism) | 551 | 0.52 | 0.64 | -2.35 | 1.28 | 541 | 0.57 | 0.63 | -2.03 | 1.28 |
| *ideology* (Post-Materialism) | 551 | 0.50 | 0.94 | -2.17 | 2.29 | 541 | 0.48 | 0.97 | -2.17 | 2.55 |
| (b) Presidential elections | | | | | | | | | | |
| *winner* (0/100) | 182 | 67.58 | 46.94 | 0.00 | 100.00 | 182 | 31.87 | 46.73 | 0.00 | 100.00 |
| *vote share* (0:50) | 182 | 37.89 | 7.62 | 18.40 | 49.88 | 182 | 27.93 | 7.05 | 13.83 | 47.92 |
| *vote share* (R2) (0:100) | 178 | 54.74 | 10.97 | 30.82 | 100.00 | 176 | 45.29 | 10.67 | 0.00 | 69.18 |
| *ideology* (Left-Right, Wikipedia) | 148 | -0.05 | 0.82 | -1.86 | 1.82 | 142 | 0.01 | 0.86 | -2.27 | 1.86 |
| *ideology* (Left-Right, V-Party) | 148 | 0.04 | 1.29 | -2.27 | 3.24 | 145 | 0.27 | 1.28 | -3.36 | 2.46 |
| *ideology* ((Il)Liberalism) | 148 | 0.11 | 0.82 | -2.35 | 1.19 | 145 | 0.14 | 0.82 | -2.03 | 1.26 |
| *ideology* (Post-Materialism) | 148 | 0.10 | 0.92 | -2.17 | 2.29 | 145 | 0.03 | 0.84 | -1.84 | 2.55 |
| (c) Gubernatorial elections | | | | | | | | | | |
| *winner* (0/100) | 140 | 70.71 | 45.67 | 0.00 | 100.00 | 140 | 29.29 | 45.67 | 0.00 | 100.00 |
| *vote share* (0:50) | 140 | 41.77 | 6.34 | 24.29 | 49.80 | 140 | 32.64 | 7.61 | 15.56 | 49.30 |
| *vote share* (R2) (0:100) | 137 | 53.62 | 9.24 | 14.78 | 93.78 | 137 | 46.38 | 9.24 | 6.22 | 85.22 |
| *ideology* (Left-Right, Wikipedia) | 103 | -0.05 | 0.59 | -1.28 | 1.32 | 104 | -0.20 | 0.69 | -1.28 | 1.32 |
| *ideology* (Left-Right, V-Party) | 114 | 0.32 | 1.34 | -2.61 | 3.24 | 115 | 0.14 | 1.40 | -2.61 | 3.24 |
| *ideology* ((Il)Liberalism) | 114 | 0.57 | 0.62 | -1.22 | 1.28 | 115 | 0.69 | 0.55 | -1.22 | 1.28 |
| *ideology* (Post-Materialism) | 114 | 0.51 | 0.78 | -2.17 | 2.29 | 115 | 0.65 | 0.94 | -2.17 | 2.29 |
| (d) Mayoral elections | | | | | | | | | | |
| *winner* (0/100) | 341 | 74.19 | 43.82 | 0.00 | 100.00 | 341 | 25.81 | 43.82 | 0.00 | 100.00 |
| *vote share* (0:50) | 341 | 39.83 | 6.70 | 18.81 | 49.98 | 341 | 29.83 | 7.28 | 10.60 | 47.78 |
| *vote share* (R2) (0:100) | 341 | 54.28 | 7.71 | 25.82 | 85.43 | 340 | 45.75 | 7.70 | 14.57 | 74.18 |
| *ideology* (Left-Right, Wikipedia) | 326 | -0.27 | 0.73 | -2.19 | 1.67 | 320 | -0.27 | 0.72 | -2.19 | 1.32 |
| *ideology* (Left-Right, V-Party) | 289 | 0.29 | 1.43 | -2.61 | 2.39 | 281 | 0.31 | 1.41 | -2.61 | 3.24 |
| *ideology* ((Il)Liberalism) | 289 | 0.70 | 0.43 | -0.28 | 1.28 | 281 | 0.74 | 0.41 | -1.22 | 1.28 |
| *ideology* (Post-Materialism) | 289 | 0.71 | 0.95 | -1.81 | 2.29 | 281 | 0.65 | 0.98 | -2.17 | 2.29 |
| (e) Subnational (Brazil) | | | | | | | | | | |
| *winner* (0/100) | 391 | 73.66 | 44.11 | 0.00 | 100.00 | 391 | 26.34 | 44.11 | 0.00 | 100.00 |
| *vote share* (0:50) | 391 | 40.21 | 6.68 | 18.81 | 49.98 | 391 | 30.09 | 7.32 | 10.60 | 49.30 |
| *vote share* (R2) (0:100) | 391 | 54.12 | 7.80 | 25.82 | 85.43 | 390 | 45.91 | 7.79 | 14.57 | 74.18 |
| *ideology* (Left-Right, Wikipedia) | 368 | -0.28 | 0.70 | -2.19 | 1.32 | 360 | -0.32 | 0.70 | -2.19 | 1.32 |
| *ideology* (Left-Right, V-Party) | 337 | 0.30 | 1.43 | -2.61 | 3.24 | 329 | 0.17 | 1.40 | -2.61 | 3.24 |
| *ideology* ((Il)Liberalism) | 337 | 0.79 | 0.35 | -1.22 | 1.28 | 329 | 0.81 | 0.38 | -1.22 | 1.28 |
| *ideology* (Post-Materialism) | 337 | 0.74 | 0.93 | -2.17 | 2.29 | 329 | 0.77 | 0.95 | -2.17 | 2.29 |
| (f) Subnational (outside Brazil) | | | | | | | | | | |
| *winner* (0/100) | 90 | 71.11 | 45.58 | 0.00 | 100.00 | 90 | 28.89 | 45.58 | 0.00 | 100.00 |
| *vote share* (0:50) | 90 | 41.19 | 6.48 | 24.29 | 49.80 | 90 | 33.08 | 7.73 | 15.56 | 47.75 |
| *vote share* (R2) (0:100) | 87 | 53.96 | 9.74 | 14.78 | 93.78 | 87 | 46.04 | 9.74 | 6.22 | 85.22 |
| *ideology* (Left-Right, Wikipedia) | 61 | 0.16 | 0.58 | -1.38 | 1.67 | 64 | 0.10 | 0.69 | -1.38 | 0.95 |
| *ideology* (Left-Right, V-Party) | 66 | 0.29 | 1.26 | -2.19 | 2.21 | 67 | 0.71 | 1.35 | -2.19 | 2.64 |
| *ideology* ((Il)Liberalism) | 66 | 0.04 | 0.60 | -1.09 | 1.15 | 67 | 0.32 | 0.55 | -1.09 | 1.15 |
| *ideology* (Post-Materialism) | 66 | 0.18 | 0.66 | -0.87 | 1.69 | 67 | 0.05 | 0.77 | -1.00 | 1.69 |

Only elections in which a second round was needed to determine the winner are included. Unless specifically noted, all variables are measured in the first round or have a common value for both rounds.

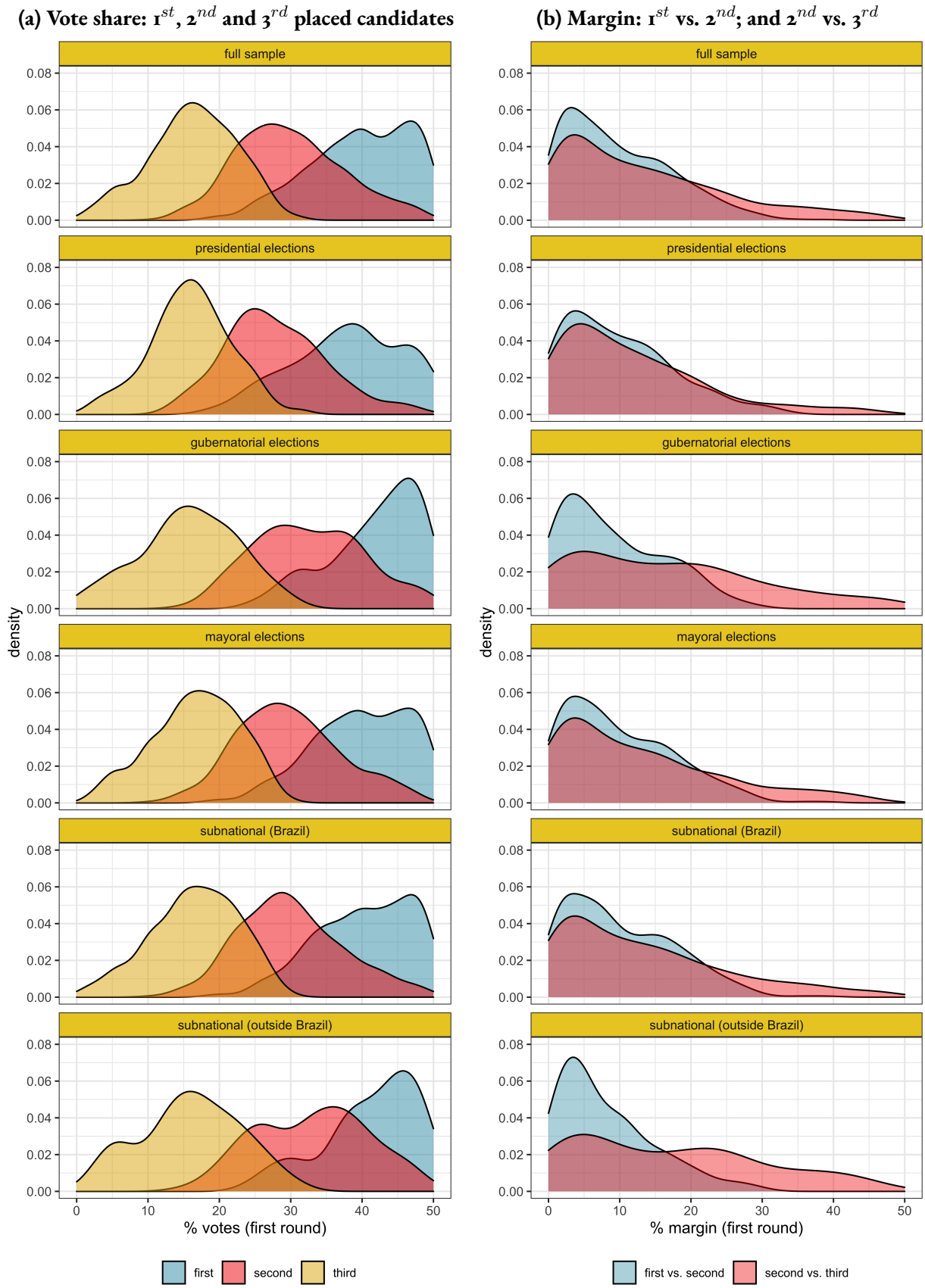



Figure A1: First-round vote shares of the first-, second- and third-placed candidates (left), as well as the first-second and second-third margins (right), by subsample. Only elections in which a second round was needed are included.

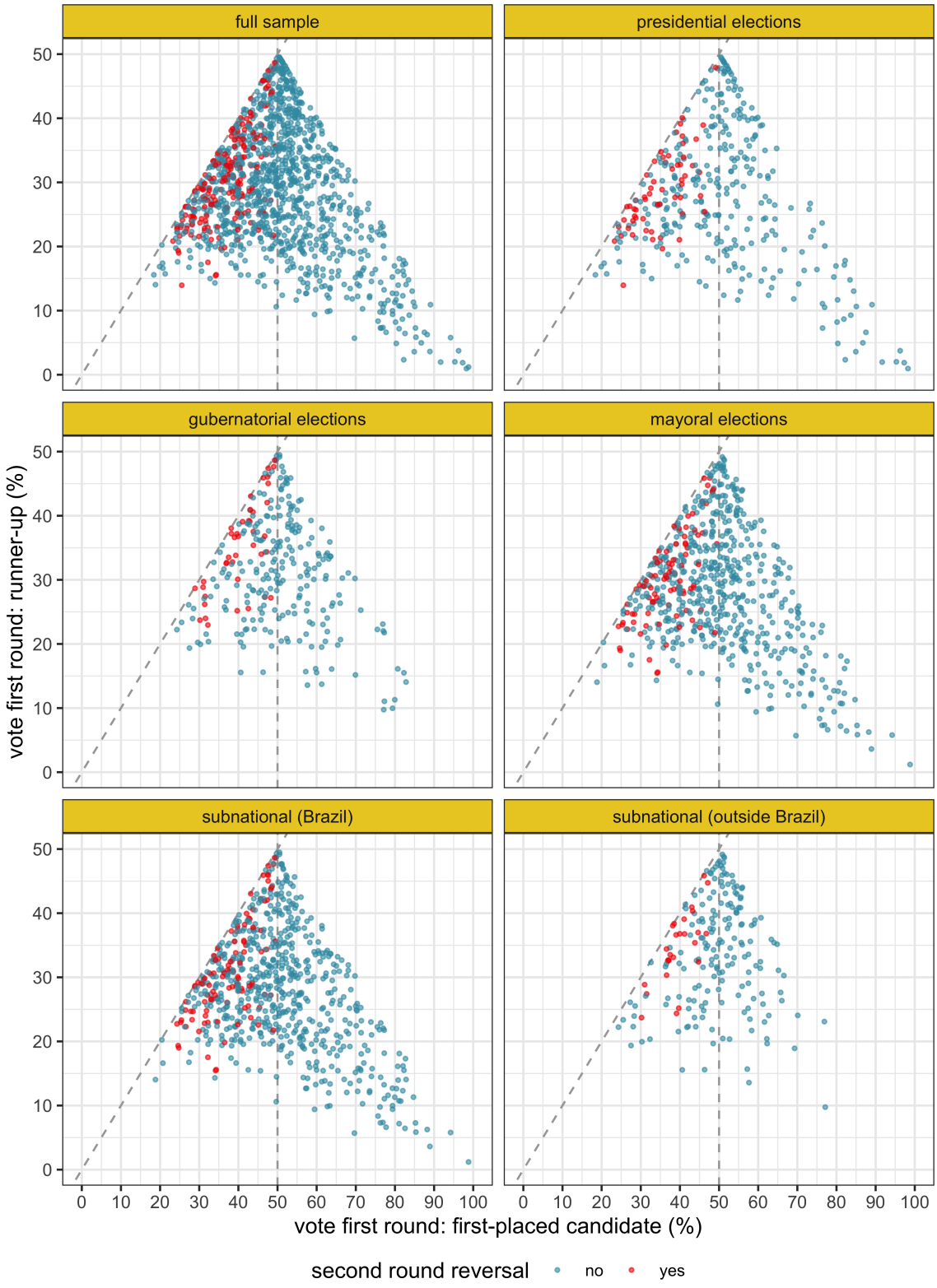



Figure A2: First-round vote shares of the first- and second-placed candidates, by subsample. Red dots indicate elections in which the first-round result was reversed in the runoff.

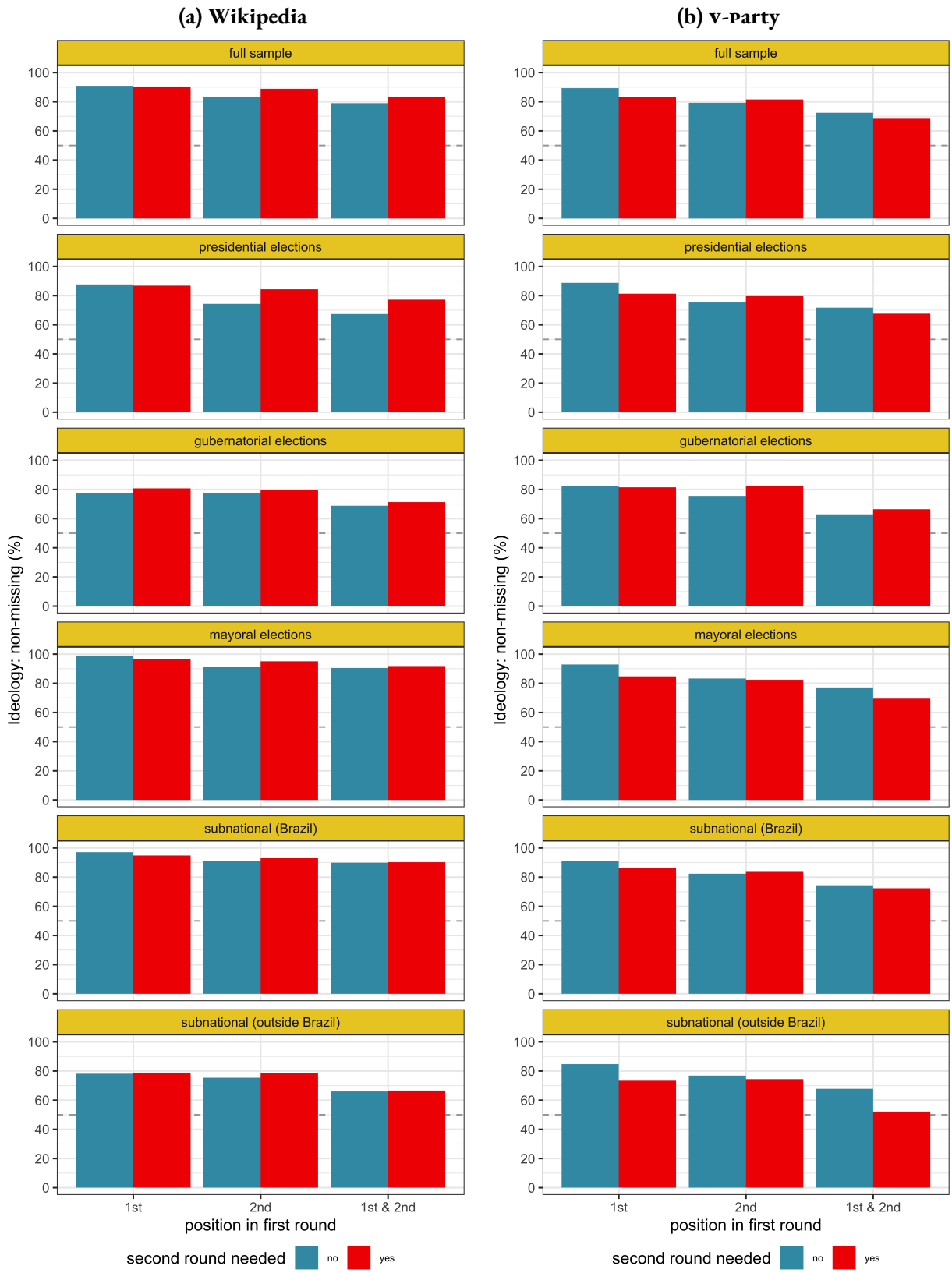



Figure A3: % of candidates with non-missing data on ideology, by source, subsample, first-round placement, and whether a second round was needed. Panel (b) reports missingness for the Left-Right v-party measure, but missingness patterns for illiberalism or post-materialism only differ for a handful of observations.

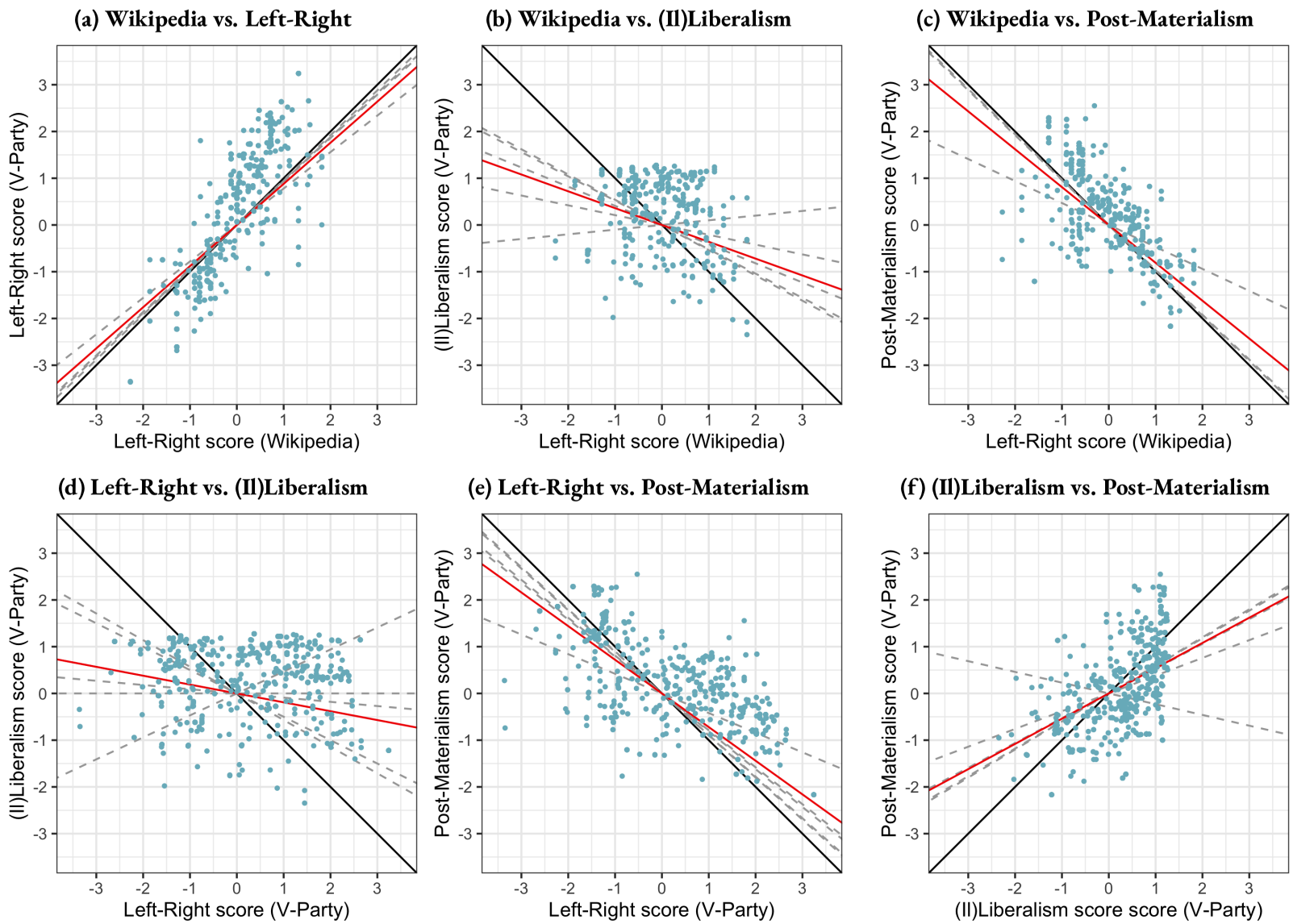



Figure A4: Pairwise relationship between ideology measures. Points represent observations in the full sample, and the red lines indicate the pairwise correlation for that sample. Broken gray lines indicate the pairwise correlation lines in each sample separately. Solid black lines indicate the perfect (negative) correlation. Only elections in which a second round was needed are included.

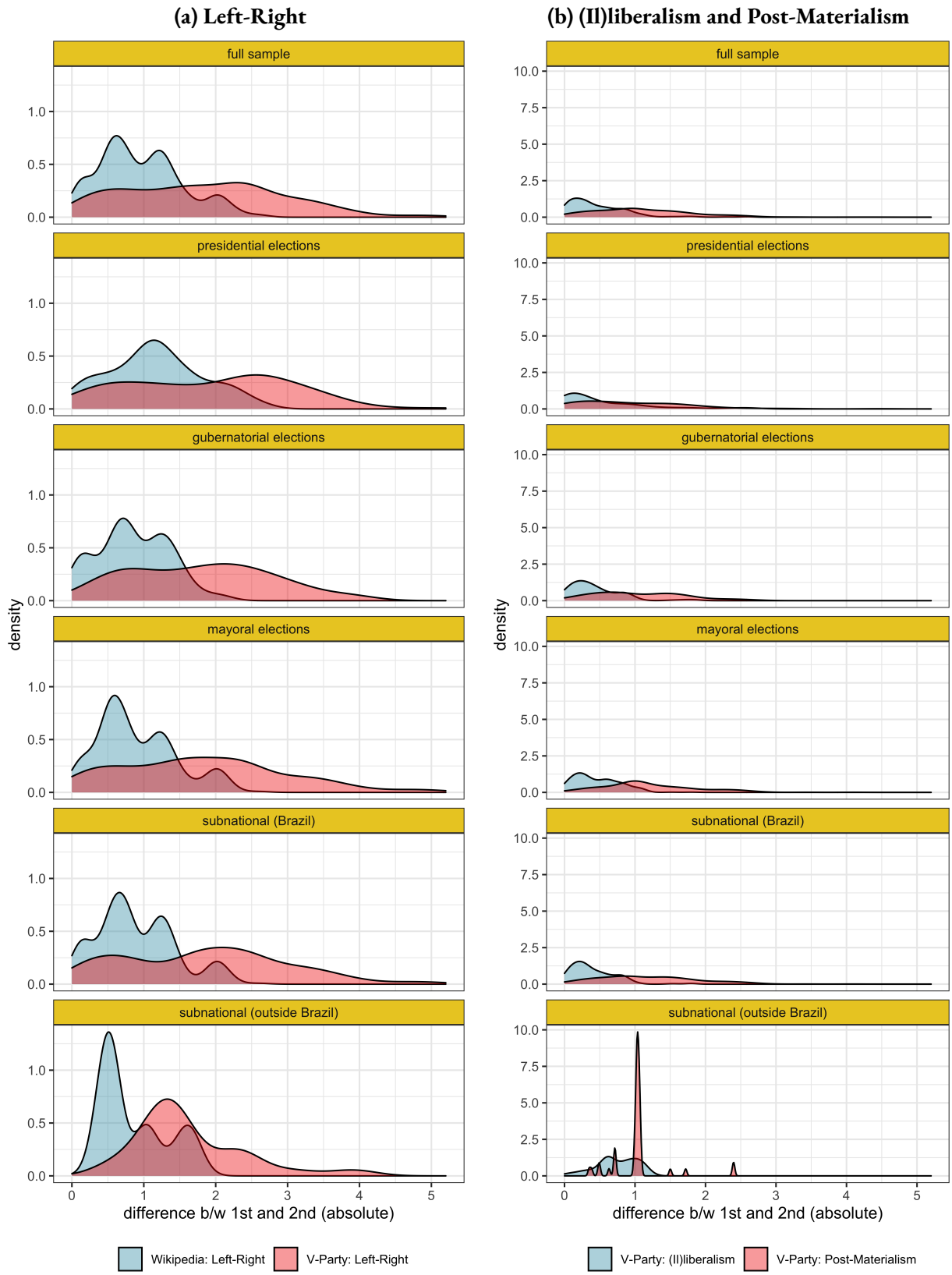



Figure A5: Ideological polarization between the first- and second-placed candidates, by subsample. Only elections in which a second round was needed are included.

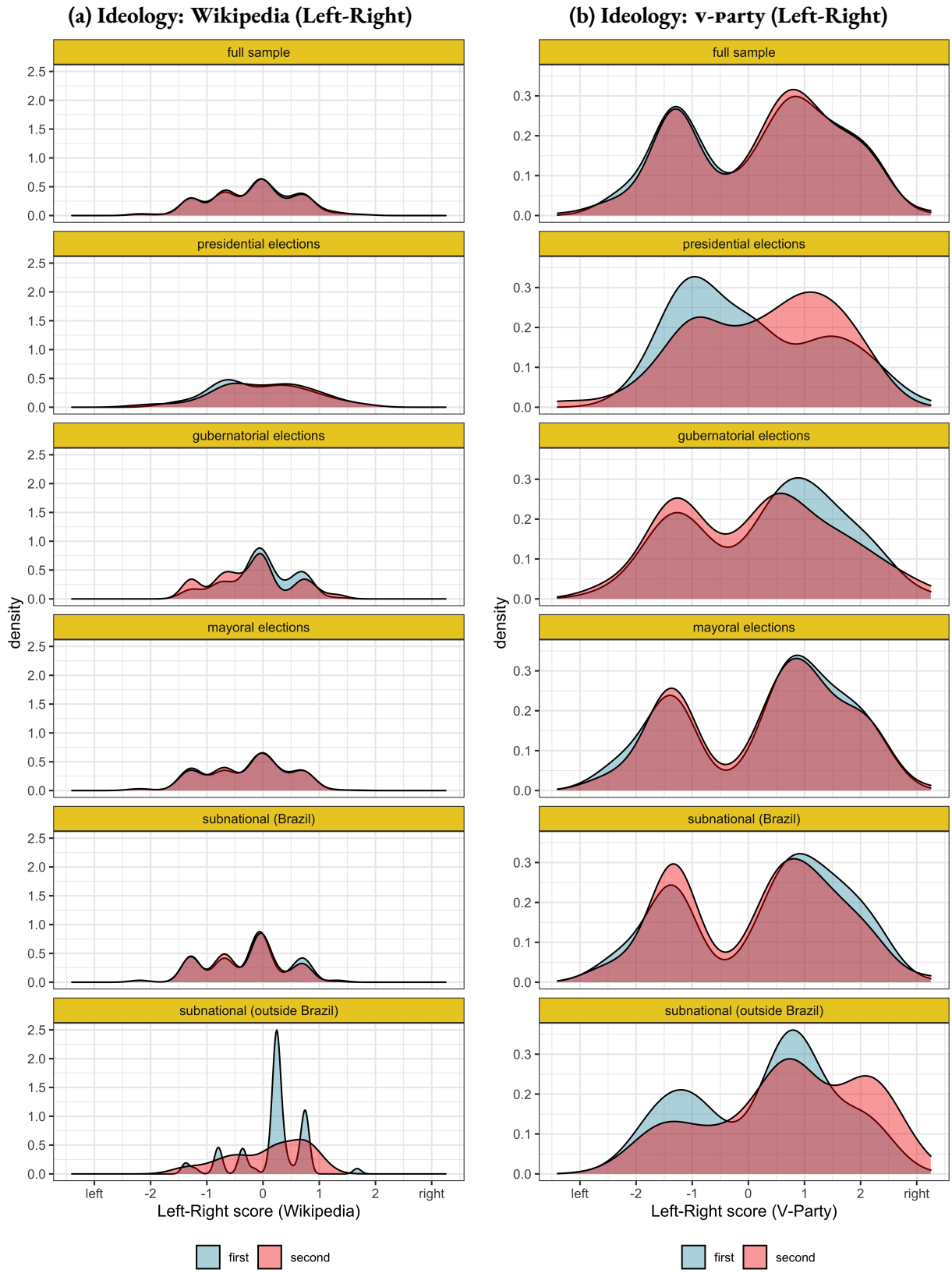



Figure A6: Distribution of Left-Right ideology scores for the top two placed candidates, by subsample. Only elections in which a second round was needed are included.

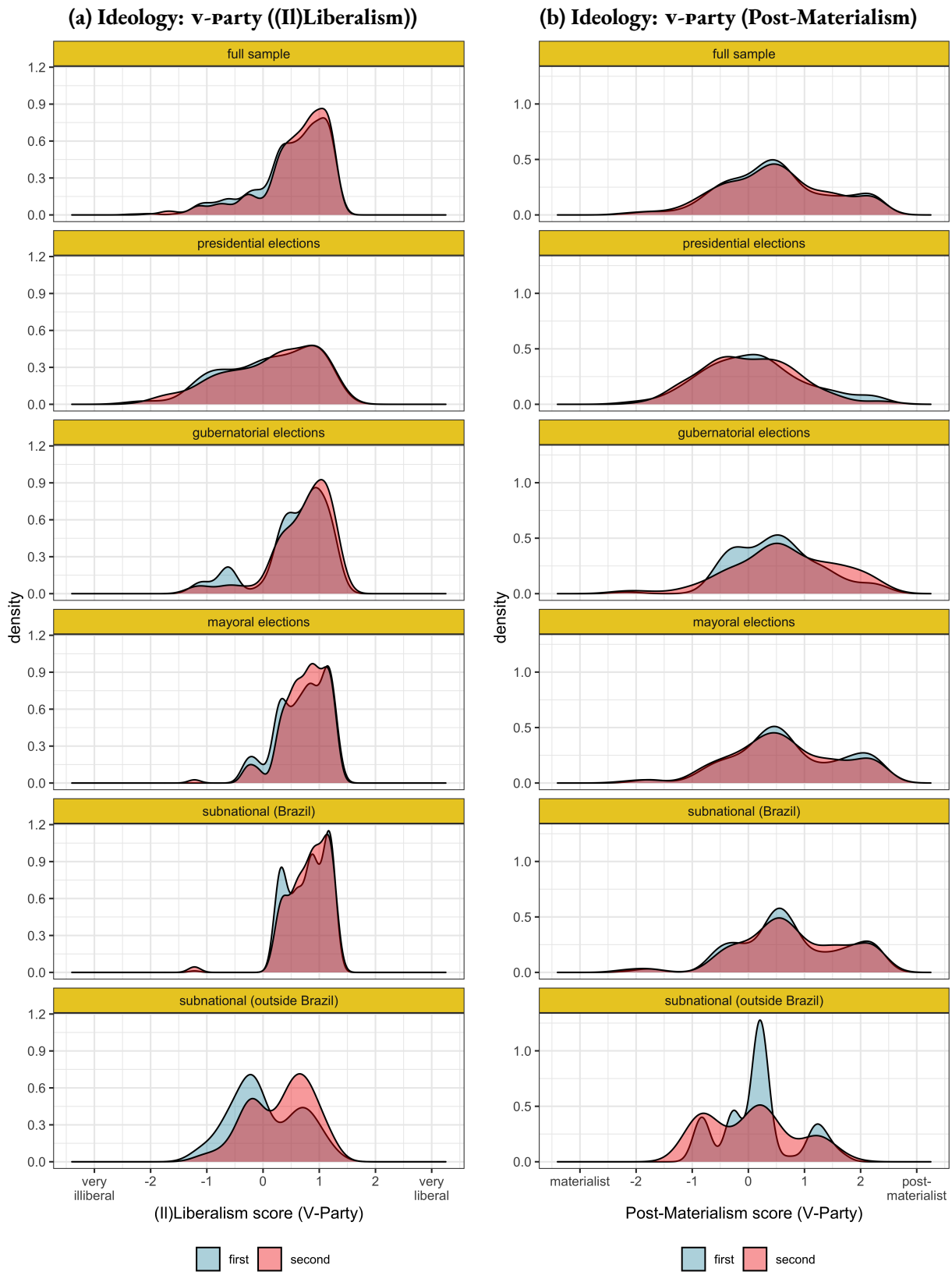



Figure A7: Distribution of (Il)Liberalism (left) and Post-Materialism (right) ideology scores for the top two placed candidates, by subsample. Only elections in which a second round was needed are included.

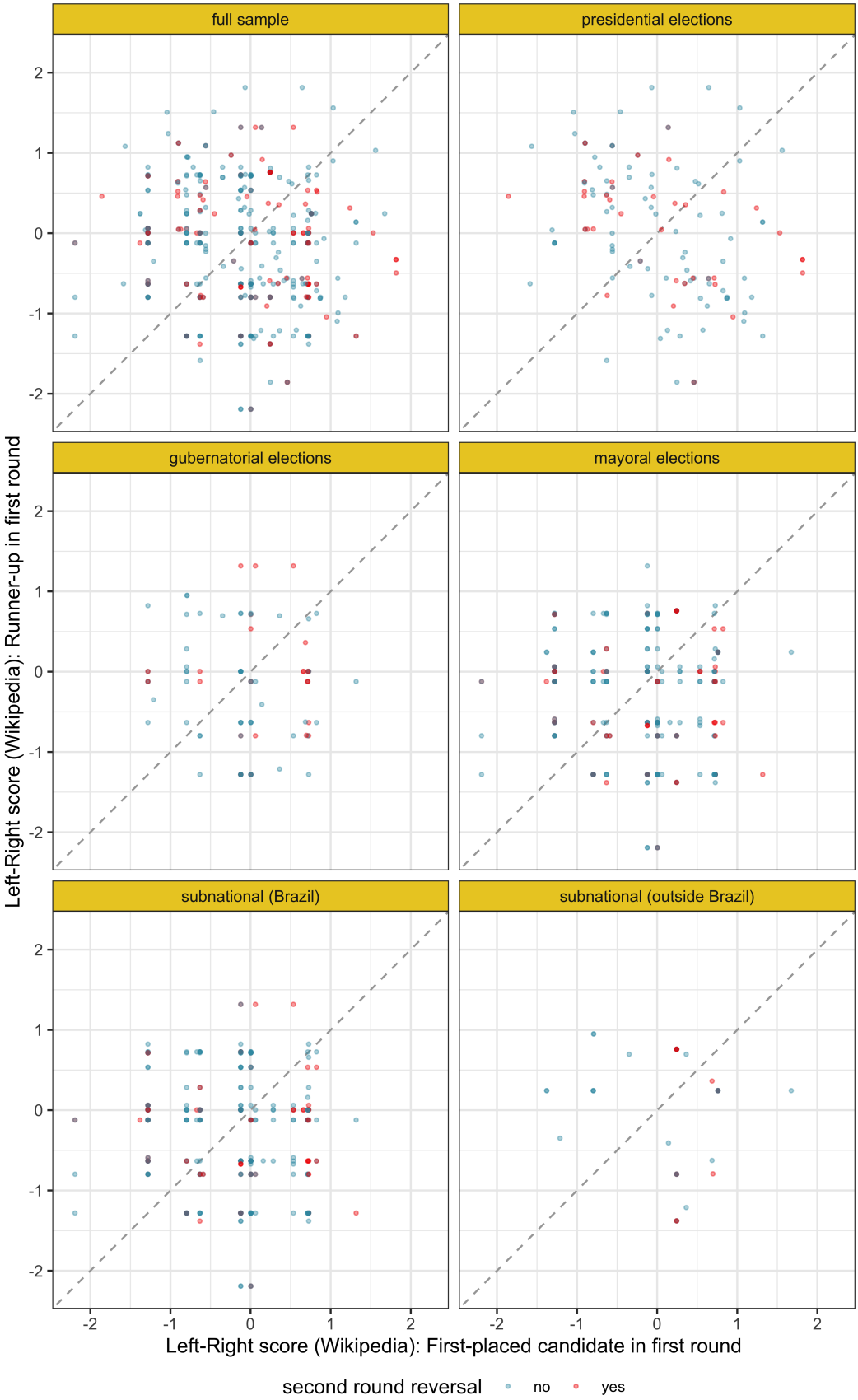



Figure A8: First-round Wikipedia left-right scores of the first- and second-placed candidates, by subsample. Only elections in which a second round was needed are included. Red dots indicate elections in which the first-round result was reversed in the runoff.

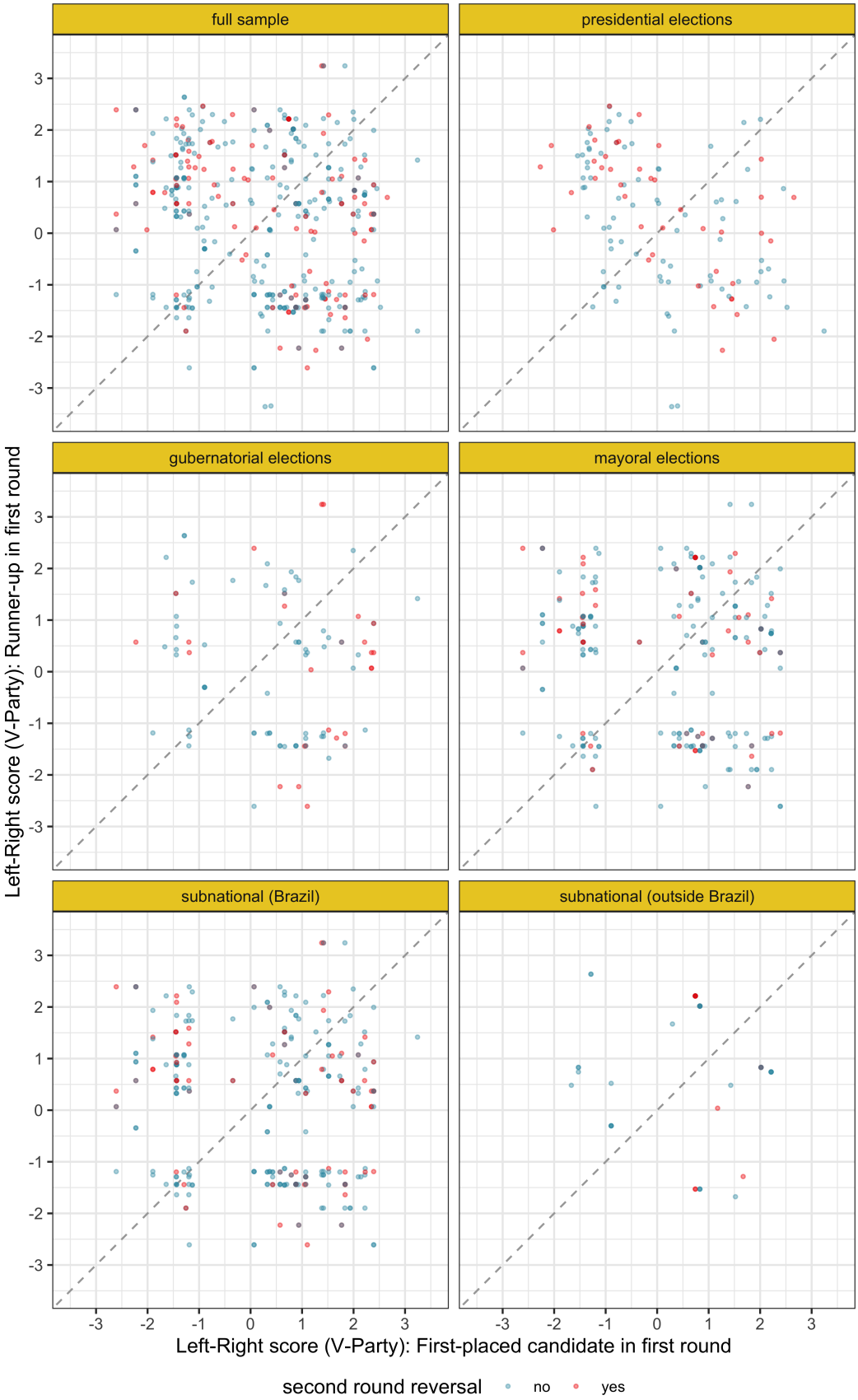



Figure A9: First-round V-Party left-right scores of the first- and second-placed candidates, by subsample. Only elections in which a second round was needed are included. Red dots indicate elections in which the first-round result was reversed in the runoff.

Table A3: Differences in means between treatment and control groups, 2 and 5 pp. bandwidths

| | | DV: *winner* (0/100) | | | DV: *vote share*$_{R2}$ (0:100) | | |
|---|---|---|---|---|---|---|---|
| (a) 2 pp. bandwidth | $N^-\|N^+$ | $\bar{y}^-$ | $\bar{y}^+$ | diff. | $\bar{y}^-$ | $\bar{y}^+$ | diff. |
| full sample | 96\|96 | 39.58 | 60.42 | 20.83 | 48.97 | 51.03 | 2.05 |
| presidential elections | 25\|25 | 52.00 | 48.00 | -4.00 | 49.95 | 50.05 | 0.10 |
| gubernatorial elections | 22\|22 | 45.45 | 54.55 | 9.09 | 50.35 | 49.65 | -0.70 |
| mayoral elections | 49\|49 | 30.61 | 69.39 | 38.78 | 47.86 | 52.14 | 4.28 |
| subnational (Brazil) | 57\|57 | 38.60 | 61.40 | 22.81 | 49.05 | 50.95 | 1.91 |
| subnational (outside Brazil) | 14\|14 | 21.43 | 78.57 | 57.14 | 46.94 | 53.06 | 6.11 |
| (b) 5 pp. bandwidth | | | | | | | |
| full sample | 222\|222 | 41.44 | 58.56 | 17.12 | 48.74 | 51.26 | 2.52 |
| presidential elections | 60\|60 | 51.67 | 48.33 | -3.33 | 49.09 | 50.91 | 1.81 |
| gubernatorial elections | 52\|52 | 46.15 | 53.85 | 7.69 | 49.34 | 50.66 | 1.33 |
| mayoral elections | 110\|110 | 33.64 | 66.36 | 32.73 | 48.27 | 51.73 | 3.45 |
| subnational (Brazil) | 122\|122 | 36.07 | 63.93 | 27.87 | 48.56 | 51.44 | 2.87 |
| subnational (outside Brazil) | 40\|40 | 42.50 | 57.50 | 15.00 | 48.77 | 51.23 | 2.46 |

Number of observations, mean outcome values of bare winners and losers, and differences in means, for observations within (a) 2 percentage points; and (b) 5 percentage points of the threshold, by subsample.

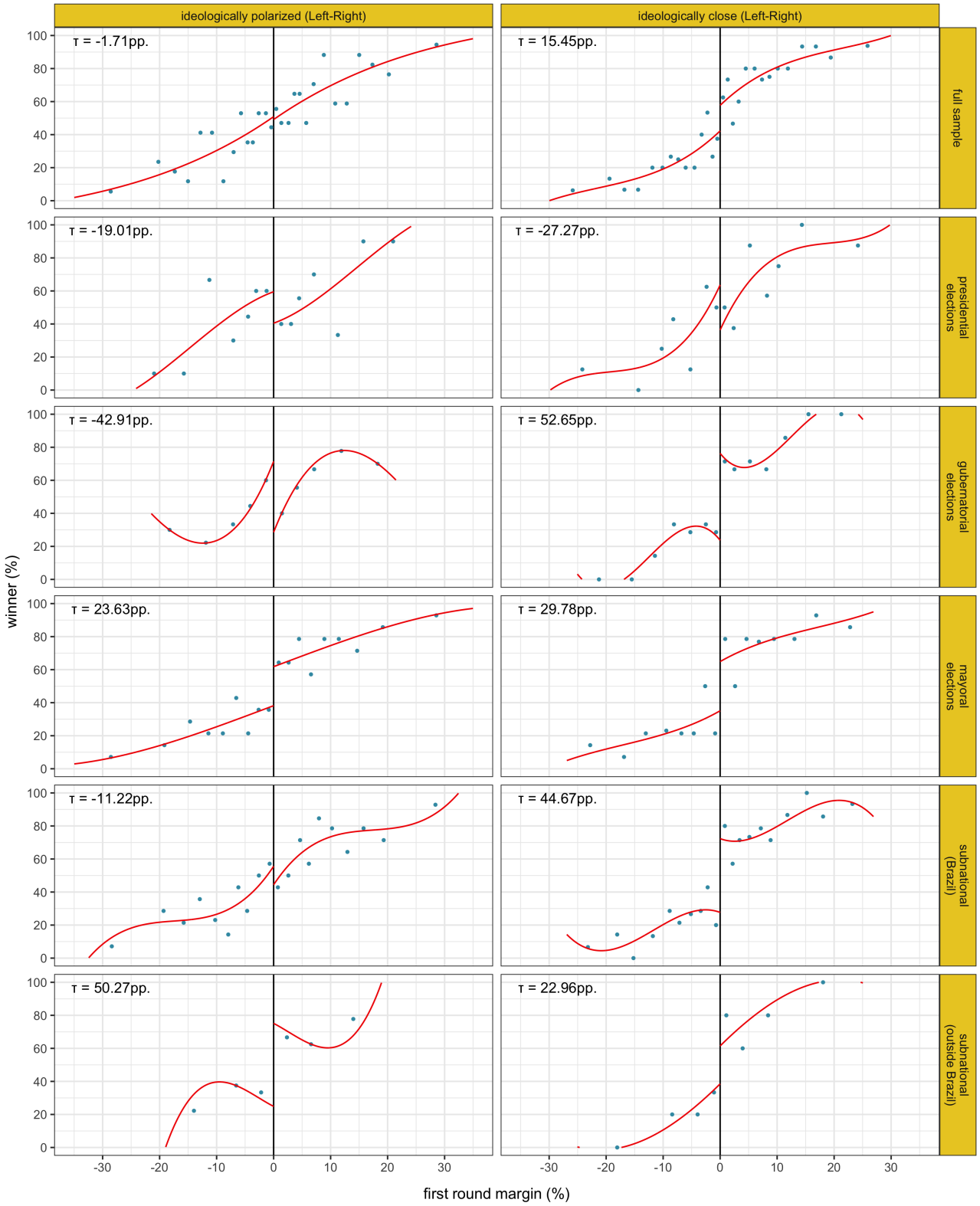



Figure A10: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the probability of winning the election. Polarized (respectively, close) elections are those in which the absolute ideological distance along the V-Party Left-Right dimension between the top-two vote getters in the first round was larger (smaller) than the median for each sample.

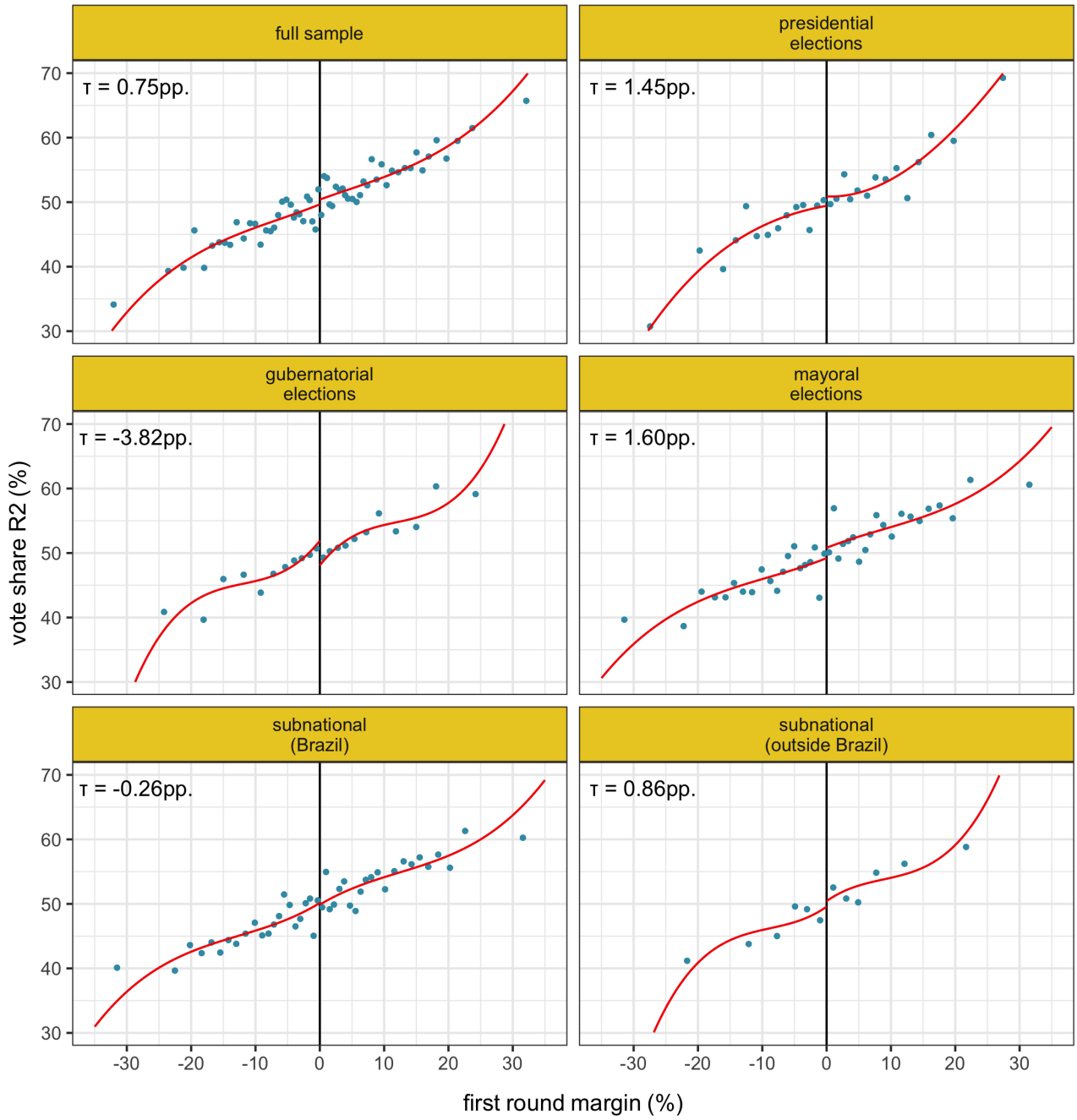



Figure A11: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the vote share in the second round. Red lines show third-order polynomials estimated separately at each side of the cutoff, using a uniform kernel.

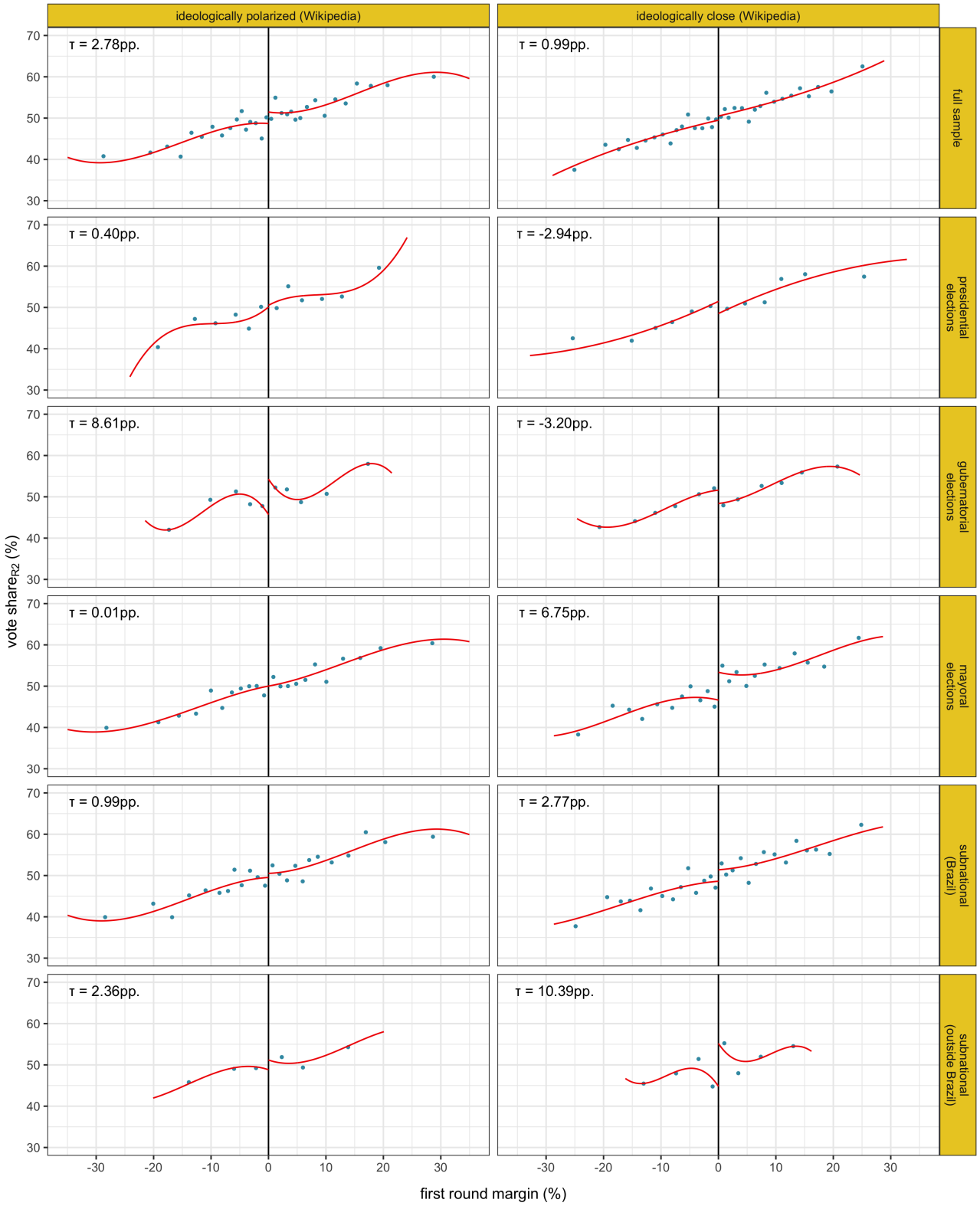



Figure A12: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the vote share in the second round. Polarized (respectively, close) elections are those in which the absolute ideological distance along the Wikipedia Left-Right dimension between the top-two vote getters in the first round was larger (smaller) than the median for each sample. Red lines show third-order polynomials estimated separately at each side of the cutoff, using a uniform kernel.

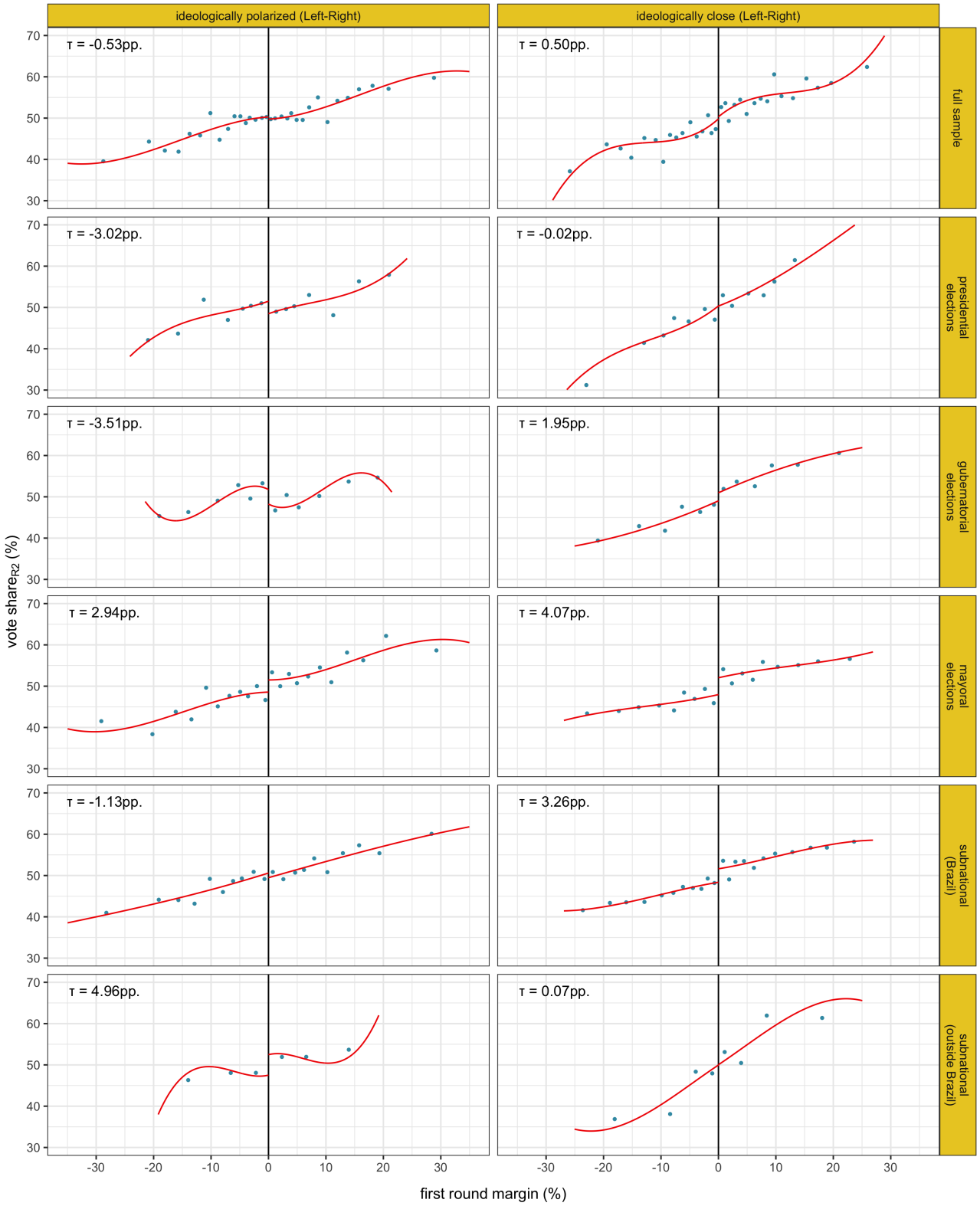



Figure A13: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the vote share in the second round. Polarized (respectively, close) elections are those in which the absolute ideological distance along the V-Party Left-Right dimension between the top-two vote getters in the first round was larger (smaller) than the median for each sample. Red lines show third-order polynomials estimated separately at each side of the cutoff, using a uniform kernel.

# A3 Balance checks

**RD plots.** Figures A14 and A15 show the effect of *first round margin* on a series of outcomes that should not be affected by the treatment: candidates' ideology scores, the presence of missing values for these variables, and candidates' incumbency status.

**RD estimates.** Tables A4 and A5 report the corresponding MSERD-optimal RD estimates.

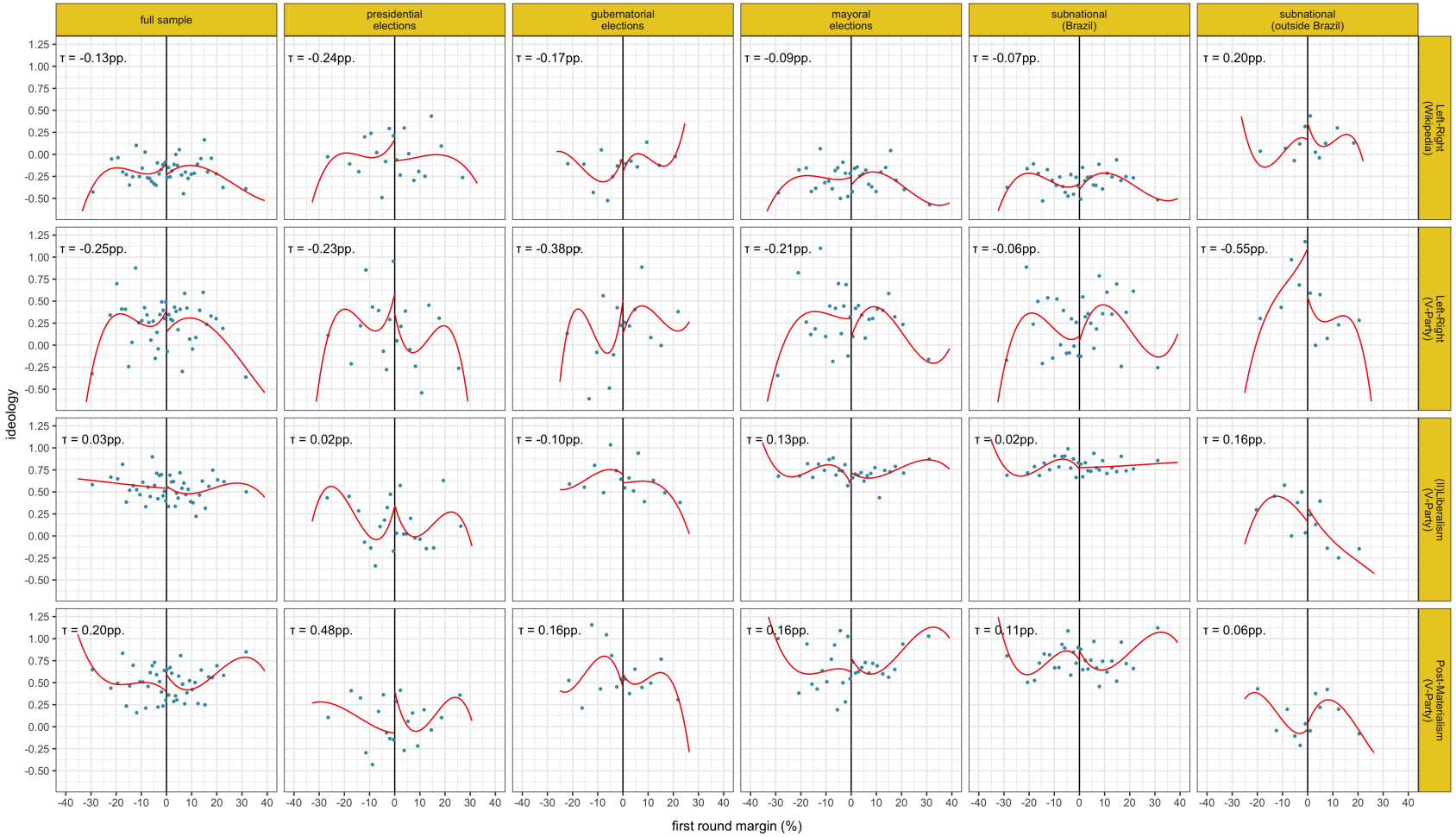



Figure A14: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of first-round advantage on candidates' ideology scores. Red lines show third-order polynomials estimated separately at each side of the cutoff, using a uniform kernel.

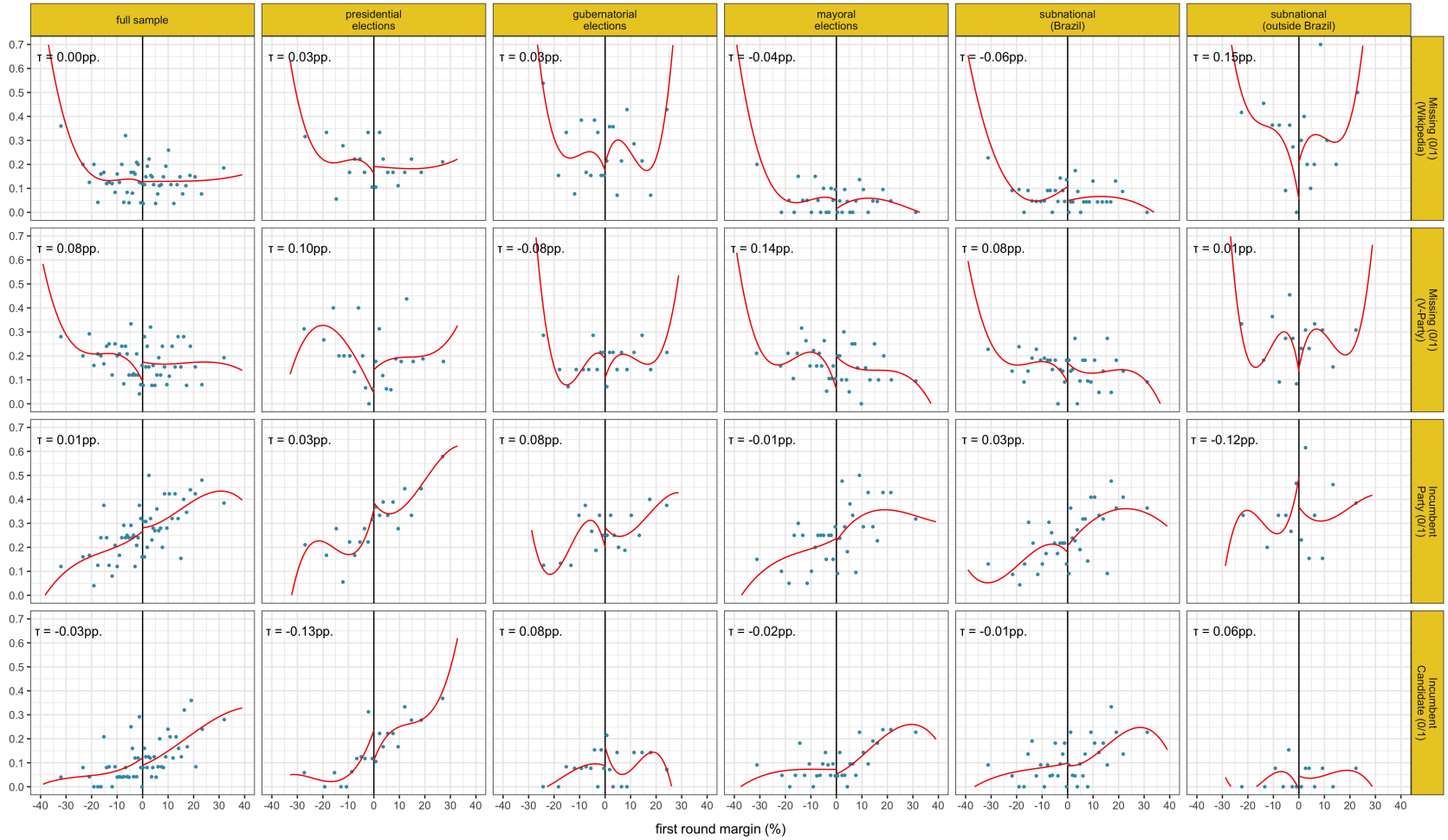



Figure A15: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of first-round advantage on missingness in ideology scores and candidates' incumbency status. Red lines show third-order polynomials estimated separately at each side of the cutoff, using a uniform kernel.

Table A4: RD estimates: Placebo outcomes (ideology scores)

| | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *Left-Right (Wikipedia)* (-2.3:1.9) | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | -0.20 | [-0.57:0.05] | 0.11 | 6.35 | 234\|235 | 0.77 | 1.00 | 0.93 | 0.43 |
| presidential | -0.37 | [-0.92:0.03] | 0.07 | 8.80 | 74\|76 | 0.87 | 1.00 | 0.72 | 0.57 |
| gubernatorial | -0.09 | [-0.70:0.41] | 0.61 | 8.32 | 59\|58 | 0.72 | 0.95 | 0.44 | 0.07 |
| mayoral | -0.00 | [-0.45:0.40] | 0.90 | 9.25 | 176\|180 | 0.72 | 1.00 | 0.64 | 0.05 |
| subnational (Brazil) | 0.01 | [-0.34:0.34] | 0.99 | 10.72 | 212\|215 | 0.69 | 1.00 | 0.80 | 0.05 |
| subnational (¬ Brazil) | 0.05 | [-0.64:0.74] | 0.88 | 7.44 | 39\|38 | 0.65 | 0.74 | 0.26 | 0.05 |
| (b) DV: *Left-Right (V-Party)* (-3.4:3.5) | | | | | | | | | |
| full sample | -0.26 | [-0.90:0.21] | 0.22 | 8.31 | 285\|280 | 1.35 | 1.00 | 0.92 | 0.25 |
| presidential | -0.59 | [-1.42:0.02] | 0.06 | 9.21 | 82\|81 | 1.26 | 1.00 | 0.68 | 0.62 |
| gubernatorial | -0.28 | [-1.58:0.76] | 0.49 | 7.99 | 60\|62 | 1.44 | 0.93 | 0.40 | 0.10 |
| mayoral | -0.01 | [-0.89:0.87] | 0.98 | 10.67 | 168\|167 | 1.37 | 0.99 | 0.58 | 0.05 |
| subnational (Brazil) | 0.13 | [-0.61:0.83] | 0.76 | 11.05 | 201\|203 | 1.36 | 1.00 | 0.75 | 0.08 |
| subnational (¬ Brazil) | -0.85 | [-2.49:0.65] | 0.25 | 8.42 | 40\|38 | 1.23 | 0.58 | 0.19 | 0.33 |
| (c) DV: *(Il)Liberalism (V-Party)* (-2.5:1.3) | | | | | | | | | |
| full sample | 0.11 | [-0.16:0.48] | 0.33 | 5.66 | 208\|204 | 0.61 | 1.00 | 0.75 | 0.17 |
| presidential | 0.22 | [-0.39:1.05] | 0.37 | 5.63 | 59\|54 | 0.74 | 0.81 | 0.30 | 0.14 |
| gubernatorial | -0.20 | [-0.75:0.28] | 0.37 | 9.31 | 66\|66 | 0.61 | 0.91 | 0.38 | 0.19 |
| mayoral | 0.09 | [-0.16:0.37] | 0.43 | 8.58 | 148\|144 | 0.45 | 1.00 | 0.66 | 0.15 |
| subnational (Brazil) | 0.00 | [-0.16:0.19] | 0.84 | 8.15 | 165\|163 | 0.41 | 1.00 | 0.90 | 0.05 |
| subnational (¬ Brazil) | 0.03 | [-0.67:0.76] | 0.91 | 7.91 | 39\|38 | 0.57 | 0.59 | 0.20 | 0.05 |
| (d) DV: *Post-Materialism (V-Party)* (-2.2:2.6) | | | | | | | | | |
| full sample | 0.22 | [-0.14:0.69] | 0.20 | 6.99 | 249\|246 | 0.96 | 1.00 | 0.89 | 0.32 |
| presidential | 0.74 | [0.21:1.52] | 0.01 | 5.85 | 60\|55 | 0.85 | 0.95 | 0.44 | 0.88 |
| gubernatorial | 0.06 | [-0.53:0.75] | 0.74 | 7.88 | 60\|62 | 0.98 | 0.99 | 0.57 | 0.06 |
| mayoral | 0.04 | [-0.58:0.68] | 0.88 | 8.81 | 152\|147 | 0.94 | 0.98 | 0.54 | 0.05 |
| subnational (Brazil) | -0.03 | [-0.48:0.46] | 0.97 | 10.72 | 196\|198 | 0.90 | 1.00 | 0.76 | 0.05 |
| subnational (¬ Brazil) | 0.19 | [-0.72:1.00] | 0.75 | 7.68 | 39\|38 | 0.68 | 0.60 | 0.20 | 0.09 |

Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample size.

Table A5: RD estimates: Placebo outcomes (missingness in ideology scores and incumbency status)

| | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *Missing Wikipedia scores* (0/1) | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | 0.00 | [-0.10:0.12] | 0.89 | 8.32 | 338\|338 | 0.34 | 1.00 | 0.99 | 0.05 |
| presidential | 0.00 | [-0.23:0.22] | 0.96 | 8.70 | 94\|94 | 0.41 | 1.00 | 0.71 | 0.05 |
| gubernatorial | 0.11 | [-0.15:0.43] | 0.34 | 9.51 | 81\|81 | 0.42 | 0.98 | 0.51 | 0.18 |
| mayoral | -0.05 | [-0.16:0.04] | 0.26 | 10.19 | 196\|196 | 0.23 | 1.00 | 0.87 | 0.26 |
| subnational (Brazil) | -0.06 | [-0.20:0.05] | 0.23 | 9.64 | 214\|214 | 0.27 | 1.00 | 0.86 | 0.30 |
| subnational (¬ Brazil) | 0.35 | [0.04:0.80] | 0.03 | 6.19 | 46\|46 | 0.40 | 0.83 | 0.31 | 0.71 |
| (b) DV: *Missing V-Party scores* (0/1) | | | | | | | | | |
| full sample | 0.08 | [-0.04:0.21] | 0.17 | 9.40 | 367\|367 | 0.37 | 1.00 | 0.98 | 0.41 |
| presidential | 0.23 | [0.04:0.49] | 0.02 | 8.22 | 90\|90 | 0.34 | 0.99 | 0.56 | 0.79 |
| gubernatorial | -0.09 | [-0.37:0.16] | 0.43 | 9.99 | 86\|86 | 0.40 | 0.99 | 0.56 | 0.16 |
| mayoral | 0.09 | [-0.07:0.27] | 0.23 | 10.92 | 203\|203 | 0.37 | 1.00 | 0.85 | 0.33 |
| subnational (Brazil) | 0.05 | [-0.14:0.22] | 0.63 | 9.77 | 217\|217 | 0.35 | 1.00 | 0.77 | 0.11 |
| subnational (¬ Brazil) | 0.01 | [-0.33:0.38] | 0.91 | 7.74 | 52\|52 | 0.44 | 0.93 | 0.40 | 0.05 |
| (c) DV: *Incumbent Party* (0/1) | | | | | | | | | |
| full sample | 0.02 | [-0.12:0.18] | 0.70 | 9.12 | 358\|358 | 0.43 | 1.00 | 0.97 | 0.07 |
| presidential | 0.05 | [-0.27:0.39] | 0.72 | 8.52 | 93\|93 | 0.43 | 0.96 | 0.45 | 0.07 |
| gubernatorial | 0.08 | [-0.23:0.46] | 0.50 | 8.92 | 77\|77 | 0.45 | 0.96 | 0.45 | 0.10 |
| mayoral | -0.01 | [-0.22:0.15] | 0.70 | 9.82 | 193\|193 | 0.41 | 1.00 | 0.85 | 0.06 |
| subnational (Brazil) | 0.04 | [-0.13:0.23] | 0.58 | 8.88 | 204\|204 | 0.41 | 1.00 | 0.88 | 0.09 |
| subnational (¬ Brazil) | -0.06 | [-0.54:0.44] | 0.83 | 10.06 | 60\|60 | 0.48 | 0.78 | 0.28 | 0.06 |
| (d) DV: *Incumbent Candidate* (0/1) | | | | | | | | | |
| full sample | 0.03 | [-0.06:0.16] | 0.36 | 6.64 | 277\|277 | 0.31 | 1.00 | 0.97 | 0.13 |
| presidential | -0.07 | [-0.31:0.17] | 0.58 | 9.76 | 99\|99 | 0.35 | 0.98 | 0.51 | 0.12 |
| gubernatorial | 0.06 | [-0.15:0.32] | 0.46 | 9.18 | 77\|77 | 0.29 | 0.93 | 0.40 | 0.11 |
| mayoral | 0.04 | [-0.07:0.20] | 0.36 | 7.44 | 156\|156 | 0.27 | 1.00 | 0.79 | 0.12 |
| subnational (Brazil) | 0.08 | [-0.05:0.27] | 0.18 | 6.13 | 148\|148 | 0.30 | 1.00 | 0.75 | 0.27 |
| subnational (¬ Brazil) | 0.04 | [-0.05:0.12] | 0.41 | 5.70 | 43\|43 | 0.21 | 1.00 | 0.92 | 0.22 |

Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample size.

# A4 Robustness checks

**Local randomization estimates.** Table A6 replicates the results from Table 2 but following a local randomization approach (Cattaneo, Titiunik and Vázquez-Bare 2016) instead of a continuity-based approach.

**Sensitivity to bandwidth choice.** Figure A16 shows that the findings reported in Table 2 are not overly sensitive to bandwidth choice. Except in the case of very small bandwidths –with the accompanying reduction in the number of observations–, the estimates remain very similar if we double the bandwidth reported in Table 2, cut it by half, employ the Imbens and Kalyanaraman (2012) bandwidth, or increase the bandwidths to up to 34 pp.

**Samples with non-missing data on ideology.** The specifications in Table A7 remove all observations with missing data on the left-right ideological positioning of the top two placed candidates. This shows that neither the results with controls nor the heterogeneous effects are an artifact of restricting the sample to observations with nonmissing values.

**Adding controls.** Table A8 replicates the results reported in Table 2, but including controls for the Left-Right Wikipedia ideology, the partisan incumbency status and the individual incumbency status of the top two placed candidates in the first round.

**CER-optimal bandwidth.** Table A9 replicates the results reported in Table 2 but employing CER-optimal instead of MSE-optimal bandwidths, which may produce different results (de Magalhães et al. 2020).

**Second-order polynomials.** Table A10 the results reported in Table 2 but employing second-order polynomials instead of a local linear regression.

**One candidate per election.** The fact that one and only one of the top-two placed candidates in the first round must win raises the possibility that observations may not be independent. To show that this

does not affect the results, in Figure A17 we compare the estimates reported in Table 2 with 500 estimates that result from randomly sampling a single candidate –either the first-placed or the runner-up– in every election.

**Heterogeneous effects (I): Visibility.** Table A11 reports results for the subsample of open seat races, ie when neither the first- nor the second-placed candidate in the first round was the incumbent. For the Brazilian sample exclusively, Table A12 distinguish between elections in which (i) neither; (ii) neither or both; (iii) the first-placed; or (iv) the second-placed candidate had previous elected experience, respectively. For gubernatorial candidates, being experienced is defined as having served as president, senator or governor at any moment in the past; experienced mayoral candidates are those that had served as either president, senator, governor, federal deputy or mayor.

**Heterogeneous effects (II): Ideology.** Tables A13 through A15 replicate the results reported in Table 3 but measuring the candidates' ideological distance using V-Party Left-Right, (Il)Liberalism and Post-Materialism dimensions, respectively.

Table A6: RD estimates: Local randomization approach

| | diff. in means | | K-S* | | rank sum | | | |
|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | estim. | *p*-val. | estim. | *p*-val. | estim. | *p*-val. | bwd. | $N^{-}\|N^{+}$ |
| full sample | -11.11 | 0.59 | 0.11 | 0.59 | 0.70 | 0.59 | 0.65 | 27\|27 |
| presidential | | | | | | | | |
| gubernatorial | 20.00 | 0.65 | 0.20 | 0.65 | -0.76 | 0.65 | 1.15 | 10\|10 |
| mayoral | 52.00 | 0.00 | 0.52 | 0.00 | -3.15 | 0.00 | 1.15 | 25\|25 |
| subnational (Brazil) | 46.67 | 0.00 | 0.47 | 0.00 | -3.10 | 0.00 | 1.15 | 30\|30 |
| subnational (¬ Brazil) | | | | | | | | |
| (b) DV: *vote share*$_{R2}$ (0:100) | | | | | | | | |
| full sample | -1.54 | 0.33 | 0.15 | 0.94 | 0.63 | 0.54 | 0.65 | 27\|27 |
| presidential | | | | | | | | |
| gubernatorial | 2.65 | 0.13 | 0.30 | 0.79 | -1.21 | 0.25 | 1.15 | 10\|10 |
| mayoral | 7.03 | 0.00 | 0.60 | 0.00 | -3.46 | 0.00 | 1.15 | 25\|25 |
| subnational (Brazil) | 5.98 | 0.00 | 0.50 | 0.00 | -3.34 | 0.00 | 1.15 | 30\|30 |
| subnational (¬ Brazil) | | | | | | | | |

Sharp local randomization RD estimates, calculated following the procedure proposed by Cattaneo, Titiunik and Vázquez-Bare (2016). Only samples with at least 10 observations at each side of the threshold are included. Exact *p*-values based on 10,000 permutations. The running variable is *first round margin*. The covariates used to determine balance are Left-Right ideology as measured with Wikipedia tags (Herrmann and Döring forthcoming) as well as partisan and individual incumbency status. Samples are restricted to elections requiring a runoff. Reported number of observations indicate the *effective* sample size. (*) Kolmogorov-Smirnov statistic.

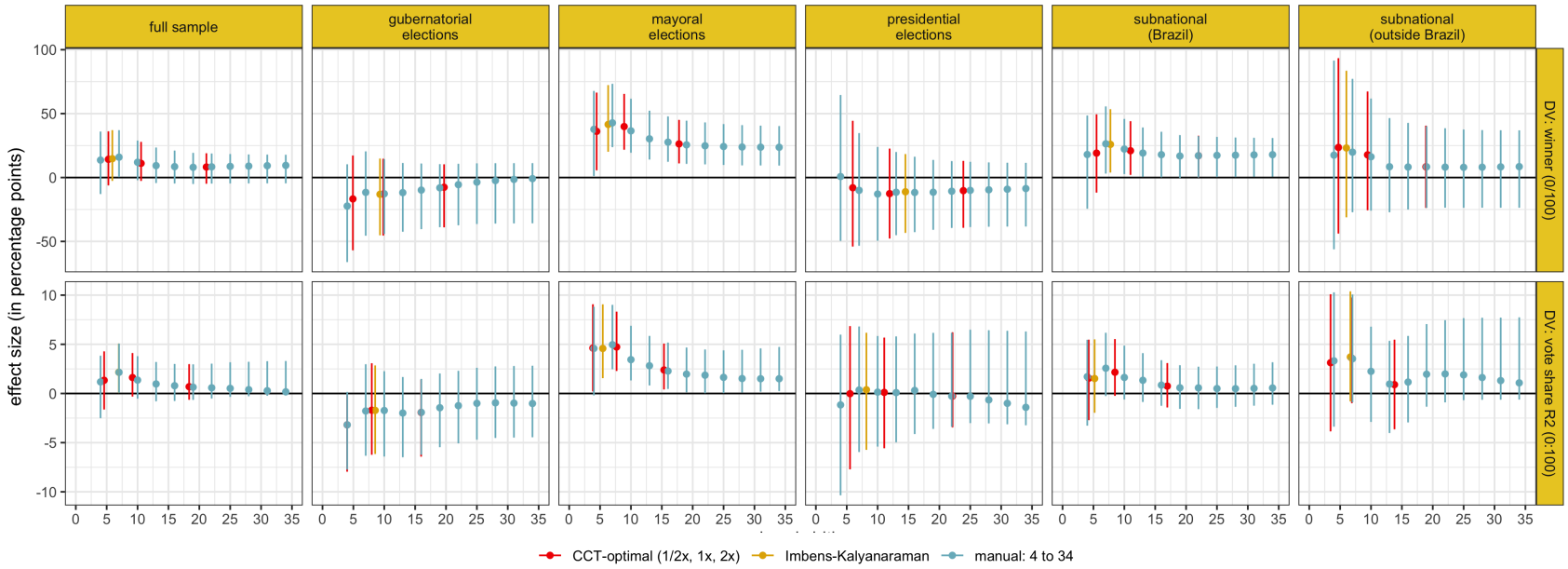



Figure A16: Sharp (conventional) RD estimates, with robust 95% CIs. The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. To calculate the estimates, we clustered observations by election and fitted a separate local linear regression at both sides of the threshold, using a triangular kernel. The CCT-optimal bandwidth is the (MSE-optimal) bandwidth reported in Table 2.

Table A7: RD estimates: Samples with nonmissing ideology scores

| | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | power against $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | | | | Ideology: Wikipedia | | | | | |
| full sample | 23.87 | [8.80:43.84] | 0.00 | 9.00 | 285\|285 | 47.92 | 1.00 | 0.97 | 0.97 |
| presidential | -4.99 | [-43.44:36.94] | 0.87 | 11.88 | 82\|82 | 49.34 | 0.93 | 0.40 | 0.06 |
| gubernatorial | 8.01 | [-36.10:51.07] | 0.74 | 9.25 | 52\|52 | 48.62 | 0.87 | 0.34 | 0.08 |
| mayoral | 43.92 | [24.42:71.35] | 0.00 | 8.29 | 157\|157 | 47.44 | 1.00 | 0.80 | 1.00 |
| subnational (Brazil) | 31.89 | [13.36:56.47] | 0.00 | 8.95 | 183\|183 | 47.27 | 1.00 | 0.86 | 0.98 |
| subnational (¬ Brazil) | 41.12 | [-14.42:106.49] | 0.14 | 7.90 | 35\|35 | 48.16 | 0.60 | 0.20 | 0.47 |
| (b) DV: *vote share*$_{R2}$ (0:100) | | | | | | | | | |
| full sample | 3.01 | [1.18:5.57] | 0.00 | 7.17 | 243\|243 | 6.83 | 1.00 | 0.99 | 0.97 |
| presidential | -0.03 | [-4.71:4.15] | 0.90 | 10.93 | 78\|79 | 8.33 | 1.00 | 0.74 | 0.05 |
| gubernatorial | 0.83 | [-3.66:6.51] | 0.58 | 8.17 | 51\|51 | 6.77 | 0.96 | 0.45 | 0.07 |
| mayoral | 5.53 | [2.81:9.58] | 0.00 | 6.87 | 136\|136 | 6.34 | 1.00 | 0.74 | 0.99 |
| subnational (Brazil) | 4.06 | [1.53:7.74] | 0.00 | 6.88 | 148\|148 | 6.57 | 1.00 | 0.83 | 0.95 |
| subnational (¬ Brazil) | 5.98 | [-0.88:14.35] | 0.08 | 6.79 | 33\|33 | 5.38 | 0.50 | 0.16 | 0.59 |
| (c) DV: *winner* (0/100) | | | | Ideology: V-Party | | | | | |
| full sample | 6.66 | [-11.51:22.47] | 0.53 | 10.89 | 286\|286 | 47.88 | 1.00 | 0.97 | 0.19 |
| presidential | -25.96 | [-69.02:12.35] | 0.17 | 10.99 | 82\|82 | 50.07 | 0.93 | 0.40 | 0.42 |
| gubernatorial | 4.80 | [-44.38:50.77] | 0.90 | 9.23 | 53\|53 | 48.94 | 0.81 | 0.30 | 0.06 |
| mayoral | 26.72 | [-0.43:51.98] | 0.05 | 11.27 | 147\|147 | 45.65 | 1.00 | 0.68 | 0.81 |
| subnational (Brazil) | 15.61 | [-7.51:36.06] | 0.20 | 10.86 | 171\|171 | 46.62 | 1.00 | 0.84 | 0.51 |
| subnational (¬ Brazil) | 40.51 | [-12.40:98.48] | 0.13 | 9.83 | 31\|31 | 47.52 | 0.66 | 0.22 | 0.53 |
| (d) DV: *vote share*$_{R2}$ (0:100) | | | | | | | | | |
| full sample | 1.31 | [-1.21:4.37] | 0.27 | 9.83 | 262\|262 | 6.96 | 1.00 | 0.93 | 0.25 |
| presidential | -1.68 | [-8.17:4.14] | 0.52 | 10.23 | 75\|75 | 8.80 | 0.98 | 0.51 | 0.12 |
| gubernatorial | -1.16 | [-6.58:5.82] | 0.90 | 7.13 | 46\|46 | 6.73 | 0.85 | 0.32 | 0.08 |
| mayoral | 4.32 | [0.72:8.82] | 0.02 | 9.13 | 128\|128 | 5.68 | 0.97 | 0.49 | 0.84 |
| subnational (Brazil) | 1.85 | [-1.66:6.05] | 0.26 | 9.60 | 156\|156 | 6.45 | 1.00 | 0.64 | 0.26 |
| subnational (¬ Brazil) | 5.57 | [-0.11:14.24] | 0.05 | 6.85 | 27\|27 | 4.82 | 0.46 | 0.15 | 0.58 |

Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections (a) requiring a runoff and with (b) nonmissing data on the Left-Right ideology of the top two placed candidates, measured either using Wikipedia or V-Party. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample sizes.

Table A8: RD estimates: Including controls

| | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | estim. | 95% CI | *p*-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | 22.24 | [7.65:41.92] | 0.00 | 8.55 | 296\|301 | 48.41 | 1.00 | 0.97 | 0.95 |
| presidential | -5.91 | [-42.89:33.13] | 0.80 | 11.99 | 94\|96 | 48.87 | 0.95 | 0.43 | 0.07 |
| gubernatorial | 1.64 | [-34.59:37.72] | 0.93 | 8.52 | 60\|58 | 49.40 | 0.97 | 0.48 | 0.05 |
| mayoral | 45.34 | [25.80:73.82] | 0.00 | 7.41 | 147\|150 | 47.54 | 1.00 | 0.78 | 1.00 |
| subnational (Brazil) | 31.35 | [11.63:57.53] | 0.00 | 8.36 | 179\|184 | 47.71 | 1.00 | 0.83 | 0.97 |
| subnational (¬ Brazil) | 30.78 | [-12.23:86.34] | 0.14 | 8.19 | 42\|40 | 49.15 | 0.70 | 0.24 | 0.34 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | |
| full sample | 2.99 | [1.12:5.62] | 0.00 | 6.91 | 251\|256 | 6.92 | 1.00 | 0.99 | 0.95 |
| presidential | 0.68 | [-4.23:5.52] | 0.79 | 11.27 | 90\|93 | 9.30 | 1.00 | 0.76 | 0.07 |
| gubernatorial | 0.64 | [-3.21:5.72] | 0.58 | 7.14 | 52\|52 | 6.31 | 0.97 | 0.49 | 0.07 |
| mayoral | 5.08 | [2.56:8.84] | 0.00 | 7.39 | 147\|150 | 6.50 | 1.00 | 0.81 | 0.99 |
| subnational (Brazil) | 3.46 | [1.10:6.95] | 0.01 | 7.45 | 163\|167 | 6.67 | 1.00 | 0.88 | 0.90 |
| subnational (¬ Brazil) | 5.18 | [-0.47:12.08] | 0.07 | 6.24 | 37\|35 | 5.40 | 0.61 | 0.20 | 0.57 |

Sharp (conventional) RD estimates, with robust CIs and *p*-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections (a) requiring a runoff and with (b) nonmissing data on the Wikipedia Left-Right ideology of the top two placed candidates. All specifications control for (a) the Left-Right Wikipedia ideology; (b) partisan incumbency status; and (c) individual incumbency status of the top two placed candidates. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample sizes.

Table A9: RD estimates: CER-optimal bandwidths

| | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | 15.75 | [-0.07:33.13] | 0.05 | 7.38 | 306\|306 | 49.24 | 1.00 | 0.99 | 0.82 |
| presidential | -10.99 | [-48.39:26.49] | 0.57 | 8.89 | 95\|95 | 49.93 | 0.98 | 0.50 | 0.14 |
| gubernatorial | -12.08 | [-44.63:17.44] | 0.39 | 7.43 | 71\|71 | 49.75 | 1.00 | 0.63 | 0.20 |
| mayoral | 41.94 | [20.35:67.46] | 0.00 | 6.42 | 136\|136 | 48.39 | 1.00 | 0.87 | 1.00 |
| subnational (Brazil) | 25.78 | [4.16:49.79] | 0.02 | 7.89 | 187\|187 | 48.38 | 1.00 | 0.89 | 0.93 |
| subnational (¬ Brazil) | 19.34 | [-27.57:70.38] | 0.39 | 7.28 | 50\|50 | 49.49 | 0.84 | 0.31 | 0.21 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | |
| full sample | 2.03 | [-0.21:4.56] | 0.07 | 6.41 | 269\|269 | 7.37 | 1.00 | 1.00 | 0.72 |
| presidential | 0.37 | [-5.42:6.10] | 0.91 | 8.25 | 90\|90 | 9.18 | 0.99 | 0.62 | 0.05 |
| gubernatorial | -1.94 | [-6.37:2.64] | 0.42 | 6.03 | 61\|61 | 6.56 | 0.97 | 0.50 | 0.21 |
| mayoral | 4.62 | [1.55:8.28] | 0.00 | 5.54 | 117\|117 | 6.32 | 1.00 | 0.83 | 0.99 |
| subnational (Brazil) | 2.19 | [-0.70:5.58] | 0.13 | 6.08 | 147\|147 | 6.59 | 1.00 | 0.89 | 0.56 |
| subnational (¬ Brazil) | 4.26 | [-0.94:10.45] | 0.10 | 5.33 | 43\|43 | 5.69 | 0.84 | 0.31 | 0.59 |

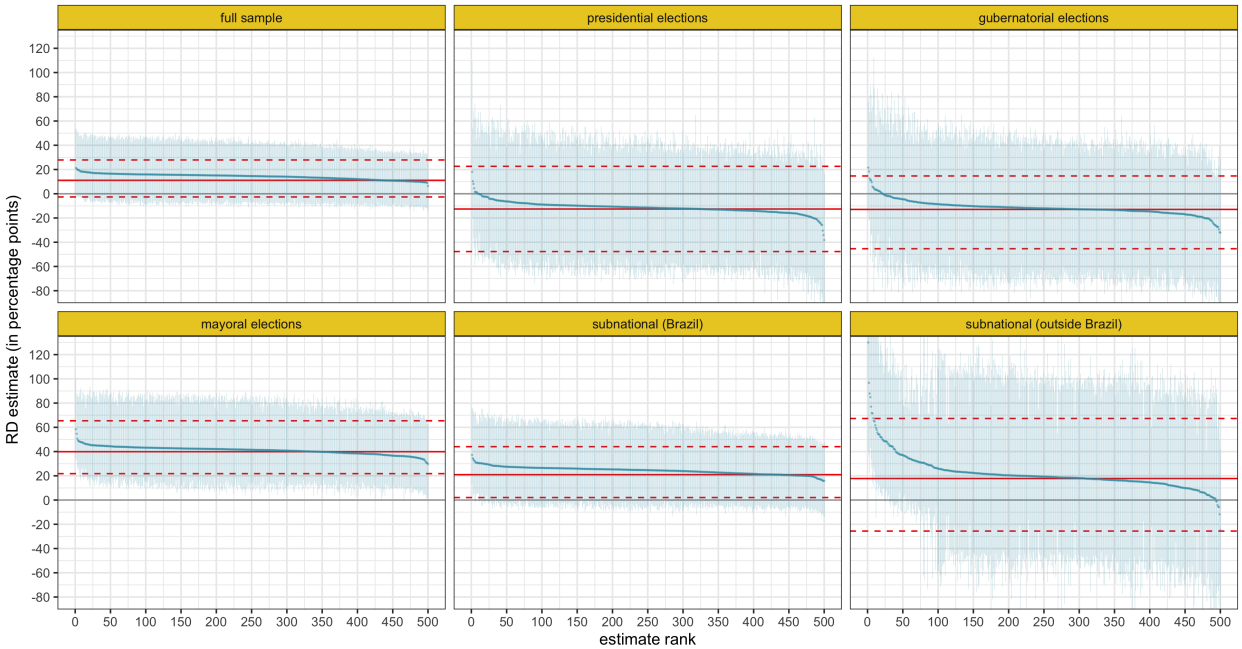
Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the CER-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample sizes.

Table A10: RD estimates: Second-order polynomials

| | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | 18.64 | [-0.65:41.59] | 0.06 | 10.70 | 399\|399 | 48.30 | 1.00 | 0.86 | 0.65 |
| presidential | -11.01 | [-57.58:38.66] | 0.70 | 12.64 | 120\|120 | 49.64 | 0.77 | 0.27 | 0.09 |
| gubernatorial | -16.22 | [-55.54:20.59] | 0.37 | 10.01 | 86\|86 | 49.42 | 0.94 | 0.41 | 0.21 |
| mayoral | 47.49 | [22.43:78.75] | 0.00 | 11.02 | 206\|206 | 47.14 | 0.99 | 0.59 | 0.99 |
| subnational (Brazil) | 26.74 | [0.53:56.51] | 0.05 | 13.05 | 259\|259 | 47.05 | 0.99 | 0.59 | 0.70 |
| subnational (¬ Brazil) | 24.19 | [-40.48:90.45] | 0.45 | 11.03 | 65\|65 | 48.19 | 0.49 | 0.16 | 0.16 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | |
| full sample | 2.35 | [-0.52:5.54] | 0.10 | 9.43 | 365\|366 | 7.46 | 1.00 | 0.91 | 0.55 |
| presidential | 0.26 | [-6.99:7.00] | 1.00 | 11.70 | 114\|115 | 9.91 | 0.96 | 0.47 | 0.05 |
| gubernatorial | -2.58 | [-7.53:1.60] | 0.20 | 8.69 | 77\|77 | 7.51 | 0.99 | 0.60 | 0.33 |
| mayoral | 5.58 | [2.33:9.75] | 0.00 | 11.23 | 210\|210 | 6.57 | 0.99 | 0.60 | 0.97 |
| subnational (Brazil) | 2.42 | [-0.51:6.14] | 0.10 | 13.19 | 261\|261 | 6.98 | 1.00 | 0.78 | 0.47 |
| subnational (¬ Brazil) | 4.54 | [-3.12:11.29] | 0.27 | 8.24 | 53\|53 | 6.74 | 0.72 | 0.25 | 0.40 |

Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Observations are clustered by election. The estimates are calculated by fitting a separate second-order polynomial regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample sizes.

**(a) DV: *winner* (0/100)**




**(b) DV: *vote share*$_{R2}$ (0:100)**

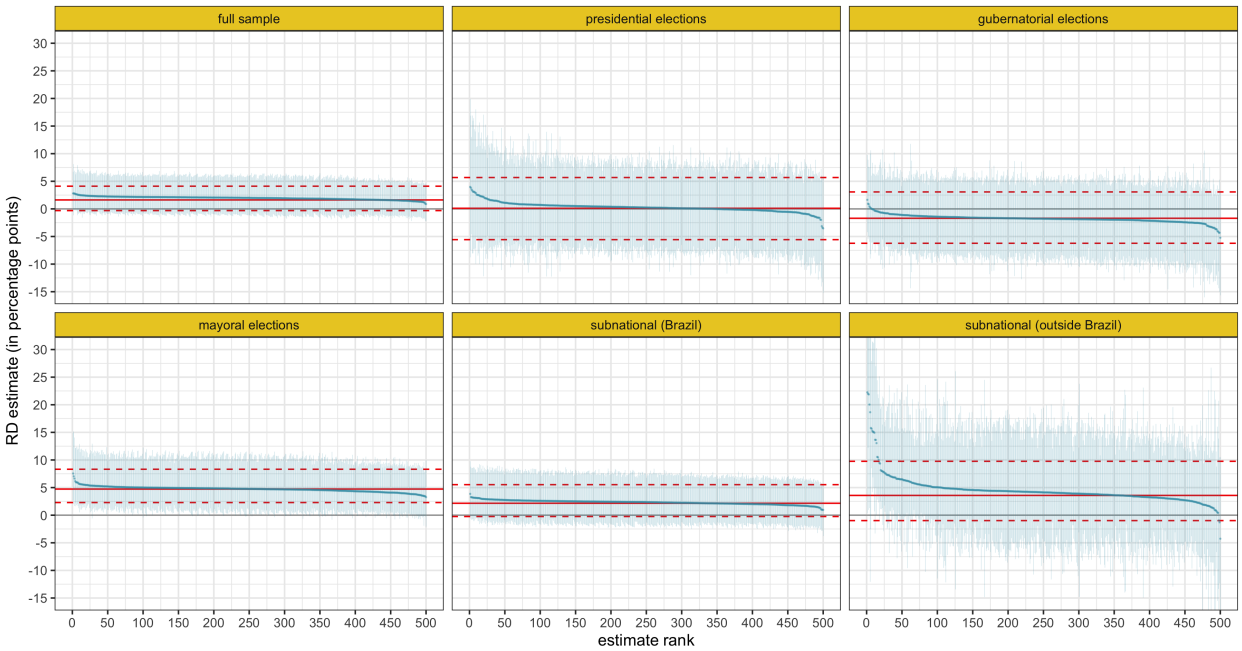



Figure A17: Results with one candidate per election. The red horizontal lines report the RD estimates and robust 95% confidence intervals reported in Table 2, which include two candidates per election. The blue vertical lines report the same estimates from 500 samples in which we randomly selected one candidate –either the first-placed or the runner-up– from every election. To facilitate comparison, these estimates are ranked form highest to lowest in size.

Table A11: Heterogeneous effects: Open seat races

| | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | 11.25 | [-5.90:29.49] | 0.19 | 11.36 | 334\|334 | 47.87 | 1.00 | 0.96 | 0.42 |
| presidential | -14.66 | [-53.45:26.85] | 0.52 | 11.33 | 78\|78 | 49.51 | 0.93 | 0.40 | 0.17 |
| gubernatorial | -24.88 | [-65.20:4.58] | 0.09 | 8.75 | 64\|64 | 49.78 | 0.98 | 0.51 | 0.51 |
| mayoral | 37.42 | [16.11:64.07] | 0.00 | 9.93 | 165\|165 | 46.84 | 1.00 | 0.77 | 0.99 |
| subnational (Brazil) | 19.09 | [-5.97:44.74] | 0.13 | 11.42 | 195\|195 | 47.61 | 1.00 | 0.74 | 0.55 |
| subnational (¬ Brazil) | 21.74 | [-22.30:69.01] | 0.32 | 9.79 | 55\|55 | 47.99 | 0.83 | 0.31 | 0.26 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | |
| full sample | 1.52 | [-1.00:4.48] | 0.21 | 9.67 | 296\|297 | 7.45 | 1.00 | 0.96 | 0.34 |
| presidential | -0.61 | [-7.82:6.96] | 0.91 | 11.56 | 77\|78 | 9.31 | 0.94 | 0.42 | 0.06 |
| gubernatorial | -2.92 | [-9.00:3.45] | 0.38 | 8.75 | 64\|64 | 7.88 | 0.94 | 0.42 | 0.26 |
| mayoral | 4.50 | [1.59:8.28] | 0.00 | 8.68 | 151\|151 | 6.36 | 1.00 | 0.75 | 0.96 |
| subnational (Brazil) | 1.77 | [-1.55:5.60] | 0.27 | 9.71 | 178\|178 | 6.88 | 1.00 | 0.76 | 0.28 |
| subnational (¬ Brazil) | 3.97 | [-0.40:10.33] | 0.07 | 6.91 | 46\|46 | 6.31 | 0.90 | 0.37 | 0.54 |

Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to (i) elections requiring a runoff in which (ii) neither the first- nor the second-placed candidate in the first round was the incumbent at the time of the election. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample size.

Table A12: Heterogeneous effects: Previous experience (Brazil only)

| | | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | experienced | estim. | 95% CI | *p*-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| mayoral (brazil) | neither | 41.96 | [9.74:84.07] | 0.01 | 8.86 | 96\|96 | 48.08 | 0.95 | 0.43 | 0.88 |
| | neither/both | 57.58 | [31.67:95.41] | 0.00 | 7.52 | 91\|91 | 47.27 | 0.98 | 0.54 | 1.00 |
| | first | 6.28 | [-59.16:79.20] | 0.78 | 12.58 | 35\|35 | 49.02 | 0.50 | 0.17 | 0.06 |
| | second | -22.49 | [-99.09:43.83] | 0.45 | 10.48 | 23\|23 | 49.90 | 0.49 | 0.16 | 0.14 |
| subnational (Brazil) | neither | 16.77 | [-13.00:47.30] | 0.26 | 10.49 | 149\|149 | 48.03 | 0.99 | 0.60 | 0.34 |
| | neither/both | 32.99 | [10.05:62.15] | 0.01 | 9.22 | 155\|155 | 47.37 | 1.00 | 0.71 | 0.94 |
| | first | -13.81 | [-78.22:49.35] | 0.66 | 13.37 | 43\|43 | 48.91 | 0.57 | 0.19 | 0.09 |
| | second | -8.65 | [-69.23:42.65] | 0.64 | 10.97 | 30\|30 | 47.95 | 0.66 | 0.22 | 0.07 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | | |
| mayoral (brazil) | neither | 5.32 | [0.17:12.00] | 0.04 | 8.73 | 94\|94 | 6.58 | 0.87 | 0.34 | 0.70 |
| | neither/both | 6.55 | [2.53:12.04] | 0.00 | 8.22 | 99\|99 | 6.49 | 0.97 | 0.47 | 0.97 |
| | first | -4.18 | [-9.52:3.24] | 0.33 | 11.10 | 33\|33 | 7.07 | 0.87 | 0.34 | 0.44 |
| | second | -2.80 | [-16.82:11.39] | 0.71 | 9.50 | 23\|23 | 7.16 | 0.29 | 0.11 | 0.09 |
| subnational (Brazil) | neither | 1.99 | [-2.19:7.06] | 0.30 | 9.19 | 137\|137 | 6.90 | 0.99 | 0.54 | 0.22 |
| | neither/both | 3.38 | [0.38:7.45] | 0.03 | 8.11 | 141\|141 | 6.83 | 1.00 | 0.76 | 0.75 |
| | first | -5.66 | [-11.37:1.08] | 0.11 | 11.94 | 38\|38 | 6.74 | 0.85 | 0.32 | 0.71 |
| | second | -0.45 | [-9.44:10.10] | 0.95 | 10.00 | 28\|28 | 6.86 | 0.49 | 0.16 | 0.05 |

Sharp (conventional) RD estimates, with robust CIs and *p*-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to subnational elections in Brazil requiring a runoff. The experience variable indicates whether the sample was restricted to elections where, respectively, none of the top-two vote getters in the first round had previous elected experience; neither or both had (i.e., none of the was advantaged and disadvantaged in this regard); only the first-placed had; or only the second-placed had, respectively. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample size.

Table A13: Heterogeneous effects: Left-Right ideological distance (V-Party)

| | | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | id. distance | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | polarized | -3.35 | [-32.19:23.20] | 0.75 | 11.57 | 153\|153 | 48.98 | 1.00 | 0.69 | 0.06 |
| | close | 17.72 | [-8.63:41.40] | 0.20 | 10.65 | 134\|134 | 46.25 | 1.00 | 0.73 | 0.50 |
| presidential | polarized | -33.13 | [-100.84:22.41] | 0.21 | 10.14 | 40\|40 | 50.57 | 0.62 | 0.21 | 0.32 |
| | close | -19.52 | [-57.21:16.24] | 0.27 | 10.64 | 37\|37 | 49.77 | 0.96 | 0.47 | 0.31 |
| gubernatorial | polarized | -36.99 | [-112.68:24.98] | 0.21 | 8.99 | 28\|28 | 50.79 | 0.53 | 0.18 | 0.32 |
| | close | 42.72 | [-18.54:110.22] | 0.16 | 8.62 | 25\|25 | 45.83 | 0.51 | 0.17 | 0.45 |
| mayoral | polarized | 22.25 | [-15.81:58.03] | 0.26 | 12.51 | 82\|82 | 45.78 | 0.93 | 0.40 | 0.39 |
| | close | 31.35 | [-9.21:69.60] | 0.13 | 9.75 | 64\|64 | 45.32 | 0.89 | 0.36 | 0.60 |
| subnational (Brazil) | polarized | -11.78 | [-57.02:23.56] | 0.42 | 10.94 | 82\|82 | 48.46 | 0.92 | 0.38 | 0.13 |
| | close | 46.11 | [12.90:85.67] | 0.01 | 7.84 | 72\|72 | 45.10 | 0.93 | 0.40 | 0.94 |
| subnational (¬ Brazil) | polarized | 53.44 | [-34.56:147.38] | 0.22 | 8.34 | 17\|17 | 49.26 | 0.32 | 0.12 | 0.37 |
| | close | 21.90 | [-78.23:117.26] | 0.70 | 9.45 | 13\|13 | 48.04 | 0.28 | 0.10 | 0.10 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | | |
| full sample | polarized | -0.68 | [-4.52:3.74] | 0.85 | 9.86 | 137\|137 | 6.54 | 0.99 | 0.59 | 0.07 |
| | close | 3.52 | [-0.64:7.98] | 0.09 | 9.36 | 120\|120 | 7.02 | 0.99 | 0.62 | 0.62 |
| presidential | polarized | -3.48 | [-7.42:-0.56] | 0.02 | 7.49 | 36\|36 | 6.47 | 1.00 | 0.74 | 0.80 |
| | close | 0.93 | [-9.66:11.73] | 0.85 | 8.79 | 30\|30 | 10.53 | 0.78 | 0.28 | 0.06 |
| gubernatorial | polarized | -6.47 | [-16.27:3.18] | 0.19 | 10.38 | 30\|30 | 8.06 | 0.63 | 0.21 | 0.45 |
| | close | 3.06 | [-0.79:8.45] | 0.10 | 5.70 | 18\|18 | 4.35 | 0.74 | 0.26 | 0.45 |
| mayoral | polarized | 3.66 | [-1.86:10.69] | 0.17 | 9.41 | 67\|67 | 5.52 | 0.68 | 0.23 | 0.37 |
| | close | 4.75 | [-1.02:10.84] | 0.10 | 9.58 | 64\|64 | 5.91 | 0.79 | 0.28 | 0.60 |
| subnational (Brazil) | polarized | -0.06 | [-5.82:6.90] | 0.87 | 10.95 | 82\|82 | 6.71 | 0.83 | 0.31 | 0.05 |
| | close | 3.65 | [-1.49:8.92] | 0.16 | 8.49 | 77\|77 | 6.51 | 0.93 | 0.41 | 0.49 |
| subnational (¬ Brazil) | polarized | 6.57 | [-7.80:21.91] | 0.35 | 9.28 | 17\|17 | 5.78 | 0.19 | 0.08 | 0.23 |
| | close | 5.71 | [0.38:12.34] | 0.04 | 5.49 | 10\|10 | 2.86 | 0.26 | 0.10 | 0.75 |

Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Polarized (respectively, close) elections are those in which the absolute ideological distance along the V-Party Left-Right dimension between the top-two vote getters in the first round was larger (smaller) than the median for each sample. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample size.

Table A14: Heterogeneous effects: (Il)Liberalism ideological distance

| | | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | id. distance | estim. | 95% CI | *p*-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | polarized | -3.46 | [-33.13:20.48] | 0.64 | 10.06 | 126\|126 | 48.76 | 1.00 | 0.71 | 0.06 |
| | close | 14.76 | [-8.87:39.93] | 0.21 | 9.68 | 137\|137 | 47.14 | 1.00 | 0.76 | 0.39 |
| presidential | polarized | -51.83 | [-126.14:5.27] | 0.07 | 8.95 | 36\|36 | 50.40 | 0.57 | 0.19 | 0.59 |
| | close | -4.66 | [-50.78:48.96] | 0.97 | 11.42 | 39\|39 | 50.50 | 0.80 | 0.29 | 0.06 |
| gubernatorial | polarized | -4.46 | [-77.06:64.92] | 0.87 | 8.91 | 22\|22 | 50.96 | 0.51 | 0.17 | 0.05 |
| | close | 18.82 | [-41.69:86.12] | 0.50 | 8.88 | 31\|31 | 47.52 | 0.54 | 0.18 | 0.13 |
| mayoral | polarized | 24.15 | [-12.11:57.86] | 0.20 | 11.06 | 71\|71 | 47.64 | 0.97 | 0.47 | 0.48 |
| | close | 28.51 | [-5.06:61.97] | 0.10 | 10.60 | 70\|70 | 44.79 | 0.96 | 0.46 | 0.65 |
| subnational (Brazil) | polarized | 5.88 | [-31.46:36.30] | 0.89 | 11.01 | 86\|86 | 47.94 | 0.98 | 0.50 | 0.08 |
| | close | 25.82 | [-5.98:64.45] | 0.10 | 9.67 | 80\|80 | 45.55 | 0.95 | 0.43 | 0.53 |
| subnational (¬ Brazil) | polarized | 44.44 | [-34.19:114.04] | 0.29 | 9.64 | 14\|14 | 36.31 | 0.27 | 0.10 | 0.38 |
| | close | 12.56 | [-116.28:130.93] | 0.91 | 9.28 | 17\|17 | 51.45 | 0.21 | 0.09 | 0.06 |
| (b) DV: *vote share*$_{R2}$ (0:100) | | | | | | | | | | |
| full sample | polarized | 0.33 | [-3.53:4.42] | 0.83 | 10.69 | 133\|133 | 8.49 | 1.00 | 0.84 | 0.06 |
| | close | 2.32 | [-1.29:6.79] | 0.18 | 8.35 | 128\|128 | 6.66 | 1.00 | 0.63 | 0.36 |
| presidential | polarized | -2.90 | [-7.11:0.61] | 0.10 | 8.53 | 35\|35 | 8.25 | 1.00 | 0.84 | 0.55 |
| | close | 0.05 | [-8.99:8.42] | 0.95 | 11.35 | 38\|38 | 8.54 | 0.78 | 0.27 | 0.05 |
| gubernatorial | polarized | -3.73 | [-12.08:7.82] | 0.67 | 7.49 | 19\|19 | 8.04 | 0.61 | 0.20 | 0.18 |
| | close | 2.74 | [-0.43:7.26] | 0.08 | 6.29 | 26\|26 | 5.63 | 0.98 | 0.53 | 0.51 |
| mayoral | polarized | 4.08 | [-0.10:8.31] | 0.06 | 10.14 | 67\|67 | 5.87 | 0.97 | 0.49 | 0.77 |
| | close | 4.88 | [-0.60:12.08] | 0.08 | 8.05 | 59\|59 | 5.71 | 0.70 | 0.24 | 0.57 |
| subnational (Brazil) | polarized | -0.12 | [-6.08:5.31] | 0.90 | 10.92 | 85\|85 | 7.68 | 0.96 | 0.46 | 0.05 |
| | close | 4.92 | [-0.02:11.78] | 0.05 | 7.38 | 68\|68 | 5.67 | 0.76 | 0.27 | 0.64 |
| subnational (¬ Brazil) | polarized | 5.08 | [1.14:9.45] | 0.01 | 7.72 | 12\|12 | 2.47 | 0.38 | 0.13 | 0.92 |
| | close | 5.24 | [-14.40:27.52] | 0.54 | 9.78 | 17\|17 | 5.82 | 0.12 | 0.07 | 0.11 |

Sharp (conventional) RD estimates, with robust CIs and *p*-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Polarized (respectively, close) elections are those in which the absolute ideological distance along the (Il)liberalism dimension between the top-two vote getters in the first round was larger (smaller) than the median for each sample. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample size.

Table A15: Heterogeneous effects: Post-Materialism ideological distance

| | | | | | | | | power against | | |
|---|---|---|---|---|---|---|---|---|---|---|
| (a) DV: *winner* (0/100) | id. distance | estim. | 95% CI | $p$-val. | bwd. | $N^-\|N^+$ | $\text{SD}_C$ | $\text{SD}_C$ | $\frac{\text{SD}_C}{2}$ | $\|\hat{\tau}_{\text{RD}}\|$ |
| full sample | polarized | -6.92 | [-39.32:17.10] | 0.44 | 10.03 | 141\|141 | 48.61 | 1.00 | 0.67 | 0.10 |
| | close | 20.36 | [-6.21:52.50] | 0.12 | 10.32 | 132\|132 | 47.04 | 0.99 | 0.60 | 0.49 |
| presidential | polarized | -23.71 | [-76.73:27.05] | 0.35 | 11.70 | 47\|47 | 50.25 | 0.77 | 0.27 | 0.24 |
| | close | -30.16 | [-102.85:29.14] | 0.27 | 9.29 | 30\|30 | 50.74 | 0.57 | 0.19 | 0.24 |
| gubernatorial | polarized | -62.95 | [-138.31:-16.85] | 0.01 | 5.81 | 22\|22 | 51.18 | 0.65 | 0.22 | 0.82 |
| | close | 40.97 | [-14.47:104.87] | 0.14 | 8.30 | 22\|22 | 45.58 | 0.56 | 0.19 | 0.48 |
| mayoral | polarized | 4.03 | [-44.65:40.17] | 0.92 | 9.64 | 65\|65 | 46.51 | 0.86 | 0.33 | 0.06 |
| | close | 45.47 | [14.95:83.76] | 0.00 | 9.88 | 68\|68 | 45.20 | 0.95 | 0.44 | 0.96 |
| subnational (Brazil) | polarized | -16.23 | [-60.26:14.94] | 0.24 | 9.44 | 80\|80 | 48.72 | 0.95 | 0.43 | 0.22 |
| | close | 49.76 | [23.41:86.67] | 0.00 | 8.46 | 72\|72 | 45.10 | 0.98 | 0.51 | 0.99 |
| subnational (¬ Brazil) | polarized | -15.76 | [-126.13:79.75] | 0.66 | 5.63 | 14\|14 | 49.72 | 0.27 | 0.10 | 0.07 |
| | close | 102.12 | [83.86:148.94] | 0.00 | 6.50 | 10\|10 | 42.16 | 0.95 | 0.43 | 1.00 |
| (b) DV: *vote share*$_{\text{R2}}$ (0:100) | | | | | | | | | | |
| full sample | polarized | 0.48 | [-3.05:4.01] | 0.79 | 10.41 | 143\|143 | 7.23 | 1.00 | 0.81 | 0.07 |
| | close | 2.38 | [-1.87:7.62] | 0.23 | 9.46 | 119\|119 | 6.63 | 0.97 | 0.49 | 0.28 |
| presidential | polarized | 2.55 | [-4.79:9.64] | 0.51 | 10.95 | 45\|45 | 9.42 | 0.95 | 0.44 | 0.17 |
| | close | -7.68 | [-18.45:2.56] | 0.14 | 8.12 | 27\|27 | 7.34 | 0.49 | 0.16 | 0.53 |
| gubernatorial | polarized | -8.17 | [-24.16:4.96] | 0.20 | 8.10 | 30\|30 | 8.20 | 0.35 | 0.12 | 0.34 |
| | close | 2.87 | [-0.44:7.61] | 0.08 | 6.25 | 18\|18 | 4.61 | 0.89 | 0.35 | 0.51 |
| mayoral | polarized | 1.38 | [-3.81:6.52] | 0.61 | 8.58 | 56\|56 | 5.48 | 0.84 | 0.31 | 0.11 |
| | close | 5.99 | [1.29:11.88] | 0.01 | 10.13 | 68\|68 | 6.20 | 0.90 | 0.37 | 0.88 |
| subnational (Brazil) | polarized | -2.12 | [-8.55:3.89] | 0.46 | 10.35 | 84\|84 | 7.28 | 0.90 | 0.37 | 0.16 |
| | close | 6.12 | [2.16:11.60] | 0.00 | 8.37 | 72\|72 | 5.84 | 0.93 | 0.40 | 0.95 |
| subnational (¬ Brazil) | polarized | 0.20 | [-10.77:10.92] | 0.99 | 6.17 | 15\|15 | 4.82 | 0.23 | 0.09 | 0.05 |
| | close | 13.79 | [6.62:27.35] | 0.00 | 5.75 | 9\|9 | 4.53 | 0.23 | 0.09 | 0.96 |

Sharp (conventional) RD estimates, with robust CIs and $p$-values based on the MSE-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Polarized (respectively, close) elections are those in which the absolute ideological distance along the Post-Materialism dimension between the top-two vote getters in the first round was larger (smaller) than the median for each sample. Observations are clustered by election. The estimates are calculated by fitting a separate local linear regression at both sides of the threshold, using a triangular kernel. Reported number of observations indicate the *effective* sample size.

# Appendix References


Bormann, Nils-Christian and Matt Golder. 2013. "Democratic Electoral Systems around the World, 1946–2011." *Electoral Studies* 32(2):360–369.

Calonico, Sebastian, Matías D. Cattaneo and Rocío Titiunik. 2015. "Optimal Data-Driven Regression Discontinuity Plots." *Journal of the American Statistical Association* 110(512):1753–1769.

Cattaneo, Matías D., Rocío Titiunik and Gonzalo Vázquez-Bare. 2016. "Inference in Regression Discontinuity Designs under Local Randomization." *The Stata Journal* 16(2):331–367.

Coppedge, Michael, John Gerring, Carl Henrik Knutsen, Staffan I. Lindberg, Jan Teorell, Nazifa Alizada, David Altman, Michael Bernhard, Agnes Cornell, M. Steven Fish et al. 2021. "V-Dem Dataset v11.1.".
**URL:** https://www.v-dem.net/vdemds.html

de Magalhães, Leandro M., Dominik Hangartner, Salomo Hirvonen, Jaakko Meriläinen, Nelson A. Ruiz and Janne Tukiainen. 2020. "How Much Should We Trust Regression Discontinuity Design Estimates? Evidence from Experimental Benchmarks of the Incumbency Advantage." Unpublished manuscript.

Elkins, Zachary, Tom Ginsburg and James Melton. 2014. "Characteristics of National Constitutions." Dataset.
**URL:** http://www.comprativeconstitutionsproject.org

Herrmann, Michael and Holger Döring. forthcoming. "Party Positions from Wikipedia Classifications of Party Ideology." *Political Analysis* .

Hyde, Susan D. and Nikolay Marinov. 2012. "Which Elections Can Be Lost?" *Political Analysis* 20(2):191–210.

Imbens, Guido and Karthik Kalyanaraman. 2012. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *Review of Economic Studies* 79(3):933–959.

Lührmann, Anna, Nils Düpont, Masaaki Higashijima, Yaman Berker Kavasogly, Kyle L. Marquardt, Michael Bernhard, Holger Döring, Allen Hicken, Melis Laebens et al. 2020. "Varieties of Party Identity and Organization (V-Party) Dataset v.1.".
**URL:** https://www.v-dem.net/vpartyds.html

Tow, Andrés. N.d. "Atlas Electoral de Andy Tow.".
**URL:** https://www.andytow.com/atlas/totalpais/

Vázquez Salguero, David Eduardo. 2013. La Segunda Vuelta Electoral en San Luis Potosí, 1995-2005. In *Política y Elecciones en San Luis Potosí, 1810-2010: De la Diputación Provincial al Spot Político-Electoral Televisivo*, ed. Sergio Alejandro Cañedo Gamboa, María de la Luz Galván Salazar and Pablo Sergio Aispuro Cárdenas. San Luis Potosí: El Colegio de San Luis pp. 113–132.