Does the Early Bird always Get the Worm?
How First-round Victories Affect the Chance of Winning the Second Round

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

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Abstract

Runoff systems allow for a reversion of the first-round results – the most voted candidate in the first round may end up losing the election in the second round. But does winning the first round increase the probability of winning the second? We investigate this question with data from national elections since 1951, as well as subnational elections in Argentina, Bolivia, Brazil, Chile and Mexico. Using a regression discontinuity design, we find that being the most voted candidate in the first round has a substantial effect on the probability of winning the second round in gubernatorial and mayoral races, but in presidential elections the effect is negative – though not statistically significant at conventional levels. The effect is much stronger when the top-two placed candidates are ideologically close, but disappears when the election is polarized. We attribute these differences to the disparate informational environment prevailing in local vs. presidential races.

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

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 (Granzier, Pons and Tricaud 2021) and the US (O'Neill 2007) –in 2021, the Democrats' control of the Senate hinged on two runoffs in Georgia–, since the 1990s it has been widely employed in presidential elections in Latin America, Eastern Europe and Francophone Africa. More than half of such elections have been decided in a second round and, conditional on a runoff being needed, the second-placed candidate emerged victorious around one-third of the time (Figure 1).

The possibility of such *reversions* –meaning 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 –ie, discard *Condorcet losers* (Bordignon, Nannicini and Tabellini, 2016), though this depends on a 50% threshold being used (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 (and, presumably, party elites and campaign donors) prefer higher-ranked candidates over lower-ranked ones, even if there is little difference between them (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 2021; Chun and Larrick forthcoming; Gulzar, Robinson and Ruiz forthcoming).

Sometimes this preference for higher-ranked candidates is consistent with coordination dynamics. If two left-wing candidates face a single right-wing rival in the runoff, coordinating around the higher-ranked leftist makes more sense than risk losing the seat by splitting the left-wing vote (Granzier, Pons and Tricaud 2021). A higher ranking may also increase a candidate's prominence, which in turn attracts attention, media coverage, money –and eventually votes (Anagol and Fujiwara 2016; Gulzar, Robinson and Ruiz forthcoming). Nevertheless, some voters also seem to display an innate preference to support the higher-ranked option – the so-called "bandwagon" effect (Kiss and Simonovits 2014; Morton et al.

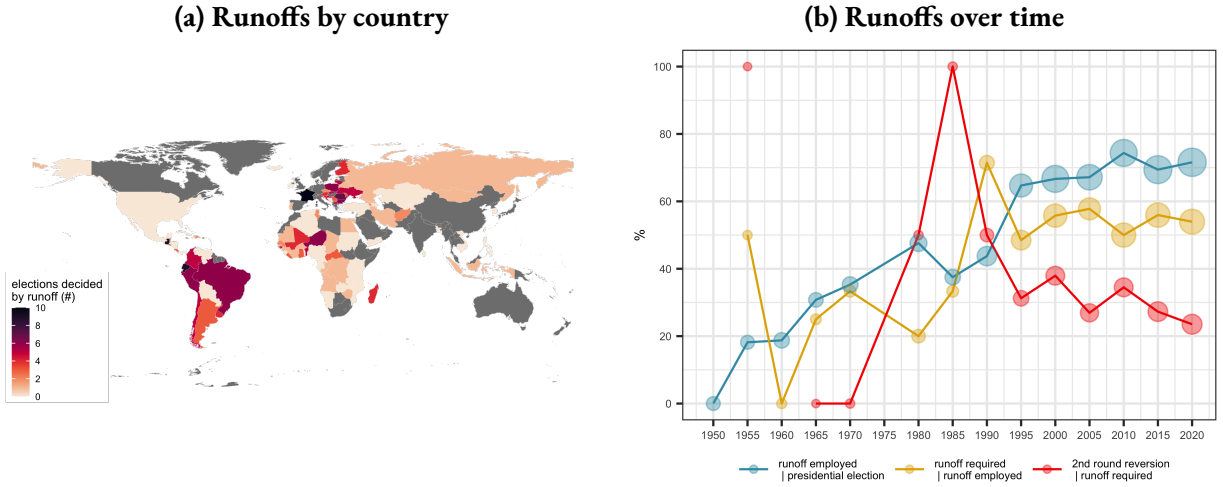

Figure 1: The runoff system in presidential elections, 1951-2020. The dots in panel (b) show averages over the previous five years, with dot sizes proportional to the number of observations.

2015; Hix, Hortala-Vallve and Riambau-Armet 2017; Granzier, Pons and Tricaud 2021; Chun and Larrick forthcoming; though cf. Chatterjee and Kamal 2021).

Besides its impact on elections and representation in two-round systems, this raises the question of when and why such kind of behavior is more common. If bandwagoning happens only rarely, or in less relevant circumstances, its implications are less dire than if we observe the pattern regularly and in high-stakes elections. Elucidating why voters rely on ranks is also important: are rankings the heuristic of choice for distinguishing between candidates, or do they provide voters' last resource when there is no other way to adjudicate between them?

In this paper we address these questions with a sample of runoff elections used to elect executive authorities: presidents in 69 countries around the world, plus governors and mayors in Argentina, Bolivia, Brazil, Chile and Mexico (Table 1). Using a regression discontinuity design to identify the effect of being placed first in the first round, we document three findings. First, rankings matter: finishing first in the first round has a large positive effect on both the probability of winning the election –between 18 and 29 percentage points– and on vote shares –a 3-4 pp. increase.

Second, these results are limited to *subnational* elections: in presidential elections, the effect of finishing first in the first round is negative, though not statistically significant. This could happen because

presidential elections are unbalanced –second-placed candidates are disproportionately more right-wing than first-placed ones–, but it may also reflect the increased attention and media coverage that national elections receive *vis-à-vis* subnational races, devaluing the informative power of first-round results.

Third, and consistent with this interpretation, the results are driven by ideologically non-polarized elections: when the top-two placed candidates are far away ideologically, the advantage of being placed first is substantially diminished, and becomes statistically insignificant. If they are ideologically close, in contrast, the first-round advantage increases to a whopping 21-65 pp., depending on the sample. This is consistent with the claim that ideological polarization increases turnout (Muñoz and Meguid 2021). Again, presidential elections are the exception.

Our results can thus be seen as an extension of Granzier, Pons and Tricaud's (2021) groundbreaking contribution, though with two important differences. First, while these authors look at legislative elections in France and other 19 (mostly European) countries, our analysis focuses on executive elections. Second, electoral rules in France permit more than two candidates to participate in the second round, which allows Granzier, Pons and Tricaud (2021) 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. For example, if a single right-wing contender faces two left-wing ones in the runoff, both left-wing voters and elites have a strong incentive to coordinate behind the highest-ranked leftist –either by voting for her or by pressuring her rival to withdraw (Granzier, Pons and Tricaud 2021). In executive elections, in contrast, typically only two contenders may participate in the second round, and withdrawals are rare –less than 10 (out of 656) runoff races in our sample featured one. Since two-candidate elections offer no possibility to vote strategically, our results can thus be read as supporting a pure bandwagon effect, in line with a novel experimental literature on the topic (Hix, Hortala-Vallve and Riambau-Armet 2017, though this also applies to some elections in France; see Granzier, Pons and Tricaud 2021).

Furthermore, the effect sizes we find are much larger than Granzier, Pons and Tricaud's (2021), suggesting that rankings matter more when party labels convey less information about candidates' ideolog-

ical positions (see also Gulzar, Robinson and Ruiz forthcoming). That said, the effect weakens considerably when candidates' ideological positions are close to each other, suggesting that rankings are just one among multiple pieces of information that 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 the effect disappears and even reverses in presidential elections, which typically receive substantial media coverage, with candidates' personalities, vita, and stances being scrutinized for weeks, if not months.

Data and Research Design

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

1. The most voted candidate needs to obtain a minimum percentage of votes –typically 50%– in order to win outright in the first round, though lower thresholds (e.g., 40%) are sometimes observed ; and
2. In case no candidate is victorious in the first round, the same electorate must choose between the top two (or more) contenders in a second round.[1]

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 (mainly in Mexico in 2003) in which a second round could be avoided if turnout in the first round was sufficiently high –this emphasizes turnout rather than candidates' vote shares.[2]

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

[1] This requirement excludes cases in which the legislature, rather than citizens, decides among the top two contenders in the first round (e.g., Chile before 1973).

[2] 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.

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	177	84	47.5	30	35.7
Brazil (mayor)	mayor	1996-2020	97	519	300	57.8	75	25.0
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
			300	1250	656	52.5	189	28.8

system. We then restricted the sample to minimally competitive elections (Hyde and Marinov 2012) with a V-Dem (Coppedge et al. 2021) polyarchy score higher than 1/3 (on a 0:1 scale). For subnational elections, we looked at Latin American cases where we know a runoff rule was employed: Argentine, Bolivian, Brazilian and Chilean governors (22 provinces in 1973; 4 provinces since the 1990s); Bolivian governors since 2010; Brazilian 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 –there are just 6 runoffs in Bolivia and 13 in Chile–, we aggregated the data into four (sub)samples:

1. The *full sample* includes all observations ($N_{\text{runoff}} = 656$);
2. The *presidential* sample only includes presidential elections ($N_{\text{runoff}} = 182$);
3. The *gubernatorial & mayoral (Brazil)* sample includes all non-presidential elections in Brazil ($N_{\text{runoff}} = 384$);
4. The *gubernatorial & mayoral (non-Brazil)* sample includes all non-presidential elections outside Brazil ($N_{\text{runoff}} = 90$).

Thus, we can both check whether presidential elections are different and whether the results are driven by subnational elections in Brazil, which constitute the bulk of our non-presidential sample. To code candidates' ideological positions, we relied on the V-Party v.1 dataset, which is based on country expert

ratings (Lührmann et al. 2020). Given that the data is limited to legislative elections, we used the coding from the previous congressional race when executive and legislative elections were not concurrent. For subnational contests, we implicitly assumed parties' national ideology scores hold. Since these are only available for relatively large parties, we only have data for the first two placed candidates for 50-70% of observations (see Figure A3).

Variables. We aggregated the data at both the election and the candidate-election levels. For the former, we recorded the date of both rounds, the runoff rule employed (absolute vs. qualified majority), whether the first-round result required a second round, and whether there was a second-round reversion. At the candidate level, we collected data on party ID, the number and percentage of (valid) votes obtained in each round, first-round rank, ideology score(s) across three dimensions (see below), 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.[3] Tables A1 and A2 report the descriptive statistics measured at the election and the candidate levels, respectively.

We examine two outcomes: *winner*, a dummy that takes the value of 100 if a candidate won the election, and 0 otherwise;[4] and *vote share*$_{R2}$, the candidates' vote % in the second round. We measure candidates' ideological positions across three dimensions: Left-Right; (Il)Liberalism; and Post-Materialism. The former is based on country experts' placement of parties across a seven-point left-right scale, while the latter capture the extent to which parties employ pro-people and anti-elite rhetoric; or advocate secularism and support for women's rights, immigrants and minorities, respectively (see Appendix A1 for details).

[3]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.

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

Identification. We seek to determine whether a candidate enjoys an electoral advantage in the second round *solely by virtue of 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, an it may be these differences, rather than finishing fist in the initial round, that explain their subsequent victory. We thus employ a regression discontinuity (RD) design, comparing candidates who finished first or second by a small margin. The fact that this can only identify local effects is not an issue, as candidates who win the first round comfortably are likely more charismatic, popular, or better funded than their peers, and thus their second-round victory is in a sense "overdetermined." The more empirically interesting –and normatively problematic– question is whether candidates who are not substantially better and/or more popular than their peers enjoy an advantage simply by virtue of finishing first in the first round.

Specifically, we estimate the following equation:

$$Y_{i,e} = \beta_0 + \tau_{\text{RD}} \cdot F_{i,e} + \beta_1 \cdot \text{first round margin}_{i,e} + \beta_2 \cdot F_{i,e} \cdot \text{first round margin}_{i,e} + \varepsilon_{i,e}, \quad (1)$$

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 the most voted candidate and the runner-up in the first round. 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 cut-off more heavily. The effect of interest, $\hat{\tau}_{\text{RD}}$, is the difference between the predicted values of the regression approaching the cutoff from the left and the right. 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.

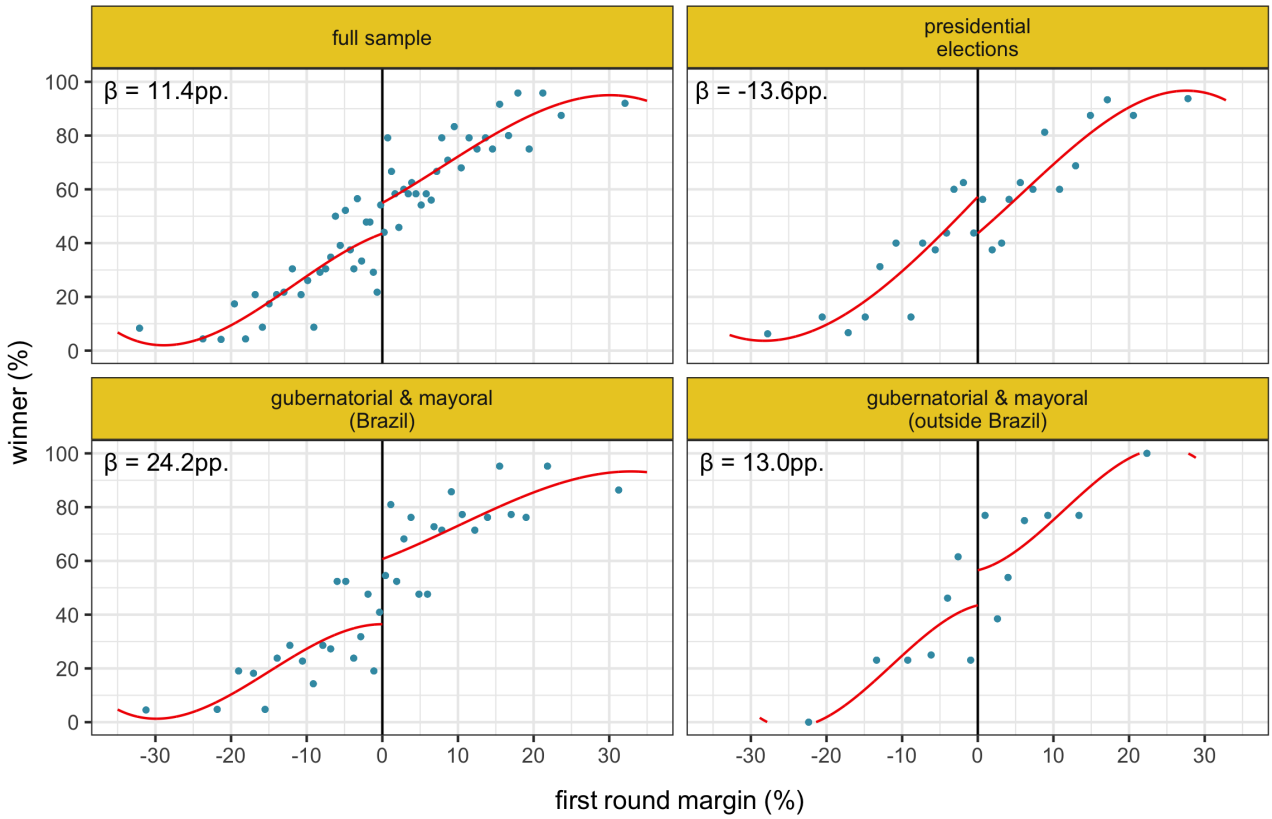

Figure 2: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the probability of winning the election. Red lines show third-order polynomials estimated separately at each side of the cutoff, using a uniform kernel.

Results

Overall effects. The mimicking-variance quantile-spaced RD plots (Calonico, Cattaneo and Titiunik 2015) in Figure 2 show how a candidate's margin of victory in the first round relates to her probability of winning the election. As expected, the relationship is positive: the better a candidate does in the first round, the more likely she is to win overall. However, all plots also show a sharp discontinuity in the probability of victory when *first round margin* equals zero; this is the RD effect. Except for the presidential sample, this jump is positive and large in substantive terms –a 11-24 percentage points increase in the probability of victory attributed solely to the fact that the first-placed candidate finished ahead of the second-placed one. Surprisingly, however, this effect is reversed for presidential elections –a negative 13.6 pp. effect. Figure A8 in the Appendix shows similar effects on candidates' vote percentages in

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

(a) DV: *winner* (0/100)	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	17.84	[4.45:35.97]	0.01	9.28	369|369
presidential elections	-11.42	[-43.83:25.12]	0.59	11.35	122|122
gubernatorial & mayoral (Brazil)	28.95	[10.73:52.90]	0.00	10.06	219|219
gubernatorial & mayoral (outside Brazil)	18.26	[-25.26:69.07]	0.36	9.15	55|55
(b) DV: *vote share*$_{R2}$ (0:100)					
full sample	2.70	[0.43:5.88]	0.02	8.84	352|354
presidential elections	-0.39	[-5.31:5.38]	0.99	11.04	116|117
gubernatorial & mayoral (Brazil)	4.15	[0.69:9.06]	0.02	8.27	188|190
gubernatorial & mayoral (outside Brazil)	3.82	[-0.83:10.15]	0.10	6.51	50|50

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

the second round, though the magnitudes are predictably smaller –between 0.4 and 1.4 pp. Still, when two candidates are otherwise equivalent (on average), even a small increase in vote share can make a large difference on the outcome.

The RD estimates reported in Table 2 tell essentially the same story: candidates who finish first in the first round experience a 17.8 pp. increase in their probability of winning the election overall, a result that is statistically significant at the 0.01 level. This is two to three times as much as the effects reported (for legislative elections) by Granzier, Pons and Tricaud (2021): 5.8 pp. in France and 7.6 pp. elsewhere. However, further inspection shows that they are mainly driven by the Brazilian sample. In presidential contests, the effect is negative and large (-11.4 pp.), but with wide confidence intervals. In subnational elections outside Brazil, the 18.3 pp. effect is insignificant at conventional levels –unsurprisingly given the small sample size. In contrast, in Brazil we observe a whooping and statistically significant 29 pp. effect. This is consistent with Figure 2 as well as with the fact that second-round reversions are more common in presidential elections (31.9%) than in subnational elections in Brazil (27.3%; see Table 1).

The second-round vote percentages reported in Table 2b tell a consistent story: the significant 2.7 pp. increase for the full sample –which is in line with the results reported by Granzier, Pons and Tricaud (2021)– is driven by the Brazilian sample, where there is a large 4.15 pp. increase for the first-placed candi-

date. For subnational elections outside Brazil, the effect is similar in size (3.8 pp.) but it is only significant at the 0.10 level. In contrast, in presidential elections, the effect is essentially zero. Comparing these estimates to the ones reported by Granzier, Pons and Tricaud (2021) 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 in the first place increases a candidate's vote share by between 1.3 and 4.0 pp.

These results are robust to a wide variety of specifications (see Appendix A4). First, Figure A12 shows that except for very small bandwidths –with the concomitant 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. Second, the point estimates remain unchanged –though the overall effect becomes insignificant– if we exclude elections with missing data on ideology. Third, including two observations per election guarantees that all election-level characteristics –such as district characteristics, election year dynamics , runoff thresholds, first-round vote shares for all candidates, and the time lapse between the first and second rounds– are implicitly controlled for.[5] Nonetheless, candidates' characteristics may differ; Table A5 shows that controlling for their ideology does not change the results, either. Nor does employing a CER-optimal rather than a MSERD-optimal bandwidth (Table A6; see de Magalhães et al. 2020) or using a quadratic rather than a linear polynomial (Table A7).

A potentially bigger concern is that candidates' outcomes are not independent: if one wins, the other must lose, and vice versa. Furthermore, their combined 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 that is 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 A13 shows that the resulting estimates and 95% confidence intervals differ little from those reported in Table 2.

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

Heterogeneous effects. What is the source of this first-placement advantage? We investigate one possible mechanism: the ideological distance between the top-two placed candidates in the first round. Intuitively, candidates' placements in the first round constitute just one piece of information that voters take into account when deciding whom to support in the runoff. As a decision rule, opting for the first-placed candidate is both simple and does not require much in terms of information collection. 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 two instances. On the one hand, presidential elections typically attract substantial attention from voters. When candidates' political and personal stances are discussed in the media over several weeks, voters are much more likely to down-weight candidates' placements in the first round and emphasize features like ideology, personal history, or policy stances. Alternatively, voters are also more likely to pay attention when there are obvious political differences between candidates. For example, 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).

To evaluate the role of polarization, we split our samples in two, depending on the ideological distance between the top-two placed candidates 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;[6] 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 (20 pp.) than in polarized elections (8 pp.). The difference is much starker in the Brazilian sample, where an effect of less than a percentage points in polarized elections translates into a whopping 57 pp. advantage if the top two candidates are ideologically similar. The results for

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

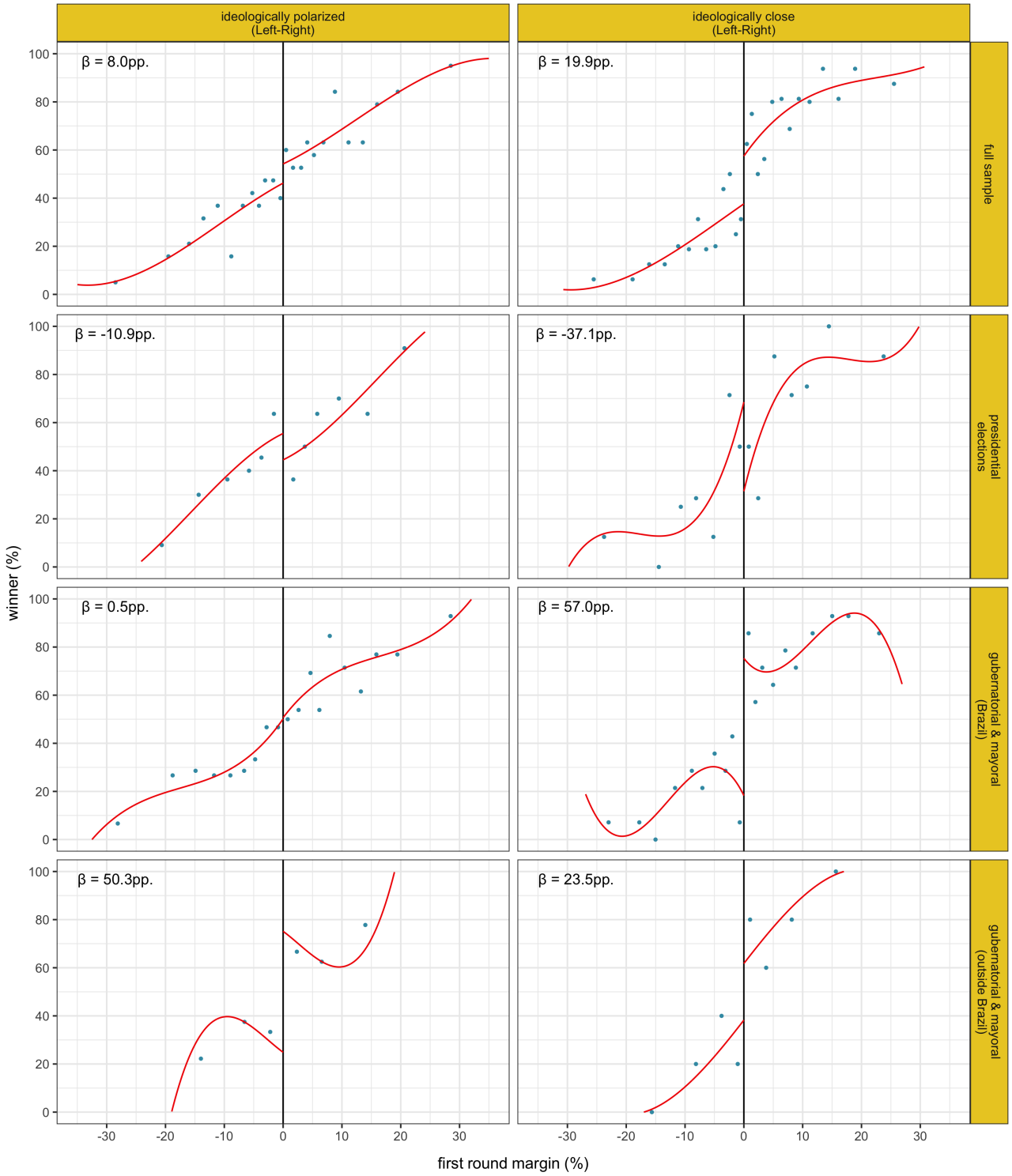

Figure 3: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the probability of winning the election. Polarized (respectively, close) elections are those in which the absolute ideological distance along the Left-Right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

presidential elections and subnational elections outside Brazil go counter to expectations, though given the small sample sizes involved it remains to be seen whether such effect sizes are statistically significant. Figure A9 shows a similar story for vote shares in the second round: an increase from 1 to almost 3 pp. in the entire sample, and a much starker one in Brazil –0.7 to 7.1 pp. Again, the results run counter to expectations in the presidential sample.

We report the RD estimates in Table 3. Despite the much smaller sample sizes –we lose up to 40% of observations to missing values (Figure A3), 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 4.8 to 21.4 pp., the second of which is statistically significant at the 0.11 level. Interestingly, these estimates are roughly similar to those reported by Granzier, Pons and Tricaud (2021) –4.9 and 16.4 pp., respectively. The effect is driven by subnational elections in Brazil, where a negative but small (3.5 pp.) and insignificant effect in polarized elections becomes a whopping 65.5 pp. increases in contests featuring two ideologically close contenders. The fact that such effects are found in a setting in which parties are generally considered weak and nonideological is remarkable, though survey data from presidential elections confirms that (some) Brazilian voters do understand thee logic of strategic voting (Plutowski, Weitz-Shapiro and Winters 2021). In presidential elections, however, the effect remains negative and becomes stronger in ideologically close elections –a 29.4 pp. decrease, though this effect is only statistically significant at the 0.10 level. Outside Brazil, the results also go contrary to expectations –always positive, though stronger in polarized elections– but the small sample sizes involved (17 and 13 elections, respectively) make these estimates unreliable.

Table 3b shows consistent results for the second-round vote %, though most estimates are statistically insignificant at conventional levels. Still, it is worth noting that except in the non-Brazil sample, where we have just 17 elections inside the bandwidth, the point estimates for polarized elections are below 2 percentage points in absolute value, while in contests between ideologically close candidates, the effect ranges between 5.2 and 7.9 pp. – except for presidential elections. Again, these results are robust and consistent with other pieces of evidence. First, Figure A7 shows that second-round reversions tend

Table 3: Heterogeneous effects: Left-Right ideological distance

(a) DV: *winner* (0/100)	id. distance	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	polarized	4.80	[-25.93:34.52]	0.78	11.71	148|148
	close	21.44	[-4.54:46.35]	0.11	10.80	132|132
presidential elections	polarized	-16.59	[-89.87:48.78]	0.56	8.73	37|37
	close	-29.35	[-75.31:7.06]	0.10	10.45	34|34
gubernatorial & mayoral (Brazil)	polarized	-3.50	[-49.07:31.93]	0.68	11.50	79|79
	close	65.47	[38.12:105.13]	0.00	5.69	53|53
gubernatorial & mayoral (outside Brazil)	polarized	53.44	[-34.56:147.38]	0.22	8.34	17|17
	close	20.07	[-82.43:116.09]	0.74	9.15	13|13
(b) DV: *vote share*$_{R2}$ (0:100)						
full sample	polarized	0.84	[-2.98:5.54]	0.56	9.02	126|126
	close	5.18	[-0.36:11.62]	0.07	10.48	127|129
presidential elections	polarized	-1.75	[-6.62:2.05]	0.30	6.73	30|30
	close	0.04	[-12.96:11.77]	0.92	7.49	24|24
gubernatorial & mayoral (Brazil)	polarized	1.43	[-3.67:7.85]	0.48	10.22	73|73
	close	7.94	[-1.81:19.68]	0.10	7.30	64|66
gubernatorial & mayoral (outside Brazil)	polarized	6.57	[-7.80:21.91]	0.35	9.28	17|17
	close	6.08	[0.16:13.18]	0.04	4.75	9|9

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

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 A8 and A9 shows that measuring ideological distance along with the (Il)-Liberalism or the Post-Materialism dimensions results in similar –and sometimes stronger– estimates, even though the correlation between these three measures is far from perfect (see Figure A4).

Conclusion

If voters choose in the second round based on first-round rankings, they do not take full advantage of the chance of reevaluating candidates between rounds. Thus, we set out to examine whether executive candidates who finish first in the first round are more likely to win the election overall. Our findings suggest the existence of a bandwagon effect in subnational races: going to a second round as the first round winner increases the probability of emerging victorious by 29 pp. in gubernatorial and mayoral

elections in Brazil; outside this case, where sample size is much smaller ($N = 55$), we find a positive but statistically insignificant 18 pp. effect. The results for vote shares are consistent. In contrast, in presidential 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, 2021), and extend our knowledge of the effect of rankings on voters' choice to national and subnational executive elections.

In sum, we document that voters have a tendency to bandwagon behind the top-placed candidate in the first round. 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 disappears. 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 2021; 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 2021) or increasing media coverage (Boas and Hidalgo 2011).

Lastly, this study has implications for the electoral system designs 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 subnational 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 modest (Bouton et al. 2022).

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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*.

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

- 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 contests, 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 our three ideology scores: the third is highly negatively and positively correlated with the other two, but the correlation between Left-Right and (Il)Liberalism is modest at best.

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 in which a second round was needed, Figure A1 presents 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, using red dots to identify reversions.

Descriptive by subsample (II): Ideology. Figure A3 presents the proportion of elections with non-missing data on the ideology scores of the first, second, and third-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 three ideology scores. For the subset of elections in which a second round was needed, Figure A5 presents the distribution of (i) our polarization measure – the absolute value of the difference in ideology between the first- and second-placed candidate(s) in the first round – and (ii) the distribution of left-right ideology scores for the first-, second- and third-placed candidates in the first round. Figure A6 presents the corresponding densities for the (il)liberalism and post-materialism ideology scores. Finally, Figure A7 plots the relationship between the (left-right) ideology of the first- and second-placed candidates in the first round, using dots to identify reversions.

Additional RD plots. Figures A8 and A9 present the mimicking variance evenly-spaced RD plots showing the effect of first-round advantage on Δ *vote share* between the first and second rounds.

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)	656	9.78	7.42	0.03	39.06	182	9.98	7.52	0.22	32.81
margin (2vs3) (0/50)	656	13.49	11.50	0.14	48.64	182	11.92	10.17	0.14	46.58
margin (1vs2) (R2) (0/100)	649	14.64	14.61	0.01	100.00	178	16.51	17.12	0.01	100.00
reversion in second round (0/1)	656	0.29	0.45	0.00	1.00	182	0.32	0.47	0.00	1.00
number of candidates (#)	656	8.23	4.45	3.00	39.00	182	11.13	6.32	3.00	39.00
effective number of candidates	656	3.58	1.02	2.04	10.43	182	3.98	1.27	2.12	10.43
|*ideol. distance (1vs2)*| (Left-Right)	436	1.82	1.10	0.00	5.14	117	1.86	1.14	0.00	5.14
|*ideol. distance (1vs2)*| ((Il)Liberalism)	436	0.48	0.44	0.00	2.61	117	0.57	0.66	0.00	2.61
|*ideol. distance (1vs2)*| (Post-Materialism)	436	1.11	0.70	0.00	4.45	117	0.97	0.81	0.00	4.45
absolute majority (0/1)	656	0.90	0.31	0.00	1.00	182	0.95	0.22	0.00	1.00
distance b/w first and second round (days)	649	27.26	16.13	7.00	224.00	178	27.49	23.93	7.00	224.00
V-Dem polyarchy score (0:1)	182	0.69	0.16	0.34	0.91	182	0.69	0.16	0.34	0.91
	(c) Gubernatorial & mayoral (Brazil)					(d) Gub. & mayoral (outside Brazil)				
	N	mean	SD	min	max	*N*	mean	SD	min	max
margin (1vs2) (0/50)	384	10.09	7.53	0.10	39.06	90	8.05	6.53	0.03	28.82
margin (2vs3) (0/50)	384	13.39	11.57	0.14	48.64	90	17.12	12.98	0.37	45.61
margin (1vs2) (R2) (0/100)	384	13.80	13.10	0.09	100.00	87	14.52	15.16	0.24	87.56
reversion in second round (0/1)	384	0.27	0.45	0.00	1.00	90	0.29	0.46	0.00	1.00
number of candidates (#)	384	7.32	2.48	3.00	16.00	90	6.27	3.62	3.00	28.00
effective number of candidates	384	3.48	0.86	2.04	9.25	90	3.20	0.78	2.11	5.71
|*ideol. distance (1vs2)*| (Left-Right)	273	1.83	1.13	0.00	5.00	46	1.64	0.77	0.59	3.92
|*ideol. distance (1vs2)*| ((Il)Liberalism)	273	0.40	0.31	0.00	1.80	46	0.75	0.28	0.05	1.08
|*ideol. distance (1vs2)*| (Post-Materialism)	273	1.18	0.68	0.04	3.42	46	1.05	0.37	0.38	2.40
absolute majority (0/1)	384	1.00	0.00	1.00	1.00	90	0.34	0.48	0.00	1.00
distance b/w first and second round (days)	384	25.08	7.94	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 (II): 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)	678	71.24	45.30	0.00	100.00	678	27.88	44.87	0.00	100.00
Δ *vote share* (-50:100)	670	14.95	11.02	-38.10	76.94	664	16.00	10.48	-24.83	58.76
vote share (0:50)	678	39.56	7.14	18.40	49.98	678	29.66	7.49	10.60	49.30
vote share (R2) (0:100)	670	54.44	9.53	9.78	100.00	664	45.73	9.15	0.00	90.22
ideology (Left-Right)	553	0.20	1.37	-2.61	3.24	540	0.25	1.38	-3.36	3.48
ideology ((Il)Liberalism)	553	0.47	0.68	-2.46	1.28	540	0.52	0.68	-2.43	1.28
ideology (Post-Materialism)	553	0.48	0.95	-2.17	2.29	540	0.46	0.98	-2.17	2.55
(b) Presidential elections										
winner (0/100)	204	68.14	46.71	0.00	100.00	204	31.37	46.51	0.00	100.00
Δ *vote share* (-50:100)	199	17.05	11.99	-38.10	62.33	197	17.87	11.94	-24.83	47.37
vote share (0:50)	204	37.78	7.75	18.40	49.88	204	27.42	7.16	12.40	47.92
vote share (R2) (0:100)	199	54.67	11.31	9.78	100.00	197	45.31	11.20	0.00	90.22
ideology (Left-Right)	160	-0.02	1.28	-2.27	3.24	153	0.20	1.30	-3.36	3.48
ideology ((Il)Liberalism)	160	-0.01	0.83	-2.46	1.19	153	0.01	0.87	-2.43	1.26
ideology (Post-Materialism)	160	0.02	0.93	-2.17	2.29	153	-0.03	0.84	-1.85	2.55
(c) Gubernatorial & mayoral (Brazil)										
winner (0/100)	384	72.92	44.50	0.00	100.00	384	25.78	43.80	0.00	100.00
Δ *vote share* (-50:100)	384	14.31	10.26	-14.19	76.94	381	15.74	9.32	-8.92	48.28
vote share (0:50)	384	40.13	6.78	18.81	49.98	384	30.04	7.27	10.60	49.30
vote share (R2) (0:100)	384	54.43	8.43	25.82	100.00	381	45.84	7.76	14.57	74.18
ideology (Left-Right)	327	0.29	1.43	-2.61	3.24	321	0.18	1.40	-2.61	3.24
ideology ((Il)Liberalism)	327	0.79	0.35	-1.22	1.28	321	0.80	0.40	-1.22	1.28
ideology (Post-Materialism)	327	0.77	0.90	-2.17	2.29	321	0.77	0.95	-2.17	2.29
(d) Gubernatorial & mayoral (outside Brazil)										
winner (0/100)	90	71.11	45.58	0.00	100.00	90	28.89	45.58	0.00	100.00
Δ *vote share* (-50:100)	87	13.00	11.32	-27.51	56.86	86	12.88	11.01	-20.51	58.76
vote share (0:50)	90	41.19	6.48	24.29	49.80	90	33.15	7.60	19.36	47.75
vote share (R2) (0:100)	87	53.96	9.74	14.78	93.78	86	46.20	9.67	6.22	85.22
ideology (Left-Right)	66	0.29	1.26	-2.19	2.21	66	0.72	1.36	-2.19	2.64
ideology ((Il)Liberalism)	66	0.04	0.60	-1.09	1.15	66	0.33	0.55	-1.09	1.15
ideology (Post-Materialism)	66	0.18	0.66	-0.87	1.69	66	0.06	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.

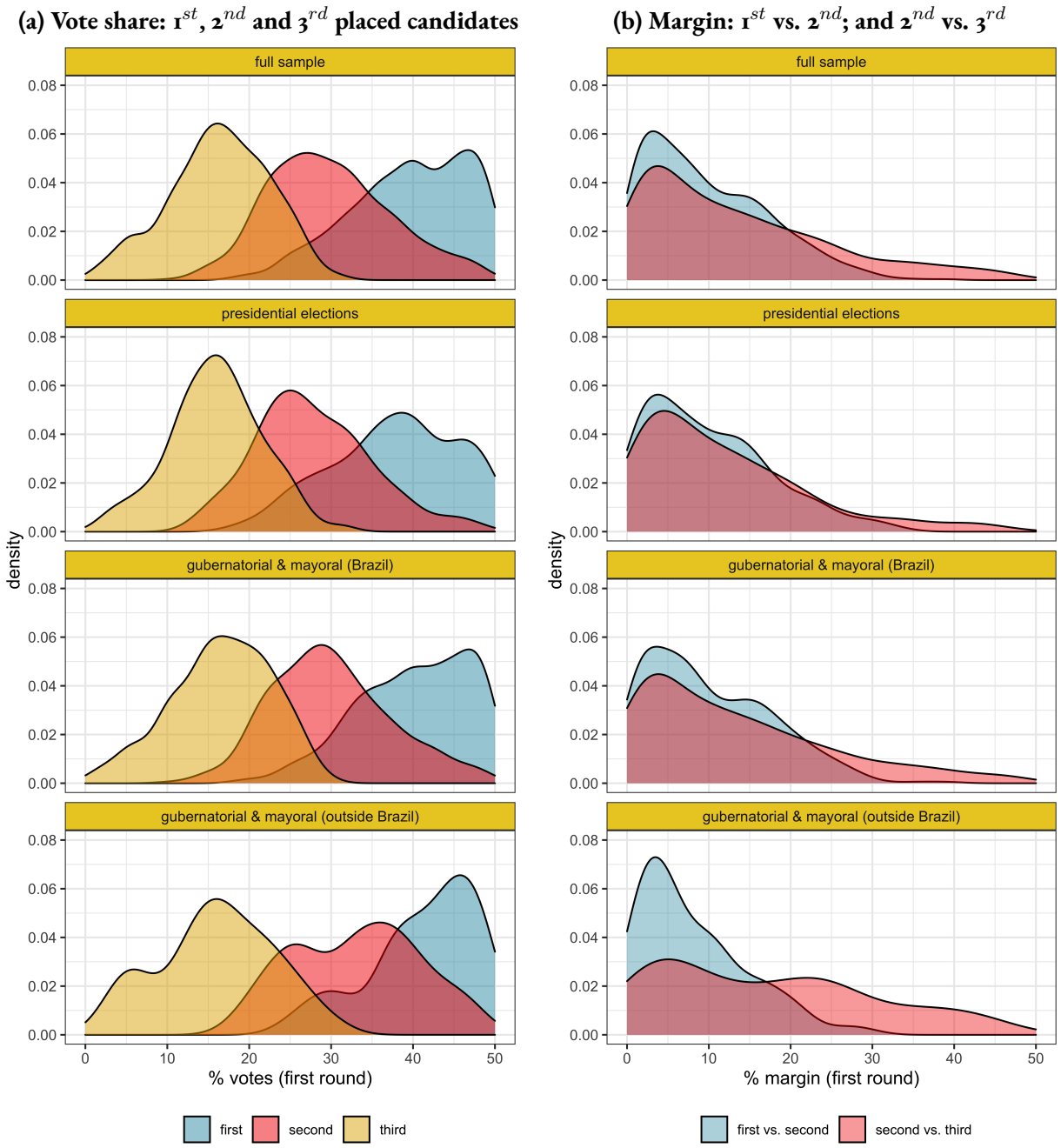


Figure A1: First-round vote shares of the first-, second- and third-placed candidates (left), as well as the first-second and second-third margins (right), by subsample. Only elections in which a second round was needed are included.

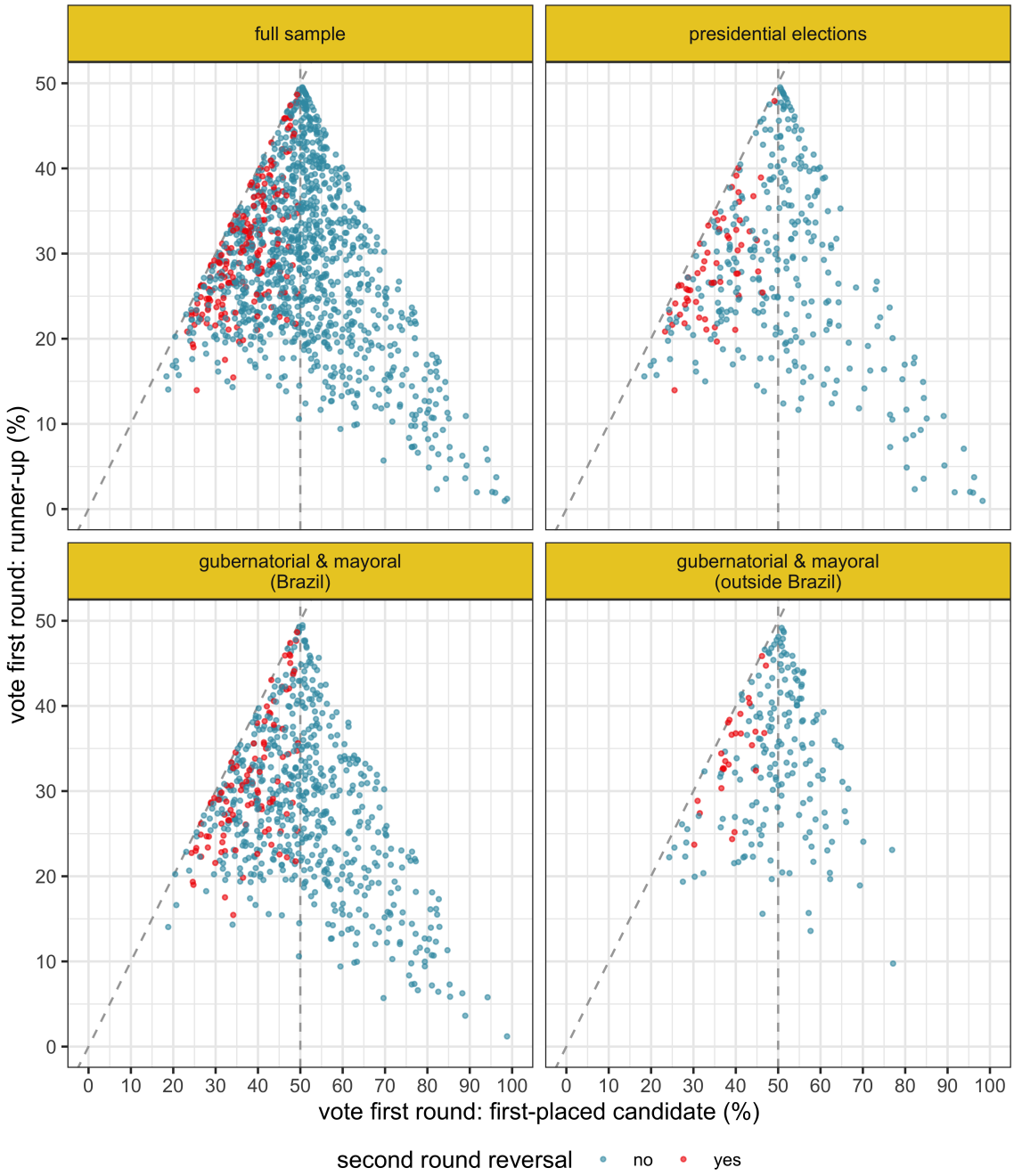


Figure A2: First-round vote shares of the first- and second-placed candidates, by subsample. Elections in which the first-round result was reverted in the runoff are displayed in red.

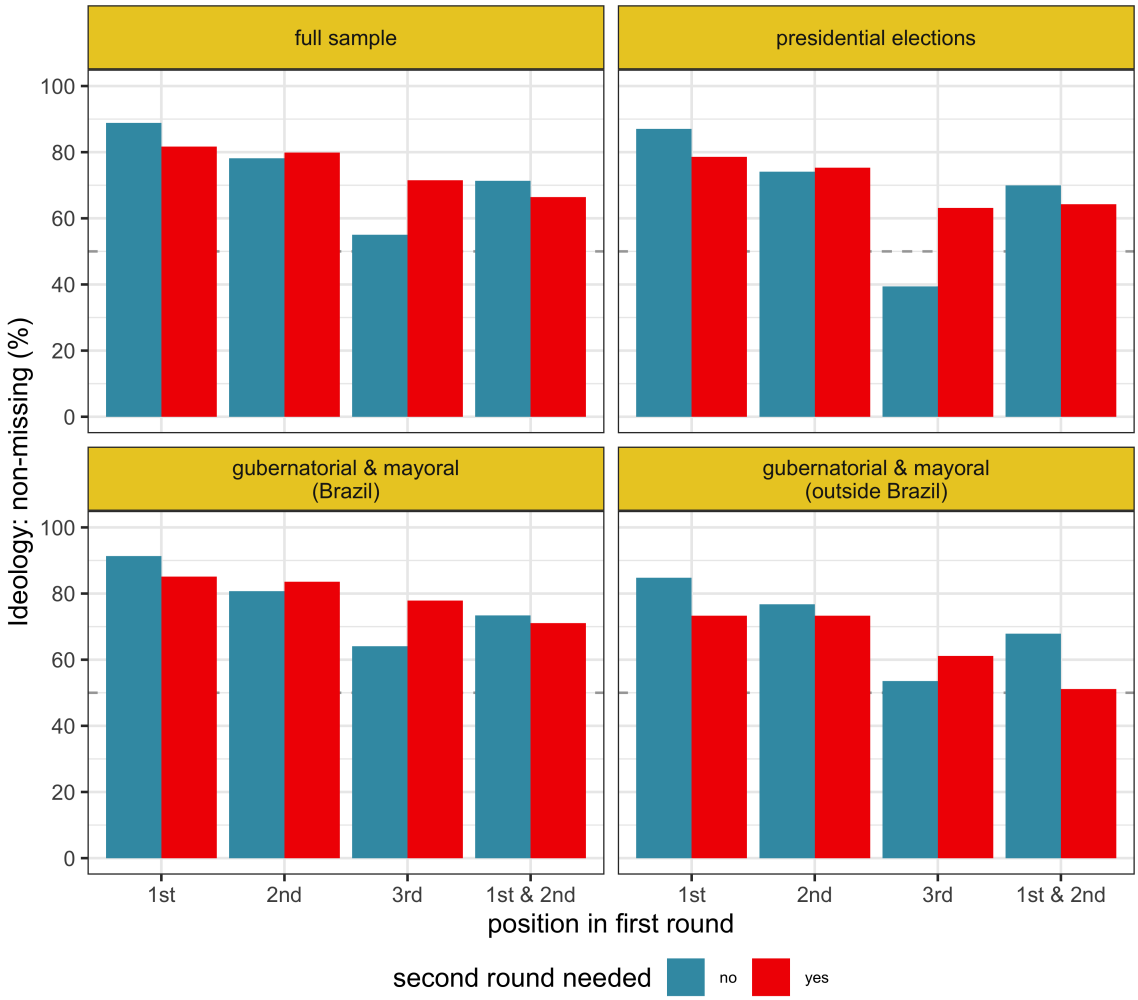


Figure A3: % of candidates with non-missing data on ideology, by subsample, first-round placement, and whether a second round was needed. Ideology is measured on the left-right spectrum, but missingness patterns for alternative measures (illiberalism or post-materialism) differ for no more than a handful of observations.

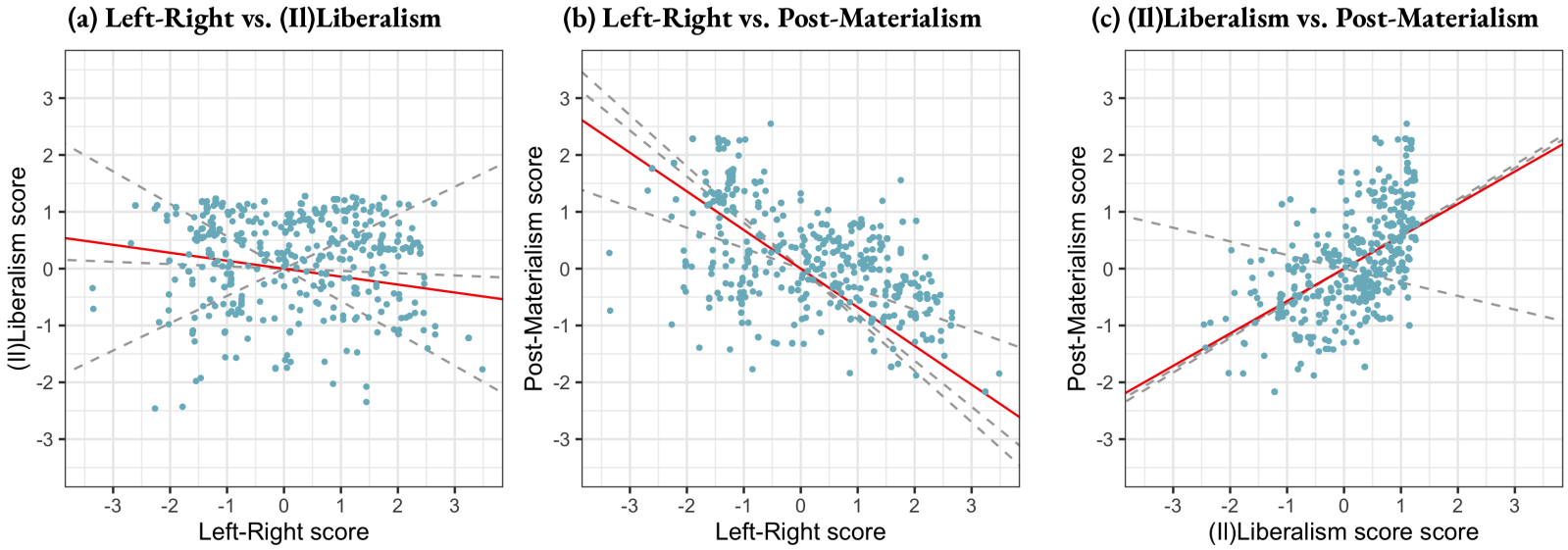


Figure A4: Pairwise relationship between ideology measures. Points represent all observations in the full sample, and the red line indicates the pairwise correlation for that sample. Broken gray lines indicate the pairwise correlation lines in each sample separately. Only elections in which a second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

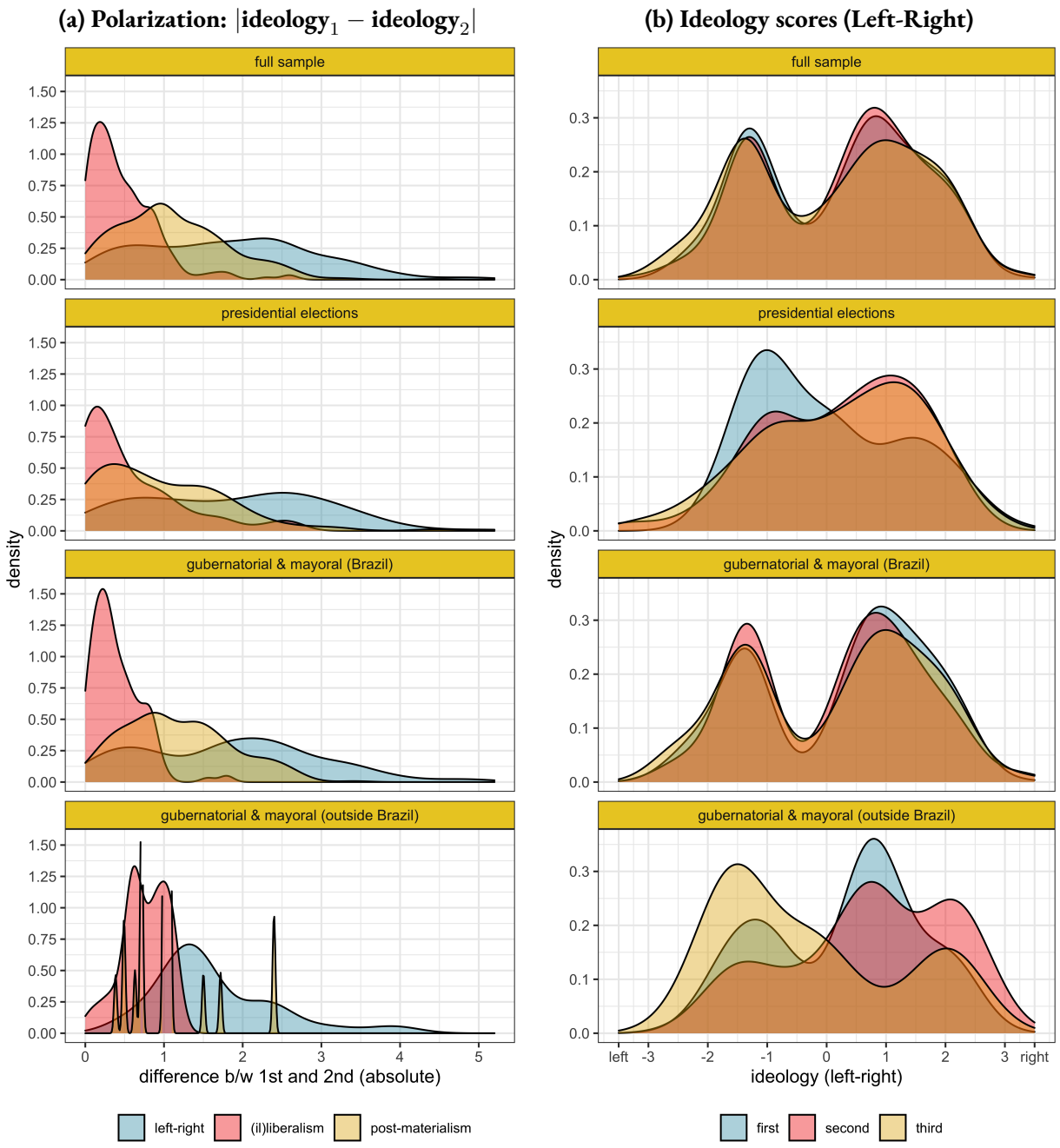


Figure A5: Ideological polarization between the first- and second-placed candidate (left) and distribution of Left-Right ideology scores for the top three placed candidates (right), by subsample. Only elections in which a second round was needed are included.

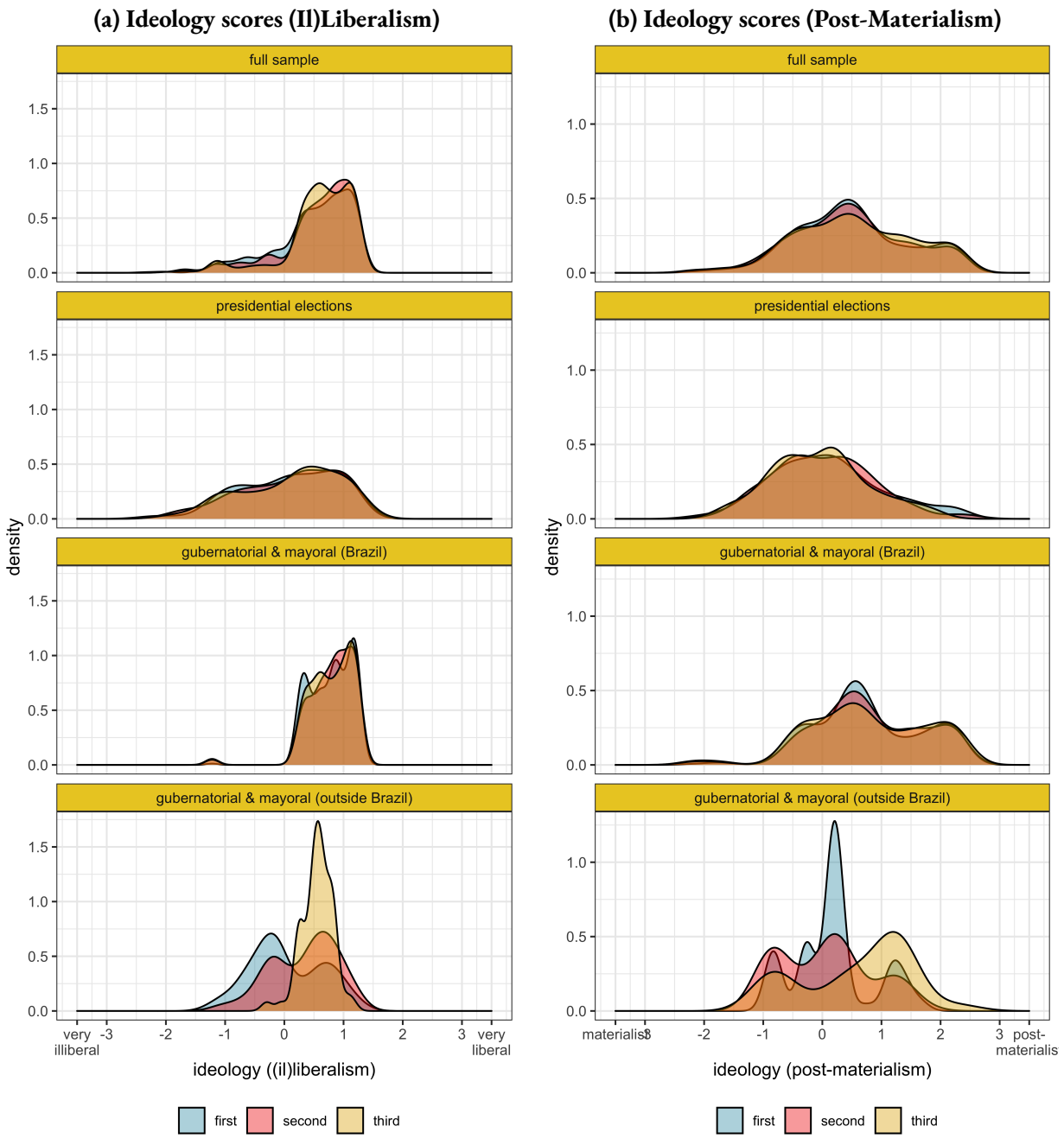


Figure A6: Distribution of (Il)Liberalism (left) and Post-Materialism (right) ideology scores for the top three placed candidates, by subsample. Only elections in which a second round was needed are included.

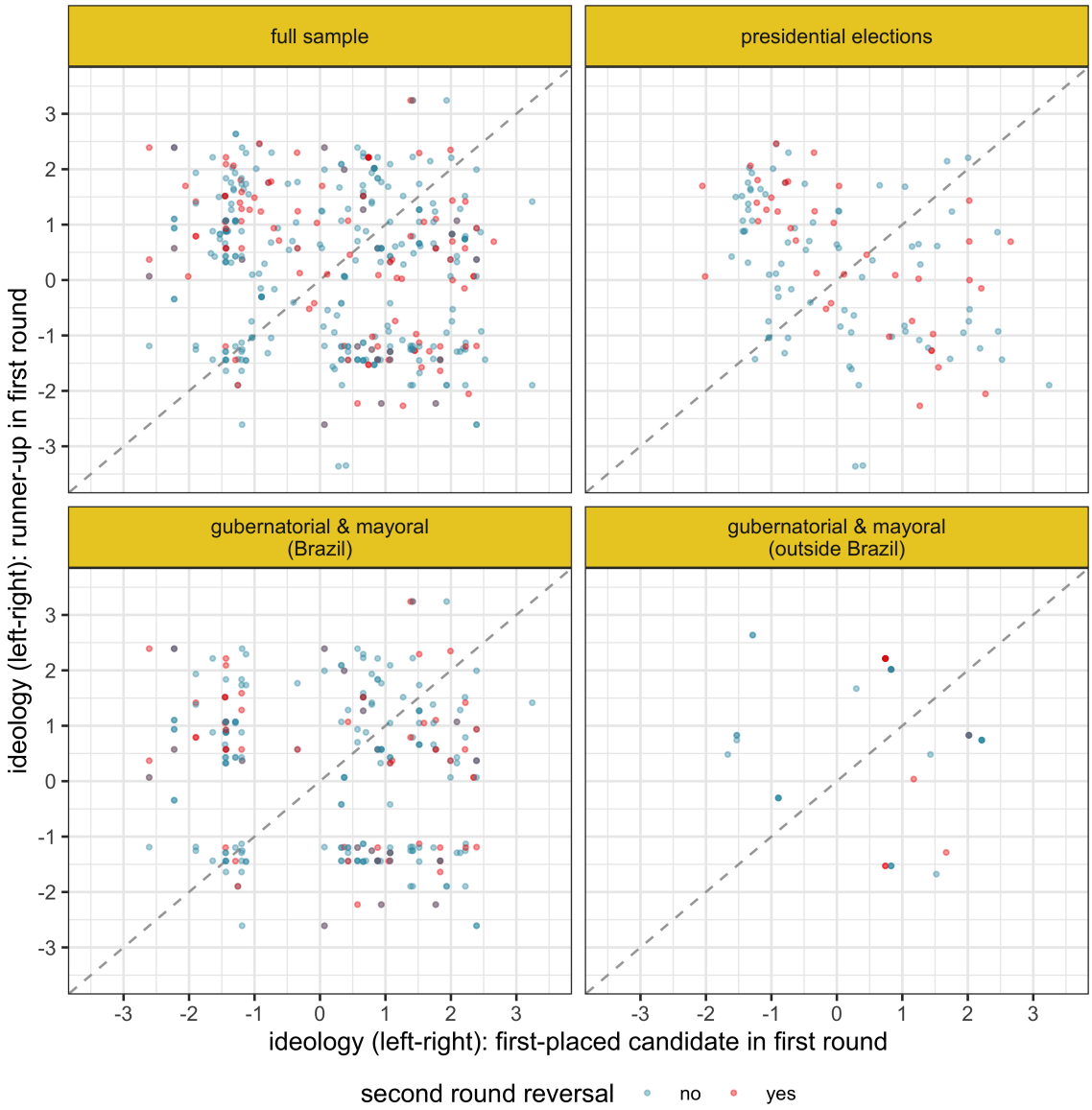


Figure A7: First-round left-right ideology scores of the first- and second-placed candidates, by subsample. Only elections in which a second round was needed are included; those in which the first-round result was reverted in the runoff are displayed in red.

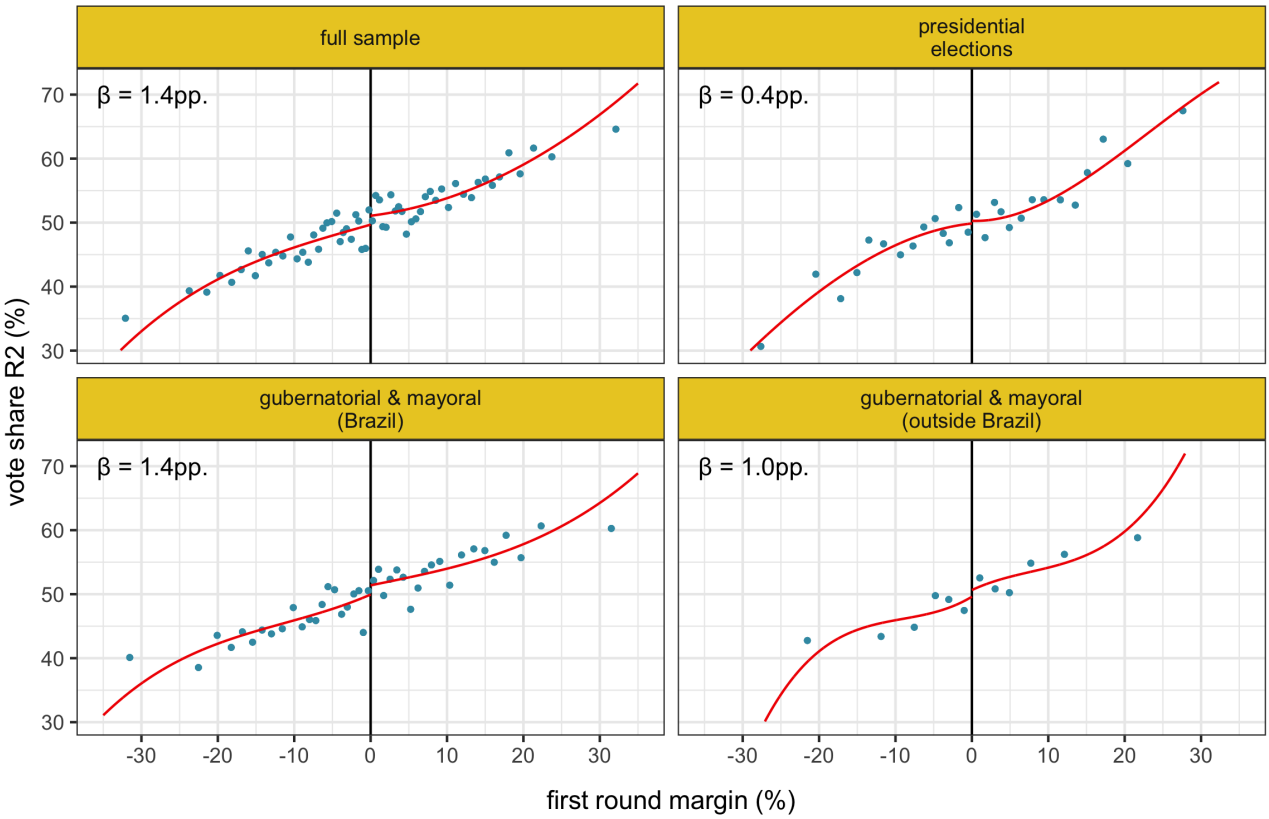


Figure A8: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the vote share in the second round. Red lines show third-order polynomials estimated separately at each side of the cutoff, using a uniform kernel.

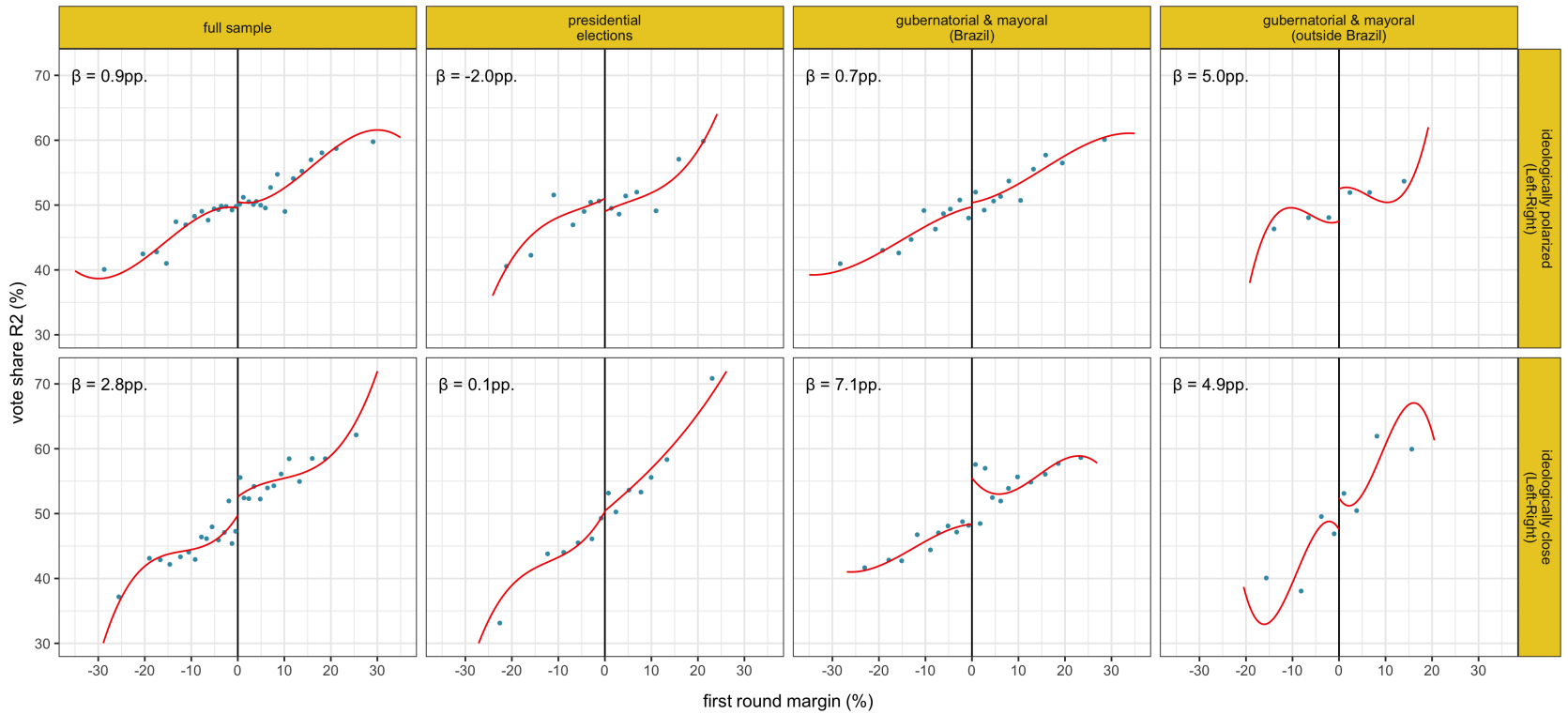


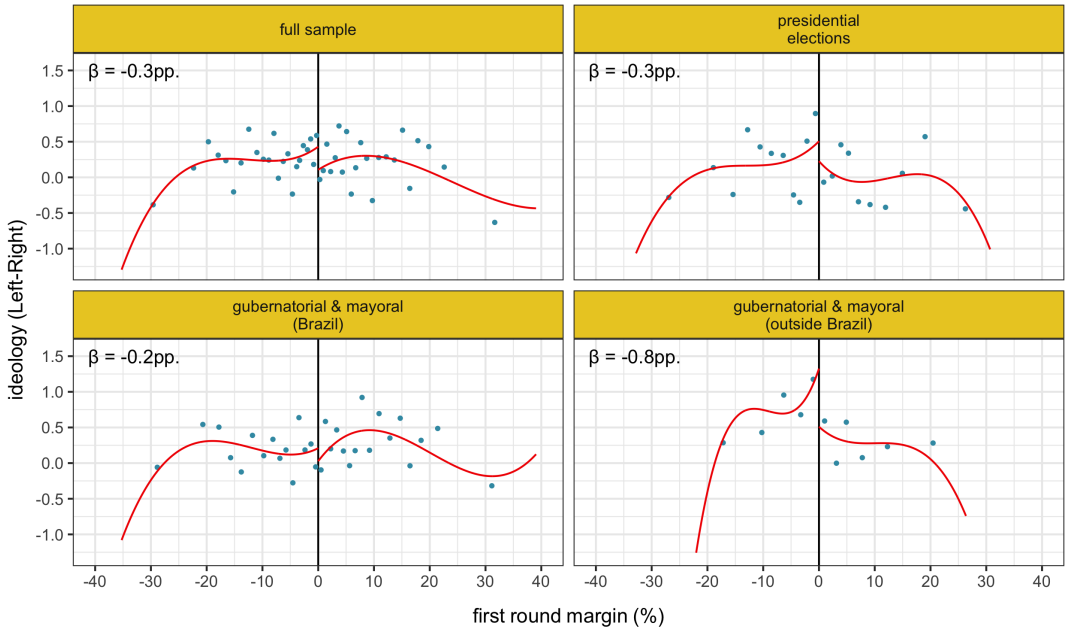
Figure A9: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of *first round margin* on the vote share in the second round. Polarized (respectively, close) elections are those in which the absolute ideological distance along the Left-Right 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 A10 and A11 show the effect of *first round margin* on a series of outcomes that should not be affected by the treatment: candidates' ideology scores and the presence of missing values for these variables.

RD estimates. Table A3 report the corresponding MSERD-optimal RD estimates.

(a) DV: *ideology: Left-Right* (-3.5:3.5)



(b) DV: *ideology: (Il)Liberalism* (-2.5:1.3)

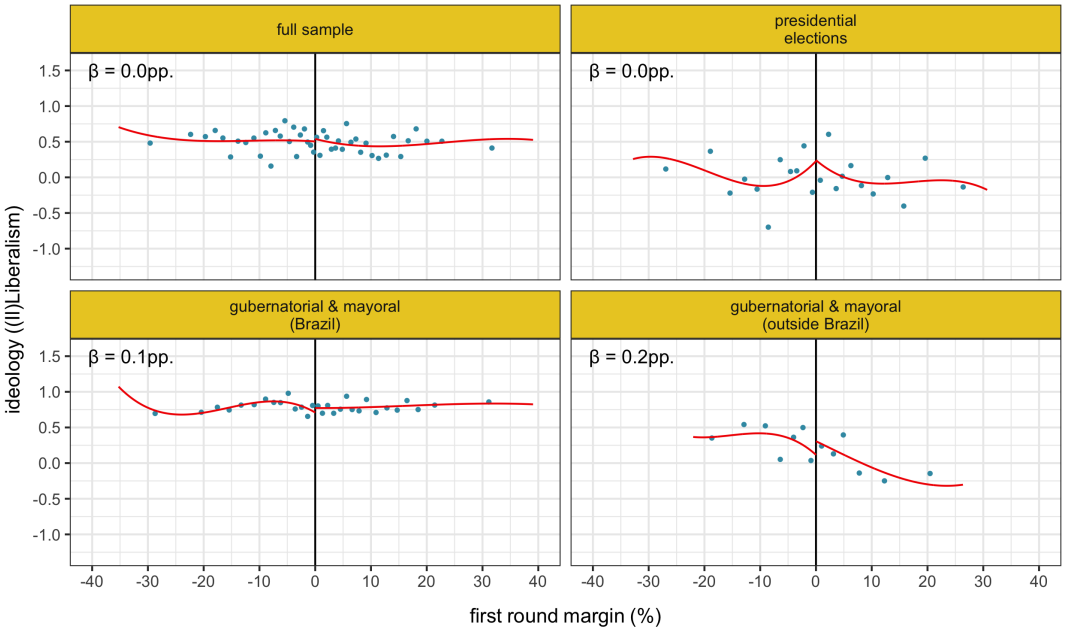
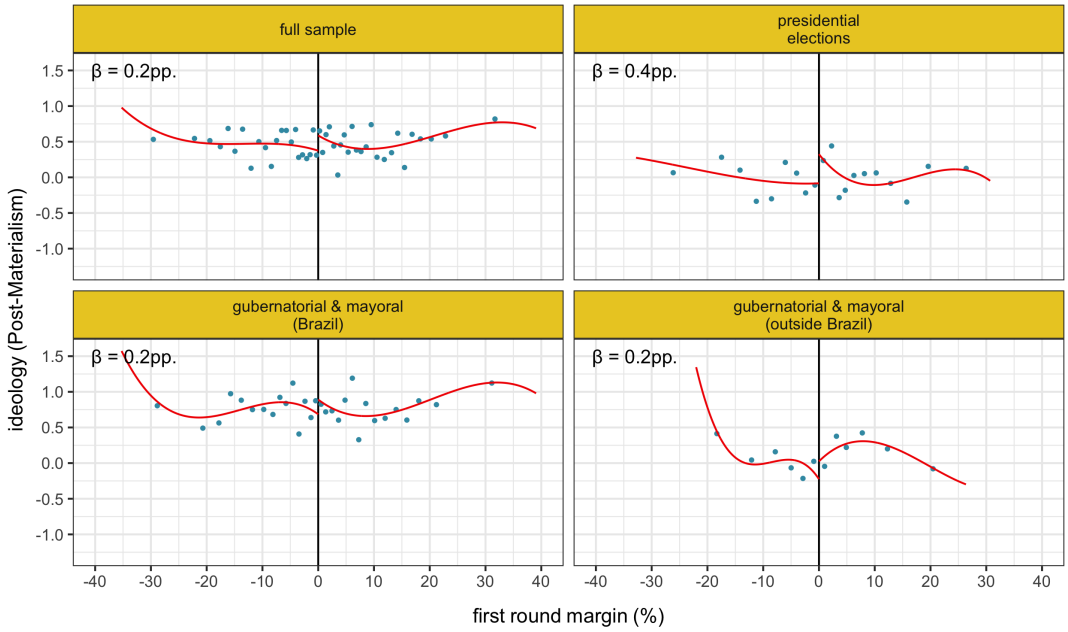


Figure A10: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of first-round advantage on candidates' Left-Right and (Il)Liberalism ideology scores. Red lines indicate the fit of a third-order polynomial regression estimated separately at each side of the cutoff, using a uniform kernel.

(c) DV: *ideology: Post-Materialism* (-2.2:2.6)



(d) DV: *missing ideology score* (0/1)

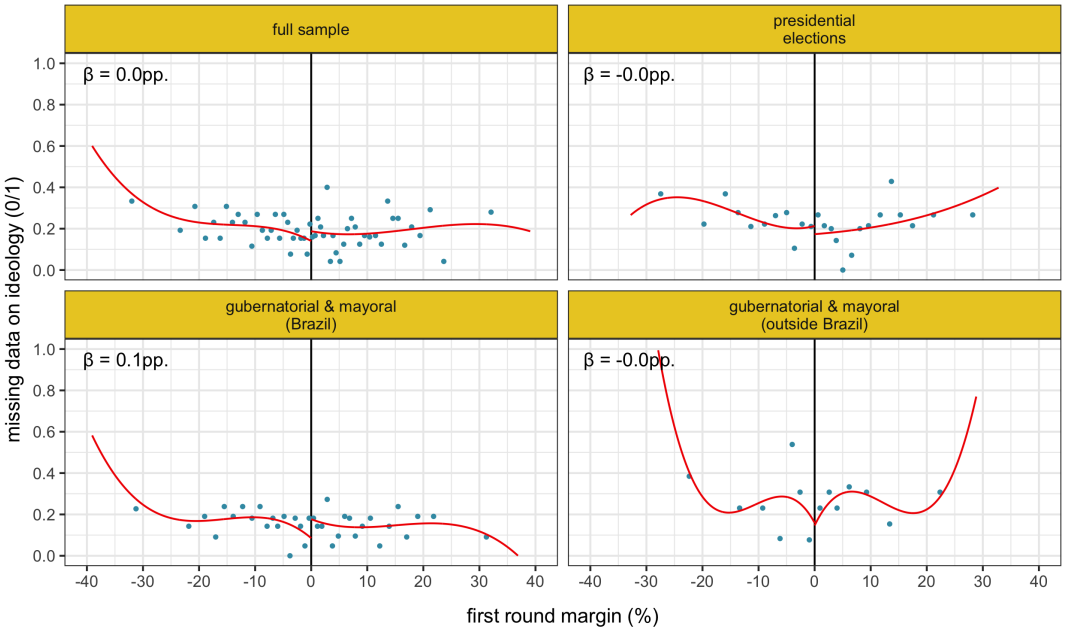


Figure AII: Mimicking variance RD plots with quantile-spaced bins (Calonico, Cattaneo and Titiunik 2015) showing the effect of first-round advantage on candidates' Post-Materialism ideology scores as well as the probability that their ideology scores are missing. Red lines indicate the fit of a third-order polynomial regression estimated separately at each side of the cutoff, using a uniform kernel.

Table A3: RD estimates: Placebo outcomes

(a) DV: *ideology: Left-Right* (-3.5:3.5)	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	-0.38	[-1.04:0.09]	0.10	7.58	259|261
presidential elections	-1.19	[-2.05:-0.61]	0.00	7.59	70|76
gubernatorial & mayoral (Brazil)	0.05	[-0.68:0.80]	0.87	10.30	190|189
gubernatorial & mayoral (outside Brazil)	-0.86	[-2.51:0.67]	0.26	8.21	40|38
(b) DV: *ideology: (Il)Liberalism* (-2.5:1.3)					
full sample	0.10	[-0.17:0.45]	0.36	6.20	222|223
presidential elections	0.23	[-0.46:1.13]	0.41	5.68	59|61
gubernatorial & mayoral (Brazil)	0.02	[-0.13:0.21]	0.66	10.19	187|187
gubernatorial & mayoral (outside Brazil)	0.04	[-0.65:0.81]	0.83	7.51	37|37
(c) DV: *ideology: Post-Materialism* (-2.2:2.6)					
full sample	0.21	[-0.14:0.65]	0.21	8.09	275|276
presidential elections	0.72	[0.18:1.52]	0.01	6.59	61|68
gubernatorial & mayoral (Brazil)	0.03	[-0.43:0.48]	0.91	11.27	199|198
gubernatorial & mayoral (outside Brazil)	0.14	[-0.86:0.98]	0.89	6.65	37|37
(d) DV: *missing ideology score* (0/1)					
full sample	0.05	[-0.09:0.20]	0.45	8.77	355|355
presidential elections	0.12	[-0.19:0.50]	0.38	7.31	86|86
gubernatorial & mayoral (Brazil)	0.05	[-0.13:0.21]	0.61	10.20	220|220
gubernatorial & mayoral (outside Brazil)	0.01	[-0.34:0.37]	0.92	7.80	52|52

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

Sensitivity to bandwidth choice. Figure A12 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 A4 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 A5 replicates the results reported in Table 2, but including controls for the Left-Right, (Il)Liberalism and Post-Materialism scores of the top two placed candidates in the first round.

CER-optimal bandwidth. Table A6 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 A7 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 A13 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. Tables A8 and A9 replicate the results reported in Table 3 but measuring the candidates' ideological distance along the (Il)Liberalism and Post-Materialism dimensions, respectively.

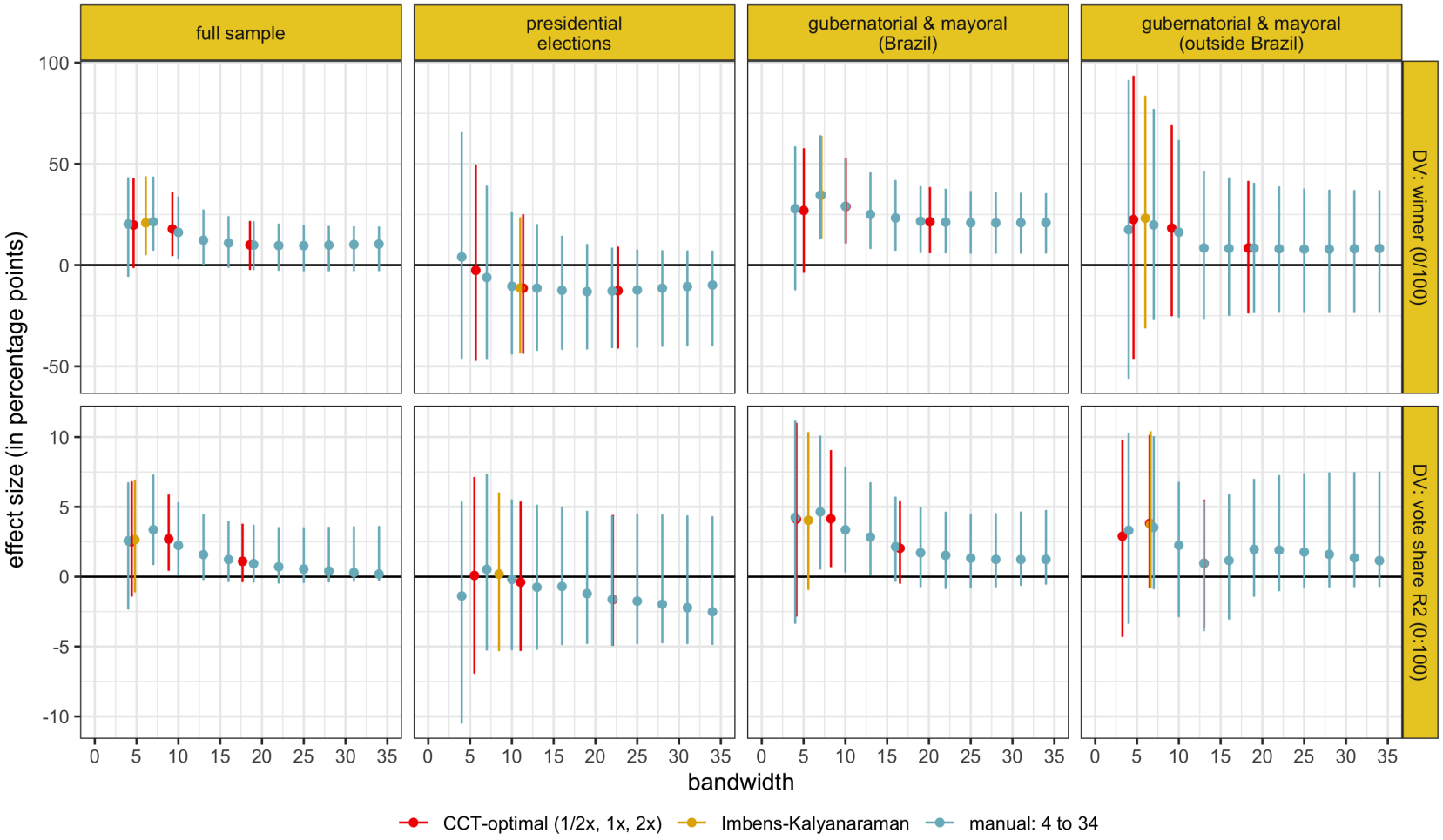


Figure A12: Sharp (conventional) RD estimates, with robust 95% CIs. The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. To calculate the estimates, we clustered observations by election and fitted a separate local linear regression at both sides of the threshold, using a triangular kernel. The CCT-optimal bandwidth is the (MSE-optimal) bandwidth reported in Table 2.

Table A4: RD estimates: Samples with nonmissing ideology scores

(a) DV: *winner* (0/100)	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	13.23	[-5.70:31.25]	0.18	11.55	285|285
presidential elections	-25.53	[-65.52:8.02]	0.13	10.49	76|76
gubernatorial & mayoral (Brazil)	24.69	[2.64:47.22]	0.03	10.97	166|166
gubernatorial & mayoral (outside Brazil)	40.34	[-14.05:99.11]	0.14	9.42	31|31
(b) DV: *vote share*$_{R2}$ (0:100)					
full sample	3.04	[-0.07:7.25]	0.05	9.70	250|252
presidential elections	-1.01	[-8.00:5.31]	0.69	10.22	73|73
gubernatorial & mayoral (Brazil)	4.56	[-0.20:10.86]	0.06	9.14	145|147
gubernatorial & mayoral (outside Brazil)	5.44	[-1.68:13.30]	0.13	7.36	27|27

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. 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 A5: RD estimates: Controlling for ideology

(a) DV: *winner* (0/100)	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	13.35	[-5.78:31.57]	0.18	11.28	282|282
presidential elections	-25.51	[-65.56:8.27]	0.13	10.54	76|76
gubernatorial & mayoral (Brazil)	25.48	[3.04:48.82]	0.03	10.16	158|158
gubernatorial & mayoral (outside Brazil)	40.26	[-14.17:99.07]	0.14	9.43	31|31
(b) DV: *vote share*$_{R2}$ (0:100)					
full sample	3.51	[0.03:8.10]	0.05	8.14	223|225
presidential elections	-0.98	[-7.97:5.70]	0.75	9.76	69|69
gubernatorial & mayoral (Brazil)	4.77	[0.05:11.07]	0.05	8.98	143|145
gubernatorial & mayoral (outside Brazil)	5.48	[-1.49:12.96]	0.12	7.32	27|27

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. 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. All specifications control for the (a) Left-Right; (b) (Il)Liberalism and (c) Post-Materialism scores 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 A6: RD estimates: CER-optimal bandwidths

(a) DV: *winner* (0/100)	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	21.20	[5.01:39.77]	0.01	6.47	276|276
presidential elections	-7.07	[-43.00:31.17]	0.75	8.40	100|100
gubernatorial & mayoral (Brazil)	34.48	[12.94:59.17]	0.00	7.22	170|170
gubernatorial & mayoral (outside Brazil)	19.72	[-27.49:71.90]	0.38	7.06	50|50
(b) DV: *vote share*$_{R2}$ (0:100)					
full sample	3.27	[0.38:6.62]	0.03	6.17	266|268
presidential elections	0.27	[-5.03:6.03]	0.86	8.19	96|96
gubernatorial & mayoral (Brazil)	4.31	[-0.19:9.56]	0.06	5.93	141|143
gubernatorial & mayoral (outside Brazil)	4.21	[-1.08:10.53]	0.11	5.03	40|40

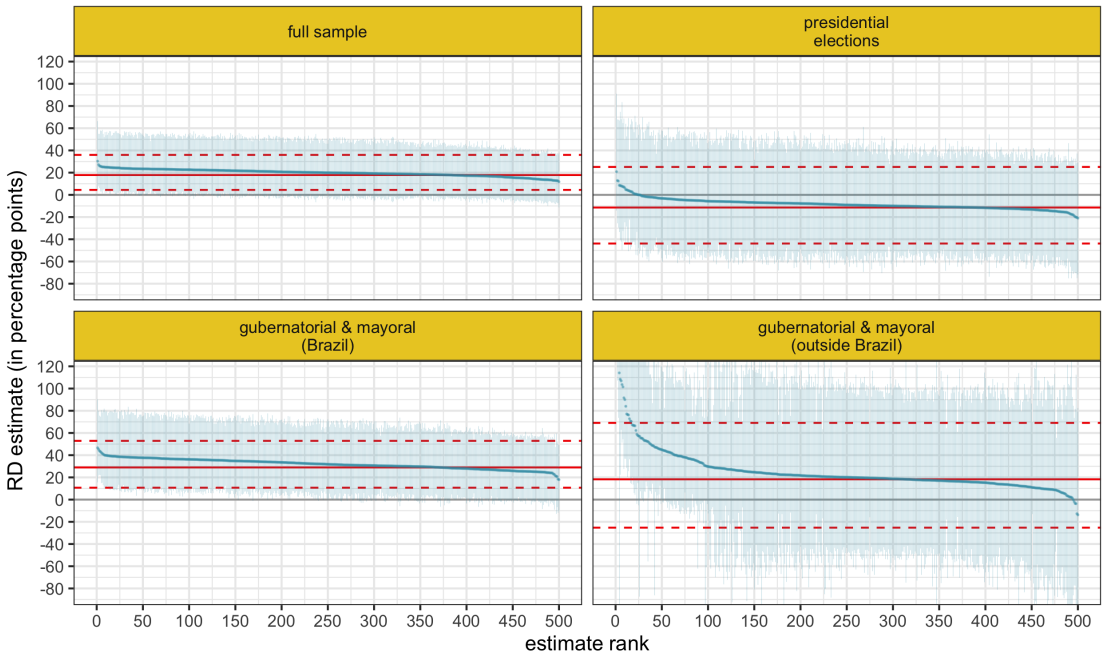
Sharp (conventional) RD estimates, with robust CIs and *p*-values based on the CER-optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014). The running variable is *first round margin*. Samples are restricted to elections requiring a runoff. Observations are clustered by election. The estimates 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 A7: RD estimates: Second-order polynomials

(a) DV: *winner* (0/100)	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	25.31	[6.69:48.88]	0.01	10.48	404|404
presidential elections	-6.08	[-49.33:41.79]	0.87	13.08	133|133
gubernatorial & mayoral (Brazil)	36.21	[11.11:66.27]	0.01	12.60	247|247
gubernatorial & mayoral (outside Brazil)	24.19	[-40.52:90.44]	0.46	11.03	65|65
(b) DV: *vote share*$_{R2}$ (0:100)					
full sample	4.06	[0.59:8.35]	0.02	10.33	395|398
presidential elections	0.75	[-6.19:8.38]	0.77	10.51	114|115
gubernatorial & mayoral (Brazil)	4.58	[0.50:9.95]	0.03	14.21	266|268
gubernatorial & mayoral (outside Brazil)	4.74	[-2.74:11.60]	0.23	8.48	54|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 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)

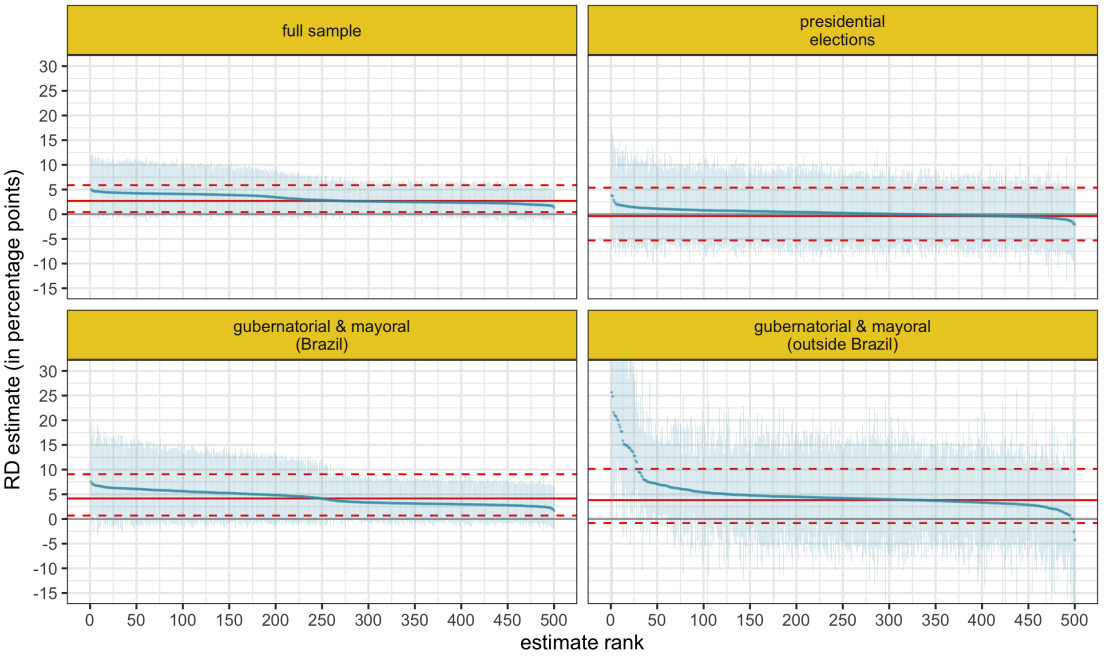


Figure A13: Results with one candidate per election. The red horizontal lines report the RD estimates and robust 95% confidence intervals reported in Table 2, which include two candidates per election. The blue areas report the same estimates from 500 samples in which we randomly selected one candidate – either the first-placed or the runner-up – from every election. To facilitate comparison, these estimates are ranked form highest to lowest.

Table A8: Heterrogeneous effects: (Il)Liberalism ideological distance

(a) DV: *winner* (0/100)	id. distance	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	polarized	8.22	[-19.17:31.72]	0.63	10.61	131|131
	close	18.98	[-5.12:46.45]	0.12	9.03	127|127
presidential elections	polarized	-39.63	[-121.12:27.09]	0.21	7.96	27|27
	close	-10.94	[-57.78:41.31]	0.74	11.21	36|36
gubernatorial & mayoral (Brazil)	polarized	13.61	[-19.74:43.14]	0.47	11.69	88|88
	close	41.71	[10.10:82.69]	0.01	8.26	71|71
gubernatorial & mayoral (outside Brazil)	polarized	44.44	[-34.19:114.04]	0.29	9.64	14|14
	close	4.02	[-171.52:143.94]	0.86	6.11	13|13
(b) DV: *vote share*$_{R2}$ (0:100)						
full sample	polarized	2.54	[-1.34:6.80]	0.19	8.26	104|105
	close	5.23	[-0.07:12.67]	0.05	6.52	101|102
presidential elections	polarized	-1.14	[-6.71:3.77]	0.58	8.60	32|32
	close	-0.11	[-10.40:9.26]	0.91	11.42	35|35
gubernatorial & mayoral (Brazil)	polarized	2.11	[-3.15:6.54]	0.49	9.06	70|71
	close	9.92	[-0.19:23.82]	0.05	5.89	55|56
gubernatorial & mayoral (outside Brazil)	polarized	5.08	[1.14:9.45]	0.01	7.72	12|12
	close	5.90	[-21.35:33.22]	0.67	6.68	15|15

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 A9: Heterrogeneous effects: Post-Materialism ideological distance

(a) DV: *winner* (0/100)	id. distance	estim.	95% CI	*p*-val.	bwd.	$N^-\|N^+$
full sample	polarized	-2.48	[-33.50:20.52]	0.64	9.85	137|137
	close	32.80	[8.37:65.30]	0.01	8.95	112|112
presidential elections	polarized	-11.06	[-69.57:47.77]	0.72	10.29	44|44
	close	-44.05	[-114.47:11.89]	0.11	10.06	28|28
gubernatorial & mayoral (Brazil)	polarized	-7.68	[-50.51:22.77]	0.46	9.43	77|77
	close	74.74	[51.89:113.36]	0.00	5.21	44|44
gubernatorial & mayoral (outside Brazil)	polarized	-16.07	[-129.26:80.18]	0.65	5.41	14|14
	close	116.26	[87.33:160.46]	0.00	5.59	9|9
(b) DV: *vote share*$_{R2}$ (0:100)						
full sample	polarized	1.81	[-1.60:5.25]	0.30	9.43	133|134
	close	6.40	[0.02:15.12]	0.05	6.96	87|88
presidential elections	polarized	4.91	[-3.50:13.34]	0.25	9.88	42|42
	close	-8.15	[-19.70:2.69]	0.14	9.61	27|27
gubernatorial & mayoral (Brazil)	polarized	0.05	[-5.42:4.96]	0.93	9.44	76|77
	close	11.80	[2.45:24.95]	0.02	5.81	50|51
gubernatorial & mayoral (outside Brazil)	polarized	0.22	[-11.42:10.54]	0.94	5.95	15|15
	close	12.50	[3.44:24.25]	0.01	6.83	11|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 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.

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