The Downside of Winning

Parliamentary Victories and Subsequent State Elections

Sujoy Upadhyay

Department of Economics, University of Illinois Chicago

October 16, 2026

Click here for the latest version

Abstract

Do electoral victories at one level of government carry over to help the same party win at another? This question is central to federal democracies, where national and subnational elections are held separately but the same parties, organizations, and often the same voters compete at both levels. I study it using close Indian parliamentary contests from 2004 to 2019, linking each party's result to its subsequent performance in the assembly constituencies within the same territory. A regression discontinuity design shows that a narrow parliamentary victory reduces assembly seat share by 8 to 9 percentage points, or about 0.6 of the seven seats in a typical constituency. The estimate is stable across bandwidths, control sets, and adjustment for prior assembly performance. Local quadratic fits and state and year fixed effects give similar results in the pooled sample. Aggregate vote share shows no detectable change, but its confidence interval allows a decline of nearly three percentage points. A simulation using observed segment margins shows that a decline of this size could account for most of the seat loss under first-past-the-post rules. Winning parties do not detectably contest fewer assembly seats or field less experienced candidates, but they win fewer of the seats they contest. The findings document an anti-coattail effect: a narrow parliamentary win can weaken a party's representation in the next state assembly.

Keywords: coattail effects, incumbency, regression discontinuity, Indian elections, federalism, anti-incumbency

JEL Codes: D72, H77, P16

1 Introduction

Indian parties compete in national and state elections using many of the same workers, local organizations, and voters. Does winning at one level help at the other? Coattail theories suggest that it should: a national victory can mobilize supporters, attract campaign resources, and help the party's candidates in other contests (Campbell et al., 1960; Miller, 1955; Hogan, 2005).

The "double engine" appeal, which asks voters to elect the same party to the national and state governments, draws on this expectation. Yet India also has a history of electoral penalties for incumbents. Candidates who narrowly win national or state elections are less likely than narrow losers to win the next election at the same level (Linden, 2004; Uppal, 2009). A party's parliamentary win may expose it to similar scrutiny in the next assembly election.

A parliamentary win gives the party visibility and an MP who can campaign for its assembly candidates. It may also redirect party workers and attention toward national politics, or lead voters to support a rival at the state level. These forces need not work in the same direction.

I estimate the effect of narrowly winning a parliamentary seat on the same party's performance in the next state assembly election. Each parliamentary constituency contains several assembly constituencies, so I can measure the two outcomes within the same territory.

Using a regression discontinuity (RD) design, I compare parties that narrowly won and narrowly lost parliamentary contests in 2004, 2009, 2014, and 2019. I then measure their vote share and seat share in the next assembly election. The main sample excludes simultaneous elections, whose voters could not have responded to a parliamentary result that was not yet known.

A narrow parliamentary win reduces subsequent assembly seat share by 8 to 9 percentage points, or about 0.6 of the seven seats in a typical constituency. All specifications in the main table are significant at the 1 percent level. The estimates change little with bandwidth, controls, or adjustment for prior assembly performance. In the pooled sample, which includes simultaneous elections, local quadratic fits and state and year fixed effects give similar results, with a seat-share loss of about 10 points.

There is no detectable effect on aggregate vote share: estimates range from -0.13 to -0.53 percentage points. This null does not mean that support is unchanged. The confidence interval allows a decline of about 2.9 points. Under first-past-the-post (FPTP) rules, a small loss of votes can cost several close seats. A simulation using observed assembly margins shows that a decline within the vote-share interval could produce most of the seat loss, depending on where the lost votes go.

Winning parties do not detectably contest fewer assembly seats or field less experienced candidates. They instead win a smaller share of the seats they contest. Tests using segment-level outcomes, rival vote shares, government status, and the time between elections do not identify a single explanation for this pattern.

The main validity concern is a pre-existing difference in party strength. Candidate-balance tests, density tests using one observation per race, and placebo cutoffs show no statistically significant discontinuities. Prior assembly seat share is higher among narrow winners at the fixed five-point bandwidth, with significance at the 10 percent level. The difference reverses

sign two elections earlier, and controlling for prior performance leaves the main estimate unchanged. Local randomization inference within two points gives a smaller, less precise difference of about 4 points; Section 6.3 discusses its assumptions and the unresolved issue of within-race assignment.

The contribution is to identify the consequence of winning an individual parliamentary seat for the party's later state-level representation. This differs from the within-level incumbency effects in Linden (2004) and Uppal (2009), and from the ruling-party spillovers studied by Ravishankar (2009). It also extends the evidence on anti-coattail effects beyond the U.S. setting of Folke and Snyder (2012), who find a 3.5-point state legislative seat-share loss after a gubernatorial win. The Indian estimate is larger, though the treatment and institutions differ. Examining votes and seats separately also shows why a vote-share null need not imply an absence of electoral consequences.

Sections 2 and 3 discuss the literature and institutions. Sections 4–6 describe the data, design, and validity checks. Sections 7–10 report the results, robustness, heterogeneity, and mechanism tests; Section 11 concludes. Appendices A–E provide the additional results, including the delimitation checks in Appendix C.

2 Related Literature

2.1 Coattail Effects in Federal Systems

Coattail theories link a party's electoral fortunes across offices. Miller (1955) emphasizes presidential appeal and straight-ticket voting. Campbell et al. (1960) connect the surge in turnout in presidential years, and its fall at the midterm, to the losses a president's party suffers in between. Hogan (2005) studies gubernatorial coattails in state legislative elections and finds that gubernatorial support predicts the performance of the party's legislative candidates, even after accounting for spending and past party strength.

Causal estimates are less favorable to the coattail hypothesis. Using close gubernatorial contests, Folke and Snyder (2012) find that the party that wins the governorship loses about 3.5 percentage points of state legislative seat share at the next election. Broockman (2009) finds no detectable presidential vote benefit from a co-partisan congressional victory. The direction and size of these spillovers therefore depend on which offices are involved and on how the elections are timed.

India combines multiparty competition with a documented incumbency disadvantage at both levels of government. The question here is whether that disadvantage also crosses levels: does winning a parliamentary seat affect the party's next assembly result?

2.2 Incumbency Effects and Cross-Level Spillovers in India

The incumbency disadvantage within each level of Indian elections is well established. Linden (2004) uses a regression discontinuity design on national parliamentary elections from 1980 to 1999 and finds that, starting in 1991, incumbents are 14 percentage points less likely to win re-election than comparable non-incumbents. He links the change to the end of Congress dominance, after which prior legislative experience became less valuable because no single

party could reliably control parliament. Uppal (2009) extends the analysis to state assembly elections from 1975 to 2003. Bare winners are 15 percentage points less likely to win the next state election before 1991 and 22 points less likely afterward, with larger penalties in states with poorer public goods provision.

Ravishankar (2009) examines effects across levels of government. Using constituency-level data from 1977 to 2005, she finds that MPs from the national ruling party are 9 percentage points less likely to win re-election and that MLAs from the state ruling party face a 14.5 point penalty. Ruling-party status at one level also confers a brief honeymoon at the other: candidates from the state ruling party perform better in early national elections, and vice versa, but the advantage fades within one to two years.

Unlike Ravishankar (2009), I study a party's victory in one constituency rather than its membership in government. The close-election design compares narrow winners and losers, and the nested boundaries allow their subsequent performance to be measured in the same area. The 2004–2019 sample also extends the evidence into the BJP-majority period.

3 Institutional Context

3.1 India's Two-Tier Electoral Architecture

Voters elect Members of Parliament (MPs) to the Lok Sabha from 543 single-member constituencies. At the state level, they elect Members of the Legislative Assembly (MLAs) to the Vidhan Sabha, also from single-member constituencies.

The analysis concerns these directly elected seats. The Rajya Sabha and state legislative councils are outside its scope. India has roughly 4,100 assembly constituencies across 28 states.

India's electoral geography is nested. Each parliamentary constituency comprises several assembly constituencies, commonly referred to as assembly segments. Voters in each segment elect an MLA, and the same territory collectively elects an MP. The Delimitation Commission of India decides how many segments each parliamentary constituency contains and which ones, through orders issued in 1973 and 2008. A typical parliamentary constituency has seven segments. In my sample the average is 7.50, with a minimum of 4 and a maximum of 60; the largest cases are small northeastern states that elect a single MP but have a 60-seat assembly.

Figure 1 shows the Bolpur parliamentary constituency and its assembly segments. A party may contest all, some, or none of those segments; the outcome measures account for this variation.

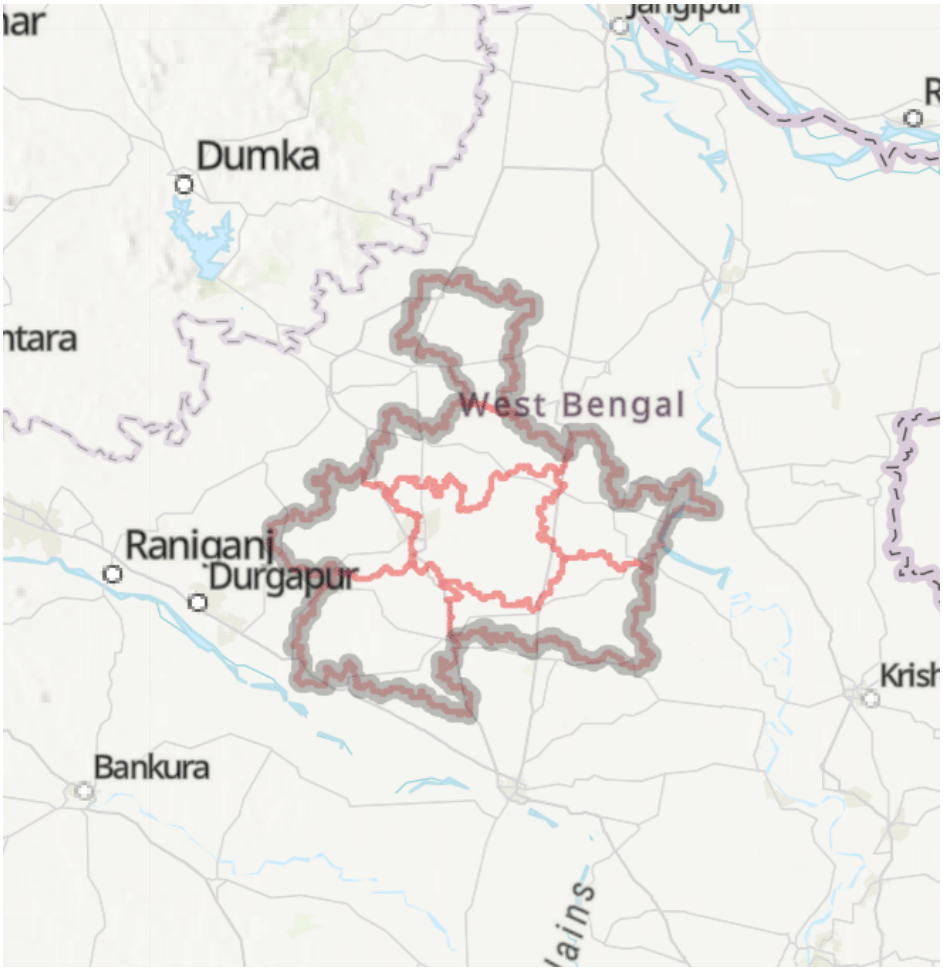


Figure 1: Nested Electoral Geography: An Example

Notes: The thick outline shows the Bolpur parliamentary constituency (West Bengal) under the 2008 delimitation; red lines mark its seven assembly segments. Each segment elects one MLA to the state assembly, and the same territory jointly elects one MP. The paper's outcomes, a party's assembly vote share and seat share, are measured across the segments within each parliamentary constituency.

3.2 The Electoral Calendar and Sample Construction

The Lok Sabha and state assemblies each serve up to five years, but either can be dissolved early. Their electoral calendars therefore differ. Even when elections coincide, voters cast separate ballots for the MP and the MLA.

For example, the 2004 general election is followed by Maharashtra's assembly election in October 2004, Bihar's in 2005, and West Bengal's in 2006. These gaps matter because the assembly vote must follow the parliamentary result to measure a response to winning.

Section 6.4 distinguishes simultaneous elections from those held later in the same year or in a later year. Calendar-year coincidence alone does not establish the order of the contests.

3.3 The First-Past-the-Post System and Vote-to-Seat Conversion

Both elections use FPTP rules: the candidate with the most votes wins, even without a majority. With three to six effective parties in a typical constituency, winning margins are often small. Seat share therefore depends on the distribution of votes across segments and rivals as well as on the party's aggregate vote share.

Section 8.4 uses observed segment margins to calculate how much seat share could change under a small uniform vote loss.

3.4 Party Organization and the Two-Level Contest

Parliamentary campaigns are generally coordinated by national or state party leaders, while assembly campaigns rely more heavily on local candidates. A new MP's national duties may draw attention and workers away from the assembly campaign. Section 10 examines observable implications of this organizational-diversion account.

4 Data

4.1 Sources

The analysis draws on three data sources. The first is the Election Commission of India (ECI), which publishes official constituency-level results for every general election. From the 2004, 2009, 2014, and 2019 elections, I extract candidate-level vote totals, party affiliations, and winning margins for all 543 parliamentary constituencies.

The second is the record of state assembly results at the constituency level. For each state and each general election, I collect results for the next assembly election, covering all major states across the four cycles. These data contain candidate-level vote totals and party affiliations for the roughly 4,100 assembly constituencies.

The third is the set of delimitation orders issued in 1973 and 2008, which define which assembly constituencies sit inside which parliamentary constituency. I use them to build a crosswalk linking each assembly constituency to its parliamentary constituency in each election year, including the 2008 redrawing that took effect before the 2009 general election. This crosswalk is the basis for aggregating assembly outcomes to the parliamentary constituency.

4.2 Sample Construction

Each observation is a party in a parliamentary constituency in a given general election. For each parliamentary race, I identify the winner and the runner-up and measure each party's results in the next assembly election. The running variable is the parliamentary margin: the party's vote share minus that of the other top-two party. It is positive for the winner and negative for the runner-up.

Assembly outcomes are built from candidate-level results within each parliamentary constituency. A party's vote share is its total votes across the assembly segments divided by all votes cast there, and its seat share is the fraction of those segments it wins. The crosswalk

matches nearly every segment: the share of segments matched has a mean of 1.00 and a minimum of 0.83.

The final sample contains $N = 3{,}914$ party–constituency–cycle observations, after dropping independent candidates and observations with a problematic crosswalk match. Every variable used in the main analysis is available for the full sample (Table 1). Because parliamentary constituencies were renumbered under the 2008 delimitation, I build the crosswalk under the 1973 order for the 2004 cycle and under the 2008 order for the later cycles. Appendix C shows that the main results hold under both.

Because the sample combines several sources, I checked four potential sources of error. First, the margin is measured against the strongest actual opponent: candidates are ranked by their total votes across the assembly segments of the parliamentary constituency, NOTA ("none of the above") is excluded from the ranking but retained in the vote-share denominator, and the runner-up is the second-highest vote-getter by construction. Second, parties are matched across the national and state records by exact party name. Seventy-six minor parties are capitalized differently in the two files, but none of them appears as a winner or runner-up in the sample, and only one observation, a minor party in one 2019 race, has no match under any spelling. Third, of the 82 parliamentary constituency-years flagged by the crosswalk, 80 are cross-delimitation mismatches addressed in Appendix C, and only 4 involve segments that genuinely could not be matched. Fourth, a party that contests no assembly segment in the matched state election is coded as winning zero votes and zero seats rather than as missing. This affects 83 observations, or 2.1 percent of the sample, mostly minor parties that did not field state candidates after a one-off parliamentary win.

4.3 Outcome Variables

The main outcome is the party's *assembly seat share*: the fraction of assembly segments within the parliamentary constituency that it wins at the next state election. It runs from 0 to 1, with a mean of 0.36 and a standard deviation of 0.30.

The second outcome is the party's *assembly vote share* across those same segments. The mean is 30.94 percent with a standard deviation of 13.63 points. The dispersion reflects the heterogeneity of parties in the sample, from dominant state parties in their strongholds to national parties competing in less favorable territory.

4.4 Running Variable and Covariates

The *running variable* is the parliamentary margin of victory, in percentage points. Because each race contributes one winner and one runner-up, its distribution is centered at zero, with a median of 0.05 points and a range of -70.09 to $+70.09$.

I use three sets of covariates in robustness specifications. *Candidate controls* record whether the party's parliamentary candidate was the sitting MP, was a woman, or had switched parties before the election, along with the party's general-election vote share. *Constituency controls* are the number of parliamentary candidates and the number of assembly segments. *Year and state fixed effects* enter the most saturated specifications.

Table 1 reports descriptive statistics. About 31 percent of parliamentary candidates in the sample are sitting MPs, 11 percent are women, and 8 percent have switched parties.

The average parliamentary race has 13.88 candidates, reflecting the fragmentation of Indian national elections.

Table 1: Descriptive Statistics

Variable	Count	Mean	SD	Min	P25	Median	P75	Max
Assembly election outcomes								
AE vote share (%)	3914	30.94	13.63	0.00	21.93	32.20	40.80	92.44
AE seat share	3914	0.36	0.30	0.00	0.13	0.33	0.59	1.00
AE vote diff.	3914	0.18	18.46	-87.16	-11.28	0.11	11.72	87.16
AE seat diff.	3914	0.00	0.54	-1.00	-0.41	0.00	0.43	1.00
General election variables								
GE margin (%)	3914	0.05	17.19	-70.09	-10.98	0.05	11.05	70.09
GE vote share (%)	3914	40.71	10.45	8.70	33.20	40.54	47.87	78.79
No. GE candidates	3914	13.88	6.58	2.00	9.00	13.00	16.00	57.00
No. ACs in PC	3914	7.50	4.39	4.00	6.00	7.00	8.00	60.00
AC match rate	3914	1.00	0.01	0.83	1.00	1.00	1.00	1.00
Candidate and election characteristics								
Incumbent	3914	0.31	0.46	0.00	0.00	0.00	1.00	1.00
Female candidate	3914	0.11	0.31	0.00	0.00	0.00	0.00	1.00
Turncoat	3914	0.08	0.27	0.00	0.00	0.00	0.00	1.00
Concurrent election	3914	0.28	0.45	0.00	0.00	0.00	1.00	1.00
Years GE to AE	3914	1.87	1.40	0.00	0.00	2.00	3.00	5.00

Notes: GE = national parliamentary election; AE = state assembly election; PC = parliamentary constituency; AC = assembly constituency. Vote share in percentage points; seat share = fraction of ACs won within the PC. The main analysis focuses on vote share and seat share. $N = 3{,}914$ party × constituency × cycle observations (all variables shown are fully non-missing on this sample).

4.5 Matching National and State Elections

For each general election and state, I link the parliamentary result to the next assembly election. The baseline retains only elections held after the parliamentary result was known.

This includes assembly elections held later in the same year and in later years. Simultaneous contests remain in the pooled sample used for comparison (Section 6.4).

4.6 A Note on Party versus Alliance Coding

Indian parliamentary elections are often fought by formal alliances. During the sample period, the two main ones are the United Progressive Alliance, led by the Indian National Congress, and the National Democratic Alliance, led by the BJP. Seat-sharing deals usually assign each parliamentary constituency to one partner, so a Congress win in a constituency can sit alongside an allied state party contesting the assembly segments inside it.

The unit of interest is the party. Alliance membership changes across cycles, and parliamentary and assembly candidates in the same territory may belong to different partners.

A consistent alliance-level outcome would require information that is not available throughout the sample.

Changes in seat-sharing with allies may therefore explain part of the party's seat loss. The present data cannot distinguish seats lost to an ally from those lost to an opponent.

5 Empirical Strategy

Winning parties are usually stronger than losing parties before the election. Comparing their later assembly results directly would confound the effect of winning with those initial differences.

5.1 The Regression Discontinuity Approach

The RD design instead compares parties close to the zero-margin cutoff. Identification requires continuity of potential assembly outcomes in the parliamentary margin: absent treatment, a party just above zero would have a similar expected outcome to one just below it.

Following Lee (2008), the design relies on parties being unable to control precisely which side of the cutoff they occupy. This assumption needs scrutiny even in close elections (Eggers et al., 2015). Section 6 reports the relevant diagnostics.

Each race contributes linked observations. If the winning margin is 0.4 percentage points, the winner enters at $+0.4$ and the runner-up at -0.4. Inference must account for this dependence.

5.2 Estimating Equation

The baseline estimating equation is a local linear regression of the form:

$$Y_{ipc} = \alpha + \tau \cdot \text{Winner}_{ipc} + \beta \cdot \text{Margin}_{ipc} + \delta \cdot \text{Margin}_{ipc} \cdot \text{Winner}_{ipc} + X'_{ipc}\gamma + \varepsilon_{ipc} \quad (1)$$

where Y_{ipc} is party i's assembly vote share or seat share within parliamentary constituency c after general election p, Winner_{ipc} indicates a parliamentary win, and Margin_{ipc} is the margin in percentage points. The equation fits separate linear functions of the margin on each side of zero, with slope β below the threshold and $\beta + \delta$ above it, using only observations within bandwidth h of the zero cutoff and a triangular kernel that assigns the greatest weight to the closest contests. This local approach is preferred to high-order global polynomials (Gelman and Imbens, 2019).

The coefficient τ measures the discontinuity at zero. A negative estimate is an anti-coattail effect on the party's subsequent assembly outcome.

X_{ipc} contains the controls described in Section 4, added progressively across specifications. Candidate incumbency, gender, and turncoat status are determined before the election; general-election vote share is not, since it is realized in the election that assigns treatment. Controls are not required for identification in an RD design, and I include them only to assess the sensitivity of the estimates.

5.3 Bandwidth Selection and Inference

I use the data-driven bandwidth methods of Calonico et al. (2014). For baseline assembly seat share, the MSE-optimal bandwidth is about 15.9 percentage points and the CER-optimal bandwidth about 10.6 points, with roughly 1,100 and 785 observations per side, respectively. The former targets point-estimate accuracy; the latter targets confidence-interval coverage. I also report fixed bandwidths of 5 and 10 points.

Inference uses the robust bias-corrected method of Calonico et al. (2014), with a separate pilot bandwidth b selected by `mserd`, including when the main bandwidth is fixed. Standard errors are clustered by parliamentary constituency and election cycle to account for dependence within each race. Section 6.3.1 explains how the conventional estimates and bias-corrected inference are reported.

5.4 What the Design Identifies

The estimand is local to parties in close parliamentary contests and applies only to the top two finishers. It does not describe comfortable victories or the effect of entering the national government.

6 Validity Checks

I assess continuity at the cutoff using density tests, candidate and prior-outcome balance, election timing, and placebo thresholds.

6.1 Sorting Around the Threshold

If parties could systematically win close races, we would observe an excess of parties immediately above the winning threshold and a deficit immediately below it. The standard diagnostic is a test for a discontinuity in the density of the running variable at zero (McCrary, 2008). On the full sample, the test detects no discontinuity ($T = -0.1285$, $p = 0.8977$; Figure A.1). This test is, however, uninformative in the present setting. Each race enters the sample twice, once at $+m$ for the winner and once at $-m$ for the runner-up, so the distribution is symmetric by construction.

To obtain an informative test, I construct samples with one observation per race and sign the margin relative to a reference party selected by a rule determined before the result is known. The preferred rule picks the party with the higher vote share in the previous assembly election, which is available for 1,507 of the 1,937 races. A second rule picks the party whose name comes first alphabetically and is available for all 1,937. Both rules are fixed before the election and do not depend on its result. Neither is neutral about party strength: the reference party wins 59.4 percent of races under the first rule and 59.1 percent under the second, the latter probably because "BJP" comes before "INC" in the alphabet. This asymmetry does not affect the test, which concerns only continuity of the density at zero. Neither test rejects continuity: $T = 0.905$ ($p = 0.365$) under the prior-election rule and $T = -0.702$ ($p = 0.483$) under the alphabetical rule (Figures A.2 and A.3). Binomial tests in narrow windows around zero are also statistically insignificant under both rules at

± 0.5, ± 1, and ± 2 points ($p \geq 0.108$ in every case; Table A.1). Both rules take the identity of the top two parties as given, so they test for sorting around the winning threshold, not for manipulation of who reaches the top two.

6.2 Covariate Balance

Table A.2 and Figure A.4 test for discontinuities at zero in candidate incumbency, gender, and turncoat status, the three characteristics that are predetermined and vary between the winner and the runner-up of a race. None exhibits a discontinuity at zero (robust p-values of 0.81, 0.85, and 0.20). Three further characteristics are not reported as formal RD estimates here: the number of candidates and the number of assembly segments are race-level characteristics that take identical values for the winner and the runner-up, and are therefore balanced by construction in the two-observations-per-race sample, while general-election vote share is realized in the election that assigns treatment and is mechanically related to the running variable, so it is not a predetermined characteristic.

6.3 Balance on Prior Assembly Outcomes

Prior assembly performance provides a direct check on pre-existing party strength. I test continuity in prior vote share, seat share, and contest rate, with particular attention to seat share, the main outcome.

Table 2 reports the results. Prior vote share exhibits no discontinuity at either the MSE-optimal bandwidth ($p = 0.95$) or a fixed ± 5 point bandwidth ($p = 0.15$), nor does the prior contest rate ($p = 0.52$ and $p = 0.78$). Prior seat share is somewhat higher among narrow winners: by 3.4 points at the MSE-optimal bandwidth ($p = 0.19$) and by 7.2 points at ± 5 points ($p = 0.073$, 95 percent interval $[-0.007, 0.160]$). The difference is not significant at the 5 percent level, but its size at the narrower bandwidth warrants further checks.

On the subsample with a usable prior election, controlling for prior seat share changes the estimate from -0.098 to -0.103 (Table 3). Both are significant at the 1 percent level. The estimate therefore does not attenuate after this adjustment, though adjustment alone cannot establish identification.

I next compare the prior gap across elections. Vote share two assembly elections earlier shows no discontinuity ($p = 0.13$ at the MSE bandwidth and $p = 0.50$ at ± 5 points; Table A.3). Table A.4 compares seat share at $t-1$ and $t-2$ on the same sample of 1,668 races. At $t-1$, the gap is 3.6 points at the MSE bandwidth ($p = 0.18$, 95 percent interval $[-0.021, 0.116]$) and 7.4 points at ± 5 ($p = 0.069$, interval $[-0.006, 0.160]$). At $t-2$, it is -2.4 and -3.2 points ($p = 0.29$ and $p = 0.37$). A stacked regression rejects equality at ± 5 ($F(1, 492) = 4.66$, $p = 0.031$), but not at ± 10 ($F(1, 897) = 1.38$, $p = 0.24$). The sign reversal weighs against a persistent advantage for narrow winners, although it does not explain the prior gap.

Using the change from prior assembly seat share as the outcome gives another sensitivity check. On the same sample and bandwidth, linearity of the conventional RD estimator means that the change estimate equals the current-level estimate minus the prior discontinuity. At ± 5 points in the pooled sample, this is -17.7 points: -10.5 minus 7.2. In the sequential sample it is -16.2 points (Tables A.5 and A.6). Subtracting the prior gap is justified only if

Table 2: Balance on Prior Assembly Election Outcomes

	(1) Prior VS	(2) Prior VS	(3) Prior Seat Shr	(4) Prior Seat Shr	(5) Prior Contest	(6) Prior Contest
RD_Estimate	-0.13	2.98	0.034	0.072*	-0.029	-0.016
	(1.47)	(2.05)	(0.035)	(0.042)	(0.048)	(0.059)
Bandwidth	MSE	Fixed 5pp	MSE	Fixed 5pp	MSE	Fixed 5pp
Bandwidth (%)	15.5	5	9.19	5	11.2	5
Obs Left	921	342	848	498	990	502
Obs Right	922	339	846	493	988	497
Bias-Corrected Est.	-0.10	2.98	0.045	0.075	-0.031	-0.017
Robust CI Lower	-2.98	-1.04	-0.023	-0.0071	-0.13	-0.13
Robust CI Upper	2.78	7.00	0.11	0.16	0.063	0.098
Conventional SE	1.24	2.01	0.030	0.040	0.041	0.057
Robust SE	1.47	2.05	0.035	0.042	0.048	0.059
Robust p-value	0.95	0.15	0.19	0.073	0.52	0.78

Notes: Dependent variable in each pair of columns is the party's own outcome at the assembly election preceding the national election (vote share, seat share, and contest rate within the parliamentary constituency). Races with no prior assembly election, or whose prior assembly election used a different delimitation order, are excluded. MSE-optimal bandwidth computed separately for each outcome; fixed bandwidth is ±5 percentage points. Point estimate is the conventional local-linear RD estimate; SE in parentheses and significance stars are based on the robust bias-corrected standard error and p-value, which in this table are not always implied by the conventional SE reported in the "Conventional SE" row (see Section 6.3.1). Clustered at the parliamentary constituency × election cycle level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 3: Seat Share Controlling for Prior Assembly Seat Share

	(1) No Controls	(2) Prior Seat Shr Only
RD_Estimate	-0.0977***	-0.103***
	(0.0252)	(0.0243)
Controls for Prior Seat Share	N	Y
Bandwidth (%)	10.63	10.63
Obs Left	945	945
Obs Right	942	942
Bias-Corrected Est.	-0.108	-0.114
Robust CI Lower	-0.163	-0.167
Robust CI Upper	-0.0539	-0.0616
Robust SE	0.0278	0.0268
Robust p-value	0.0000950	0.0000203

Notes: Dependent variable is state assembly seat share, restricted to the subsample with a usable prior AE outcome (same restriction as Table 2). Column 2 adds the party's prior AE seat share as a covariate; adding it alongside the full candidate and constituency control set changes the estimate to -0.102^{***} ($p = 0.0000212$), negligibly different from column 2. MSE-optimal bandwidth. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

that gap would have persisted without treatment. If it reflects sampling variation, differencing can overstate the loss. I therefore report these estimates as a sensitivity exercise, not a bound.

Finally, I use local randomization inference (Cattaneo et al., 2015). Rather than extrapolating to the threshold from each side, this approach treats contests within a small window as if the outcome had been randomly assigned and compares winners and losers directly. This requires a stronger assumption than continuity. A covariate balance procedure using prior seat share, general-election vote share, the number of candidates, and the three candidate characteristics selects a window of ± 2 points ($p = 0.11$) and rejects balance in every window from ± 3 points onward ($p \leq 0.004$). Within ± 2 points, the prior seat-share difference is small and insignificant (3.9 points, $p = 0.16$), and narrow winners' assembly seat share is 4.2 points lower, with a permutation p-value of 0.108 (Table A.7). In wider windows, the differences in means are -5.0 points at ± 3 ($p = 0.022$) and -2.5 points at ± 5 ($p = 0.14$). Local linear adjustment within those windows yields -9.4 and -8.3 points ($p = 0.023$ and $p = 0.012$), close to the main estimate, but these windows are rejected by the balance test. The local randomization evidence therefore points in the same direction as the main estimate, with a smaller and less precise effect in the balance-selected window, which contains only about 430 observations. A further caveat is that the permutations were not restricted to keep each race's winner and runner-up on opposite sides of zero, so they treat the two observations from a race as if they could be permuted independently.

6.3.1 A Note on Inference

Each table reports the conventional local linear estimate. The significance stars, robust standard errors, and confidence intervals come from the robust bias-corrected method of Calonico et al. (2014), which corrects for the approximation bias of the local linear fit and accounts for the additional uncertainty introduced by the correction. Because inference is centered on the bias-corrected estimate, a robust p-value need not equal the reported estimate divided by the robust standard error. To permit verification of the reported significance levels, each table also reports the bias-corrected estimate and the robust 95 percent confidence interval. The pilot bandwidth for the bias correction is chosen separately with `bwselect(mserd)` in every specification, including those with a fixed main bandwidth. With this choice, the conventional and bias-corrected estimates are close throughout. For example, in column 4 of Table 2, the conventional estimate is 0.072 with a robust standard error of 0.042, which on its own would suggest $p \approx 0.087$; the bias-corrected estimate is 0.075, which gives the reported $p = 0.073$.

6.4 Timing of the Assembly Election

This check distinguishes assembly elections held after the parliamentary result from those held simultaneously. Calendar-year coincidence alone does not establish the order of the two contests: some states hold the assembly vote on the same day as the general election, while others vote later in the year. Using polling months, I divide the pooled sample into assembly elections held in a later calendar year (72.5 percent), those held later in the same year (13.7 percent), and those held at the same time as the general election (13.8 percent). The first two groups form the main sample.

For assembly elections held in a later calendar year, a narrow parliamentary victory reduces seat share by 9.6 and 9.5 points, significant at the 1 percent level (Table A.8). For those held later in the same year, the estimates are small and insignificant (-0.9 and -0.3 points), although this group is small. Simultaneous elections show the largest discontinuity, 19 and 21 points (Figure A.5). Vote-share estimates are insignificant in all three groups (Table A.9).

The simultaneous-election discontinuity cannot reflect a response to the parliamentary result, since those assembly votes were cast before the result was known. I therefore treat it as descriptive. It persists under local quadratic fits (-0.21 at the MSE bandwidth and -0.23 at ± 10 points, both $p < 0.001$) and alternative linear bandwidths (-0.23 at ± 5, -0.14 at ± 10, and -0.19 at the MSE bandwidth). Dropping these races leaves 86.2 percent of the pooled sample and estimates of -0.083 and -0.081, compared with -0.098 in the pooled sample, all significant at the 1 percent level (Table A.10). Vote-share effects remain insignificant (Table A.11).

The simultaneous elections are concentrated in a distinct set of states, mainly Andhra Pradesh, Odisha, Sikkim, and in some cycles Arunachal Pradesh and Telangana, where parliamentary races are less fragmented: 9.7 candidates on average compared with 14.6 elsewhere ($t = -20.3$, $p < 0.001$). With fewer effective competitors, a party's parliamentary performance may track its assembly performance more closely, so a common shock to both contests could generate a strong association without a causal relationship. This is a plausible explanation for the larger discontinuity, although I cannot test it directly.

6.5 Placebo Thresholds

I also estimate discontinuities at margins of -10, -5, $+5$, and $+10$ points, where treatment does not change. Each window extends 2.5 points from its placebo cutoff and does not cross zero. None of the eight estimates is significant (Tables A.12 and A.13; Figures A.6 and A.7). The seat-share p-values are 0.36, 0.56, 0.59, and 0.67, but the intervals are wide enough to include effects as large as the main estimate.

7 Main Results

7.1 Effect on Assembly Vote Share

Table 4 reports baseline vote-share estimates at the CER- and MSE-optimal bandwidths, with and without controls. They range from -0.13 to -0.53 percentage points, with $p \geq 0.56$. Figure B.1 shows no visible discontinuity.

The controlled MSE specification has a 95 percent confidence interval of $[-2.85, 1.54]$ points. This leaves room for a meaningful decline in support despite the small point estimate. Section 8.4 examines the seat consequences of vote losses within this interval.

7.2 Effect on Assembly Seat Share

Table 5 reports a seat-share loss in all four baseline specifications. Conventional estimates range from -0.080 to -0.093 and bias-corrected estimates from -0.088 to -0.096, with every

Table 4: Effect on State-Election Vote Share: RD Estimates (Sequential Elections)

	(1)	(2)	(3)	(4)
RD_Estimate	-0.15	-0.097	-0.53	-0.43
	(1.37)	(1.13)	(1.14)	(0.95)
Bandwidth	CER	CER	MSE	MSE
Bandwidth (%)	10.0	10.0	15.0	15.0
Obs Left	752	752	1061	1061
Obs Right	749	749	1059	1059
Bias-Corrected Est.	-0.25	-0.20	-0.74	-0.66
Robust CI Lower	-3.13	-2.58	-3.38	-2.85
Robust CI Upper	2.63	2.19	1.89	1.54
Robust SE	1.47	1.22	1.34	1.12
Robust p-value	0.86	0.87	0.58	0.56
Controls	N	Y	N	Y

Notes: Sample excludes simultaneous-election races (N=3375 of 3914; see Section 8.3). Cols 1–2: CER-optimal bandwidth (≈10.0%); cols 3–4: MSE-optimal bandwidth (≈15.0%); within each pair, no controls then the full candidate and constituency control set. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

p-value below 0.002. Figure B.2 shows the corresponding discontinuity.

In a parliamentary constituency with seven assembly segments, an 8 point reduction in seat share corresponds to approximately 0.6 fewer assembly seats for the party that won the parliamentary seat.

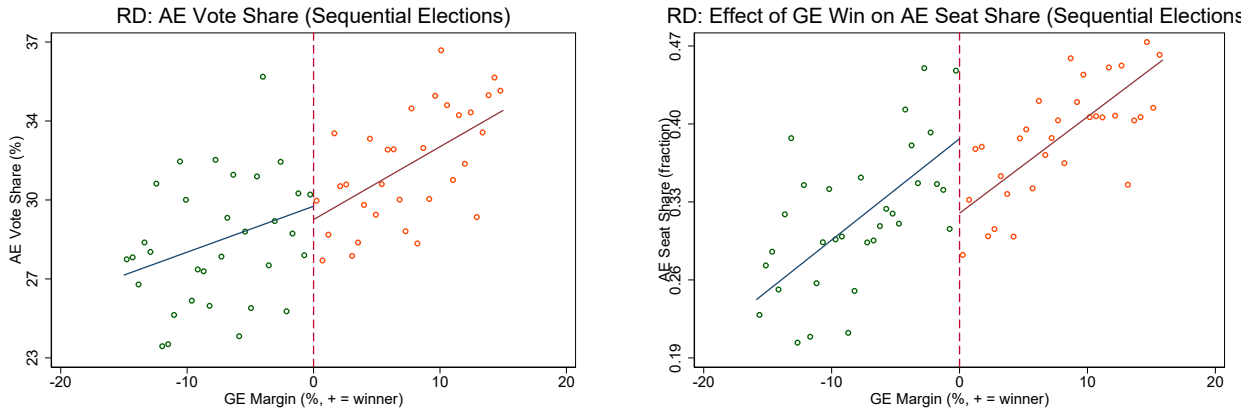
Table 5: Effect on State Assembly Seat Share: RD Estimates (Sequential Elections)

	(1)	(2)	(3)	(4)
RD_Estimate	-0.093***	-0.092***	-0.083***	-0.080***
	(0.029)	(0.028)	(0.024)	(0.023)
Bandwidth	CER	CER	MSE	MSE
Bandwidth (%)	10.6	10.6	15.9	15.9
Obs Left	785	785	1101	1101
Obs Right	782	782	1098	1098
Bias-Corrected Est.	-0.096	-0.095	-0.090	-0.088
Robust CI Lower	-0.16	-0.15	-0.14	-0.14
Robust CI Upper	-0.037	-0.038	-0.036	-0.036
Robust SE	0.030	0.029	0.027	0.026
Robust p-value	0.0015	0.0011	0.0011	0.00092
Controls	N	Y	N	Y

Notes: Sample excludes simultaneous-election races (N=3375 of 3914; see Section 8.3). Cols 1–2: CER-optimal bandwidth (≈10.6%); cols 3–4: MSE-optimal bandwidth (≈15.9%); within each pair, no controls then the full candidate and constituency control set. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

7.3 The Vote-Seat Divergence

Figure 2 places the vote- and seat-share results side by side. The seat loss is precisely estimated, while the vote-share interval allows both no change and a modest decline. These results leave open changes in the distribution of support across segments or rivals, as well as a small broad-based vote loss. Sections 8.4 and 10 examine these possibilities.



(a) State-Election Vote Share

(b) State Assembly Seat Share

Figure 2: Vote Share vs. Seat Share: The Vote-Seat Divergence

Notes: Left: assembly vote share. Right: assembly seat share. Sequential-elections sample, MSE-optimal bandwidth.

7.4 Contesting versus Winning

I separate changes in the share of seats contested from changes in the win rate among contested seats. The first could reflect withdrawal or seat-sharing with allies; the second could reflect weaker support or a less effective campaign.

Winners do not contest detectably fewer segments. The estimated difference in the share of segments contested is 1.8 points without controls and 1.9 points with controls, with 95 percent intervals of $[-4.4, 6.4]$ and $[-4.3, 6.6]$ points (Table 6). The estimates rule out a decline in contesting of more than about 4 points.

Nor do winners contest less favorable segments. Among the segments a party contests, its margin in the previous assembly election is 4.78 points higher for narrow winners at the MSE bandwidth (95 percent interval $[1.60, 9.43]$, $p = 0.006$) and 4.72 points higher at ± 10 points (interval $[1.58, 9.38]$, $p = 0.006$; Table 8). Part of this difference may reflect the prior seat-share difference discussed in Section 6.3; in either case, winners contest segments that are at least as favorable as those contested by losers. Their win rate in contested segments is 11.5 points lower without controls and 11.3 points lower with controls ($p < 0.001$ in both; Table 7). Because the choice of which seats to contest may itself respond to the parliamentary result, this conditional estimate does not identify a causal effect on a fixed set of seats. The pattern is nonetheless informative. Winning parties contest as many seats, in segments that are at least as favorable, and win fewer of them.

Table 6: RD Estimate: Fraction of Assembly Segments Contested

	(1)	(2)
RD_Estimate	0.0176	0.0193
	(0.0239)	(0.0239)
Controls	N	Y
Bandwidth (%)	13.75	13.75
Obs Left	990	990
Obs Right	987	987
Bias-Corrected Est.	0.00980	0.0115
Robust CI Lower	-0.0444	-0.0426
Robust CI Upper	0.0640	0.0656
Robust SE	0.0277	0.0276
Robust p-value	0.723	0.677

Notes: Sequential-elections sample (see Section 8.3). Dependent variable is the fraction of matched assembly segments within the parliamentary constituency that the party contests in the subsequent state election, regardless of outcome. MSE-optimal bandwidth. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 7: RD Estimate: Seat Share Conditional on Contesting

	(1)	(2)
RD_Estimate	-0.115***	-0.113***
	(0.0296)	(0.0292)
Controls	N	Y
Bandwidth (%)	14.03	14.03
Obs Left	976	976
Obs Right	984	984
Bias-Corrected Est.	-0.122	-0.120
Robust CI Lower	-0.190	-0.187
Robust CI Upper	-0.0534	-0.0527
Robust SE	0.0348	0.0343
Robust p-value	0.000474	0.000475

Notes: Sequential-elections sample (see Section 8.3). Dependent variable is seats won divided by segments contested (not divided by all matched segments, as in the headline seat share). See Section 7.4 for the caveat on conditioning on a potentially post-treatment variable. MSE-optimal bandwidth. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 8: RD Estimate: Composition of Contested Segments

	(1)	(2)
RD_Estimate	4.78***	4.72***
	(1.78)	(1.76)
Bandwidth	Own MSE	Fixed 10pp
Bandwidth (%)	9.83	10
Obs Left	489	497
Obs Right	487	496
Bias-Corrected Est.	5.52	5.48
Robust CI Lower	1.60	1.58
Robust CI Upper	9.43	9.38
Robust SE	2.00	1.99
Robust p-value	0.0057	0.0059

Notes: Sequential-elections sample. Dependent variable is the race-side mean prior-AE margin against the segment's own strongest opponent, averaged across the segments the party actually contests in the subsequent AE (one observation per race-side). A positive estimate means narrow winners contest a more favorable set of segments than narrow losers. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

8 Robustness

This section examines sensitivity to bandwidth, polynomial order, and sample construction. The local quadratic and bandwidth-summary tables (Tables B.1 and B.3) use the full pooled sample, including simultaneous elections, so they check the stability of the pooled estimate. Sections 8.3 and 8.6 return to the baseline sequential sample. Appendix C compares the two delimitation regimes.

8.1 Fixed Bandwidths

Replacing the data-driven bandwidths with fixed bandwidths of ±5 and ±10 points leaves the results unchanged (Table B.3): vote-share estimates are −0.21 and −0.44 points and neither is significant, while seat-share estimates are −0.10 at ±5 points and −0.098 at ±10 points, both significant at the 1 percent level.

8.2 Local Quadratic Specification

The baseline specification uses a local linear fit, following Gelman and Imbens (2019). A local quadratic specification yields vote-share estimates of −0.56 and −0.55 points, neither significant, and a seat-share estimate of −0.11 with and without controls, significant at the 1 percent level (Table B.1).

8.3 Sample Restriction: Sequential versus Simultaneous Elections

The main sample drops 539 of the 3,914 pooled observations because the assembly election was held simultaneously with the general election, leaving 3,375 observations. As Section 6.4 shows, these simultaneous-election races display a markedly larger seat-share discontinuity (19 to 21 points) than the sequential sample, yet the assembly vote in these races was cast before the parliamentary result was known, so it cannot reflect a response to it. Because that discontinuity cannot be causal, pooling these races back in and showing that the headline estimate changes little is not an informative robustness check: the two subsamples answer different questions, and their similarity could just as easily reflect a shared non-causal source of correlation (Section 6.4 discusses a candidate explanation) as it could reflect robustness of the causal estimate. All results in the paper therefore use the sequential-elections sample unless otherwise noted.

8.4 How Large a Vote Loss Would Be Needed

Could a vote loss within the interval $[-2.85, 1.54]$ account for an 8-point seat-share loss? I examine an average vote–seat relationship and then use the margins in individual assembly segments.

As a descriptive benchmark, regressing assembly seat share on vote share with state and year fixed effects gives a slope of about 1.75. A 2.85-point vote loss would then imply about 5 points of seat share. This average masks the importance of close contests: the same vote loss costs more seats where margins are narrow.

The second approach uses observed margins. For parliamentary winners within ± 10 points of the threshold, I take each assembly segment the party won, impose a uniform decline of Δ points, and count the segments that would change hands. I consider two scenarios. In the first, the lost votes are dispersed among other parties or lost to abstention, so the margin over the strongest opponent narrows by Δ. In the second, every lost vote transfers to that opponent, so the margin narrows by 2Δ. Because the exercise uses post-treatment margins, it measures the vulnerability of winners' seats rather than reconstructing the counterfactual outcome. It also abstracts from other changes in turnout or candidate entry.

In Table 9, a 2.85-point decline implies seat-share losses of 7.3 points with the opponent's vote share fixed and 15.4 points with full transfer. To generate an 8-point loss requires declines of 3.08 and 1.54 points, respectively. The first is just outside the vote-share interval; the second is inside it. Thus a modest vote loss could account for most or all of the seat loss under these assumptions. The exercise does not establish that such a loss occurred.

8.5 Bandwidth Sensitivity

Table B.3 reports all four bandwidth choices under the fully saturated specification. Vote-share estimates range from -0.21 to -0.62 points, none significant. Seat-share estimates range from -0.097 to -0.10, all significant at the 1 percent level. The seat-share estimate varies by less than 0.003 across bandwidths, a small fraction of its standard error.

Table 9: Uniform-Swing Simulation: Implied Additional Seat-Share Decline

Uniform swing Δ (pp)	0.5	1.0	1.5	2.0	2.85	3.0	4.0	5.0
Implied seat-share decline (pp), fixed opponent	1.42	2.72	3.56	5.22	7.31	7.76	10.65	13.76
Implied seat-share decline (pp), full transfer	2.72	5.22	7.76	10.65	15.43	16.18	20.79	24.12

Notes: Population is 740 PC-races where the focal party barely won the GE (fixed ±10pp bandwidth), sequential-elections sample, 4,673 matched segments with a usable subsequent-AE margin against the segment's own strongest opponent. For each swing size Δ, a currently-won segment flips if its margin against its strongest opponent falls below zero after the swing. "Fixed opponent" holds the opponent's own vote share fixed (margin shrinks by Δ); "full transfer" sends every lost vote to the opponent (margin shrinks by 2Δ). The implied decline is the mean, across races, of flipped segments divided by matched segments. This is a sensitivity exercise on the party's actual post-treatment segment margins, not a reconstruction of the RD design's causal counterfactual; see text for all assumptions.

8.6 Combined Robustness: Excluding Simultaneous Elections and Controlling for Prior Seat Share

The two most consequential validity concerns are the prior seat-share difference (Section 6.3) and the simultaneous elections (Section 6.4). Table 10 addresses both on a common sample that excludes simultaneous elections and retains only races with a usable prior assembly election, so that the columns differ only in whether prior seat share is controlled for. At the MSE bandwidth, the estimate is −0.084 without the control, −0.088 with it, and −0.087 with all controls, each significant at the 1 percent level. At ±5 points, the estimates are −0.079 ($p = 0.048$) and −0.101; at ±10.5 points, −0.093 and −0.101. The negative estimate is robust to both restrictions jointly, and controlling for prior seat share never attenuates it.

Table 10: Combined Robustness: Excluding Simultaneous Elections and Controlling for Prior Seat Share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	-0.0841***	-0.0883***	-0.0865***	-0.0789**	-0.101***	-0.0925***	-0.101***
	(0.0243)	(0.0234)	(0.0232)	(0.0400)	(0.0376)	(0.0290)	(0.0276)
Specification	Unadjusted	Prior-Controlled	Prior + Full Controls	Unadjusted	Prior-Controlled	Unadjusted	Prior-Controlled
Bandwidth Type	MSE	MSE	MSE	5pp	5pp	10.5pp	10.5pp
Bandwidth (%)	15.57	15.57	15.57	5	5	10.50	10.50
Obs Left	1073	1073	1073	409	409	773	773
Obs Right	1070	1070	1070	404	404	771	771
Bias-Corrected Est.	-0.0919	-0.0967	-0.0948	-0.0797	-0.102	-0.0961	-0.105
Robust CI Lower	-0.146	-0.149	-0.147	-0.159	-0.176	-0.156	-0.162
Robust CI Upper	-0.0373	-0.0443	-0.0428	-0.000778	-0.0270	-0.0359	-0.0477
Robust SE	0.0279	0.0267	0.0265	0.0403	0.0381	0.0307	0.0292
Robust p-value	0.000968	0.000295	0.000351	0.0478	0.00763	0.00176	0.000328

Notes: Dependent variable is state assembly seat share. Sample excludes simultaneous-election races (Section 6.4) and is restricted to races with a usable prior AE outcome (Section 6.3), identically across all seven columns. "Unadjusted" omits prior seat share; "Prior-Controlled" adds it as a covariate. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

9 Heterogeneity

The average seat-share penalty need not be uniform across contexts. I focus on two dimensions along which the paper's own mechanisms generate a specific ex-ante prediction, and summarize three further splits, by region, party, and electoral era, in Appendix D, where they serve as a check that the result is broad-based rather than as tests of a particular account.

9.1 State Capacity and Organizational Diversion

If part of the penalty reflects organizational diversion, a newly elected MP's attention and resources shifting toward national politics (Section 10), it should be larger where parties have thinner organizational benches to begin with. Uppal (2009) finds this pattern for the within-level incumbency disadvantage itself: it is larger in states with poorer public goods provision. I compare the BIMARU states (Bihar, Madhya Pradesh, Rajasthan, Uttar Pradesh), among India's poorest, with four of its most developed (Gujarat, Karnataka, Maharashtra, Tamil Nadu). The seat-share effect is -0.116 to -0.123 in the BIMARU states and -0.082 to -0.086 in the wealthier ones (Appendix Table D.1); vote share shows no effect in either group (Table D.2). The difference runs in the direction the organizational account predicts, but it is small relative to its standard error ($z = -0.53$ at most), so I read this as showing the effect is present in both poorer and wealthier states rather than as evidence of a capacity gradient.

9.2 Electoral Structure and FPTP Amplification

The paper's leading account of the vote-seat divergence (Section 8.4) is that first-past-the-post rules convert a small vote loss into a large seat loss precisely because many contests are close. If so, the penalty should be larger where votes are split across more parties and smaller where one party or alliance dominates. I classify states into a multiparty group (Assam, Bihar, Jammu and Kashmir, Jharkhand, Maharashtra, Manipur, Odisha, Uttar Pradesh) and a two-bloc group (Andhra Pradesh, Goa, Himachal Pradesh, Kerala, Punjab, Tamil Nadu, Uttarakhand, West Bengal). The result runs the opposite way: -0.069 to -0.073 in multiparty states versus -0.110 to -0.113 in two-bloc states (Appendix Table D.3), though the intervals overlap. A parliamentary two-bloc system does not guarantee a two-candidate assembly race, West Bengal, Andhra Pradesh, and Tamil Nadu all have competitive multiparty assembly contests despite dominant national-level blocs, so this comparison is a weaker test of FPTP amplification than one based on the effective number of parties within each assembly segment, which I leave to future work.

Three further splits, by region, party, and electoral era, are reported in full in Appendix D. The negative seat-share effect is present in nearly every subgroup there as well, and no single party or region drives it; vote-share estimates are insignificant throughout. Given the small subsamples involved, I treat these as evidence that the effect is broad-based rather than as mechanism tests in their own right.

10 Mechanisms and Interpretation

I consider three accounts of the seat loss: voter balancing or backlash, diversion of party resources, and responses that depend on government status or the time since the parliamentary election.

Voters may support a rival to balance power across levels or to punish the party now represented by the MP. Alternatively, the MP and party organization may devote less effort to the assembly campaign. A referendum or honeymoon account, following Ravishankar (2009), instead predicts differences by government status or time elapsed.

The tests use assembly-segment outcomes, government status, timing, and candidate experience. Splitting the local sample limits precision, so most comparisons cannot distinguish these explanations.

10.1 Evidence from Individual Assembly Segments

Location of seat losses. I classify each party's assembly segments in three ways and estimate the effect within each group at a fixed ±5 point bandwidth. The first classification ranks segments into terciles by the party's segment-level vote share in the general election. Neither vote share nor win probability shows a significant effect in any tercile (Tables E.1 and E.2). The largest win-probability estimate, in the middle tercile, is -0.078 ($p = 0.21$). On the full pooled sample, which includes simultaneous elections, the middle-tercile estimate is larger and significant (-0.13, $p = 0.017$), but the baseline sample does not confirm it. Because this ranking uses the election that assigns treatment, the second classification ranks segments by the party's vote share in the preceding assembly election. Under this classification, the loss is largest in the party's weakest segments, where win probability is 12 to 13 points lower ($p = 0.075$ and $p = 0.065$), with small and insignificant estimates in the middle and strongest terciles (Table E.3). The two classifications assign a segment to the same tercile only 45.5 percent of the time, which partly explains their divergent patterns.

The third classification measures competitiveness in the preceding assembly election by the party's margin against its strongest opponent: Safely Held (above +10 points), Previously Close (within ±10 points), and Clearly Losing (below −10 points). Subsequent win rates in these groups are 66.3, 43.0, and 24.6 percent, confirming that the classification behaves as expected. Win probability declines in all three groups: by 13 to 15 points in Clearly Losing segments ($p = 0.037$ and $p = 0.016$), by 9 points in Previously Close segments ($p = 0.17$ and $p = 0.18$), and by 15 to 16 points in Safely Held segments ($p = 0.085$ and $p = 0.080$; Table 12). Stacked regressions do not reject equality for any pair (F-statistics between 0.01 and 0.09, $p \geq 0.77$). Vote-share estimates under this classification are insignificant in every group (Table E.4).

The point estimates are negative across all three competitiveness groups, but the equality tests do not establish whether losses are concentrated in any one group.

Close versus non-competitive segments. The three-way classification above pools a genuinely close call (Previously Close) together with two less competitive extremes whose own labels oversell their safety: at the ±10pp threshold, Safely Held segments are subsequently won only 66.3 percent of the time. I collapse the classification into two groups, Close (within

a wider threshold of zero margin against the strongest prior-AE opponent) and Not Close (pooling segments comfortably ahead with segments clearly behind, since under FPTP a vote shift far from the margin on either side does not change who wins the segment), and widen the threshold to ±15 and ±20 points. On vote share, the direction is weakly consistent with a resource-defense account: in Close segments the estimate is −1.05 and −1.38 points at ±15pp ($p = 0.61$ and $p = 0.47$) and −0.60 and −1.20 at ±20pp ($p = 0.77$ and $p = 0.53$), while in Not-Close segments it is +2.96 and +0.97 at ±15pp ($p = 0.34$ and $p = 0.71$) and +3.29 and +1.80 at ±20pp ($p = 0.35$ and $p = 0.53$; Table 11). None of these estimates is individually significant, and a formal test does not reject equality between the two groups at either threshold ($F(1, 276) = 2.22$, $p = 0.14$ at ±15pp; $F(1, 276) = 1.46$, $p = 0.23$ at ±20pp). Vote share is modestly lower in competitive segments and modestly higher in safe-or-hopeless ones, the pattern a resource-reallocation account would predict, but the standard errors are far too wide to treat this as evidence rather than as a hypothesis for a larger sample.

Table 11: Mechanism Test: AC-Level Vote Share by Close vs. Not-Close Competitiveness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	-1.05	-1.38	2.96	0.97	-0.60	-1.20	3.29	1.80
	(2.07)	(1.94)	(3.03)	(2.67)	(2.04)	(1.92)	(3.49)	(2.91)
Threshold	+-15pp	+-15pp	+-15pp	+-15pp	+-20pp	+-20pp	+-20pp	+-20pp
Group	Close	Close	NotClose	NotClose	Close	Close	NotClose	NotClose
Controls	N	Y	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5	5	5
Obs Left	1065	1065	522	522	1237	1237	350	350
Obs Right	1117	1117	529	529	1275	1275	371	371
Bias-Corrected Est.	-1.07	-1.41	3.01	1.00	-0.60	-1.21	3.44	1.89
Robust CI Lower	-5.19	-5.26	-3.11	-4.27	-4.63	-5.01	-3.70	-3.93
Robust CI Upper	3.05	2.43	9.14	6.27	3.44	2.58	10.6	7.71
Robust SE	2.10	1.96	3.13	2.69	2.06	1.94	3.64	2.97
Robust p-value	0.61	0.47	0.34	0.71	0.77	0.53	0.34	0.53

Notes: Sequential-elections sample (see Section 8.3). Outcome is the focal party's AE vote share within the specific assembly segment. Close/Not Close collapses the three-way prior-AE margin-against-strongest-opponent classification used in Table 12 into two groups at a widened threshold: Close is within the stated threshold of zero margin; Not Close pools segments comfortably ahead and segments clearly behind. Fixed ±5pp bandwidth, cluster(PC × GE year). Difference-in-discontinuity tests (Close vs. Not Close) are reported in the text. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Rival vote shares. If voters switch to the party's principal rival, the rival's vote share should increase. Tables E.5 and E.6 examine the vote share of the party's strongest opponent in each segment. The estimates are positive, 0.73 and 0.58 points at ±5 points and 1.28 points at the MSE bandwidth, but none is significant ($p = 0.50$, 0.55, and 0.16). The largest gain is in Previously Close segments, at 2.4 and 2.2 points ($p = 0.076$ and $p = 0.080$). This is consistent with some consolidation of support behind the principal rival in competitive segments, but the evidence is weak. The strongest opponent is also defined from the subsequent election and therefore need not be the same party on both sides of the threshold.

Table 12: Mechanism Test: AC-Level Win Probability by Margin Against Strongest Opponent

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.13**	-0.15**	-0.090	-0.088	-0.15*	-0.16*
	(0.062)	(0.061)	(0.066)	(0.065)	(0.088)	(0.088)
Competitiveness	Clearly_Losing	Clearly_Losing	Previously_Close	Previously_Close	Safely_Held	Safely_Held
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	591	591	842	842	290	290
Obs Right	509	509	849	849	368	368
Bias-Corrected Est.	-0.14	-0.15	-0.091	-0.088	-0.15	-0.16
Robust CI Lower	-0.26	-0.28	-0.22	-0.22	-0.33	-0.33
Robust CI Upper	-0.0081	-0.029	0.040	0.041	0.021	0.019
Robust SE	0.065	0.064	0.067	0.066	0.089	0.089
Robust p-value	0.037	0.016	0.17	0.18	0.085	0.080

Notes: Sequential-elections sample (see Section 8.3). Outcome is a binary indicator for winning the specific assembly segment. Competitiveness is based on the party's prior-AE margin against its own strongest opponent in that specific segment (Safely Held $> +10$pp, Previously Close within ± 10pp, Clearly Losing < -10pp), not the party's own vote share in isolation. Fixed ± 5pp bandwidth, cluster(PC $\times$ GE year). Pairwise equality tests are reported in the text. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Margin against the strongest rival. Aggregate vote share could mask an offsetting composition shift: the focal party might lose ground to its strongest rival while gaining from minor candidates, leaving its own vote share roughly unchanged even as its position against the rival that actually determines the segment worsens. I construct this margin directly, the focal party's AE vote share minus its strongest opponent's AE vote share in the same segment, and re-estimate the RD effect on it. The overall effect is small and insignificant, 0.01 and -0.05 points at ± 5pp ($p = 0.97$ and $p = 0.99$) and -1.31 points at the MSE bandwidth ($p = 0.65$; Table 13), so this outcome shows no more of a decline than either of its two components taken separately. Splitting by the same Close/Not-Close classification at ± 15pp gives -2.89 and -3.13 points in Close segments ($p = 0.25$ and $p = 0.21$) and $+3.26$ and $+0.63$ in Not-Close segments ($p = 0.37$ and $p = 0.83$; Table 14), the same directional pattern as the vote-share-only split above and, among every split reported in this subsection, the closest any estimate comes to conventional significance, though still well short of it.

Concentration of support. One way a party can lose seats without losing votes is by concentrating its votes in its strongholds. The data do not support this account. The dispersion of the party's vote share across its segments, measured by the standard deviation, declines by about one point (-0.98 and -1.03, $p = 0.048$ and $p = 0.034$), while the coefficient of variation, which scales the spread by the average, shows a small, insignificant decline ($p = 0.16$ and $p = 0.15$; Table E.7). If anything, winners' support becomes slightly more uniform across segments rather than more concentrated.

10.2 Government Status and Election Timing

Two further accounts make specific, and partly conflicting, predictions. A referendum account holds that voters treat the assembly election as a judgment on the party in office, so the penalty should be larger when the party actually holds power, whether nationally or in the

Table 13: Mechanism Test: AC-Level Vote Margin Against Strongest Opponent

	(1) Fixed 5pp	(2) Fixed 5pp	(3) Own MSE
RD_Estimate	0.0080	-0.051	-1.31
	(2.16)	(2.16)	(1.57)
Controls	N	Y	N (MSE bw)
Bandwidth (%)	5	5	12.1
Obs Left	2433	2433	5128
Obs Right	2572	2572	5467
Bias-Corrected Est.	0.091	0.024	-0.84
Robust CI Lower	-4.23	-4.30	-4.40
Robust CI Upper	4.41	4.35	2.72
Robust SE	2.20	2.21	1.82
Robust p-value	0.97	0.99	0.65

Notes: Sequential-elections sample (see Section 8.3). Outcome is the focal party's AE vote share minus its strongest opponent's AE vote share, both within the specific assembly segment. Cols 1–2: fixed ±5pp bandwidth; col 3: own-sample MSE-optimal bandwidth. cluster(PC × GE year). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 14: Mechanism Test: AC-Level Vote Margin Against Strongest Opponent by Close vs. Not-Close Competitiveness

	(1)	(2)	(3)	(4)
RD_Estimate	-2.89	-3.13	3.26	0.63
	(2.48)	(2.46)	(3.79)	(3.56)
Group	Close	Close	NotClose	NotClose
Controls	N	Y	N	Y
Bandwidth (%)	5	5	5	5
Obs Left	1065	1065	522	522
Obs Right	1117	1117	529	529
Bias-Corrected Est.	-2.92	-3.16	3.49	0.77
Robust CI Lower	-7.85	-8.07	-4.09	-6.33
Robust CI Upper	2.02	1.75	11.1	7.87
Robust SE	2.52	2.50	3.87	3.62
Robust p-value	0.25	0.21	0.37	0.83

Notes: Sequential-elections sample (see Section 8.3). Outcome as in Table 13. Close/Not Close classification as in Table 11, threshold ±15pp. Fixed ±5pp bandwidth, cluster(PC × GE year). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

state, than when it does not; applied to the time elapsed since the general election, the same logic predicts the penalty is largest while the parliamentary result is still fresh in voters' minds and fades as it recedes from memory. A honeymoon account, in the spirit of Ravishankar (2009), instead holds that holding office confers an initial boost that fades within a year or two; layered on top of a persistent penalty, this predicts the opposite time pattern from the referendum account: the net penalty should be smallest soon after the general election, while the honeymoon boost still partly offsets it, and should grow larger as that boost wears off. I test both using stacked local linear interaction models at ±5 points, as in Appendix D.5.

Table 16 compares the prime minister's party (Congress in 2004 and 2009; BJP in 2014 and 2019) with all other parties. Estimates are −6.3 and −7.0 points for the former and −8.0 and −7.6 for the latter: if anything smaller, not larger, for the governing party, the opposite of what the referendum account predicts, though the interaction is far from significant ($F(1, 416) = 0.24$, $p = 0.62$, 95 percent interval $[-10.0, 16.7]$ points) and both subgroup estimates are individually imprecise (p between 0.097 and 0.35). "Other" includes national coalition partners and is not a pure opposition group, which works against finding a clean contrast. Table 17 compares parties holding the chief ministership at the assembly election with those that do not, and here the pattern points the other way, toward the referendum account: estimates are −10.3 and −8.9 points for governing parties and −8.1 and −7.3 for others, again with an insignificant interaction ($F(1, 416) = 0.24$, $p = 0.62$) and individually imprecise estimates (p between 0.099 and 0.16). Because state government status can change after the parliamentary election, this second comparison is descriptive rather than predetermined. Taken together, the two office-status tests point in opposite directions and neither interaction approaches significance, so neither supports nor rules out a referendum account; the data are simply too imprecise to say.

Table 15 tests the timing predictions directly. The interval between elections, measured from first polling dates, is interacted with treatment; among later-year assembly elections it ranges from 9 to 67 months (mean 32). The continuous interaction is small and insignificant on its own (−0.0024 points of seat share per month, $F(1, 343) = 0.29$, $p = 0.59$, 95 percent interval $[-1.11, 0.63]$ points), which by itself is uninformative about either account. Splitting the sample into terciles by this interval gives estimates of −0.129, −0.044, and −0.064 at mean gaps of 20, 30, and 47 months (only the shortest-interval estimate is marginally significant, $p = 0.070$): the penalty is largest at the shortest interval and smaller, though not monotonically, at longer ones. Descriptively, this pattern sits closer to the referendum account, under which the penalty fades as the parliamentary result recedes from memory, than to the honeymoon account, which predicts the opposite, a penalty that grows over time as an initial office-holding boost wears off. But the tercile pattern is not monotonic, only the shortest-interval estimate clears conventional significance, and it is not formally distinguishable from the flat continuous interaction. I therefore do not read this as evidence favoring the referendum account over the honeymoon account, only as evidence against the specific claim that the penalty grows with time.

The interval between elections is measured from their first polling dates. Among later-year assembly elections, it ranges from 9 to 67 months, with a mean of 32. The maximum is the 2009 parliamentary election matched to Jammu and Kashmir's December 2014 assembly election, its first since 2008. The interaction estimate is −0.24 seat-share points per month ($F(1, 343) = 0.29$, $p = 0.59$, 95 percent interval $[-1.11, 0.63]$). Tercile estimates are −12.9,

Table 15: Mechanism Test: Seat Share by Interval Between Elections

	Continuous (per month)	Short (20mo)	Mid (30mo)	Long (47mo)
RD Estimate	−0.0024	−0.129*	−0.044	−0.064
p-value	0.59	0.070	0.578	0.413

Notes: Sequential-elections sample, fixed ±5pp bandwidth, cluster(PC × GE year). "Continuous" interacts treatment with the number of months between the general and assembly elections ($F(1, 343) = 0.29$, $p = 0.59$, 95% CI $[-1.11, 0.63]$ points); the tercile columns split the same sample by that interval, with mean gaps of 20, 30, and 47 months. Only the shortest-interval estimate is significant at conventional levels. $^{*}p < 0.10$.

Table 16: Mechanism Test: Seat Share by National Prime-Minister-Party Status

	(1)	(2)	(3)	(4)
RD_Estimate	-0.0803*	-0.0756	-0.0627	-0.0695
	(0.0490)	(0.0481)	(0.0670)	(0.0617)
Group	Other	Other	Ruling	Ruling
Controls	N	Y	N	Y
Bandwidth (%)	5	5	5	5
Obs Left	307	307	106	106
Obs Right	261	261	147	147
Bias-Corrected Est.	-0.0833	-0.0789	-0.0639	-0.0711
Robust CI Lower	-0.182	-0.176	-0.197	-0.194
Robust CI Upper	0.0151	0.0178	0.0693	0.0518
Robust SE	0.0502	0.0493	0.0680	0.0627
Robust p-value	0.0971	0.110	0.347	0.257

Notes: Sequential-elections sample. Ruling party is the single party holding the Prime Minister's office after the relevant GE (Congress for 2004/2009, BJP for 2014/2019); "Other" pools all remaining parties, including national coalition partners of the ruling party, and is not a strict political opposition. Fixed ±5pp bandwidth, cluster(PC × GE year). Interaction test $F(1, 416) = 0.242$, $p = 0.623$. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

−4.4, and −6.4 points at mean gaps of 20, 30, and 47 months; only the first is marginally significant ($p = 0.070$). These estimates do not establish whether the penalty fades with time.

10.3 Candidates and Turnout

Tables E.8–E.10 test whether winning parties field less experienced assembly candidates. The measures are sitting-MLA incumbency, prior terms won, and recontesting the same seat. The incumbency and prior-terms estimates are insignificant throughout ($p \geq 0.11$). Recontesting estimates are positive in the strongest segments (10 and 9 points, $p = 0.063$ and $p = 0.098$), although the intervals allow declines of 1 to 2 points. These tests concern experience and continuity; they do not measure candidate quality or campaign effort more broadly.

Turnout cannot be examined within this design. Turnout is a characteristic of the segment rather than of either party, so it takes identical values for the winner and runner-up of each race, and the mirrored sample constrains the estimate to zero by construction. Raw

Table 17: Mechanism Test: Seat Share by State-Government Status

	(1)	(2)	(3)	(4)
RD_Estimate	-0.0809*	-0.0728	-0.103	-0.0892
	(0.0494)	(0.0482)	(0.0675)	(0.0645)
Group	NotGoverns	NotGoverns	Governs	Governs
Controls	N	Y	N	Y
Bandwidth (%)	5	5	5	5
Obs Left	278	278	135	135
Obs Right	239	239	169	169
Bias-Corrected Est.	-0.0832	-0.0752	-0.106	-0.0916
Robust CI Lower	-0.182	-0.172	-0.240	-0.220
Robust CI Upper	0.0157	0.0214	0.0277	0.0363
Robust SE	0.0505	0.0493	0.0682	0.0653
Robust p-value	0.0992	0.127	0.120	0.160

Notes: Sequential-elections sample. "Governs" indicates the focal party held the state government (the Chief Minister's party) at the time of the relevant assembly election, hand-coded from Election Commission of India schedules and state-by-state election records; always coded as not governing under President's/Governor's Rule. Fixed ±5pp bandwidth, cluster(PC × GE year). Interaction test $F(1, 416) = 0.240$, $p = 0.624$. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

turnout is essentially constant across terciles (70.9, 70.6, and 70.8 percent). A design with one observation per race would be required to examine turnout.

10.4 Interpretation

The mechanism tests do not identify a single explanation. There is no detectable reduction in contesting or candidate experience, and no evidence of greater concentration of votes in strongholds. Comparisons by government status and timing remain too imprecise to distinguish sizable differences.

A small decline in support remains possible, as the simulation in Section 8.4 shows. The weak rival-vote evidence does not establish voter balancing, and candidate experience does not reveal how much effort parties put into campaigns. Distinguishing these accounts would require campaign spending, party-worker deployment, or alliance records. The last are also needed to establish whether seats lost by the party went to a rival or an ally.

11 Conclusion

A narrow parliamentary win reduces the party's seat share in the next state assembly election by 8 to 9 percentage points. The result holds after excluding simultaneous elections and adjusting for prior assembly performance. It documents a cost of winning at one level of government that appears in representation at another.

Aggregate vote share shows no detectable change, but the estimates allow a small decline that could translate into substantial seat losses under FPTP. The mechanism remains unresolved. Parties do not detectably contest fewer seats or field less experienced candidates, but neither result measures campaign effort directly.

The findings qualify the constituency-level version of the "double engine" argument: a party's local parliamentary victory need not strengthen its subsequent assembly position. They also show why electoral spillovers should be assessed using both votes and seats.

Campaign and alliance data could help explain the seat loss. Further work could also examine spillovers in the reverse direction, from assembly victories to parliamentary elections, and whether cross-level electoral losses affect public goods or local development.

References

Broockman, David E., "Do Congressional Candidates Have Reverse Coattails? Evidence from a Regression Discontinuity Design," *Political Analysis*, 2009, 17 (4), 418–434.

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

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, 2008, 90 (3), 414–427.

Campbell, Angus, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter*, Chicago: University of Chicago Press, 1960.

Cattaneo, Matias D., Brigham R. Frandsen, and Rocío Titiunik, "Randomization Inference in the Regression Discontinuity Design: An Application to Party Advantages in the U.S. Senate," *Journal of Causal Inference*, 2015, 3 (1), 1–24.

Eggers, Andrew C., Anthony Fowler, Jens Hainmueller, Andrew B. Hall, and James M. Snyder, "On the Validity of the Regression Discontinuity Design for Estimating Electoral Effects: New Evidence from Over 40,000 Close Races," *American Journal of Political Science*, 2015, 59 (1), 259–274.

Folke, Olle and James M. Snyder, "Gubernatorial Midterm Slumps," *American Journal of Political Science*, 2012, 56 (4), 931–948.

Gelman, Andrew and Guido Imbens, "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs," *Journal of Business and Economic Statistics*, 2019, 37 (3), 447–456.

Hogan, Robert E., "Gubernatorial Coattail Effects in State Legislative Elections," *Political Research Quarterly*, 2005, 58 (4), 587–597.

Lee, David S., "Randomized Experiments from Non-Random Selection in U.S. House Elections," *Journal of Econometrics*, 2008, 142 (2), 675–697.

Linden, Leigh L., "Are Incumbents Really Advantaged? The Preference for Non-Incumbents in Indian National Elections," Working Paper, 2004. Columbia University.

McCrary, Justin, "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test," *Journal of Econometrics*, 2008, 142 (2), 698–714.

Miller, Warren E., "Presidential Coattails: A Study in Political Myth and Methodology," *Public Opinion Quarterly*, 1955, 19 (4), 353–368.

Ravishankar, Nirmala, "The Cost of Ruling: Anti-Incumbency in Elections," *Economic and Political Weekly*, 2009, 44 (10), 92–98.

Uppal, Yogesh, "The Disadvantaged Incumbents: Estimating Incumbency Effects in Indian State Legislatures," *Public Choice*, 2009, 138 (1), 27–49.

A Additional Validity Checks

This appendix reports the validity checks discussed in Section 6 that are not shown in the main text: the density tests and binomial sign tests (Section 6.1), covariate balance (Section 6.2), balance on the second-prior assembly election, the first-differenced estimates, and local randomization inference (Section 6.3), the timing comparisons (Section 6.4), and the placebo thresholds.

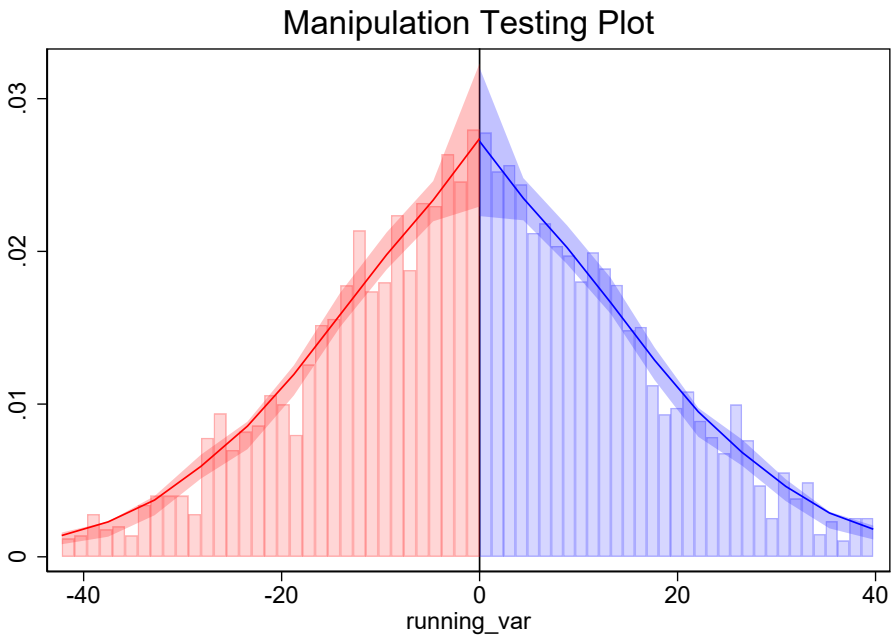


Figure A.1: Density Continuity Test (McCrary, 2008)

Notes: Distribution of the national-election margin of victory around the cutoff, pooling winner and runner-up rows. $T = -0.1285$, $p = 0.8977$. Because each race contributes a winner and a runner-up at mirrored margins, this pooled distribution is symmetric by construction (Section 6.1); Figures A.2 and A.3 report tests with one observation per race.

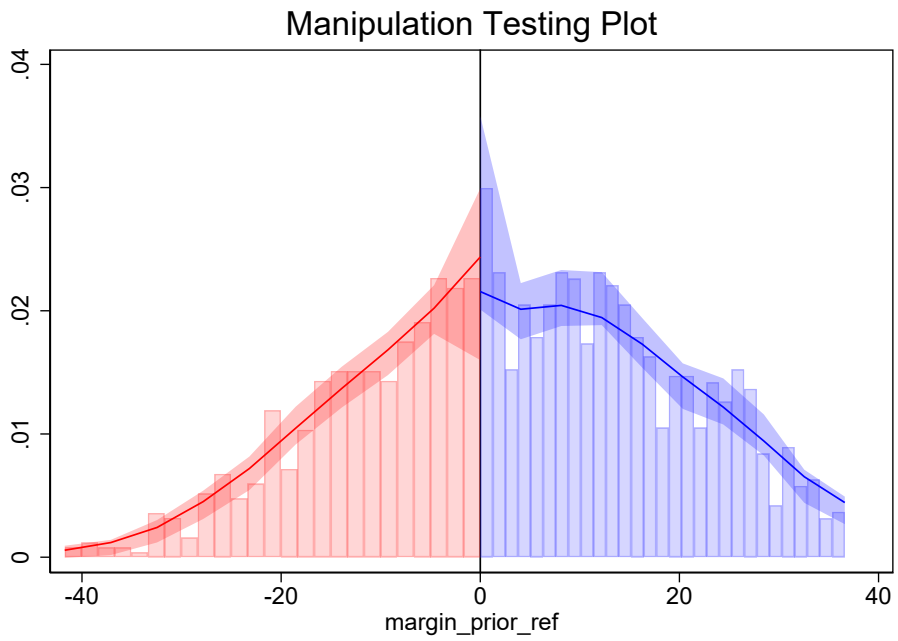


Figure A.2: Density Continuity Test, One Row Per Race: Prior-AE Reference Rule

Notes: One row per race ($N = 1{,}507$); the reference party is whichever of the two top finishers had the higher vote share in the assembly election preceding this general election, a rule fixed before the general election result is known. $T = 0.905$, $p = 0.365$. The reference party wins 59.4% of races.

Table A.1: Binomial Sign Tests Around the Cutoff

Reference rule	Window (pp)	N	Above cutoff	Share	p-value
Prior-AE	±0.5	49	27	0.551	0.568
Prior-AE	±1	80	46	0.575	0.219
Prior-AE	±2	155	88	0.568	0.108
Alphabetical	±0.5	65	33	0.508	1.000
Alphabetical	±1	107	55	0.514	0.847
Alphabetical	±2	216	111	0.514	0.734

Notes: For each reference-party running variable and window width, the reference party is above the cutoff (i.e. won this GE) in "Above cutoff" of "N" races within the window. The p-value is from a two-sided exact binomial test against a null share of 0.5. None of the six tests rejects that null.

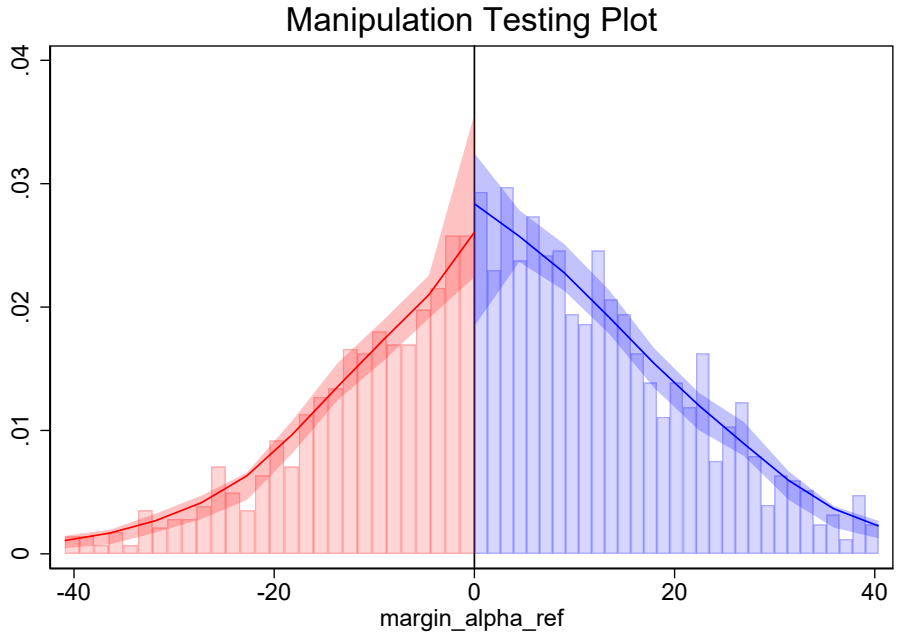


Figure A.3: Density Continuity Test, One Row Per Race: Alphabetical Reference Rule

Notes: One row per race ($N = 1{,}937$, no missing data); the reference party is whichever of the two top finishers' party names comes first alphabetically, a rule fixed before the election and independent of its result; the alphabetically first party wins 59.1% of races. $T = -0.702$, $p = 0.483$. This rule is available for all races, including the 430 without a usable prior assembly election.

Table A.2: Covariate Balance: RD Estimates

	(1) Incumbent	(2) Female	(3) Turncoat
RD_Estimate	0.0019	-0.0057	0.033
	(0.039)	(0.025)	(0.023)
Bandwidth (%)	14.2	14.2	14.2
Obs Left	1211	1211	1211
Obs Right	1210	1210	1210
Bias-Corrected Est.	0.011	-0.0054	0.034
Robust CI Lower	-0.079	-0.062	-0.018
Robust CI Upper	0.10	0.051	0.086
Robust SE	0.046	0.029	0.027
Robust p-value	0.81	0.85	0.20

Notes: Dependent variable in each column is the labelled covariate, predetermined and varying between the winner and runner-up of a race. Number of candidates and number of assembly segments (race-level, balanced by construction in the two-rows-per-race sample) and general-election vote share (determined in the same election as treatment) are not reported here; see text. MSE-optimal bandwidth (≈14.2%) used throughout. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses.

Table A.3: Balance on the Second-Prior (Two-Elections-Back) Assembly Outcome

	(1) Vote Shr	(2) Vote Shr
RD_Estimate	-3.668	-2.039
	(1.998)	(2.926)
Bandwidth	MSE	Fixed 5pp
Bandwidth (%)	12.12	5
Obs Left	488	228
Obs Right	488	227
Bias-Corrected Est.	-3.626	-2.031
Robust CI Lower	-8.304	-7.905
Robust CI Upper	1.052	3.842
Robust SE	2.387	2.997
Robust p-value	0.129	0.498

Notes: Dependent variable is the party's own vote share in the assembly election immediately before the one used in Table 2 (i.e. two full AE cycles before the GE). The corresponding seat-share balance check, on an identical sample, is reported in Table A.4. Races with no usable second-prior AE, or one under a different delimitation, are excluded. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.4: Prior versus Second-Prior Seat Share, Identical Sample, Common Bandwidths

	(1)	(2)	(3)	(4)
RD_Estimate	0.0364	-0.0240	0.0735*	-0.0318
	(0.0305)	(0.0215)	(0.0407)	(0.0356)
Election	Prior (t-1)	Prior2 (t-2)	Prior (t-1)	Prior2 (t-2)
Bandwidth	Own MSE	Own MSE	Fixed 5pp	Fixed 5pp
Bandwidth (%)	9.221	13.54	5	5
Obs Left	835	1150	490	490
Obs Right	833	1148	485	485
Bias-Corrected Est.	0.0475	-0.0274	0.0768	-0.0323
Robust CI Lower	-0.0212	-0.0778	-0.00603	-0.103
Robust CI Upper	0.116	0.0231	0.160	0.0389
Robust SE	0.0350	0.0258	0.0423	0.0363
Robust p-value	0.175	0.288	0.0692	0.374

Notes: Dependent variable is state assembly seat share. Sample restricted to races with a usable outcome at both the prior ($t-1$) and second-prior ($t-2$) election (N=1,668), identical across all four columns, so columns differ only in which election is tested and which bandwidth is used. Columns 1–2: MSE-optimal bandwidth, selected independently for each column. Columns 3–4: common fixed ±5pp bandwidth. A separate stacked-regression test, reported in the text, formally confirms the $t-1$ and $t-2$ discontinuities differ at the fixed bandwidth ($p = 0.031$) but not at a wider ±10pp bandwidth ($p = 0.24$). Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

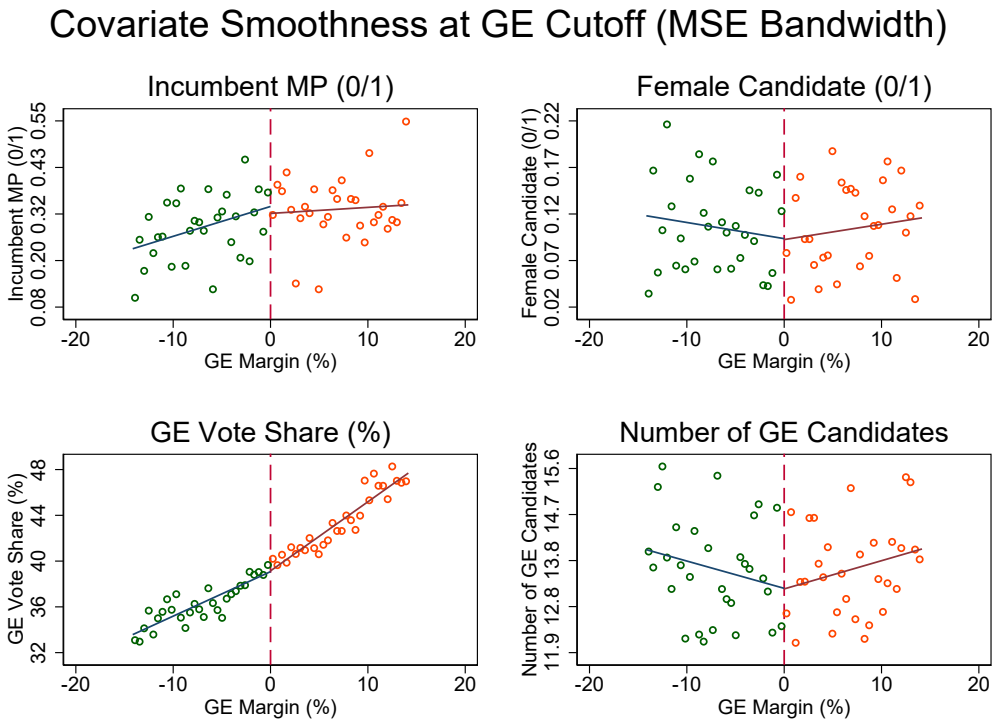


Figure A.4: Covariate Balance at the Cutoff

Notes: Binned means and local linear fits for each covariate. GE vote share is determined in the same election as treatment, and the number of GE candidates is race-level and balanced by construction (Section 6.2). MSE-optimal bandwidth (≈14.2%).

Table A.5: First-Differenced Seat Share (Change from Prior AE), Full Pooled Sample

	(1) Fixed 5pp	(2) Fixed 5pp	(3) Own MSE
RD_Estimate	-0.177***	-0.173***	-0.143***
	(0.0455)	(0.0442)	(0.0375)
Controls	N	Y	N (MSE bw)
Bandwidth (%)	5	5	7.639
Obs Left	498	498	715
Obs Right	493	493	712
Bias-Corrected Est.	-0.184	-0.180	-0.160
Robust CI Lower	-0.277	-0.271	-0.241
Robust CI Upper	-0.0910	-0.0897	-0.0780
Robust SE	0.0475	0.0462	0.0416
Robust p-value	0.000107	0.0000944	0.000126

Notes: Outcome is AE seat share minus prior AE seat share, on the same usable-prior-AE subsample as Table 3. Cols 1–2: fixed ±5pp bandwidth; col 3: own-sample MSE-optimal bandwidth (7.6pp). Differencing assumes a stable untreated discontinuity across elections; this is a sensitivity exercise, not a causal bound. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

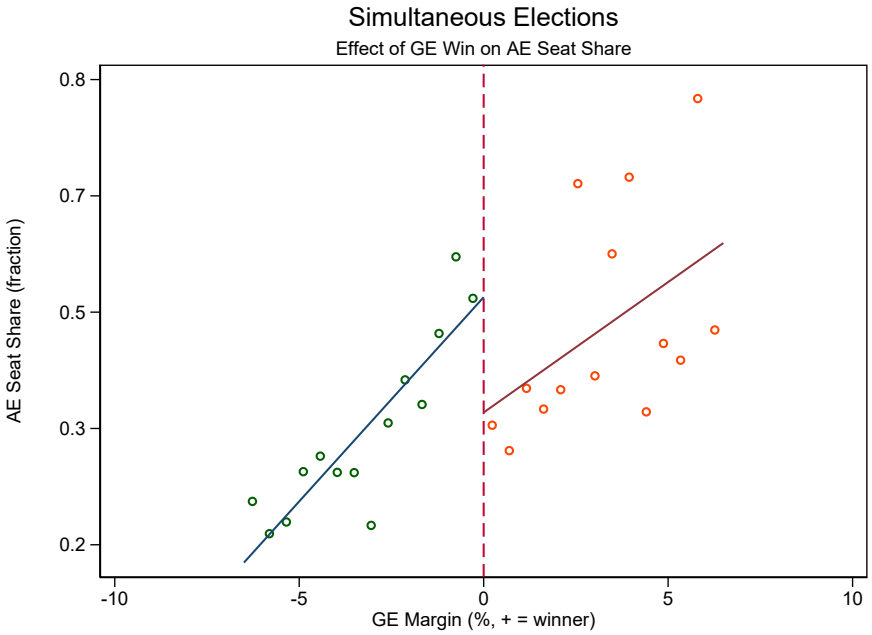


Figure A.5: State Assembly Seat Share: Local Linear RD Plot (Simultaneous Elections)

Notes: Simultaneous-election subsample only (see Table A.8). Own-MSE-optimal bandwidth, triangular kernel, local linear fit. This discontinuity cannot reflect a response to the parliamentary result, since these assembly votes were cast before the result was known; see the discussion following Table A.8.

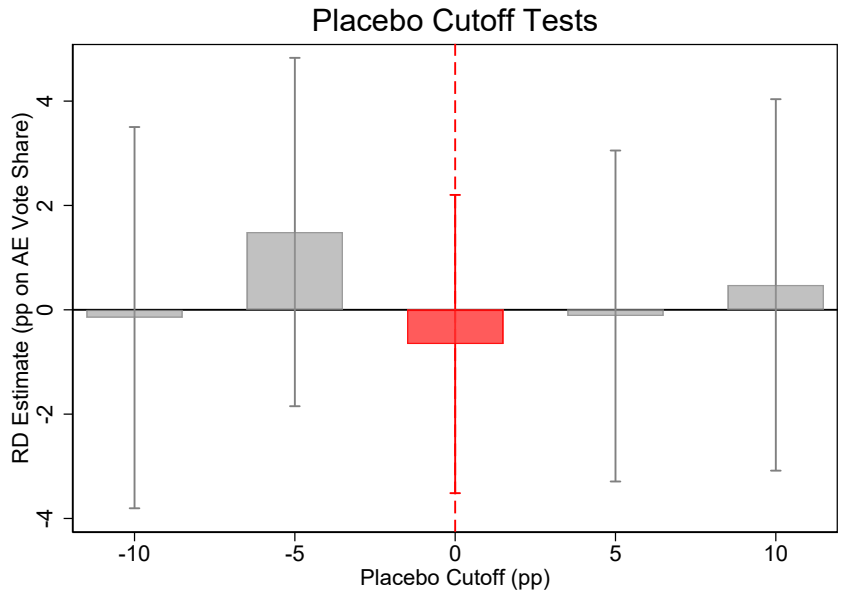


Figure A.6: Placebo Cutoff Tests

Notes: Outcome is state-election vote share. Gray bars: estimates at four placebo cutoffs. Red bar: estimate at the true cutoff, whose interval also includes zero, consistent with the vote-share null in Table 4.

Table A.6: First-Differenced Seat Share (Change from Prior AE), Sequential-Elections Sample

	(1) Fixed 5pp	(2) Fixed 5pp	(3) Own MSE
RD_Estimate	-0.162***	-0.152***	-0.133***
	(0.0490)	(0.0481)	(0.0384)
Controls	N	Y	N (MSE bw)
Bandwidth (%)	5	5	8.713
Obs Left	409	409	665
Obs Right	404	404	664
Bias-Corrected Est.	-0.168	-0.157	-0.148
Robust CI Lower	-0.267	-0.255	-0.233
Robust CI Upper	-0.0678	-0.0590	-0.0636
Robust SE	0.0509	0.0499	0.0431
Robust p-value	0.000993	0.00168	0.000593

Notes: Outcome as in Table A.5, restricted to the sequential-elections sample (see Section 8.3). Cols 1–2: fixed ±5pp bandwidth; col 3: own-sample MSE-optimal bandwidth (8.7pp). Differencing assumes a stable untreated discontinuity across elections; this is a sensitivity exercise, not a causal bound. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.7: Local Randomization Inference, Narrow Windows Around the Cutoff

Window (pp)	Obs (L/R)	Prior Seat Share: Diff. (p)	Current Seat Share: Diff. (p), $p(0)$	Current Seat Share: Diff. (p), $p(1)$
±2 (data-driven)	217 / 216	0.039 (0.162)	−0.042 (0.108)	–
±3	299 / 299	0.029 (0.206)	−0.050** (0.022)	−0.094** (0.023)
±4	400 / 398	0.008 (0.698)	−0.027 (0.166)	–
±5	498 / 493	0.019 (0.298)	−0.025 (0.142)	−0.083** (0.012)

Notes: Randomization-inference p-values (Cattaneo et al., 2015), via `rdrandinf`, uniform kernel, usable-prior-AE subsample. The ±2pp window is selected by a covariate-balance test over the same covariates used in Table 2 and is used as the primary window; wider windows are shown for comparison and are rejected by the same balance test. $p(0)$ is the simple finite-sample permutation difference in means (no covariates, no extrapolation beyond the window); $p(1)$ adds a local-linear adjustment for the running variable, reported as a large-sample HC3 p-value. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.8: Seat Share by Election Timing

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.096***	-0.095***	-0.0090	-0.0030	-0.19***	-0.21***
	(0.027)	(0.026)	(0.046)	(0.044)	(0.054)	(0.050)
Timing Group	True Sequential	True Sequential	Same-Year Sequential	Same-Year Sequential	Simultaneous	Simultaneous
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	16.5	16.5	15.8	15.8	6.56	6.56
Obs Left	959	959	169	169	115	115
Obs Right	958	958	167	167	115	115
Bias-Corrected Est.	-0.10	-0.10	-0.022	-0.013	-0.22	-0.24
Robust CI Lower	-0.16	-0.16	-0.13	-0.11	-0.34	-0.35
Robust CI Upper	-0.042	-0.043	0.082	0.087	-0.099	-0.13
Robust SE	0.031	0.031	0.053	0.051	0.061	0.056
Robust p-value	0.00089	0.00076	0.68	0.80	0.00033	0.000024

Notes: Dependent variable is state assembly seat share. True Sequential: assembly election held in a later calendar year than the national election. Same-Year Sequential: assembly election held in the same calendar year but polled after the national result (e.g. October or December). Simultaneous: assembly election polled alongside the national election and counted the same day. MSE-optimal bandwidth computed separately for each group. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.9: Vote Share by Election Timing

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-1.20	-1.23	2.55	2.80	-2.63	-2.86*
	(1.31)	(1.19)	(2.18)	(2.07)	(1.97)	(1.54)
Timing Group	True Sequential	True Sequential	Same-Year Sequential	Same-Year Sequential	Simultaneous	Simultaneous
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	13.8	13.8	14.2	14.2	7.65	7.65
Obs Left	839	839	156	156	128	128
Obs Right	837	837	155	155	128	128
Bias-Corrected Est.	-1.52	-1.63	2.98	3.34	-3.12	-3.32
Robust CI Lower	-4.53	-4.35	-2.08	-1.46	-7.63	-6.84
Robust CI Upper	1.50	1.09	8.04	8.13	1.40	0.20
Robust SE	1.54	1.39	2.58	2.45	2.31	1.80
Robust p-value	0.32	0.24	0.25	0.17	0.18	0.065

Notes: Dependent variable is state assembly vote share. Timing groups defined as in Table A.8. MSE-optimal bandwidth computed separately for each group. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.10: Seat Share Excluding Simultaneous Elections

	(1)	(2)	(3)	(4)
RD_Estimate	-0.0981***	-0.0979***	-0.0825***	-0.0812***
	(0.0251)	(0.0248)	(0.0240)	(0.0235)
Sample	Full	Full	Excl. Simultaneous	Excl. Simultaneous
Controls	N	Y	N	Y
Bandwidth (%)	10.55	10.55	15.89	15.89
Obs Left	946	946	1101	1101
Obs Right	944	944	1098	1098
Bias-Corrected Est.	-0.109	-0.109	-0.0900	-0.0887
Robust CI Lower	-0.163	-0.162	-0.144	-0.142
Robust CI Upper	-0.0544	-0.0553	-0.0361	-0.0358
Robust SE	0.0277	0.0273	0.0275	0.0270
Robust p-value	0.0000866	0.0000683	0.00106	0.00101

Notes: Dependent variable is state assembly seat share. "Full" is the baseline sample used throughout the paper (all timing groups pooled). "Excl. Simultaneous" drops the simultaneous-election races identified in this section (true sequential plus same-year-sequential elections only, 86.2 percent of the baseline sample). MSE-optimal bandwidth computed separately for each sample. Robust bias-corrected SE clustered at the parliamentary constituency × election cycle level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.11: Vote Share Excluding Simultaneous Elections

	(1)	(2)	(3)	(4)
RD_Estimate	-0.657	-0.698	-0.527	-0.519
	(1.025)	(0.941)	(1.140)	(1.051)
Sample	Full	Full	Excl. Simultaneous	Excl. Simultaneous
Controls	N	Y	N	Y
Bandwidth (%)	14.16	14.16	15.02	15.02
Obs Left	1211	1211	1061	1061
Obs Right	1210	1210	1059	1059
Bias-Corrected Est.	-0.859	-0.929	-0.745	-0.773
Robust CI Lower	-3.216	-3.092	-3.376	-3.198
Robust CI Upper	1.497	1.234	1.887	1.651
Robust SE	1.202	1.104	1.343	1.237
Robust p-value	0.475	0.400	0.579	0.532

Notes: Same samples and specification as Table A.10. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.12: Placebo Cutoff Tests: RD Estimates

	(1) -10pp	(2) -5pp	(3) +5pp	(4) +10pp
RD_Estimate	-3.56	3.53	-1.66	-1.67
	(3.19)	(2.88)	(2.68)	(2.72)
Placebo Cutoff	-10pp	-5pp	5pp	10pp
Bandwidth (%)	2.50	2.50	2.50	2.50
Obs Left	182	205	240	200
Obs Right	200	244	207	184
Bias-Corrected Est.	-3.56	3.54	-1.67	-1.67
Robust CI Lower	-9.85	-2.13	-6.94	-7.18
Robust CI Upper	2.73	9.21	3.60	3.84
Robust SE	3.21	2.89	2.69	2.81
Robust p-value	0.27	0.22	0.53	0.55

Notes: Outcome is state-election vote share. Placebo cutoffs at −10pp, −5pp, +5pp, +10pp relative to the true threshold of zero. Fixed ±2.5pp bandwidth at each cutoff, chosen so no estimation window crosses the true cutoff. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A.13: Placebo Cutoff Tests: State Assembly Seat Share

	(1) -10pp	(2) -5pp	(3) +5pp	(4) +10pp
RD_Estimate	-0.0604	0.0327	0.0315	-0.0317
	(0.0650)	(0.0568)	(0.0600)	(0.0733)
Placebo Cutoff	-10pp	-5pp	5pp	10pp
Bandwidth (%)	2.500	2.500	2.500	2.500
Obs Left	182	205	240	200
Obs Right	200	244	207	184
Bias-Corrected Est.	-0.0611	0.0330	0.0321	-0.0319
Robust CI Lower	-0.190	-0.0791	-0.0860	-0.176
Robust CI Upper	0.0683	0.145	0.150	0.112
Robust SE	0.0660	0.0572	0.0602	0.0736
Robust p-value	0.355	0.564	0.594	0.665

Notes: Outcome is state assembly seat share. Placebo cutoffs at −10, −5, +5, +10pp. Fixed ±2.5pp bandwidth at each cutoff, chosen so no estimation window crosses the true cutoff. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

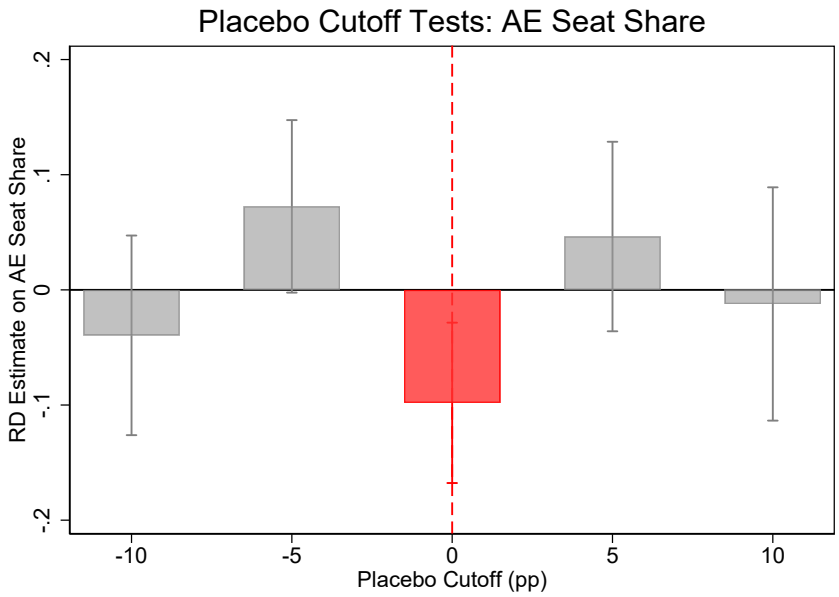


Figure A.7: Placebo Cutoff Tests: State Assembly Seat Share

Notes: Outcome is state assembly seat share. Gray bars: estimates at placebo cutoffs of −10, −5, +5, and +10pp. Red bar: estimate at the true cutoff, whose interval excludes zero.

B Additional Robustness Results

This appendix reports the separate RD plots for the main estimates (Section 7) and the robustness tables discussed in Section 8: the full pooled sample, the local quadratic specification, the same-calendar-year comparison, and the bandwidth summary.

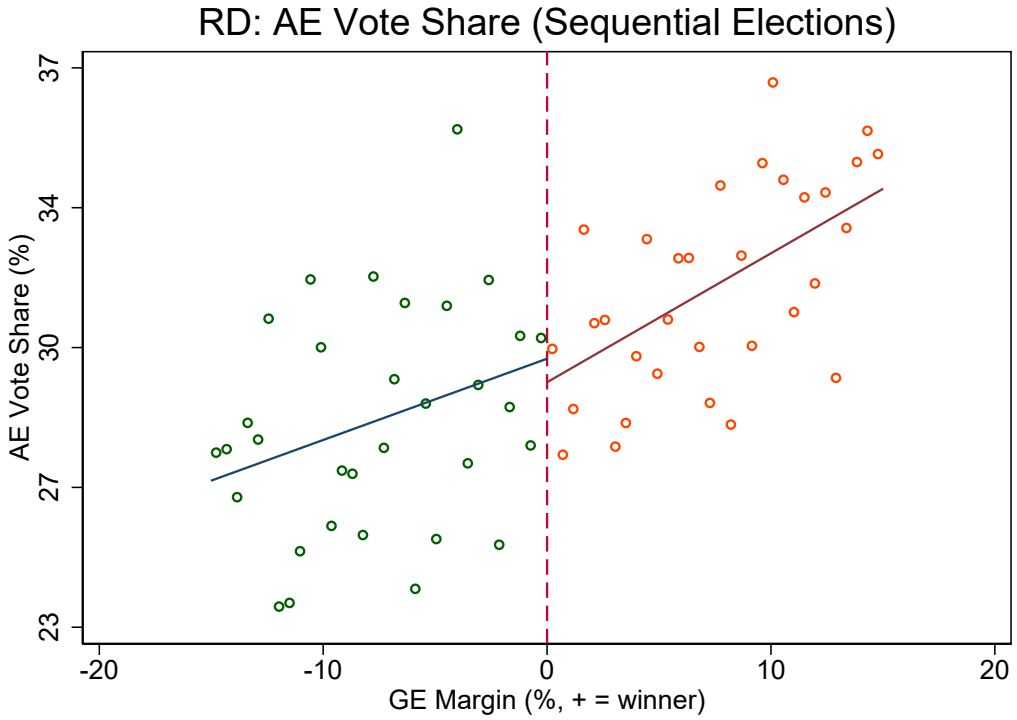


Figure B.1: State-Election Vote Share: Local Linear RD Plot (Sequential Elections)

Notes: Sequential-elections sample, MSE-optimal bandwidth. Dots are binned means; lines are local linear fits on each side of the cutoff.

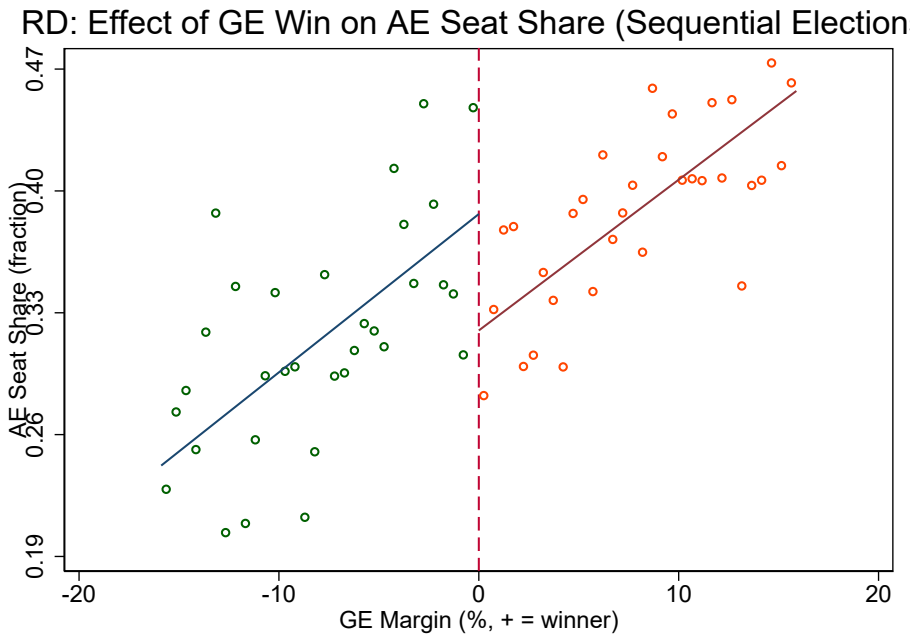


Figure B.2: State Assembly Seat Share: Local Linear RD Plot (Sequential Elections)

Notes: Sequential-elections sample, MSE-optimal bandwidth. Dots are binned means; lines are local linear fits on each side of the cutoff.

Table B.1: Robustness: Quadratic Polynomial

	AE Vote Share		AE Seat Share	
	(1)	(2)	(3)	(4)
RD_Estimate	-0.56	-0.55	-0.11***	-0.11***
	(1.35)	(1.10)	(0.031)	(0.029)
Bandwidth	MSE	MSE	MSE	MSE
Bandwidth (%)	17.0	17.0	14.9	14.9
Obs Left	1379	1379	1261	1261
Obs Right	1377	1377	1260	1260
Bias-Corrected Est.	-0.39	-0.40	-0.12	-0.12
Robust CI Lower	-3.35	-2.81	-0.18	-0.18
Robust CI Upper	2.57	2.00	-0.051	-0.053
Robust SE	1.51	1.23	0.034	0.033
Robust p-value	0.80	0.74	0.00056	0.00036
Controls	N	Y	N	Y

Notes: Local quadratic RD ($p = 2$), MSE-optimal bandwidth. Cols 1–2: vote share; cols 3–4: seat share. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table B.2: Robustness: Same-Calendar-Year (Concurrent) Assembly Elections

	Vote Share		Seat Share	
	(1)	(2)	(3)	(4)
RD_Estimate	0.40	0.47	-0.12***	-0.12***
	(1.58)	(1.42)	(0.042)	(0.041)
BW	MSE	MSE	MSE	MSE
Bandwidth (%)	15.3	15.3	7.70	7.70
Obs Left	373	373	225	225
Obs Right	373	373	224	224
Bias-Corrected Est.	0.41	0.53	-0.13	-0.14
Robust CI Lower	-3.24	-2.77	-0.23	-0.23
Robust CI Upper	4.06	3.83	-0.042	-0.049
Robust SE	1.86	1.68	0.047	0.045
Robust p-value	0.82	0.75	0.0042	0.0024
Controls	N	Y	N	Y

Notes: Concurrent: assembly election held in the same calendar year as the general election, whether simultaneously or later in that year; this group combines the Same-Year Sequential and Simultaneous rows of Table A.8 and Table A.9. The comparable estimate for assembly elections held in a later calendar year is −0.096 for seat share (Table A.8) and −1.20 for vote share (Table A.9). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table B.3: Robustness: Bandwidth Sensitivity Summary

	AE Vote Share				AE Seat Share			
	(1) CER	(2) MSE	(3) ±5*pp*	(4) ±10*pp*	(5) CER	(6) MSE	(7) ±5*pp*	(8) ±10*pp*
RD_Estimate	-0.38	-0.62	-0.21	-0.44	-0.099***	-0.097***	-0.10***	-0.098***
	(0.99)	(0.84)	(1.29)	(0.97)	(0.029)	(0.024)	(0.033)	(0.025)
BW Algorithm	CER	MSE	5pp	10pp	CER	MSE	5pp	10pp
Bandwidth (%)	9.36	14.2	5	10	6.97	10.5	5	10
Obs Left	858	1211	502	907	667	946	502	907
Obs Right	856	1210	497	904	663	944	497	904
Bias-Corrected Est.	-0.48	-0.83	-0.24	-0.55	-0.10	-0.11	-0.11	-0.11
Robust CI Lower	-2.57	-2.75	-2.81	-2.60	-0.16	-0.16	-0.17	-0.16
Robust CI Upper	1.61	1.09	2.33	1.51	-0.045	-0.056	-0.039	-0.055
Robust SE	1.07	0.98	1.31	1.05	0.030	0.027	0.034	0.027
Robust p-value	0.65	0.40	0.86	0.60	0.00055	0.000051	0.0017	0.000063

Notes: Full controls and year/state FE throughout. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

C Robustness to the 2008 Delimitation

India's Delimitation Commission redrew parliamentary and assembly boundaries in 2008, reassigning constituency numbers and restructuring the assembly-to-parliamentary mapping in most states. Because the two regimes are not directly comparable, I build the crosswalk under the 1973 order for the 2004 cycle and under the 2008 order for the 2009, 2014, and 2019 cycles.

Table C.1 reports the main estimates separately for the two delimitation regimes. The seat-share effect is -0.080 in 2004 ($p = 0.078$) and -0.101 in 2009–2019 ($p = 0.001$). The vote-share effect is insignificant in both, at -2.41 points ($p = 0.32$) and -0.04 points ($p = 0.90$). The 2004 estimate is less precise, as expected given 904 observations compared with 3,010, but the same pattern holds under both regimes: a decline in seats without a detectable decline in votes.

Table C.1: Robustness to the 2008 Delimitation: RD Estimates

	(1) SS Pre-2008	(2) SS Post-2008	(3) VS Pre-2008	(4) VS Post-2008
RD_Estimate	-0.0801*	-0.101***	-2.407	-0.0384
	(0.0407)	(0.0321)	(2.217)	(1.169)
Period	Pre-2008	Post-2008	Pre-2008	Post-2008
Bandwidth (%)	13.59	9.036	13.17	13.94
Obs Left	299	615	290	891
Obs Right	299	613	291	889
Bias-Corrected Est.	-0.0843	-0.115	-2.569	-0.175
Robust CI Lower	-0.178	-0.185	-7.663	-2.893
Robust CI Upper	0.00949	-0.0460	2.525	2.543
Robust SE	0.0479	0.0354	2.599	1.387
Robust p-value	0.0781	0.00111	0.323	0.899

Notes: Pre-2008 sample = 2004 cycle only; post-2008 = 2009, 2014, 2019 cycles. MSE-optimal bandwidth, local linear. Significance from the robust bias-corrected p-value (Section 6.3.1). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

D Heterogeneity: Full Results (Pooled Sample)

This appendix reports the full heterogeneity results summarized in Section 9. All estimates use the full pooled sample, including simultaneous elections. Each comparison divides an already local sample into smaller groups, so I focus on whether the effect is present within each group rather than on between-group differences, which are rarely significant.

D.1 State Development and Organizational Capacity

If a parliamentary victory strains a party's organization, the penalty should be larger where parties face tighter resource constraints. State development provides a rough proxy. Parties in wealthier states can draw on larger donor bases and more developed campaign infrastructure, and Uppal (2009) finds that the within-level incumbency disadvantage is larger in states with poorer public goods provision. I compare the BIMARU states (Bihar, Madhya Pradesh, Rajasthan, and Uttar Pradesh), India's poorest and most populous, with four of its most developed: Gujarat, Karnataka, Maharashtra, and Tamil Nadu.

The seat-share effect is -0.116 and -0.123 in the BIMARU states ($p = 0.048$ and $p = 0.024$) and -0.086 and -0.082 in the wealthier states ($p = 0.068$ and $p = 0.079$; Table D.1). Vote share shows no effect in either group (Table D.2). The difference between the groups is in the direction predicted by the organizational account, but it is small relative to its standard error: 3.0 points without controls ($z = -0.39$, $p = 0.70$) and 4.1 points with controls ($z = -0.53$, $p = 0.60$). The bandwidths are also selected separately for each group, which limits the comparability of the estimates. I interpret this comparison as showing that the effect is present in both poorer and wealthier states, not as evidence of a gradient.

D.2 Electoral Structure: Multiparty versus Two-Bloc States

One hypothesis is that FPTP amplification is stronger where votes are dispersed across more parties. I classify states by their parliamentary party system. The multiparty group comprises Assam, Bihar, Jammu and Kashmir, Jharkhand, Maharashtra, Manipur, Odisha, and Uttar Pradesh. The two-bloc group, characterized by single-party dominance or alternation between two alliances, comprises Andhra Pradesh, Goa, Himachal Pradesh, Kerala, Punjab, Tamil Nadu, Uttarakhand, and West Bengal.

The seat-share effect is -0.069 and -0.073 in multiparty states ($p = 0.060$ and $p = 0.047$) and -0.113 and -0.110 in two-bloc states ($p = 0.009$ and $p = 0.010$; Table D.3). Vote share shows no effect in either group (Table D.4). The difference runs opposite to the simple fragmentation prediction, although the confidence intervals overlap. One explanation is that a two-bloc parliamentary system does not imply a two-party assembly contest: West Bengal, Andhra Pradesh, and Tamil Nadu, for example, have highly competitive assembly elections. A more direct test would use the effective number of parties in assembly contests within each parliamentary constituency.

D.3 Regional Heterogeneity

Table D.7 reports negative estimates in every region: -0.10 and -0.11 in the north ($p = 0.021$ and $p = 0.007$), -0.14 and -0.12 in the east ($p = 0.017$ and $p = 0.032$), -0.13 and -0.11 in the west ($p = 0.022$ and $p = 0.026$), and -0.065 and -0.068 in the south ($p = 0.14$ and $p = 0.10$). The southern estimates are smaller and less precise. Without formal tests of regional differences, this ordering does not establish that southern party organizations buffer the penalty.

D.4 Heterogeneity by Party

A natural concern is that a single party drives the pooled result. Table D.8 and Figure D.3 estimate the effect separately for the BJP, the Congress, Left parties, and state-level regional parties. The estimates are similar across the three large groups: -0.11 for the BJP ($p = 0.041$ and $p = 0.031$), -0.095 and -0.099 for the Congress ($p = 0.058$ and $p = 0.040$), and -0.12 and -0.11 for regional parties ($p = 0.009$ and $p = 0.012$). The Left estimate is positive, but it rests on fewer than 100 observations and is not informative. No single party accounts for the result.

D.5 Electoral Nationalization and Organizational Capacity

The BJP substantially expanded its national organization after 2014. If organizational capacity mitigates the penalty, the effect may differ between the coalition-era elections of 2004 and 2009 and the BJP-majority elections of 2014 and 2019. The split also captures many other political changes, so it cannot isolate organization.

At the MSE bandwidth, the effect is -0.12 before 2014 ($p < 0.001$) and -0.060 and -0.056 after ($p = 0.065$ and $p = 0.078$; Table D.5 and Figure D.1). At a fixed ± 5 point bandwidth, the estimates for the two periods are similar, at about -0.10 before and -0.11 after (Table D.6 and Figure D.2). A stacked interaction test does not reject equal effects across periods ($F(1, 505) = 0.32$, $p = 0.57$). Because post-2014 organizational expansion was specific to the BJP, I also conduct the comparison separately for the BJP and for all other parties. The BJP estimate moves from -0.167 before 2014 ($p = 0.092$) to -0.036 after ($p = 0.72$), and the estimate for other parties from -0.074 ($p = 0.13$) to -0.130 ($p = 0.056$), but neither change is significant ($F(1, 251) = 1.84$, $p = 0.18$ for the BJP; $F(1, 503) = 0.02$, $p = 0.90$ for other parties). Neither the pooled nor the party-specific interaction tests establish a change across periods, so this comparison does not identify organizational buffering.

Table D.1: Heterogeneity by State Development Level: State Assembly Seat Share

	(1)	(2)	(3)	(4)
RD_Estimate	-0.116**	-0.123**	-0.0860*	-0.0819*
	(0.0479)	(0.0482)	(0.0429)	(0.0437)
Group	BIMARU	BIMARU	WEALTHY	WEALTHY
Controls	N	Y	N	Y
Bandwidth (%)	13.25	11.97	13.81	12.69
Obs Left	334	310	311	286
Obs Right	334	310	310	285
Bias-Corrected Est.	-0.113	-0.130	-0.0942	-0.0914
Robust CI Lower	-0.224	-0.242	-0.195	-0.193
Robust CI Upper	-0.00111	-0.0171	0.00702	0.0104
Robust SE	0.0569	0.0575	0.0517	0.0520
Robust p-value	0.0478	0.0239	0.0681	0.0786

Notes: BIMARU = Bihar, Madhya Pradesh, Rajasthan, Uttar Pradesh ($N = 1{,}164$); Wealthy = Gujarat, Karnataka, Maharashtra, Tamil Nadu ($N = 1{,}119$). MSE-optimal bandwidth selected independently for each column (including covariates changes the optimal bandwidth). Significance from the robust bias-corrected p-value (Section 6.3.1). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table D.2: Heterogeneity by State Development Level: State-Election Vote Share

	(1)	(2)	(3)	(4)
RD_Estimate	-0.834	-0.546	-1.374	-1.110
	(1.913)	(1.869)	(2.129)	(1.868)
Group	BIMARU	BIMARU	WEALTHY	WEALTHY
Controls	N	Y	N	Y
Bandwidth (%)	15.50	10.90	14.10	13.13
Obs Left	371	289	316	297
Obs Right	372	289	315	296
Bias-Corrected Est.	-0.488	-0.270	-1.483	-1.141
Robust CI Lower	-4.873	-4.604	-6.540	-5.553
Robust CI Upper	3.898	4.063	3.574	3.271
Robust SE	2.238	2.211	2.580	2.251
Robust p-value	0.828	0.903	0.565	0.612

Notes: Same sample and specification as Table D.1. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table D.3: Heterogeneity by Electoral Structure: State Assembly Seat Share

	(1)	(2)	(3)	(4)
RD_Estimate	-0.0690*	-0.0727**	-0.113***	-0.110**
	(0.0314)	(0.0330)	(0.0451)	(0.0448)
Group	MULTIPARTY	MULTIPARTY	TWOBLOC	TWOBLOC
Controls	N	Y	N	Y
Bandwidth (%)	14.32	12.24	7.982	7.885
Obs Left	560	493	256	252
Obs Right	555	488	255	251
Bias-Corrected Est.	-0.0698	-0.0774	-0.132	-0.129
Robust CI Lower	-0.143	-0.154	-0.231	-0.228
Robust CI Upper	0.00300	-0.00109	-0.0330	-0.0308
Robust SE	0.0371	0.0389	0.0505	0.0502
Robust p-value	0.0602	0.0468	0.00893	0.0101

Notes: Multiparty = Assam, Bihar, J&K, Jharkhand, Maharashtra, Manipur, Odisha, Uttar Pradesh ($N = 1{,}750$). Two-bloc = Andhra Pradesh, Goa, Himachal Pradesh, Kerala, Punjab, Tamil Nadu, Uttarakhand, West Bengal ($N = 1{,}285$). MSE-optimal bandwidth selected independently for each column. Significance from the robust bias-corrected p-value (Section 6.3.1). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table D.4: Heterogeneity by Electoral Structure: State-Election Vote Share

	(1)	(2)	(3)	(4)
RD_Estimate	0.208	0.961	-1.867	-1.818
	(1.573)	(1.616)	(2.060)	(1.957)
Group	MULTIPARTY	MULTIPARTY	TWOBLOC	TWOBLOC
Controls	N	Y	N	Y
Bandwidth (%)	11.39	9.300	10.30	9.618
Obs Left	469	402	313	299
Obs Right	464	398	313	298
Bias-Corrected Est.	0.587	1.438	-2.565	-2.475
Robust CI Lower	-3.036	-2.237	-7.171	-6.866
Robust CI Upper	4.211	5.113	2.041	1.917
Robust SE	1.849	1.875	2.350	2.241
Robust p-value	0.751	0.443	0.275	0.269

Notes: Same sample and specification as Table D.3. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

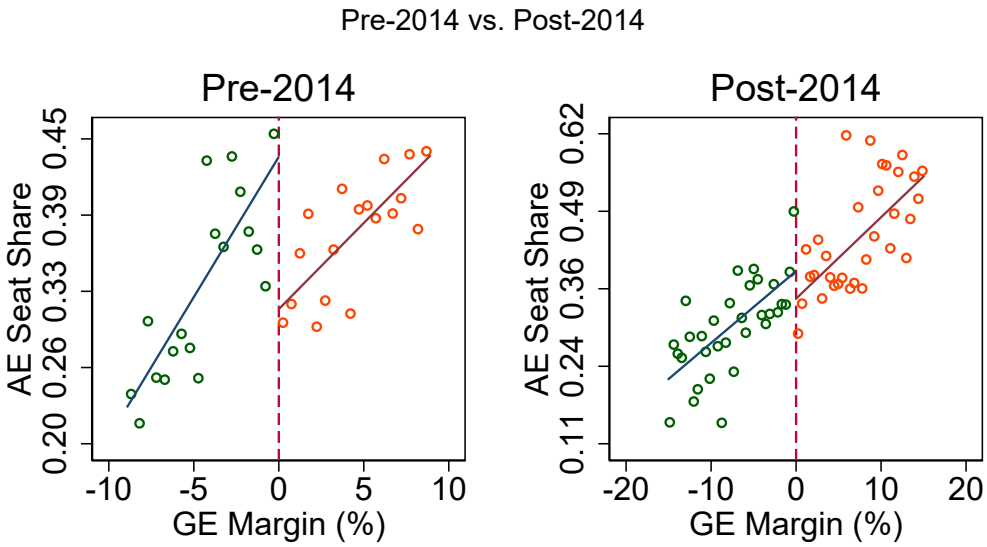


Figure D.1: Heterogeneity by Electoral Era: Pre-2014 vs. Post-2014

Notes: MSE-optimal bandwidth, full controls. Pre-2014 = 2004 and 2009; Post-2014 = 2014 and 2019.

Table D.5: Heterogeneity by Electoral Era: RD Estimates

	Pre-2014 (2004+2009)				Post-2014 (2014+2019)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	-0.91	-0.89	-0.12***	-0.12***	0.041	0.087	-0.060*	-0.056*
	(1.50)	(1.38)	(0.034)	(0.033)	(1.48)	(1.35)	(0.036)	(0.035)
Bandwidth (%)	11.4	11.4	8.96	8.96	15.5	15.5	15.1	15.1
Obs Left	620	620	535	535	528	528	522	522
Obs Right	619	619	534	534	528	528	522	522
Bias-Corrected Est.	-0.94	-0.92	-0.13	-0.13	-0.18	-0.17	-0.074	-0.070
Robust CI Lower	-4.42	-4.11	-0.21	-0.20	-3.58	-3.27	-0.15	-0.15
Robust CI Upper	2.54	2.28	-0.058	-0.057	3.22	2.93	0.0045	0.0078
Robust SE	1.78	1.63	0.038	0.038	1.73	1.58	0.040	0.039
Robust p-value	0.60	0.57	0.00054	0.00054	0.92	0.91	0.065	0.078
Period	Pre-2014	Pre-2014	Pre-2014	Pre-2014	Post-2014	Post-2014	Post-2014	Post-2014
Controls	N	Y	N	Y	N	Y	N	Y

Notes: Pre-2014 = 2004+2009; Post-2014 = 2014+2019. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

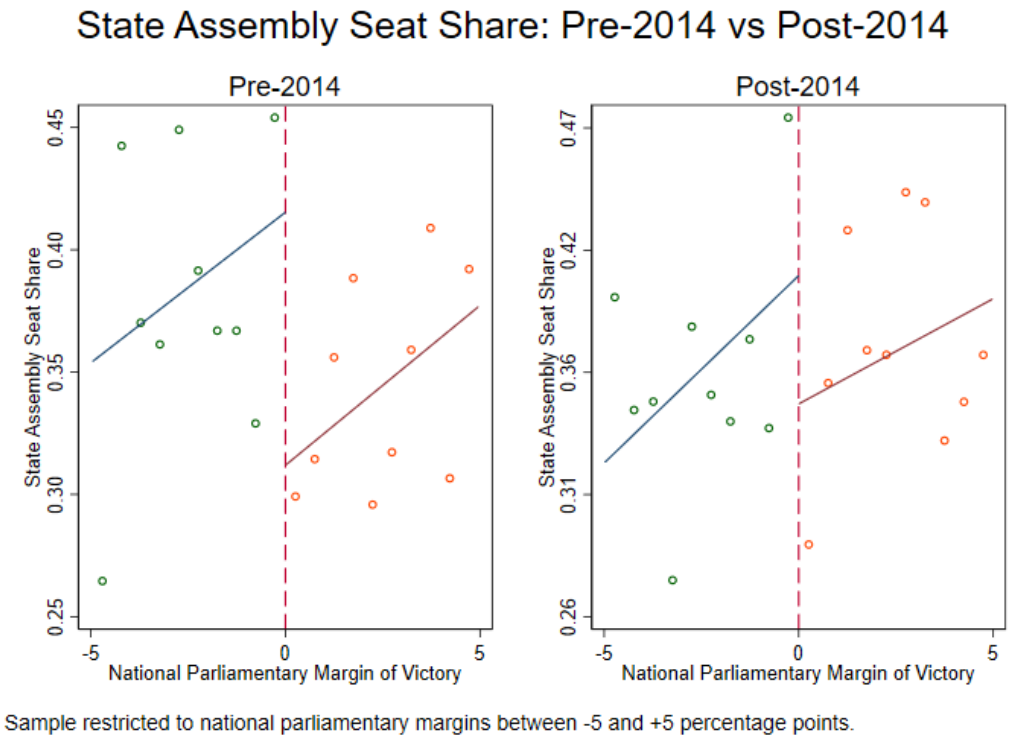


Figure D.2: Fixed-Bandwidth RD Estimates by Electoral Era

Notes: Sample restricted to ±5pp. Left: 2004+2009. Right: 2014+2019.

Table D.6: Fixed-Bandwidth Robustness by Electoral Era: State Assembly Seat Share

	Pre-2014		Post-2014	
	(1)	(2)	(3)	(4)
RD_Estimate	-0.101*	-0.0947*	-0.113**	-0.105**
	(0.0442)	(0.0434)	(0.0559)	(0.0550)
Bandwidth (%)	5	5	5	5
Obs Left	329	329	173	173
Obs Right	326	326	171	171
Bias-Corrected Est.	-0.519	-0.566	-0.383	-0.390
Robust CI Lower	-1.108	-1.150	-0.731	-0.727
Robust CI Upper	0.0703	0.0174	-0.0356	-0.0533
Robust SE	0.301	0.298	0.177	0.172
Robust p-value	0.0843	0.0572	0.0307	0.0232
Period	Pre-2014	Pre-2014	Post-2014	Post-2014
Controls	N	Y	N	Y

Notes: Fixed ±5pp bandwidth. Cols 1–2: Pre-2014; cols 3–4: Post-2014. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table D.7: Regional Heterogeneity: RD Estimates

	North		South		East		West	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	-0.10**	-0.11***	-0.065	-0.068	-0.14**	-0.12**	-0.13**	-0.11**
	(0.040)	(0.038)	(0.047)	(0.044)	(0.065)	(0.062)	(0.053)	(0.046)
Bandwidth (%)	12.7	12.7	10.00	10.00	9.21	9.21	12.8	12.8
Obs Left	419	419	245	245	137	137	153	153
Obs Right	417	417	244	244	136	136	153	153
Bias-Corrected Est.	-0.11	-0.12	-0.078	-0.083	-0.17	-0.14	-0.14	-0.12
Robust CI Lower	-0.20	-0.21	-0.18	-0.18	-0.31	-0.28	-0.26	-0.23
Robust CI Upper	-0.016	-0.034	0.027	0.017	-0.030	-0.012	-0.021	-0.015
Robust SE	0.048	0.045	0.054	0.051	0.071	0.068	0.062	0.054
Robust p-value	0.021	0.0068	0.14	0.10	0.017	0.032	0.022	0.026
Region	NORTH	NORTH	SOUTH	SOUTH	EAST	EAST	WEST	WEST
Controls	N	Y	N	Y	N	Y	N	Y

Notes: Dependent variable: state assembly seat share. Odd columns: no controls; even: full controls. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

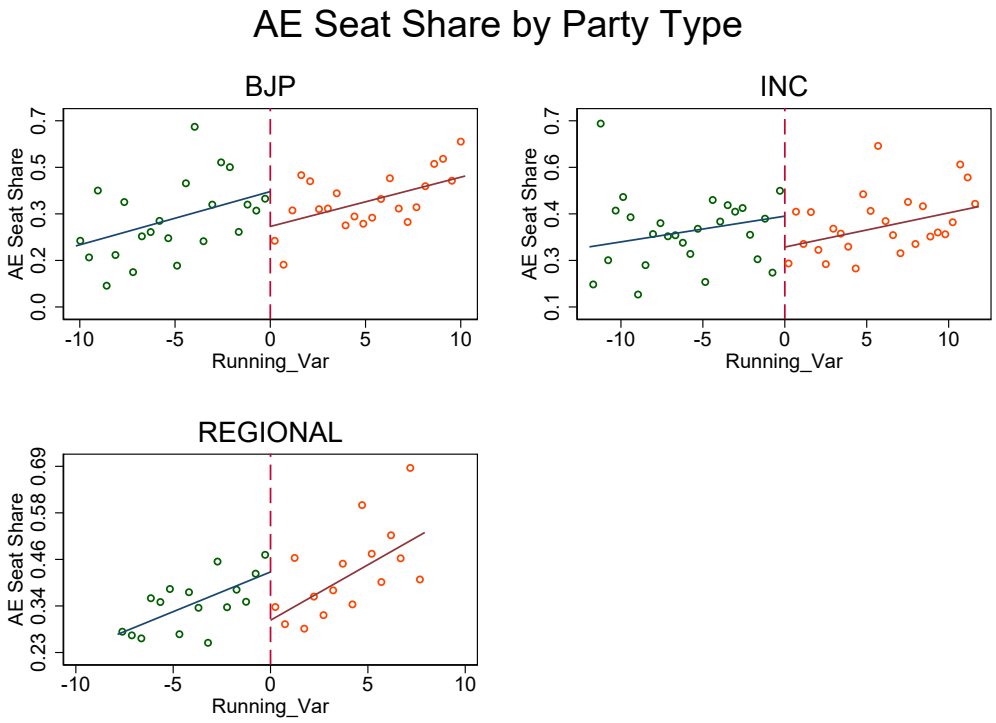


Figure D.3: Heterogeneity by Party: State Assembly Seat Share

Notes: Local linear RD plots of state assembly seat share by party group. Estimates are reported in Table D.8.

Table D.8: Heterogeneity by Party: RD Estimates

	BJP		INC		Left		Regional	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	-0.11**	-0.11**	-0.095*	-0.099**	0.12	0.14**	-0.12***	-0.11**
	(0.054)	(0.049)	(0.044)	(0.043)	(0.088)	(0.068)	(0.046)	(0.045)
Bandwidth (%)	10.3	10.3	12.0	12.0	9.68	9.68	7.93	7.93
Obs Left	188	188	293	293	58	58	338	338
Obs Right	272	272	272	272	40	40	280	280
Bias-Corrected Est.	-0.13	-0.12	-0.098	-0.10	0.14	0.16	-0.14	-0.13
Robust CI Lower	-0.25	-0.24	-0.20	-0.20	-0.068	0.0056	-0.24	-0.23
Robust CI Upper	-0.0055	-0.012	0.0032	-0.0049	0.34	0.31	-0.035	-0.028
Robust SE	0.063	0.057	0.052	0.051	0.10	0.077	0.052	0.051
Robust p-value	0.041	0.031	0.058	0.040	0.19	0.042	0.0085	0.012
Party Group	BJP	BJP	INC	INC	LEFT_P	LEFT_P	REGIONAL	REGIONAL
Controls	N	Y	N	Y	N	Y	N	Y

Notes: Dependent variable: state assembly seat share. Left subsample small (≈58/40 obs). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

E Additional Mechanism Results

This appendix reports the mechanism tables discussed in Section 10 that are not shown in the main text: segment-level vote share and win probability by alternative strength classifications, the strongest opponent's vote share, the dispersion of the party's vote share, and the candidate-experience checks.

Table E.1: Mechanism Test: AC-Level Vote Share by GE-Strength Tercile

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	2.16	2.03	0.061	-0.14	0.57	0.34
	(2.24)	(2.05)	(2.29)	(2.06)	(2.00)	(1.91)
AC Tercile	Weak	Weak	Mid	Mid	Strong	Strong
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	646	646	838	838	949	949
Obs Right	713	713	890	890	969	969
Bias-Corrected Est.	2.17	2.04	0.095	-0.12	0.61	0.37
Robust CI Lower	-2.36	-2.05	-4.48	-4.23	-3.38	-3.41
Robust CI Upper	6.70	6.13	4.67	3.99	4.61	4.16
Robust SE	2.31	2.08	2.34	2.10	2.04	1.93
Robust p-value	0.35	0.33	0.97	0.95	0.76	0.85

Notes: Sequential-elections sample (see Section 8.3). Outcome is the focal party's AE vote share within the specific assembly segment. Fixed ±5pp bandwidth, cluster(PC × GE year). Weak/Mid/Strong = within-constituency tercile of the party's own segment-level GE vote share. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.2: Mechanism Test: AC-Level Win Probability by GE-Strength Tercile

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.0023	-0.000034	-0.078	-0.078	-0.032	-0.027
	(0.051)	(0.051)	(0.062)	(0.063)	(0.055)	(0.054)
AC Tercile	Weak	Weak	Mid	Mid	Strong	Strong
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	867	867	1026	1026	1121	1121
Obs Right	871	871	1028	1028	1120	1120
Bias-Corrected Est.	-0.0027	-0.00047	-0.079	-0.079	-0.032	-0.027
Robust CI Lower	-0.10	-0.10	-0.20	-0.20	-0.14	-0.13
Robust CI Upper	0.099	0.10	0.044	0.045	0.077	0.079
Robust SE	0.052	0.052	0.062	0.063	0.056	0.055
Robust p-value	0.96	0.99	0.21	0.21	0.56	0.61

Notes: Sequential-elections sample (see Section 8.3). Outcome is a binary indicator for winning the specific assembly segment. Fixed ±5pp bandwidth, cluster(PC × GE year). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.3: Mechanism Test: AC-Level Win Probability by Pre-Determined (Prior-AE) Strength Tercile

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.12*	-0.13*	-0.0091	-0.029	-0.053	-0.067
	(0.070)	(0.069)	(0.067)	(0.065)	(0.071)	(0.069)
AC Tercile (Prior-AE)	Weak	Weak	Mid	Mid	Strong	Strong
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	457	457	604	604	662	662
Obs Right	461	461	598	598	667	667
Bias-Corrected Est.	-0.13	-0.13	-0.0080	-0.029	-0.053	-0.067
Robust CI Lower	-0.26	-0.27	-0.14	-0.16	-0.19	-0.20
Robust CI Upper	0.013	0.0078	0.12	0.099	0.088	0.071
Robust SE	0.071	0.070	0.067	0.065	0.072	0.070
Robust p-value	0.075	0.065	0.91	0.66	0.46	0.34

Notes: Sequential-elections sample (see Section 8.3). Outcome is a binary indicator for winning the specific assembly segment. Tercile is based on the party's own vote share in that segment in the assembly election BEFORE the GE, not the contemporaneous GE-time vote share used in Table E.2. Fixed ±5pp bandwidth, cluster(PC × GE year). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.4: Mechanism Test: AC-Level Vote Share by Margin Against Strongest Opponent

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	2.14	1.02	-2.30	-2.60	-3.39	-3.46
	(3.21)	(2.88)	(2.08)	(1.96)	(2.43)	(2.31)
Competitiveness	Clearly_Losing	Clearly_Losing	Previously_Close	Previously_Close	Safely_Held	Safely_Held
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	507	507	798	798	282	282
Obs Right	460	460	822	822	364	364
Bias-Corrected Est.	2.18	1.05	-2.35	-2.68	-3.31	-3.41
Robust CI Lower	-4.28	-4.68	-6.52	-6.61	-8.28	-8.08
Robust CI Upper	8.64	6.78	1.83	1.25	1.66	1.27
Robust SE	3.29	2.92	2.13	2.01	2.54	2.38
Robust p-value	0.51	0.72	0.27	0.18	0.19	0.15

Notes: Sequential-elections sample (see Section 8.3). Outcome is the focal party's AE vote share within the specific assembly segment. Same competitiveness classification as Table 12. Fixed ±5pp bandwidth, cluster(PC × GE year). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.5: Mechanism Test: Strongest Opponent's Own AE Vote Share

	(1) Fixed 5pp	(2) Fixed 5pp	(3) Own MSE
RD_Estimate	0.73	0.58	1.28
	(1.01)	(0.91)	(0.70)
Controls	N	Y	N (MSE bw)
Bandwidth (%)	5	5	12.1
Obs Left	2433	2433	5139
Obs Right	2572	2572	5481
Bias-Corrected Est.	0.71	0.55	1.16
Robust CI Lower	-1.34	-1.27	-0.46
Robust CI Upper	2.75	2.38	2.79
Robust SE	1.04	0.93	0.83
Robust p-value	0.50	0.55	0.16

Notes: Sequential-elections sample (see Section 8.3). Outcome is the own AE vote share, within the specific assembly segment, of the focal party's strongest opponent in that segment (the segment's actual runner-up where the focal party won, or actual winner where it lost). Cols 1–2: fixed ±5pp bandwidth; col 3: own-sample MSE-optimal bandwidth. cluster(PC × GE year). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.6: Mechanism Test: Strongest Opponent's Own AE Vote Share by Pre-Determined Competitiveness

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.88	1.54	2.43*	2.18*	1.33	1.24
	(1.80)	(1.61)	(1.36)	(1.23)	(1.96)	(1.89)
Competitiveness	Clearly_Losing	Clearly_Losing	Previously_Close	Previously_Close	Safely_Held	Safely_Held
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	507	507	798	798	282	282
Obs Right	460	460	822	822	364	364
Bias-Corrected Est.	0.85	1.52	2.47	2.20	1.32	1.23
Robust CI Lower	-2.77	-1.73	-0.26	-0.26	-2.65	-2.54
Robust CI Upper	4.48	4.77	5.19	4.66	5.28	4.99
Robust SE	1.85	1.66	1.39	1.26	2.02	1.92
Robust p-value	0.64	0.36	0.076	0.080	0.52	0.52

Notes: Sequential-elections sample (see Section 8.3). Outcome as in Table E.5. Competitiveness classification is the same predetermined (prior-AE margin against the segment's own strongest opponent) measure used in Table 12, not the subsequent-AE outcome. Fixed ±5pp bandwidth, cluster(PC × GE year). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.7: Mechanism Test: Within-Constituency Dispersion of Assembly Vote Share

	(1) SD	(2) SD	(3) CV	(4) CV
RD_Estimate	-0.981**	-1.032**	-0.0307	-0.0305
	(0.506)	(0.497)	(0.0243)	(0.0231)
Controls	N	Y	N	Y
Bandwidth (%)	14.31	14.31	13.03	13.03
Obs Left	910	910	850	850
Obs Right	940	940	876	876
Bias-Corrected Est.	-1.144	-1.205	-0.0389	-0.0386
Robust CI Lower	-2.279	-2.318	-0.0937	-0.0908
Robust CI Upper	-0.00943	-0.0925	0.0158	0.0136
Robust SE	0.579	0.568	0.0279	0.0266
Robust p-value	0.0481	0.0338	0.163	0.147

Notes: Sequential-elections sample (see Section 8.3). Outcome is the within-parliamentary-constituency dispersion of the party's own subsequent AE vote share across its nested assembly segments: columns (1)–(2) use the standard deviation, columns (3)–(4) the coefficient of variation. Sample restricted to parliamentary-constituency races with at least three assembly segments with a non-missing AE vote share. MSE-optimal bandwidth, cluster(PC × GE year). $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.8: Candidate-Quality Check: AC Candidate Incumbency by Tercile

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.057	0.049	0.075	0.068	0.089	0.074
	(0.049)	(0.048)	(0.055)	(0.053)	(0.056)	(0.054)
AC Tercile	Weak	Weak	Mid	Mid	Strong	Strong
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	646	646	838	838	949	949
Obs Right	713	713	890	890	972	972
Bias-Corrected Est.	0.058	0.049	0.076	0.069	0.091	0.076
Robust CI Lower	-0.040	-0.046	-0.033	-0.036	-0.021	-0.033
Robust CI Upper	0.16	0.14	0.18	0.17	0.20	0.18
Robust SE	0.050	0.049	0.056	0.053	0.057	0.055
Robust p-value	0.24	0.31	0.17	0.20	0.11	0.17

Notes: Sequential-elections sample (see Section 8.3). Outcome is a binary indicator for whether the AE candidate fielded by the focal party in that segment was a sitting incumbent MLA. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.9: Candidate-Quality Check: AC Candidate Prior Terms Won by Tercile

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.14	0.10	0.082	0.055	-0.0062	-0.049
	(0.16)	(0.15)	(0.17)	(0.16)	(0.19)	(0.18)
AC Tercile	Weak	Weak	Mid	Mid	Strong	Strong
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	646	646	838	838	949	949
Obs Right	713	713	890	890	972	972
Bias-Corrected Est.	0.14	0.10	0.088	0.060	0.0035	-0.041
Robust CI Lower	-0.18	-0.20	-0.26	-0.26	-0.38	-0.40
Robust CI Upper	0.47	0.41	0.43	0.38	0.38	0.32
Robust SE	0.17	0.16	0.18	0.16	0.19	0.18
Robust p-value	0.39	0.51	0.62	0.71	0.99	0.82

Notes: Sequential-elections sample (see Section 8.3). Outcome is the number of prior terms won by the AE candidate fielded by the focal party in that segment. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table E.10: Candidate-Quality Check: AC Candidate Recontest by Tercile

	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.084	0.060	0.083	0.072	0.10*	0.087*
	(0.067)	(0.063)	(0.054)	(0.052)	(0.057)	(0.055)
AC Tercile	Weak	Weak	Mid	Mid	Strong	Strong
Controls	N	Y	N	Y	N	Y
Bandwidth (%)	5	5	5	5	5	5
Obs Left	646	646	838	838	949	949
Obs Right	713	713	890	890	972	972
Bias-Corrected Est.	0.086	0.061	0.089	0.077	0.11	0.094
Robust CI Lower	-0.050	-0.065	-0.021	-0.030	-0.0059	-0.018
Robust CI Upper	0.22	0.19	0.20	0.18	0.23	0.21
Robust SE	0.069	0.064	0.056	0.054	0.059	0.057
Robust p-value	0.22	0.35	0.11	0.16	0.063	0.098

Notes: Sequential-elections sample (see Section 8.3). Outcome is a binary indicator for whether the AE candidate fielded by the focal party in that segment is recontesting the same seat. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.