The Downside of Winning

Parliamentary Victories and Subsequent State Elections in India

Sujoy Upadhyay
Department of Economics, University of Illinois Chicago

October 16, 2026

Abstract

This paper examines whether winning a seat in India's national parliament helps a party in the next state assembly election. India's nested electoral geography makes this question tractable: each parliamentary constituency comprises several assembly constituencies, which allows a party's parliamentary result to be linked to its subsequent assembly performance within the same territory. Using a regression discontinuity design that compares parties narrowly winning and narrowly losing parliamentary contests between 2004 and 2019, I find that a narrow victory reduces the party's share of assembly seats in that constituency by 8 to 9 percentage points, or roughly 0.6 of the seven assembly seats in a typical constituency. The estimate is robust to alternative bandwidths, control sets, and adjustment for the party's prior assembly performance, and in the full pooled sample to a local quadratic specification and state and year fixed effects. The effect on aggregate vote share is statistically indistinguishable from zero, but the confidence interval admits a decline of nearly three percentage points, and simulations based on observed segment margins show that a decline of this magnitude can account for most of the seat loss under first-past-the-post rules. Winning parties do not detectably reduce the number of assembly seats they contest or field less experienced candidates; rather, their win rate in contested seats declines. Taken together, the findings document an anti-coattail effect in Indian federal politics: a narrow parliamentary victory weakens the party's subsequent position in state elections within the same constituency.

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

JEL Codes: D72, H77, P16

1 Introduction

Parties in federal democracies compete for power at more than one level of government. In India, the same party organization, the same local workers, and very often the same voters take part in national parliamentary elections and in state assembly elections held a few months or a few years later. This raises the question of whether electoral success at one level of government carries over to the other. Classic coattail theories say that it should: a national victory mobilizes the party's supporters, attracts campaign resources, and lends credibility to the party's candidates further down the ballot (Campbell et al., 1960; Miller, 1955; Hogan, 2005).

Indian parties frequently campaign on this premise. The "double engine" appeal, which urges voters to elect the same party to the national and state governments, rests on the assumption that success at the two levels is mutually reinforcing. India is, however, also characterized by a pronounced anti-incumbency tendency. Candidates who narrowly win national or state elections are less likely than those who narrowly lose to win the subsequent election (Linden, 2004; Uppal, 2009). A party that has recently won a parliamentary seat may face similar scrutiny when the same electorate votes in the subsequent assembly election.

Theoretically, a parliamentary win can cut both ways for the party's next state campaign. On the positive side, the winning party gains visibility, a sitting MP who can campaign for its assembly candidates, and improved access to central government resources. On the negative side, voters who now hold the party responsible for the constituency may support a rival in the assembly election, and the party's attention and personnel may be redirected toward national politics. The net effect is therefore ambiguous and warrants empirical investigation.

In this paper, I ask whether narrowly winning a parliamentary constituency improves or weakens the same party's performance in the next state assembly election. India's nested electoral geography makes this question tractable. Each parliamentary constituency comprises several assembly constituencies, which allows me to observe the subsequent assembly performance of the parties that won and lost the parliamentary seat within the same territory.

A simple comparison of winners and losers would not identify this effect, because winning parties are typically stronger to begin with. To separate the effect of winning from the underlying strength that produced the victory, I employ a regression discontinuity (RD) design. The design compares parties that won their parliamentary race by a very small margin with parties that lost by a very small margin. In such close races, the outcome is determined largely by chance, so the two groups should be comparable in all respects except the result. I apply this design to the 2004, 2009, 2014, and 2019 general elections, link each contest to the next assembly election in the state, and measure two outcomes within the parliamentary constituency: the party's share of the assembly vote and its share of the assembly seats. The main sample retains only assembly elections held after the parliamentary result was known.

The findings indicate that a parliamentary win carries an electoral cost at the state level. A narrow victory reduces the party's assembly seat share by 8 to 9 percentage points. In a constituency with seven assembly seats, this corresponds to approximately 0.6 fewer seats for the party that won the parliamentary seat. The estimate is significant at the 1 percent level in every specification of the main table, and it changes little across bandwidths, control sets, and adjustment for the party's prior assembly performance; in the full pooled sample, it is also robust to a local quadratic specification and to state and year fixed effects. Adding

assembly elections held on the same day as the parliamentary vote raises the estimate to about 10 points, but I exclude those contests from the main sample, since voters in those contests could not have responded to a result that was not yet known.

The results for vote share differ markedly. The estimated effect on the party's assembly vote share lies between -0.10 and -0.53 percentage points and is never close to significant. This divergence between votes and seats is the central pattern of the paper, and India's first-past-the-post (FPTP) electoral rules account for much of it. Under FPTP, the candidate with a plurality wins the seat regardless of the margin, so a small loss of votes in close contests can translate into a substantial loss of seats. The vote-share confidence interval admits a decline of up to approximately 2.9 points. Using observed margins in each assembly segment, I show that a uniform decline of this size would reduce the party's seat share by roughly 7 points if the lost votes are dispersed among other parties, and considerably more if they go to its strongest rival. A small decline in support spread across many segments, too small to be detected in aggregate vote share, is therefore sufficient to account for most of the seat loss.

I also ask what parties do differently after a win. On the dimensions I can observe, they change little. Winners do not contest detectably fewer assembly seats, and the estimates rule out a decline of more than about 4 percentage points. Nor do they field less experienced candidates. The difference lies in their success rate: in the seats a winning party contests, its win rate is approximately 11 percentage points lower than that of a party that narrowly lost, even though winners tend to contest somewhat more favorable seats. The loss therefore arises at the polls rather than through withdrawal. I also examine where within the constituency the losses occur, how the party's strongest rival performs, and whether the effect varies with national or state government status or with the interval between the two elections. These tests are too imprecise to identify a single mechanism, and I report them as such.

The principal threat to identification is that narrow winners may have been stronger in the constituency to begin with. Several checks indicate that this does not drive the result. There is no evidence that parties can manipulate the outcome of close races, narrow winners and losers field similar candidates, and placebo thresholds away from zero show no effect. Narrow winners held a somewhat larger share of assembly seats before the general election, but this difference is significant only at the 10 percent level and only at the narrowest bandwidth, it reverses sign in the preceding assembly election, and controlling for it leaves the estimate unchanged. Local randomization inference, which treats contests within two percentage points as randomly assigned, yields a smaller and less precise difference of about 4 points, which I discuss in Section 6.3.

This paper makes three key contributions. First, it provides causal evidence on electoral spillovers from national to state elections in India. Earlier work documents an incumbency disadvantage within each level (Linden, 2004; Uppal, 2009) and short-lived honeymoon effects associated with ruling-party status (Ravishankar, 2009). The treatment here is narrower: a party's victory in a single parliamentary constituency, measured against its performance in the assembly seats nested within it. Second, it adds to the evidence on anti-coattail effects. Folke and Snyder (2012) find that winning a U.S. governorship costs the party about 3.5 points of state legislative seat share. The Indian estimate is more than twice as large, in a multiparty setting in which small changes in votes produce large changes in seats, although the institutions and the treatment differ. Third, by studying votes and seats separately, it shows that vote share alone can miss an electoral penalty that is clearly visible in seats, a

point that matters for any study of party performance under first-past-the-post rules.

The remainder of the paper proceeds as follows. Section 2 reviews the related literature, and Section 3 describes the institutional setting. Section 4 introduces the data and Section 5 presents the empirical strategy. Section 6 reports the validity checks, and Sections 7 and 8 present the main results and robustness exercises. Section 9 summarizes the heterogeneity analysis, Section 10 examines mechanisms, and Section 11 concludes. The appendices report additional validity checks (Appendix A), additional robustness results (Appendix B), robustness to the 2008 delimitation (Appendix C), the full heterogeneity results (Appendix D), and additional mechanism results (Appendix E).

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 differs from the U.S. settings in this literature in three ways that matter here. Competition is multiparty, so small changes in votes can produce large changes in seats. Incumbents face a documented disadvantage at both the national and the state level, which makes a negative spillover plausible. And because constituencies are nested inside one another, I can compare a party's performance across the two levels within the same territory.

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.

My paper differs from Ravishankar (2009) in its treatment, design, and geographic unit. The treatment is a party's victory in one parliamentary constituency, which need not make the party part of the national government. The comparison is between narrow winners and narrow losers. And nesting lets me measure assembly performance within the same territory. The four general elections from 2004 to 2019 also extend the evidence into a period of stronger national-party dominance.

3 Institutional Context

3.1 India's Two-Tier Electoral Architecture

India is a federal parliamentary republic in which state institutions broadly mirror the national architecture. Nationally, voters elect Members of Parliament (MPs) to the Lok Sabha, the lower house of Parliament, from 543 single-member constituencies. The upper house, the Rajya Sabha, is chosen indirectly by state legislatures, does not represent territorial constituencies, and plays no role in this analysis.

In each state, voters elect Members of the Legislative Assembly (MLAs) to the Vidhan Sabha, again from single-member constituencies. Some states also have an indirectly elected legislative council, but the directly elected assembly is the main state legislature and the one I study. There are roughly 4,100 assembly constituencies across India's 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 illustrates this structure for one parliamentary constituency. This nested structure is central to the analysis. It allows me to aggregate assembly outcomes to the parliamentary constituency and compare a party's performance across the two levels within the same territory. A party may contest all, some, or none of the segments, and I keep track of this when measuring its vote share, seat share, and the share of segments it contests.

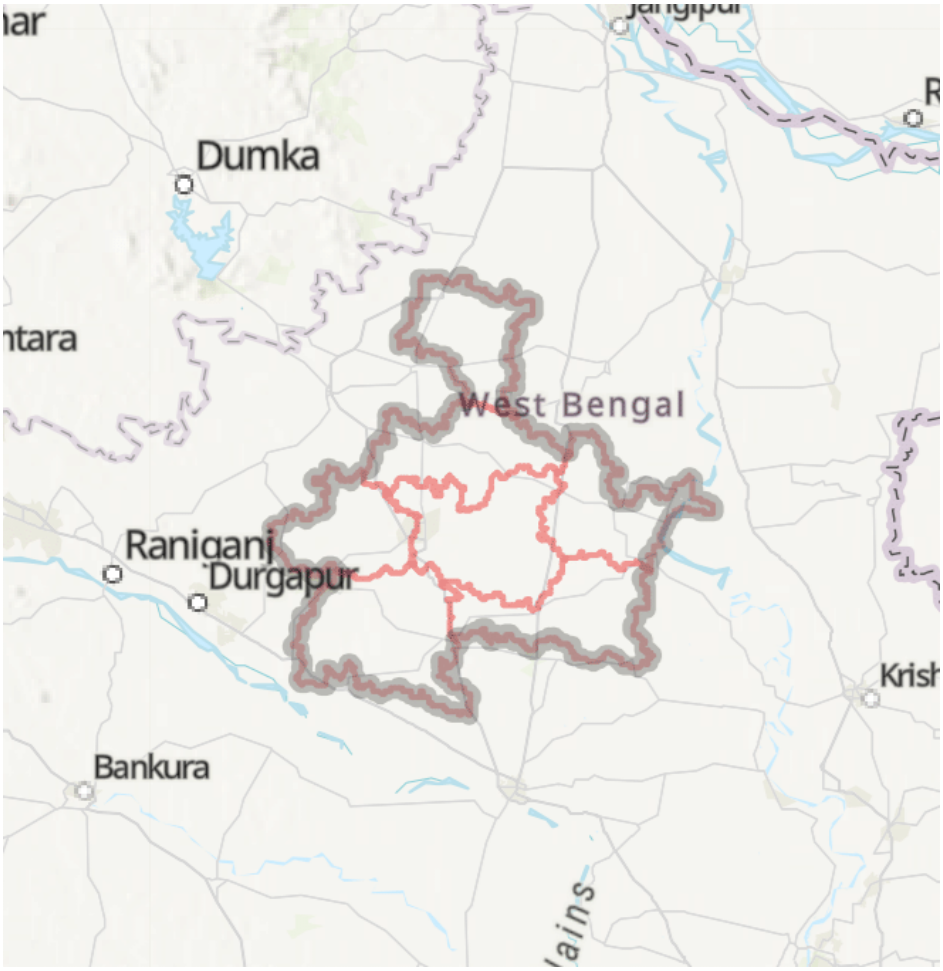


Figure 1: Nested Electoral Geography: An Example

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

National and state elections in India are not synchronized. The Lok Sabha serves up to five years unless dissolved early, and each state assembly runs on its own five-year clock, which can also end early if the state government loses a confidence vote. Most state elections are therefore held at a different time from the national election. Even when the two are held together, they are separate contests with separate ballots for the MP and the MLA.

I match each of the 2004, 2009, 2014, and 2019 general elections to the next assembly election in each state. The 2004 general election, for example, is matched to Maharashtra's October 2004 election, Bihar's 2005 election, and West Bengal's 2006 election. The main analysis keeps assembly elections held after the parliamentary result, so that the outcome could respond to it. Elections held on the same day are analyzed separately.

The gap between the two elections ranges from a few months to several years. Section 6.4

separates assembly elections held in a later calendar year, those held later in the same year, and those held at the same time, since sharing a calendar year does not by itself establish which election came first.

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

Both national and state elections use first-past-the-post (FPTP) rules: the candidate with the most votes wins the seat, whether or not that candidate has a majority. With three to six effective parties in a typical Indian constituency, winning margins are often small and the relationship between votes and seats is highly non-linear. A small shift in votes in a few close contests can change many seats without moving a party's overall vote share very much.

This distinction is important for interpreting the results. A narrow parliamentary victory reduces the party's subsequent assembly seat share by 8 to 9 percentage points without a detectable change in its overall vote share. Under FPTP, this pattern may reflect a change in the distribution of the party's support relative to its rivals, or a modest decline in support that the vote-share estimates lack the precision to detect. Section 8.4 uses observed segment-level margins to examine the second possibility.

3.4 Party Organization and the Two-Level Contest

Indian parties contest both national and state elections, but the two campaigns are organized differently. Parliamentary campaigns are usually run by the party's national or state leadership, with resources allocated centrally. Assembly campaigns are more locally organized and candidate-driven. A party that wins a parliamentary seat gains an MP with national duties, committee work, and access to the central government. These responsibilities may divert attention, personnel, and financial resources from the subsequent state campaign. Section 10 examines whether the data are consistent with such diversion.

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 state and each general election, I use the *very next* assembly election as the outcome. This rule has three advantages. It ensures that the parliamentary result precedes the assembly

outcome. It minimizes the window in which confounding state-level events can occur. And it yields a one-to-one match between each general election and each state's assembly election.

The main sample includes assembly elections held in a later calendar year and those held later in the same year, and excludes elections held at the same time as the general election (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.

I measure outcomes for the party rather than the alliance, for three reasons. Alliance composition changes substantially across election cycles, so no consistent alliance unit exists over the sample period. The question itself is whether a party's own win affects its own later performance. And alliance-level coding would require resolving numerous ambiguous cases in which the parliamentary winner and the assembly candidates belong to different partners.

The estimated effect therefore concerns the party, not its alliance. Part of it could reflect changes in seat-sharing arrangements with alliance partners. Without consistent alliance data, I cannot distinguish seats lost to an ally from seats lost to an opponent.

5 Empirical Strategy

The central empirical challenge is selection. Parties that win parliamentary seats tend to be stronger, better organized, and better funded in their constituencies than parties that lose. A simple comparison of winners and losers in the next assembly election would conflate the effect of winning with these differences in underlying strength. A party that wins its parliamentary seat comfortably and subsequently wins most of the assembly seats provides little evidence on whether the first outcome caused the second.

5.1 The Regression Discontinuity Approach

I therefore compare parties that narrowly won their parliamentary contest with parties that narrowly lost. When a contest is decided by a very small margin, the identity of the winner is determined largely by chance. If a party's potential assembly outcomes vary continuously with its parliamentary margin, a discontinuity in those outcomes at a margin of zero can be attributed to the effect of winning.

Following Lee (2008), this approach requires that parties cannot precisely control whether they finish immediately above or below the winning threshold. Close elections make this assumption plausible, but it requires empirical scrutiny (Eggers et al., 2015), which Section 6 provides.

To fix ideas, consider two parties separated by 0.4 percentage points in a parliamentary race. The winner enters the sample at $+0.4$ and the runner-up at -0.4. The design pools

many such close contests to estimate the discontinuity at zero, while accounting for the fact that each race contributes two linked observations.

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 a bandwidth of zero 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 of interest is τ, the discontinuity at zero. A negative τ indicates that narrowly winning the parliamentary seat reduces the party's subsequent assembly performance, an anti-coattail effect; a positive τ would indicate a conventional coattail effect.

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

A key choice in an RD design is the *bandwidth*, which determines how close a contest must be to enter the estimation. A narrower bandwidth improves comparability between winners and losers at the cost of precision; a wider bandwidth increases precision at the cost of greater potential bias. I choose the bandwidth with the data-driven methods of Calonico et al. (2014). The MSE-optimal bandwidth minimizes the mean squared error of the point estimate. The CER-optimal bandwidth minimizes the coverage error of the confidence interval and is typically narrower. For assembly seat share in the main sample, these are about 15.9 and 10.6 percentage points, corresponding to roughly 1,100 and 785 observations on each side of the threshold. I also report results with fixed bandwidths of 5 and 10 points.

For inference, I use the robust bias-corrected method of Calonico et al. (2014). This method corrects the point estimate for the approximation bias of the local linear fit and accounts for the additional uncertainty introduced by that correction, using a separate pilot bandwidth b. I select b with the `mserd` procedure in every specification, including those where the main bandwidth is fixed by hand. Standard errors are clustered by parliamentary constituency and election cycle, since the winner and runner-up from the same contest are dependent observations. Section 6.3.1 discusses the interpretation of the reported estimates.

5.4 What the Design Identifies

The design identifies a local effect of winning for parties in close contests. It does not directly speak to comfortable victories, nor does it identify the effect of entering the national government. Because each race contributes its winner and its runner-up, the comparison also applies only to the top two finishers; it does not extend to parties finishing third or lower.

6 Validity Checks

The RD design rests on the assumption that parties immediately above and below the winning threshold are comparable in all respects other than the outcome of the contest. This section assesses that assumption through tests of sorting around the threshold, covariate balance, balance on prior assembly outcomes, the timing of the assembly election, 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

A sharper test examines whether narrow winners and losers already differed in the assembly election preceding the general election. If the identifying assumption holds, the party's prior vote share, seat share, and contest rate should be continuous at zero. This test is most consequential for prior seat share, which corresponds to 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 conventional levels, but its magnitude at the narrower bandwidth warrants further examination, which I undertake in four ways.

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

First, I control for it directly. On the subsample with a usable prior election, the estimate changes from -0.098 without the control to -0.103 with it, and both are significant at the

1 percent level (Table 3). If the main effect reflected a pre-existing advantage of narrow winners, adjusting for that advantage should attenuate the estimate. It does not.

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Second, I examine whether the difference is persistent. If narrow winners were systematically stronger, the difference should also appear two assembly elections earlier. It does not. On its own largest available sample, vote share two elections earlier shows no discontinuity ($p = 0.13$ at the MSE bandwidth and $p = 0.50$ at a fixed ±5 points; Table A.3), and the same holds for seat share, reported below together with the first-prior comparison. Table A.4 compares the two elections on an identical sample of 1,668 races. The difference at $t-1$ is 3.6 points at the MSE bandwidth ($p = 0.18$, 95 percent interval $[-0.021, 0.116]$) and 7.4 points at ±5 points ($p = 0.069$, interval $[-0.006, 0.160]$). The difference at $t-2$ is negative in both cases (-2.4 and -3.2 points; $p = 0.29$ and $p = 0.37$). A stacked regression rejects equality of the two at ±5 points ($F(1, 492) = 4.66$, $p = 0.031$) but not at ±10 points ($F(1, 897) = 1.38$, $p = 0.24$). A difference that reverses sign across consecutive elections is more consistent with sampling variation in a single election than with a persistent difference between the two groups.

Third, I use the change in seat share relative to the prior election as the outcome, which asks whether winners experienced a larger decline than losers rather than whether they attained a lower level. Because the conventional RD estimator is linear in the outcome, the estimate for the change equals the estimate for the level minus the estimated prior difference, on the same sample and bandwidth. Subtracting a positive prior difference therefore mechanically enlarges the change estimate. At ±5 points in the pooled sample, the change estimate is -17.7 points, exactly the level estimate on that subsample (-10.5) minus the prior difference (7.2); in the main sequential sample it is -16.2 points (Tables A.5

and A.6). Differencing is valid only if the prior difference would have persisted unchanged in the absence of treatment, an assumption I cannot verify; if the difference partly reflects sampling variation, differencing overstates the effect. I therefore treat it as a sensitivity exercise rather than as a bound on the true effect. Its value lies in showing that the estimate remains negative and significant regardless of how the prior difference is treated: ignored, controlled for, or subtracted.

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. 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

As a final check, I estimate discontinuities at thresholds where no effect should exist: margins of -10, -5, $+5$, and $+10$ points, where the same party wins or loses on both sides. Each test uses races within 2.5 points of the placebo threshold, so that no comparison crosses the true one. None of the eight estimates is significant (Tables A.12 and A.13; Figures A.5 and A.6). For seat share, the robust p-values are 0.36, 0.56, 0.59, and 0.67. Because each test uses relatively few observations, the resulting intervals are wide enough to include an effect as large as the main estimate. They rule out large spurious discontinuities rather than small ones.

7 Main Results

7.1 Effect on Assembly Vote Share

I begin with vote share. Table 4 reports four specifications on the baseline sample, using the two data-driven bandwidths (about 10 and 15 points) with and without the full control set. The estimates range from -0.10 to -0.53 percentage points, and no p-value is below 0.56. Figure B.1 shows no visible discontinuity at zero.

This does not establish that support is unchanged. In the specification with all controls at the MSE bandwidth, the 95 percent confidence interval runs from −2.85 to 1.54 percentage points, so the data are consistent with a decline in vote share of up to approximately 2.9 points. Whether a decline of this magnitude is consequential depends on where the votes are lost, which I examine in Section 8.4.

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

7.2 Effect on Assembly Seat Share

The results for seat share are markedly different. Table 5 and Figure B.2 show that a narrow parliamentary victory reduces the party's subsequent assembly seat share by 8 to 9 percentage points. Across the four specifications, the estimates range from −0.080 to −0.093, the bias-corrected estimates from −0.088 to −0.096, and every p-value is below 0.002. The estimate is stable across control sets and bandwidths.

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.

7.3 The Vote-Seat Divergence

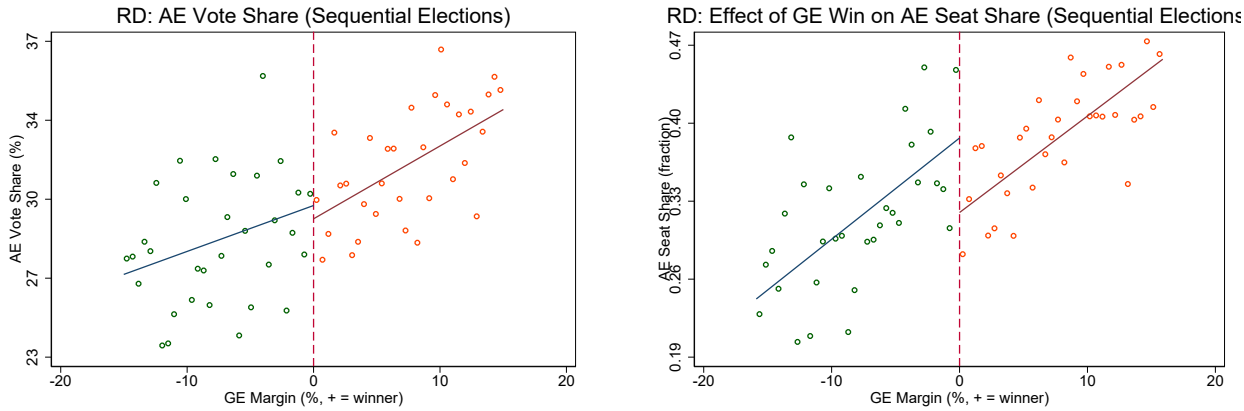
Figure 2 presents the two results together: a clear decline in seats and no detectable decline in votes. This divergence is the central pattern of the paper and is consistent with two classes of explanation. Narrow winners may lose support in a manner that changes the distribution of their votes relative to their rivals, or they may experience a small decline in support spread across many segments, which FPTP rules convert into a substantially larger loss of seats.

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Section 8.4 shows that the second explanation is quantitatively plausible, and Section 10 examines the first.



(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

A party may win fewer seats because it contests fewer of them, because it wins a smaller share of those it contests, or both. A reduction in contesting would suggest organizational

withdrawal or seat-sharing with allies; a lower win rate in contested seats would suggest 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.

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

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 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.2 and B.4) 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.4): 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.2).

8.3 Sequential versus Simultaneous Elections: The Full Pooled Sample

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. For comparison, Table B.1 reports estimates on the full pooled sample, which are close to −0.10 in every specification and significant at the 1 percent level. Splitting the sample by calendar year instead gives −0.12 for assembly elections held in the same year (Table B.3) and −0.096 for those held in a later year (Table A.8). As Section 6.4 shows, the same-year group combines simultaneous and subsequent contests, and the larger same-year estimate is driven by the simultaneous ones.

8.4 How Large a Vote Loss Would Be Needed

The vote-share interval runs from −2.85 to 1.54 points, while the seat-share effect is about −8 points. I examine whether a vote decline small enough to lie within that interval could account for a seat loss of this magnitude, using two approaches.

The first is a descriptive benchmark. Across constituencies, a regression of assembly seat share on vote share with state and year fixed effects yields a slope of approximately 1.75, so that each point of vote share is associated with 1.75 points of seat share. On this basis, a 2.85 point decline in votes would imply a seat-share loss of about 5 points. This is, however, an average relationship. The seat consequences of a vote loss depend on where it occurs: the

same decline costs few seats where the party leads comfortably and many where its lead is 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.

Table 9 reports the results. A decline of 2.85 points, exactly at the lower bound of the vote-share interval, implies a seat-share loss of 7.3 points in the first scenario and 15.4 points in the second. Generating the full 8 point loss requires a swing of 3.08 points in the first scenario, just outside the interval, and 1.54 points in the second, well within it. A decline in support too small to be detected in aggregate vote share is therefore sufficient, under plausible assumptions about the destination of lost votes, to account for most or all of the seat loss. This does not establish that such a decline occurred, but it shows that the vote-share null is not evidence against an explanation based on a modest loss of support.

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.

8.5 Bandwidth Sensitivity

Table B.4 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.

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

Appendix D reports heterogeneity by state development, party system, region, party, and electoral era, using the full pooled sample. The negative seat-share effect is present in nearly every subgroup. It is somewhat larger in the poorer BIMARU states than in wealthier states, and larger in two-bloc than in multiparty states, but neither difference is statistically significant. By region, it is significant in the north, east, and west and weakest in the south. By party, the estimates for the BJP, the Congress, and regional parties are all close to −0.10, so no single party drives the result. The effect is larger in 2004 and 2009 than in 2014 and 2019 at the MSE bandwidth, but similar across the two periods at a fixed ±5 point bandwidth, and a formal test does not reject equality. Vote-share estimates are insignificant in every subgroup. Given the small subsamples, I interpret these results as evidence that the effect is broad-based rather than as tests of any particular mechanism.

10 Mechanisms and Interpretation

The evidence thus far establishes three facts that any explanation must accommodate: the party loses seats without a detectable aggregate loss of votes, it does not contest fewer seats,

and it wins a smaller share of the seats it contests. Several mechanisms could produce this pattern.

The first is voter backlash or balancing. Voters who now hold the party responsible for the constituency through its MP may support a rival in the assembly election, either out of dissatisfaction or to divide control between the two levels of government. Under FPTP, such switching can cost many seats with only a modest change in votes, and Section 8.4 shows that a loss small enough to fit inside the vote-share interval is large enough to explain most of the seat loss. The second is organizational diversion. A newly elected MP and the associated party organization may redirect attention to national politics, leaving the assembly campaign with fewer personnel and resources. The third is a referendum or honeymoon account in the spirit of Ravishankar (2009), in which the effect depends on whether the party holds national or state office and on how long ago the parliamentary election took place.

This section tests the implications of these accounts using assembly-segment outcomes, government status, election timing, and candidate characteristics. Most of these tests divide an already local sample into smaller groups, which limits their precision. I report what they show and what they cannot distinguish.

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 11). 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).

Taken together, the segment-level results suggest that the loss is distributed across the party's segments rather than concentrated in one type, although their precision does not support stronger conclusions.

Table 11: 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

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.

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

A referendum account predicts that the penalty depends on whether the party holds office, and a honeymoon account predicts that it diminishes with the time elapsed since the parliamentary election. I test both using stacked local linear interaction models at ± 5 points, as in Appendix D.5.

Table 12 distinguishes parties by whether they held the prime ministership after the general election: Congress in 2004 and 2009 and the BJP in 2014 and 2019. The estimates are -8.0 and -7.6 points for all other parties and -6.3 and -7.0 points for the prime minister's party. Each subgroup is too small for its own estimate to be precise (p between 0.097 and

0.35), and the interaction is far from significant ($F(1, 416) = 0.24$, $p = 0.62$, 95 percent interval $[-10.0, 16.7]$ points). The "other parties" group, labeled "Other" in the table, also includes national coalition partners, so it is not a pure opposition group. Table 13 instead distinguishes parties by whether they held the chief ministership at the time of the assembly election. The estimates are -8.1 and -7.3 points when they did not and -10.3 and -8.9 points when they did, again individually imprecise (p between 0.099 and 0.16), with an insignificant interaction ($F(1, 416) = 0.24$, $p = 0.62$). The two tests are separate estimates that round to the same values; the groupings overlap only partly, since many parties outside the national government govern their own state. State government status can also change after the general election, so this second split is descriptive. Neither comparison provides evidence that the penalty depends on holding office, although the intervals are too wide to rule out sizable differences.

Table 12: 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 $F(1, 416) = 0.242$, $p = 0.623$. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

For timing, I measure the interval between the general and assembly elections in months, using the first polling date of each. Among assembly elections held in a later year, the interval ranges from about 9 to 67 months, with a mean of 32. The longest gap comes from Jammu and Kashmir, whose assembly was not re-elected between 2008 and December 2014, so its 2014 assembly election is genuinely the first one after the 2009 general election. The interaction with the interval is insignificant: -0.24 points of seat share per month ($F(1, 343) = 0.29$, $p = 0.59$, 95 percent interval $[-1.11, 0.63]$). Tercile estimates are -12.9, -4.4, and -6.4 points at mean intervals of 20, 30, and 47 months, with only the first marginally significant ($p = 0.070$). There is no evidence that the penalty diminishes over time, as a honeymoon account would predict, although the confidence interval is wide.

Table 13: 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 $F(1, 416) = 0.240$, $p = 0.624$. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

10.3 Candidates and Turnout

A specific version of the diversion account predicts that winning parties field weaker or less committed assembly candidates. Tables E.8–E.10 examine three measures of candidate experience in each tercile: incumbency as a sitting MLA, the number of prior terms won, and whether the candidate is recontesting the same seat. None shows a decline. The incumbency and prior-terms estimates are small and insignificant throughout ($p \geq 0.11$). Recontesting is, if anything, slightly more common among winners in their strongest segments (10 and 9 points, $p = 0.063$ and $p = 0.098$), although the intervals still allow small declines of 1 to 2 points. These measures capture experience and continuity rather than candidate quality more broadly, but they provide no support for the hypothesis that winning parties neglect their assembly candidates.

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 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 evidence is more informative about what can be excluded than about what can be confirmed. Winning parties do not detectably withdraw from assembly contests, do not field less experienced candidates, and do not concentrate their votes in their strongholds. I find no evidence that the penalty differs by national or state office or diminishes with the interval between elections, although these tests are imprecise. The remaining pattern is that

winning parties contest as many seats, in segments that are at least as favorable, and win fewer of them.

The most parsimonious account consistent with this evidence is a small decline in support spread across many segments, which FPTP rules convert into a substantially larger loss of seats. A vote decline of two to three points would be too small to detect in aggregate vote share, would show up as a lower win rate in contested seats, and, given observed margins, would be sufficient to produce the seat loss. The weak evidence of rival gains in previously close segments is consistent with this account. The data cannot determine the source of the seat loss. Voter balancing across levels of government and a less visible reduction in campaign effort, such as fewer party workers in the constituency, are both plausible, and the sample is not large enough to distinguish them. Data on campaign spending, party-worker deployment, and alliance arrangements would help distinguish these explanations. Alliance records are particularly important, since without them I cannot determine whether seats lost by the party went to a rival or to an ally.

11 Conclusion

Indian parties often expect a parliamentary victory to strengthen their position in the subsequent state election. This paper finds that, at least for narrow victories, the opposite holds. Using close parliamentary contests from 2004 to 2019, I show that a party narrowly winning a parliamentary seat subsequently wins 8 to 9 percentage points fewer assembly seats in the same constituency at the next state election, or about 0.6 of a seat in a typical constituency. The estimate is robust to alternative bandwidths, control sets, and adjustment for the party's prior assembly performance, and in the full pooled sample to a local quadratic specification and state and year fixed effects. It is not driven by assembly elections held simultaneously with the general election.

The loss appears in seats rather than in aggregate votes. The vote-share estimate is close to zero, but its confidence interval admits a decline of nearly three points, and under first-past-the-post rules a decline of that size is sufficient to produce most of the seat loss. Winning parties do not detectably withdraw from assembly contests or field less experienced candidates; rather, they win a smaller share of the seats they contest. One explanation consistent with this evidence is a small decline in support spread across many segments, which first-past-the-post rules convert into a substantially larger loss of seats, although the vote-share estimates are not precise enough to establish it.

These results have implications for the understanding of electoral success in federal systems. They suggest that the "double engine" logic, according to which success at the national and state levels is mutually reinforcing, may not hold at the level of individual constituencies, where a parliamentary victory appears to make the party's subsequent state contest more difficult. They also indicate that studies of party performance under FPTP should examine seats as well as votes, since a null result for vote share can conceal an electoral penalty that is clearly visible in seats.

Future work could examine spillovers in the reverse direction, from assembly victories to subsequent parliamentary elections, and could use data on campaign spending, party workers, and alliances to identify the sources of the seat loss. A further question is whether these electoral losses affect governance outcomes, such as public goods provision or local development. By clarifying what a parliamentary win does and does not deliver at the state level, this paper contributes to a better understanding of political competition across levels of government in India's federal democracy.

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.

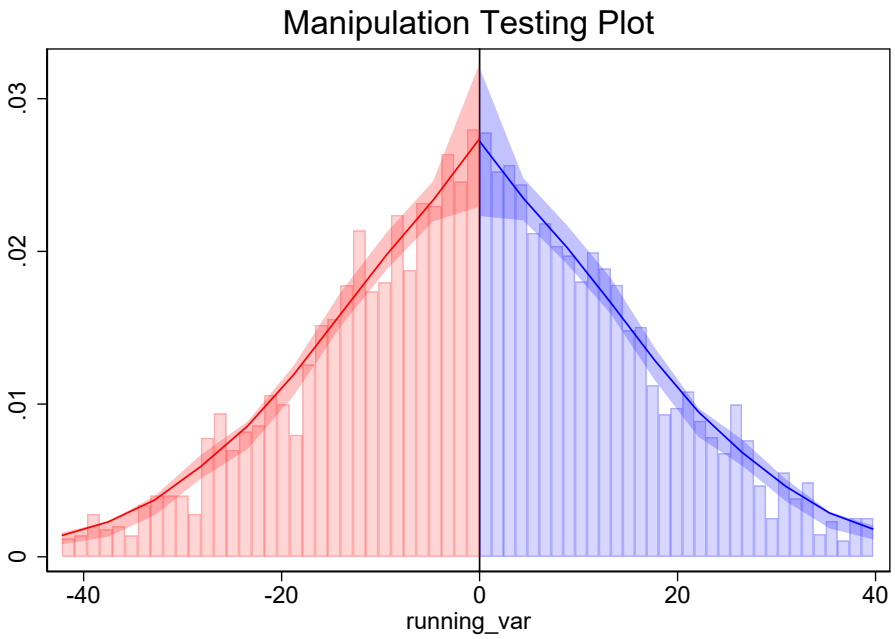


Figure A.1: Density Continuity Test (McCrary, 2008)

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.

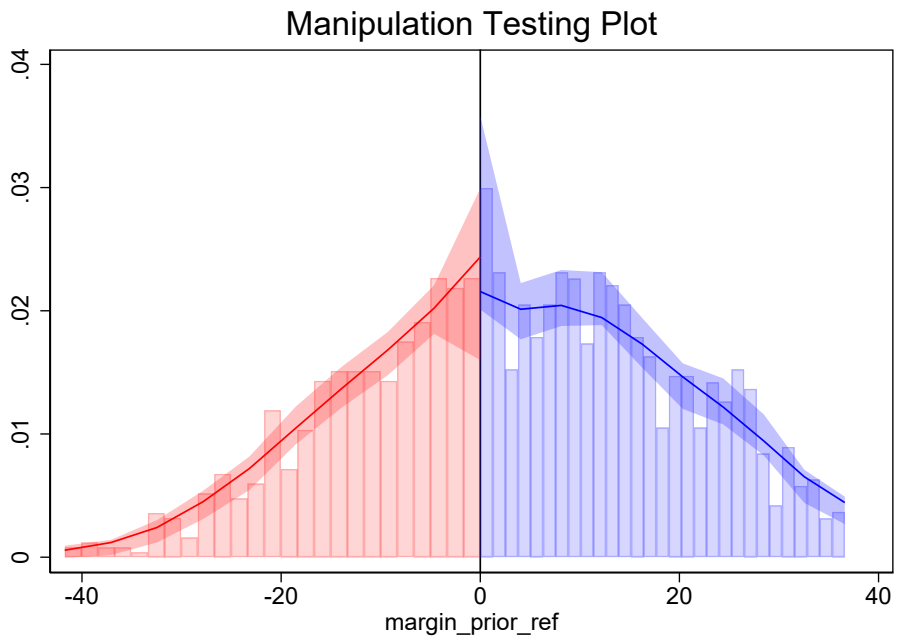


Figure A.2: Density Continuity Test, One Row Per Race: Prior-AE Reference Rule

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.

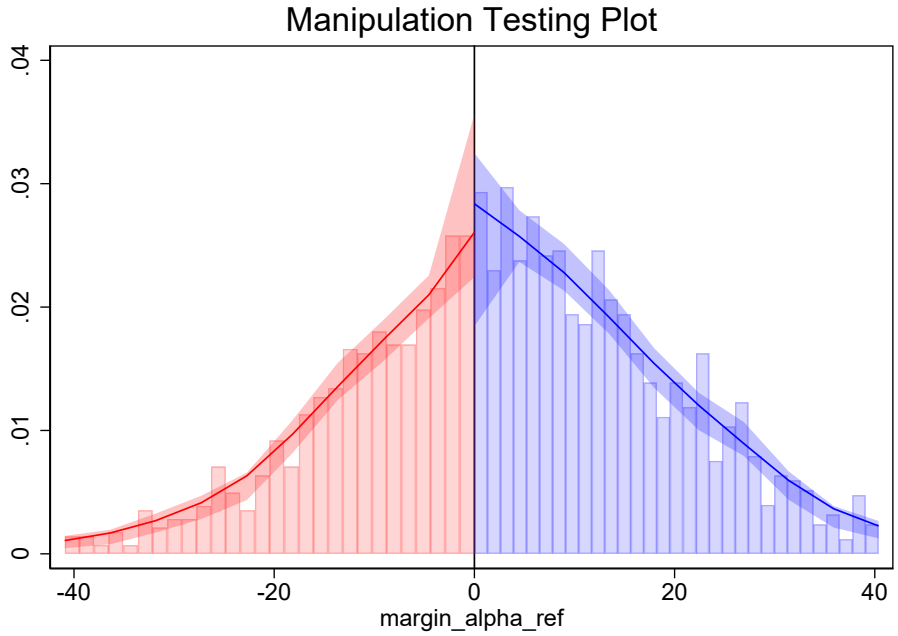


Figure A.3: Density Continuity Test, One Row Per Race: Alphabetical Reference Rule

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

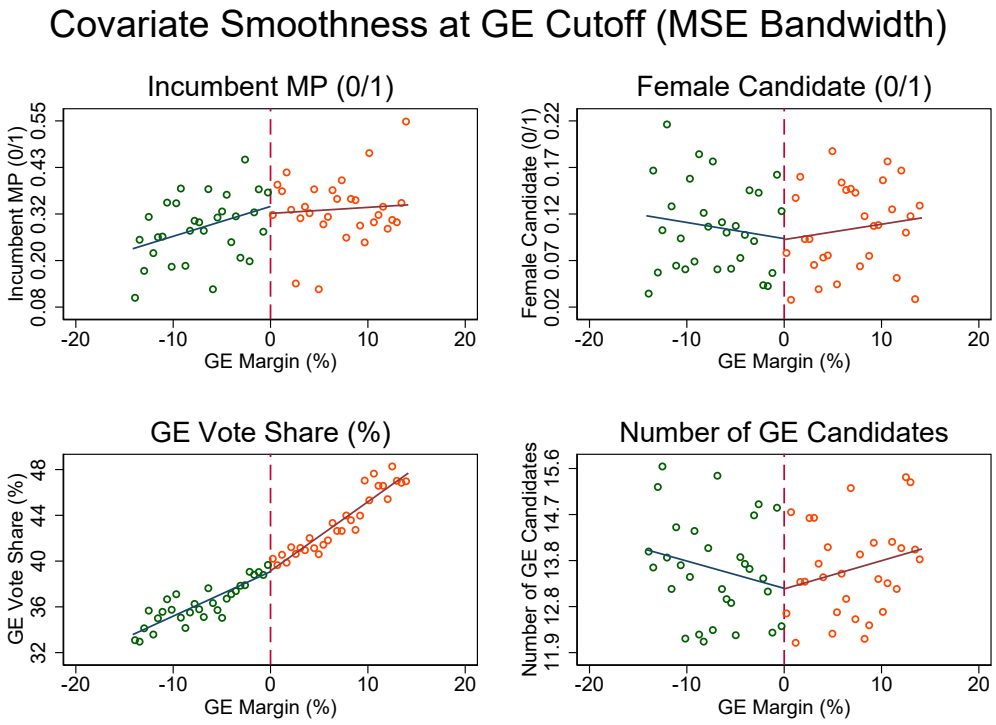


Figure A.4: Covariate Balance at the Cutoff

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

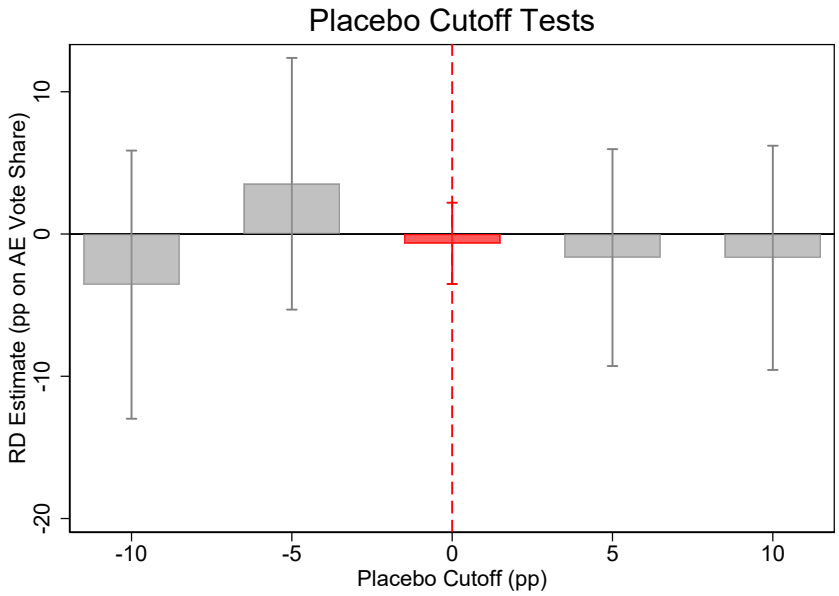


Figure A.5: Placebo Cutoff Tests

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.

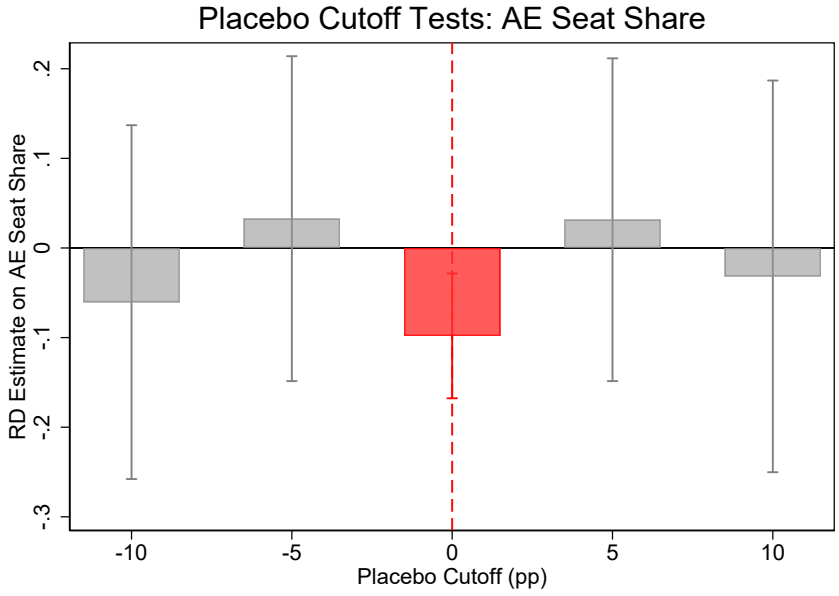


Figure A.6: Placebo Cutoff Tests: State Assembly Seat Share

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.

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

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.

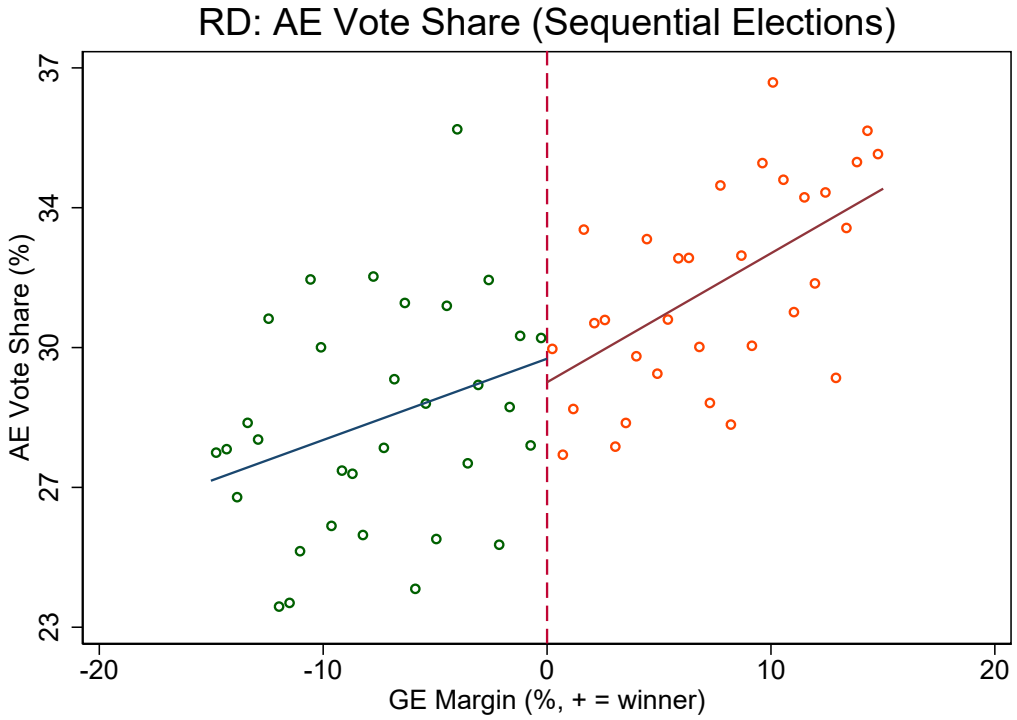


Figure B.1: State-Election Vote Share: Local Linear RD Plot (Sequential Elections)

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

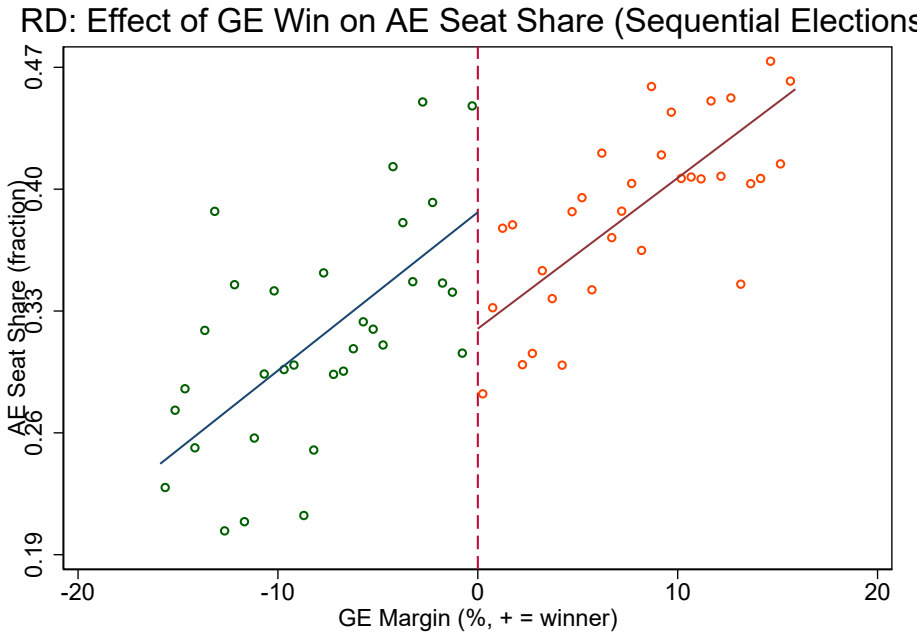


Figure B.2: State Assembly Seat Share: Local Linear RD Plot (Sequential Elections)

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: Full Pooled Sample (Including Simultaneous Elections): Seat Share RD Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	-0.100***	-0.098***	-0.099***	-0.099***	-0.098***	-0.097***	-0.098***	-0.097***
	(0.030)	(0.030)	(0.030)	(0.029)	(0.025)	(0.025)	(0.025)	(0.024)
Bandwidth	CER	CER	CER	CER	MSE	MSE	MSE	MSE
Bandwidth (%)	6.97	6.97	6.97	6.97	10.5	10.5	10.5	10.5
Obs Left	667	667	667	667	946	946	946	946
Obs Right	663	663	663	663	944	944	944	944
Bias-Corrected Est.	-0.10	-0.10	-0.10	-0.10	-0.11	-0.11	-0.11	-0.11
Robust CI Lower	-0.17	-0.16	-0.16	-0.16	-0.16	-0.16	-0.16	-0.16
Robust CI Upper	-0.043	-0.042	-0.043	-0.045	-0.054	-0.054	-0.055	-0.056
Robust SE	0.031	0.031	0.031	0.030	0.028	0.028	0.027	0.027
Robust p-value	0.00080	0.00090	0.00075	0.00055	0.000087	0.000088	0.000068	0.000051
Candidate Controls	N	Y	Y	Y	N	Y	Y	Y
Constituency Controls	N	N	Y	Y	N	N	Y	Y
Year + State FE	N	N	N	Y	N	N	N	Y

Notes: Same specification as Table 5 but on the full pooled sample (N=3914), which also includes the 539 simultaneous-election races discussed above and excluded from the baseline sample. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table B.2: 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.3: 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.4: Robustness: Bandwidth Sensitivity Summary

	AE Vote Share				AE Seat Share			
	(1) CER	(2) MSE	(3) $\pm 5pp$	(4) $\pm 10pp$	(5) CER	(6) MSE	(7) $\pm 5pp$	(8) $\pm 10pp$
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

If FPTP amplification drives the effect, it should be larger 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 estimates the effect by region. It is significant in the north (-0.10 and -0.11; $p = 0.021$ and $p = 0.007$), the east (-0.14 and -0.12; $p = 0.017$ and $p = 0.032$), and the west (-0.13 and -0.11; $p = 0.022$ and $p = 0.026$). It is smallest and imprecise in the south (-0.065 and -0.068; $p = 0.14$ and $p = 0.10$). This ordering is consistent with the hypothesis that dominant southern parties, whose national and state campaigns are closely integrated, are better able to absorb a parliamentary victory. The regional samples are small, however, and the differences are not tested formally, so I treat this evidence as exploratory.

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). These differences are suggestive at most.

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

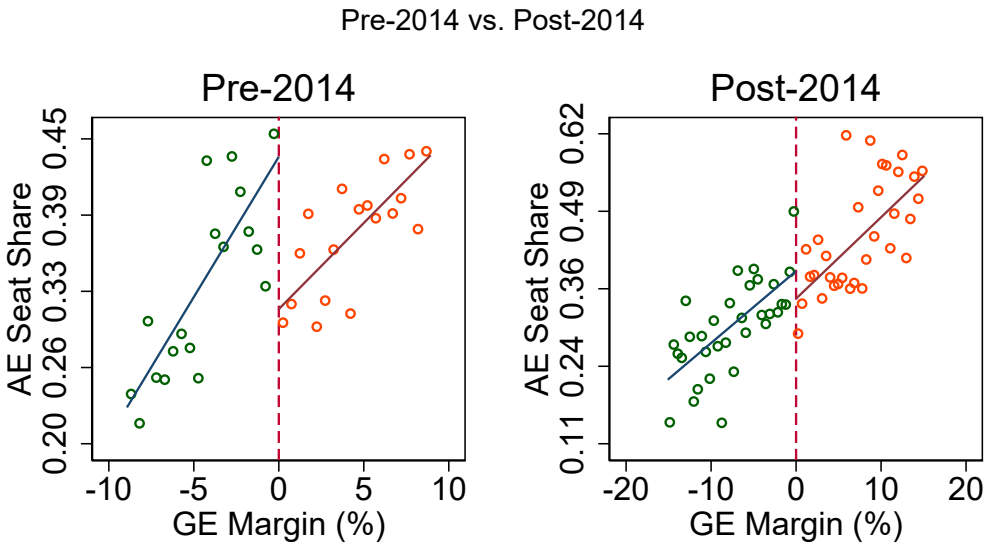


Figure D.1: Heterogeneity by Electoral Era: Pre-2014 vs. Post-2014

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

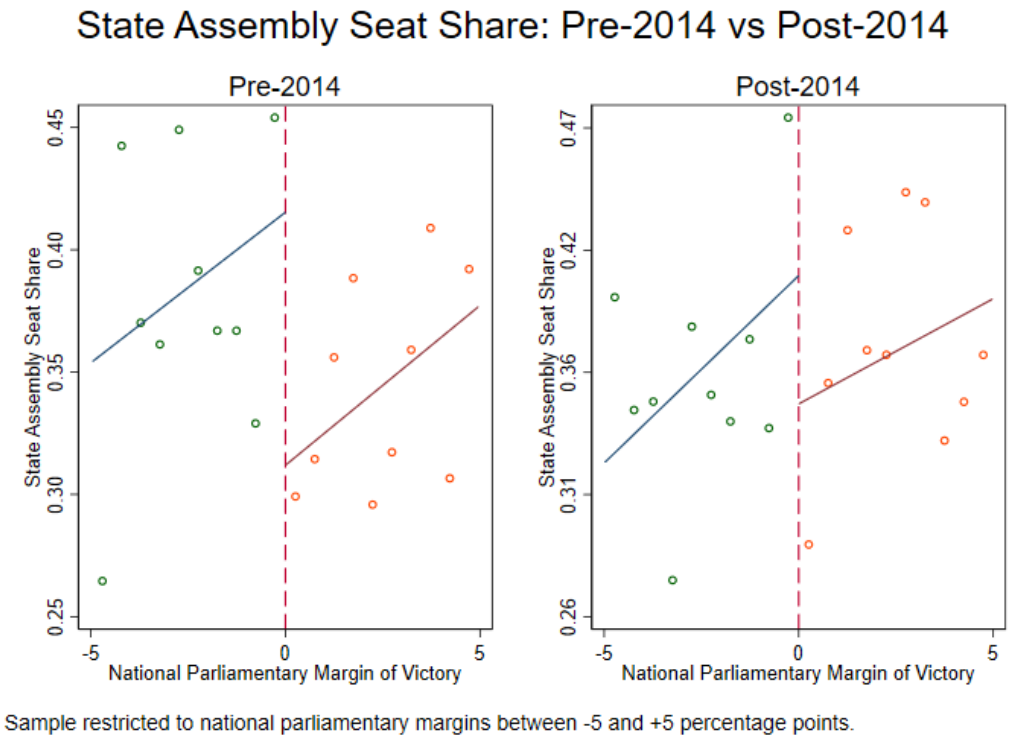


Figure D.2: Fixed-Bandwidth RD Estimates by Electoral Era

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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

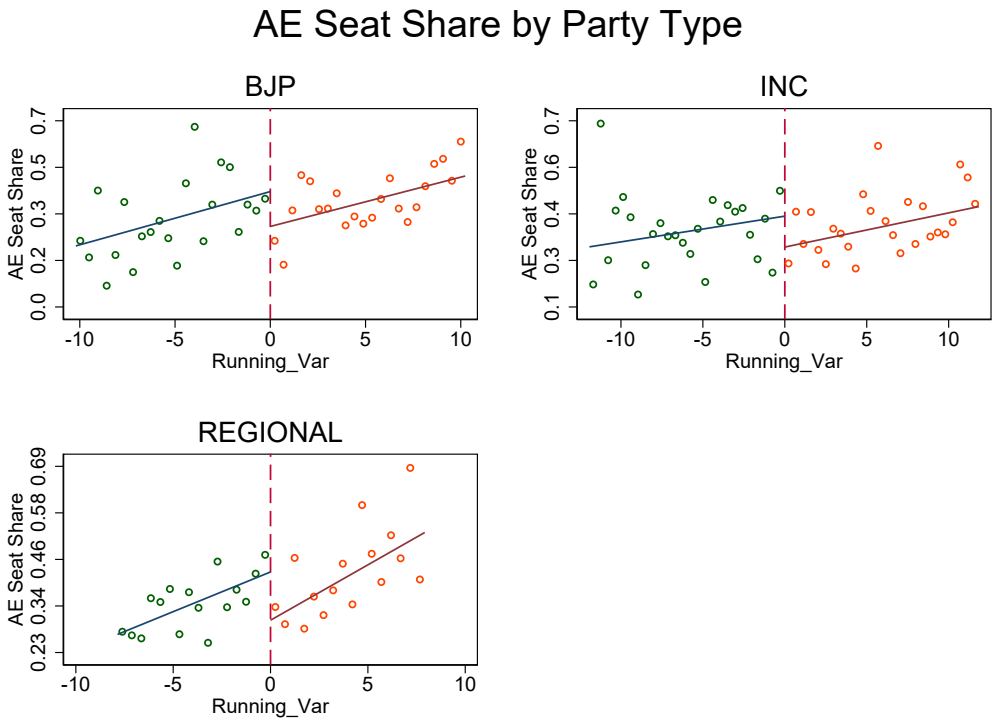


Figure D.3: Heterogeneity by Party: State Assembly Seat Share

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 11. 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 11, 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. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.