DISCUSSION PAPER SERIES

DP21790

MAJORITY LOST: GLOBAL EVIDENCE AND FIELD EXPERIMENTS ON ELECTORAL MISCOORDINATION

Francesco Capozza, Ruben Durante and Siddharth Eapen George

POLITICAL ECONOMY

CEPR

ISSN 0265-8003

MAJORITY LOST: GLOBAL EVIDENCE AND FIELD EXPERIMENTS ON ELECTORAL MISCOORDINATION

Francesco Capozza, Ruben Durante and Siddharth Eapen George

Discussion Paper DP21790
Published 26 July 2026
Submitted 24 July 2026

Centre for Economic Policy Research
187 boulevard Saint-Germain, 75007 Paris, France
2 Coldbath Square, London EC1R 5HL
Tel: +44 (0)20 7183 8801
www.cepr.org

MAJORITY LOST: GLOBAL EVIDENCE AND FIELD EXPERIMENTS ON ELECTORAL MISCOORDINATION

Abstract

Voters in multiparty systems often fail to coordinate on majority-preferred parties. Using constituency-level election data linked to party-ideology measures across 96 countries, we show that in 20–30% of multiparty elections since 1940 the Condorcet winner does not win, and in roughly 10% the winner is the Condorcet loser. Failures are most common under plurality rule and in fragmented, volatile party systems, and systematically disadvantage opposition and culturally liberal parties while benefiting organizationally strong ones. Can information fix this? In four pre-registered experiments around Indian elections (N > 4,000), constituency-level polls shift voting intentions by at most 2 percentage points, a precisely estimated null, despite modest belief updating, though undecided voters become 7 points more likely to support a viable party. A model with expressive motives rationalizes both results: polls refine beliefs but lack the leverage to overcome entrenched attachments, identifying boundary conditions on polls as coordination devices.

JEL Classification: D72, P17

Keywords: Condorcet winner, Coordination failure, Polls

Francesco Capozza - francesco.capozza@ub.edu
UB, IEB and CESifo

Ruben Durante - ruben.durante@upf.edu
National University of Singapore, ICREA-UPF, CESifo, IZA and CEPR

Siddharth Eapen George - segeorge@nus.edu.sg
National University of Singapore

Acknowledgements
We are grateful to Eleonora Alabrese, Enrico Cantoni, Simone Cremaschi, Francesco Ferlenga, Georg Granic, Helios Herrera, Aniol Llorente-Saguer, Jordi Muñoz, Emilie Sartre, Miriam Venturini for their helpful comments, and to Alessandro Izzo and Simone Genetin for excellent research assistance. This work was supported by the Centre for Trusted Internet and Community at the National University of Singapore. The project has received an exemption to IRB by the National University of Singapore. The authors have no conflicts of interest to declare. The experiment has been pre-registered on AEA Registry https://doi.org/10.1257/rct.15337-1.0.

Majority Lost: Global Evidence and Field Experiments on Electoral Miscoordination *

Francesco Capozza[†] Ruben Durante[‡] Siddharth George[§]

July 24, 2026

Abstract

Voters in multiparty systems often fail to coordinate on majority-preferred parties. Using constituency-level election data linked to party-ideology measures across 96 countries, we show that in 20–30% of multiparty elections since 1940 the Condorcet winner does not win, and in roughly 10% the winner is the Condorcet loser. Failures are most common under plurality rule and in fragmented, volatile party systems, and systematically disadvantage opposition and culturally liberal parties while benefiting organizationally strong ones. Can information fix this? In four pre-registered experiments around Indian elections ($N > 4{,}000$), constituency-level polls shift voting intentions by at most 2 percentage points, a precisely estimated null, despite modest belief updating, though undecided voters become 7 points more likely to support a viable party. A model with expressive motives rationalizes both results: polls refine beliefs but lack the leverage to overcome entrenched attachments, identifying boundary conditions on polls as coordination devices.

Keywords: Condorcet Winner, Coordination Failures, Polls

JEL Classification codes: D72, K16, P17

*We are grateful to Eleonora Alabrese, Enrico Cantoni, Simone Cremaschi, Francesco Ferlenga, Georg Granic, Helios Herrera, Aniol Llorente-Saguer, Jordi Muñoz, Emilie Sartre, Miriam Venturini for their helpful comments, and to Alessandro Izzo and Simone Genetin for excellent research assistance. This work was supported by the Centre for Trusted Internet and Community at the National University of Singapore. The project has received an exemption to IRB by the National University of Singapore. The authors have no conflicts of interest to declare. The experiment has been pre-registered on AEA Registry https://doi.org/10.1257/rct.15337-1.0

[†]UB, IEB and CESifo. francesco.capozza@ub.edu

[‡]NUS, ICREA-UPF, IZA, CESifo, and CEPR. rubendurante@gmail.com

[§]National University of Singapore. segeorge@nus.edu.sg

1 Introduction

Elections aggregate citizens' preferences into collective choices. Yet voters in multiparty systems face a coordination problem: many share broad preferences but disperse their support across ideologically similar parties, enabling less-preferred options to prevail even when they represent only a minority. Bouton and Castanheira (2012) terms this the divided-majority problem, and classical thinkers including Condorcet saw it as a core impediment to effective collective decision-making. Such coordination failure is more likely when voters lack precise information about the relative strength of each party, impeding their ability to vote strategically (Duverger's law; Duverger, 1954; Cox, 1997). When such informational gaps persist, even small frictions can generate substantial "wasted votes," and these dynamics are further compounded by expressive motives, which can shape both turnout and vote choice (Pons and Tricaud, 2018).[1] Laboratory and theoretical research confirms that when viability information is scarce or noisy, coordination becomes fragile and even dominated options may prevail (Forsythe et al., 1993, 1996; McKelvey and Ordeshook, 1985), while alternative voting rules and improved poll designs can help implement the Condorcet winner in divided-majority environments (Granić, 2017; Potthoff, 2011).

In this paper, we ask three questions: How common are voter coordination failures across democracies? What explains their occurrence? And can low-cost information, such as polling, mitigate them in real-world settings? Our approach presents new facts about the prevalence of coordination failure among voters, and experimentally examines the potential of information, specifically polls, to reduce these failures.

We define two sets of election events. First, *weak* coordination failures arise when a Condorcet winner exists (that is, a party that would defeat every other party in pairwise majority contests), yet that party does not win the election. Second, *strong* coordination failures occur when the winner is the Condorcet loser, the party that would lose to every other party in all pairwise majority contests, if such a party exists. We benchmark outcomes against the Condorcet winner because it is estimable from vote shares and ideological distances alone across our full cross-national sample,

[1] Piketty (2000) formalises this tension by modelling voters as trading off current electoral payoffs against the communicative value of supporting a non-viable candidate. In his framework, voters are neither purely strategic nor purely sincere, occupying an intermediate position that can sustain equilibria in which the majority-preferred candidate consistently fails to win.

and because it carries direct normative content: when the Condorcet winner fails to win, a majority-preferred and feasible alternative has been displaced, a violation of majority rule rather than a deviation from some welfare optimum. Section 2 develops the benchmark in full and contrasts it with median-voter and disproportionality alternatives. These failures matter because they can distort representation, shift policy, and, by advantaging well-organized or incumbent actors, reshape patterns of political competition (Ferlenga et al., 2025).

To construct these measures, we assemble constituency-level election data from the Constituency-Level Elections Archive (CLEA) (Kollman et al., 2020) and link them to party ideological positions from V–Party v2 (Lindberg et al., 2022) and the MARPOR/Manifesto Corpus (Lehmann et al., 2025) via the Party Facts project (Bederke et al., 2019; Döring and Regel, 2019). Section 2 sets out the resulting sample of *valid* elections, which spans tens of thousands of constituency-elections across both ideology sources.

We document four main patterns. First, coordination failures are widespread. Between 1940 and 2020, average weak failure rates range from 20% to 30% of elections and appear on every continent; the series also shows substantial persistence across decades. Second, institutional design shapes these outcomes: plurality systems exhibit the highest failure rates, followed by ranked-choice and mixed systems, whereas runoff systems display the lowest, in line with classic strategic-coordination logic (Duverger, 1954; Cox, 1997). Third, fragmentation of the political landscape is strongly and positively associated with failure, while economic development shows no robust partial correlation once election fixed effects are included; the age of democracy, by contrast, correlates slightly positively with failure rates. Finally, the distributional consequences of miscoordination are patterned: the parties that tend to lose are often opposition, stigmatized ("pariah"), or culturally liberal, whereas the beneficiaries are typically organizationally strong or locally embedded. These asymmetries align with theories linking organization and mobilization to electoral performance under uncertainty (Panebianco, 1988; Tavits, 2012; Kitschelt and Wilkinson, eds, 2007), and they imply that miscoordination is not a mere technical inefficiency: it can generate a persistent representational bias even when all voters behave sincerely.

We then examine whether targeted viability information can reduce miscoordination. To that end, we field four survey experiments in India, an archetypal high-stakes, first-past-the-post, multiparty democracy with coordination challenges

comparable to those in the UK, Canada, and Australia. The Indian context features dense multiparty competition, frequent viable third parties, and substantial shares of undecided voters, making it a demanding environment in which to test coordination interventions. Each experiment relies on two phone surveys conducted in constituencies across four states (Bihar, Delhi, Telangana, and Uttar Pradesh), in which respondents are randomly assigned to receive or not receive constituency-level "polls" constructed from baseline voting intentions, information not otherwise available at the constituency level in these races. This design allows us to isolate how locally relevant viability information shapes voters' choices in settings where coordination is typically difficult.

Across these states, we find precisely estimated null effects of constituency-level polls on the core behavioral margins that would embody successful coordination. Receiving viability information does not measurably shift party-specific voting intentions (the estimated shift toward the top-two parties is at most 2 percentage points and statistically indistinguishable from zero), nor does it change parties' positions in respondents' rankings or their affective ratings. Interaction terms between treatment and pre-treatment party preferences are small and statistically indistinguishable from zero, allowing us to rule out moderate persuasion effects for both major and minor parties. At the belief level, respondents already anticipate substantial support for the top-two parties even without treatment, and the polls only modestly reinforce these expectations. The one margin on which the information moves behavior is among voters who had not yet formed an intention: initially undecided respondents who received the poll are 7 percentage points more likely to vote for a top-two party, a result that is likely a lower bound on the true effect, since undecided voters may also resolve their preferences through natural exposure to campaign information between survey waves. Concerns about "wasting" a vote do not emerge as a dominant or uniquely salient consideration in either group.

The most direct interpretation is not that voters ignore viability information, but that in high-salience first-past-the-post contests with entrenched partisan identities, modest informational refinements have limited behavioral leverage: voters already hold reasonably accurate priors about which parties are viable, so constituency polls refine second-order beliefs at the margin rather than meaningfully changing perceived pivotality. This also explains the gap with laboratory findings, where viability signals reliably induce strategic defection under conditions—minimal expressive attachments, clearly defined payoffs, limited prior information—that are

precisely absent in the field (Forsythe et al., 1993, 1996; McKelvey and Ordeshook, 1985), and why observational evidence from settings with weaker partisan loyalties detects belief updating but stops short of large intention shifts (Bursztyn et al., 2024). The binding constraint is not information alone, but the limited leverage of small informational refinements in electorates where preferences are identity-laden and stable: information works precisely where expressive attachment does not block it, among the initially unattached. A simple model of voting with expressive motives and poll information, developed in Appendix A, delivers this null as the prediction of an identifiable region of the parameter space rather than an anomaly, nesting fulfilled-expectations models with polls (McKelvey and Ordeshook, 1985) and mixed expressive–instrumental utility (Brennan and Lomasky, 1993; Piketty, 2000).

We contribute to three strands of literature. First, theories of strategic coordination emphasize the pressures toward two-party competition under plurality rule (Duverger, 1954; Cox, 1997; Laakso and Taagepera, 1979) but offer no systematic way to quantify the severity of coordination breakdowns across settings; we supply that measurement, mapping where and when coordination frictions bite and which parties gain or lose.

Second, we contribute to the work on party systems, polarization, and churn (Harmel and Robertson, 1985; Mainwaring and Zoco, 2007; Mair, 1997; Sartori, 1976; Dalton, 2008; Benoit and Laver, 2006) and on coordination and incumbency advantage (Ferlenga et al., 2025). Our results challenge the assumption that party size is the primary correlate of coordination success: classic theories predict that large parties benefit mechanically from strategic desertion, whereas we find that organizational capacity, not size, is what predicts gaining from coordination failures—a pattern aligned instead with organizational theories of electoral mobilization (Panebianco, 1988; Tavits, 2012; Kitschelt and Wilkinson, eds, 2007).

Third, we extend the experimental literature on polls and information by documenting a large-scale field setting in which carefully designed, constituency-level poll information leaves *intended* votes and *preferences* effectively unchanged (despite modest shifts in second-order beliefs), thus providing a disciplined null result that helps delineate when polls can, and cannot, mitigate miscoordination in multiparty elections (Agranov et al., 2018; Kittel et al., 2014; Großer and Schram, 2010; Potthoff, 2011; Meffert et al., 2011; Bursztyn et al., 2024; Alabrese and Fetzer, 2024). The contrast with the closely related laboratory study of Feltham et al. (2025), in

which viability-revealing polls do help protest voters coordinate, is instructive: the same instrument produces coordination in the lab but not in high-salience electoral environments where expressive attachments are entrenched and voters already hold accurate priors about which parties are viable. Relatedly, whereas informing voters about candidates' ideology, valence, capacities, or fulfilled promises has been shown to shift vote shares and intentions (Pons, 2018; Baysan, 2022; Kendall et al., 2015; Cruz et al., 2021, 2024b,a), in our setting voters value the information we provide yet do not change their voting intentions toward parties more likely to win.

The paper proceeds as follows. Section 2 describes the observational data and the construction of our coordination-failure measures. Section 3 presents descriptive evidence on coordination failures. Section 4 describes the experimental design and reports the results. Section 5 discusses the implications and relates our findings to the broader literature. Section 6 concludes.

2 Data

2.1 Secondary Data

Our corpus of constituency-level electoral results is drawn from the Constituency-Level Elections Archive (CLEA), Version 20201216 (Kollman et al., 2020). CLEA consolidates votes at the constituency level for lower- and upper-chamber elections worldwide and, in its constituency unit, records more than one million constituency-level results across roughly 180 countries. To attach ideological content to these results, we draw on two complementary sources of party characteristics and policy stances: the Manifesto Project (MARPOR) and the Varieties of Party Identity and Organization (V–Party) dataset. We describe each in turn, together with the indices we use, and then explain how the three datasets are linked.

The Manifesto Project (MARPOR). The Manifesto Project provides the most extensive collection of digitally annotated electoral programs available, covering 1,373 parties in 67 countries across 849 national-level elections (Lehmann et al., 2025). Trained coders read each party manifesto and assign every quasi-sentence to a single policy category, organised into seven domains: external relations, freedom and democracy, political system, economy, welfare and quality of life, fabric of society, and social groups. The category variables (the `perXXX` fields) record the share of quasi-sentences in a manifesto devoted to each sub-category, so that, for

example, a value of 0.58 on `per101` indicates that 0.58 percent of the manifesto consists of positive references to special foreign relationships.

From MARPOR we use the RILE (Right–Left) index, which summarises a party's general left–right programmatic orientation. RILE is constructed as the sum of manifesto emphases on thirteen "right" categories minus the sum of emphases on thirteen "left" categories. Left-coded categories include internationalism (positive), market regulation, and welfare-state expansion; right-coded categories include freedom and human rights, free-market economy, and protectionism (negative). A positive RILE value indicates a manifesto weighted toward right-coded content, a negative value indicates left-coded content, and values near zero indicate a centrist programmatic profile.

Varieties of Party Identity and Organization (V–Party). V–Party offers expert-coded assessments of political parties' policy positions and organisational capacity, covering 3,467 parties in 178 countries across 3,151 national-level elections (Lindberg et al., 2022). Its variables are grouped into four domains: party identity, party organisation, populism, and liberal–traditional scaling. Each coded value aggregates the judgments of at least four country experts per party-year; experts typically choose a single option on an ordinal scale, and the released value is a standardised interval-scale score summarising the resulting distribution. This expert-coded design gives V–Party broader country coverage than the manifesto data, at the cost of a shorter time span.

From V–Party we use two interval-scale indices on $[0, 1]$. The *Anti-Pluralism Index* captures the extent to which a party shows a lacking commitment to democratic norms prior to elections, aggregating indicators such as stances toward political opponents, commitment to pluralism, protection of minority rights, and rejection of political violence. The *Populism Index* captures the extent to which party representatives employ populist rhetoric (narrowly defined), aggregating components such as anti-elitism and people-centrism. Together, the one-dimensional RILE measure and the two-dimensional anti-pluralism / populism space provide two conceptually distinct ways of locating parties in ideological space, which we exploit as a robustness check throughout.

Table 1 reports summary statistics for the three indices. RILE ranges widely around a mildly left-of-centre mean, while the anti-pluralism and populism indices, bounded on $[0, 1]$, are concentrated in the lower portion of their range.

Table 1. Summary statistics for the RILE, anti-pluralism, and populism indices

Index	Mean	Std. Dev.	Min	Max
RILE	−3.0	22.2	−74.3	91.9
Anti-Pluralism	0.341	0.309	0.012	1.000
Populism	0.326	0.246	0.019	0.550

Notes: RILE is from the Manifesto Project and is unbounded; positive values denote right-leaning manifestos. Anti-pluralism and populism are from V–Party and are normalised to the unit interval. Statistics are computed over the party-elections used in constructing our distance matrices.

Linking the datasets. MARPOR, V–Party, and CLEA each use their own party identifiers, so the three must be reconciled before vote outcomes can be matched to ideological positions. We link them through the Party Facts project, which synchronises party identifiers across the core datasets of comparative political science (Bederke et al., 2019; Döring and Regel, 2019). Party Facts codes identifiers for parties that have won at least five percent of the vote in a national election; parties not coded by Party Facts are necessarily dropped when we merge the distance matrices with the vote records. This linking determines the effective sample: an election is usable only when its contesting parties appear both in CLEA's vote records and in the ideological coding of at least one of the two ideology sources.

Finally, we proxy institutional and development histories using years of continuous democracy (Boix et al., 2013) and GDP per capita (PPP) from Eurostat et al. (2025), extended with Gapminder Foundation (2025).

2.2 Constructing Coordination Failures

2.2.1 Ideological Distance Matrices (MARPOR and V–Party)

For each election e, jointly identified by country c and year–month t, let $\mathcal{P}_e$ denote the set of parties contesting election e. Using party-level ideology measures from MARPOR and V–Party, we construct pairwise ideological distances $d_{ij,e}$ for all party pairs $(i, j) \in \mathcal{P}_e^2$. For each election e, these distances form a symmetric matrix $D_e = [d_{ij,e}]_{i,j\in\mathcal{P}_e}$ with $d_{ii,e} = 0$.

We construct two sets of distance matrices: one using only the MARPOR *RILE* index, and one using the two V–Party indices, *anti-pluralism* and *populism*.

Z-score normalization. To ensure comparability across indices, we standardize each index using z-scores. For index $k \in \{\text{rile}, \text{anti}, \text{pop}\}$ and party i in election e, let $X_{i,k,e}$ denote the raw value. The standardized value is

$$Z_{i,k,e} \;=\; \frac{X_{i,k,e} - \mu_{k,e}}{\sigma_{k,e}}, \tag{1}$$

where $\mu_{k,e}$ and $\sigma_{k,e}$ are the mean and standard deviation of index k in election e.[2]

Pairwise distances. We define ideological distance as Euclidean distance in the standardized index space. Under the MARPOR specification (one-dimensional), the distance between parties i and j in election e is

$$d_{ij,e}^{\text{MARPOR}} \;=\; \left\| Z_{i,\text{rile},e} - Z_{j,\text{rile},e} \right\|_2 \;=\; \sqrt{\left(Z_{i,\text{rile},e} - Z_{j,\text{rile},e}\right)^2}. \tag{2}$$

Under the V–Party specification (two-dimensional), let $\mathbf{Z}_{i,\text{V},e} = (Z_{i,\text{anti},e},\, Z_{i,\text{pop},e})$. The distance is then

$$d_{ij,e}^{\text{V}} \;=\; \left\| \mathbf{Z}_{i,\text{V},e} - \mathbf{Z}_{j,\text{V},e} \right\|_2 \;=\; \sqrt{\left(Z_{i,\text{anti},e} - Z_{j,\text{anti},e}\right)^2 + \left(Z_{i,\text{pop},e} - Z_{j,\text{pop},e}\right)^2}. \tag{3}$$

2.2.2 Computing Pairwise Vote Outcomes

Given the pairwise ideological distances $d_{ij,e}$, we simulate the outcomes of counterfactual *pairwise* elections within each election e. For every ordered party pair (p_i, p_j), we compute which party would win if *only* p_i and p_j contested the election. Collecting these simulated outcomes across all pairs yields a pairwise vote (or majority) matrix for each election. We construct two such matrices: one based on MARPOR distances and one based on V–Party distances.

Reallocating votes in a two-party counterfactual. Let $v_{k,e}$ denote the observed vote share of party p_k in election e, with $\sum_{p_k \in \mathcal{P}_e} v_{k,e} = 1$. Consider the counterfactual contest between parties p_i and p_j. The key object is the simulated vote share that p_i would obtain in this two-party contest, denoted $V_{i \succ j,e}$. We assume that voters affiliated with a third party $p_k \notin \{p_i, p_j\}$ reallocate their votes to whichever of p_i or

[2] If normalization is instead performed over a different reference set (e.g., the global sample or within-country), replace $\mu_{k,e}$ and $\sigma_{k,e}$ accordingly.

p_j is ideologically closer to p_k. Formally, define the indicator

$$1_{k \to i|ij,e} = 1\left\{d_{ik,e} < d_{jk,e}\right\}, \tag{4}$$

so that p_k's vote share is assigned to p_i if p_i is closer to p_k than p_j is. When $d_{ik,e} = d_{jk,e}$, we split the vote evenly.[3]

The simulated two-party vote share for p_i against p_j is

$$V_{i \succ j,e} = v_{i,e} + \sum_{p_k \in \mathcal{P}_e \setminus \{p_i, p_j\}} v_{k,e} \cdot 1\left\{d_{ik,e} < d_{jk,e}\right\} + \frac{1}{2} \sum_{p_k \in \mathcal{P}_e \setminus \{p_i, p_j\}} v_{k,e} \cdot 1\left\{d_{ik,e} = d_{jk,e}\right\}. \tag{5}$$

Symmetrically, $V_{j \succ i,e} = 1 - V_{i \succ j,e}$. Party p_i is the simulated winner of the pairwise contest if $V_{i \succ j,e} > \frac{1}{2}$. This reallocation procedure operationalizes the divided-majority structure formalized by Bouton and Castanheira (2012): voters affiliated with eliminated parties transfer their support to the ideologically closest remaining option, and the question is whether the resulting majority coalesces on the Condorcet winner or fragments sufficiently for a minority-preferred party to prevail.

Pairwise vote matrix and Condorcet winners. Define the pairwise majority matrix M_e with entries

$$M_{ij,e} = 1\left\{V_{i \succ j,e} > \tfrac{1}{2}\right\} \qquad \text{for } i \neq j, \tag{6}$$

and $M_{ii,e} = 0$. A party $p_w \in \mathcal{P}_e$ is a *Condorcet winner* in election e if it defeats every other party in simulated pairwise contests:

$$M_{wj,e} = 1 \quad \forall\, j \in \mathcal{P}_e,\ j \neq w. \tag{7}$$

Intuitively, the Condorcet winner, when it exists, is the party that would win against any opponent in a head-to-head election. This procedure corresponds to a deterministic spatial-voting rule in which party vote shares proxy the mass of voters located at each party's position, and voters support the closest available party when their preferred party is absent.

Using the computed pairwise preferences, we define two sets of election events: (i) *weak coordination failures*, comprising elections in which the winning party is not the Condorcet winner, the party that would defeat every other party in pairwise

[3]That is, if $d_{ik,e} = d_{jk,e}$, we allocate $\frac{1}{2}v_{k,e}$ to each of p_i and p_j. Alternative deterministic tie-breakers yield very similar results when exact ties are rare.

majority contests, if such a party exists (Condorcet, 1785; Nurmi, 1987); and (ii) *strong coordination failures*, comprising elections in which the winner is the Condorcet loser, the party that would lose to every other party in all pairwise majority contests, if such a party exists (Arrow et al., eds, 2002). The latter is a strict subset of the former.

Three features recommend the Condorcet criterion as our benchmark. It requires only ordinal pairwise majority preferences over the realized party set, so it is estimable from vote shares and ideological distances across the full cross-national sample without imposing cardinal utility or a single policy dimension; median-voter optima require assumptions about dimensionality and ideal points untenable across 100 countries, disproportionality indices such as Gallagher are silent on whether the outcome respects majority preferences, and Borda or approval measures require preference data unavailable at this scale. It carries direct normative content: when the Condorcet winner loses, a feasible alternative preferred by a majority has been displaced. And it maps onto the theoretical object of interest, since the divided-majority problem of Bouton and Castanheira (2012) is precisely the failure of the Condorcet winner to prevail when majority-preferred voters disperse across similar parties. Where preference cycles arise the criterion is indeterminate, so we define failures only for elections in which a Condorcet winner exists; cycles are rare in programmatically structured party systems (Potthoff, 2011), so this restriction has little bite on coverage.

2.3 An Illustrative Example: Switzerland 1995, Schaffhausen

To make the construction concrete, consider the October 1995 lower-chamber election in the Swiss constituency of Schaffhausen, using the MARPOR (RILE) distances. Four parties contest the seat: the Swiss People's Party (party identifier 43810), the Freedom Party of Switzerland (43951), the Social Democratic Party (43220), and the Free Democratic Party (43420).

Table 2 reports the pairwise distance matrix obtained by z-score normalising the RILE index within the election and taking absolute differences. Reading along the first row, the Swiss People's Party is closest to the Free Democratic Party (0.416) and furthest from the Social Democratic Party (1.927), so were it absent its supporters would transfer to the Free Democrats.

Applying the reallocation rule in equation (5) to every ordered pair yields the simulated pairwise vote matrix in Table 3.

Table 2. Pairwise ideological distance matrix, Switzerland 1995 (Schaffhausen, MARPOR)

	Swiss People's Party (43810)	Freedom Party of Switzerland (43951)	Social Democratic Party (43220)	Free Democratic Party (43420)
43810	0	1.047	1.927	0.416
43951	1.047	0	2.973	0.630
43220	1.927	2.973	0	2.343
43420	0.416	0.630	2.343	0

Notes: Entries are Euclidean distances in z-score-normalised RILE space; the matrix is symmetric and the diagonal is zero. Smaller values indicate ideologically closer parties.

Table 3. Simulated pairwise vote matrix, Switzerland 1995 (Schaffhausen, MARPOR)

	Swiss People's Party (43810)	Freedom Party of Switzerland (43951)	Social Democratic Party (43220)	Free Democratic Party (43420)
43810	0.203	0.900	0.608	0.581
43951	0.085	0.085	0.608	0.085
43220	0.378	0.378	0.378	0.378
43420	0.404	0.900	0.608	0.318

Notes: The diagonal reports realised four-way vote shares. Each off-diagonal entry $V_{i \succ j}$ is the simulated share of the row party i in a two-way contest against the column party j, after reallocating the two remaining parties' votes to the ideologically closer contestant. Party i wins the pairwise contest against j when its entry exceeds 0.5.

The Swiss People's Party wins every pairwise contest (0.900, 0.608, 0.581) and is therefore the Condorcet winner, even though it places third in the realised election with 20.3 percent of the vote. The Social Democratic Party loses every pairwise contest (0.378 against each opponent) and is thus the Condorcet loser, yet wins the actual election with 37.8 percent. Schaffhausen 1995 is consequently a *strong* coordination failure: the realised winner is precisely the party a majority would reject in every head-to-head comparison.

2.4 Selecting Valid Elections

CLEA's full archive is far larger than the sample on which a Condorcet benchmark can be meaningfully evaluated, so we restrict attention to elections that satisfy a small set of requirements. We retain an election as *valid* when it meets three criteria.

First, **at least three parties contest the election**. With only two contestants there is always a majority winner, the pairwise comparison is degenerate, and coordination failure cannot arise by construction; the divided-majority problem we study is defined only when support can fragment across three or more options.

Second, **the parties we can locate ideologically account for at least seventy-five percent of the realised vote**. Parties drop out of an election either because no ideological distance can be computed for them or because Party Facts assigns them no identifier. Requiring broad coverage of the vote ensures that the reallocation of third-party support, and hence the inferred pairwise outcomes, rests on the bulk of the electorate rather than on a thin and possibly unrepresentative slice of it.

Third, **the election is not held under a party-list rule** (operationally, the number of candidates does not exceed the number of parties). Under a list system a party winning, say, thirty percent of the vote is allocated roughly thirty percent of the seats, so there is no discrete winner against which to define a Condorcet violation; our notion of coordination failure presupposes the single-winner, plurality-style contests that dominate single-member-district systems.

We construct our vote outcomes consistently with these restrictions. Where CLEA reports both first- and second-round results, we use first-round votes to build the counterfactual pairwise vote matrix and, where present, second-round votes to determine the realised winner; this is why the failure rates we report for two-round systems capture first-stage miscoordination only. Where a single party fields multiple candidates, we use party-level vote percentages to form the pairwise matrix, falling back on candidate-level percentages only when the party-level shares are unavailable. In roughly half of all valid elections the winner also commands an outright majority of the vote and is therefore automatically a Condorcet winner; the complementary set, elections with a Condorcet winner but no majority winner, bounds the share of contests in which coordination failure can in principle occur.

Because our Condorcet classifications are generated from party-distance matrices, a natural concern is sensitivity to the choice of ideology source. We therefore compute coordination failures under two conceptually distinct distance constructions: a one-dimensional programmatic left–right distance from MARPOR (RILE) and a

two-dimensional distance based on V–Party's anti-pluralism and populism indices. Table C.1 reports coverage and agreement. V–Party provides broader country coverage (96 countries) than the Manifesto data (50), with 47 countries in common; in the overlapping sample we observe 307 elections and 23,883 constituency-elections. A Condorcet winner can be identified in nearly all valid constituency-elections: 41,716 of 41,717 under MARPOR and 33,397 of 33,550 under V–Party. The restriction to elections in which a Condorcet winner exists therefore has almost no bite on coverage, and the failure measures are defined on essentially the full sample rather than a selected subset. Failure rates in the full samples are similar (weak: 27.8% vs. 26.5%; strong: 10.2% vs. 9.5%). In the common sample, the two classifications agree in 76.3% of constituency-elections for weak failures and 85.3% for strong failures. Implied failure prevalences in the overlap are nearly identical across sources (weak: 27.9% in V–Party vs. 28.9% in MARPOR; strong: 9.5% vs. 9.3%), and cross-source disagreements are approximately symmetric under both definitions. Our election sample pools elections unique to either MARPOR or V–Party; when an election appears in both sources, we retain the V–Party version, which provides the larger set of covered elections.

2.5 Methodological Considerations

Spatial-voting approach. Constructing pairwise preferences from ideological distances necessarily embeds assumptions about how voters map parties into utility. Our approach follows standard spatial-voting models in assuming that the dominant dimension summarized in MARPOR and V–Party, typically the left–right axis, captures the bulk of programmatic conflict (Laver and Schofield, 1990; Adams et al., 2005). We treat the resulting pairwise comparisons as proxies for majority preferences over feasible alternatives in the observed party set, and emphasize that this approach abstracts from valence, candidate quality, competence, and non-programmatic considerations.

Strategic party entry. Party entry is itself strategic, and the anticipated presence of a Condorcet loser could deter some parties from running. Our benchmarks are defined over the realized party system and post-election vote shares, so they capture coordination failures *conditional on* observed entry; to the extent that potential entrants anticipate losing and stay out, our estimates are a lower bound on the prevalence of coordination problems under richer choice sets. Entry is in any case

reflected in our regression evidence on fragmentation, new and renamed parties, and party-system churn, which conditions directly on these features.

Pre-electoral alliances. Pre-electoral alliances raise a related concern. Because our procedure treats each party as an independent contestant and reallocates votes on bilateral ideological proximity, it ignores the coordinating role of the alliance agreement itself, which could attenuate estimated failure rates where alliances are most common. Alliances are, however, endogenous to the coordination problem: they arise precisely because elites anticipate voter miscoordination and seek to preempt it (Golder, 2006). Our failure rates therefore capture coordination outcomes *net* of whatever elite-level coordination alliances already provide, and are again a lower bound.

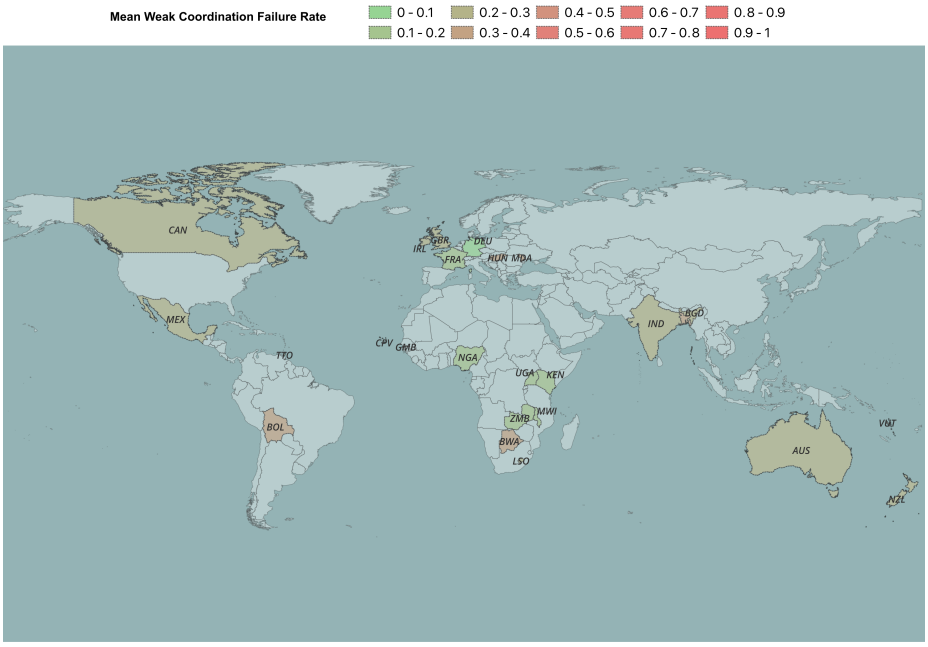
Alternative approaches to preference elicitation. Survey-based thermometer ratings offer a preference-based alternative to ideological distances, but are unsuitable at this scale for two reasons. They are individual-level, so building constituency-level majority matrices demands within-constituency sample sizes that available cross-national surveys, including WPID (Gethin et al., 2021), do not reach; and they are fielded inconsistently across countries and waves, with anchors whose meaning varies across contexts.

3 Empirical Analysis

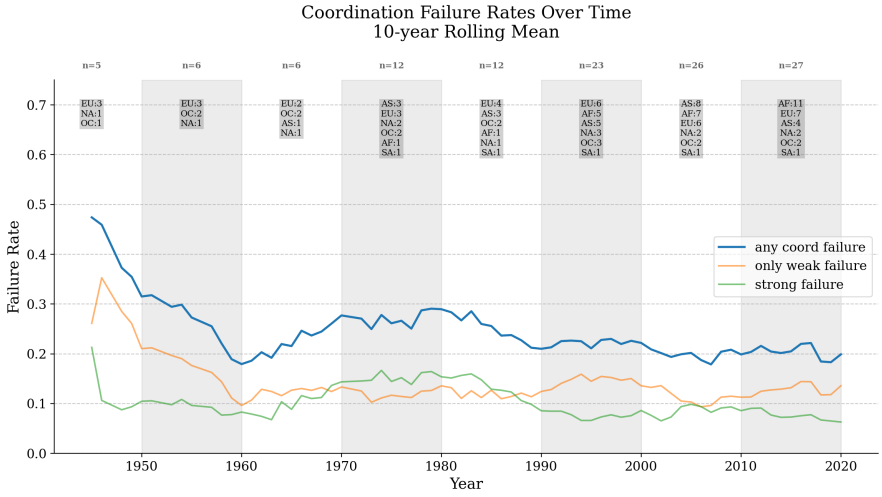
We compute mean weak coordination failure rates across countries with multiparty systems, averaging data from 1940 to 2020. Figure 1a maps these rates and shows that the phenomenon is widespread, affecting all continents. Figure 1b plots the 10-year rolling mean and reveals substantial persistence over time: since 1960, the mean rate of any coordination failure has remained between 20% and 30%, rising from 1960 to 1980, declining from 1980 to 2000, and plateauing thereafter. Series restricted to weak failures only and to strong failures only display similar patterns.

In what follows, we explore the drivers of these cross-national differences, organizing the analysis around institutional features (electoral rules, democratic age, and economic development), supply-side party characteristics, and election-level structural features.

Figure 1. Coordination failure over time



(a) Coordination failure rates, full 1940–2020 period.



(b) 10-year rolling mean of coordination failure.

Notes: Panel (a) plots mean weak coordination failure rates at the country level over the full 1940–2020 period, using the combined V–Party/Manifesto sample. Countries are shaded according to the legend; grey indicates missing coverage. Panel (b) reports the 10-year rolling mean of three series, any coordination failure (weak or strong), only weak failure, and strong failure only, across the balanced panel of countries with at least one election per decade. The number of countries included in each decade window is shown above the plot. Coordination failures are constructed from constituency-level vote shares reallocated under spatial-voting assumptions using ideological distances from V–Party (anti-pluralism and populism indices) and, where V–Party is unavailable, from the Manifesto Project (RILE index). See Section 2 for details.

3.1 Institutional and Economic Drivers

We focus on four drivers that map to canonical mechanisms in the voting literature. Electoral rules shape strategic incentives: plurality concentrates competition but generates severe "spoiler" incentives when more than two viable candidates persist, whereas runoffs and ranked-choice rules mitigate vote-splitting through second-stage or transfer mechanisms (Duverger, 1954; Cox, 1997; Myerson and Weber, 1993). Fragmentation mechanically raises the dimensionality of the coordination problem, making inference about relative viability harder (Cox, 1997; Laakso and Taagepera, 1979). Years of democratic experience proxy for party maturation and voter learning, though information frictions and expressive motives may persist in mature democracies, motivating an empirical assessment rather than an a priori sign restriction (Besley and Prat, 2006; Pons and Tricaud, 2018). Finally, GDP per capita proxies for state capacity and the information environment (education, media supply, information acquisition), which may ease coordination if voters have better access to credible electoral information (Gentzkow et al., 2011).

Electoral system. Figure 2 reports mean coordination failure rates across four electoral system types. Throughout, note that we construct pairwise comparisons from first-round vote shares only, so rates for two-round systems capture first-stage miscoordination alone. With this caveat, plurality systems exhibit the highest mean failure rate, with more than 25% of elections showing coordination problems; ranked-choice and mixed systems follow at 20–25%, and runoff systems fall just below 20%. The same ordering holds for strong failures, indicating that institutional design shapes first-round coordination behavior.

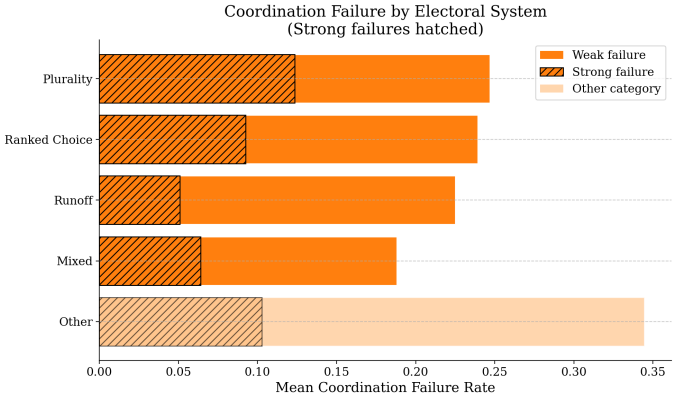
The comparatively low first-round failure rates for runoff systems are consistent with the canonical prediction that two-round institutions reduce the cost of deserting a non-viable option, since voters can express a first-round preference without sacrificing influence over the final outcome (Cox, 1997; Duverger, 1954).[4] Because our measure is constructed from first-round data, however, these rates should not be read as evidence that runoff systems generate fewer coordination difficulties overall. The theoretical and empirical literature suggests that two-round institutions do not eliminate coordination problems but rather *relocate* them: the second-round

[4]This pattern also accords with Piketty (2000)'s argument that two-round systems facilitate more efficient preference communication: first-round expressive votes are less costly when voters retain full influence over the decisive second round, reducing the trade-off between sincere signaling and strategic coordination that drives failures under plurality rule.

safety net encourages expressive first-round voting and lowers barriers to party entry, generating a divided-majority problem specific to multi-round institutions (Bouton, 2013; Bouton and Gratton, 2015).

Consistent with this, Bordignon et al. (2016) exploit quasi-experimental variation across Italian municipalities and find that runoff systems induce greater party entry and more dispersed first-round vote shares, precisely the upstream conditions our cross-national evidence associates with elevated coordination failure. A full accounting of both rounds would therefore likely attenuate or reverse the apparent advantage of runoffs visible in Figure 2.

Figure 2. Coordination failure rates by electoral system, 1940–2020



Notes: The figure plots mean coordination failure rates by electoral system type for the period 1940–2020. Solid bars represent the mean weak coordination failure rate; hatched areas represent the subset of elections that also qualify as strong coordination failures (i.e., the Condorcet loser wins). Electoral systems are classified into five categories: plurality (first-past-the-post), ranked-choice (including alternative vote and single transferable vote), runoff (two-round systems), mixed (combined proportional and plurality tiers), and other (all remaining systems used in multiparty elections). Failure rates are computed as the share of constituency-elections within each system type in which a coordination failure is observed. The sample is restricted to multiparty elections with at least three competing parties. See Section 3 for discussion.

Years of democracy and GDP per capita. Table 4 reports estimates of how coordination failure correlates with democratic age, party-system fragmentation, and economic prosperity, using constituency and year fixed effects with two-way clustered standard errors (country and year). Fragmentation enters with a robust positive association: across OLS specifications (cols. 1–3), the coefficient on the log number of parties is approximately 0.27–0.28 ($p \leq 0.10$), and the corresponding logit models (cols. 4–6) yield log-odds coefficients of roughly 2.29–2.32 ($p \leq 0.05$). Elections with more competing parties are thus systematically more susceptible to

coordination breakdowns.

Log GDP per capita (PPP), by contrast, enters with a negative sign but is statistically indistinguishable from zero in both the OLS (approximately -0.13) and logit models (approximately -1.8 to -1.9), indicating no detectable partial correlation between economic prosperity and coordination failure once fixed effects are included. Years of democracy shows a small but precisely estimated *positive* association with coordination failure (OLS: 0.001; logit: 0.006–0.009, significant at conventional levels), suggesting that greater democratic age does not correspond to fewer coordination problems.

Once a broader set of election-specific controls is added, however, the associations for both democratic age and GDP lose statistical significance, indicating that election-specific factors, rather than institutional longevity or income levels, primarily account for the emergence of coordination failures. The next section turns to these drivers.

Table 4. Effect of GDP per capita and years of continued democracy on coordination failure.

	Weak Coordination Failure											
	No supply-side controls						**With supply-side PCs**					
	OLS			Logit			OLS			Logit		
Years of continued democracy	0.001**		0.001***	0.014***		0.017***	0.002		0.001	−0.001		−0.006
	(0.000)		(0.000)	(0.004)		(0.002)	(0.001)		(0.001)	(0.020)		(0.022)
# Parties (log)	0.264*	0.237	0.257*	2.535*	2.126*	2.314*						
	(0.146)	(0.149)	(0.147)	(1.313)	(1.218)	(1.208)						
GDP per capita (log)		−0.173	−0.171		−2.846*	−2.942*		0.534	0.501		6.532	6.856
		(0.116)	(0.123)		(1.700)	(1.685)		(0.629)	(0.637)		(5.597)	(5.782)
Election characteristics (PCs)	—						Yes					
Num. obs.	35349	30245	30245	17067	13742	13742	17869	17869	17869	6478	6478	6478
Num. groups: cst_n	12543	11572	11572	3364	2684	2684	8140	8140	8140	1612	1612	1612
Num. groups: year	70	65	65	68	63	63	44	44	44	41	41	41
R^2 (full model)	0.493	0.482	0.483				0.538	0.538	0.538			
R^2 (proj model)	0.004	0.004	0.006				0.014	0.014	0.014			
Adj. R^2 (full model)	0.212	0.158	0.159				0.146	0.146	0.146			
Adj. R^2 (proj model)	0.004	0.004	0.006				0.013	0.013	0.013			

Notes: The dependent variable is an indicator equal to one if a weak coordination failure occurs in the constituency-election (i.e., the Condorcet winner, if it exists, does not win). OLS estimates (cols. 1–3 and 7–9) and logit log-odds estimates (cols. 4–6 and 10–12) are reported. All models include constituency (*cst_n*) and year fixed effects. Columns 1–6 exclude supply-side controls; columns 7–12 add the first ten principal components of the election-level covariate matrix, which together explain approximately 85% of the variance in the full set of election-characteristic controls. Principal components are computed from a pairwise-complete correlation matrix and projected to the nearest positive semidefinite form when needed; component scores for each observation use only its observed variables (no imputation). Standard errors are clustered two-way at the country and year levels. GDP per capita (PPP) is from Eurostat/OECD/World Bank (via Our World in Data) extended with Gapminder. Years of continued democracy are from Boix et al. (2013). Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.2 reports the corresponding results for strong coordination failures; the patterns are virtually unchanged.

Sensitivity to the reallocation rule. Our failure measures assume that when non-competing parties are removed from a counterfactual pairwise contest, all of their supporters turn out and transfer to the ideologically closest remaining party. Because this full-transfer, closest-takes-all rule maximizes the leverage of reallocated votes, and is not directly testable, we recompute the failure rates while relaxing it, holding the ideology sources, distance metric, and sample fixed (Figure 3). Allowing a fraction of transferring voters to abstain lowers the rates smoothly and in the expected direction: at a uniform abstention rate of 30%, weak failures fall from roughly 26–28% to 15–18% and strong failures from about 10% to 3–4% across the two ideology sources, and a distance-based abstention rule and a probabilistic (logit) transfer rule yield the same pattern; varying the tie-breaking rule has no material effect. Coordination failures thus remain a pervasive feature of multiparty competition throughout, but the headline 20–30% figure should be read as the upper end of a range that contracts once eliminated-party voters are assumed to transfer less fully or less decisively. The full set of analysis is available in Section B.

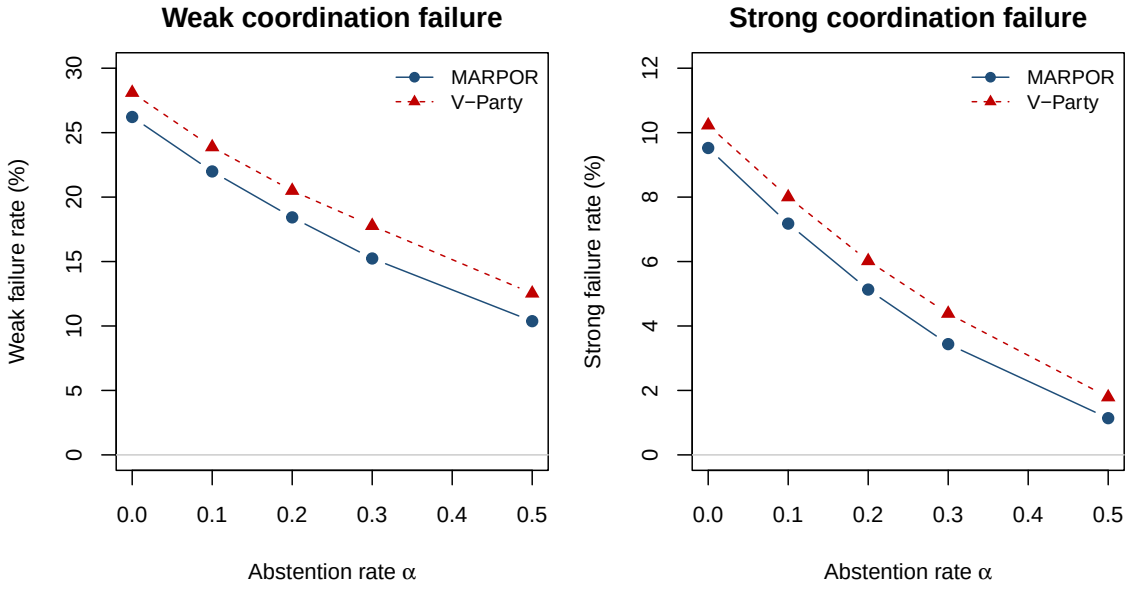


Figure 3. Coordination-failure rates under uniform abstention.

Notes: Weak (left) and strong (right) coordination-failure rates as a function of the uniform abstention rate α, the fraction of would-be transferring voters who abstain rather than switch to a surviving party in each counterfactual pairwise contest. $\alpha = 0$ is the paper's full-transfer baseline. Solid navy: MARPOR (RILE); dashed red: V–Party. Rates fall smoothly in α under both ideology sources but remain substantial across the empirically plausible range.

3.2 Supply-Side Drivers

Who loses from coordination failure. Figure 4b reports conditional OLS estimates with constituency and year fixed effects and standard errors clustered by party and year. Several traits are positively associated with ending up on the losing side: opposition parties and those labeled as pariah are more likely to lose when voters miscoordinate,[5] as are parties emphasizing culturally liberal positions (e.g., support for LGBT rights or immigration) and parties that use demonizing rhetoric toward opponents. By contrast, proxies for party size and legislative strength (vote share and lower-chamber seats) are strongly negatively correlated with losing, indicating that larger, more established parties are comparatively insulated from the downside risks of miscoordination. Viewed alongside classic coordination theories (Duverger, 1954; Cox, 1997), these patterns suggest that fragmented or stigmatized opposition blocs are particularly vulnerable to coordination breakdowns.[6]

Who benefits from coordination failure. Figure 4a examines parties that benefit from coordination failures. Organizational capacity is positively associated with benefiting: parties with more formalized candidate selection, stronger local embeddedness (affiliations with social organizations, grassroots activity, and local branches), and, in some cases, clientelistic linkages are more likely to gain when voter coordination breaks down (Larreguy et al., 2016; Duarte et al., 2025). We also find a positive association for right-wing economic positioning, whereas programmatic profiles centered on welfare expansion or minority rights correlate negatively with benefiting. Taken together with the results on losing parties, this evidence suggests that coordination failures reallocate votes toward relatively smaller but organizationally capable parties, rather than systematically advantaging large incumbents.

[5]This is consistent with evidence that expressive motives and social-image concerns affect turnout and vote choice (Pons and Tricaud, 2018; DellaVigna et al., 2017).

[6]This vulnerability is compounded by evidence that strategic behavior is itself unevenly distributed across voter types. Eggers and Vivyan (2020), using British election data and a scalar measure of individual strategic incentives, find that older and wealthier voters vote significantly more strategically than younger and lower-income voters, independently of differences in turnout. If the supporters of opposition and culturally liberal parties are disproportionately drawn from demographic groups with weaker strategic propensities, as patterns of political cleavage in both established and developing democracies suggest, their comparative reluctance to defect from non-viable options would compound the organizational disadvantages documented above.

Strong coordination failures. Restricting attention to strong coordination failures (Figures 4d and 4c), the patterns become more pronounced. On the beneficiary side, leader-centric parties, those with dense local organization, and parties scoring higher on anti-pluralism are more likely to gain. On the losing side, opposition parties and those emphasizing culturally liberal stances (immigration and LGBT rights) are especially likely to be harmed. The positive association between anti-pluralism and benefiting from strong miscoordination is consistent with research identifying anti-pluralism as a core component of populist strategies that perform well under fragmented competition and voter uncertainty (Mudde, 2004; Mueller, 2016; Norris and Inglehart, 2019). These relationships reflect conditional correlations rather than causal effects, but their structure accords with established theoretical expectations regarding how organizational resources, programmatic orientations, and party-system positioning shape exposure to strategic-voting frictions (Cox, 1997; Panebianco, 1988; Tavits, 2012).

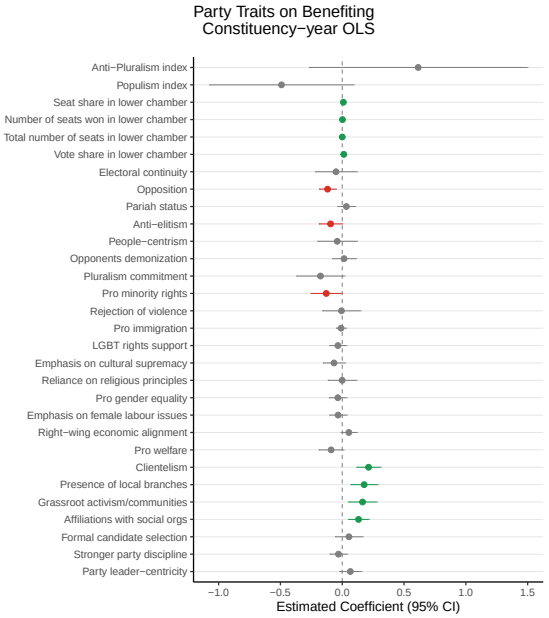
3.3 Election Characteristics

Coordination failure should be more likely when voters face a higher-dimensional problem and weaker common knowledge about which alternatives are locally viable. Figures 5a (OLS) and 5c (logit) summarize how election-level characteristics correlate with failure, conditional on constituency and year fixed effects. Indicators of party-system fragmentation, such as higher effective numbers of parties (Laakso–Taagepera index) and greater entropy of votes or seats, are positively associated with coordination failure, whereas concentration (higher HHI) is negatively associated: as the viable choice set expands and support disperses, voters must infer viability from noisier signals and strategic incentives become harder to internalize (Duverger, 1954; Cox, 1997; Laakso and Taagepera, 1979). Fragmentation is especially relevant here because it is not purely a slow-moving cultural feature of party systems: it can respond to relatively modest institutional or administrative shifts that affect candidate entry and the supply of competitors.[7]

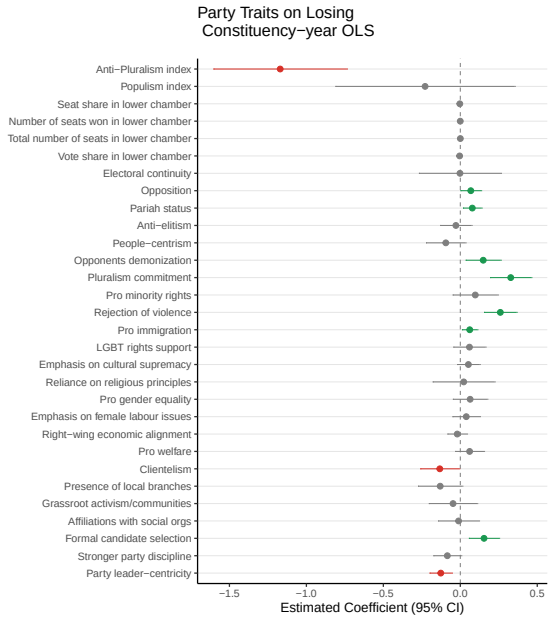
Electoral novelty and churn also matter. Elections with larger shares and vote shares of new or renamed parties are more prone to coordination failures, whereas a greater presence and vote share of continuing parties correlate with fewer failures.

[7]Consistent with this view, evidence from India shows that lowering barriers to entry increases the number of candidates and electoral fragmentation (Kapoor and Magesan, 2018), precisely the upstream conditions under which our framework predicts higher rates of coordination failure.

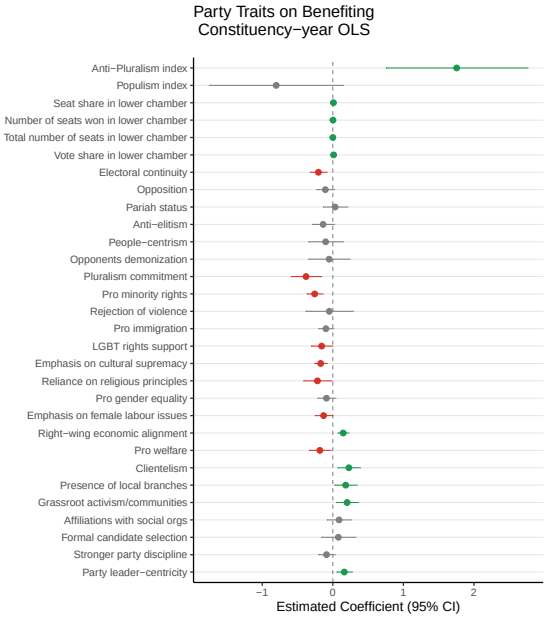
Figure 4. Winners and losers from coordination failure on party characteristics



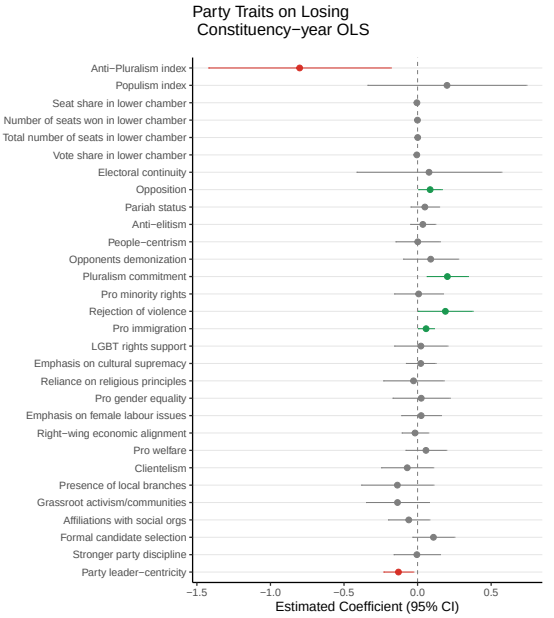
(a) Beneficiary from coordination failure (OLS).



(b) Loser from coordination failure (OLS).



(c) Beneficiary from *strong* coordination failure (OLS).



(d) Loser from *strong* coordination failure (OLS).

Notes (*all panels*): Party traits are drawn from V–Party v2. Each regression controls for $\log(\text{number of parties} + 1)$ and includes constituency (cst_n) and year fixed effects. Coefficients are reported in standardized units where the trait is continuous. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

This is consistent with research linking new-party entry and success to heightened electoral uncertainty and volatility (Harmel and Robertson, 1985; Mainwaring and Zoco, 2007; Mair, 1997), and it is through exactly these observed features of the party system that strategic entry or exit would operate.

Measures of ideological structure similarly point to greater risks of miscoordination when party systems are more dispersed. Wider dispersion and separation on the left–right dimension (reflected in larger standard deviations and interquartile ranges of party positions, greater centroid distances, and higher maximum outlier scores) are associated with more failures, whereas greater overlap in party platforms correlates with fewer failures. These patterns accord with theories of polarized pluralism, in which centrifugal competition and broader interparty distances make it harder for voters to coordinate on focal alternatives (Sartori, 1976; Dalton, 2008; Benoit and Laver, 2006).

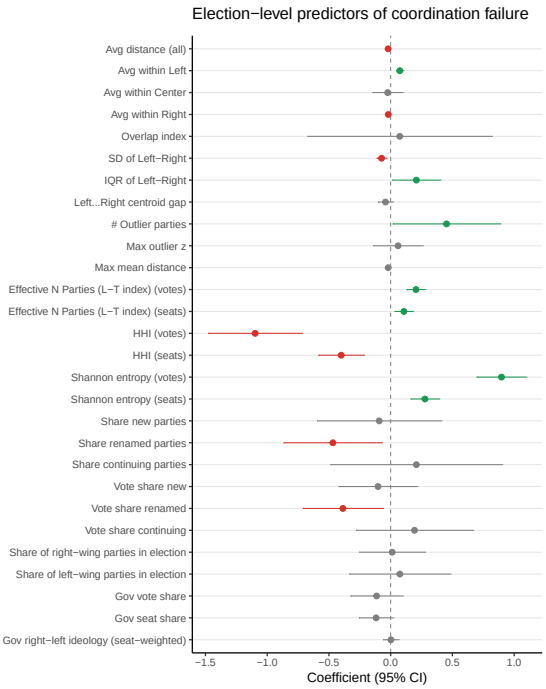
These patterns also speak to the validity of our constructed pairwise preferences: were they driven by mechanical features of the coding scheme or arbitrary spatial assumptions, we would expect weak or inconsistent relationships between ideological structure and coordination failure, rather than the correlations spatial-voting theory predicts.

Strong coordination failures. Focusing on strong coordination failures (Figures 5b and 5d), the associations generally intensify. Fragmentation measures (effective number of parties and entropy) show stronger positive correlations, while concentration (HHI) exhibits more pronounced negative correlations. Similarly, elections marked by greater party-system churn and wider ideological dispersion display larger estimated associations with strong failures. These results suggest that the conditions most conducive to severe miscoordination are those that simultaneously expand the viable choice set, introduce organizationally or programmatically novel competitors, and widen ideological distances, precisely the environments in which strategic voting becomes most cognitively and informationally demanding (Cox, 1997; Sartori, 1976; Laakso and Taagepera, 1979).

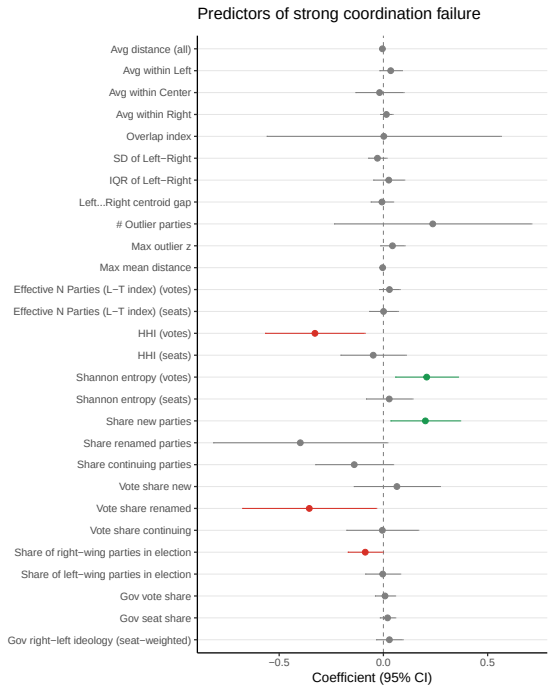
3.4 Link to the Experiments

Coordination failures are thus most prevalent precisely where the informational burden on voters is highest, consistent with theories in which outcomes depend not only on preferences but on information about competitiveness and pivotality (Feddersen

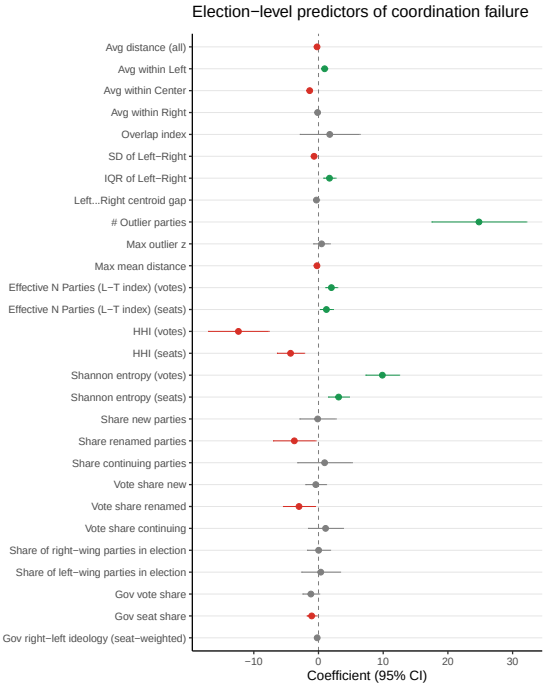
Figure 5. Effects of election characteristics on the probability of coordination failure



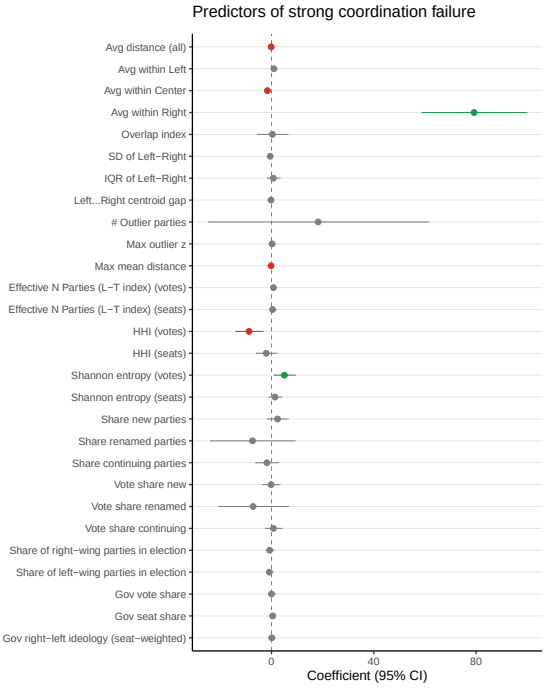
(a) OLS: coordination failure (weak).



(b) OLS: *strong* coordination failure.



(c) Logit: coordination failure (weak).



(d) Logit: *strong* coordination failure.

Notes (all panels): Fragmentation is measured by the Laakso–Taagepera effective number of parties (vote- and seat-weighted), the Herfindahl–Hirschman index (HHI), and Shannon entropy. Ideological dispersion measures (SD, IQR, centroid gap, outlier scores) are computed from RILE scores (Manifesto Project) or V–Party indices, depending on the available source for each election. All regressions include constituency (*cst_n*), year, and source fixed effects. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

and Pesendorfer, 1996; Granzier et al., 2023). Observational data alone, however, cannot separate whether failures arise because voters lack reliable constituency-level viability information or because expressive motives dominate even when viability is understood (Pons and Tricaud, 2018). Section 4 provides a causal test of the information channel.

4 Experimental Evidence

4.1 Intervention

Having established the prevalence of coordination failure and documented its correlates, we turn to testing whether polling information can reduce its incidence, using a series of experimental studies conducted in India around the time of state elections. India is an especially useful setting: it is the world's largest democracy with rising voter polarization; it operates under first-past-the-post, an electoral system prone to coordination failures; its estimated coordination-failure rate of about 30% is comparable to rates in the UK, Canada, and Australia; and state elections frequently involve electorally relevant third parties and substantial shares of undecided voters.[8] A further reason India is well suited to isolate the information mechanism is that its media environment constrains voters' access to independent political information: India is ranked 151st in the 2025 World Press Freedom Index (Reporters Without Borders , RSF).

State elections select members of the Legislative Assembly (Vidhan Sabha) in single-member districts by plurality, and typically feature multiparty competition, strong regional parties, and shifting national–state alliances, producing complex strategic environments in which coordination failures are substantively meaningful (Palshikar et al., eds, 2017).

We conducted four independent experiments around high-stakes first-past-the-post elections in India: the 2024 general (Lok Sabha) election in Uttar Pradesh and Telangana, and the 2025 legislative assembly elections in Delhi and Bihar. All the elections share the same electoral features for our purposes. Together, these states have a combined population of roughly 445 million.

Each experiment comprises two sequential telephone surveys: a baseline survey conducted earlier in the campaign, and a follow-up survey administered in the week

[8] Limited strategic defection in India under FPTP has been documented in survey research (Heath and Ziegfeld, 2022).

before the election. Across all four experiments, more than 4,000 respondents were recruited via random phone sampling and randomly assigned to one of two equally sized groups: a treatment group that received additional polling information in the follow-up survey, and a control group that did not. The polling information provided to treated respondents was constructed from self-reported voting intentions collected at baseline. While national and state-level opinion polls are published in India (subject to Election Commission restrictions on timing), constituency-level vote-intention estimates are essentially never publicly available for these races. Our treatment therefore provides information respondents could not obtain elsewhere at the level relevant for strategic voting in a single-member district. The experimental structure was held constant across states, with minor adjustments to state-specific modules, particularly those assessing political knowledge and voting intentions, to reflect local political contexts. The full set of instructions is available on this link.

Surveys and treatment. The baseline survey covered six modules: demographics (age, gender, religion, caste, education, income, occupation); political knowledge (current and previous Chief Ministers, recognition of state parties); political preferences (ratings, rankings, and voting intentions for the major parties); second-order beliefs (expected voting behavior in the respondent's constituency, plus frequency of political discussion); issue importance (caste, Hindutva, a party's realistic chance of winning, community voting behavior, candidate integrity, support for tribals and farmers, employment); and stated interest in receiving polling information. The follow-up survey repeated every module except demographics. Treated respondents first received a concise summary of the distribution of voting intentions in their constituency: the top-two parties, their estimated vote shares, and the percentages for minor parties and undecided voters, generated from baseline responses and functioning as an internally constructed poll. Control respondents received nothing. Comparing post-treatment responses therefore identifies the causal effect of poll information on voting intentions, beliefs about others' preferences, and the weight assigned to different electoral considerations.

Attrition and Balance Checks Of the 4,712 respondents who completed the baseline survey, 4,026 (85.4%) completed the follow-up, leaving an overall attrition rate of 14.6% (Table C.15, Panel A). Because our polling treatment is assigned and delivered during the follow-up call, attrition occurs entirely *prior* to randomization: no

attriter is assigned to a treatment arm, so differential attrition across arms cannot arise by construction and cannot bias our estimated treatment effects.

Attrition does, however, vary sharply across states, from 6.8% in Telangana to 36.6% in Delhi. Comparing baseline characteristics of retained and attrited respondents (Panel B), attriters are somewhat less likely to be male, Hindu, of a scheduled tribe, or engaged in farming, and somewhat more likely to be salaried; baseline political engagement—including whether a respondent had already formed a vote preference and their stated interest in polls—is balanced. A linear-probability model shows that these differences are driven largely by cross-state composition rather than within-state selection: the baseline covariates jointly predict attrition when states are pooled ($F = 6.73$, $p < 0.001$) but are jointly insignificant once state fixed effects are included ($F = 1.58$, $p = 0.091$, Panel C), with only scheduled-caste status remaining a weak predictor. Attrition thus poses no threat to internal validity; it implies only that our estimates are most representative of the lower-attrition states (Telangana, Uttar Pradesh, and Bihar).

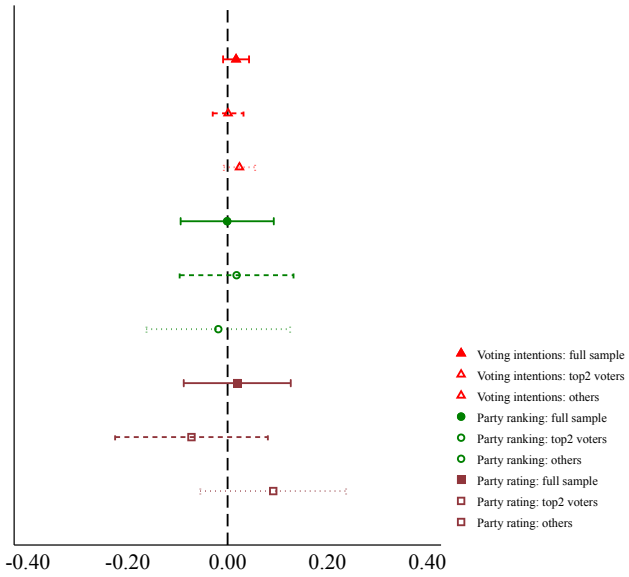
Table C.16 confirms that randomization worked as intended. Across gender, age, education, caste, and profession, treated and control respondents look virtually identical, with differences typically below two percentage points. The only covariate approaching significance is the share of Hindu respondents, about 2 percentage points higher in the treatment group ($p < 0.10$), consistent with random chance.

4.2 Results

Effects of polls on voting intentions and ratings. Figure 6 shows no evidence that polls persuade respondents by shifting their relative evaluations of parties. Across specifications, the *Treated* $\times$ *Post* coefficients for party-specific voting intentions are very close to zero and statistically insignificant, both in the full sample and when separating respondents whose pre-treatment favorite is a top-two party from those whose preferred party lies outside the top two (columns 1–3). Parties' ordinal positions in respondents' rankings (columns 4–6) and their affective ratings (columns 7–9) likewise show no systematic treatment effects. The interaction terms with pre-treatment party preference are small and imprecisely estimated, and the triple interactions (*Treated* $\times$ *Fav top-2 party* (*pre*) $\times$ *Post*) are statistically indistinguishable from zero. These estimates allow us to rule out moderate shifts in rankings or ratings for either top-two or minor parties. This pattern is consistent with coordination-without-persuasion being unavailable as a channel here: the

poll neither persuades (rankings and ratings are unchanged) nor triggers strategic reallocation (intended votes are unchanged). The only margin on which viability information moves behavior is among voters who had not yet formed an intention

Figure 6. Treatment effects on voting intentions and preferences



Notes: The figure plots difference-in-differences (*Treated* × *Post*) coefficients and 95% confidence intervals from panel regressions of three outcomes (party-specific voting intentions, party ranking (ordinal position, 1–5), and party affective rating (1–5)) on a treatment indicator, a post indicator, their interaction, and party × voter, party × time, and voter × time fixed effects. Separate estimates are reported for the full sample, for respondents whose pre-treatment favourite party is among the top-two parties ("top-2 voters"), and for all other respondents ("others"). The sample pools all four state experiments (Bihar, Delhi, Telangana, Uttar Pradesh). Standard errors are clustered at the respondent level. Point estimates close to zero indicate no systematic shift in voting intentions, rankings, or ratings following exposure to constituency-level poll information.

Putting the results in perspective. To assess the economic significance of these null effects, we quantify the maximum plausible effect sizes implied by our confidence intervals. For party-specific voting intentions, the *Treated* × *Post* coefficients remain close to zero across specifications, and the corresponding 95% confidence intervals rule out changes larger than roughly 1.5 percentage points, less than 4% of a standard deviation and an order of magnitude smaller than the 3–5 p.p. turnout and vote-choice shifts commonly found in field experiments that manipulate perceived electoral closeness (Gerber et al., 2020; Bursztyn et al., 2024). For party rankings (1–5 scale), estimated effects never exceed 0.07 positions (under 0.06 standard deviations); for affective ratings (also 1–5 scale), the upper bounds of the confidence

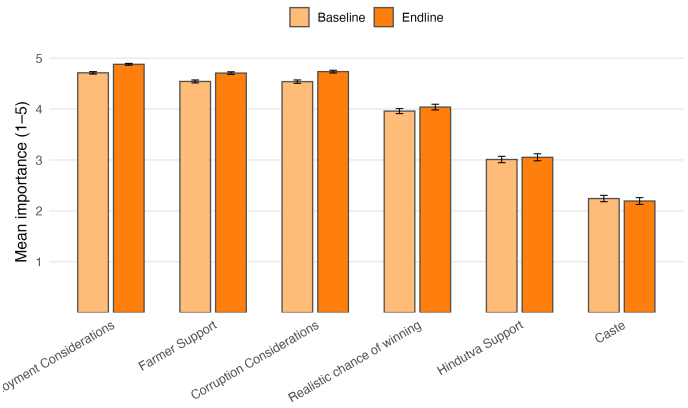
intervals are around 0.20 points (under 0.16 standard deviations). These magnitudes fall well below the 0.25–0.40 standard-deviation preference shifts observed in laboratory settings where viability cues successfully induce strategic defection (Agranov et al., 2018; Kittel et al., 2014). Taken together, these comparisons confirm that our experiment yields a precise null: even sharply targeted, constituency-specific polling information does not generate economically meaningful changes in intended votes, party rankings, or affective ratings.

The role of voting motives. Figure 7 shows how respondents weight different considerations when deciding how to vote: party ideology, candidate quality, coethnic representation, local development promises, and the desire to support the eventual winner. Notably, concerns about "wasting" one's vote do not emerge as a dominant consideration, nor do they become substantially more important after receiving poll information. Table 5 confirms that there is no treatment-effect heterogeneity between voters who regard avoiding a wasted vote as a dominant consideration and those who do not. Respondents' baseline behavior already reflects viability: most support for top-two parties is in place before treatment, and expectations about others' votes are concentrated on the leading parties. Whatever coordination exists is thus embedded in stable prior intentions rather than produced by marginal information.

Effects of polls on undecided voters. Table C.22 examines treatment effects among respondents who were unsure of their vote choice at baseline. The post coefficient in column (1) (-0.749) indicates a large overall reduction in the probability of non-voting among initially unsure respondents, and the treatment–post interaction (-0.012), though imprecisely estimated, corresponds to roughly a 1.2-percentage-point additional increase in turnout for those who received the poll. Conditional on voting (column (2)), the treatment–post interaction is 0.070, indicating a 7-percentage-point higher probability of choosing a top-two party among treated respondents at endline.

This estimate is best read as a *lower bound*. Indecision is a transient state: voters with no baseline preference are precisely those whose choices crystallize naturally through campaign exposure, social interaction, and media coverage between the two waves. Because this resolution occurs in both arms, it compresses the treatment–control gap at endline relative to the effect measured at the moment of receipt, and

Figure 7. Reasons behind voting decisions (Bihar only)



Notes: The figure plots mean importance scores (scale 1–5) for five voting considerations (employment prospects, farmer support, corruption concerns, realistic chance of winning, and Hindutva ideology) measured at baseline (lighter bars) and at endline (darker bars) in the Bihar experiment. Respondents assigned importance on a five-point scale from "not at all important" (1) to "most important" (5). Capped lines indicate 95% confidence intervals. The sample includes all Bihar respondents who completed both surveys. Differences between baseline and endline bars reflect within-respondent changes across the two survey rounds; treatment–control differences are reported in Table 5.

Table 5. Treatment effect: heterogeneity by vote-count importance (Bihar).

	Voting Intentions			Party Ranking			Party Rating		
	1	2	3	4	5	6	7	8	9
Treated × Post × Top-2 Party (Pre)	−0.045	0.049	−0.063	−0.066	0.151	−0.252	0.327	−0.173	0.615*
	(0.060)	(0.089)	(0.062)	(0.268)	(0.333)	(0.383)	(0.246)	(0.367)	(0.318)
Post × Top-2 Party (Pre) × Vote Count Importance	0.008	0.022	0.017	0.021	0.069	−0.014	0.012	−0.091	0.076
	(0.011)	(0.016)	(0.011)	(0.046)	(0.064)	(0.061)	(0.045)	(0.074)	(0.055)
Treated × Post × Top-2 Party (Pre) × Vote Count Importance	0.014	−0.018	0.020	0.013	−0.025	0.052	−0.065	0.050	−0.135*
	(0.015)	(0.021)	(0.015)	(0.062)	(0.079)	(0.088)	(0.060)	(0.091)	(0.078)
Sample	Full	Top-2 Voters	Others	Full	Top-2 Voters	Others	Full	Top-2 Voters	Others
Dep. Var. Mean	0.111	0.111	0.111	3.080	3.080	3.080	3.344	3.344	3.344
Party × Voter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Voter × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Num. obs.	20244	7140	13104	18210	6798	11412	17498	6231	11267
R^2 (full model)	0.712	0.759	0.718	0.784	0.805	0.776	0.783	0.808	0.773
R^2 (proj model)	0.001	0.001	0.003	0.000	0.001	0.000	0.001	0.001	0.001
Adj. R^2 (full model)	0.135	0.274	0.151	0.271	0.369	0.222	0.168	0.263	0.124
Adj. R^2 (proj model)	0.000	−0.001	0.002	−0.000	−0.001	−0.001	−0.000	−0.001	0.000

Notes: The table reports triple-interaction difference-in-differences estimates from the Bihar experiment. The three outcome groups are party-specific voting intentions (cols. 1–3), ordinal party rankings on a 1–5 scale (cols. 4–6), and affective party ratings on a 1–5 scale (cols. 7–9). Within each group, columns report estimates for the full sample, for respondents whose pre-treatment favourite party is among the top two ("Top-2 voters"), and for all other respondents ("Others"). The variable *Vote Count Importance* measures, on a scale from 1 to 5, the importance respondents attached to making their vote count, as elicited at baseline. All models include party × voter, party × time, and voter × time fixed effects. Standard errors are clustered at the respondent level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

the attenuation grows with the length of the inter-survey window—likely substantial in the high-salience environments we study.

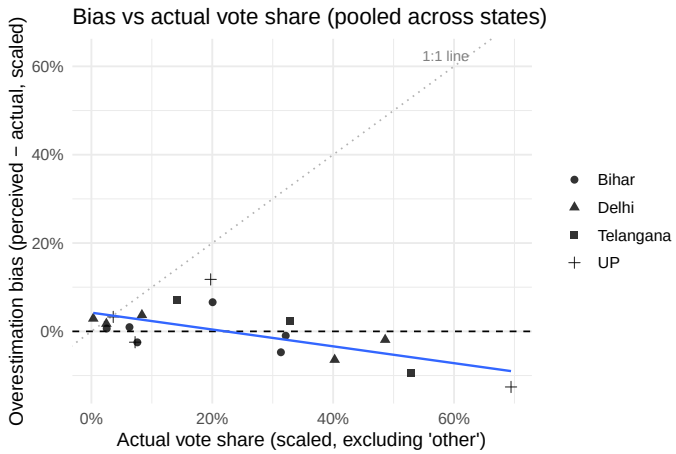
Second-order beliefs. We also examine whether the limited treatment effects on voting intentions could stem from the intervention failing to correct respondents' beliefs about who is likely to win. If respondents already hold accurate expectations about the leading parties, additional polling information would be unlikely to shift their choices. Table C.18 rules this out. Across all four states, respondents correctly anticipated the *ranking* of the main competitors but were systematically wrong about the *levels* of support, perceiving substantially more competition than actually existed (Table C.17; Figures 8 and 9). In every state, the distribution of respondents' own voting intentions differs statistically from their reported second-order beliefs at baseline. This result contrasts with Heath and Ziegfeld (2022), who find that voters in Uttar Pradesh in the 2017 election overestimated the chance that their own party would win, a form of motivated rather than competitive misperception.

In Table C.19, the coefficients on the *Treated* $\times$ *Top-2 party* $\times$ *Post* interaction are positive across specifications (0.99, 1.09, and 0.73 percentage points, respectively), although imprecisely estimated and not statistically distinguishable from zero at conventional levels. While we cannot claim a precisely estimated shift in perceived vote shares, the pattern of point estimates is more consistent with modest belief updating in favor of the leading parties than with systematic skepticism that pushes perceptions away from the poll. Importantly, we can rule out large negative effects: the confidence intervals are inconsistent with voters systematically revising their beliefs in the opposite direction of the treatment information.

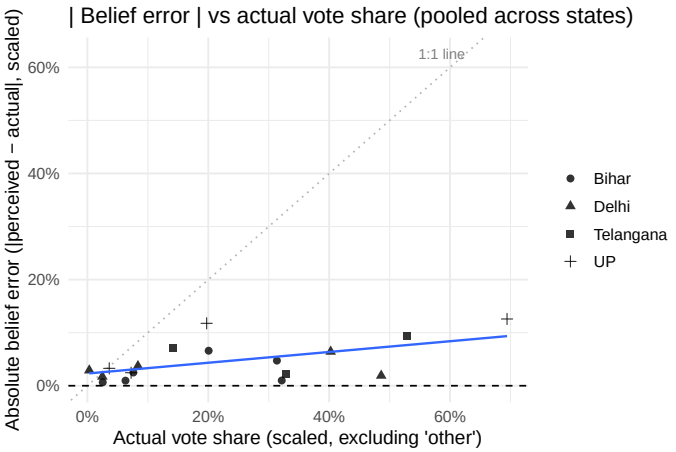
Alternative mechanisms: trust and informativeness. A natural alternative to our preferred expressive-voting interpretation is that the null effects arise because respondents either did not trust the polls or found them uninformative, especially given that they were constructed from baseline intentions rather than an independent polling source. We cannot directly measure trust in our internally generated polls and therefore cannot completely rule out such mechanisms. However, several pieces of evidence suggest that distrust or non-informativeness are unlikely to be the primary drivers.

First, there is substantial stated demand for poll information. Figure 10a shows that 75% of respondents report being interested in receiving polling information,

Figure 8. Pooled comparison between perceived and actual vote shares at baseline



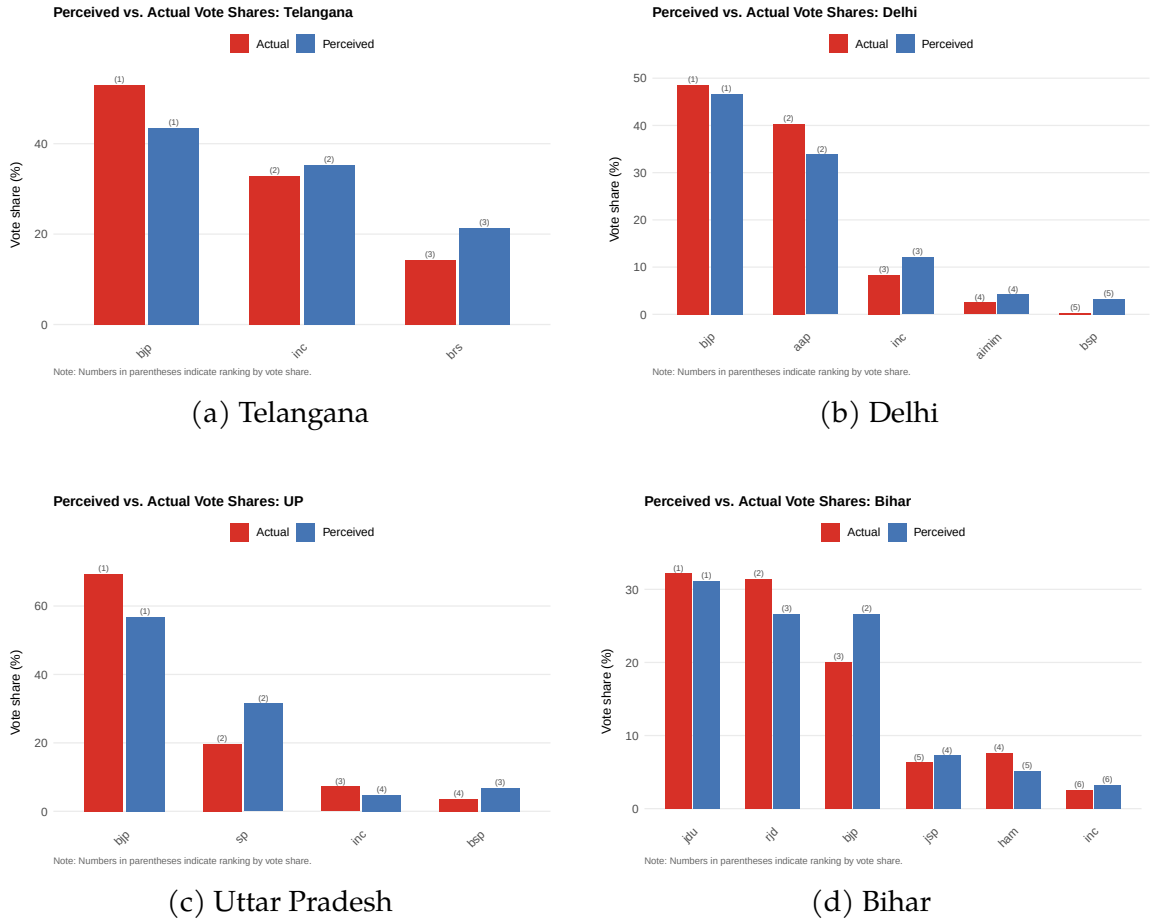
(a) Bias vs. actual share (pooled).



(b) Absolute error vs. actual share (pooled).

Notes (all panels): Perceived vote shares are constructed from respondents' answers to the second-order beliefs question at baseline ("Out of 100 voters in your constituency, how do you expect others to vote?"). Actual vote shares are taken from official election results for the corresponding constituency. All shares are rescaled to exclude votes for parties outside the set listed in the survey instrument. State indicators: Bihar (●), Delhi (▲), Telangana (■), Uttar Pradesh (+). The dashed horizontal line in Panel (a) at zero indicates unbiased beliefs; the dotted diagonal is the 45-degree line. In Panel (b), the x-axis plots the party's actual vote share (scaled) and the y-axis plots the absolute belief error, defined as $|\text{perceived} - \text{actual}|$. The fitted line is a linear regression pooled across all four states. Points above the diagonal indicate that perceived shares diverge from actual shares by more than the actual share itself.

Figure 9. State-level comparison between perceived and actual vote-share distributions at baseline. Ranks are annotated.



(a) Telangana

(b) Delhi

(c) Uttar Pradesh

(d) Bihar

Notes (*all panels*)*:* Perceived vote shares are computed as the mean second-order belief reported by baseline respondents in each state. Actual vote shares are from official election results, scaled to the set of parties listed in each state-specific survey. Ranks are annotated above each bar in parentheses. Discrepancies between perceived and actual ranks indicate systematic misprediction of party ordering.

and this interest is present among top-two voters, voters for other parties, and respondents without an expressed preference alike. This is difficult to reconcile with the view that voters wholesale dismiss poll information as irrelevant or untrustworthy. Figure 10b further shows that the share of respondents correctly identifying the top-two parties in their constituency is similar among those interested in polls and those who are not (around half in each group). Interest in polls is thus neither confined to a highly informed elite nor driven solely by the confused; it is widespread across informational types.

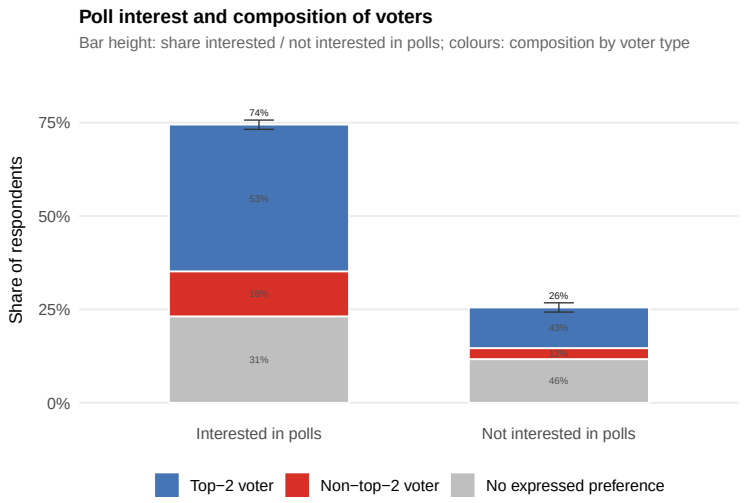
Second, the intervention had clear potential to be informative. At baseline, respondents systematically misperceived the *levels* of support for different parties: while they typically identified the correct top-two parties, they overestimated the competitiveness of non-top-two options and underestimated the leading parties' margins. Respondents who overestimated the vote share of non-top-two parties or misidentified the top-two competitors exhibit large and statistically significant changes in perceived support between baseline and follow-up (Tables C.20 and C.21), demonstrating that beliefs are responsive to new information. Our polls, constructed from a much larger sample of baseline intentions within each constituency than any individual voter could observe, summarize distributional information that is not trivially available to respondents. They could therefore meaningfully correct systematic overestimation of non-top-two support, especially for those whose baseline misperceptions were largest.

Voters therefore express strong demand for poll information and hold beliefs that are neither accurate nor unresponsive, yet show no detectable change in intentions, rankings, ratings, or stated criteria, even among those with the largest baseline misperceptions. This combination of belief updating and behavioral non-response is more consistent with voters who care about viability but also derive utility from expressive support than with voters who simply ignore the poll.

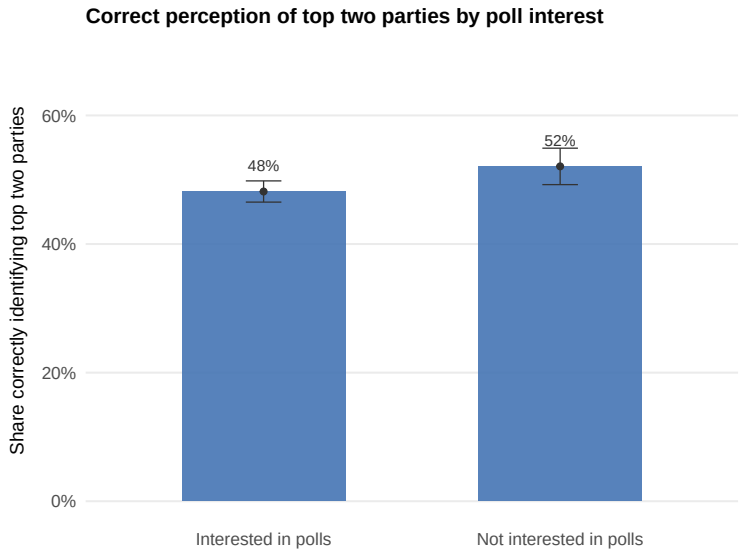
5 Implications

Interpreting the null through the model. Appendix A shows that poll-induced responses fall into three regions of the parameter space (Figure A.1). In Region I, the poll signal crosses a tipping point and low expressive attachment permits discontinuous equilibrium switching — the environment laboratory experiments construct by design. In Region II, polls update beliefs but leave behavior unchanged,

Figure 10. Poll interest and informational accuracy.



(a) Composition of respondents by interest in poll information.



(b) Correct identification of top-two parties by poll interest.

Notes (*Panel a*)*:* Stacked bar chart showing the composition of respondents by stated interest in receiving polling information (left bar: interested; right bar: not interested). Colors distinguish top-2 voters (those intending to vote for one of the top-two parties at baseline), non-top-2 voters, and respondents with no expressed preference. Bar heights reflect each category's share of the full sample; within-bar proportions reflect voter-type composition. Numbers inside bars are percentages. The sample pools all four state experiments. *Notes* (*Panel b*)*:* Bar chart showing the share of respondents who correctly identify the top-two parties in their constituency (defined as the two parties with the highest baseline vote-intention shares), separately for those who expressed interest in polls (left bar) and those who did not (right bar). Capped lines indicate 95% confidence intervals. The sample pools all four state experiments.

because few voters sit near the defection threshold and/or the poll reveals a less competitive race than expected. In Region III, priors are precise enough that polls convey nothing. Our experiments exhibit the exact signature of Region II: significant belief updating, null effects on intentions, rankings, ratings, and stated criteria — and, per the model, the poll content itself pushed against defection, since respondents underestimated the leaders' margins, so the information reduced perceived pivotality (Remark A.4).

External validity. Our intervention represents one type of viability information in a single country, so we do not claim that information cannot reduce miscoordination anywhere. India is, however, a deliberately demanding environment: strong partisan attachments, entrenched identities, viable third parties, and high-stakes single-member districts make strategic defection costly and expressive voting attractive. That precise, locally relevant viability signals fail to move intended votes or rankings here identifies meaningful boundary conditions on when information can change coordination behavior. The null effects are also consistent with Eggers and Vivyan's (2020) finding that the voters most susceptible to coordination failure, those with the strongest incentive to defect from a dominated option but the weakest propensity to act on it, are precisely the groups for whom informational nudges are least effective even when strategic incentives are unambiguous.

Implications for poll design and policy. Our results carry four practical lessons. First, context matters: in settings with strong partisan attachments and well-formed prior beliefs, low-cost polls alone are likely insufficient to shift voter behavior. Second, poll design matters: our polls conveyed simple vote-intention shares, but more diagnostically rich formats (such as pairwise matchups, Condorcet-winner identification, or information on candidate characteristics) might be more effective. Third, complementary interventions may be needed: polls are more likely to induce strategic coordination when combined with voter-education campaigns or endorsements from trusted figures that explicitly frame the strategic tradeoff. Fourth, targeting matters: our cross-national evidence suggests that coordination failures are most prevalent in fragmented party systems with weaker institutions, and these are likely the contexts where well-designed polls have the greatest potential to improve coordination. More generally, our results suggest that simple viability signals, top-two vote-intention numbers, may be too blunt to overcome expressive motives in many

real elections.

6 Conclusions

This paper has shown that voter coordination failures are neither rare aberrations nor artifacts of unusual elections, but a recurrent feature of multiparty competition: between 1940 and 2020, the Condorcet winner failed to win in 20–30% of multiparty constituency-elections across every continent, and in roughly one in ten the winner was the Condorcet loser. Failures are most common precisely where the classic incentives for strategic defection should be strongest—plurality and related systems—and where party systems are fragmented, volatile, and ideologically dispersed; their distributional consequences systematically disadvantage opposition, stigmatized, and culturally liberal parties while benefiting organizationally strong and locally embedded ones. Our four pre-registered experiments in India then tested whether low-cost viability information can mitigate these failures. The answer, for voters with established preferences, is no: constituency polls leave intended votes, party rankings, affective ratings, and stated decision criteria essentially unchanged despite modest belief updating, with detectable coordination only among the initially unattached.

Beyond these headline results, the paper makes two conceptual contributions that open paths for future theoretical work. First, distinguishing weak from strong failures shows that electoral miscoordination is not a single phenomenon but a graded outcome: some failures merely prevent a Condorcet winner from taking office, while others elevate a party dominated by every feasible alternative. By providing the first cross-national estimates of the incidence and correlates of strong failures, we invite formal work to treat coordination failure as a spectrum whose severity responds to identifiable features of the informational and party-system environment.

Second, the strong association between organizational capacity and benefiting from coordination failure—holding size constant—points to a mechanism absent from classic Duvergerian frameworks: the organizational production of coordination. Parties with dense local branches, grassroots networks, and formalized candidate selection can substitute organization for size in generating focality under uncertainty. Models of strategic coordination built on size-based incentives and informational shortcuts alone therefore miss an important dimension of real-world elections.

Two directions for future work follow. First, our cross-national measures show that coordination failures persist across decades and continents, yet we know little about which combinations of media environments, polling industries, and party organizations can shift the informational equilibrium we document. Second, we have examined short-run voter responses in a single, though paradigmatic, democracy. In equilibrium, parties, media, and regulators will respond—by investing in local polling capacity, adjusting campaign strategies, or regulating poll publication—and those reactions may either attenuate or amplify the modest belief changes we detect.

References

Adams, James, Samuel Merrill, and Bernard Grofman, *A Unified Theory of Party Competition*, Cambridge: Cambridge University Press, 2005.

Agranov, Marina, Jacob K. Goeree, Julian Romero, and Leeat Yariv, "What Makes Voters Turn Out: The Effects of Polls and Beliefs," *Journal of the European Economic Association*, 2018, *16* (3), 825–856.

Alabrese, Eleonora and Thiemo Fetzer, "Opinion Polls, Turnout and the Demand for Safe Seats," 2024.

Arrow, Kenneth J., Amartya Sen, and Kotaro Suzumura, eds, *Handbook of Social Choice and Welfare, Volume 1*, Amsterdam: Elsevier, 2002.

Baysan, Ceren, "Persistent polarizing effects of persuasion: Experimental evidence from Turkey," *American Economic Review*, 2022, *112* (11), 3528–3546.

Bederke, Paul, Holger Döring, and Sven Regel, "Party Facts: Linking Party Datasets across Political Science," 2019.

Benoit, Kenneth and Michael Laver, *Party Policy in Modern Democracies*, London: Routledge, 2006.

Besley, Timothy and Andrea Prat, "Handcuffs for the Grabbing Hand? Media Capture and Government Accountability," *American Economic Review*, 2006, *96* (3), 720–736.

Boix, Carles, Michael Miller, and Sebastian Rosato, "A complete data set of political regimes, 1800–2007," *Comparative political studies*, 2013, *46* (12), 1523–1554.

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

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

_ **and Gabriele Gratton**, "Majority Runoff Elections: Strategic Voting and Duverger's Hypothesis," *Social Choice and Welfare*, 2015, *45* (3), 509–551.

_ **and Micael Castanheira**, "One Person, Many Votes: Divided Majority and Information Aggregation," *Econometrica*, 2012, *80* (1), 43–87.

Brennan, Geoffrey and Loren Lomasky, *Democracy and Decision: The Pure Theory of Electoral Preference*, Cambridge: Cambridge University Press, 1993.

Bursztyn, Leonardo, Davide Cantoni, Patricia Funk, Felix Schönenberger, and Noam Yuchtman, "Identifying the effect of election closeness on voter turnout: Evidence from Swiss referenda," *Journal of the European Economic Association*, 2024, *22* (2), 876–914.

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

Cruz, Cesi, Julien Labonne, and Francesco Trebbi, "Campaigning Against Populism Emotions and Information in Real Election Campaigns," Technical Report, National Bureau of Economic Research 2024.

_ **, Philip Keefer, and Julien Labonne**, "Buying informed voters: New effects of information on voters and candidates," *The Economic Journal*, 2021, *131* (635), 1105–1134.

_ **,** _ **,** _ **, and Francesco Trebbi**, "Making policies matter: Voter responses to campaign promises," *The Economic Journal*, 2024, *134* (661), 1875–1913.

Dalton, Russell J., "The Quantity and the Quality of Party Systems: Party System Polarization, Its Measurement, and Its Consequences," *Political Studies*, 2008, *56* (2), 266–288.

de Caritat Condorcet, Marie-Jean-Antoine-Nicolas, *Essai sur l'application de l'analyse à la probabilité des décisions rendues à la pluralité des voix*, Paris: Imprimerie Royale, 1785.

DellaVigna, Stefano, John A. List, Ulrike Malmendier, and Gautam Rao, "Voting to Tell Others," *The Review of Economic Studies*, 2017, *84* (1), 143–181.

Döring, Holger and Sven Regel, "Party Facts: A Database of Political Parties Worldwide," 2019. Party Politics.

Duarte, Raul, Frederico Finan, Horacio Larreguy, and Laura Schechter, "Brokering Votes With Information Spread Via Social Networks," *The Review of Economic Studies*, 2025. Advance Access, rdaf075.

Duverger, Maurice, *Political Parties: Their Organization and Activity in the Modern State*, London: Methuen, 1954.

Eggers, Andrew C. and Nick Vivyan, "Who Votes More Strategically?," *American Political Science Review*, 2020, *114* (2), 470–485.

Eurostat, OECD, and World Bank, "GDP per capita – World Bank – In constant international-$ (processed by Our World in Data)," Dataset 2025. Original data from Eurostat, OECD, and World Bank, "World Development Indicators." Minor processing by Our World in Data.

Feddersen, Timothy J. and Wolfgang Pesendorfer, "The Swing Voter's Curse," *American Economic Review*, 1996, *86* (3), 408–424.

Feltham, Oliver, Arthur Schram, and Randolph Sloof, "The Role of Opinion Polls in Coordination Amongst Protest Voters: An Experimental Study," Discussion Paper TI 2025-013/I, Tinbergen Institute 2025.

Ferlenga, Francesco, Vincenzo Galasso, Kevin Dano, Vincent Pons, and Caroline Le Pennec, "Coordination and Incumbency Advantage in Multi-Party Systems—Evidence from French Elections," *Journal of the European Economic Association*, 2025, *23* (4), 1563–1597.

Forsythe, Robert, Roger B. Myerson, Thomas A. Rietz, and Robert J. Weber, "An experiment on coordination in multi-candidate elections: The importance of polls and election histories," *Social Choice and Welfare*, 1993, *10*, 223–247.

_ , Thomas A. Rietz, Roger B. Myerson, and Robert J. Weber, "An experimental study of voting rules and polls in three-candidate elections," *International Journal of Game Theory*, 1996, *25*, 355–383.

Gapminder Foundation, "Gapminder Data Portal," 2025. Data compiled and processed by Gapminder from multiple international sources.

Gentzkow, Matthew, Jesse M. Shapiro, and Michael Sinkinson, "The Effect of Newspaper Entry and Exit on Electoral Politics," *American Economic Review*, 2011, *101* (7), 2980–3018.

Gerber, Alan S., Mitchell Hoffman, John Morgan, and Collin Raymond, "One in a Million: Field Experiments on Perceived Closeness of the Election and Voter Turnout," *American Economic Journal: Applied Economics*, 2020, *12* (3), 287–325.

Gethin, Amory, Clara Martínez-Toledano, and Thomas Piketty, "World Political Cleavages and Inequality Database (WPID)," 2021. Available from the World Inequality Lab, Paris School of Economics.

Golder, Sona N., *The Logic of Pre-Electoral Coalition Formation*, Columbus: Ohio State University Press, 2006.

Granić, Georg, "The Problem of the Divided Majority: Preference Aggregation Under Uncertainty," *Journal of Economic Behavior & Organization*, 2017, *133*, 21–38.

Granzier, Riako, Vincent Pons, and Clemence Tricaud, "Coordination and Bandwagon Effects: How Past Rankings Shape the Behavior of Voters and Candidates," *American Economic Journal: Applied Economics*, 2023, *15* (4), 177–217.

Großer, Jens and Arthur Schram, "Public Opinion Polls, Voter Turnout, and Welfare: An Experimental Study," *American Journal of Political Science*, 2010, *54* (3), 700–717.

Harmel, Robert and John D. Robertson, "Formation and Success of New Parties: A Cross-National Analysis," *International Political Science Review*, 1985, *6* (4).

Heath, Oliver and Adam Ziegfeld, "Why so little strategic voting in India?," *American Political Science Review*, 2022, *116* (4), 1523–1529.

Kapoor, Sacha and Arvind Magesan, "Independent Candidates and Political Representation in India," *American Political Science Review*, August 2018, *112* (3), 678–697.

Kendall, Chad, Tommaso Nannicini, and Francesco Trebbi, "How do voters respond to information? Evidence from a randomized campaign," *American Economic Review*, 2015, *105* (1), 322–353.

Kitschelt, Herbert and Steven I. Wilkinson, eds, *Patrons, Clients, and Policies: Patterns of Democratic Accountability and Political Competition*, Cambridge: Cambridge University Press, 2007.

Kittel, Bernhard, Wolfgang J. Luhan, and Rebecca B. Morton, "Communication and Voting in Multi-Party Elections: An Experimental Study," *The Economic Journal*, 2014, *124* (574), F196–F225.

Kollman, Ken, Allen Hicken, Daniele Caramani, David Backer, and David Lublin, "Constituency-Level Elections Archive (CLEA)," 2020. Produced and distributed by the Center for Political Studies, University of Michigan.

Laakso, Markku and Rein Taagepera, ""Effective" Number of Parties: A Measure with Application to West Europe," *Comparative Political Studies*, 1979, *12* (1), 3–27.

Larreguy, Horacio, John Marshall, and Pablo Querubin, "Parties, Brokers, and Voter Mobilization: How Turnout Buying Depends Upon the Party's Capacity to Monitor Brokers," *American Political Science Review*, 2016, *110* (1), 160–179.

Laver, Michael and Norman Schofield, *Multiparty Government: The Politics of Coalition in Europe*, Oxford: Oxford University Press, 1990.

Lehmann, Pola, Simon Franzmann, Denise Al-Gaddooa, Tobias Burst, Christoph Ivanusch, Sven Regel, Felicia Riethmüller, Andrea Volkens, Bernhard Weßels, and Lisa Zehnter, "The Manifesto Data Collection. Manifesto Project (MRG/CMP/MARPOR). Version 2025a," 2025.

Lindberg, Staffan I., Nils Düpont, Masaaki Higashijima, Yaman Berker Kavasoglu, Kyle L. Marquardt, Michael Bernhard, Holger Döring, Allen Hicken, Melis Laebens, Juraj Medzihorsky, Anja Neundorf, Ora John Reuter, Saskia Ruth-Lovell, Keith R. Weghorst, Nina Wiesehomeier, Joseph Wright, Nazifa Alizada, Paul Bederke, Lisa Gastaldi, Sandra Grahn, Garry Hindle, Nina Ilchenko, Johannes von Römer, Steven Wilson, Daniel Pemstein, and Brigitte Seim, "Varieties of Party Identity and Organization (V–Party) Dataset V2," 2022.

Mainwaring, Scott and Juan P. Zoco, "Political Sequences and the Stabilization of Interparty Competition: Electoral Volatility in Old and New Democracies," *Party Politics*, 2007, *13* (2), 155–178.

Mair, Peter, *Party System Change: Approaches and Interpretations*, Oxford: Oxford University Press, 1997.

McKelvey, Richard D. and Peter C. Ordeshook, "Elections with Limited Information: A Fulfilled Expectations Model Using Contemporaneous Poll and Endorsement Data as Information Sources," *Journal of Economic Theory*, 1985, *36* (1), 55–85.

Meffert, Michael F., Sascha Huber, Thomas Gschwend, and Franz Urban Pappi, "More than Wishful Thinking: Causes and Consequences of Voters' Electoral Expectations about Parties and Coalitions," *Electoral Studies*, 2011, *30* (4), 804–815.

Mudde, Cas, "The Populist Zeitgeist," *Government and Opposition*, 2004, *39* (4), 541–563.

Mueller, Jan-Werner, *What Is Populism?*, Philadelphia: University of Pennsylvania Press, 2016.

Myerson, Roger B. and Robert J. Weber, "A Theory of Voting Equilibria," *American Political Science Review*, 1993, *87* (1), 102–114.

Norris, Pippa and Ronald Inglehart, *Cultural Backlash: Trump, Brexit, and Authoritarian Populism*, Cambridge: Cambridge University Press, 2019.

Nurmi, Hannu, *Comparing Voting Systems*, Dordrecht: D. Reidel, 1987.

Palshikar, Suhas, Sanjay Kumar, and R. Yunus, eds, *Electoral Politics in India: The Resurgence of the Bharatiya Janata Party*, London: Routledge, 2017.

Panebianco, Angelo, *Political Parties: Organization and Power*, Cambridge: Cambridge University Press, 1988.

Piketty, Thomas, "Voting as Communicating," *The Review of Economic Studies*, 2000, *67* (1), 169–191.

Pons, Vincent, "Will a five-minute discussion change your mind? A countrywide experiment on voter choice in France," *American Economic Review*, 2018, *108* (6), 1322–1363.

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

Potthoff, Richard F., "Condorcet Polling," *Public Choice*, 2011, *148* (1–2), 67–86.

Reporters Without Borders (RSF), "India: RSF calls for press freedom in the world's largest democracy," 2025. Accessed 2026-02-03.

Sartori, Giovanni, *Parties and Party Systems: A Framework for Analysis*, Cambridge: Cambridge University Press, 1976.

Tavits, Margit, "Organizing for Success: Party Organizational Strength and Electoral Performance," *The Journal of Politics*, 2012, *74* (1), 83–97.

For online publication only:

Majority Lost: Global Evidence and Field Experiments on Electoral Miscoordination

Francesco Capozza Ruben Durante Siddharth George

A Theoretical Framework

This appendix develops a model of strategic voting with expressive motives and poll information.

A.1 Environment

Consider a single first-past-the-post (FPTP) constituency with $K \geq 3$ parties indexed by $\mathcal{C} = \{1, \ldots, K\}$. Parties 1 and 2 are the main contenders (the "top two"); $k \geq 3$ indexes minor parties. There are N voters indexed by $i = 1, \ldots, N$.

Voter types. Voter i's type is $\tau_i = \big(v_i(\cdot), \lambda_i, e_i\big)$, where

- $v_i(c) \in \mathbb{R}$ is the outcome utility if party c wins, with strict ranking $v_i(1) \geq v_i(2) \geq \cdots \geq v_i(K)$ and favourite party $t(i) := \arg\max_{c \in \mathcal{C}} v_i(c)$;
- $\lambda_i \in [0, 1]$ is the weight placed on expressive utility;
- $e_i > 0$ is the expressive payoff from supporting $t(i)$.

Types are drawn i.i.d. from a joint distribution $\mathcal{F}$ on $\mathbb{R}^K \times [0, 1] \times \mathbb{R}_{++}$. We impose no parametric restriction on $\mathcal{F}$ and in particular do not assume that expressive motives dominate for almost all voters—this is an *outcome* of the model, not a premise.

Composite expressive attachment. Define

$$A_i \;:=\; \frac{\lambda_i\, e_i}{1-\lambda_i}, \qquad A_i \in [0,\infty), \tag{8}$$

with $A_i = \infty$ when $\lambda_i = 1$ (purely expressive voters). Let F_A denote the population CDF of A_i and f_A its density. The distribution F_A is a primitive object that can be estimated from survey measures of partisan strength and issue salience (e.g. the importance of caste, ideology, and candidate integrity elicited in our baseline survey).

Utility. Each voter chooses $a_i \in \mathcal{C}$. Given the election winner $w \in \mathcal{C}$, voter i's payoff is

$$U_i(a_i) \;=\; (1-\lambda_i)\,\mathbb{E}\big[v_i(w) \mid a_i\big] \;+\; \lambda_i\,\mathbf{1}[a_i = t(i)]\,e_i. \tag{9}$$

The first term is standard pivotal-voter expected utility (Brennan and Lomasky, 1993); the second captures expressive satisfaction from voting for one's favourite party.

A.2 Prior Formation and the Information Environment

Dirichlet prior. Voters share a common prior over constituency vote shares modelled as a Dirichlet distribution with parameter vector $\boldsymbol{\alpha} = (\alpha_1, \ldots, \alpha_K)$ and concentration $\alpha_0 := \sum_{c=1}^{K} \alpha_c$. The concentration α_0 governs prior precision: a larger α_0 means that individual observations move beliefs less.

Endogenous precision. Rather than treating α_0 as a free parameter, we derive it from three observable information-environment variables:

- $m \in [0,1]$: local media penetration (fraction of voters with regular access to coverage of the constituency race);
- $H \geq 0$: years of continuous electoral experience in the constituency (voter learning from repeated elections);
- ENPP ≥ 1: the Laakso–Taagepera effective number of parties, capturing the dimensionality of the inference problem.

Assumption 1 (Information environment)**.** The prior concentration satisfies

$$\alpha_0(m, H, \mathrm{ENPP}) \;=\; \exp\big(\gamma_0 + \gamma_1 m + \gamma_2 \log(1+H) - \gamma_3 \log \mathrm{ENPP}\big), \quad \gamma_1, \gamma_2, \gamma_3 > 0. \tag{10}$$

Equation (10) embeds three testable mechanisms: more media exposure sharpens priors ($\gamma_1 > 0$); longer democratic experience yields voter learning ($\gamma_2 > 0$); greater fragmentation widens the inference problem and reduces precision ($\gamma_3 > 0$). All three comparative statics are linked to the cross-national evidence in Section 3: among the strongest correlates of coordination failure there are high ENPP and party-system churn, both of which reduce α_0 in (10).

Poll signal. A poll of size n draws respondents i.i.d. and records vote intentions $x = (x_1, \ldots, x_K)$ with $\sum_c x_c = n$. Conditional on θ, x is multinomial. The posterior is Dirichlet with parameters $\boldsymbol{\alpha} + x$, giving posterior mean

$$\mu_c^{\mathrm{post}} \;:=\; \mathbb{E}[\theta_c \mid x] \;=\; \frac{\alpha_c + x_c}{\alpha_0 + n}. \tag{11}$$

A.3 Pivotality as a Function of Primitives

We focus on the strategically relevant comparison for a minor-party supporter: defecting to top-two party $\ell \in \{1, 2\}$ versus staying with minor party $k \geq 3$.

Derived pivotality formula. Define the posterior mean gap and its variance:

$$\delta_\mu \;:=\; \mathbb{E}[\theta_\ell - \theta_k \mid \mu] \;=\; \frac{(\alpha_\ell + x_\ell) - (\alpha_k + x_k)}{\alpha_0 + n}, \tag{12}$$

$$\sigma_\mu^2 \;:=\; \mathrm{Var}[\theta_\ell - \theta_k \mid \mu] \;=\; \frac{\big[(\alpha_\ell + x_\ell) + (\alpha_k + x_k)\big](\alpha_0 + n) - \big[(\alpha_\ell + x_\ell) - (\alpha_k + x_k)\big]^2}{(\alpha_0 + n)^2(\alpha_0 + n + 1)}. \tag{13}$$

[Pivotality from primitives] Under the normal approximation for large N, the probability that a single ballot for ℓ rather than k is decisive is

$$\pi(\ell, k \mid \mu, N) \;=\; \frac{1}{\sqrt{2\pi N}\,\sigma_\mu} \exp\!\left(-\frac{N\delta_\mu^2}{2\sigma_\mu^2}\right). \tag{14}$$

Proof. The pairwise vote margin $V_\ell - V_k$ follows approximately a Normal$(N\delta_\mu,\ N\sigma_\mu^2)$ distribution by the CLT applied to the Dirichlet–multinomial likelihood. A vote is pivotal iff $V_\ell - V_k \in \{-1, 0\}$, whose probability under the normal approximation equals the density at zero divided by N, yielding (14). □

Poll-induced change in pivotality. Denote prior quantities by $\delta_0 := \delta_{\mu^{\text{prior}}}$, $\sigma_0 := \sigma_{\mu^{\text{prior}}}$, and $\pi_0 := \pi(\ell, k \mid \mu^{\text{prior}}, N)$. After observing a poll result x:

$$\Delta\delta \ :=\ \delta_{\mu^{\text{post}}} - \delta_0 \ =\ \frac{n\big(\hat\theta_\ell - \hat\theta_k - \delta_0\big)}{\alpha_0 + n}, \tag{15}$$

$$\Delta\pi \ :=\ \pi(\ell, k \mid \mu^{\text{post}}, N) - \pi_0 \ \approx\ -\frac{N\delta_0}{\sigma_0^2}\,\pi_0\,\Delta\delta \ +\ O\!\left(\frac{1}{(\alpha_0 + n)^2}\right). \tag{16}$$

Equations (15)–(16) express $\Delta\pi$ in terms of three observable primitives:

1. *Poll surprise*: $|\hat\theta_\ell - \hat\theta_k - \delta_0|$ — how much the poll deviates from the prior-expected gap;
2. *Prior imprecision*: $1/\alpha_0$ — lower precision magnifies belief updates per unit of surprise;
3. *Prior closeness*: $|\delta_0|$ — tighter prior races generate larger proportional changes in pivotality.

Crucially, $\Delta\pi$ can be positive or negative. If the poll reveals a *tighter* race than voters expected ($\hat\theta_\ell - \hat\theta_k < \delta_0$), then $\Delta\pi > 0$, increasing the incentive to defect. If it reveals a *larger* lead for the top-two party, then $\Delta\pi < 0$, reducing the incentive. This sign-dependence generates directly testable predictions: the direction of any behavioural response should co-vary with whether the poll is a positive or negative surprise relative to voter priors—a prediction the observational data in Tables B.16–B.17 can discipline.

A.4 Strategic Defection and Voter Heterogeneity

Voter i with favourite minor party k defects to top-two party ℓ when the instrumental gain exceeds the expressive loss:

$$\underbrace{C\,\pi(\ell, k \mid \mu, N)\,\Delta v_i}_{\text{instrumental gain}} \ >\ \underbrace{A_i}_{\text{expressive attachment}}, \tag{17}$$

where $\Delta v_i := v_i(\ell) - v_i(k) > 0$ is voter i's instrumental stake in the top-two outcome, and $C > 0$ is a positive constant determined by the density of the vote-difference distribution at zero (from Lemma A.3). This defines a *type-specific defection threshold*

$$A_i^*(\mu) \;:=\; C\,\pi(\ell, k \mid \mu, N)\,\Delta v_i. \tag{18}$$

Voter i defects if and only if $A_i < A_i^*(\mu)$.

Aggregate defection rate. Using the population-average instrumental stake $\overline{\Delta v} := \mathbb{E}[\Delta v_i \mid t(i) = k]$ and shorthand $A^*(\mu) := C\,\pi(\ell, k \mid \mu, N)\,\overline{\Delta v}$, the equilibrium defection rate among minor-party supporters is

$$\rho(\mu) \;:=\; F_A\big(A^*(\mu)\big). \tag{19}$$

Poll-induced behavioural change. The fraction of minor-party supporters who switch from non-defection to defection after receiving the poll is

$$\Delta\rho \;:=\; \rho(\mu^{\text{post}}) - \rho(\mu^{\text{prior}}) \;\approx\; f_A\big(A_0^*\big) \cdot C\,\overline{\Delta v} \cdot \Delta\pi, \tag{20}$$

where $A_0^* := A^*(\mu^{\text{prior}})$ and we use a first-order Taylor expansion around the prior threshold.

Equation (20) is the central non-circular result. It expresses $\Delta\rho$ as a product of three separately estimable quantities:

- $f_A(A_0^*)$: the density of the expressive-attachment distribution at the pre-poll threshold—high when many voters are "on the margin" of switching;
- $C\,\overline{\Delta v}$: the scale of instrumental stakes;
- $\Delta\pi$: the poll-induced change in pivotality from (16).

Behavioural invariance ($\Delta\rho \approx 0$) arises when *at least one* of these is near zero; it is not assumed globally. The framework nests full expressive dominance (large $\bar{A}$ relative to A_0^*, so $f_A(A_0^*) \approx 0$), full instrumental voting ($\lambda_i \approx 0$ for all i, so $A_0^* \gg \bar{A}$ and almost everyone defects), and all intermediate cases.

[Indian experiments] Tables B.16–B.17 document that Indian voters *underestimate* the leading party's vote share: $\hat{\theta}_\ell - \hat{\theta}_k > \delta_0^{\text{perceived}}$ in every state. Equation (15) therefore implies $\Delta\delta > 0$ and, since $\delta_0 > 0$, equation (16) implies $\Delta\pi < 0$: the

poll reveals a *less competitive* race than voters expected, reducing the pivotality of defection. This pushes in the direction of *less* strategic voting, not more—consistent with the null behavioural effects and with the modest poll-induced reinforcement of top-two vote shares documented in Table B.18.

A.5 Multiple Fulfilled-Expectations Equilibria

Equilibrium vote shares. When fraction ρ of minor-party k's supporters defect to the *trailing* top-two party ($\ell = 2$, with $\theta_2^0 < \theta_1^0$), realised vote shares are

$$\theta_1(\rho) = \theta_1^0, \qquad \theta_2(\rho) = \theta_2^0 + \rho\,\theta_k^0, \qquad \theta_k(\rho) = (1-\rho)\,\theta_k^0. \tag{21}$$

The effective margin between the two top-two parties is

$$\delta(\rho) \;:=\; \theta_1(\rho) - \theta_2(\rho) \;=\; (\theta_1^0 - \theta_2^0) - \rho\,\theta_k^0. \tag{22}$$

This changes sign at $\rho^* := (\theta_1^0 - \theta_2^0)/\theta_k^0$, which lies in $(0,1)$ whenever $\theta_k^0 > (\theta_1^0 - \theta_2^0)$ — i.e. when the minor party is large enough to tip the race. Note that the pivotality $\pi(\rho) \propto \exp(-N\delta(\rho)^2/(2\sigma^2))$ is then *maximised* at ρ^*, where the race is exactly tied.

Fixed-point condition. In a fulfilled-expectations equilibrium (FEE), beliefs must be consistent with equilibrium actions: $\delta_\mu = \delta(\rho)$. The equilibrium defection rate therefore satisfies the fixed-point equation

$$\rho \;=\; F_A\big(C\,\pi(\rho)\,\overline{\Delta v}\big) \;=:\; \Psi(\rho). \tag{23}$$

Proposition 1 (Multiple equilibria)**.** *Suppose minor-party supporters consider defecting to the trailing top-two party ($\ell = 2$) and that $\theta_k^0 > (\theta_1^0 - \theta_2^0)$ so that $\rho^* \in (0,1)$. If there exist $\rho_a < \rho^* < \rho_b$ such that $\Psi(\rho_a) < \rho_a$, $\Psi(\rho^*) > \rho^*$, and $\Psi(\rho_b) < \rho_b$, then (23) admits at least three fixed points $0 \le \rho^L < \rho^M < \rho^H \le 1$, where ρ^L and ρ^H are stable and ρ^M is unstable.*

Proof. Since $\pi(\rho)$ is hump-shaped and F_A is non-decreasing, $\Psi(\rho) = F_A(C\pi(\rho)\overline{\Delta v})$ is also hump-shaped with maximum at ρ^*. The stated sign conditions imply that $\Psi(\rho) - \rho$ changes sign at least twice on $(0,\rho^*)$ and $(\rho^*,1)$, so by the intermediate value theorem there are at least three fixed points. Stability follows from the standard argument: a fixed point ρ^s is stable iff $\Psi'(\rho^s) < 1$. □

The two stable equilibria have interpretations that map directly onto the empirical phenomena of interest:

- **Coordination failure equilibrium** (ρ^L): few minor-party supporters defect; the trailing top-two party remains behind; pivotality is low and defection is irrational for most voters. This is the equilibrium associated with Condorcet-winner losses documented in Section 3.
- **Coordination success equilibrium** (ρ^H): many supporters defect; the trailing party becomes competitive; pivotality rises and defection is rational. This corresponds to successful strategic coordination.

A.6 Polls as Equilibrium Selection Devices

In the presence of multiple FEE, a poll serves not only as a Bayesian belief-updating device but also as a *coordination focal point* that can shift the system between equilibria through changes in common knowledge. This is a qualitatively distinct channel from the marginal pivotality mechanism in (20), and it generates different empirical signatures.

Equilibrium-switching threshold. Define ρ^M as the unstable fixed point in Proposition 1. A poll result $\hat{\rho}_{\text{poll}}$—the implied defection rate from constituency baseline intentions—provides a public signal visible to all respondents simultaneously.

Proposition 2 (Polls as coordination focal points)**.** *Suppose the system is at the coordination-failure equilibrium ρ^L. If $\hat{\rho}_{\text{poll}} > \rho^M$, the poll places common knowledge of the defection rate above the tipping point; the unique rationalizable continuation is ρ^H, and the equilibrium switches. The resulting behavioural response is* discontinuous *and large ($\Delta\rho \approx \rho^H - \rho^L$). If $\hat{\rho}_{\text{poll}} < \rho^M$, the system remains in the basin of attraction of ρ^L, and poll effects are governed entirely by* (20)*: continuous, small, and proportional to $f_A(A_0^*) \cdot \Delta\pi$.*

Proof sketch. Under the iterated-dominance (global-games) argument, if all voters observe $\hat{\rho}_{\text{poll}} > \rho^M$ and believe others observe the same signal, then playing ρ^H strictly dominates playing ρ^L for every voter with $A_i < A^*(\rho^H)$, which by the stability of ρ^H includes a majority of minor-party supporters. Sequential deletion of dominated strategies then uniquely selects ρ^H. □

Null effects revisited. Proposition 2 implies two empirically distinguishable sufficient conditions for behavioural null effects in our experiments:

(a) *Within-basin, marginal channel weak.* The system is at ρ^L and $\hat{\rho}_{\text{poll}} < \rho^M$, so equilibrium switching does not occur; the marginal adjustment from (20) is small because $f_A(A_0^*) \approx 0$ (most voters have $A_i \gg A_0^*$) and/or $|\Delta\pi| \approx 0$ (the poll reveals a less competitive race than expected, as documented in Remark A.4).

(b) *Unique equilibrium.* The fixed-point equation has a unique solution—either because $\theta_k^0 < (\theta_1^0 - \theta_2^0)$ (minor party cannot tip the race) or because F_A has no mass near $C\pi_0\overline{\Delta v}$. In this case equilibrium selection is irrelevant and marginal effects govern.

Condition (a) predicts a structural break in poll effects as $\hat{\rho}_{\text{poll}}$ crosses ρ^M, generating non-linearity in the treatment effect. Condition (b) predicts uniformly small effects. These are *empirically distinguishable* given sufficient variation in poll content across constituencies.

A.7 Phase Diagram

Define the *behavioural sensitivity index*

$$\zeta \;:=\; f_A(A_0^*) \cdot C\,\overline{\Delta v} \cdot \frac{N|\delta_0|}{\sigma_0^2}\,\pi_0 \cdot \frac{n\left|\hat{\theta}_\ell - \hat{\theta}_k - \delta_0\right|}{\alpha_0 + n}, \tag{24}$$

which combines the four multiplicative determinants of $|\Delta\rho|$ from equations (15)–(20). Each component is observable or estimable from survey data, election records, and the poll result itself.

[Phase diagram] Poll-induced responses fall into one of three non-overlapping regions of the parameter space $(\alpha_0,\ \bar{A},\ \delta_0,\ \theta_k^0)$:

Region I (*Equilibrium switching; large behavioural response.*) Necessary conditions: $\theta_k^0 > (\theta_1^0 - \theta_2^0)$ and $\hat{\rho}_{\text{poll}} > \rho^M$. Sufficient condition: $F_A(A^*(\rho^H)) > 0$. Outcome: $\Delta\rho \approx \rho^H - \rho^L \gg 0$.

Empirical signature: large, non-linear shifts in voting intentions concentrated among minor-party supporters; effect magnitude discontinuous in poll content; no systematic shift in affective ratings (coordination without persuasion).

Region II (*Belief updating, no behavioural change.*) Condition: $\hat{\rho}_{\text{poll}} < \rho^M$ (or unique equilibrium) **and** $\zeta < \tau$ for some threshold $\tau > 0$. This obtains when any one of the following holds: (i) α_0 is large (precise priors, so $|\Delta\delta|$ is small); (ii) $f_A(A_0^*)$ is small (few voters near the defection threshold); (iii) the poll reveals a less competitive race ($\Delta\pi < 0$), reducing rather than increasing pivotality. Outcome: significant belief updating, $\Delta\rho \approx 0$.

Empirical signature: significant treatment effect on second-order beliefs, null effect on voting intentions and party rankings, null effect on wasted-vote salience. **This is the region characterising the Indian experiments** (combining channels (i) through (iii) from Remark A.4).

Region III (*No response.*) Condition: $\alpha_0 \to \infty$ or $n \to 0$. From (15): $\Delta\delta \to 0$, so $\Delta\pi \to 0$ and the poll adds no information. Outcome: $\Delta\rho = 0$ and belief updating is also negligible.

Empirical signature: null effects on both beliefs and behaviour; effectively unresponsive second-order beliefs.

Proof. Region I follows from Proposition 2. Regions II and III follow from (20): $\Delta\rho \approx \zeta$, and $\zeta \to 0$ when any multiplicative component of ζ vanishes—specifically when $\alpha_0 \to \infty$ (Region III), when $f_A(A_0^*) \to 0$ (no mass at the threshold), or when $|\Delta\delta| \to 0$ (uninformative poll). Distinct signatures for Regions II and III follow from whether the belief-updating channel $|\Delta\delta|$ is positive or zero. □

A.8 Testable Predictions

Corollary 1 (Heterogeneous individual-level responses)**.** *Within Region II, treatment effects on intended votes are increasing in* $1/A_i$ *(decreasing in expressive attachment):*

$$\frac{\partial\, \mathbb{E}[\mathbf{1}[a_i = \ell] \mid \text{Treated}]}{\partial\,(1/A_i)} \;=\; f_A(A_0^*) \cdot C\, \pi_0\, \overline{\Delta v} \;>\; 0. \tag{25}$$

Testable implication*: treatment effects should be concentrated among voters with weak partisan identification—those who at baseline reported no party preference, assigned low importance to ideology or caste, or were classified as undecided. This is directly testable against Table B.21 (unsure voters) and the issue-salience heterogeneity in Table 5.*

Corollary 2 (Surprise-driven belief updating)**.** *The magnitude of belief revision for party c satisfies*

$$|\Delta\mu_c^{\text{post}} - \Delta\mu_c^{\text{prior}}| \;=\; \frac{n\,|\hat{\theta}_c - \mu_c^{\text{prior}}|}{\alpha_0 + n}, \tag{26}$$

from (11)*. This is increasing in poll surprise* $|\hat{\theta}_c - \mu_c^{\text{prior}}|$ *and decreasing in prior precision* α_0*.*

Testable implication*: respondents whose baseline second-order beliefs were furthest from the poll results should show the largest revisions. This prediction is directly falsifiable using the individual-level baseline misperception data in Table B.17 and the belief-updating regressions in Tables B.19–B.20.*

Corollary 3 (Cross-national implications)**.** *From Assumption 1 and Theorem A.7:*

(*i*) *Constituencies with lower media penetration m or fewer past elections H have lower* α_0*, placing them closer to Region I; coordination failures are more prevalent and polling interventions more likely to be effective.*

(*ii*) *More fragmented party systems (higher ENPP) have lower* α_0 *and larger* θ_k^0*, placing them closer to the Region I / Region II boundary; these are both the settings where coordination failure is most common (Section 3) and where information could theoretically help most.*

(*iii*) *High-*$\bar{A}$ *electorates (strong partisan identities) remain in Region II even when fragmentation is high and the structural conditions for Region I are satisfied— expressive attachment prevents the translation of belief updating into behavioural change.*

These three predictions jointly rationalise the cross-national pattern that coordination failure is most severe in fragmented, institutionally younger party systems, while also explaining why simple viability polls do not resolve failures even where they are worst.

Corollary 4 (When do polls function as coordination devices?)**.** *Polls overcome coordination failure only when all three of the following conditions hold simultaneously:*

(*i*) Structural condition*:* $\theta_k^0 > (\theta_1^0 - \theta_2^0)$ *— the minor party is large enough to tip the race.*

(*ii*) Content condition*:* $\hat{\rho}_{\text{poll}} > \rho^M$ *— the poll signals sufficient momentum toward the trailing top-two party.*

(*iii*) Attachment condition: $F_A(A^*(\rho^H)) > 0$ — *a positive mass of voters has expressive attachment below the defection threshold at the high-defection equilibrium.*

Laboratory experiments operate in Region I by design: conditions (*i*)–(*iii*) *are engineered into the protocol* (*no real partisan stakes, clear viability signals, no prior equilibrium*). *High-salience FPTP field settings typically fail condition* (*iii*): *entrenched partisanship places most voters well above* $A^*(\rho^H)$. *When the poll also reveals a less competitive race than expected* (*condition* (*ii*) *fails*), *Region II obtains and null behavioural effects are the predicted outcome even under belief updating.*

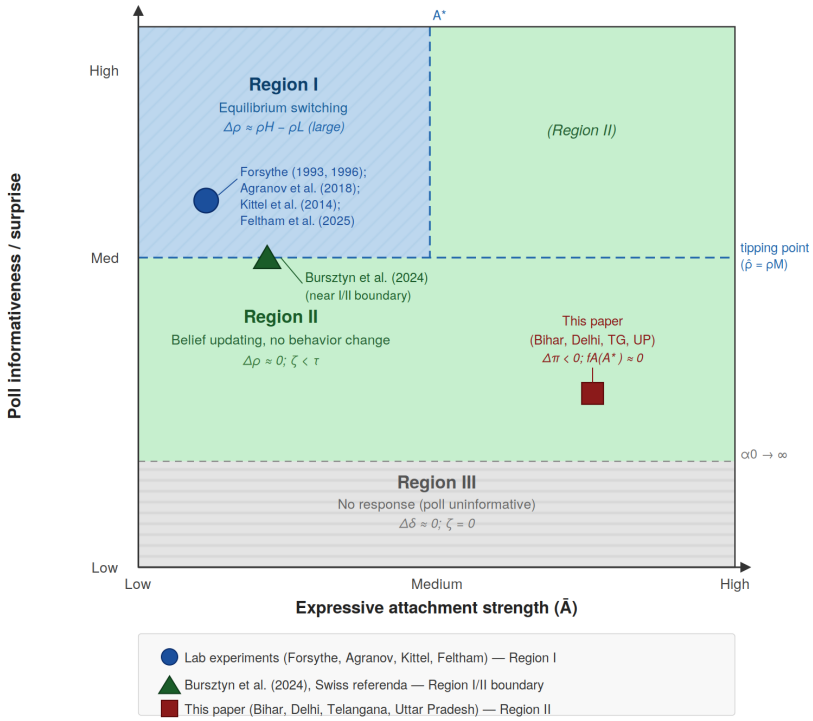


Figure A.1. Phase diagram of poll-induced coordination effects. The horizontal axis measures expressive attachment strength ($\bar{A}$); the vertical axis measures poll informativeness and surprise ($|\Delta\delta|$). *Region I* (equilibrium switching): the poll signal exceeds the tipping point ($\hat{\rho}_{\text{poll}} > \rho_M$) and expressive attachment is low, producing a large discontinuous shift $\Delta\rho \approx \rho^H - \rho^L$. *Region II* (belief updating, no behavior change): the poll refines second-order beliefs but $\zeta < \tau$, so $\Delta\rho \approx 0$; this obtains when $\Delta\pi < 0$ (poll reveals a less competitive race than voters expected) and/or $f_A(A_0^*) \approx 0$ (few voters near the defection threshold). *Region III* (no response): priors are sufficiently precise ($\alpha_0 \to \infty$) that the poll conveys no new information.

B Robustness to the vote-reallocation assumption

Our coordination-failure measures rest on a behavioral assumption embedded in the counterfactual pairwise contests of equation: when all parties other than the two contenders p_i and p_j are removed, *every* supporter of an eliminated party is assumed to turn out and transfer to whichever of p_i, p_j is ideologically closer, with exact ties split evenly. This full-transfer, closest-takes-all rule is the sharpest possible version of the divided-majority mechanism, and it is precisely the step that gives reallocated votes the leverage to overturn the realized plurality outcome (as in the Schaffhausen example of Section **??**, where the Condorcet winner places only third in the raw vote). Because the rule is not directly testable in observational data, we assess how sensitive the failure rates are to relaxing it. We hold the ideology sources, distance metric, and valid-election sample fixed at their paper definitions and vary only the reallocation step, recomputing the Condorcet winner, the Condorcet loser, and hence the weak and strong failure classification for each of the 41,717 (MARPOR) and 33,550 (V–Party) valid constituency-elections. Results appear in Table B.1; Figure 3 plots the abstention sweep.

Uniform abstention. We first allow a fixed fraction α of would-be transferring voters to abstain rather than switch to a remaining party, so party p_k's contribution to the two-way contest is scaled by $(1 - \alpha)$ while the closest-party assignment is unchanged. This nests the baseline at $\alpha = 0$. Failure rates decline smoothly and monotonically in α: for MARPOR the weak-failure rate falls from 26.2% at $\alpha = 0$ to 22.0%, 18.4%, 15.2%, and 10.4% at $\alpha = 0.1, 0.2, 0.3, 0.5$, with the strong-failure rate falling from 9.5% to 1.1% over the same range; the V–Party series behaves almost identically (28.1% $\rightarrow$ 12.5% weak, 10.2% $\rightarrow$ 1.8% strong). The direction is mechanical and expected: abstention strips reallocated votes of exactly the leverage they need to lift a low-plurality Condorcet winner past the realized winner, so the simulated and realized winners coincide more often. The economically relevant point is the magnitude. Even under substantial abstention— $\alpha = 0.3$, i.e. nearly a third of transferring voters staying home—weak coordination failures remain common (15–18%) and strong failures remain non-negligible (3–4%), so the paper's central qualitative claim, that coordination failures are a widespread rather than marginal feature of multiparty elections, survives. What the exercise does establish is that the *precise headline figure* of 20–30% is conditional on the full-turnout assumption and should be read as an upper region of a range that shifts downward once transfer

is imperfect.

Distance-based abstention. As an extension we let abstention depend on ideological distance rather than a flat rate, retaining a share $\exp(-k\, d_{\min})$ of each transferring party's supporters, where $d_{\min}$ is the standardized distance to the nearest surviving contender. This captures the intuition that voters marooned far from both remaining options are likelier to abstain than those with a close substitute. The pattern mirrors the uniform case—weak failures fall to 19.9%, 16.5%, and 12.8% for MARPOR at $k = 0.25, 0.5, 1.0$—but attenuates more slowly under V–Party (to 20.5% at $k = 1.0$), because V–Party's two-dimensional distances place fewer eliminated parties at extreme distances from both contenders.

Tie-breaking. The baseline splits an eliminated party's votes evenly when it is equidistant from the two contenders. Replacing this with either dropping tied votes or assigning them at random leaves the failure rates essentially unchanged (differences below 0.05 percentage points in both sources), confirming that exact ideological ties are rare in programmatically structured party systems and play no material role in the measured failure rates.

Probabilistic (logit) reallocation. Finally we replace the hard closest-takes-all rule with a smooth one, in which a third party p_k sends fractions proportional to $\exp(-\beta\, d)$ to each contender, so that both remaining parties receive some of its votes and the split sharpens as β grows (recovering the baseline as $\beta \to \infty$). Failure rates increase in β, from near zero under a nearly uniform split ($\beta = 0.5$: 4.3% weak for MARPOR, 1.5% for V–Party) up toward the baseline as assignment becomes decisive ($\beta = 4$: 17.2% and 8.9%). This localizes the source of the measured failures: they arise specifically from the assumption that reallocated voters concentrate *decisively* on the single closest party. When proximity governs reallocation only probabilistically, dispersed transfers rarely coalesce enough behind a low-plurality party to make it the Condorcet winner.

Takeaway. Across all four perturbations the qualitative conclusion is stable— coordination failures are pervasive and strong failures occur in a meaningful minority of elections—while the point estimates are sensitive, in the expected direction, to how decisively and how fully eliminated-party voters are assumed to transfer. The

baseline rule should therefore be understood as the upper end of a plausible range: it maximizes the leverage of reallocated votes, and any departure toward abstention or probabilistic transfer moves the failure rates downward without overturning the paper's substantive findings.

Table B.1. Coordination-failure rates under alternative vote-reallocation assumptions

		MARPOR (RILE)		V–Party	
Reallocation rule	Parameter	Weak	Strong	Weak	Strong
Baseline (paper, eq. 5)	full transfer	26.2	9.5	28.1	10.2
Uniform abstention	$\alpha = 0.10$	22.0	7.2	23.9	8.0
	$\alpha = 0.20$	18.4	5.1	20.5	6.0
	$\alpha = 0.30$	15.2	3.4	17.8	4.4
	$\alpha = 0.50$	10.4	1.1	12.5	1.8
Distance-based abstention	$k = 0.25$	19.9	6.9	25.6	9.3
	$k = 0.50$	16.5	5.1	23.6	8.5
	$k = 1.00$	12.8	2.9	20.5	7.2
Tie-breaking rule	drop	26.2	9.5	28.1	10.2
	random	26.2	9.5	28.1	10.2
Logit reallocation	$\beta = 0.5$	4.3	0.3	1.5	0.0
	$\beta = 1.0$	7.7	1.2	2.8	0.1
	$\beta = 2.0$	12.5	3.3	5.3	0.6
	$\beta = 4.0$	17.2	5.5	8.9	2.2

Notes: Each cell reports the share of valid constituency-elections classified as a weak (Condorcet winner does not win) or strong (Condorcet loser wins) coordination failure, recomputed under a different rule for reallocating the votes of parties absent from each counterfactual pairwise contest. The baseline row reproduces the paper's rule (equation 5): all supporters of eliminated parties turn out and transfer to the ideologically closest of the two remaining parties, with equal distances split. *Uniform abstention* lets a fraction α of would-be transferring voters abstain rather than switch. *Distance-based abstention* instead retains a share $\exp(-k \cdot d)$ of transferring voters, where d is the distance to the nearest surviving party, so voters far from both contenders abstain more. *Tie-breaking* varies how equal-distance third parties are handled (baseline splits; "drop" discards; "random" assigns at random). *Logit reallocation* replaces the hard closest-takes-all assignment with a probabilistic split in which a third party sends shares $\propto \exp(-\beta \cdot d)$ to each contender, nesting the baseline as $\beta \to \infty$. The distance metric and the valid-election sample are held fixed at the paper's definitions throughout; only the reallocation step changes.

C Additional Tables

Table C.1. Overlap and Agreement Between V-Party and Manifesto Coordination Failure Sources

	V-Party	**Manifesto**	**Both**
Panel A: Coverage			
Countries	96	50	47
Elections	490	518	307
Constituency-elections	33,550	41,717	23,883
Panel B: Failure Rates (full sample)			
Weak failure rate (%)	27.8	26.5	—
Strong failure rate (%)	10.2	9.5	—
Panel C: Agreement (overlapping constituency-elections, n=23,883)			
Weak failure agreement (%)		76.3	
Strong failure agreement (%)		85.3	

Panel D: Cross-tabulation of Classifications (Overlapping Observations)

	Weak Failure		**Strong Failure**	
	MP=0	MP=1	MP=0	MP=1
VP=0	14,274	2,945	19,887	1,719
VP=1	2,714	3,950	1,783	494

Notes: Panel A reports coverage statistics for each ideology source and for the intersection. V–Party v2 uses a two-dimensional distance metric (anti-pluralism and populism indices); the Manifesto Project uses the one-dimensional RILE (right–left) index. Panel B reports unconditional weak and strong coordination failure rates in the full sample of each source. Panel C reports the share of constituency-elections in the overlapping sample for which both sources agree on the classification (weak or strong failure vs. no failure). Panel D shows the full cross-tabulation of V–Party (VP) and Manifesto (MP) classifications for overlapping constituency-elections. When an election appears in both sources, the main analysis retains the V–Party classification, since it provides broader country coverage. The overlapping sample includes 23,883 constituency-elections from 47 countries and 307 elections.

Table C.2. Effects of GDP per capita and years of continued democracy on coordination failure

	Strong Coordination Failure											
	No supply-side controls						**With supply-side PCs**					
	OLS			Logit			OLS			Logit		
Years of continued democracy	0.000		0.000	0.000		0.005	0.000		0.000	−0.073		−0.065
	(0.000)		(0.000)	(0.006)		(0.004)	(0.001)		(0.001)	(0.092)		(0.087)
# Parties (log)	−0.059	−0.053	−0.051	−1.538	−2.339	−2.289						
	(0.070)	(0.072)	(0.074)	(1.994)	(1.951)	(1.968)						
GDP per capita (log)		−0.058	−0.058		−3.111*	−3.149*		−0.227	−0.236		−4.100	−3.084
		(0.082)	(0.083)		(1.657)	(1.673)		(0.236)	(0.245)		(7.202)	(7.177)
Election characteristics (PCs)	—						Yes					
Num. obs.	35349	30245	30245	10315	7070	7070	17869	17869	17869	2869	2869	2869
Num. groups: cst_n	12543	11572	11572	1880	1188	1188	8140	8140	8140	626	626	626
Num. groups: year	70	65	65	64	60	60	44	44	44	38	38	38
R^2 (full model)	0.458	0.497	0.497				0.559	0.559	0.559			
R^2 (proj model)	0.000	0.001	0.001				0.005	0.005	0.005			
Adj. R^2 (full model)	0.158	0.182	0.182				0.186	0.186	0.186			
Adj. R^2 (proj model)	0.000	0.000	0.000				0.003	0.004	0.004			
Deviance												
Log Likelihood												
Pseudo R^2												

Notes: The dependent variable is an indicator equal to one if a *strong* coordination failure occurs (i.e., the Condorcet loser wins). Specification is otherwise identical to Table 1: OLS (cols. 1–3 and 7–9) and logit (cols. 4–6 and 10–12) with constituency and year fixed effects; columns 7–12 add the first ten principal components of election-level covariates. Standard errors clustered two-way at the country and year levels. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.3. Party-level OLS FE: Party Traits on Benefiting (Fixed Effects: *cst_n*, *year*)

Outcome: Benefiting from Weak Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Anti-Pluralism index	0.614 (0.451)	14810	4457	50	0.373	0.098
Populism index	−0.491 (0.297)	14810	4457	50	0.378	0.106
Seat share in lower chamber	0.009 (0.002)***	16800	5034	69	0.480	0.253
Number of seats won in lower chamber	0.002 (0.001)***	16800	5034	69	0.466	0.233
Total number of seats in lower chamber	0.000 (0.000)***	16800	5034	69	0.379	0.108
Vote share in lower chamber	0.013 (0.003)***	15161	4521	69	0.470	0.239
Electoral continuity	−0.050 (0.086)	14810	4457	50	0.349	0.064
Opposition	−0.118 (0.034)***	14810	4457	50	0.432	0.183
Pariah status	0.033 (0.036)	14797	4457	50	0.356	0.074
Anti-elitism	−0.094 (0.048)*	14810	4457	50	0.387	0.119
People-centrism	−0.040 (0.081)	14810	4457	50	0.352	0.069
Opponents demonization	0.015 (0.049)	14810	4457	50	0.348	0.064
Pluralism commitment	−0.176 (0.099)*	14810	4457	50	0.372	0.098
Pro minority rights	−0.129 (0.063)**	14810	4457	50	0.395	0.130
Rejection of violence	−0.006 (0.078)	14810	4457	50	0.348	0.063
Pro immigration	−0.009 (0.021)	14810	4457	50	0.349	0.064
LGBT rights support	−0.035 (0.035)	14810	4457	50	0.355	0.072
Emphasis on cultural supremacy	−0.066 (0.045)	14810	4457	50	0.377	0.104
Reliance on religious principles	−0.000 (0.059)	14810	4457	50	0.348	0.063
Pro gender equality	−0.035 (0.036)	14810	4457	50	0.351	0.068
Emphasis on female labour issues	−0.033 (0.035)	14810	4457	50	0.355	0.073
Right-wing economic alignment	0.053 (0.033)	14810	4457	50	0.373	0.098
Pro welfare	−0.090 (0.051)*	14810	4457	50	0.375	0.101
Clientelism	0.213 (0.049)***	14810	4457	50	0.439	0.193
Presence of local branches	0.177 (0.055)***	14810	4457	50	0.394	0.129
Grassroot activism/communities	0.165 (0.058)***	14810	4457	50	0.391	0.125
Affiliations with social orgs	0.132 (0.042)***	14810	4457	50	0.399	0.136
Formal candidate selection	0.054 (0.057)	14810	4457	50	0.356	0.074
Stronger party discipline	−0.030 (0.036)	14810	4457	50	0.353	0.070
Party leader-centricity	0.066 (0.045)	14810	4457	50	0.368	0.092

Notes: Each row reports a separate OLS regression of the binary outcome (benefiting / losing from a weak coordination failure) on a single party trait, with constituency (*cst_n*) and year fixed effects. Standard errors are clustered at the party × year level. The sample varies slightly across traits owing to availability of the V–Party variables. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.4. Party-level OLS FE: Party Traits on Losing (Fixed Effects: *cst_n*, *year*)

Outcome: Losing from Weak Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Anti-Pluralism index	−1.170 (0.221)***	14810	4457	50	0.304	−0.001
Populism index	−0.229 (0.297)	14810	4457	50	0.210	−0.135
Seat share in lower chamber	−0.003 (0.003)	16800	5034	69	0.239	−0.093
Number of seats won in lower chamber	−0.000 (0.001)	16800	5034	69	0.226	−0.111
Total number of seats in lower chamber	0.000 (0.001)	16800	5034	69	0.225	−0.113
Vote share in lower chamber	−0.004 (0.004)	15161	4521	69	0.228	−0.107
Electoral continuity	−0.002 (0.136)	14810	4457	50	0.203	−0.146
Opposition	0.068 (0.034)*	14810	4457	50	0.234	−0.101
Pariah status	0.078 (0.031)**	14797	4457	50	0.252	−0.075
Anti-elitism	−0.029 (0.052)	14810	4457	50	0.207	−0.140
People-centrism	−0.094 (0.066)	14810	4457	50	0.229	−0.109
Opponents demonization	0.149 (0.058)**	14810	4457	50	0.259	−0.064
Pluralism commitment	0.328 (0.068)***	14810	4457	50	0.299	−0.008
Pro minority rights	0.097 (0.075)	14810	4457	50	0.233	−0.103
Rejection of violence	0.260 (0.054)***	14810	4457	50	0.299	−0.008
Pro immigration	0.061 (0.025)**	14810	4457	50	0.237	−0.096
LGBT rights support	0.060 (0.053)	14810	4457	50	0.225	−0.114
Emphasis on cultural supremacy	0.053 (0.038)	14810	4457	50	0.223	−0.117
Reliance on religious principles	0.022 (0.103)	14810	4457	50	0.203	−0.145
Pro gender equality	0.064 (0.057)	14810	4457	50	0.216	−0.127
Emphasis on female labour issues	0.039 (0.046)	14810	4457	50	0.214	−0.130
Right-wing economic alignment	−0.020 (0.032)	14810	4457	50	0.206	−0.141
Pro welfare	0.061 (0.048)	14810	4457	50	0.216	−0.126
Clientelism	−0.133 (0.065)**	14810	4457	50	0.243	−0.088
Presence of local branches	−0.131 (0.074)*	14810	4457	50	0.231	−0.105
Grassroot activism/communities	−0.047 (0.080)	14810	4457	50	0.207	−0.140
Affiliations with social orgs	−0.011 (0.068)	14810	4457	50	0.203	−0.146
Formal candidate selection	0.154 (0.050)***	14810	4457	50	0.276	−0.041
Stronger party discipline	−0.084 (0.046)*	14810	4457	50	0.244	−0.086
Party leader-centricity	−0.127 (0.037)***	14810	4457	50	0.288	−0.023

Notes: Each row reports a separate OLS regression of the binary outcome (benefiting / losing from a weak coordination failure) on a single party trait, with constituency (*cst_n*) and year fixed effects. Standard errors are clustered at the party × year level. The sample varies slightly across traits owing to availability of the V–Party variables. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.5. Party-level OLS FE: Party Traits on Benefiting (Fixed Effects: *cst_n*, *year*)

Outcome: Benefiting from Strong Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Anti-Pluralism index	1.756 (0.511)***	5924	2251	47	0.568	0.295
Populism index	−0.803 (0.483)	5924	2251	47	0.522	0.219
Seat share in lower chamber	0.009 (0.003)***	6820	2579	63	0.573	0.304
Number of seats won in lower chamber	0.002 (0.001)**	6820	2579	63	0.562	0.286
Total number of seats in lower chamber	0.001 (0.000)**	6820	2579	63	0.487	0.163
Vote share in lower chamber	0.011 (0.005)**	6031	2308	62	0.566	0.285
Electoral continuity	−0.204 (0.060)***	5924	2251	47	0.456	0.112
Opposition	−0.106 (0.063)*	5924	2251	47	0.507	0.194
Pariah status	0.033 (0.088)	5914	2251	47	0.450	0.101
Anti-elitism	−0.138 (0.079)*	5924	2251	47	0.521	0.217
People-centrism	−0.102 (0.125)	5924	2251	47	0.471	0.136
Opponents demonization	−0.052 (0.149)	5924	2251	47	0.450	0.102
Pluralism commitment	−0.380 (0.109)***	5924	2251	47	0.533	0.237
Pro minority rights	−0.257 (0.057)***	5924	2251	47	0.604	0.353
Rejection of violence	−0.050 (0.171)	5924	2251	47	0.448	0.098
Pro immigration	−0.098 (0.053)*	5924	2251	47	0.484	0.157
LGBT rights support	−0.158 (0.075)**	5924	2251	47	0.536	0.242
Emphasis on cultural supremacy	−0.172 (0.045)***	5924	2251	47	0.586	0.323
Reliance on religious principles	−0.218 (0.101)**	5924	2251	47	0.497	0.179
Pro gender equality	−0.090 (0.064)	5924	2251	47	0.462	0.122
Emphasis on female labour issues	−0.131 (0.063)**	5924	2251	47	0.531	0.233
Right-wing economic alignment	0.147 (0.039)***	5924	2251	47	0.598	0.344
Pro welfare	−0.184 (0.078)**	5924	2251	47	0.541	0.251
Clientelism	0.227 (0.080)***	5924	2251	47	0.546	0.259
Presence of local branches	0.182 (0.080)**	5924	2251	47	0.487	0.162
Grassroot activism/communities	0.202 (0.080)**	5924	2251	47	0.500	0.183
Affiliations with social orgs	0.089 (0.088)	5924	2251	47	0.462	0.121
Formal candidate selection	0.078 (0.123)	5924	2251	47	0.456	0.112
Stronger party discipline	−0.089 (0.060)	5924	2251	47	0.475	0.142
Party leader-centricity	0.163 (0.055)***	5924	2251	47	0.519	0.214

Notes: Each row reports a separate OLS regression of the binary outcome (benefiting / losing from a weak coordination failure) on a single party trait, with constituency (*cst_n*) and year fixed effects. Standard errors are clustered at the party × year level. The sample varies slightly across traits owing to availability of the V–Party variables. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.6. Party-level OLS FE: Party Traits on Losing (Fixed Effects: *cst_n, year*)

Outcome: Losing from Strong Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Anti-Pluralism index	−0.802 (0.316)**	5924	2251	47	0.262	−0.205
Populism index	0.200 (0.275)	5924	2251	47	0.237	−0.247
Seat share in lower chamber	−0.004 (0.003)	6820	2579	63	0.293	−0.154
Number of seats won in lower chamber	−0.001 (0.001)	6820	2579	63	0.279	−0.177
Total number of seats in lower chamber	0.001 (0.001)	6820	2579	63	0.267	−0.196
Vote share in lower chamber	−0.005 (0.006)	6031	2308	62	0.260	−0.219
Electoral continuity	0.078 (0.251)	5924	2251	47	0.233	−0.253
Opposition	0.085 (0.041)**	5924	2251	47	0.279	−0.178
Pariah status	0.050 (0.049)	5914	2251	47	0.244	−0.236
Anti-elitism	0.036 (0.043)	5924	2251	47	0.237	−0.246
People-centrism	0.001 (0.077)	5924	2251	47	0.231	−0.256
Opponents demonization	0.089 (0.095)	5924	2251	47	0.247	−0.231
Pluralism commitment	0.203 (0.071)***	5924	2251	47	0.261	−0.207
Pro minority rights	0.007 (0.085)	5924	2251	47	0.231	−0.256
Rejection of violence	0.189 (0.094)*	5924	2251	47	0.263	−0.204
Pro immigration	0.058 (0.027)**	5924	2251	47	0.247	−0.230
LGBT rights support	0.023 (0.092)	5924	2251	47	0.233	−0.252
Emphasis on cultural supremacy	0.022 (0.051)	5924	2251	47	0.234	−0.252
Reliance on religious principles	−0.027 (0.105)	5924	2251	47	0.232	−0.254
Pro gender equality	0.025 (0.099)	5924	2251	47	0.233	−0.254
Emphasis on female labour issues	0.024 (0.069)	5924	2251	47	0.235	−0.250
Right-wing economic alignment	−0.017 (0.046)	5924	2251	47	0.234	−0.252
Pro welfare	0.057 (0.070)	5924	2251	47	0.242	−0.238
Clientelism	−0.070 (0.090)	5924	2251	47	0.243	−0.237
Presence of local branches	−0.137 (0.125)	5924	2251	47	0.259	−0.210
Grassroot activism/communities	−0.136 (0.109)	5924	2251	47	0.261	−0.207
Affiliations with social orgs	−0.060 (0.071)	5924	2251	47	0.240	−0.242
Formal candidate selection	0.108 (0.073)	5924	2251	47	0.255	−0.217
Stronger party discipline	−0.005 (0.080)	5924	2251	47	0.231	−0.256
Party leader-centricity	−0.130 (0.052)**	5924	2251	47	0.288	−0.164

Notes: Each row reports a separate OLS regression of the binary outcome (benefiting / losing from a weak coordination failure) on a single party trait, with constituency (*cst_n*) and year fixed effects. Standard errors are clustered at the party × year level. The sample varies slightly across traits owing to availability of the V–Party variables. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.7. Party-level WLS FE: Party Traits on Benefiting (Fixed Effects: *cst_n, year*)

Outcome: Benefiting from Weak Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Anti-Pluralism index	0.614 (0.451)	14810	4457	50	0.373	0.098
Populism index	−0.491 (0.297)	14810	4457	50	0.378	0.106
Seat share in lower chamber	0.009 (0.002)***	16800	5034	69	0.480	0.253
Number of seats won in lower chamber	0.002 (0.001)***	16800	5034	69	0.466	0.233
Total number of seats in lower chamber	0.000 (0.000)***	16800	5034	69	0.379	0.108
Vote share in lower chamber	0.013 (0.003)***	15161	4521	69	0.470	0.239
Electoral continuity	−0.050 (0.086)	14810	4457	50	0.349	0.064
Opposition	−0.118 (0.034)***	14810	4457	50	0.432	0.183
Pariah status	0.033 (0.036)	14797	4457	50	0.356	0.074
Anti-elitism	−0.094 (0.048)*	14810	4457	50	0.387	0.119
People-centrism	−0.040 (0.081)	14810	4457	50	0.352	0.069
Opponents demonization	0.015 (0.049)	14810	4457	50	0.348	0.064
Pluralism commitment	−0.176 (0.099)*	14810	4457	50	0.372	0.098
Pro minority rights	−0.129 (0.063)**	14810	4457	50	0.395	0.130
Rejection of violence	−0.006 (0.078)	14810	4457	50	0.348	0.063
Pro immigration	−0.009 (0.021)	14810	4457	50	0.349	0.064
LGBT rights support	−0.035 (0.035)	14810	4457	50	0.355	0.072
Emphasis on cultural supremacy	−0.066 (0.045)	14810	4457	50	0.377	0.104
Reliance on religious principles	−0.000 (0.059)	14810	4457	50	0.348	0.063
Pro gender equality	−0.035 (0.036)	14810	4457	50	0.351	0.068
Emphasis on female labour issues	−0.033 (0.035)	14810	4457	50	0.355	0.073
Right-wing economic alignment	0.053 (0.033)	14810	4457	50	0.373	0.098
Pro welfare	−0.090 (0.051)*	14810	4457	50	0.375	0.101
Clientelism	0.213 (0.049)***	14810	4457	50	0.439	0.193
Presence of local branches	0.177 (0.055)***	14810	4457	50	0.394	0.129
Grassroot activism/communities	0.165 (0.058)***	14810	4457	50	0.391	0.125
Affiliations with social orgs	0.132 (0.042)***	14810	4457	50	0.399	0.136
Formal candidate selection	0.054 (0.057)	14810	4457	50	0.356	0.074
Stronger party discipline	−0.030 (0.036)	14810	4457	50	0.353	0.070
Party leader-centricity	0.066 (0.045)	14810	4457	50	0.368	0.092

Notes: Observations are weighted by the inverse of the number of parties contesting each election to ensure that large multi-party elections do not mechanically dominate the estimates. Specification and clustering are otherwise identical to the OLS tables. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.8. Party-level WLS FE: Party Traits on Losing (Fixed Effects: *cst_n, year*)

Outcome: Losing from Weak Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Anti-Pluralism index	−1.170 (0.221)***	14810	4457	50	0.304	−0.001
Populism index	−0.229 (0.297)	14810	4457	50	0.210	−0.135
Seat share in lower chamber	−0.003 (0.003)	16800	5034	69	0.239	−0.093
Number of seats won in lower chamber	−0.000 (0.001)	16800	5034	69	0.226	−0.111
Total number of seats in lower chamber	0.000 (0.001)	16800	5034	69	0.225	−0.113
Vote share in lower chamber	−0.004 (0.004)	15161	4521	69	0.228	−0.107
Electoral continuity	−0.002 (0.136)	14810	4457	50	0.203	−0.146
Opposition	0.068 (0.034)*	14810	4457	50	0.234	−0.101
Pariah status	0.078 (0.031)**	14797	4457	50	0.252	−0.075
Anti-elitism	−0.029 (0.052)	14810	4457	50	0.207	−0.140
People-centrism	−0.094 (0.066)	14810	4457	50	0.229	−0.109
Opponents demonization	0.149 (0.058)**	14810	4457	50	0.259	−0.064
Pluralism commitment	0.328 (0.068)***	14810	4457	50	0.299	−0.008
Pro minority rights	0.097 (0.075)	14810	4457	50	0.233	−0.103
Rejection of violence	0.260 (0.054)***	14810	4457	50	0.299	−0.008
Pro immigration	0.061 (0.025)**	14810	4457	50	0.237	−0.096
LGBT rights support	0.060 (0.053)	14810	4457	50	0.225	−0.114
Emphasis on cultural supremacy	0.053 (0.038)	14810	4457	50	0.223	−0.117
Reliance on religious principles	0.022 (0.103)	14810	4457	50	0.203	−0.145
Pro gender equality	0.064 (0.057)	14810	4457	50	0.216	−0.127
Emphasis on female labour issues	0.039 (0.046)	14810	4457	50	0.214	−0.130
Right-wing economic alignment	−0.020 (0.032)	14810	4457	50	0.206	−0.141
Pro welfare	0.061 (0.048)	14810	4457	50	0.216	−0.126
Clientelism	−0.133 (0.065)**	14810	4457	50	0.243	−0.088
Presence of local branches	−0.131 (0.074)*	14810	4457	50	0.231	−0.105
Grassroot activism/communities	−0.047 (0.080)	14810	4457	50	0.207	−0.140
Affiliations with social orgs	−0.011 (0.068)	14810	4457	50	0.203	−0.146
Formal candidate selection	0.154 (0.050)***	14810	4457	50	0.276	−0.041
Stronger party discipline	−0.084 (0.046)*	14810	4457	50	0.244	−0.086
Party leader-centricity	−0.127 (0.037)***	14810	4457	50	0.288	−0.023

Notes: Observations are weighted by the inverse of the number of parties contesting each election to ensure that large multi-party elections do not mechanically dominate the estimates. Specification and clustering are otherwise identical to the OLS tables. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.9. Party-level WLS FE: Party Traits on Benefiting (Fixed Effects: *cst_n, year*)

Outcome: Benefiting from Strong Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Anti-Pluralism index	1.777 (0.519)***	5924	2251	47	0.583	0.319
Populism index	−0.821 (0.492)	5924	2251	47	0.537	0.244
Seat share in lower chamber	0.009 (0.003)***	6820	2579	63	0.586	0.324
Number of seats won in lower chamber	0.002 (0.001)**	6820	2579	63	0.576	0.307
Total number of seats in lower chamber	0.001 (0.000)**	6820	2579	63	0.499	0.182
Vote share in lower chamber	0.011 (0.005)**	6031	2308	62	0.577	0.303
Electoral continuity	−0.204 (0.060)***	5924	2251	47	0.467	0.130
Opposition	−0.108 (0.064)*	5924	2251	47	0.520	0.216
Pariah status	0.035 (0.089)	5914	2251	47	0.462	0.120
Anti-elitism	−0.142 (0.081)*	5924	2251	47	0.536	0.243
People-centrism	−0.104 (0.126)	5924	2251	47	0.484	0.156
Opponents demonization	−0.053 (0.152)	5924	2251	47	0.461	0.120
Pluralism commitment	−0.383 (0.110)***	5924	2251	47	0.546	0.259
Pro minority rights	−0.258 (0.058)***	5924	2251	47	0.618	0.375
Rejection of violence	−0.049 (0.174)	5924	2251	47	0.458	0.115
Pro immigration	−0.101 (0.056)*	5924	2251	47	0.497	0.178
LGBT rights support	−0.161 (0.078)**	5924	2251	47	0.550	0.265
Emphasis on cultural supremacy	−0.176 (0.046)***	5924	2251	47	0.602	0.350
Reliance on religious principles	−0.222 (0.104)**	5924	2251	47	0.510	0.200
Pro gender equality	−0.089 (0.066)	5924	2251	47	0.473	0.139
Emphasis on female labour issues	−0.133 (0.065)**	5924	2251	47	0.545	0.256
Right-wing economic alignment	0.149 (0.040)***	5924	2251	47	0.615	0.371
Pro welfare	−0.189 (0.080)**	5924	2251	47	0.557	0.276
Clientelism	0.226 (0.082)***	5924	2251	47	0.558	0.278
Presence of local branches	0.184 (0.083)**	5924	2251	47	0.498	0.181
Grassroot activism/communities	0.206 (0.082)**	5924	2251	47	0.513	0.205
Affiliations with social orgs	0.091 (0.090)	5924	2251	47	0.474	0.140
Formal candidate selection	0.082 (0.125)	5924	2251	47	0.468	0.131
Stronger party discipline	−0.092 (0.061)	5924	2251	47	0.488	0.163
Party leader-centricity	0.166 (0.056)***	5924	2251	47	0.532	0.236

Notes: Observations are weighted by the inverse of the number of parties contesting each election to ensure that large multi-party elections do not mechanically dominate the estimates. Specification and clustering are otherwise identical to the OLS tables. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.10. Party-level WLS FE: Party Traits on Losing (Fixed Effects: *cst_n*, *year*)

Outcome: Losing from Strong Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Anti-Pluralism index	−0.788 (0.325)**	5924	2251	47	0.282	−0.173
Populism index	0.212 (0.269)	5924	2251	47	0.256	−0.215
Seat share in lower chamber	−0.005 (0.003)	6820	2579	63	0.315	−0.118
Number of seats won in lower chamber	−0.001 (0.001)	6820	2579	63	0.301	−0.141
Total number of seats in lower chamber	0.001 (0.001)	6820	2579	63	0.288	−0.163
Vote share in lower chamber	−0.006 (0.006)	6031	2308	62	0.277	−0.191
Electoral continuity	0.086 (0.265)	5924	2251	47	0.252	−0.222
Opposition	0.088 (0.042)**	5924	2251	47	0.303	−0.139
Pariah status	0.050 (0.050)	5914	2251	47	0.263	−0.205
Anti-elitism	0.035 (0.044)	5924	2251	47	0.256	−0.216
People-centrism	0.008 (0.074)	5924	2251	47	0.250	−0.226
Opponents demonization	0.085 (0.099)	5924	2251	47	0.264	−0.201
Pluralism commitment	0.200 (0.074)***	5924	2251	47	0.281	−0.175
Pro minority rights	0.005 (0.085)	5924	2251	47	0.250	−0.226
Rejection of violence	0.186 (0.096)*	5924	2251	47	0.282	−0.172
Pro immigration	0.056 (0.031)*	5924	2251	47	0.265	−0.200
LGBT rights support	0.021 (0.098)	5924	2251	47	0.251	−0.223
Emphasis on cultural supremacy	0.020 (0.052)	5924	2251	47	0.252	−0.222
Reliance on religious principles	−0.033 (0.108)	5924	2251	47	0.251	−0.223
Pro gender equality	0.018 (0.109)	5924	2251	47	0.250	−0.225
Emphasis on female labour issues	0.019 (0.074)	5924	2251	47	0.252	−0.223
Right-wing economic alignment	−0.014 (0.047)	5924	2251	47	0.251	−0.223
Pro welfare	0.054 (0.075)	5924	2251	47	0.260	−0.210
Clientelism	−0.069 (0.090)	5924	2251	47	0.261	−0.206
Presence of local branches	−0.133 (0.129)	5924	2251	47	0.277	−0.181
Grassroot activism/communities	−0.124 (0.116)	5924	2251	47	0.275	−0.185
Affiliations with social orgs	−0.064 (0.072)	5924	2251	47	0.260	−0.209
Formal candidate selection	0.110 (0.074)	5924	2251	47	0.276	−0.183
Stronger party discipline	−0.006 (0.083)	5924	2251	47	0.250	−0.226
Party leader-centricity	−0.131 (0.054)**	5924	2251	47	0.309	−0.128

Notes: Observations are weighted by the inverse of the number of parties contesting each election to ensure that large multi-party elections do not mechanically dominate the estimates. Specification and clustering are otherwise identical to the OLS tables. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.11. Election-level OLS FE: Predictors of Coordination Failure

Outcome: Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Avg distance (all)	−0.02 (0.01)**	20282	9136	45	0.525	0.132
Avg within Left	0.07 (0.01)***	7999	3640	38	0.544	0.155
Avg within Center	−0.02 (0.06)	1489	1390	18	0.953	0.129
Avg within Right	−0.02 (0.00)***	6194	3683	32	0.742	0.355
Overlap index	0.07 (0.38)	20282	9136	45	0.524	0.131
SD of Left-Right	−0.07 (0.02)***	20282	9136	45	0.526	0.134
IQR of Left-Right	0.21 (0.10)*	3041	2212	33	0.791	0.200
Left–Right centroid gap	−0.04 (0.03)	11899	4909	43	0.486	0.120
Outlier parties	0.45 (0.22)**	20287	9141	45	0.524	0.131
Max outlier z	0.06 (0.10)	20282	9136	45	0.524	0.131
Max mean distance	−0.02 (0.01)*	20282	9136	45	0.525	0.132
Effective N Parties (L-T index) (votes)	0.21 (0.04)***	18906	7886	45	0.496	0.131
Effective N Parties (L-T index) (seats)	0.11 (0.04)***	20282	9136	45	0.526	0.135
HHI (votes)	−1.10 (0.19)***	18906	7886	45	0.496	0.132
HHI (seats)	−0.40 (0.09)***	20282	9136	45	0.528	0.137
Shannon entropy (votes)	0.90 (0.10)***	18906	7886	45	0.497	0.134
Shannon entropy (seats)	0.28 (0.06)***	20282	9136	45	0.528	0.137
Share new parties	−0.09 (0.26)	20282	9136	45	0.524	0.131
Share renamed parties	−0.47 (0.20)**	20282	9136	45	0.525	0.131
Share continuing parties	0.21 (0.35)	20282	9136	45	0.524	0.131
Vote share new	−0.10 (0.16)	18906	7886	45	0.490	0.121
Vote share renamed	−0.39 (0.17)**	18906	7886	45	0.490	0.122
Vote share continuing	0.19 (0.24)	18906	7886	45	0.490	0.122
Share of right-wing parties in election	0.01 (0.14)	20282	9136	45	0.524	0.131
Share of left-wing parties in election	0.07 (0.21)	20282	9136	45	0.524	0.131
Gov vote share	−0.11 (0.11)	18906	7886	45	0.491	0.123
Gov seat share	−0.12 (0.07)	20282	9136	45	0.526	0.134
Gov right-left ideology (seat-weighted)	0.00 (0.03)	15496	7797	44	0.599	0.187

Notes: Each row reports a separate regression of the weak (or strong) coordination failure indicator on a single election-level predictor, controlling for $\log(n\_parties + 1)$ and including constituency, year, and source fixed effects. Fragmentation measures (Laakso–Taagepera index, HHI, Shannon entropy) are computed from vote and seat shares. Party-continuity indicators are based on whether parties are new entrants, renamed, or continuing across consecutive elections. Ideological dispersion measures are derived from RILE scores (Manifesto Project) or V–Party indices. Standard errors are clustered two-way at the country × year level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.12. Election-level Logit FE: Predictors of Coordination Failure

Outcome: Coordination Failure (=1)							
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	Deviance	Log Likelihood	Pseudo R^2
Avg distance (all)	−0.23 (0.06)***	7906	1853	43	8927.63	−4463.82	0.14
Avg within Left	0.95 (0.21)***	3513	873	32	3936.15	−1968.08	0.17
Avg within Center	−1.36 (0.00)***	59	31	8	33.13	−16.57	0.59
Avg within Right	−0.14 (0.18)	1011	438	27	1019.48	−509.74	0.27
Overlap index	1.74 (2.36)	7906	1853	43	8955.55	−4477.77	0.14
SD of Left-Right	−0.69 (0.16)***	7906	1853	43	8904.74	−4452.37	0.14
IQR of Left-Right	1.70 (0.47)***	638	201	24	685.91	−342.96	0.22
Left–Right centroid gap	−0.32 (0.22)	5349	1195	40	5988.29	−2994.14	0.16
Outlier parties	24.81 (3.73)***	7906	1853	43	8953.61	−4476.81	0.14
Max outlier z	0.48 (0.65)	7906	1853	43	8947.42	−4473.71	0.14
Max mean distance	−0.24 (0.09)**	7906	1853	43	8921.98	−4460.99	0.14
Effective N Parties (L-T index) (votes)	1.99 (0.46)***	7847	1824	43	8728.12	−4364.06	0.15
Effective N Parties (L-T index) (seats)	1.22 (0.51)**	7906	1853	43	8865.40	−4432.70	0.15
HHI (votes)	−12.38 (2.38)***	7847	1824	43	8689.00	−4344.50	0.16
HHI (seats)	−4.32 (1.07)***	7906	1853	43	8825.15	−4412.58	0.15
Shannon entropy (votes)	9.87 (1.32)***	7847	1824	43	8661.13	−4330.57	0.16
Shannon entropy (seats)	3.11 (0.82)***	7906	1853	43	8825.16	−4412.58	0.15
Share new parties	−0.13 (1.43)	7906	1853	43	8956.76	−4478.38	0.14
Share renamed parties	−3.73 (1.67)**	7906	1853	43	8946.67	−4473.33	0.14
Share continuing parties	0.95 (2.15)	7906	1853	43	8953.71	−4476.85	0.14
Vote share new	−0.42 (0.81)	7847	1824	43	8880.49	−4440.24	0.14
Vote share renamed	−3.01 (1.26)**	7847	1824	43	8872.51	−4436.26	0.14
Vote share continuing	1.10 (1.37)	7847	1824	43	8875.50	−4437.75	0.14
Share of right-wing parties in election	0.02 (0.90)	7906	1853	43	8956.83	−4478.41	0.14
Share of left-wing parties in election	0.35 (1.52)	7906	1853	43	8955.40	−4477.70	0.14
Gov vote share	−1.18 (0.66)*	7847	1824	43	8833.34	−4416.67	0.14
Gov seat share	−1.05 (0.38)***	7906	1853	43	8890.65	−4445.33	0.14
Gov right-left ideology (seat-weighted)	−0.20 (0.11)*	5027	1307	43	5703.62	−2851.81	0.15

Notes: Each row reports a separate regression of the weak (or strong) coordination failure indicator on a single election-level predictor, controlling for $\log(n\_parties + 1)$ and including constituency, year, and source fixed effects. Fragmentation measures (Laakso–Taagepera index, HHI, Shannon entropy) are computed from vote and seat shares. Party-continuity indicators are based on whether parties are new entrants, renamed, or continuing across consecutive elections. Ideological dispersion measures are derived from RILE scores (Manifesto Project) or V–Party indices. Standard errors are clustered two-way at the country × year level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.13. Election-level OLS FE: Predictors of Strong Coordination Failure

Outcome: Strong Coordination Failure (=1)						
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	R^2	Adj. R^2
Avg distance (all)	−0.00 (0.01)	20282	9136	45	0.528	0.138
Avg within Left	0.04 (0.03)	7999	3640	38	0.557	0.179
Avg within Center	−0.02 (0.06)	1489	1390	18	0.949	0.054
Avg within Right	0.01 (0.02)	6194	3683	32	0.765	0.412
Overlap index	0.00 (0.29)	20282	9136	45	0.528	0.138
SD of Left-Right	−0.03 (0.02)	20282	9136	45	0.529	0.139
IQR of Left-Right	0.03 (0.04)	3041	2212	33	0.837	0.376
Left–Right centroid gap	−0.01 (0.03)	11899	4909	43	0.457	0.070
Outlier parties	0.24 (0.24)	20287	9141	45	0.528	0.137
Max outlier z	0.04 (0.03)	20282	9136	45	0.528	0.138
Max mean distance	−0.00 (0.01)	20282	9136	45	0.528	0.138
Effective N Parties (L-T index) (votes)	0.03 (0.03)	18906	7886	45	0.484	0.112
Effective N Parties (L-T index) (seats)	0.00 (0.03)	20282	9136	45	0.528	0.138
HHI (votes)	−0.33 (0.12)**	18906	7886	45	0.485	0.113
HHI (seats)	−0.05 (0.08)	20282	9136	45	0.528	0.138
Shannon entropy (votes)	0.21 (0.08)**	18906	7886	45	0.485	0.113
Shannon entropy (seats)	0.03 (0.06)	20282	9136	45	0.528	0.138
Share new parties	0.20 (0.09)**	20282	9136	45	0.529	0.139
Share renamed parties	−0.40 (0.21)*	20282	9136	45	0.529	0.139
Share continuing parties	−0.14 (0.10)	20282	9136	45	0.528	0.138
Vote share new	0.06 (0.11)	18906	7886	45	0.484	0.111
Vote share renamed	−0.36 (0.16)**	18906	7886	45	0.485	0.112
Vote share continuing	−0.01 (0.09)	18906	7886	45	0.484	0.111
Share of right-wing parties in election	−0.09 (0.04)**	20282	9136	45	0.528	0.138
Share of left-wing parties in election	−0.00 (0.04)	20282	9136	45	0.528	0.138
Gov vote share	0.01 (0.02)	18906	7886	45	0.484	0.111
Gov seat share	0.02 (0.02)	20282	9136	45	0.528	0.138
Gov right-left ideology (seat-weighted)	0.03 (0.03)	15496	7797	44	0.577	0.144

Notes: Each row reports a separate regression of the weak (or strong) coordination failure indicator on a single election-level predictor, controlling for $\log(n\_parties + 1)$ and including constituency, year, and source fixed effects. Fragmentation measures (Laakso–Taagepera index, HHI, Shannon entropy) are computed from vote and seat shares. Party-continuity indicators are based on whether parties are new entrants, renamed, or continuing across consecutive elections. Ideological dispersion measures are derived from RILE scores (Manifesto Project) or V–Party indices. Standard errors are clustered two-way at the country × year level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.14. Election-level Logit FE: Predictors of Strong Coordination Failure

Outcome: Strong Coordination Failure (=1)							
Variable	Coef (SE)	N	Groups: cst_n	Groups: year	Deviance	Log Likelihood	Pseudo R^2
Avg distance (all)	−0.13 (0.05)***	3889	795	40	3608.31	−1804.15	0.21
Avg within Left	0.94 (0.51)*	1596	392	28	1469.77	−734.89	0.27
Avg within Center	−1.59 (0.00)***	43	23	8	28.23	−14.12	0.52
Avg within Right	79.21 (10.43)***	279	113	24	288.10	−144.05	0.24
Overlap index	0.34 (3.01)	3889	795	40	3612.08	−1806.04	0.21
SD of Left-Right	−0.51 (0.31)	3889	795	40	3600.18	−1800.09	0.21
IQR of Left-Right	0.73 (1.24)	212	66	21	182.46	−91.23	0.35
Left–Right centroid gap	−0.20 (0.28)	2695	592	35	2568.57	−1284.29	0.22
Outlier parties	18.25 (21.92)	3889	795	40	3609.56	−1804.78	0.21
Max outlier z	0.23 (0.52)	3889	795	40	3611.35	−1805.67	0.21
Max mean distance	−0.17 (0.07)***	3889	795	40	3603.97	−1801.98	0.21
Effective N Parties (L-T index) (votes)	0.75 (0.53)	3801	779	39	3534.65	−1767.32	0.21
Effective N Parties (L-T index) (seats)	0.41 (0.58)	3889	795	40	3607.88	−1803.94	0.21
HHI (votes)	−8.79 (2.69)***	3801	779	39	3502.13	−1751.06	0.22
HHI (seats)	−2.06 (2.05)	3889	795	40	3602.31	−1801.15	0.21
Shannon entropy (votes)	5.04 (2.07)**	3801	779	39	3517.73	−1758.87	0.22
Shannon entropy (seats)	1.37 (1.28)	3889	795	40	3603.28	−1801.64	0.21
Share new parties	2.37 (2.03)	3889	795	40	3605.40	−1802.70	0.21
Share renamed parties	−7.40 (8.38)	3889	795	40	3605.69	−1802.84	0.21
Share continuing parties	−1.80 (2.25)	3889	795	40	3608.70	−1804.35	0.21
Vote share new	−0.15 (1.68)	3801	779	39	3545.32	−1772.66	0.21
Vote share renamed	−7.20 (6.96)	3801	779	39	3538.61	−1769.31	0.21
Vote share continuing	0.77 (1.61)	3801	779	39	3544.29	−1772.15	0.21
Share of right-wing parties in election	−0.72 (0.80)	3889	795	40	3610.06	−1805.03	0.21
Share of left-wing parties in election	−0.85 (0.62)	3889	795	40	3609.22	−1804.61	0.21
Gov vote share	−0.01 (0.62)	3801	779	39	3545.37	−1772.68	0.21
Gov seat share	0.44 (0.35)	3889	795	40	3608.06	−1804.03	0.21
Gov right-left ideology (seat-weighted)	0.12 (0.48)	2745	638	40	2529.96	−1264.98	0.24

Notes: Each row reports a separate regression of the weak (or strong) coordination failure indicator on a single election-level predictor, controlling for $\log(n\_parties + 1)$ and including constituency, year, and source fixed effects. Fragmentation measures (Laakso–Taagepera index, HHI, Shannon entropy) are computed from vote and seat shares. Party-continuity indicators are based on whether parties are new entrants, renamed, or continuing across consecutive elections. Ideological dispersion measures are derived from RILE scores (Manifesto Project) or V–Party indices. Standard errors are clustered two-way at the country × year level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.15. Attrition from baseline to follow-up

Panel A. Attrition rate, overall and by state

	Baseline N	Attrited	Completed	Rate
All states	4,712	686	4,026	14.6%
Telangana	1,044	71	973	6.8%
Uttar Pradesh	1,189	126	1,063	10.6%
Bihar	2,001	314	1,687	15.7%
Delhi	478	175	303	36.6%

χ^2 of equal attrition across states: $\chi^2_{(3)} = 254.5$, $p < 0.001$

Panel B. Baseline characteristics: retained vs. attrited

	Retained (1)	Attrited (2)	Diff. (2)−(1)	p
Male	0.807	0.761	−0.046	0.009
Age	39.41	38.70	−0.702	0.183
Hindu	0.828	0.765	−0.063	0.000
High school or lower	0.388	0.382	−0.006	0.754
Caste: General	0.240	0.270	+0.029	0.106
Caste: SC	0.149	0.175	+0.026	0.100
Caste: ST	0.069	0.029	−0.039	0.000
Caste: OBC	0.424	0.448	+0.024	0.247
Salaried employee	0.195	0.238	+0.043	0.015
Own business	0.148	0.168	+0.020	0.190
Farming	0.199	0.118	−0.081	0.000
Farm labour	0.063	0.050	−0.014	0.139
Expressed baseline vote pref.	0.646	0.660	+0.014	0.466
Interested in polls (baseline)	1.247	1.236	−0.010	0.551

*Panel C. Determinants of attrition (LPM, dep. var. = $\mathbf{1}$[*attrited*])*

	Coef.	Std. err.
Male	−0.016	(0.015)
Age	−0.000	(0.000)
Hindu	−0.009	(0.015)
High school or lower	−0.008	(0.011)
Caste: SC	+0.041**	(0.016)
Caste: ST	−0.022	(0.018)
Caste: OBC	+0.016	(0.011)
Salaried employee	+0.007	(0.015)
Own business	+0.019	(0.016)
Farming	−0.012	(0.014)
Farm labour	+0.006	(0.022)
Expressed baseline vote pref.	+0.003	(0.011)
State fixed effects	Yes	
Observations	4,712	
Joint F, no state FE	$F = 6.73$, $p < 0.001$ ($R^2 = 0.014$)	
Joint F, with state FE	$F = 1.58$, $p = 0.091$ ($R^2 = 0.057$)	

Notes. A respondent is *attrited* if they completed the baseline but no follow-up survey. Treatment is assigned and delivered during the follow-up call, so attrition occurs prior to randomization: none of the 686 attriters has a recorded treatment arm, and differential attrition across arms is not defined. Panel A reports raw attrition rates. Panel B compares baseline characteristics of retained and attrited respondents; p-values are from two-sided t-tests with unequal variances. Panel C reports a linear-probability regression of the attrition indicator on baseline characteristics with state fixed effects and heteroskedasticity-robust standard errors; the joint F-tests assess whether the covariates jointly predict attrition without and with state fixed effects. Stars: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.16. Balance checks

		Control			Treated			Difference	
	Obs	Obs	Mean	s.d.	Obs	Mean	s.d.	T - C	p-value
				Demographics					
Male	4,027	1,821	0.80	0.40	2,206	0.81	0.39	0.012	0.348
Age	4,027	1,821	39.06	12.85	2,206	39.69	12.73	0.627	0.122
Hindu	4,027	1,821	0.82	0.39	2,206	0.84	0.37	0.022*	0.064
High school or lower	4,027	1,821	0.40	0.49	2,206	0.38	0.49	-0.017	0.264
Caste									
General	4,027	1,821	0.25	0.43	2,206	0.24	0.42	-0.010	0.472
SC	4,027	1,821	0.14	0.35	2,206	0.16	0.36	0.013	0.255
ST	4,027	1,821	0.06	0.24	2,206	0.07	0.26	0.011	0.173
OBC	4,027	1,821	0.43	0.50	2,206	0.42	0.49	-0.015	0.351
Profession									
Salaried Employee	4,027	1,821	0.21	0.40	2,206	0.19	0.39	-0.019	0.130
Own Business	4,027	1,821	0.15	0.36	2,206	0.14	0.35	-0.008	0.455
Farming	4,027	1,821	0.19	0.39	2,206	0.21	0.40	0.013	0.298
Farm Labour	4,027	1,821	0.07	0.25	2,206	0.06	0.24	-0.008	0.292

Notes. The table reports means and standard deviations of pre-treatment demographic characteristics separately for the control and treatment groups, pooled across all four state experiments (Bihar, Delhi, Telangana, Uttar Pradesh). The column "T−C" reports the treatment–control difference in means and the associated p-value from a two-sided t-test. Assignment to treatment was random within each experiment. The only covariate that approaches conventional significance ($p < 0.10$) is the share of Hindu respondents; all regressions in the main analysis control for religious composition. Stars: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.17. Comparison of perceived vs. actual constituency-level vote-share distributions across states.

city	chi2_stat	chi2_p	ks_stat	ks_p	share_correct_top2
Bihar	41.02	0.00	0.33	0.93	0.27
Delhi	309.66	0.00	0.40	0.87	0.74
Telangana	54.22	0.00	0.33	1.00	0.59
UP	131.23	0.00	0.25	1.00	0.67

Panel A. Chi-square and KS statistics for equality between perceived and actual vote-share distributions, and share of respondents correctly identifying top-two parties.

city	spearman_rho	pearson_r	top2_overlap	herf_actual	herf_perceived	delta_herf
Bihar	0.89	0.96	1	0.25	0.25	-0.00
Delhi	1.00	0.99	2	0.41	0.35	-0.06
Telangana	1.00	0.99	2	0.41	0.36	-0.05
UP	0.80	0.95	2	0.53	0.43	-0.10

Panel B. Goodness-of-fit and structural comparisons between perceived and actual vote-share distributions. Rank correlations indicate accurate ordering of parties, while lower perceived concentration shows systematic underestimation of leading-party dominance.

Table C.18. Perceived vs Actual Vote Shares (scaled), Four States

Panel A: Bihar

Party	Perceived share	Actual share	Abs. error	Rel. error	Bias (perc - act)
jdu	31.20	32.20	1.00	3.10	-1.00
rjd	26.60	31.40	4.70	15.10	-4.70
bjp	26.70	20.10	6.60	32.90	6.60
ham	5.10	7.60	2.50	32.70	-2.50
jsp	7.30	6.30	1.00	15.20	1.00
inc	3.20	2.50	0.60	25.60	0.60

Panel B: Delhi

Party	Perceived share	Actual share	Abs. error	Rel. error	Bias (perc - act)
bjp	46.70	48.60	1.90	3.90	-1.90
aap	33.80	40.20	6.40	16.00	-6.40
inc	12.10	8.40	3.70	44.30	3.70
aimim	4.20	2.50	1.70	69.60	1.70
bsp	3.20	0.30	2.90	936.00	2.90

Panel C: Telangana

Party	Perceived share	Actual share	Abs. error	Rel. error	Bias (perc - act)
bjp	43.50	52.90	9.40	17.80	-9.40
inc	35.20	32.90	2.30	7.00	2.30
brs	21.30	14.20	7.10	50.40	7.10

Panel D: Uttar Pradesh

Party	Perceived share	Actual share	Abs. error	Rel. error	Bias (perc - act)
bjp	56.80	69.40	12.60	18.10	-12.60
sp	31.50	19.70	11.80	59.60	11.80
inc	4.80	7.20	2.50	33.90	-2.50
bsp	6.90	3.60	3.30	90.60	3.30

Notes: Each panel reports, for every major party in the relevant state, the mean perceived vote share (from second-order belief questions at baseline), the actual vote share from official election results (rescaled to exclude residual "other" parties), the absolute error $|\text{perceived} - \text{actual}|$, the relative error as a percentage of the actual share, and the signed bias (perceived $-$ actual). All shares are expressed in percentage points. Positive bias values indicate overestimation; negative values indicate underestimation. Bihar: JDU, RJD, BJP, HAM, JSP, INC. Delhi: BJP, AAP, INC, AIMIM, BSP. Telangana: BJP, INC, BRS. Uttar Pradesh: BJP, SP, INC, BSP.

Table C.19. Second order beliefs: Treatment effect on pooled sample

Dep. variable	(1)	(2)	(3)
	Perceived vote share		
Top 2 party × Post	-0.319	-4.202*	1.786
	(1.598)	(2.340)	(2.049)
Treated × Top 2 party × Post	0.987	1.085	0.725
	(0.664)	(0.868)	(0.932)
$\mathbb{E}(y)$	20.894	22.238	19.865
	(22.879)	(24.879)	(21.164)
Sample	Full	Top 2 voters	Other voters
Adjusted-R^2	0.439	0.504	0.385
F statistic	1.118	1.994	0.928
Obs	37,678	16,336	21,342
Party FE	Yes	Yes	Yes
Respondent FE	Yes	Yes	Yes
Respondent FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Respondent × Party FE	Yes	Yes	Yes
Respondent × Time FE	Yes	Yes	Yes
Party × Time FE	Yes	Yes	Yes

Notes. The dependent variable is the respondent's stated perceived vote share for each party (from the second-order beliefs question). The key coefficient of interest is Treated × Top-2 party × Post, which captures whether exposure to poll information differentially shifts perceptions of the top-two parties' vote shares relative to other parties. All models include party, respondent, and time fixed effects, as well as respondent × party and respondent × time interactions. Columns 1–3 report estimates for the full sample, for top-2 voters, and for other respondents, respectively. Standard errors are clustered at the respondent level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.20. Second-order beliefs: Treatment effect by baseline over-estimation of non-top-2 share

Dep. variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Voting Intentions			Party Ranking			Party Rating		
Top 2 party × Post	-0.026	-0.098**	0.063***	-0.267***	-0.030	-0.570***	0.274***	-0.085	0.617***
	(0.024)	(0.048)	(0.024)	(0.070)	(0.091)	(0.108)	(0.091)	(0.128)	(0.127)
Overestimates non-top-2 share × Top 2 party × Post	-0.020		-0.177***	0.365		0.536	-0.108		-0.170
	(0.014)		(0.017)	(0.531)		(0.531)	(0.309)		(0.328)
Treated × Top 2 party × Post	0.017	-0.000	0.021	0.011	0.029	-0.004	0.012	-0.058	0.067
	(0.013)	(0.015)	(0.015)	(0.045)	(0.054)	(0.070)	(0.052)	(0.072)	(0.072)
Overestimates non-top-2 share × Treated × Top 2 party × Post	-0.017		-0.021	0.128		0.143	-0.180		-0.239
	(0.013)		(0.015)	(0.755)		(0.757)	(0.309)		(0.325)
$\mathbb{E}(y)$	0.145	0.208	0.098	2.821	2.750	2.891	3.277	3.218	3.324
	(0.352)	(0.406)	(0.298)	(1.202)	(1.204)	(1.195)	(1.240)	(1.311)	(1.178)
Sample	Full	Top 2 voters	Other voters	Full	Top 2 voters	Other voters	Full	Top 2 voters	Other voters
Adjusted-R^2	0.154	0.315	0.037	0.309	0.389	0.242	0.248	0.348	0.158
F statistic	.	2.153	.	4.151	0.157	8.131	5.636	0.795	9.582
Obs	38,946	16,346	22,600	30,500	15,218	15,282	27,606	12,258	15,348
Respondent × Party FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Respondent × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. The table examines whether treatment effects on voting intentions, party rankings, and affective ratings differ according to respondents' baseline tendency to overestimate the vote share of non-top-two parties. *Overestimates non-top-2 share* is a binary indicator equal to one if the respondent's stated second-order belief for non-top-two parties exceeds those parties' actual combined vote share. The triple interaction Treated × Overestimates × Top-2 party × Post tests for additional treatment effects among overestimators. All models include respondent × party, respondent × time, and party × time fixed effects. Standard errors are clustered at the respondent level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.21. Second-order beliefs: Treatment effect by baseline over-estimation of non-top-2 share

Dep. variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Voting Intentions			Party Ranking			Party Rating		
Top 2 party × Post	-0.059*	-0.102**	0.020	-0.035	0.071	-0.249*	0.074	-0.205	0.433***
	(0.033)	(0.049)	(0.040)	(0.079)	(0.099)	(0.134)	(0.099)	(0.135)	(0.146)
Perceived top-two parties incorrectly × Top 2 party × Post	0.085***	0.009	-0.015	-0.423***	-0.268***	-0.454***	0.382***	0.342***	0.276**
	(0.019)	(0.023)	(0.024)	(0.067)	(0.083)	(0.114)	(0.078)	(0.117)	(0.111)
Treated × Top 2 party × Post	0.022	0.000	0.019	0.047	0.050	0.069	-0.003	-0.057	0.045
	(0.017)	(0.018)	(0.024)	(0.063)	(0.071)	(0.125)	(0.072)	(0.091)	(0.117)
Perceived top-two parties incorrectly × Treated × Top 2 party × Post	-0.009	-0.002	-0.000	-0.084	-0.059	-0.129	0.032	-0.019	0.043
	(0.025)	(0.030)	(0.031)	(0.087)	(0.107)	(0.149)	(0.102)	(0.148)	(0.147)
$\mathbb{E}(y)$	0.148	0.208	0.102	2.821	2.750	2.891	3.277	3.218	3.324
	(0.355)	(0.406)	(0.302)	(1.202)	(1.204)	(1.195)	(1.240)	(1.311)	(1.178)
Sample	Full	Top 2 voters	Other voters	Full	Top 2 voters	Other voters	Full	Top 2 voters	Other voters
Adjusted-R^2	0.150	0.315	0.030	0.316	0.392	0.249	0.253	0.350	0.160
F statistic	9.549	1.137	0.681	29.552	7.370	19.085	16.850	5.228	11.404
Obs	37,758	16,346	21,412	30,500	15,218	15,282	27,606	12,258	15,348
Respondent × Party FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Respondent × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party × Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

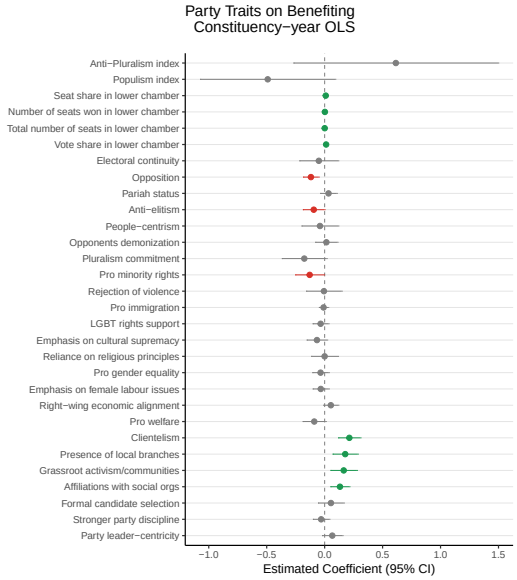
Notes. The table examines whether treatment effects differ for respondents who incorrectly identify the top-two parties at baseline. *Perceived top-two parties incorrectly* is a binary indicator equal to one if the respondent fails to name both parties that rank first and second in the baseline vote-intention distribution of their constituency. The triple interaction Treated × Perceived top-two incorrectly × Top-2 party × Post tests for additional shifts in intentions, rankings, and ratings among misidentifiers. All models include respondent × party, respondent × time, and party × time fixed effects. Standard errors are clustered at the respondent level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table C.22. Baseline-uncertain voters: treatment effects at endline

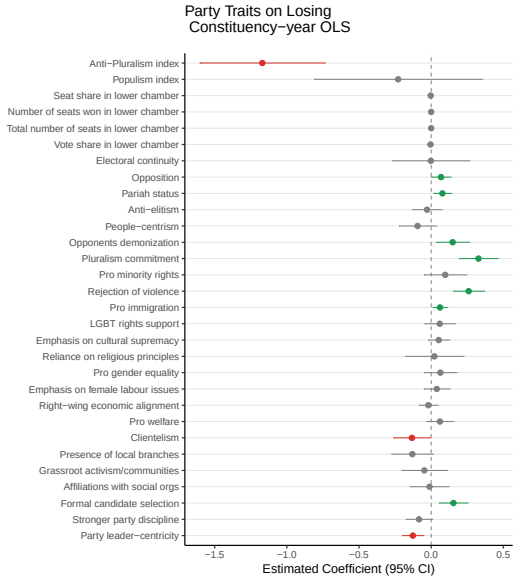
	(1)	(2)
Dep. variable	No Vote (uncertain)	Votes for top-two party
Treated	-0.000	0.000
	(0.000)	(.)
Post	-0.749***	0.518***
	(0.024)	(0.031)
Treated × Post	-0.012	0.070*
	(0.032)	(0.042)
$\mathbb{E}(y)$	0.623	0.278
	(0.485)	(0.448)
Sample	Baseline-uncertain	Baseline-uncertain & voted in endline
Adjusted-R^2	0.606	0.386
F statistic	751.020	.
Obs	1,470	1,108

Notes. The sample is restricted to respondents who reported being uncertain about their vote choice at baseline (i.e., they did not express a party preference). Column (1) models the probability of not voting at endline (*No Vote* (*uncertain*)), which includes responding "uncertain", "prefer not to answer", or other non-disclosed reasons. Column (2) models the probability of voting for a top-two party among respondents who did cast a vote at endline. The coefficient on Treated × Post identifies the causal effect of receiving poll information among baseline-uncertain voters. Both models are estimated by OLS with respondent fixed effects and standard errors clustered at the respondent level. Stars: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

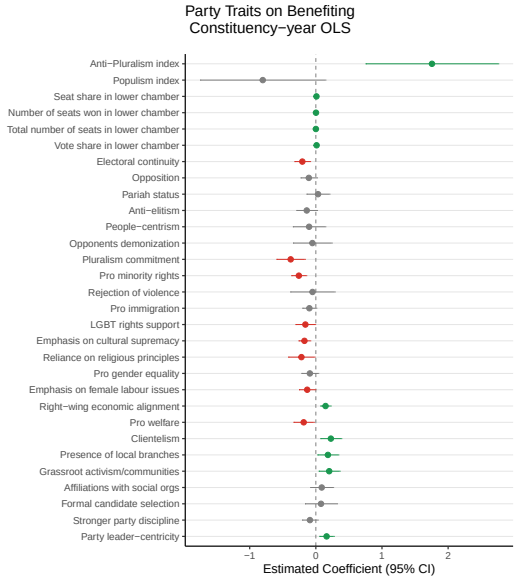
D Additional Figures



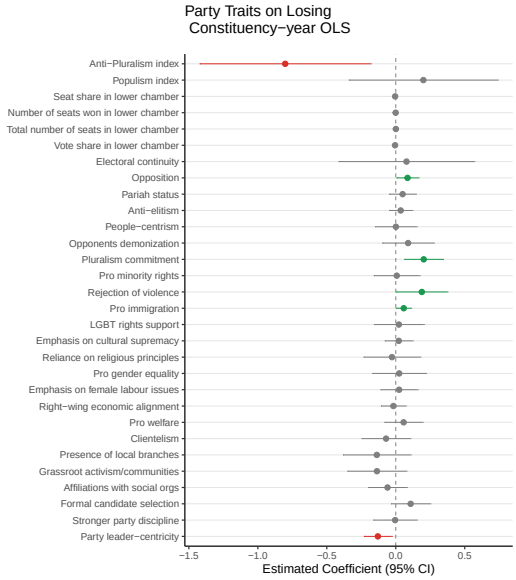
(a) Beneficiary from Coordination Failure on Party Characteristics (OLS)



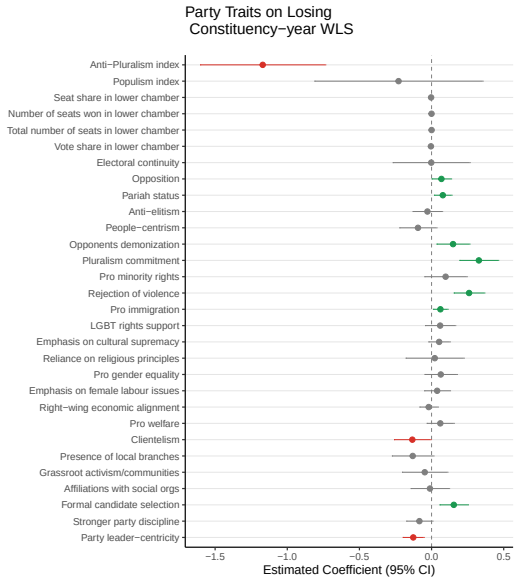
(b) Loser from Coordination Failure on Party Characteristics (OLS)



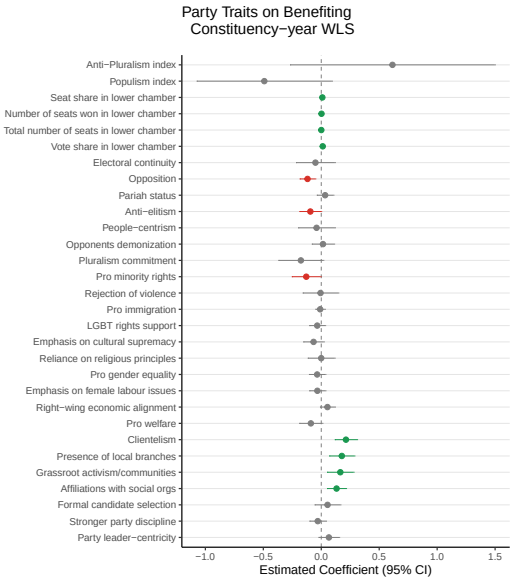
(a) Beneficiary from Strong Coordination Failure on Party Characteristics (OLS)



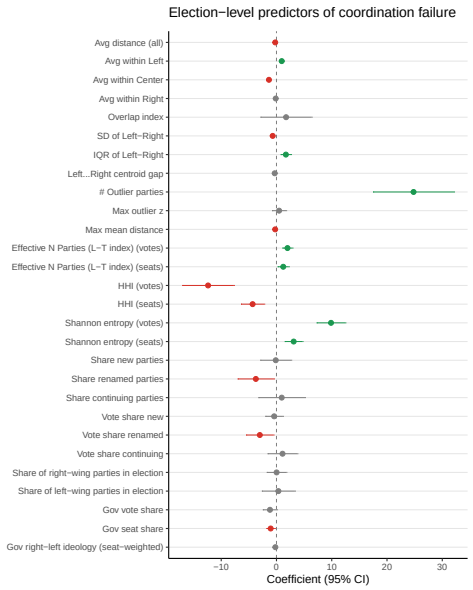
(b) Loser from Strong Coordination Failure on Party Characteristics (OLS)



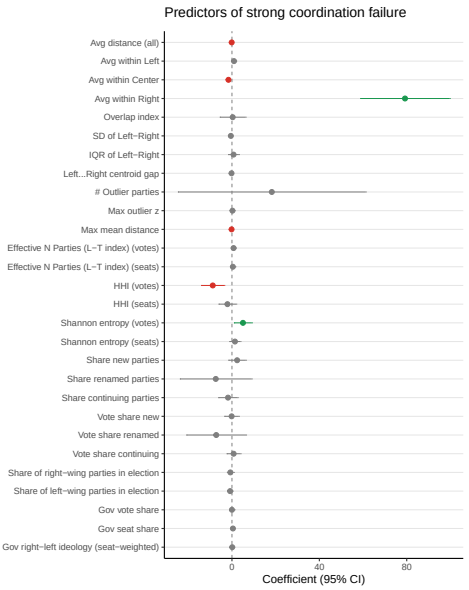
(a) Loser from Coordination Failure on Party Characteristics (WLS)



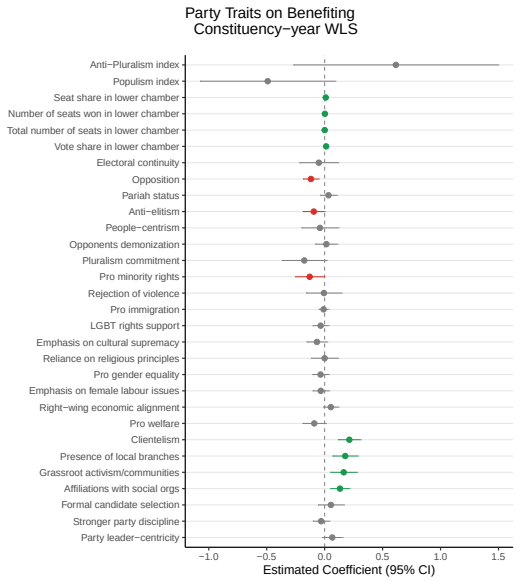
(b) Beneficiary from Coordination Failure on Party Characteristics (WLS)



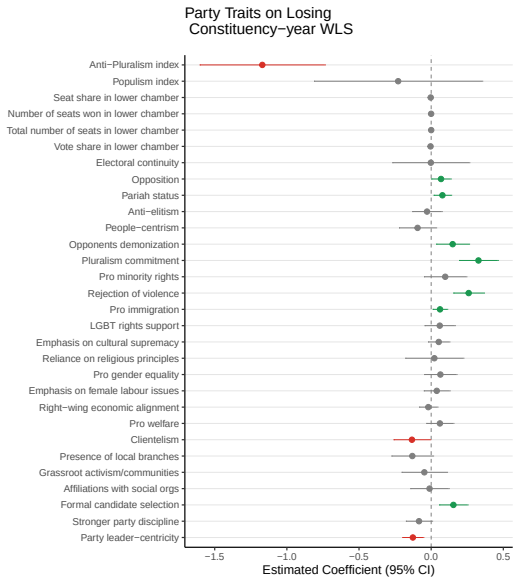
(a) Effect of Election Characteristics on Probability of Coordination Failure (Logit)



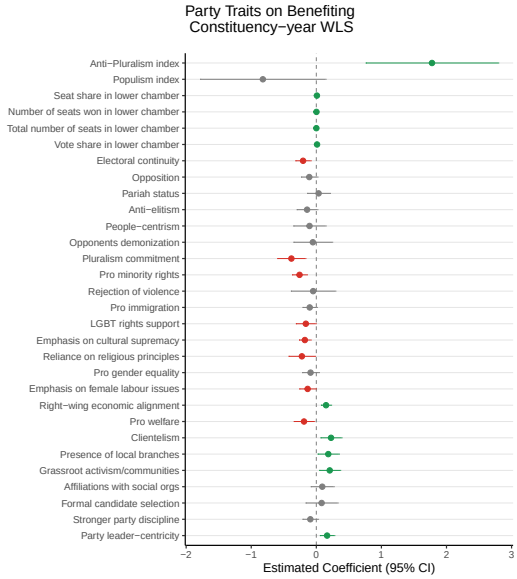
(b) Effect of Election Characteristics on Probability of Strong Coordination Failure (Logit)



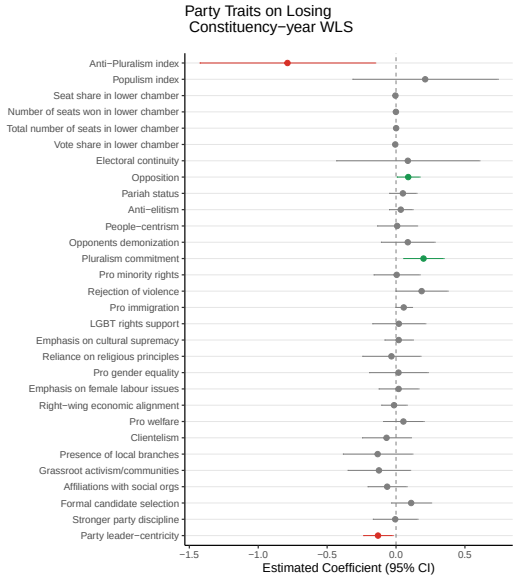
(a) Beneficiary from Coordination Failure on Party Characteristics (WLS)



(b) Loser from Coordination Failure on Party Characteristics (WLS)



(a) Beneficiary from Strong Coordination Failure on Party Characteristics (WLS)



(b) Loser from Strong Coordination Failure on Party Characteristics (WLS)