

When Votes Are Already Fixed: Electoral Reopening and Voter Turnout

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Abstract

The potential electoral consequences of voting are central to theories of participation but difficult to isolate empirically. Berlin’s 2024 partial repeat of the 2021 federal election provides an unusual setting: voters in selected polling districts could cast replacement ballots, while valid 2021 votes elsewhere remained legally fixed. We measure reopening as the share of each constituency’s 2021 electorate in precincts designated for the repeat. Across 294 harmonized reporting areas in twelve constituencies, a one-percentage-point higher reopening share is associated with a 0.055-percentage-point smaller decline in turnout between 2021 and 2024. The historically adjusted gradient remains positive at 0.039 percentage points per percentage point of exposure, although its statistical significance is sensitive to the small-cluster inference procedure. In a direct comparison, the 2024 federal-minus-2023 state turnout difference has a positive exposure gradient; the separate 2021–2025 federal change has a much smaller gradient. These comparisons are descriptive and do not identify the effect of partially retained votes. Reconstructing legally retained first votes also identifies two constituencies in which the fixed lead exceeded the entire repeat-eligible electorate. The findings document an association between participation and the institutional scope of electoral reopening, consistent with responses to electoral stakes and mobilization but not a direct estimate of individual pivotality.

Keywords: voter turnout, electoral stakes, repeat elections, pivotality, political mobilization, Germany

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1. Introduction

Why do people vote when the prospect of changing an electoral outcome is small? The calculus of voting places electoral stakes at the center of participation: a ballot can matter because it changes the probability of an outcome a voter values (Downs, 1957; Riker and Ordeshook, 1968; Feddersen, 2004; Blais, 2000). Identifying the empirical importance of these incentives is difficult. Competitive elections also attract attention, campaign spending, and voter mobilization, while turnout itself can influence the eventual margin (Matsusaka and Palda, 1993; Cox and Munger, 1989; Shachar and Nalebuff, 1999). Observed electoral closeness therefore reflects several potentially endogenous forces that are difficult to disentangle. We study an election in which institutional rules determined, before polling began, how much of an earlier electoral result could still be changed.

In February 2024, Berlin partially repeated its September 2021 federal election after a court ruling on voting irregularities. Eligible voters in designated districts could cast replacement ballots, while votes cast in unaffected districts remained legally fixed and continued to count toward the same constituency totals. As a result, the share of the 2021 electorate located in repeated precincts varied widely across constituencies. We define *repeat exposure* as the share of the September 2021 constituency electorate registered in precincts later designated for the February 2024 repeat. This baseline-denominator measure is distinct from the identities and number of people eligible under the updated February 2024 register. It records the institutional scope of reopening, independently of the eventual margin of victory. The unusual arrangement also permits a reconstruction of the candidate votes that were already fixed before polling began.

Turnout declined less in constituencies with larger reopening shares. In 294 harmonized postal precincts located in repeat-affected areas, a one-percentage-point higher constituency repeat share is associated with a 0.0548-percentage-point smaller turnout decline between 2021 and 2024. A 50-percentage-point difference in reopening corresponds to 2.74 percentage points in the turnout change. Thus, although turnout fell sharply in the stand-alone repeat

throughout Berlin, participation was relatively stronger where a larger share of the original electorate lived in precincts covered by the repeat.

Allowing for an exposure-specific linear trend fitted to earlier federal elections yields a coefficient of 0.0393. Its interpretation depends on the historical extrapolation: the restricted wild-cluster-bootstrap test gives $p = 0.0303$, whereas CR2/Satterthwaite gives $p = 0.0547$. The direct 2024 federal-minus-2023 state turnout contrast has a positive exposure gradient of 0.0318; separately, the ordinary 2021–2025 federal turnout change has a near-zero gradient of 0.0039. These contrasts do not supply formal cross-election tests of equality or an untreated counterfactual. They describe how the exposure gradient differs across election contexts, consistent with—but not establishing—a role for the distinctive stakes of partial reopening.

The retained first-vote totals provide a second measure of electoral stakes. In two of Berlin’s twelve Bundestag constituencies, the retained first-vote lead could not be overturned even if every eligible repeat voter supported the same challenger. We use the retained vote margins and the electorate eligible to vote again to construct a scenario-based measure of ex ante contestability. This measure is positively associated with turnout, although its historical-trend-adjusted relationship is less precisely estimated. The analysis distinguishes the share of the electorate entitled to vote again from the mathematical feasibility of changing the constituency winner.

This paper contributes evidence on electoral participation in a setting where the institutional scope of an electoral decision was observable before voting resumed. Unlike measures of competitiveness inferred from realized margins, the partial repeat allows us to measure how much of a previous election remained legally open to new ballots. We document an exposure-related difference in turnout changes, including relative to a fitted historical trend, and reconstruct retained first-vote totals to distinguish the scope of reopening from the feasibility of changing the constituency winner. These are observable dimensions of electoral stakes that need not be inferred from the final margin. Existing theory and evidence examine how pivotality, strategic uncertainty, and group-based incentives shape the expected benefits

of participation (Duffy and Tavits, 2008; Levine and Palfrey, 2007; Agranov et al., 2018; Enos and Fowler, 2014; Lyytikäinen and Tukiainen, 2019; Coate and Conlin, 2004; Feddersen and Sandroni, 2006). Empirically, however, close contests also generate information, campaign activity, and mobilization, complicating the interpretation of the relationship between turnout and electoral closeness (Cox and Munger, 1989; Shachar and Nalebuff, 1999; Gerber and Green, 2000; Spenkuch, 2015). Bursztyn et al. (2024) identify turnout responses to new information about anticipated closeness in Swiss referenda. Berlin’s partial repeat offers a complementary perspective on instrumental voting incentives: the share of the constituency electorate entitled to replace its ballots varies while votes cast elsewhere remain fixed. We find smaller turnout declines where a larger share of the electorate was legally entitled to replace ballots, consistent with participation responding to perceived stakes or the mobilization they induce. Nonrandom repeat designation and historical turnout differences constrain causal interpretation. The analysis also relates to research on voting costs (Fischer and Flügel, 2025; Cantoni, 2020; Alipour and Lindlacher, 2025) and electoral incentives in mixed-member systems (Shugart and Wattenberg, 2001; Pappi and Thurner, 2002; Gschwend, 2007).

Section 2 develops the participation framework and clarifies its relationship to institutional reopening. Section 3 describes the partial repeat and reconstructs the legally retained votes. Section 4 introduces the data, differential-change estimand, historical model, and identification concerns. Section 5 presents baseline and historical-adjusted gradients, comparison-election contrasts, and objective contestability. Section 6 discusses alternative explanations, complementary specifications, and the scope of the mechanism evidence. The online appendix preserves detailed institutional calculations, historical diagnostics, inference, and data audits.

2. Instrumental Voting and Electoral Stakes

In the calculus of voting, an individual’s incentive to participate depends partly on the expected electoral benefit of casting a ballot (Downs, 1957; Riker and Ordeshook, 1968). A

simple representation of the participation decision is

$$p_{ic}B_{ic} + D_{ic} - C_{ic} \geq 0, \quad (1)$$

where p_{ic} denotes voter i 's perceived probability of affecting an electoral outcome the voter values (whether the direct constituency contest or a broader parliamentary consequence), B_{ic} is the value of the preferred outcome relative to the alternative, D_{ic} captures other benefits of participation, including civic duty, and C_{ic} denotes the costs of voting. Instrumental participation thus depends on both the probability that a ballot matters and the stakes associated with the outcome. Group-based and strategic models extend this logic beyond literal single-vote decisiveness (Feddersen, 2004; Coate and Conlin, 2004; Feddersen and Sandroni, 2006; Levine and Palfrey, 2007).

Holding the value of the electoral outcome (B_{ic}), the costs of voting (C_{ic}), and other benefits of participation (D_{ic}) constant, an increase in perceived pivotality (p_{ic}) raises the expected instrumental benefit of voting. For voters near the participation threshold, even a small increase in the perceived probability of influencing the outcome may change the decision to vote. Consequently, the calculus of voting predicts a positive relationship between perceived pivotality and turnout, all else equal.

The relationship between the scope of reopening and individual pivotality is less straightforward. A close contest generally increases the potential importance of an individual ballot, but pivotality also depends on the size of the electorate, uncertainty about other voters' choices, and beliefs about the likely outcome. Increasing the number of votes that can still be cast may expand the range of feasible outcomes without necessarily increasing the probability that any particular vote is decisive. The sign of the effect of a larger reopened electorate on any individual's pivotality is therefore not generally determined. Whether reopening more ballots increases participation depends on how voters interpret the resulting electoral stakes. Moreover, every ballot in this Bundestag election could contain a first vote for a constituency

candidate and a second vote for a party list. A fixed first-vote lead does not remove the possible parliamentary consequences of second votes.

The partial repeat therefore changes the feasible set of outcomes without identifying the perceived chance of decisiveness. Parties and candidates may also respond to reopening through campaigning and mobilization, and voters may respond to information, expressive incentives, or voting costs. Our empirical test concerns aggregate turnout changes along predetermined constituency-level repeat exposure, not a structural estimate of individual pivotality.

3. Electoral Setting and Legally Revisable Stakes

3.1. The partial reopening of an earlier election

Berlin’s September 2021 election combined a federal Bundestag ballot with state and district contests and a referendum, requiring most voters to make six decisions on five ballot papers. The resulting administrative burden was compounded by inadequate preparation. Election officials underestimated the time needed to vote, provided insufficient polling-booth capacity, and relied too heavily on expectations of increased postal voting. COVID-19 hygiene requirements further complicated polling-station operations, while road closures for the Berlin Marathon hindered the delivery of replacement ballot papers. Shortages and misallocation of ballots consequently led to interruptions, long queues, and voting continuing beyond the scheduled closing time. Both constitutional courts identified serious organizational failures, but ordered different remedies: the Berlin Constitutional Court required a full repeat of the state and district elections, held in February 2023, whereas the Federal Constitutional Court’s ruling of December 19, 2023, required a narrower federal repeat on February 11, 2024. The latter covered 455 of Berlin’s 2,256 in-person precincts as described in the Federal Returning Officer’s contemporaneous press release; the official final-results tabulation lists 455 of 2,257

source precincts and 294 associated postal precincts.¹ Newly cast first and second votes replaced the invalidated ballots, while valid votes from unaffected districts remained in the final tally (Verfassungsgerichtshof des Landes Berlin, 2022; Bundesverfassungsgericht, 2023; The Federal Returning Officer, 2024).

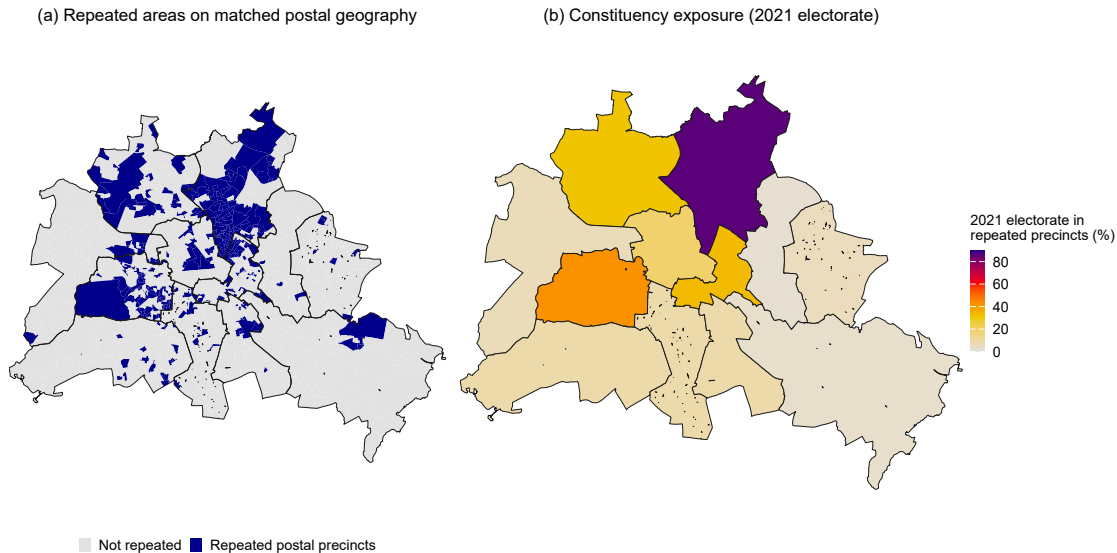
The partial repeat was not an ordinary new election. Although the original constituency boundaries and, with narrow legal exceptions, candidate lists were retained, the vote took place more than two years later under changed political circumstances. New electoral registers were prepared, meaning that eligibility in the repeated districts reflected the circumstances of the 2024 election rather than exactly reproducing the original 2021 electorate. At the same time, votes cast outside the repeated districts remained legally fixed. Thus, the eventual constituency result combined previously retained votes with new votes cast in 2024. Moreover, the original 2021 election had been bundled with several other contests, whereas the 2024 federal repeat was held on its own. These differences help explain the substantial citywide reduction in turnout.

The extent of reopening varied considerably across Berlin’s twelve Bundestag constituencies, from approximately 2.9% to 85.1% of their 2021 eligible electorates. We measure repeat exposure as the number of eligible voters in the 2021 in-person precincts covered by the repeat divided by the total number of eligible voters in the corresponding 2021 constituency. This constituency-level measure captures the institutional scope of reopening, rather than an individual voter’s probability of being pivotal. Our estimation sample consists of 294 harmonized postal-voting reporting areas affected by the repeat. Although every reporting area in the sample participated in the 2024 election, areas within the same constituency share the same repeat-exposure value. Figure 1 illustrates both the geography of repeated

¹The Federal Returning Officer’s final-results volume reports 2,257 in-person precincts; a footnote identifies one precinct in WK 78 combined with another in the determination of results under Section 68(2) of the Federal Electoral Regulations. The earlier press release and the court’s description use 2,256. This documents a plausible accounting distinction, but it does not by itself establish that all source classifications are one-to-one comparable. We preserve the 2,257 administrative source identifiers in the replication data (Die Bundeswahlleiterin, 2024; The Federal Returning Officer, 2024; Bundesverfassungsgericht, 2023).

polling units and the eligible-voter-weighted constituency exposure. The exposure measure is a proportion of eligible people, not of geographic surface area.

Figure 1: Geographic scope of Berlin’s 2024 partial federal repeat



Notes: This figure maps the official repeat-affected in-person precincts and the constituency-level reopening measure. The left panel distinguishes repeated and non-repeated in-person precincts on harmonized reporting geography. The right panel shows the constituency share of eligible voters entitled to participate in the repeat. Shading represents the share of the 2021 constituency electorate registered in precincts designated for the repeat, not the share of geographic area affected. Geographic construction and underlying counts are documented in Appendix A.

3.2. Fixed first votes and constituency contestability

Under the electoral rules governing the 2021 Bundestag election and its 2024 partial repeat, voters cast a first vote for a constituency candidate (*Erststimme*) and a second vote for a party list (*Zweitstimme*). The first vote determined the direct constituency winner, whereas the second vote primarily determined parties’ proportional parliamentary representation. The partial repeat could therefore change the first-vote winner in some constituencies even though votes from unaffected polling districts remained fixed. In others, the retained votes already made a change in the first-vote winner mathematically impossible. These constituency-level stakes are distinct from the incentives associated with party-list votes and parliamentary representation (Shugart and Wattenberg, 2001; Pappi and Thurner, 2002; Gschwend, 2007).

Using only votes that remained legally valid before the 2024 repeat, we reconstruct each

candidate’s retained first-vote total. The difference between the two leading candidates in these retained votes defines the *fixed gap*, denoted by F_c . The maximum number of valid replacement first votes is bounded by the electorate entitled to participate in the repeat. We construct the main ex ante exposure and headroom indices using the relevant 2021 eligible-voter counts, while the repeat was administered on updated 2024 registers. The official 2024 in-person precinct records permit a separate upper-bound audit. For WK 84 and WK 86, the retained first-vote gap exceeds both the 2021 and 2024 eligible repeat electorates. Thus the two first-vote leads are mathematically irreversible even if every 2024 eligible repeat voter casts a valid first vote for the same challenger. This conclusion concerns direct first votes, not the parliamentary consequences of the second vote.

To measure how much room remains for a reversal, we relate the fixed gap to the number of valid first votes potentially cast in the repeat. Let $N_c^{\text{repeat},2021}$ be the eligible 2021 electorate in the repeated in-person precincts, and let q_c^{2021} be the valid-first-vote fraction among 2021 repeat-area voters. The modeled valid-vote pool is $V_c(t) = N_c^{\text{repeat},2021} t q_c^{2021}$ at assumed turnout rate t . We define constituency *headroom* as

$$\text{Headroom}_c(t) = 100 \max \left\{ 0, 1 - \frac{F_c}{V_c(t)} \right\}. \quad (2)$$

Higher values indicate that the modeled pool of valid replacement first votes is larger relative to the retained gap. A value of zero means that the assumed valid-vote pool is insufficient to overturn that lead. We use 75% as an ex ante benchmark motivated by participation at the original federal election rather than the realized turnout of the 2024 repeat; 60% and 90% are alternative scenarios, not estimated turnout forecasts. Reversibility under full eligible participation and nonzero headroom at a particular scenario are different criteria.

Table 1 reports the full twelve-constituency reconstruction, rather than a selected subset. Appendix A.2 documents the source-level reconstruction, the 2024 electorate audit, and the 60%, 75%, and 90% turnout assumptions. The complete twelve-constituency display is in the

main text rather than repeated in the appendix.

Table 1: Legally fixed first-vote gaps and the scope for reversing a constituency lead

WK	Repeat (%)	Leader party	Fixed gap	Eligible (2021)	Reversible?	Headroom 75%
75	19.4	Greens	8,498	40,704	Yes	71.8
76	85.1	SPD	2,259	201,001	Yes	98.5
77	31.3	CDU	1,876	56,142	Yes	95.4
78	6.8	SPD	10,703	12,592	Yes	0.0
79	9.2	CDU	5,683	20,416	Yes	62.3
80	42.4	SPD	2,708	84,060	Yes	95.6
81	9.4	SPD	3,545	21,832	Yes	78.0
82	9.3	SPD	8,313	18,713	Yes	39.3
83	33.4	Greens	22,609	74,318	Yes	58.9
84	3.7	DIE LINKE	29,631	7,783	No	0.0
85	6.4	CDU	10,097	12,714	Yes	0.0
86	2.9	DIE LINKE	8,430	6,050	No	0.0

Notes: This table summarizes the twelve constituency-level legally retained first-vote leaders and gaps, the 2021 repeat-eligible electorates, and scenario-specific headroom, constructed from official first-vote returns. All twelve Bundestag constituencies are displayed. The retained first-vote leader is the leading candidate after excluding votes invalidated for the repeat, rather than necessarily the winner of the full September 2021 ballot. Repeat share is the percentage of the 2021 constituency electorate eligible for the partial repeat. Reversible compares the fixed first-vote lead with the entire eligible repeat electorate; a zero under Headroom 75 indicates that reversal is infeasible under the assumed 75% turnout and 2021 first-vote validity rate, not necessarily under full participation. The retained-vote construction and 2024 electorate upper-bound checks are documented in Appendix A.2; 60% and 90% headroom scenarios appear in Table A.2.

Fixed-vote gaps differ substantially relative to the size of the repeat electorate. Pankow and Reinickendorf had relatively small fixed gaps given the size of their repeat electorates. By contrast, the retained first-vote leads in Treptow-Köpenick and Lichtenberg were mathematically irreversible even if every eligible repeat voter supported a single challenger. Spandau-Charlottenburg Nord and Marzahn-Hellersdorf remained mathematically reversible under full eligible participation, but not under the benchmark turnout scenario.

These differences illustrate why the share of the electorate entitled to vote again and the feasibility of changing the constituency winner capture distinct dimensions of electoral stakes. Headroom describes the mathematical possibility of a reversal under specified turnout assumptions, not its likelihood or voters’ subjective assessments of pivotality.

4. Data and Empirical Strategy

4.1. Data

The main outcomes come from official returns of the Amt für Statistik Berlin-Brandenburg, harmonized to 294 repeat-affected *postal precincts* (*Briefwahlbezirke*) observed in September 2021 and February 2024. Each matched reporting area contributes observations for both elections, yielding 588 precinct-election records. Each repeat-affected harmonized area is entirely classified as repeated in the official in-person-to-postal crosswalk; none of the 294 is a partially repeated reporting unit. Multiple in-person source precincts can map to a single harmonized reporting area. In constituency 75, for example, source precincts 01W100 and 01W124 both map to area 011A. The 455 repeated in-person precincts therefore represent 294 matched reporting areas, not 455 independent turnout observations. The source mapping is retained in the official 2021 Strukturdaten workbook. Throughout the paper, *postal precinct* denotes the harmonized reporting geography, not the method by which a ballot was cast; *in-person precinct* (*Urnenwahlbezirk*) refers specifically to a polling-station district. Turnout is the number of recorded voters divided by eligible voters, expressed as a percentage for each postal precinct. It encompasses both in-person and postal participation assigned to that reporting area, rather than mail-voting participation alone. The separately matched in-person-only outcome measures something different and is discussed in Appendix C.

Across the 294 areas, the unweighted mean recorded turnout is 77.37% in 2021 and 51.13% in 2024. The mean decline of approximately 26.23 percentage points reflects the overall change between a multi-election day and the later standalone repeat as well as other time-varying factors. The principal coefficient measures how the turnout change covaries with exposure, not the cause of that large common decline. A few original official source records imply turnout slightly above 100% due to administrative inconsistencies; these are retained as provided and explicitly assessed in a sensitivity check rather than silently adjusted.

We assemble a historical panel on the same 294-area geography using the Berlin election panel of Wirthgen (2026). The underlying dataset harmonizes election returns to a common

2021 postal reporting-area geography. Earlier federal elections and the 2025 federal election are spatially reallocated using population-density-weighted (dasymetric) interpolation; the 2021 federal election and the 2023 state and 2024 federal repeats use the reference geography without this interpolation. Some early crosswalks rely on 2004 population-density surfaces because earlier layers are unavailable. The original inventory contains 1,507 postal units, while the harmonized target panel contains 1,503 units following small-area mergers. All 294 repeat-affected reporting areas are present in the balanced analysis sample. The replication archive contains the historical input snapshot and records its checksum. Appendix A.3 documents the crosswalk and its limitations.

The historical panel contains nine Bundestag election waves (1998, 2002, 2005, 2009, 2013, 2017, 2021, 2024, 2025) and seven Berlin state-election waves (1999, 2001, 2006, 2011, 2016, 2021, 2023), yielding 4,704 reporting-area–election observations. The simultaneous 2021 state and federal turnout measures retain their distinct administrative numerators and denominators. Crucially, the citywide 2024 records in non-repeated areas carry forward valid 2021 votes rather than observing new participation in February 2024. Those areas cannot provide contemporaneously observed untreated turnout outcomes. Our principal 2021–2024 analysis accordingly uses the 294 reporting areas in which replacement voting occurred; the state and federal comparisons retain election-type-specific 2021 references.

4.2. Empirical Strategy

Let S_c denote the percentage of the 2021 constituency electorate registered in precincts subsequently designated for the 2024 partial repeat; S_c does not measure the proportion of individuals eligible under the 2024 register. All 294 reporting areas in the estimation sample participated in the repeat, but S_c varies only across Berlin’s twelve Bundestag constituencies. We estimate

$$T_{it} = \alpha_i + \lambda_t + \beta (S_{c(i)} \times \mathbf{1}\{t = 2024\}) + \varepsilon_{it}, \quad (3)$$

where T_{it} denotes recorded turnout in percentage points, and α_i and λ_t are reporting-area and election fixed effects. With two periods, this specification is equivalent to regressing the change in turnout between 2021 and 2024 on constituency repeat exposure and an intercept. For example, a coefficient of 0.05 associates a one-percentage-point difference in repeat exposure with a 0.05-percentage-point difference in the change in recorded turnout.

Each matched postal precinct receives equal regression weight, regardless of its eligible electorate. Consequently, constituencies with more postal precincts in the estimation sample receive greater weight than those with fewer postal precincts. Figure 2 plots constituency means with both an equal-constituency descriptive fit and a fitted line weighted by the number of matched reporting areas. The latter has the same slope as the two-period reporting-area fixed-effects regression; it is still a visualization of twelve exposure values, not a display of 294 independent regressor values. The coefficient therefore describes an equal-reporting-area-weighted association, which need not equal an association weighted by eligible-voter populations. Appendix Table A.4 reports the underlying twelve area counts and constituency summaries.

To account for pre-existing differences in turnout trajectories, we also estimate a specification with an exposure-specific historical trend:

$$T_{it} = \alpha_i + \lambda_t + \gamma (S_{c(i)} \times \tau_t) + \delta (S_{c(i)} \times \mathbf{1}\{t = 2024\}) + u_{it}, \quad (4)$$

where τ_t denotes calendar years relative to 2021 (one unit is one year, not a decade). This specification uses federal elections from 1998 through 2021, together with the 2024 repeat. The coefficient δ measures the additional 2024 exposure gradient relative to the extrapolation of a differential linear historical trend. The adjustment imposes a model-based counterfactual: without an exposure-related 2024 deviation, constituencies with different repeat shares would continue along the fitted linear differential trend. A causal interpretation requires that the extrapolated relationship accurately capture otherwise unobserved turnout changes. Including

trend terms does not validate that assumption. We examine alternative historical starting years, a quadratic trend, and artificial historical break dates as sensitivity analyses.

We additionally compare turnout in the 2023 Berlin state repeat directly with turnout in the 2024 federal partial repeat, and examine the 2021–2025 change in the ordinary federal election, holding the constituency’s 2024 repeat share fixed. These comparisons describe different election contexts and are not untreated counterfactuals.

Statistical inference accounts for the constituency-level variation in repeat exposure. We cluster standard errors by Bundestag constituency rather than by reporting area. With only twelve constituencies and substantial concentration of the variation identifying β , conventional cluster-robust inference may be unreliable. We therefore additionally report restricted, null-imposed wild-cluster-bootstrap tests and CR2/Satterthwaite inference. Details are provided in Appendix D.

The main specification is a continuous-exposure differential-change design, not a comparison of repeated areas against observed 2024 untreated outcomes. Its coefficient measures how area-level turnout changes covary with twelve predetermined constituency-level exposure values. The counterfactual assumptions, sources of selection, and constituency-clustered inference are assessed explicitly in the following subsection.

4.3. Identification

A causal interpretation of the exposure gradient requires that turnout changes under the relevant counterfactual electoral conditions would not vary systematically with constituency repeat exposure, or would follow the specified differential historical trend. This assumption is demanding because the repeat was ordered in response to administrative irregularities in September 2021 rather than randomly assigned. Such irregularities may themselves have affected participation in the baseline election: if they disproportionately depressed recorded 2021 turnout in higher-exposure constituencies, the subsequent turnout-change gradient could be positive even without a corresponding response to electoral stakes in 2024. Reporting-area and election fixed effects absorb time-invariant geographic differences and common

election-wide changes, but not exposure-correlated shocks.

We examine several observable implications of these concerns. First, we assess whether the relationship between turnout and future repeat exposure was already changing in earlier federal elections. Our historical specification allows this relationship to follow a differential linear trend and estimates the additional 2024 gradient relative to its extrapolation. We also consider alternative historical windows, artificial break dates, and a quadratic trend. These exercises distinguish the observed 2024 association from longer-run geographic turnout patterns, although their interpretation depends on the historical counterfactual being appropriately specified. Section 5.2 presents the corresponding evidence.

Second, we examine whether observable differences in socioeconomic and political composition account for the baseline relationship. Specifications interacting pre-election socioeconomic characteristics and the original 2021 candidate margin with the 2024 indicator allow turnout changes to vary with these characteristics. The baseline exposure gradient remains positive and similar in magnitude after these adjustments (Appendix C). These checks address observable compositional differences but do not eliminate unobserved time-varying confounding. A separate diagnostic relates repeat exposure to genuinely pre-2021 constituency characteristics (Appendix Table A.6).

Third, we apply the same exposure measure to other election contexts. Berlin’s fully repeated 2023 state election provides a comparison without a partially retained state-election result, while the ordinary 2025 federal election provides a subsequent geographic benchmark. These comparisons help assess whether the positive exposure gradient is distinctive to the partial federal repeat. Because the elections differ in offices, political circumstances, and electoral rules, they are diagnostics rather than untreated counterfactuals.

The design identifies a relationship between participation and an *ex ante* institutional measure of the electorate still able to affect the result, while the historical and cross-election analyses assess important alternative explanations. A causal interpretation remains conditional on the absence of unaccounted differential turnout shocks. Moreover, the aggregate

relationship may reflect voters’ responses to electoral stakes, changes in party mobilization, or information provision; it does not separately identify individual perceptions of pivotality.

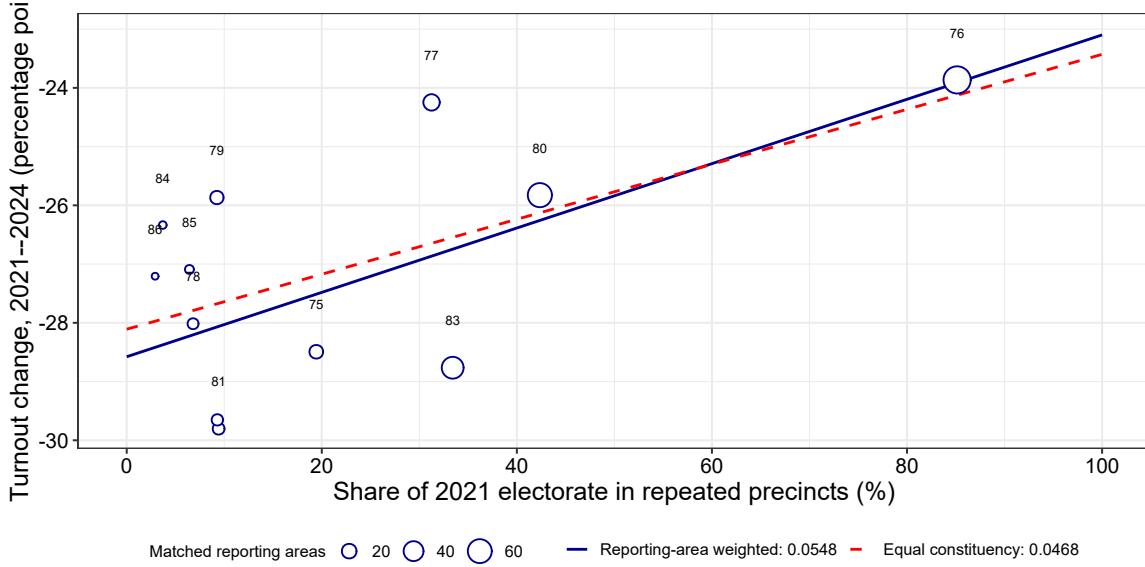
The observed change, $T_{i,2024} - T_{i,2021}$, can reflect both the legacy of disruptions in the original election and differences in participation during the repeat, alongside other changes between elections. Changing eligible-voter counts can also affect the administrative turnout ratio. Historical comparisons characterize longer-run geographical patterns, but they do not establish how turnout in September 2021 would have evolved without those disruptions. We therefore interpret the estimated gradient as an association between recorded turnout changes and constituency-level reopening, not as an isolated effect of electoral stakes.

5. Results

5.1. Repeat exposure and turnout changes

Figure 2 plots the change in recorded turnout between the September 2021 Bundestag election and the February 2024 partial repeat against constituency repeat exposure. Turnout declined substantially in all twelve constituencies, but the decline tended to be smaller where a larger share of the electorate could replace its earlier ballots. Pankow combines particularly high repeat exposure with a large number of matched reporting areas. The figure displays each constituency separately; its two fitted lines distinguish equal-constituency weighting from the equal-reporting-area weighting used by the baseline fixed-effects estimator.

Figure 2: Constituency repeat exposure and the change in federal turnout



Notes: This figure plots the twelve constituency means of the 2021–2024 turnout change against constituency repeat exposure, defined from the 2021 electorate. Bubble size indicates the number of matched reporting areas. The dashed line gives equal weight to constituencies (slope 0.0468); the solid line weights by reporting-area counts and reproduces the baseline fixed-effects slope of 0.0548 in Equation 3. The lines describe different weighting targets, not independent estimates. No confidence band is shown; twelve-cluster inference appears in Table 2 and Appendix D. The underlying values appear in Table A.4.

Column (1) of Table 2 reports the baseline estimate from Equation 3. A one-percentage-point higher constituency repeat share is associated with a positive 0.0548-percentage-point gradient in the turnout change. A 50-percentage-point contrast in repeat exposure corresponds to 2.74 percentage points in the turnout change, against an average decline of 26.23 points across the sample. This describes a smaller turnout decline where the legally reopened share was greater. The aggregate association may also reflect geographic differences in voting conditions and mobilization.

Table 2: Repeat exposure and the change in federal turnout: baseline and historical adjustment

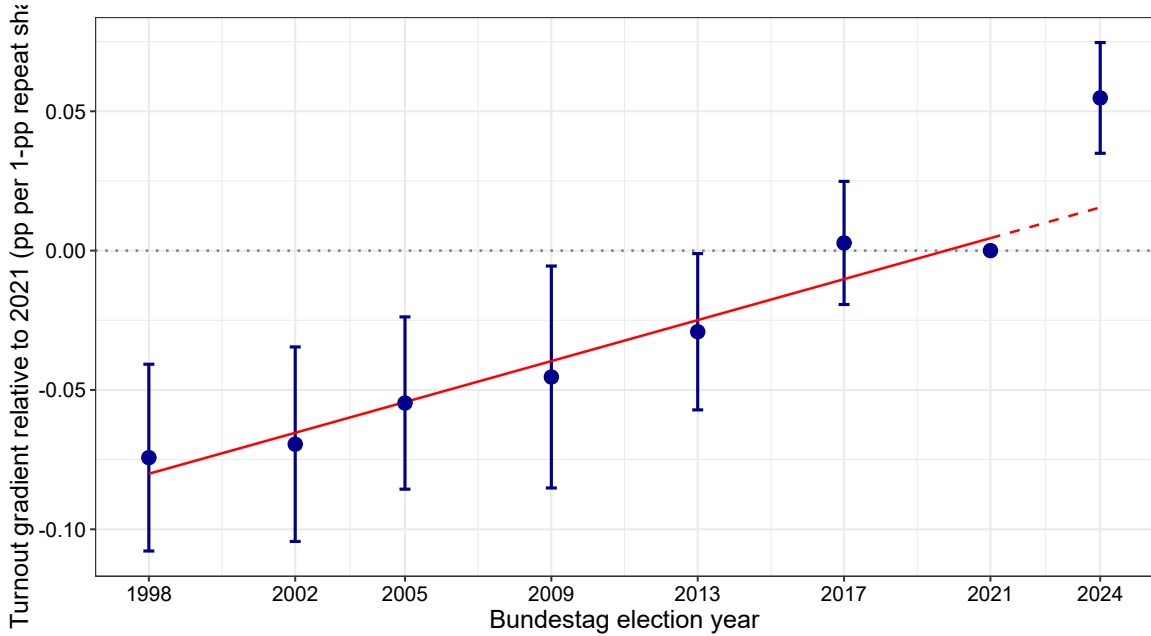
	(1) Baseline	(2) Historical adjustment
Repeat share $\times$ post	0.0548 (0.0090)	0.0393 (0.0080)
Repeat share $\times$ calendar trend		0.0037 (0.0006)
Restricted wild-bootstrap p	0.0166	0.0303
CR2/Satterthwaite p	0.0177	0.0547
CR2 95% confidence interval	[0.0204, 0.0891]	[-0.0017, 0.0803]
Observations	588	2,352
Postal-precinct FE	✓	✓
Election FE	✓	✓
Historical differential trend	No	Yes

Notes: This table reports the turnout-gradient estimates from the baseline specification in Equation 3 and its historical adjustment in Equation 4. The dependent variable is overall recorded federal turnout in percentage points for the same 294 repeat-affected postal precincts. Column (1) compares September 2021 with February 2024; column (2) uses the federal election waves from 1998 through 2021 and 2024, additionally allowing the exposure gradient to evolve with calendar time. Both specifications include postal-precinct and election fixed effects. Repeat share is measured in percentage points at the Bundestag constituency level. Parentheses contain conventional `fixest` constituency-clustered standard errors; no conventional significance stars are used in the coefficient block. The additional rows directly report the two-sided, null-imposed restricted wild-cluster-bootstrap p -values and CR2/Satterthwaite p -values and 95% confidence intervals, computed over twelve constituencies. The historically adjusted coefficient has bootstrap $p = 0.0303$ but CR2 $p = 0.0547$ and a CR2 interval containing zero. The full inferential procedures and finite-sample covariance conventions are documented in Tables D.1 and D.2.

5.2. Historical turnout patterns

Historical federal elections reveal a systematic concern for interpreting the baseline gradient. In the election-wave interaction regression, the association between turnout and future 2024 repeat share changes from approximately -0.074 in 1998 to 0.003 in 2017, with 2021 as the reference election. These pre-repeat coefficients describe longstanding geographic differences, not anticipation of the court decision. Figure 3 displays the historical coefficients together with a fitted pre-2024 linear trend, while Appendix Table B.1 reports their numerical estimates. The historical pattern makes an assumption of no differential turnout trends difficult to defend.

Figure 3: Historical Exposure–Turnout Gradients and the Extrapolated 2024 Benchmark



Notes: This figure plots year-specific exposure gradients and the pre-2024 fitted trend underlying Equation 4. Points are coefficients on 2024 repeat-share interactions with federal election years in a common reporting-area and election fixed-effects regression; 2021 is the reference. Error bars show conventional constituency-clustered 95% intervals. The solid red segment summarizes the fitted pre-2024 linear relationship; the dashed red extension to 2024 is a model-based extrapolation, not an observed untreated path. The blue point at 2024 is observed separately. The observed 2024 slope is about 0.0548, compared with a fitted 2024 historical slope of about 0.0155, yielding a 2024 excess gradient of about 0.0393. Historical slopes are descriptive, and the line relies on a functional-form assumption. The point estimates are tabulated in Table B.1.

Column (2) of Table 2 therefore applies Equation 4, allowing the turnout gradient associated with repeat share to evolve linearly over calendar time before the partial repeat. The 2024 excess gradient is 0.0393, compared with 0.0548 in the baseline model. Thus the historical adjustment reduces the estimated association by approximately 28%. Over a 50-percentage-point exposure contrast, the adjusted coefficient corresponds to a 1.97-percentage-point difference relative to the fitted historical trajectory. The point estimate remains positive relative to the extrapolated historical gradient, not a directly observed untreated turnout path. This interpretation depends on the extrapolation and cannot be presented as an automatic test of a stable parallel trend.

To assess the sensitivity of the extrapolation, we estimate the historical trend beginning in 1998, 2005, 2009, or 2013. The estimates remain similar. A trend beginning only in 2017 produces a larger estimate of 0.0569 but relies on just two pre-repeat federal election waves,

2017 and 2021; it is therefore a particularly fragile extrapolation. A quadratic historical trend yields a positive but less precise 2024 excess gradient. The alternative windows, artificial historical break dates, and quadratic specification are documented in Appendix Tables B.2, B.3, and B.4, respectively.

Inference is also sensitive to having only twelve constituencies and highly concentrated identifying variation. For the 0.0393 adjusted coefficient, the restricted wild-cluster-bootstrap two-sided test yields $p = 0.0303$, whereas CR2/Satterthwaite yields $p = 0.0547$ and a 95% confidence interval of $[-0.0017, 0.0803]$. The wild-bootstrap test rejects the zero-coefficient null at 5%, whereas CR2/Satterthwaite gives $p = 0.0547$ and its interval includes zero. The methods address the limited number of clusters differently; their disagreement is material. Both procedures and their implementation are reported in Appendix Tables D.1 and D.2. The main table therefore displays both small-cluster procedures as inference rows; the conventional clustered standard errors are retained for comparability but do not determine the main evidentiary assessment. Appendix Table D.3 documents the concentration of repeat-share variation across constituencies. Neither statistical adjustment removes the extrapolation assumption or the nonrandom placement of the repeat districts.

5.3. *Comparison elections*

The fully repeated 2023 Berlin state election provides a comparison in which the original state-election result was not partly retained within constituencies. We apply the same two-period fixed-effects specification as Equation 3, holding the constituency’s 2024 federal repeat share fixed, to the direct difference between turnout in the 2023 state repeat and the 2024 federal partial repeat. The first row of Table 3 reports a positive exposure gradient of 0.0318: the 2024-minus-2023 turnout difference is more positive in constituencies with greater federal repeat exposure. Because this comparison crosses election types and years, it also reflects differences in political circumstances, offices, and mobilization. It does not isolate the causal effect of retaining earlier votes.

The ordinary 2025 federal election provides a second descriptive geographic benchmark.

Using the same constituency repeat share in a fixed-effects comparison of the 2021 and 2025 federal elections gives a small, statistically insignificant coefficient of 0.0039, reported in the second row. The 0.0039 point estimate is much smaller than the 2021–2024 point estimate, but this alone is not a statistical test of their difference. It is not a strict placebo test: the election occurred later, under different political conditions and reformed rules for parliamentary representation. Both comparisons are undetrended diagnostics, not untreated control elections. Appendix C.4 sets out their interpretation and scope.

5.4. *Fixed-Vote Contestability and Turnout*

Repeat share measures the institutional extent of reopening; it does not establish whether the retained first-vote leader could be overtaken. The fixed-vote calculations in Table 1 show that Treptow-Köpenick and Lichtenberg were mathematically irreversible even with full participation by eligible repeat voters. At the benchmark 75% turnout scenario, headroom as defined in Equation 2 is zero in four constituencies, including two where a reversal would remain possible under full participation. Main Table 1 gives the complete twelve-constituency reconstruction; Appendix Table A.2 examines alternative turnout scenarios.

Panel B of Table 3 replaces repeat share with the 75%-turnout headroom index in the 2021–2024 federal fixed-effects regression. The estimated gradient is positive at 0.0469, showing that turnout changes also covary with a scenario-based measure of objective first-vote contestability. It is not an independent causal test: headroom and repeat share are constructed partly from the same institutional information, and second-vote consequences remain possible even when a first-vote lead is locked. Headroom is not an individual pivotality measure. Its historically adjusted coefficient is 0.0296 and is less precisely estimated; Appendix Tables C.4 and C.5 provide the turnout-scenario and historical sensitivity analyses.

6. **Alternative Explanations and Sensitivity**

Additional specifications examine alternative correlates of turnout without treating them as separately assigned electoral incentives. Controlling for the original *complete-election*

Table 3: Comparison elections and objective constituency contestability

Specification	Estimate	WK SE	WCR p	CR2 p
<i>Panel A. Descriptive election contrasts</i>				
2023 AGH to 2024 BTW; repeat share	0.0318	0.0045	0.0293	0.0400
2021 BTW to 2025 BTW; repeat share	0.0039	0.0111	0.8433	0.8713
<i>Panel B. First-vote contestability</i>				
2021 BTW to 2024 BTW; headroom (75%)	0.0469	0.0163	0.0107	0.0475
Observations per specification				588
Postal-precinct FE				✓
Election FE				✓

Notes: This table uses the two-election fixed-effects specification in Equation 3 and the headroom definition in Equation 2. Each model has 294 matched reporting areas, 588 observations, and twelve constituency clusters. Panel A reports two distinct descriptive election contrasts; Panel B replaces repeat share with headroom at the 75% turnout scenario in the 2021–2024 federal change. Coefficients and conventional clustered SEs are in percentage-point units. Restricted wild-cluster-bootstrap and CR2/Satterthwaite p -values come from Appendix Tables D.1 and D.2. The comparisons are not untreated counterfactuals or formal tests of coefficient equality across election contexts.

2021 candidate margin changes the baseline repeat-share coefficient from 0.0548 to 0.0463 (Appendix Table C.2). The original margin is distinct from the legally retained fixed gap. The baseline exposure association is also similar when adding socioeconomic characteristics measured before the 2021 election or before the 2024 repeat, interacted with the post indicator (Appendix Table C.1). The latter characteristics postdate the original election, so they do not function as strictly pre-treatment controls; both sets are descriptive sensitivity checks. These are adjustments to the undetrended two-period specification, not independent validation of the historical extrapolation. The 75% headroom estimate likewise becomes 0.0296 under a differential linear historical trend and is not precisely estimated (Appendix Table C.5); the historical sensitivity should accompany any interpretation of its positive two-period coefficient. Further checks of party composition, the distinct in-person voting outcome, and administrative turnout anomalies are reported in Appendix C.

A response to electoral stakes need not operate through voters’ private calculations of pivotality alone. Candidates, parties, and the media could also adjust campaign activity, contact, and information provision to the scope of reopening. Contemporaneous reporting distinguished constituencies where first-vote outcomes could still change from those whose

retained leads were mathematically insurmountable; it also discussed the relevance of turnout and list positions (rbb24 Inforadio, 2024; Reinhardt, 2024). Contemporary reporting discussed the differing electoral stakes across constituencies, but the available data do not show what information individual voters received or acted upon. The indexed media evidence in Appendix E does not show a robust positive association between constituency-level electoral-stakes coverage and objective contestability independently of repeat exposure. Nor do the available data separately identify voter beliefs, campaign mobilization, and other participation motives.

7. Conclusion

Berlin’s 2024 partial federal repeat created an unusually observable institutional measure of electoral reopening: some ballots could be cast anew, while previously valid votes elsewhere remained legally fixed. Across 294 matched reporting areas in twelve constituencies, turnout declined less where a larger share of the 2021 electorate lived in precincts designated for the repeat. The estimated gradient is 0.0548 percentage points per percentage point of exposure and remains positive at 0.0393 relative to an extrapolated historical differential trend. Its statistical significance depends on the treatment of uncertainty across twelve constituencies, and the historical comparison relies on a modeled extrapolation.

The reconstruction of legally retained first votes identifies a second dimension of electoral stakes. In two constituencies, the retained lead could not be reversed even under full participation by the 2024 repeat-eligible electorate; elsewhere, the feasibility of reversal varied with the assumed pool of valid replacement votes. This first-vote arithmetic is distinct from the possible consequences of second votes for parliamentary representation. Taken together, the institutional calculations separate the scope of reopening from the feasibility of changing an electoral outcome.

Nonrandom repeat designation, possible disruptions in the 2021 baseline election, and pre-existing geographical turnout differences limit causal interpretation. The results do not

isolate individual perceptions of pivotality from mobilization or other participation incentives. They nevertheless show how turnout changes covary with a legally determined feature of electoral competition that cannot be inferred from the eventual margin alone.

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Online Appendix: Data, Institutional Calculations, and Additional Evidence

This appendix documents the underlying administrative geography, the legally retained votes and turnout scenarios, the historical adjustment, supplementary specifications, statistical inference, and the indexed media evidence. Tables based on regression output report descriptive associations unless stronger identifying assumptions are stated. Additional candidate-level and numerical audits are preserved in the replication materials; they are not treated as separate tests of the paper’s central mechanism.

Appendix A. Data and Institutional Calculations

Appendix A.1. Geographic construction and repeat exposure

The analysis follows 294 harmonized, repeat-affected postal precincts (*Briefwahlbezirke*) in twelve Bundestag constituencies. The postal precinct identifier denotes the reporting geography, not the mode of voting: the main outcome measures overall recorded administrative turnout attributable to the area, including both in-person and postal participation. Repeat exposure is the constituency share of the 2021 electorate registered in the designated in-person precincts whose votes could be replaced. It is therefore an eligible-voter-weighted institutional measure, not the share of map area covered by the repeat.

Table A.1 documents the units underlying Figure 1. Official 2021 source files contain 2,257 in-person precincts (*Urnenwahlbezirke*) and 1,507 postal precincts, including 455 repeated in-person precincts and 294 repeat-affected postal precincts. The court and the Federal Returning Officer’s contemporaneous press release describe 455 of 2,256 polling districts, whereas the latter’s official final-results volume reports 455 of 2,257 in-person precincts. A footnote to the final-results count records that one precinct in WK 78 was aggregated with another for result determination under Section 68(2) of the Federal Electoral Regulations. This supplies a plausible counting-convention explanation, not a verified unit-level concordance. The geographic reconstruction retains the 2,257 source identifiers and the officially flagged 455 repeated in-person units. The map and analysis use the 2,257 source-unit identifiers and their matched repeat classifications.

Table A.1: Source precincts and the geographic scope of the partial repeat

WK	Postal precincts		In-person precincts		Repeat share (%)
	Total	Repeated	Total	Repeated	
75	90	17	192	34	19.4
76	94	79	175	149	85.1
77	87	26	156	48	31.3
78	114	10	177	11	6.8
79	176	16	176	16	9.2
80	148	61	176	74	42.4
81	134	12	198	18	9.4
82	152	11	194	17	9.3
83	176	49	203	64	33.4
84	118	4	234	8	3.7
85	114	6	166	10	6.4
86	104	3	210	6	2.9
Total	1,507	294	2,257	455	—

Notes: This table documents official polling geographies and the constituency-level scope of the partial repeat. The two postal and two in-person precinct columns give original source counts. No source postal precinct was partly repeated; the all-zero column is omitted. The source contains 1,507 postal and 2,257 in-person precincts, of which 294 and 455, respectively, were classified as affected by the court-ordered repeat. No postal precinct in this source classification is partly repeated. Repeat share denotes the percentage of the 2021 constituency electorate registered in precincts designated for the 2024 repeat, not the share of land area. The official final-results volume reports that one in-person precinct was combined with another for result determination under Section 68(2) BWO; the earlier 2,256 published figure is retained as a distinct reporting convention. No unpublished one-to-one concordance is presumed (Die Bundeswahlleiterin, 2024; The Federal Returning Officer, 2024). These are source counts for Figure 1, not counts of observations in the turnout regression.

Appendix A.2. Legally retained first votes and turnout scenarios

The fixed-vote reconstruction removes invalidated 2021 ballots from each candidate’s original first-vote total and retains all votes that remained legally valid for the 2024 result. For candidate j in constituency c , retained votes are $R_{jc} = O_{jc} - I_{jc}$, where O_{jc} is the original 2021 first-vote total and I_{jc} the votes from the later-invalidated precincts. If $R_{(1)c}$ and $R_{(2)c}$ are the largest two retained totals, then $F_c = R_{(1)c} - R_{(2)c}$. This calculation does not use realized replacement votes. A concrete example is Pankow (WK 76), for which the retained first-vote gap is 2,259 votes and the 2021 repeat-eligible electorate is 201,001. The corresponding values in Treptow-Köpenick (WK 84) are 29,631 and 7,783. Main Table 1 reports all twelve constituencies. To avoid duplicating its display, the replication package retains the underlying source-level retained-vote table, while this appendix documents the reconstruction equation, worked cases, 2024 register-based upper bound, and turnout-scenario sensitivity. The fixed gap is the difference between the two highest *retained* candidate totals, rather than the 2021 full-election winning margin. Two constituencies, Treptow-Köpenick (WK 84) and Lichtenberg (WK 86), have a fixed gap larger than both the 2021 and 2024 repeat-eligible electorates in the official source records. In WK 84, the gap is 29,631 against 7,783 (2021) and 7,655 (2024) eligible repeat voters; in WK 86 it is 8,430 against 6,050 and 5,838, respectively. Hence their retained first-vote leaders cannot be overtaken by valid replacement first votes even with full participation by all 2024 eligible repeat voters. This does not rule out changes through second votes. The 2024 counts are an upper-bound audit only; the modeled headroom denominator remains based on 2021 eligible voters.

Table A.2 varies the assumed turnout of the repeat-eligible electorate between 60%, 75%, and 90%. For constituency c , let F_c denote the retained top-two first-vote gap and $V_c(t)$ the modeled number of valid repeated first votes at turnout t . The headroom index is $100 \max\{0, 1 - F_c/V_c(t)\}$. The modeled valid-vote pool is $V_c(t) = N_c^{\text{repeat}, 2021} t q_c^{2021}$, using the 2021 repeat-area first-vote validity rate q_c^{2021} and rounding the result to a whole vote in R. The 75% benchmark reflects participation at the original election rather than observed repeat-election turnout. A zero means that a reversal is infeasible *under that scenario*; it is not necessarily impossible with full eligible participation. Thus Spandau–Charlottenburg Nord (WK 78) and Marzahn–Hellersdorf (WK 85) have zero headroom at 75% but are not mathematically locked at 100% participation. Headroom is a feasibility bound, not a forecast of vote shares or an individual pivot probability.

Table A.2: Fixed-vote headroom under alternative turnout scenarios

WK	Repeat (%)	Fixed gap	Reversible	Headroom 60	Headroom 75	Headroom 90
75	19.4	8498	Yes	64.8	71.8	76.5
76	85.1	2259	Yes	98.1	98.5	98.7
77	31.3	1876	Yes	94.3	95.4	96.2
78	6.8	10703	Yes	0.0	0.0	1.2
79	9.2	5683	Yes	52.9	62.3	68.6
80	42.4	2708	Yes	94.6	95.6	96.4
81	9.4	3545	Yes	72.5	78.0	81.7
82	9.3	8313	Yes	24.1	39.3	49.4
83	33.4	22609	Yes	48.7	58.9	65.8
84	3.7	29631	No	0.0	0.0	0.0
85	6.4	10097	Yes	0.0	0.0	9.7
86	2.9	8430	No	0.0	0.0	0.0

Notes: This table reports constituency-level contestability indices constructed as defined in Equation 2. For each Bundestag constituency, headroom is $100 \max\{0, 1 - F_c/V_c(t)\}$, where F_c is the legally retained first-vote gap and $V_c(t)$ is the potential valid repeated first-vote pool at assumed turnout $t = 60, 75$, or 90 percent, using the 2021 first-vote validity rate. Reversible instead uses the entire eligible repeat electorate. Thus a zero in a scenario column need not imply mathematical irreversibility at 100% participation. None of these constructed indices measures an individual voter’s subjective probability of being decisive.

Appendix A.3. Historical spatial harmonization, descriptive statistics, and Figure 2

The historical election panel is anchored to postal reporting-area boundaries used in 2021. The administrative map inventory contains 1,507 source postal precincts, while the external harmonized panel uses 1,503 target units after merging several smaller districts. The matching identifiers used here recover all 294 repeat-affected areas and their observed election waves; the discrepancy between total-source and harmonized-target counts must not be interpreted as four missing treated observations. Results from the 2021 federal election and the 2023 and 2024 repeats are already reported on the reference geography. Earlier federal elections (1998, 2002, 2005, 2009, 2013, and 2017), as well as the 2025 federal benchmark, use population-density-weighted spatial interpolation from their historical source precincts. For some early waves the panel provider relies on 2004 population-density surfaces in the absence of earlier layers. These are modeled allocations of source numerators and denominators, not direct observation of the same unchanged reporting districts. Such crosswalk uncertainty affects the differential-trend extrapolation but not the geographic matching required for the two-period 2021–2024 baseline. The later-start trend checks in Appendix Table B.2 reduce reliance on the oldest interpolations, though they do not eliminate spatial reconstruction from the remaining pre-2021 waves. The replication archive records a checksum of the exact historical

input snapshot used in the analysis.

The 2024 citywide results additionally mix new votes from repeated districts with legally carried-forward 2021 results from non-repeated districts; the latter are not February 2024 turnout observations. All calculations reported in the article are restricted to the 294 matched repeat-affected areas where a genuine new turnout outcome is observed. The original and harmonized unit counts, source-election classifications, and file checksum are tested by the added replication audit.

The harmonized election panel supplies 16 federal and Berlin state election waves for the 294-area sample. The eight-wave historical federal regression combines the seven elections from 1998 through 2021 with the 2024 repeat; the ordinary 2025 federal election is reserved for a separate geographic comparison. The state and federal 2021 turnout series are retained separately because their recorded administrative numerators and denominators differ. Table A.3 reports the waves and basic turnout accounting. The geographic harmonization follows Wirthgen (2026); the repeated-area selection and institutional measures are constructed for this study.

Table A.3: Election waves in the harmonized historical panel

Election	Turnout over 100%	Mean turnout (%)	Eligible voters
Bundestag 1998	0	81.27	525,504
Abgeordnetenhaus 1999	0	65.68	522,420
Abgeordnetenhaus 2001	0	68.69	528,469
Bundestag 2002	0	78.87	536,297
Bundestag 2005	0	78.59	538,415
Abgeordnetenhaus 2006	0	59.68	535,515
Bundestag 2009	0	73.31	549,848
Abgeordnetenhaus 2011	0	63.35	549,834
Bundestag 2013	0	75.15	558,574
Abgeordnetenhaus 2016	0	69.61	555,274
Bundestag 2017	0	78.43	560,976
Abgeordnetenhaus 2021	1	77.56	550,307
Bundestag 2021	1	77.37	556,325
Abgeordnetenhaus 2023	0	65.92	547,681
Bundestag 2024	0	51.13	548,024
Bundestag 2025	0	82.46	553,644

Notes: This table describes the historical federal and state election waves used in the turnout analysis. Each of the sixteen federal and Berlin state election waves covers the same 294 repeat-affected postal precincts, yielding 4,704 postal-precinct–election observations in the balanced panel. The 2021 state and federal turnout records have distinct source numerators and denominators despite elections on the same date. The reported mean turnout is the arithmetic mean across the 294 matched reporting areas in each wave (not the ratio of aggregate voters to aggregate eligibles), while the eligible-voter column sums registered eligibles across those areas. Eligible-voter counts and turnout values are retained as recorded; above-100% administrative ratios are not capped. The main historical specification uses federal elections through 2024, reserving 2025 for the ordinary-election benchmark.

Figure 2 displays twelve constituency means, not 294 individual postal precinct observations. Table A.4 supplies all twelve plotted points and the number of reporting areas underlying each mean. The unweighted line across the twelve constituency points has slope 0.0468, while the area-level baseline fixed-effects estimator assigns equal weight to each postal precinct and yields 0.0548. These slopes need not coincide. The underlying regressor takes only twelve distinct constituency-level

values.

Table A.4: Constituency observations underlying Figure 2

WK	Repeat (%)	postal precincts	2021 turnout	2024 turnout	Change (pp)
75	19.43	17	73.52	45.03	-28.49
76	85.13	79	79.46	55.59	-23.86
77	31.25	26	71.86	47.62	-24.25
78	6.80	10	71.05	43.03	-28.02
79	9.24	16	78.06	52.19	-25.87
80	42.36	61	79.82	53.99	-25.83
81	9.42	12	78.62	48.81	-29.80
82	9.29	11	67.44	37.79	-29.65
83	33.42	49	79.25	50.48	-28.76
84	3.70	4	81.76	55.42	-26.34
85	6.43	6	66.46	39.37	-27.09
86	2.91	3	75.84	48.63	-27.21

Notes: This table reports the twelve plotted constituency-level observations underlying Figure 2. The twelve rows report constituency repeat share and unweighted mean 2021 and 2024 overall federal turnout among the matched repeat-affected postal precincts. The number of matched reporting areas determines the bubble area in Figure 2. Changes are measured in percentage points. The descriptive line gives equal weight to the twelve constituencies; the main fixed-effects regression gives equal weight to each of the 294 postal precincts.

Table A.5 summarizes overall turnout, the turnout change, and socioeconomic indicators for the 294 reporting areas. The two official Strukturdaten workbooks share geographic identifiers but use different reference periods. Population information in the 2021 workbook predates the September 2021 election; the corresponding indicators in the repeat-election workbook predate February 2024 but were measured after the original federal election. Source in-person precinct percentages are population-weighted when aggregated to postal precinct geography. These are geographic characteristics, not observations of individual voters.

Table A.5: Descriptive statistics for the repeat-affected postal precincts

Variable	N	Mean	SD	Minimum	Maximum
Turnout, federal election 2021 (percent)	294	77.37	7.93	48.05	100.99
Turnout, federal repeat 2024 (percent)	294	51.13	8.84	17.42	69.90
Change in turnout (percentage points)	294	-26.23	4.28	-44.35	-15.27
Constituency repeat exposure (percent)	294	42.80	28.27	2.91	85.13
Residents aged 65 or older, 2021 source (percent)	294	18.02	8.80	1.56	47.64
Unmarried German adults, 2021 source (percent)	294	45.09	12.44	20.06	73.56
SGB II prevalence under age 65, 2021 source (percent)	294	11.46	8.75	0.51	56.39
German adults with migration background, 2021 source (percent)	294	13.66	8.01	2.73	50.80
East Berlin share, 2021 source (0–1)	294	0.44	0.50	0.00	1.00
Residents aged 65 or older, 2024 source (percent)	294	17.87	8.39	0.80	46.74
Unmarried German adults, 2024 source (percent)	294	45.87	12.11	19.07	72.91
SGB II prevalence under age 65, 2024 source (percent)	294	10.89	7.72	0.91	50.79
German adults with migration background, 2024 source (percent)	294	14.76	8.27	2.67	53.47
East Berlin share, 2024 source (0–1)	294	0.44	0.50	0.00	1.00

Notes: This table describes the repeat-affected postal-precinct estimation sample and its observed characteristics. The sample comprises 294 harmonized postal precincts. Overall recorded turnout includes both voting modes, not exclusively mail voting. Official socioeconomic indicators are aggregated from the underlying geographic source units; the 2021 Strukturdaten refer to pre-2021-election characteristics, whereas the repeat-election workbook contains more recent characteristics observed before February 2024 but after September 2021. Summary statistics describe geographic composition, not individual voters. Every displayed row contains 294 matched reporting-area observations; no variable-specific observation count differs in the publication table. Original official data are not imputed.

Appendix A.4. Pre-election characteristics and selection into repeat exposure

Table A.6 examines whether constituencies with larger subsequent repeat shares already differed in observable characteristics before the original September 2021 election. It relates continuous 2024 repeat exposure to socioeconomic characteristics measured in 2019–2020 and to 2017 federal turnout. Rather than divide the twelve constituencies into arbitrarily defined groups, the table reports the unweighted association across their constituency means. These are diagnostics of observable selection, not tests that establish random assignment; the repeat was ordered in response to administrative problems in 2021.

Table A.6: Pre-election characteristics and subsequent repeat exposure

Characteristic	Mean	SD	Gradient	95% CI
Residents aged 65 or older (%)	19.23	5.56	-0.63	[-2.21, 0.95]
Unmarried German adults (%)	43.45	8.81	0.64	[-1.92, 3.20]
SGB II prevalence, under age 65 (%)	13.56	6.50	-0.79	[-2.63, 1.04]
German adults with migration background (%)	14.24	7.10	-0.43	[-2.50, 1.64]
East Berlin population share (%)	41.23	47.42	2.99	[-10.84, 16.82]
Federal turnout, 2017 (%)	76.20	4.75	0.86	[-0.41, 2.12]

Notes: This table presents descriptive pre-2021 selection gradients against the future constituency repeat share defined for Equation 3. Each row reports the mean and standard deviation of a pre-election characteristic across the twelve Bundestag constituencies, using their 294 matched repeat-affected postal precincts. In-person precinct source indicators are aggregated to postal-precinct geography using population weights; constituency means then weight each matched postal precinct equally. The gradient is the coefficient from a separate unweighted regression of the constituency mean on the future 2024 constituency repeat share, scaled to a 10-percentage-point increase in repeat share. Brackets report conventional 95% confidence intervals from the twelve-constituency regressions. The 2021 *Strukturdaten* measures residents as of December 2020 and SGB II prevalence in 2019; marital-status and migration-background variables cover German adults. The 2017 turnout outcome uses the harmonized federal election returns. Associations are descriptive and cannot establish balance from random assignment or exclude unobserved selection. No multiple-testing adjustment is applied.

Appendix A.5. Candidate lists and parliamentary representation

Overturning a retained first-vote lead is not synonymous with changing parliamentary representation under the electoral rules governing the 2021 Bundestag election and its 2024 repeat. Some candidates had an alternative route to a seat through a party list, and changes in second votes could affect seat allocation at a different level of aggregation. Table A.7 preserves only the contemporary list-protection classifications supported by the cited source, including conditional cases. These are not a verified census of the status of every candidate. Table A.8 presents the separately modeled Berlin-wide second-vote thresholds; its calculations should not be read as deterministic seat changes caused by an individual first vote.

Table A.7: Reported party-list protection for selected candidates

WK	Party	Candidate	List protection	Fixed votes
76	SPD	Klaus Mindrup	Not protected	6095
76	CDU	Manuela Anders-Granitzki	Not protected	3836
76	GRUENE	Stefan Gelbhaar	Protected	3695
76	LINKE	Udo Wolf	Not protected	3824
77	SPD	Torsten Einstmann	Not protected	22147
77	CDU	Monika Grütters	Protected	24023
80	SPD	Michael Müller	Protected	24312
80	CDU	Klaus-Dieter Gröhler	Not protected	19328
80	GRUENE	Lisa Paus	Protected	21604
81	SPD	Kevin Kühnert	Protected (conditional)	43012
81	CDU	Jan-Marco Luczak	Protected (conditional)	35618
81	GRUENE	Renate Künast	Protected (conditional)	39467

Notes: This table reports selected contemporary claims about party-list protection relevant to the partial-repeat institutional calculation, not a comprehensive verification of individual mandate security. Classifications reproduce only the selected candidates whose party-list route to a Bundestag seat was explicitly discussed in contemporaneous reporting, including conditional cases. They are not a full census of every candidate's mandate prospects. First-vote reversibility and party-list seat protection need not coincide, because second votes affect seat allocations under the rules applying to the partial repeat.

Table A.8: Berlin-wide second-vote thresholds and possible seat consequences

Party	Additional votes	Relative to 2021 (%)	Potential consequence
SPD	37,347	39.5	National compensation seat regained (initially Lower Saxony)
SPD	48,126	51.0	Berlin/Lower Saxony list shift
SPD	95,246	100.8	Additional national seat
SPD	106,891	113.2	Berlin/Lower Saxony list shift
CDU	46,462	80.3	Berlin/NRW list shift (conditional)
CDU	54,130	93.6	National compensation seat regained
CDU	108,311	187.3	Berlin/NRW list shift (conditional)
CDU	112,029	193.7	Additional national seat
GRUENE	7,609	6.6	National seat regained
GRUENE	65,509	57.2	National seat regained
GRUENE	81,122	70.8	Berlin list: Nina Stahr
GRUENE	123,409	107.8	National seat regained
FDP	16,039	42.0	National compensation seat regained
FDP	17,146	44.9	Berlin list: Lars Lindemann
FDP	73,938	193.6	Additional national seat
AfD	2,342	7.9	National compensation seat regained
AfD	20,380	69.0	Berlin list: Goetz Froemming
AfD	60,241	203.9	Additional national seat
AfD	78,194	264.7	Berlin list: Georg Pazderski
LINKE	8,317	16.6	National compensation seat regained
LINKE	40,661	81.2	Berlin list: Pascal Meiser
LINKE	66,217	132.2	Additional national seat

Notes: This table presents the modeled additional second-vote thresholds under the applicable seat-allocation rules. Thresholds are modeled additional second votes in the repeated Berlin areas under the seat-allocation rules applicable to the February 2024 repeat. The relative percentage divides the modeled threshold by the party's original 2021 second votes in the repeated areas. Some regional seat shifts are conditional rather than guaranteed. These thresholds are calculated holding other parties' second-vote totals fixed within the modeled allocation scenario unless the source explicitly indicates otherwise; they are conditional party-level institutional calculations, not estimates of campaign effort or an individual's pivotality. Source: the published 5 January 2024 Wahlrecht.de calculation and its February updates (Wahlrecht.de, 2024), transcribed into `econ_revision/data/article_tables/second_vote_thresholds.csv` for the R export. These conditional source calculations are not a newly implemented complete seat-allocation algorithm.

Appendix B. Historical Patterns

Main Figure 3 shows the exposure-gradient coefficients from the federal-election fixed-effects election-wave interaction model relative to the omitted 2021 election. The historical coefficients describe associations with *future* repeat share; they do not indicate anticipation of the court ruling. The fitted line uses pre-2024 historical variation and is extrapolated to 2024 solely to illustrate a linear-trend counterfactual, not an observed untreated trajectory. It is not constrained to pass through the 2021 reference coefficient of zero.

Table B.1 reports the underlying coefficients of this election-wave interaction model. Because the future high-exposure constituencies followed different historical turnout paths, a flat differential trend is not a credible automatic benchmark. The historical specification instead fits an exposure-specific linear calendar-time trend to the earlier federal waves and estimates the additional 2024 deviation. Its positive point estimate is consequently a departure from a model-based extrapolation, not an unqualified causal effect. The linear time interaction is reported per calendar year in main Table 2; the linear and quadratic sensitivity table below reports time in decades, so its linear-time coefficient is ten times the corresponding annual coefficient when the linear specification is otherwise identical.

Table B.2 changes the starting year of the linear trend. The estimated 2024 excess remains close to 0.035–0.039 in several longer windows, but increases to approximately 0.057 when the fitted history begins only in 2017. Table B.3 imposes artificial breaks in earlier elections. These exercises test sensitivity to historical specification choices, not the validity of the unobserved 2024 counterfactual.

Table B.4 allows the historical exposure gradient to follow a quadratic rather than a straight line. The 2024 excess coefficient remains positive (0.0341) but is less precisely estimated. The quadratic specification raises the full-sample within- R^2 only from 0.0931 to 0.0933, while allowing a more flexible extrapolation with few historical waves. These fit statistics use all eight waves, including 2024, and do not measure a separately estimated pre-2024 prediction error. The linear estimate remains the main historical benchmark; neither specification can determine which extrapolation would have prevailed absent the partial repeat.

Table B.1: Election-specific associations with future 2024 repeat exposure

Outcome: Turnout (pp) Specification	(1) Federal panel
Repeat share $\times$ 1998	-0.0743*** (0.0152)
Repeat share $\times$ 2002	-0.0695*** (0.0159)
Repeat share $\times$ 2005	-0.0547*** (0.0140)
Repeat share $\times$ 2009	-0.0454** (0.0181)
Repeat share $\times$ 2013	-0.0291** (0.0127)
Repeat share $\times$ 2017	0.0028 (0.0100)
Repeat share $\times$ 2021	Omitted
Repeat share $\times$ 2024	0.0548*** (0.0090)
Observations	2,352
R^2	0.899
Within R^2	0.095
Postal-precinct FE	✓
Election FE	✓

Notes: This table reports the historical election-wave interaction estimates used to assess pre-existing trends in Equation 4. All election-wave interactions are estimated jointly in one postal-precinct and election fixed-effects regression; September 2021 is the omitted reference wave. The constituency exposure measure is defined by the later 2024 repeat, so earlier coefficients summarize pre-existing geographic turnout patterns rather than anticipatory effects. Standard errors in parentheses are clustered by Bundestag constituency. The same coefficients appear graphically in Figure 3.

Table B.2: Historical-trend sensitivity to the starting election

Outcome: Turnout (pp) Specification	(1) From 1998	(2) From 2005	(3) From 2009	(4) From 2013	(5) From 2017
2024 excess repeat-share gra- dient	0.0393*** (0.0080)	0.0368*** (0.0092)	0.0349** (0.0119)	0.0381** (0.0123)	0.0569*** (0.0156)
Historical repeat-share trend	0.0037*** (0.0006)	0.0039*** (0.0009)	0.0042** (0.0015)	0.0036** (0.0016)	-0.0007 (0.0025)
Observations	2,352	1,764	1,470	1,176	882
R^2	0.898	0.931	0.943	0.958	0.967
Within R^2	0.093	0.095	0.092	0.082	0.065
Postal-precinct FE	✓	✓	✓	✓	✓
Election FE	✓	✓	✓	✓	✓
Exposure-specific trend	Yes	Yes	Yes	Yes	Yes

Notes: This table reports window-specific estimates of the historical adjustment in Equation 4. Each column estimates the 2024 excess repeat-share gradient relative to a differential linear calendar-time trend fitted from the indicated pre-2024 federal election window. Postal-precinct and election fixed effects are included throughout. The choice of starting wave changes which historical observations determine the extrapolation; the 2017 start has especially limited pre-period information. Standard errors in parentheses are clustered by Bundestag constituency; stars use conventional clustered tests.

Table B.3: Artificial breaks in pre-repeat federal turnout histories

Outcome: Turnout (pp) Specification	(1) 2013	(2) 2017	(3) 2021
Exposure gradient at artificial break	0.0053 (0.0101)	0.0216 (0.0163)	-0.0086 (0.0133)
Historical exposure trend	0.0028 (0.0016)	0.0031** (0.0011)	0.0039*** (0.0007)
Observations	1,470	1,764	2,058
R^2	0.809	0.790	0.745
Within R^2	0.026	0.057	0.052
Postal-precinct FE	✓	✓	✓
Election FE	✓	✓	✓
Exposure-specific trend	Yes	Yes	Yes

Notes: This table reports artificial historical-break specifications as functional-form diagnostics for Equation 4. The columns use only historical federal waves up to and including the indicated artificial break: 1998, 2002, 2005, 2009 and 2013 for the 2013 break (five waves); these plus 2017 for the 2017 break (six); and these plus 2021 for the 2021 break (seven). In each sample, the fake-event regressor is future 2024 repeat share multiplied by an indicator for the selected endpoint wave; the exposure-specific linear trend is measured in calendar years relative to that fake break. Each regression includes all 294 areas in each included wave. These are functional-form diagnostics, not placebo treatments under random assignment. The exposure variable is the future 2024 constituency repeat share. Postal-precinct and election fixed effects are included; standard errors in parentheses are clustered by constituency.

Table B.4: Sensitivity to a quadratic historical exposure trend

	(1) Linear	(2) Quadratic
Repeat share $\times$ 2024 excess	0.0393*** (0.0080)	0.0341* (0.0160)
Repeat share $\times$ time (decades)	0.0368*** (0.0064)	0.0447* (0.0242)
Repeat share $\times$ time squared		0.0035 (0.0102)
Postal-precinct FE	Yes	Yes
Election FE	Yes	Yes
Observations	2,352	2,352
Within R^2	0.0931	0.0933

Notes: This table compares linear and quadratic extensions of the historical trend specification in Equation 4. Both specifications use the eight federal election waves from 1998 through 2024 for 294 postal precincts, with postal-precinct and election fixed effects. The linear model allows the exposure gradient to evolve with calendar time; the quadratic adds a squared-time interaction before measuring the excess gradient in 2024. Quadratic extrapolation is more flexible but can overfit the limited pre-repeat history. In this table, time is measured in decades relative to 2021, whereas Equation 4 and main Table 2 use calendar years. Consequently the linear-trend coefficient in the otherwise identical linear model is ten times the annual-unit coefficient, while the 2024 excess coefficient is unchanged. Standard errors in parentheses are clustered by the twelve Bundestag constituencies. Superscripts *, **, and *** denote two-sided conventional constituency-clustered $t(11)$ tests at the 10%, 5%, and 1% levels, respectively; they are not restricted wild-bootstrap or CR2/Satterthwaite results. The quadratic comparison is exploratory, not a separately identified causal effect.

Appendix C. Robustness

Appendix C.1. Socioeconomic composition

Table C.1 estimates the same 2021–2024 two-period fixed-effects model as the main specification, adding five socioeconomic indicators measured in either the 2021 or the pre-2024 Strukturdaten. The indicators enter interacted with the post-election dummy; their time-invariant levels are absorbed by postal precinct fixed effects. The baseline repeat-share coefficient changes from 0.0548 to 0.0564 with 2021 characteristics and 0.0557 with pre-2024 characteristics. Thus the *undetrended* exposure gradient is similar after conditioning on these observed characteristics. The 2024-source covariates were measured before the repeat but after September 2021. These are checks of the baseline comparison; they do not establish the validity of the historical-trend extrapolation. Their conventional constituency-clustered standard errors fall from approximately 0.0090 without controls to 0.0043 and 0.0036 with controls, respectively. This reduction is large despite unchanged sample size and an unchanged twelve-constituency exposure support. It may reflect partialling out correlated changes, but it does not add independently assigned variation or eliminate finite-cluster uncertainty. The estimates use the same observations and constituency-level exposure values, so the smaller conventional standard errors do not imply additional independent variation or stronger causal identification.

Appendix C.2. Original electoral margins and party composition

The complete September 2021 first-vote margin is a measure of prior electoral competitiveness. Unlike the legally retained fixed gap, it includes votes subsequently invalidated for the repeat.

Table C.1: Repeat exposure and turnout: socioeconomic controls

Dependent Variable: Model:	turnout		
	No controls	2021-source controls	2024-source controls
Repeat share $\times$ 2024	0.0548*** (0.0090)	0.0564*** (0.0043)	0.0557*** (0.0036)
Aged 65+ $\times$ 2024		-0.0881** (0.0366)	-0.0860** (0.0346)
Unmarried German adults $\times$ 2024		-0.1572*** (0.0209)	-0.1530*** (0.0252)
SGB II prevalence $\times$ 2024		0.0327 (0.0192)	0.0364 (0.0236)
Migration background $\times$ 2024		-0.1736** (0.0619)	-0.1793** (0.0758)
East Berlin $\times$ 2024		-2.031*** (0.6155)	-2.119*** (0.6698)
Observations	588	588	588
Within R ²	0.13093	0.33595	0.34678
Postal-precinct FE	✓	✓	✓
Election FE	✓	✓	✓

Notes: This table reports socioeconomic-control sensitivity for the two-period fixed-effects model in Equation 3. The dependent variable is overall recorded federal turnout in 2021 and 2024 across 294 postal precincts. All columns include postal-precinct and election fixed effects; socioeconomic variables enter interacted with the 2024 indicator. The 2021 source variables predate the original election; the more recent repeat-election source variables predate February 2024 but not September 2021. The marital-status and migration-background indicators refer to German adults, and the SGB II measure to residents under 65. These controls assess compositional sensitivity of the undetrended baseline specification only. Standard errors in parentheses use the conventional **fixest** twelve-constituency cluster correction; the markedly smaller controlled-model SEs are not interpreted as additional independent identifying variation. The R replication audit checks that all three models use exactly 588 observations from 294 areas, twelve exposure values, and the same covariance convention.

Table C.2 shows that controlling for this original margin interacted with 2024 changes the repeat-share coefficient from 0.0548 to 0.0463. This is a conditional association rather than evidence that original competitiveness and legal reopening were separately assigned.

Table C.3 similarly interacts 2021 party-list vote shares with the post indicator to assess sensitivity to pre-existing party geography. These covariates can absorb baseline political composition; they do not identify party-specific causal mobilization or individual instrumental incentives.

Appendix C.3. Alternative contestability measures

Headroom depends on the retained fixed-vote gap and the potential valid-vote pool and is therefore mechanically related to repeat exposure. Table C.4 evaluates the headroom-only association under 60%, 75%, and 90% repeat turnout assumptions. The coefficients are broadly similar, but the proxy remains a constructed feasibility measure, not a pivotality probability. The repeat-share and headroom measures share institutional inputs; their separate associations should not be interpreted as independently identified causal effects.

Table C.5 reports the historically adjusted 75%-headroom result, which is positive (0.0296) but considerably less precise. Its interpretation additionally depends on the historical exposure-gradient

Table C.2: Repeat exposure and turnout controlling for the original 2021 margin

Dependent Variable:	turnout	
Model:	(1)	(2)
Repeat share $\times$ 2024	0.0548*** (0.0090)	0.0463*** (0.0094)
Original margin $\times$ 2024		-0.1503*** (0.0389)
Observations	588	588
R ²	0.98363	0.98453
Within R ²	0.13093	0.17873
Postal-precinct FE	✓	✓
Election FE	✓	✓

Notes: This table examines sensitivity of Equation 3 to the original 2021 candidate margin. The dependent variable is overall recorded federal turnout in the 2021 and 2024 elections. Each model includes postal-precinct and election fixed effects. The candidate-margin control is the complete original September 2021 top-two first-vote difference as a percentage of valid first votes, interacted with the 2024 indicator. It is not the legally retained fixed gap after the partial-repeat invalidations. Standard errors in parentheses are clustered by Bundestag constituency; stars indicate conventional clustered significance.

extrapolation. Thus the unadjusted headroom association should not be taken as evidence of a robust independent effect of objective contestability.

Appendix C.4. Comparison elections

Main Table 3 reports the direct 2023-state-to-2024-federal turnout contrast and the 2021-to-2025 federal benchmark. Both use the 2024 constituency repeat share and the same two-period postal precinct and election fixed-effects structure as Equation 3. The first comparison asks how participation differed between two distinct repeat elections; the second asks whether the same geographic exposure variable predicts change in an ordinary subsequent federal election. Neither is an untreated counterfactual: the 2023 election concerned different offices, and the 2025 federal election took place under reformed electoral rules. We do not repeat the former joint historical state–federal comparison models here because those answer a different estimand and add little to this descriptive comparison.

Appendix C.5. Alternative turnout measure and source anomalies

The in-person participation analysis in Table C.6 measures a different outcome and draws on different reporting units. Since it excludes postal votes, the estimate can also reflect changes in the use of voting modes and should not be substituted for the overall administrative-turnout outcome. The underlying ratio is 100 times the in-person votes recorded at an in-person source precinct divided by its registered in-person eligible electorate, rather than the combined voting-mode numerator and geographic denominator used for the main outcome. The separate panel contains 910 precinct-election observations.

One postal precinct records turnout slightly above 100% in the official federal 2021 sample, and the same postal precinct has a second above-100% value in the state 2021 series. These are retained in the main data rather than capped or imputed. Table C.7 reports the sensitivity obtained by removing the entire affected postal precinct from the relevant balanced panels. The raw coefficient changes from 0.0548 to 0.0524 and the historical coefficient from 0.0393 to 0.0383. This shows

Table C.3: Sensitivity to original party-list vote shares

Outcome: Turnout (pp)	(1)	(2)
Specification	Baseline	Selected major-party shares
Repeat share $\times$ 2024	0.0548*** (0.0090)	0.0637*** (0.0082)
CDU share $\times$ 2024		0.4139** (0.1827)
SPD share $\times$ 2024		0.2829 (0.1589)
Greens share $\times$ 2024		0.2342 (0.1656)
FDP share $\times$ 2024		-0.1258 (0.1277)
AfD share $\times$ 2024		0.3521 (0.2652)
Left share $\times$ 2024		0.0840 (0.1522)
Observations	588	588
R^2	0.984	0.986
Within R^2	0.131	0.273
Postal-precinct FE	✓	✓
Election FE	✓	✓

Notes: This table examines party-composition adjustments to the fixed-effects specification in Equation 3. The models add original 2021 party-list vote shares interacted with the 2024 indicator to the two-period postal-precinct and election fixed-effects specification. These measures capture prior political composition; they do not isolate party mobilization responses. All estimates use constituency-clustered standard errors reported in parentheses. Significance stars correspond to conventional clustered tests.

limited sensitivity to the identified anomaly, not that the source data are otherwise error-free. The underlying administrative records and source-unit counts are documented in Tables C.8 and C.9.

Table C.4: Headroom–turnout gradients under alternative assumed turnout

	(1)	(2)	(3)
Assumed turnout	60%	75%	90%
Headroom $\times$ 2024	0.0484*** (0.0141)	0.0469** (0.0163)	0.0462** (0.0175)
Observations	588	588	588
Postal-precinct FE	✓	✓	✓
Election FE	✓	✓	✓

Notes: This table reports fixed-effects gradients under the three turnout assumptions used for the headroom index in Equation 2. Each column substitutes a fixed-vote headroom measure constructed before the 2024 repeat for constituency repeat share. Headroom is calculated using the retained 2021 first-vote gap, the potential eligible repeat electorate, and the 2021 valid-first-vote rate, assuming turnout of 60%, 75%, or 90%. All columns are presented as the two-period postal-precinct and election fixed-effects specification, which is algebraically equivalent in slope to the underlying first-difference regression. The estimates are descriptive institutional associations and do not measure individual pivotality; standard errors in parentheses are clustered by Bundestag constituency.

Table C.5: Historically adjusted turnout gradient in fixed-vote headroom

	Historical adjustment
Headroom (75%) $\times$ 2024	0.0296 (0.0177)
Headroom (75%) $\times$ calendar trend	0.0019 (0.0015)
Observations	2,352
Postal-precinct FE	✓
Election FE	✓

Notes: This table reports the historical trend-adjusted headroom gradient combining Equation 4 and the 75% scenario in Equation 2. The dependent variable is overall recorded federal turnout for 294 postal precincts in the 1998–2024 federal historical panel. The model includes postal-precinct and election fixed effects and a linear calendar-time trend interacted with the 75%-turnout headroom index. The headroom interaction in 2024 measures an excess gradient relative to that fitted historical benchmark. Headroom is a scenario-based feasibility index, not a voter-specific pivotality probability. The time interaction here is per calendar year relative to 2021; compare the decade scaling in Appendix Table B.4. Standard errors in parentheses use the `fixest` default constituency-clustered correction and are not numerically identical to the CRV1 bootstrap-studentization standard errors shown in Table D.1. Alternative inference methods are described in Appendix D. The historically adjusted headroom coefficient has restricted wild-bootstrap $p = 0.1294$ and CR2/Satterthwaite $p = 0.1925$ (Appendix Tables D.1 and D.2).

Table C.6: Repeat exposure and participation recorded at in-person precincts

In-person participation (pp) Specification	(1) Two-period FE
Repeat share $\times$ 2024	0.0263** (0.0111)
Observations	910
R^2	0.932
Within R^2	0.034
In-person precinct FE	✓
Election FE	✓

Notes: This table reports a distinct in-person participation sensitivity relative to the overall-turnout outcome in Equation 3. The outcome is the number of recorded in-person voters divided by the eligible in-person precinct electorate, expressed in percentage points, rather than the overall administrative turnout outcome in the main analysis. The two-period sample contains 455 repeat-affected in-person precincts observed twice (910 precinct-election records). The outcome excludes postal ballots and can therefore reflect changes in voting mode as well as participation. Standard errors in parentheses are clustered by the twelve Bundestag constituencies. Superscripts *, **, and *** denote two-sided conventional constituency-clustered $t(11)$ tests at the 10%, 5%, and 1% levels; no restricted wild-bootstrap or CR2 inference is claimed for this distinct outcome. These estimates should not be interpreted as a direct replication of the baseline overall-turnout estimand.

Table C.7: Turnout gradients and anomalous source records

Federal turnout (pp) Specification	(1) Baseline	(2) Exclude anomaly	(3) Historical	(4) Historical, exclude anomaly
Repeat share $\times$ 2024	0.0548*** (0.0090)	0.0524*** (0.0086)	0.0393*** (0.0080)	0.0383*** (0.0082)
Observations	588	586	2,352	2,344
Postal-precinct FE	✓	✓	✓	✓
Election FE	✓	✓	✓	✓
Exposure-specific trend	No	No	Yes	Yes

Notes: This table reports the administrative-turnout anomaly sensitivity for the main estimators in Equation 3 and Equation 4. The sample restriction omits the complete affected postal precinct with an official 2021 administrative turnout ratio exceeding 100%, preserving balanced election histories in the relevant models. Columns (1) and (2) re-estimate the two-period fixed-effects specification in Equation 3, including the repeat-share-by-2024 interaction, with 588 and 586 precinct-election observations, respectively. Their coefficients equal those from first-difference regressions on 294 and 293 matched precincts; a panel specification may use a different finite-sample correction for its conventional clustered standard errors. Columns (3) and (4) estimate the historical fixed-effects specification in Equation 4, with 2,352 and 2,344 observations. This is a check of one documented source anomaly, not evidence that the remaining administrative records are error-free. Standard errors in parentheses are clustered by the twelve Bundestag constituencies. Superscripts *, **, and *** refer to conventional two-sided $t(11)$ tests (10%, 5%, 1%); no new wild-bootstrap or CR2 tests of the restricted samples are implied.

Table C.8: Administrative source turnout ratios exceeding 100 percent

postal precinct	Election	Year	Eligible	Voters	Turnout (%)
074N	21_AGH	2021	1101	1112	100.999
074N	21_BTW	2021	1116	1127	100.986

Notes: This table reproduces the source records underlying the administrative turnout-anomaly check. Rows reproduce the affected source records in the balanced state/federal election panel without truncation or imputation. The original federal and state 2021 turnout numerators and denominators are separately retained. The identifiers refer to harmonized postal precincts, not to individual voters. The effect of excluding the affected geographic unit is reported in Table C.7.

Table C.9: Administrative and geographic construction audit

Metric	Value
2021/2024 affected postal precinct observations	588
Unique affected postal precincts	294
Bundestag constituencies	12
2021/2024 postal precinct ratios above 100 percent	1
Affected postal precincts with a 2021/2024 ratio above 100 percent	1
Historical AGH/BTW postal precinct-election observations	4704
Federal 1998–2024 postal precinct-election observations	2352
Historical AGH/BTW turnout ratios above 100 percent	2
2021 first-vote official data units	3764
2021 first-vote postal units	1507
2021 first-vote in-person units	2257
Official repeat in-person precinct units	455
Official repeat postal precinct units	294
in-person precinct classification units	2257
postal precinct classification units	1507
Repeated-area eligible voters in 2021	556325
Repeated-area actual voters in 2021	426956

Notes: This table audits the construction of the repeat-affected postal-precinct sample. The entries summarize source files, voting-unit classifications, and the number of harmonized postal-precinct observations retained for estimation. The sample is selected by the official repeat classifications, rather than by changing source-unit counts to reconcile published summaries. The 2021 and 2024 records are joined using the documented geographic identifiers. This table is a construction audit, not a balance test.

Appendix D. Inference

For inference with twelve constituency-level exposure values, we report null-imposed restricted wild-cluster-bootstrap tests and CR2/Satterthwaite procedures (MacKinnon and Webb, 2017; Pustejovsky and Tipton, 2018). The restricted bootstrap enumerates all $2^{12} = 4,096$ constituency Rademacher sign assignments; its displayed CRV1 standard errors are conventional cluster-robust quantities used for studentization, not bootstrap standard errors. Finite-sample correction differences explain why the historical repeat-share standard error is 0.0086 in Table D.1 but 0.0080 in main Table 2, and why the analogous headroom standard errors are 0.0190 and 0.0177. Table D.2 instead gives CR2 standard errors, Satterthwaite degrees of freedom, tests and intervals. These approaches address finite-cluster uncertainty differently; neither makes the nonrandom exposure or the historical extrapolation causal. In particular, the historical-adjusted repeat-share test gives bootstrap $p = 0.0303$ and CR2 $p = 0.0547$.

Table D.1: Restricted wild-cluster-bootstrap tests

Specification	Estimate	CRV1 SE	Bootstrap p
2021–2024 baseline	0.0548	0.0090	0.0166
Historical-adjusted baseline	0.0393	0.0086	0.0303
Direct 2023–2024	0.0318	0.0045	0.0293
2021–2025 benchmark	0.0039	0.0111	0.8433
Headroom (75%)	0.0469	0.0163	0.0107
Historical-adjusted headroom	0.0296	0.0190	0.1294

Notes: This table reports two-sided null-imposed restricted wild-cluster-bootstrap tests based on all $2^{12} = 4,096$ constituency Rademacher sign assignments for the models in Equation 3 and Equation 4. The CRV1 column is a conventional clustered standard error calculated with the explicit studentization correction described immediately above; it is not a bootstrap standard error. The small-cluster procedure addresses uncertainty with twelve constituencies, not nonrandom exposure placement. See also MacKinnon and Webb (2017).

Table D.2: CR2/Satterthwaite inference with twelve constituencies

Specification	Estimate	CR2 SE	df	CR2 p	95% CI
2021–2024 baseline	0.0548	0.0094	2.42	0.0177	[0.0204, 0.0891]
Historical-adjusted baseline	0.0393	0.0112	2.42	0.0547	[-0.0017, 0.0803]
Direct 2023–2024	0.0318	0.0079	2.42	0.0400	[0.0031, 0.0606]
2021–2025 benchmark	0.0039	0.0219	2.42	0.8713	[-0.0762, 0.0840]
Headroom (75%)	0.0469	0.0187	5.77	0.0475	[0.0007, 0.0931]
Historical-adjusted headroom	0.0296	0.0201	5.77	0.1925	[-0.0200, 0.0792]

Notes: This table reports CR2/Satterthwaite uncertainty for the turnout-gradient estimators in Equation 3 and Equation 4. The CR2 estimator adjusts the cluster covariance for finite-sample bias and uses Satterthwaite effective degrees of freedom to form two-sided tests and confidence intervals. All clustering is at the Bundestag constituency level, the source of variation in repeat exposure. The methods may yield different significance assessments from the restricted wild-cluster bootstrap in Table D.1; neither supplies an untreated counterfactual.

Table D.3 reports the constituency share of squared demeaned repeat-share variation in the 294-area estimator. Pankow (WK 76) contributes approximately 60.5% of this regressor variation.

The exercise measures where the fitted slope is informed by variation in exposure; it is not an estimate of the effect of removing a constituency. No constituency-deletion exercise is used.

Table D.3: Concentration of identifying repeat-share variation across constituencies

WK	Repeat-affected postal precincts	Repeat share (%)	Variation share (%)
75	17	19.4	4.0
76	79	85.1	60.5
77	26	31.3	1.5
78	10	6.8	5.5
79	16	9.2	7.7
80	61	42.4	0.0
81	12	9.4	5.7
82	11	9.3	5.3
83	49	33.4	1.8
84	4	3.7	2.6
85	6	6.4	3.4
86	3	2.9	2.0

Notes: This table decomposes exposure-regressor variation in the first-difference representation of Equation 3. Specifically, $w_c = n_c(S_c - \bar{S})^2 / \sum_k n_k(S_k - \bar{S})^2$, where n_c is the number of matched reporting areas in constituency c and $\bar{S}$ is the area-weighted mean repeat share. This is the share of squared demeaned exposure variation contributed by constituency c in the first-difference representation. They reflect where variation in the regressor comes from, not the effect of omitting any constituency. Pankow (WK76) accounts for approximately 60.5% of this variation. No constituency-deletion analysis is conducted.

Appendix E. Information and Reporting

The media data describe indexed keyword matches before each election, not unique articles or readership. Media Cloud searches use constituency names and election terminology, with a stricter subset requiring first-vote, direct-mandate, or electoral-competitiveness terms. The query audit specifies two matched 54-calendar-day windows, inclusive of their start and end dates but excluding election day: August 3–September 25, 2021, and December 19, 2023–February 10, 2024. The 2024 window begins with the December 19, 2023 Federal Constitutional Court judgment. These date ranges are verified against the archived query definitions, and the index does not count unique publications or reader exposures. Search counts are index matches, not necessarily unique news articles: a story may be matched by several constituency queries, and the composition and coverage of the two archives may differ.

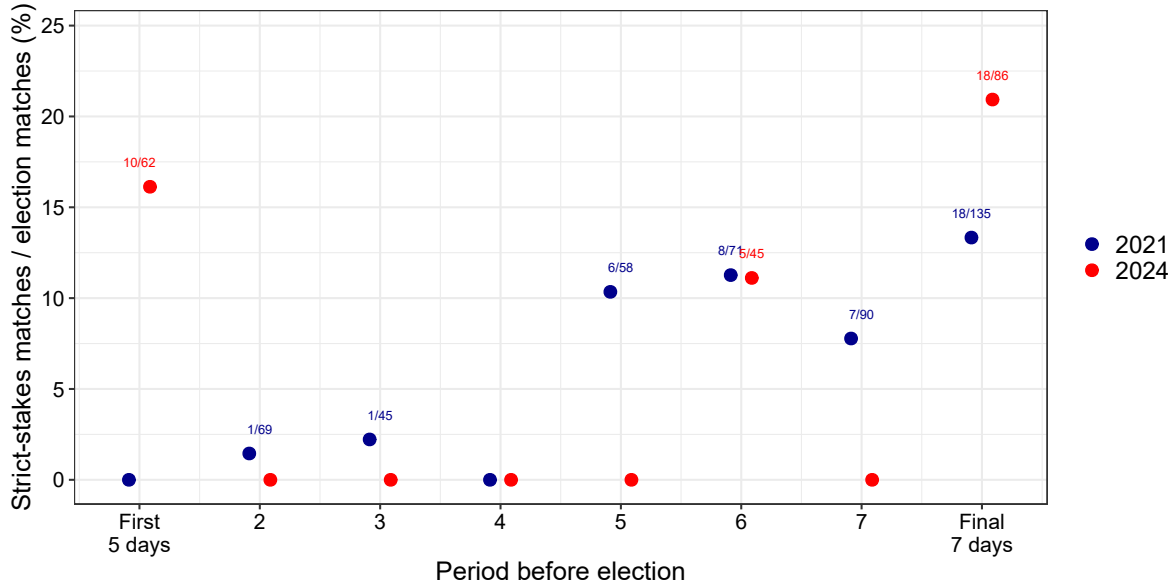
Table E.1 shows 41 strict-stakes indexed search matches among 556 local-election matches in 2021, compared with 33 among 351 in 2024. The share is higher in the latter archive, but the raw count is lower, and neither comparison is a harmonized measure of actual news exposure. Figure E.1 documents when these matches were indexed. Ten of the 2024 strict-stakes matches occur in the initial court-decision bin and eighteen in the final pre-election week; bin lengths and denominators differ, limiting temporal comparisons.

Table E.1: Indexed electoral-stakes search matches in comparable election windows

Window	Strict-stakes matches	Election matches	Strict-stakes share
2021 pre-election window	41	556	7.4%
2024 pre-election window	33	351	9.4%

Notes: This table describes indexed reporting matches used for the descriptive information-environment discussion. The archived queries use 54 calendar dates per election (August 3–September 25, 2021, and December 19, 2023–February 10, 2024), always excluding election day. Strict-stakes matches require terms related to direct mandates, first votes, or electoral contestability among constituency/election search results. The totals are indexed keyword-search matches rather than verified unique articles or audience exposure; a news item can match multiple constituency searches, and archive coverage differs across years. The table is descriptive evidence of the information environment, not a causal mediation test.

Figure E.1: Indexed electoral-stakes references before the 2021 and 2024 elections



Notes: This figure compares indexed weekly stakes-related reporting in the two pre-election windows. Each point is the share of strict electoral-stakes keyword matches among indexed local-election matches in a pre-election time bin. Labels identify numerator/denominator counts for positive-match bins; the remaining bins have zero strict-stakes matches, and their denominators are retained in the replication data. The first bin covers five calendar dates, starting with the December 19, 2023 court ruling for the 2024 window; each subsequent bin covers seven dates. Denominators and archive coverage differ across bins and elections, so point-to-point changes should not be interpreted as a continuous series. The plotted period-specific shares represent indexed search matches rather than verified articles or readership; the first bin is shorter than the others.

Table E.2 reports constituency-specific indexed matches for queries about the repeat, its procedure, and direct mandates. The distribution confirms that repeat-related terminology appeared in searches for several constituencies, including ones with mathematically fixed first-vote winners. It does not show a robust monotone association between constituency-specific reporting and objective contestability independent of repeat exposure. Across twelve constituencies, the change in the strict-stakes search share has a descriptive correlation with repeat exposure of about -0.12 . The media index therefore supports at most the plausibility of an information channel; it neither measures campaign intensity nor identifies an informational mediation effect on turnout.

Table E.2: Constituency-specific indexed references to the 2024 repeat

WK	Constituency	Repeat and mandate	Repeat and procedure
75	Mitte	1	2
76	Pankow	10	15
77	Reinickendorf	3	10
78	Spandau-Charlottenburg Nord	0	1
79	Steglitz-Zehlendorf	1	1
80	Charlottenburg-Wilmersdorf	5	9
81	Tempelhof-Schöneberg	0	1
82	Neukölln	1	2
83	Friedrichshain-Kreuzberg- Prenzlauer Berg Ost	0	2
84	Treptow-Köpenick	6	10
85	Marzahn-Hellersdorf	1	2
86	Lichtenberg	5	9

Notes: This table documents constituency-specific indexed references to the 2024 repeat. The two query groups count indexed matches linking constituency terms with the repeat and direct-mandate stakes or with election procedure. Counts need not correspond to distinct news stories because different constituency queries may overlap; they do not measure individual media consumption. Constituencies with legally locked direct-vote leaders can still appear in reporting because the repeat had procedural and second-vote implications.