Administrative Failure, State Capacity, and Democratic Exclusion: Evidence from Berlin's 2021 Election Breakdown[*]

Marius Kröper[†]

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Abstract

This paper studies the long-run participation consequences of a salient, non-strategic administrative breakdown in the delivery of elections as a recurring public service. I exploit Berlin's 2021 elections, when hundreds of precincts experienced ballot shortages, unlawful polling-place closures, and multi-hour queues—exposure later validated by a Federal Constitutional Court annulment. Using precinct-level administrative data and a stacked event-study design spanning eight elections (2016–2025), I find persistent turnout losses in affected precincts. Total turnout declines by about 1.8 percentage points (2.4 percent), with effects concentrated in in-person voting: in-person turnout falls by roughly 2.1 percentage points in subsequent elections. Postal voting increases and offsets about half of this decline, implying substantial but incomplete substitution across voting modes. Heterogeneity analyses show larger effects in younger precincts. Survey evidence is consistent with two mechanisms: acute election-day frictions that induce last-minute abstention and a contemporaneous decline in perceived procedural fairness.

[†]TUD Dresden University of Technology (TU Dresden). Helmholtzstr. 10, 01069 Dresden (Germany). marius.kroeper@tu-dresden.de.

1 Introduction

Elections are a publicly provided service: the state supplies access to political representation, and participation is highest when the time costs, uncertainty, and logistical frictions of voting are low. Administrative competence affects not only the realized cost of voting on election day but also the perceived reliability of the in-person channel, which can alter expected participation costs in future elections. Administrative failures—such as ballot shortages, unlawful polling-place closures, or multi-hour queues—raise the effective *cost* of voting at the polling place and may differentially affect citizens with tighter time and resource constraints. A growing literature shows that administrative frictions at the polling place—including long waits, polling-place logistics, and opening hours—depress turnout and can generate effects beyond the election day itself (Stewart III and Ansolabehere, 2015; Herron and Smith, 2016; Pettigrew, 2021; Potrafke and Roesel, 2020; Bagwe et al., 2026). Unlike formal voting restrictions, these barriers are not legislated and need not be strategic, but they can still reduce participation by preventing some voters from casting a ballot and discouraging others from attempting to vote in person in subsequent elections. This paper studies the downstream effects of *non-strategic administrative failure* on participation, focusing on the margin most directly affected by election-day breakdowns: *in-person* turnout. Because modern election systems often provide alternative participation technologies—most notably postal voting—I quantify substitution into postal voting and assess the extent to which vote-by-mail buffers participation losses following a shock to in-person service delivery. Prior evidence shows that alternative voting modes can shift participation patterns and affect turnout through convenience and social channels (Funk, 2010; Bagwe et al., 2026), but less is known about whether such technologies offset persistent participation losses following acute failures in election administration.

I study Berlin's September 26, 2021, federal election, when many polling places experienced ballot misallocations, hours-long queues, and temporary closures that violated electoral law. I use the Federal Constitutional Court's ex post annulment of 431 precincts as a court-validated measure of severe disruption. Precincts were classified as treated because administrators failed to provide a lawful voting process due to documented procedural violations, rather than because of realized turnout, local partisan contestation, or ex post strategic complaints. This setting isolates a concrete dimension of state capacity in the delivery of a recurring public service and allows me to examine whether an alternative voting mode partially mitigates failures in the in-person delivery channel over time.

The main finding is a persistent decline in *in-person* voting and only partial adaptation. Using a stacked event-study difference-in-differences design, I show that precincts exposed to severe administrative failure in 2021 experience a sustained participation gap over the next four years. In-person turnout falls sharply in 2021 and remains depressed thereafter, reaching about -2.1 percentage points in both 2024 and 2025. Over the same horizon, postal voting rises in later elections—about $+1.1$ percentage points in 2024 and 2025—indicating adaptation through substitution across modes. This substitution is substantial but incomplete: the implied offset rate averages about 0.40 across post-2021 elections and reaches roughly one-half in 2024–2025,

yet total turnout remains persistently lower (about -1.1 percentage points on average across the three post-2021 elections). Because the breakdown became a citywide scandal while polls were still open, even precincts whose results were not annulled were plausibly exposed to information about misadministration; accordingly, the estimated treated–control gap should be interpreted as the incremental effect of severe, court-validated disruption relative to lower levels of disruption elsewhere in Berlin, and is conservative with respect to the direct-exposure component.

Concurrent with this project, Fischer and Flügel (2025) show that the unanticipated election-day administrative breakdown in Berlin's 2021 contest produced a small same-day decline (approximately 0.7 percentage points) in in-person turnout across affected voting districts. I complement and extend their analysis in three ways that are central to interpreting administrative failure as a public-service shock rather than a one-off election-day inconvenience. First, I follow affected and unaffected precincts across subsequent elections to study persistence and recovery. Second, I distinguish in-person from postal turnout, showing that the enduring effect operates primarily through reduced in-person participation and only partial substitution into postal voting, which allows me to characterize vote-by-mail as an imperfect buffer against administrative breakdown. Third, I document heterogeneity in scarring across precinct demographics and present complementary survey patterns consistent with habit disruption and belief updating following administrative failure.

My identification strategy exploits that the Federal Constitutional Court retrospectively reviewed the 2021 federal election and annulled local results in specific precincts due to severe procedural violations—including missing or incorrect ballots, legally impermissible polling-place closures, and queueing that extended beyond the statutory voting window. Treatment is therefore based on documented failures in service delivery rather than realized electoral behavior. The design compares changes within precincts before and after 2021, controlling for precinct fixed effects and district-by-election fixed effects, and shows that pre-treatment trends are parallel.

I interpret these results as evidence that election administration is a recurring public service whose reliability shapes democratic inclusion. This paper contributes in two main ways. First, by exploiting court-validated precinct exposure, it isolates the participation consequences of administrative failure in a high-capacity democracy, separate from changes in formal voting rules or partisan contestation. Second, it quantifies the limits of technological buffering after a shock to in-person service delivery: postal voting absorbs a meaningful share of the in-person decline but does not eliminate the participation deficit, consistent with adaptation that requires planning and learning. This mechanism is distinct from—but similar in consequence to—intentional barriers such as registration restrictions or identification requirements. In line with Besley and Persson (2010) and Acemoglu et al. (2020), I view election administration as part of the public service infrastructure through which the state signals competence, inclusion, and respect; visible breakdowns can therefore raise both realized voting costs and expected future costs through belief updating about service reliability. When that infrastructure fails in a visible way, it can disrupt civic habits and shift expectations about the cost and reliability of participating in person.

Survey evidence from the German Longitudinal Election Study (GLES) is consistent with these mechanisms. Because the survey does not identify respondents' precinct exposure, I compare Berlin respondents to respondents in other German states. Immediately after the 2021 election, Berlin respondents report lower perceived procedural fairness and a higher likelihood of last-minute abstention relative to respondents elsewhere. These patterns are consistent with acute election-day frictions and belief updating following a highly visible administrative failure, but they do not identify the causal effect of direct precinct-level exposure. Fujiwara et al. (2016), Gerber et al. (2003), and Coppock and Green (2016) model voting as habit-forming behavior, while Meredith et al. (2009) and Shino and Smith (2018) show that early disruptions can inhibit routine formation. Separately, work on belief updating in public service delivery (Alsan and Wanamaker, 2018; Lowes and Montero, 2021; Akhtari et al., 2022) finds that bureaucratic failure can reduce institutional trust. My results echo this pattern in the electoral domain, with the important distinction that the long-run turnout decline is concentrated in in-person voting, while the rise in postal voting suggests delayed and partial adaptation rather than full recovery.

This paper contributes to three strands of literature. First, it extends work on voting costs by showing that *how* elections are administered—beyond formal rules—has durable effects on participation (Cantoni et al., 2025; Cantoni, 2020; Alipour and Lindlacher, 2025; Hodler et al., 2015; Burden et al., 2014; Gerber et al., 2013; Fischer and Flügel, 2025). Second, it adds to behavioral political economy by highlighting habit disruption and belief updating as channels linking public service failure to political disengagement (Bechtel et al., 2018; Kaplan and Yuan, 2020; Olawole, 2023). Third, it speaks to the political economy of state capacity: breakdowns in electoral logistics resemble failures in other public services and can produce distributive consequences for democratic inclusion (Herron and Smith, 2012; Toral, 2023).

Finally, the results have direct policy implications. Administrative failure produces persistent declines in in-person participation, and vote-by-mail provides meaningful but incomplete buffering through substitution. Capacity safeguards—ballot stock buffers, real-time queue monitoring, deployable backup staff and materials, and scalable postal and early-voting infrastructure—should therefore be viewed as complementary components of electoral resilience: they reduce the probability that an in-person capacity shock translates into avoidable, persistent participation losses.

The remainder of this paper proceeds as follows. Section 2 describes the institutional setting, data sources, and the empirical strategy. Section 3 reports the main results. Section 4 explores political consequences. Section 5 presents the conceptual framework. Section 6 concludes.

2 Institutional Setting, Data, and Empirical Strategy

2.1 Institutional Setting

Berlin offers a valuable case for studying the consequences of administrative failure in a high-capacity democratic state. Germany is widely regarded as having strong institutions, high procedural trust, and a professional civil service. The electoral system is well-established, with

routine federal, state, and district-level contests conducted under a uniform legal framework. That such large-scale electoral failure occurred in this setting underscores the potential fragility of even well-functioning democracies when administrative delivery falters.

On 26 September 2021, Berlin held four concurrent elections: federal (*Bundestag*), state (*Abgeordnetenhaus*), district councils (*Bezirksverordnetenversammlung*), and a city-wide referendum. These elections took place under COVID-19 precautions and coincided with the Berlin Marathon, which restricted traffic flow and added further stress to the voting process. The result was a cascade of administrative breakdown: ballot shortages, misallocated or missing ballots, polling stations opening late or closing early, and queues exceeding two hours. Media reports and citizen complaints described widespread dysfunction across the city.

These failures triggered an extensive legal review. The Berlin Constitutional Court annulled the entire state and district elections, citing "systematic, city-wide electoral disruptions" that had made a proper conduct of the election "impossible in broad parts of the territory" (Verfassungsgerichtshof des Landes Berlin, 2022, VerfGH 154/21). The court documented dozens of violations, including ballots for the wrong district, missing materials, overcrowded polling stations, and unlawful closures. In a separate ruling, the Federal Constitutional Court annulled the federal election in 431 precincts, citing "mandatsrelevante Wahlfehler"—errors severe enough to plausibly affect seat allocation in the Bundestag (Bundesverfassungsgericht, 2023, BVerfG 2 BvC 4/23). These included ballot delivery failures, voting after 6:00 p.m. in violation of federal law, and prolonged queuing that exceeded the court's threshold of tolerable burden for voters. The administrative data allow me to follow these precincts over time by harmonizing voting units into consistent postal precinct identifiers across elections, yielding a panel that spans multiple election cycles. The court ruling lists 431 in-person voting precincts; in the panel harmonized to 2021 postal precincts, these correspond to 294 unique postal precincts (treatment units).

These rulings offer a rare source of court-validated, precinct-level treatment assignment. Unlike studies relying on media reports, perceptions, or self-reported experience, treatment here is defined by formal judicial determinations of procedural violations rather than by realized turnout or partisan outcomes. This substantially reduces concerns that treatment status is mechanically correlated with electoral behavior.

The administrative breakdown constituted a multidimensional disruption to voting. Ballot unavailability and long queues raised participation costs; procedural confusion and unauthorized polling closures likely reduced trust in the electoral process; and road closures due to the marathon imposed physical access frictions. Prior research shows that even minor logistical barriers—such as relocating a polling place or increasing distance to it—can reduce turnout, particularly among marginal voters (Alipour and Lindlacher, 2025; Cantoni, 2020). The Berlin case, by contrast, involves severe, visible breakdowns in electoral delivery in a setting otherwise known for bureaucratic order, providing a test of behavioral and informational mechanisms under extreme but non-strategic administrative stress. Because the failures materialized at the polling place (queues, closures, missing ballots), their most direct behavioral effect should appear in in-person participation; any rise in postal voting is best interpreted as subsequent adaptation rather than the primary incidence of the shock.

Figure 1: Turnout by Election Type in Berlin since 2016
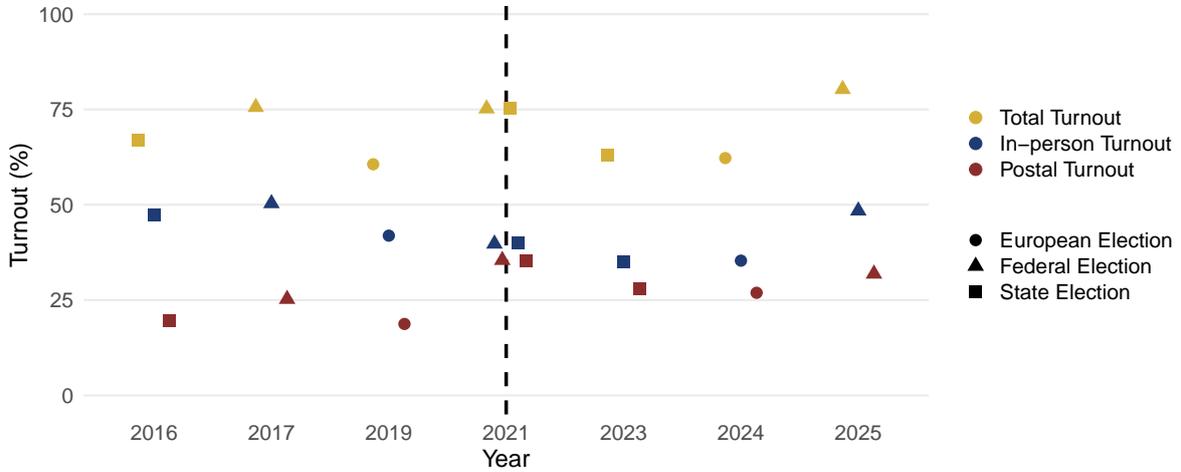
Notes: The figure presents in-person, postal, and total turnout for the European, Federal, and State elections in the sample. The dotted vertical line marks the 2021 election, which triggered the constitutional review.

2.2 Data

Importantly, Germany's electoral system allows for postal voting without excuse, which has steadily expanded since 2008. Voters may request ballots in advance and return them by post or by deposit. As shown in Figure 1, postal turnout has increased over time and now constitutes a major channel of participation. This allows me to distinguish whether participation losses are concentrated in in-person voting and whether they are partly offset by increases in postal voting. An increase in postal turnout alongside a reduction in in-person turnout is consistent with substitution across voting modes, while a decline in total turnout indicates net disengagement.

I construct a precinct-level panel of electoral returns across eight State, European, and Federal elections between 2016 and 2025. The unit of observation is the postal precinct (*Briefwahlbezirk*), the smallest level for which mode-specific turnout is reported and the relevant administrative unit for the court's precinct-level findings. To allow consistent comparisons over time, all data are harmonized to the 2021 precinct geography using population-weighted overlays based on 100-meter census grid data (see Appendix A for details on data sources, harmonization, and treatment coding). Because the 2021 failures were concentrated at polling places (queues, closures, missing ballots), in-person turnout is the primary outcome of interest; changes in postal turnout capture subsequent adaptation.

My main analysis focuses on the 431 precincts annulled by the Federal Constitutional Court. This set provides a uniform, court-validated classification of severe procedural violations at the precinct level, and it aligns with the panel structure of the turnout data.[1] I treat precincts

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[1] While these precincts were subject to a court-ordered federal rerun in 2024, I do not treat the rerun as a separate intervention. The rerun occurred more than two years after the original disruption, featured limited campaign activity, and had very low turnout; Appendix C discusses implications.

affected only by the state court ruling as part of the broader universe of disrupted locations and show in Appendix B that including them yields qualitatively similar but attenuated estimates.

Figure 2 shows the geographic distribution of affected postal precincts. While disruptions cluster in inner-city districts, affected postal precincts are present across most boroughs, supporting comparisons with a broad control group.

Figure 2: Berlin Precincts by Court Mention
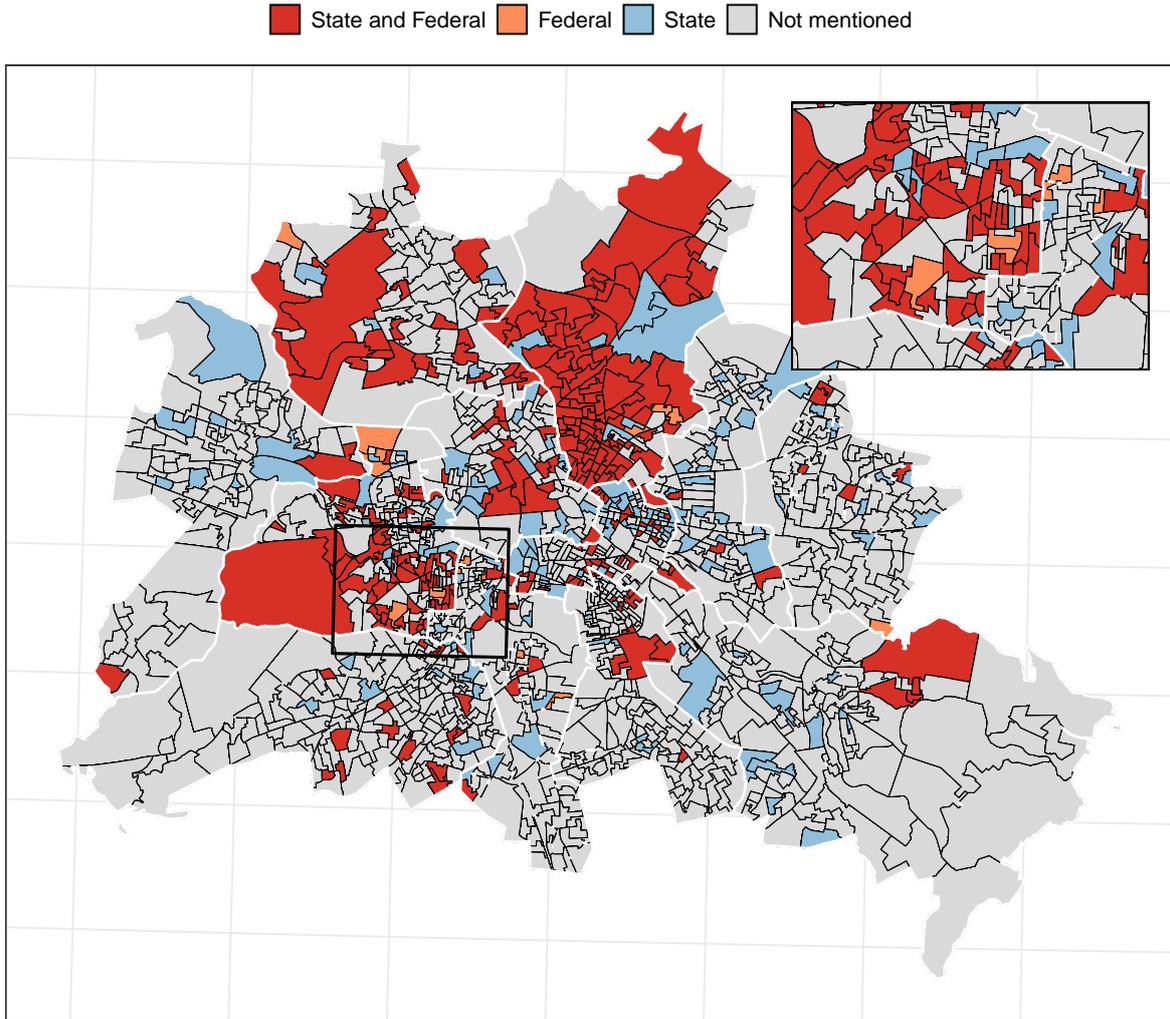
Notes: The figure displays Berlin's postal precincts (*Briefwahlbezirke*) classified by whether they were formally designated as affected by administrative breakdown during the 2021 elections. Status is based on legal rulings by the *Berlin Constitutional Court* (state election), the *Federal Constitutional Court* (federal election), or both. These designations reflect judicial findings of procedural violations and do not distinguish among types of disruption (e.g., ballot errors, long queues). District boundaries are shown in white. The inset zooms into a high-density area to illustrate the fine-grained spatial distribution; its location is chosen for visual clarity.

2.3   Identification

I define Treatment$_i$ = 1 for any postal precinct i that appears on the Federal Constitutional Court's annulment list. Control precincts are the remaining postal precincts within Berlin: they

were administered under the same election, on the same day, by the same city, but were not annulled. The Court annuls a local election result only if it documents concrete procedural violations that are plausibly outcome-relevant ("*mandatsrelevante Wahlfehler*"), including (i) voters receiving incorrect ballot papers, (ii) ballot shortages that forced temporary closures or turned voters away, (iii) queueing that was disproportionate or extended well beyond the legal closing time, and (iv) in some cases, voting after 06:00 p.m. in violation of federal election law. The Court's determination is procedural rather than political: a precinct is annulled because the administration failed to provide a lawful opportunity to vote, not because turnout was low or because parties contested the local result.

Before the 2021 breakdown, treated precincts do not look like "weak" or disengaged areas. In the last pre-breakdown federal election (2017), turnout in treated precincts averaged 78.5%, compared to 76.0% in control precincts; I see similarly higher baseline turnout in the last pre-breakdown state (2016) and European (2019) elections. Treated and control precincts are also similar in age structure and migration background, and treated precincts have lower welfare receipt (11.4% vs. 13.7%; see Appendix Table A9). Treatment is determined ex post by the Court based on documented administrative failures rather than realized electoral outcomes. Identification then comes from within-precinct changes after 2021, controlling for precinct fixed effects and district-by-election fixed effects. Because the disruptions occurred at polling places (queues, closures, missing ballots), the most direct incidence should appear in in-person turnout, with any increase in postal voting capturing subsequent adaptation.

A potential concern is that control precincts are not a "pure" counterfactual, because the 2021 breakdown in Berlin's election administration was widely publicized citywide while polls were still open and in the days that followed. Queues lasting hours, ballot shortages, unlawful polling station closures, and voters being told to return later became a scandal for the entire city and ultimately triggered constitutional review and partial annulment. This creates the possibility of spillovers: residents of precincts that were not formally annulled were nonetheless exposed to the same information shock about administrative competence and could have updated their expectations about the cost of voting in future contests. Such spillovers would tend to depress turnout in both treated and control precincts after 2021, biasing difference-in-differences estimates toward zero. Accordingly, any remaining turnout gap between annulled and non-annulled precincts should be interpreted as the causal effect of severe local exposure relative to the lower level of disruption experienced elsewhere in Berlin; if the citywide information shock reduces participation in both groups, this contrast will mechanically be attenuated and should be viewed as conservative with respect to the direct-exposure component.

2.4 Estimation

I estimate a dynamic difference-in-differences model using a stacked event-study design. All treated precincts were affected simultaneously in 2021, and I observe voting outcomes for the same spatial units across multiple elections before and after the disruption.

The estimating equation is:

$$Y_{idt} = \sum_{\tau \neq -1} \beta_\tau \, \mathbb{1}\{t = \tau\} \times \text{Treatment}_i \; + \; \alpha_i \; + \; \alpha_{dt} \; + \; \varepsilon_{idt}, \tag{1}$$

where Y_{idt} denotes the natural log of turnout (total, in-person, or postal) in postal precinct i, district d, and election t. Treatment_i is an indicator for whether precinct i appears on the Federal Constitutional Court's annulment list. $\mathbb{1}\{t = \tau\}$ is an indicator for election t being the election indexed by event time τ, with $\tau = -1$ normalized to the 2019 European Parliament election. Event time $\tau \in \{-3, -2, 0/F, 0/S, 1, 2, 3\}$ indexes elections from 2016 to 2025.[2]

The model includes postal precinct fixed effects α_i to absorb time-invariant spatial heterogeneity and district-by-election fixed effects α_{dt} to flexibly capture time shocks that vary across districts. Standard errors are clustered at the precinct level. The coefficients β_τ trace the dynamic effect of exposure to administrative failure: estimates at $\tau = 0$ capture the immediate impact in 2021, while post-treatment coefficients ($\tau > 0$) describe persistence or attenuation over time. Estimating Equation 1 separately for in-person and postal turnout allows me to characterize whether the shock primarily reduces in-person participation and the extent to which subsequent postal voting offsets these declines.

The main specifications estimate Equation 1 with turnout outcomes in logs. Using logs corresponds to proportional treatment effects and helps stabilize residual variance across precincts and across election types with different baseline participation levels. To facilitate interpretation in percentage points, I report implied level effects computed as

$$\Delta \text{Turnout}_t^{pp} = \left(\exp(\hat{\beta}_t) - 1 \right) \times \overline{\text{Turnout}}_t^{\text{ctrl}}, \tag{2}$$

where $\overline{\text{Turnout}}_t^{\text{ctrl}}$ is the mean turnout among control precincts in election t. Appendix D reproduces the main event-study estimates in levels and reports the implied percentage-point translation from the log specification, which makes the substitution accounting transparent.

3 Results

3.1 Effects on Voter Turnout

This section presents the main event-study estimates of the impact of Berlin's 2021 election-day administrative breakdown on subsequent electoral participation. The disruption occurred at polling places (queues, closures, missing ballots), so the most direct incidence should appear in *in-person* turnout; changes in postal turnout capture subsequent adaptation. Using Equation 1, I track in-person turnout, postal turnout, and total turnout in treated and untreated precincts across multiple election cycles. Each election is indexed by event time $\tau \in \{-3, -2, -1, 0/F, 0/S, 1, 2, 3\}$, with $\tau = -1$ corresponding to the 2019 European Parliament

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[2] $0/F$ and $0/S$ refer to the 2021 Federal and State elections, which were held concurrently.

election. Figure 3 visualizes the estimated coefficients $\hat{\beta}_\tau$ by voting mode; the corresponding estimates are reported in Table 1.[3]

Figure 3: Main Specification
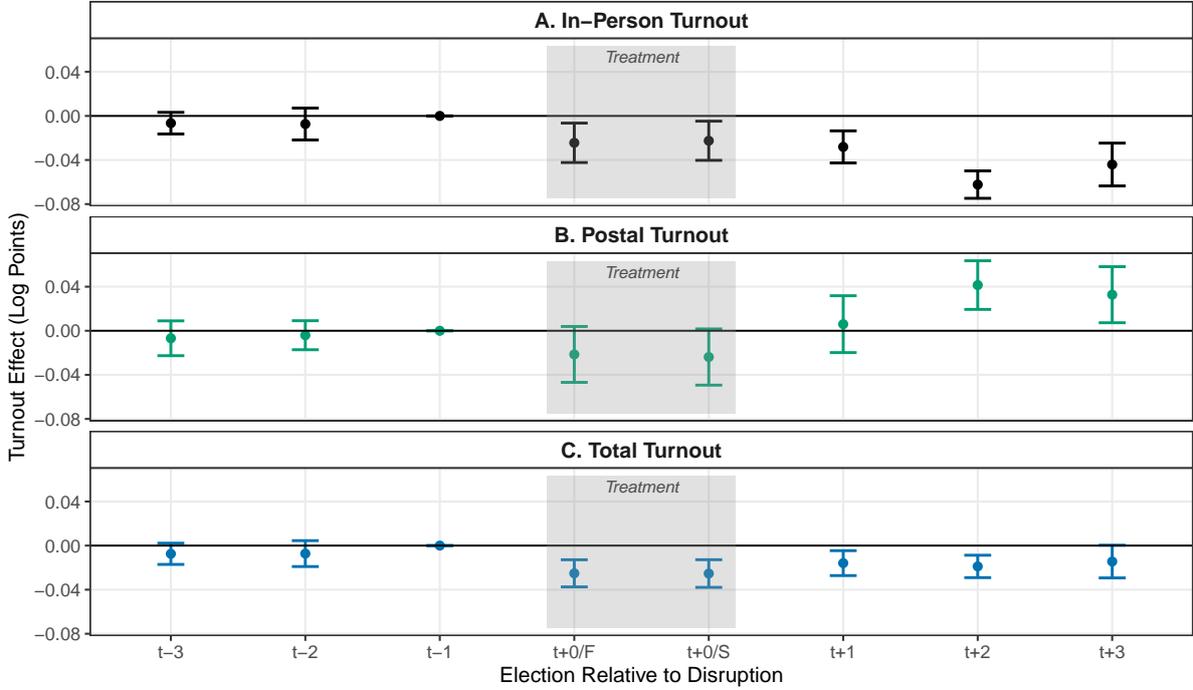
Notes: The figure presents event study results based on Equation 1 for the natural log of in-person, postal, and total turnout. The reference election $(t-1)$ is the 2019 European election. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the postal precinct level. The point estimates and standard errors underlying the results appear in Table 1.

The pre-treatment coefficients ($\tau = -3$ and $\tau = -2$) are small and statistically indistinguishable from zero across all turnout outcomes, supporting the plausibility of parallel trends. In implied percentage-point terms, pre-period differences are substantively small (Appendix D).

The largest immediate effects occur in the disruption year 2021 ($\tau = 0$). In treated precincts, in-person turnout declines sharply in both the federal and state elections held on the same day. Total turnout also falls. Postal voting does not rise contemporaneously; if anything it slightly declines in 2021, consistent with the shock arriving after postal decisions were already made. This pattern is consistent with an election-day cost shock that is concentrated at the polling place: voters who intended to vote in person faced queues, closures, and ballot shortages, and a subset did not complete the act of voting. The absence of an immediate increase in postal turnout in 2021 is consistent with the fact that postal voting requires advance action (information, requesting materials, and meeting deadlines) and therefore cannot fully insure against an unanticipated election-day shock, even if it becomes a margin of adjustment in subsequent cycles.

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[3] Because the outcome is in logs, $\hat{\beta}_\tau$ is a proportional change. For interpretability, I report implied percentage-point effects computed as $(\exp(\hat{\beta}_\tau) - 1) \times \bar{Y}_t^{ctrl}$, where $\bar{Y}_t^{ctrl}$ is the control-group mean turnout in election t. Appendix Figure A2 reproduces the main event study in levels.

In subsequent elections, the in-person turnout gap persists and, if anything, becomes more pronounced. In the 2023 state rerun ($\tau = 1$), in-person turnout remains depressed in treated precincts, with little evidence of offsetting changes in postal voting. By the 2024 European Parliament election ($\tau = 2$), postal turnout rises significantly in treated areas while in-person turnout is substantially lower, indicating behavioral adaptation in the form of substitution toward mail voting.[4] The substitution is nonetheless incomplete: total turnout remains below the control precincts. The same pattern continues into the 2025 federal election ($\tau = 3$): postal participation is higher in treated precincts, but in-person participation remains persistently lower, leaving a residual deficit in total turnout.

Figure 3 and Table 1 show that the treatment effect operates primarily through a persistent decline in *in-person* participation, with only partial substitution into postal voting. Because the event-study coefficients are estimated in logs, mode-specific effects are not mechanically additive. Appendix D therefore reports an implied percentage-point decomposition based on Equation 2, which makes the substitution accounting transparent: in later elections, postal turnout offsets a sizable share of the in-person decline, but not all of it, so total turnout remains lower in treated precincts.

The results imply that administrative breakdown can have durable consequences beyond the affected election. The persistence of the in-person turnout gap is consistent with disrupted civic habits and/or belief updating about the reliability of election administration, while the delayed rise in postal voting suggests partial adaptation rather than full recovery.

Finally, because turnout shocks can also affect representation by changing the composition of the electorate, Appendix E examines treatment effects on party vote shares and finds modest shifts relative to the size of the participation effects.

3.2 Disruption Intensity: Waiting Times

While the main analysis uses a binary treatment indicator based on legal rulings, not all affected precincts experienced administrative breakdown of equal severity. This subsection examines whether the intensity of disruption—measured as maximum waiting time in hours—is associated with differential effects on turnout. I thus shift from a dichotomous to a continuous treatment definition, testing for dose-response effects of election-day failure.

Disruption intensity is operationalized using precinct-level estimates of queue length during peak voting hours, as recorded in the official ruling of the Berlin Constitutional Court. These data, reported in hours, capture the severity of procedural breakdowns at the polling station level and are aggregated to the postal precinct (*Briefwahlbezirk*).[5]

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[4] The 2024 partial rerun of the federal election is not included in the panel analysis because voting occurred only in affected precincts, leaving no within-election control group. Importantly, the participation gap is already visible prior to the partial rerun: in the universal rerun of the state election (open to all precincts), treated precincts exhibit lower in-person and total turnout. The partial federal rerun itself is best understood as an institutional response to the 2021 administrative failure and therefore a potential downstream channel through which the failure may affect subsequent participation, rather than a separate confounding shock.

[5] For more detail, see Appendix A.

Table 1: Main Specification

	In-person Turnout (1)	Postal Turnout (2)	Total Turnout (3)
Treatment (t-3)	-0.0066	-0.0068	-0.0074
	(0.0050)	(0.0081)	(0.0049)
Treatment (t-2)	-0.0073	-0.0040	-0.0073
	(0.0074)	(0.0067)	(0.0060)
Treatment (t+0/Federal)	-0.0244***	-0.0215*	-0.0252***
	(0.0091)	(0.0129)	(0.0063)
Treatment (t+0/State)	-0.0225**	-0.0238*	-0.0254***
	(0.0091)	(0.0130)	(0.0064)
Treatment (t+1)	-0.0281***	0.0060	-0.0159***
	(0.0074)	(0.0132)	(0.0058)
Treatment (t+2)	-0.0623***	0.0414***	-0.0189***
	(0.0064)	(0.0112)	(0.0052)
Treatment (t+3)	-0.0441***	0.0327**	-0.0145*
	(0.0099)	(0.0130)	(0.0076)
R^2	0.88387	0.92498	0.91729
Observations	10,800	10,800	10,800
Precinct FE	✓	✓	✓
Election-District FE	✓	✓	✓

Notes: The table presents event study results based on Equation 1 for the natural log of in-person turnout, postal turnout, and total turnout. The reference election $(t-1)$ is the 2019 European Election. Standard errors are clustered at the postal precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 4 plots the estimated effects using an interaction between waiting time and relative election year, following the stacked event-study framework. The results show a clear and persistent relationship between disruption intensity and turnout suppression.

In the immediate aftermath $(t+0)$, each additional hour of waiting is associated with a decline in turnout that is concentrated in in-person participation. Consistent with the binary treatment results, the in-person effect is larger than the effect on total turnout. In later elections, the dose-response pattern persists: longer waiting times predict larger and more persistent reductions in in-person turnout.

Postal turnout exhibits the opposite pattern in later elections: precincts with longer waiting times in 2021 show increases in mail voting in $t+2$ and $t+3$, consistent with delayed adaptation. These increases partially offset in-person losses but do not eliminate the total turnout gap.

I interpret this as administrative failures depressing turnout in a dose-dependent manner. Voters appear to react not only to the presence of administrative breakdown but to their severity. The results also support the substitution narrative: while some affected voters eventually shift to mail voting, this behavioral compensation is incomplete and delayed.

These patterns have both theoretical and policy implications. From a theoretical perspective, the dose-response shape is consistent with models of civic habit scarring and belief updating: more severe disruptions should lead to stronger belief shocks and greater habit discontinuities. From a policy standpoint, the results highlight queue duration as a critical administrative metric.

Figure 4: Treatment Intensity
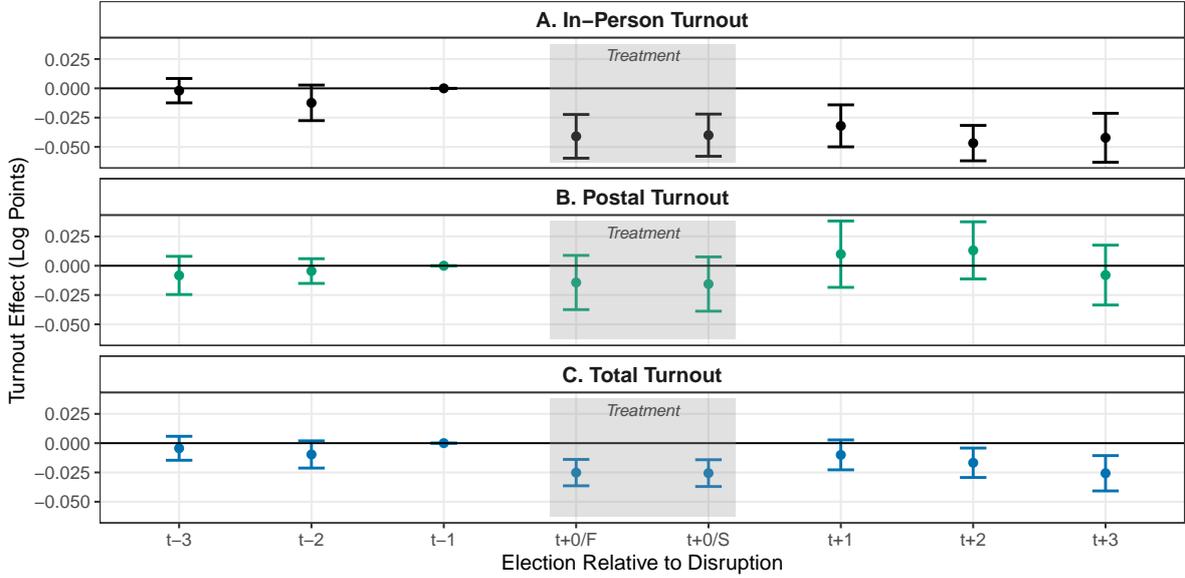
Notes: The figure presents triple-interaction estimates based on Equation 1 for the natural log of in-person turnout, postal turnout, and total turnout. The treatment variable is the maximum waiting time (in hours) per postal precinct during the 2021 elections, as recorded by the Berlin Constitutional Court. Each point shows the effect of one additional hour of waiting on turnout in a given election. The sample is restricted to postal precincts that are affected by longer waiting times, disruptions, or longer opening hours, and the control group. The reference election ($t-1$) is the 2019 European Election. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the postal precinct level. The point estimates and standard errors underlying the results appear in Appendix Table A10. All first and second-order interaction terms required for the identification of the triple-difference estimator are included in the specification or absorbed by the fixed effects.

Long lines are not merely logistical frictions; they are visible signals of state dysfunction that can undermine democratic participation in the long run. Monitoring and mitigating wait times—via staffing buffers, queue management systems, or expanded early/postal voting—should be viewed as core components of electoral resilience.

3.3 Heterogeneous Effects by Socio-Demographics

The average effects in Figure 3 show a persistent decline in in-person turnout following the 2021 administrative breakdown. Theory suggests that such scarring may vary across socio-demographic environments: participation costs, time constraints, and voting habits are not evenly distributed, and disruptions at the polling place may differentially affect voters whose participation routines are less entrenched. I examine heterogeneity by interacting treatment with standardized precinct characteristics in a triple-difference event-study specification (Equation 3). Because the disruption occurred at polling places, the analysis focuses on the natural log of *in-person* turnout.

$$Y_{idt} = \sum_{\tau \neq -1} \gamma_\tau \, \mathbb{1}\{t = \tau\} \times \text{Treatment}_i + \sum_{\tau \neq -1} \mu_\tau \, \mathbb{1}\{t = \tau\} \times \text{Treatment}_i \times Z_i$$
$$+ \sum_{\tau \neq -1} \rho_\tau \, \mathbb{1}\{t = \tau\} \times Z_i + \eta_i + \eta_{dt} + \varepsilon_{idt}, \tag{3}$$

where Z_i is a standardized (mean-zero, unit-variance) precinct characteristic. The coefficients μ_τ capture whether the treatment effect varies with Z_i at event time τ. Appendix Table A12 reports the corresponding estimates for age shares; Appendix B reports analogous results for other socio-demographic covariates.

Age. Age exhibits the clearest and most systematic gradient. As shown in Figure 5, precincts with larger shares of younger adults display larger declines in in-person turnout after 2021, whereas precincts with older age profiles exhibit smaller effects. This pattern is consistent with models of voting as habit-forming behavior: younger cohorts have less entrenched participation routines and may be more vulnerable to shocks that raise the cost of voting early in their electoral participation histories (Fujiwara et al., 2016; Shino and Smith, 2018; Coppock and Green, 2016). It is also consistent with differential responsiveness to salient signals about institutional reliability among newer voters.

Figure 5: Heterogeneity by Age
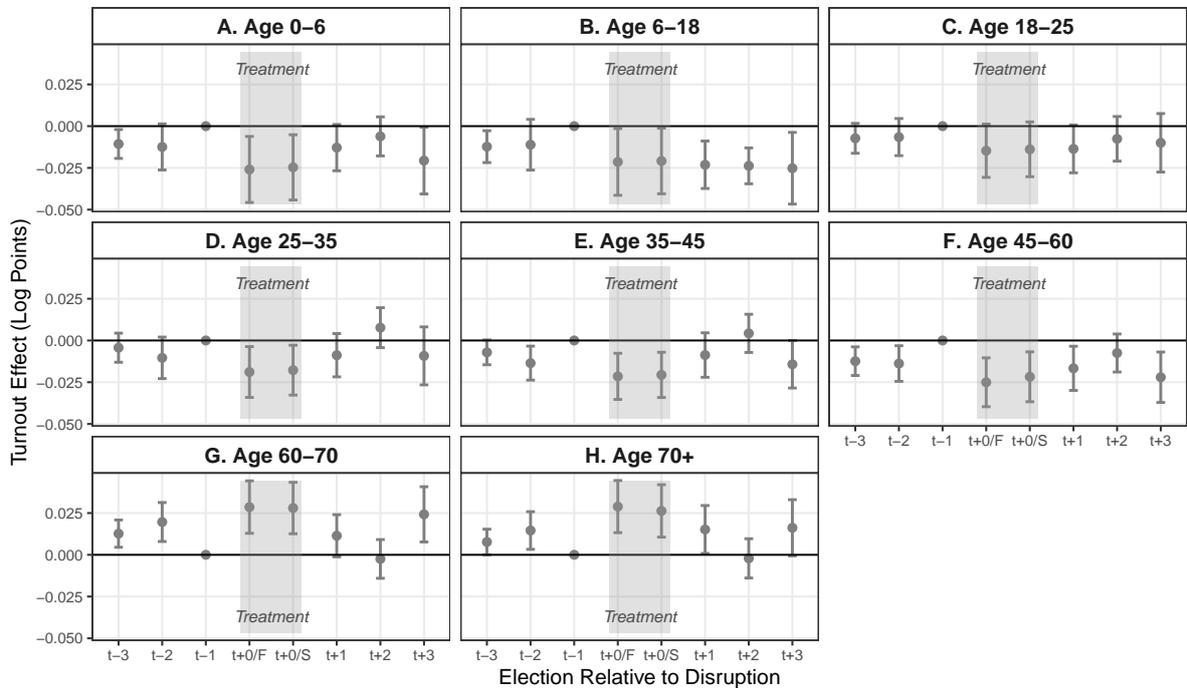
Notes: The figure presents triple-interaction estimates based on Equation 3 for the natural log of in-person turnout. The reference election $(t-1)$ is the 2019 European Election. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the postal precinct level. The point estimates and standard errors underlying the results appear in Appendix Table A12. All first- and second-order interaction terms required for the triple-difference estimator are included or absorbed by the fixed effects.

Other socio-demographic covariates. In contrast, I find little evidence that the in-person turnout response is systematically heterogeneous by precinct shares of residents with a migration background, welfare receipt, or gender composition. The corresponding interaction coefficients are generally small relative to the average treatment effect and are not robustly distinguishable from zero across event times; results are reported in Appendix Figure A4 and Appendix Table A11. The heterogeneity analysis suggests that the scarring effect is broad-based in the precinct-level data, with the clearest gradient appearing along age.

4 Political Consequences of Electoral Disruption

4.1 Behavioral and Attitudinal Effects

This section provides survey-based evidence consistent with the mechanisms behind the turnout results. The event-study estimates show a persistent decline in *in-person* voting in treated precincts, with only gradual and incomplete substitution toward postal ballots. These reduced-form patterns are consistent with (i) habit disruption—some voters who intended to vote in person in 2021 did not complete the act of voting—and (ii) belief updating about the cost and reliability of election administration. Precinct-level turnout cannot separately identify these channels, so I turn to individual-level survey measures that speak to both.

I use pooled data from the German Longitudinal Election Study (GLES) Rolling Cross-Section surveys for the 2017, 2021, and 2025 federal elections. I estimate a difference-in-differences specification comparing respondents in Berlin to respondents in the rest of Germany, using 2017 as the pre-treatment baseline:

$$Y_{ist} = \sum_{\kappa \neq 2017} \beta^{\kappa} \left(\mathbb{1}_{\kappa=t} \times \text{Berlin}_i \right) + \gamma_s + \lambda_t + u_{ist}, \tag{4}$$

where Y_{ist} is the outcome for individual i in state s and election year t, $gamma_s$ are state fixed effects, and λ_t are year fixed effects. Standard errors are clustered at the state level. The coefficients β^{κ} capture differential changes in Berlin relative to the rest of Germany compared to 2017. Panel A of Figure 6 reports beliefs about procedural integrity. Immediately after the 2021 election, Berlin respondents are substantially less likely to agree that "the federal election was conducted correctly and fairly" than respondents elsewhere. This is consistent with an informational shock: queues, ballot shortages, temporary closures, and voting after the legal closing time provided salient evidence of administrative failure. By 2025, the Berlin–rest-of-Germany gap narrows, suggesting partial recovery in stated trust.

Panel B focuses on abstention timing among self-identified nonvoters. In 2021, Berlin nonvoters are far more likely than nonvoters elsewhere to report deciding not to vote on election day or being effectively prevented from voting. This pattern is consistent with acute, same-day frictions at the polling place rather than gradual disengagement. The survey evidence matches the turnout dynamics. The 2021 breakdown is associated with a sharp decline in perceived procedural integrity and with unusually high reports of last-minute or forced abstention. While

stated trust partly normalizes by 2025, the precinct-level in-person turnout gap does not fully close, consistent with the idea that behavioral scarring can outlast the recovery of expressed attitudes.

I emphasize two caveats. First, Berlin sample sizes in the GLES are modest, especially within the nonvoter subsample, so estimates in Panel B are imprecise. Second, Equation 4 is not a causal design for precinct-level exposure; I therefore treat these results as descriptive evidence consistent with the mechanisms highlighted by the turnout analysis.

Figure 6 summarizes these results.

Figure 6: Behavioral and Attitudinal Responses in Berlin vs. the Rest of Germany
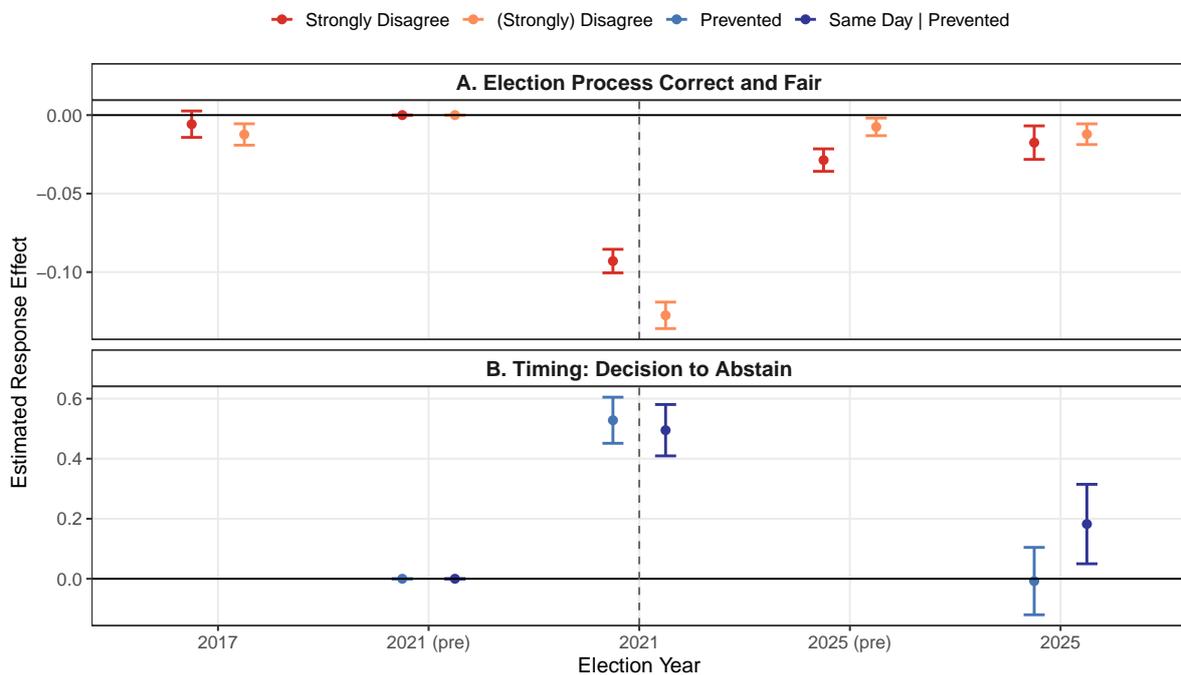
Notes: I plot coefficients from Equation 4 using GLES Rolling Cross-Section data from the 2017, 2021, and 2025 federal elections. Panel A shows agreement with the statement that the election was conducted correctly and fairly. Panel B is restricted to self-reported nonvoters (approximately 510 individuals across years) and reports the probability that respondents either (i) decided not to vote on election day or (ii) state that they were prevented from voting. For interpretability, the y-axis in each panel can be read as the difference (in percentage points) between Berlin and the rest of Germany in a given election year, relative to 2017. Standard errors are clustered at the state level. The reference category is 2017. The point estimates and standard errors underlying the results appear in Appendix Table A13.

4.2 Administrative Response and Institutional Adjustment

A natural question is whether administrators adjusted capacity in response to the 2021 breakdown. I examine whether precincts that experienced more severe disruption subsequently received additional polling infrastructure. I estimate difference-in-differences regressions where the outcome is the number of polling stations in a precinct and election. The key regressor is disruption intensity in 2021, measured as total reported queueing time (in hours) across in-person polling stations within a postal precinct, interacted with indicators for 2023, 2024, and 2025.

The specification includes precinct fixed effects and election fixed effects. Results are shown in Table 2.[6]

The estimates indicate a delayed capacity response. Precincts with longer waiting times in 2021 are associated with larger increases in polling-station capacity in 2024, with a smaller positive association in 2025, and little change in 2023. While these patterns do not establish that expanded capacity mitigated turnout declines, they are consistent with administrators reallocating resources toward previously disrupted areas.

Table 2: Administrative Response: Change in Polling Station Capacity

	Number PS	
	(1)	(2)
Treatment (t+1)	-0.0171	0.0407
	(0.0260)	(0.0321)
Treatment (t+2)	0.8199***	0.4798***
	(0.1918)	(0.1697)
Treatment (t+3)	0.0566	0.0927***
	(0.0351)	(0.0336)
Specification	Binary	Waiting Time in h
R^2	0.89819	0.89723
Observations	5,400	5,400
Precinct FE	✓	✓
Election-Distr. FE	✓	✓

Notes: The table presents event study results based on a similar regression as Equation 1 for the number of polling stations within a postal precinct. The reference election $(t+0)$ is the 2021 Federal Election. Standard errors are clustered at the postal precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5 Theoretical Interpretation

Why can a one-off breakdown in election administration generate persistent participation losses after the immediate logistics normalize? This section outlines a simple behavioral framework in which an election-day disruption affects future turnout through two reinforcing channels: (i) habit scarring—missing an election weakens subsequent voting propensity—and (ii) belief updating about the expected cost and reliability of in-person voting. The framework is intended to organize interpretation rather than deliver sharp structural predictions.

Consider a voter's net utility from voting at election t:

$$U_t = p_t B + d_t - c_t, \tag{5}$$

where $p_t B$ captures the perceived benefit of voting (instrumental or expressive), d_t is a habit or civic-duty component, and c_t is the anticipated cost of participation. The voter turns out if $U_t > 0$.

---

[6] Administrative data on polling station counts are available from 2021 through 2025.

Election-day disruptions (queues, closures, ballot shortages) raise c_t and can induce abstention even among voters who intended to participate. If voting is habit-forming (Fujiwara et al., 2016; Coppock and Green, 2016), abstention has dynamic consequences. A parsimonious representation is

$$d_{t+1} = \bar{d} + \rho v_t, \tag{6}$$

where $v_t \in \{0,1\}$ denotes turnout at t and $\rho > 0$ captures reinforcement. Missing an election reduces d_{t+1}, lowering turnout at subsequent elections. Because elections are infrequent, a single missed act can have effects that persist across multiple cycles, especially for voters with weaker pre-existing routines.

Disruptions also provide a salient signal about the reliability of election administration. Let voters observe a noisy signal $s = \theta + \varepsilon$ about administrative quality θ. A negative signal shifts beliefs and increases the expected cost of future participation, particularly for in-person voting:

$$c_{t+1} = \bar{c} + \phi(s), \quad \phi'(s) < 0. \tag{7}$$

Even if objective service quality improves, pessimistic beliefs can keep expected costs elevated for some time, reducing turnout (Kawai et al., 2021; Alsan and Wanamaker, 2018; Lowes and Montero, 2021; Akhtari et al., 2022).

The model also rationalizes substitution across voting modes. If the disruption primarily increases the perceived cost of in-person voting, some voters may adapt by shifting toward postal voting in later elections. However, if habit scarring reduces the propensity to participate at all, or if the perceived cost increase is not fully avoidable, the result is incomplete substitution and a persistent decline in total turnout.

The empirical patterns match these implications. The turnout results show a persistent decline in in-person voting and only gradual, partial substitution into postal voting, leaving a residual deficit in total turnout. The GLES evidence shows a sharp contemporaneous decline in perceived procedural integrity and elevated reports of last-minute or forced abstention in 2021, consistent with both channels. Moreover, stated trust partially recovers by 2025 while the in-person turnout gap does not fully close, consistent with the idea that behavior can remain scarred even after attitudes begin to normalize.

The framework extends the classical calculus of voting (Riker and Ordeshook, 1968) by highlighting that voting costs are not only legal or structural, but also experiential: a salient administrative failure can shift both habits and expectations, generating durable effects on democratic participation.

6   Conclusion

This paper shows that even *non-strategic* administrative failure in the delivery of a core public service can durably reduce democratic participation. Using court-validated exposure to the 2021 Berlin election breakdown and a precinct-level stacked event study, I find that the persistent effect operates primarily on the *in-person* margin. In elections after 2021, treated precincts

exhibit sizable and sustained declines in in-person turnout (about -1.0 pp in 2023 and about -2.1 pp in 2024 and 2025), indicating lasting scarring in the channel directly affected by election-day disruptions.

At the same time, voters partially adapt by shifting to postal voting. Postal participation rises in later elections (about $+1.1$ pp in 2024 and 2025), offsetting a meaningful share of the in-person decline. The implied offset rate averages about 0.40 across post-2021 elections and reaches roughly one-half in 2024–2025. However, substitution is incomplete: total turnout remains persistently lower in treated precincts (about -1.1 pp on average across the three post-2021 elections). In short, administrative failure primarily depresses in-person participation; vote-mode substitution mitigates but does not eliminate the overall participation loss.

Survey evidence is consistent with two reinforcing mechanisms: acute election-day frictions that induce last-minute abstention and a contemporaneous decline in perceived procedural fairness. While stated trust partially recovers over time, depressed in-person turnout in affected precincts persists, consistent with durable habit scarring in the participation mode most exposed to administrative breakdown.

The broader implication is that elections are recurring encounters with state capacity. When the state fails at basic electoral logistics, it can raise the realized and expected cost of participation, and the behavioral consequences persist beyond the election day itself. From a policy perspective, the results are cautionary but actionable: safeguarding in-person voting through capacity buffers, real-time queue monitoring, deployable staffing and materials, and scalable postal/early-voting infrastructure can prevent avoidable, persistent losses in democratic participation.

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Online Appendix

Administrative Failure, State Capacity, and Democratic Exclusion: Evidence from Berlin's 2021 Election Breakdown

by *Marius Kröper*

A Data Sources and Harmonization

A.1 Main Administrative and Electoral Data

The core dataset is a harmonized precinct-level panel covering all Berlin state, federal, and European elections between 2016 and 2025. The unit of observation is the 2021 postal precinct, which serves as the constant spatial frame across time. Election results are available at the precinct level (*Urnen-* and *Briefwahlbezirke*). Because precinct boundaries vary between elections, all historical data are harmonized to the 2021 delineations. For each election, I first aggregate results from in-person precincts (*Urnenwahlbezirke*) to the postal precinct (*Briefwahlbezirke*) level, the smallest administrative unit consistently defined across elections. I then construct population-weighted spatial crosswalks linking each election's precinct geometries (2016, 2017, 2019, 2024, and 2025) to the 2021 configuration.

The crosswalks are based on 100-meter raster population data from the 2022 census (restricted to German citizens aged 18 and older), aggregated over the intersection of historical and 2021 precinct polygons. Within each source precinct, population shares are normalized to yield conversion weights that reallocate votes and socio-demographic variables to the 2021 boundaries. This approach primarily aggregates smaller historical precincts to larger 2021 units but proportionally divides overlapping areas where necessary. The resulting panel includes consistent precinct identifiers, official second-ballot results for all elections, harmonized socio-demographic indicators, and treatment flags for precincts affected by the 2021 election administrative breakdown. Table A15 reports summary statistics. In the main analysis panel, a small number (38 of 10,800) of harmonized precinct-years exhibit turnout above 100% due to the interaction of boundary harmonization and the allocation of external/postal ballots.

A potential limitation of the harmonization is that precinct borders changed substantially between some elections, particularly prior to 2016. Fixing all observations to the 2021 delineations may thus introduce minor measurement errors if population density or voter composition differs within reallocated areas. Three considerations mitigate this concern: (i) precincts are small, with about 1,500 eligible voters on average; (ii) crosswalk weights are based on 100-meter census population counts (rather than polygon area); and (iii) results are robust to alternative harmonization strategies that downweight, or exclude in a balanced panel, precinct–election observations with fragmented crosswalk mappings as measured by HHI and $k90$ (see Appendix B).

The dataset spans eight elections across seven election years: European Parliament (2019, 2024), state (*Abgeordnetenhaus*) elections (2016, 2021, 2023), and federal (*Bundestag*) elections (2017, 2021, 2025). Precinct shapefiles are obtained from the Berlin Electoral Office.[7] Turnout and party vote shares are computed relative to eligible voters, with separate indicators for postal and in-person voting. I retain postal precincts as the analytical unit, as they provide complete coverage of both voting modes and cleanly map to treatment assignments defined by the 2021 rerun decision.

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[7] See, https://daten.berlin.de/datensaetze, last accessed on Oct 27, 2025.

Socio-demographic controls come from Berlin's structural reports (*Strukturdaten*) for 2021, including population size, age composition, gender, migration background, and welfare dependency rates, merged via unique precinct identifiers.[8] Administrative disruption data are derived from the Berlin State Court and Federal Constitutional Court rulings on the 2021 elections, which identify ballot shortages, waiting times, and other administrative breakdown by polling station. These indicators are aggregated to the postal precinct level to define treatment and exposure intensities.

I quantify the severity of disruption in 2021 at the precinct level using official documentation from the State Constitutional Court (Verfassungsgerichtshof des Landes Berlin, 2022) and the Federal Constitutional Court (Bundesverfassungsgericht, 2023). The court record describes, for each annulled postal precinct, concrete administrative failures: polling stations that temporarily closed, stations that ran out of the correct ballots or distributed the wrong ballots, and queues that forced voters to wait well beyond the scheduled close of polls at 06:00 p.m. or to vote after 06:00 p.m. in violation of federal election law. I code these reports into a measure of waiting time (in hours) and aggregate them to the postal precinct, which allows us to estimate dose–response relationships rather than a simple treated/control contrast. Higher recorded waiting times in 2021 predict substantially larger and more persistent reductions in subsequent in-person turnout, as well as a gradual shift toward postal voting in later elections.

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[8] See, `https://www.statistik-berlin-brandenburg.de/`, last accessed on Oct 27, 2025.

B   Robustness Checks

B.1   Placebo: Randomization Inference

I utilize a randomization inference approach following Heß (2017), conducting 999 permutations while maintaining the number of treated postal precincts within each district. This method assesses whether the observed results could arise by random chance. The results, presented in Table A1, show significant treatment effects consistent with the original analysis, thus reinforcing the reliability of my findings. This approach helps to ensure that the results are due to the actual treatment effect of the administrative breakdown during the 2021 Federal elections and not random variation.

Table A1: Randomized Inference

	In-person Turnout (1)	Postal Turnout (2)	Total Turnout (3)
Treatment (t-3)	-0.0066	-0.0068	-0.0074
	(0.0050)	(0.0081)	(0.0049)
	[0.1812]	[0.3734]	[0.1111]
Treatment (t-2)	-0.0073	-0.0040	-0.0073
	(0.0074)	(0.0067)	(0.0060)
	[0.3313]	[0.5265]	[0.1982]
Treatment (t+0/Federal)	-0.0244***	-0.0215*	-0.0252***
	(0.0091)	(0.0129)	(0.0063)
	[0.0080]***	[0.0951]*	[0.0000]***
Treatment (t+0/State)	-0.0225**	-0.0238*	-0.0254***
	(0.0091)	(0.0130)	(0.0064)
	[0.0210]**	[0.0661]*	[0.0000]***
Treatment (t+1)	-0.0281***	0.0060	-0.0159***
	(0.0074)	(0.0132)	(0.0058)
	[0.0000]***	[0.6446]	[0.0090]***
Treatment (t+2)	-0.0623***	0.0414***	-0.0189***
	(0.0064)	(0.0112)	(0.0052)
	[0.0000]***	[0.0050]***	[0.0010]***
Treatment (t+3)	-0.0441***	0.0327**	-0.0145*
	(0.0099)	(0.0130)	(0.0076)
	[0.0000]***	[0.0040]***	[0.0631]*
Observations	10,800	10,800	10,800
Precinct FE	✓	✓	✓
Election-District FE	✓	✓	✓

Notes: The table presents event study results based on Equation 1 for the natural log of in-person turnout, postal turnout, and total turnout. The p-values calculated based on the randomized inference approach following Heß (2017) are shown in brackets. The reference election $(t-1)$ is the 2019 European Election. Parentheses report cluster-robust asymptotic standard errors from the baseline specification. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.2 Treatment Definition: Federal vs. State Disruptions

While the core empirical design aggregates disruptions into a single treatment indicator, election-day administrative breakdown varied substantially in nature and timing. In particular, disruptions occurred during either the Federal or the State election held concurrently in 2021, enabling an indirect exploration of heterogeneity by election tier. The most severe disruptions occurred during the Federal election and involved highly visible administrative failures: precincts accepting ballots after the legal closing time, redirecting voters between locations, or remaining open well past 6:00 p.m. By contrast, precincts affected only at the State level were typically flagged for procedural violations such as missing or misallocated ballots—issues that may have been less visible to voters or corrected before deterring participation. To probe whether the salience of disruptions matters for turnout effects, I estimate separate event-study specifications for three mutually exclusive groups: (i) precincts affected during the Federal election (my primary treatment group); (ii) precincts affected only during the State election; and (iii) unaffected precincts, which serve as the counterfactual. Figure A1 presents the resulting trends. Turnout suppression is concentrated in precincts disrupted during the Federal election: the immediate decline at $t + 0$ is statistically significant and remains directionally negative in subsequent elections. By contrast, precincts with State-only disruptions exhibit no detectable turnout effects, consistent with the view that these disruptions were less salient or behaviorally consequential. This interpretation is supported by descriptive evidence on the nature of administrative breakdown. Table A2 tabulates the frequency of ballot-related errors, operational breakdowns, and violations of legal closing times. Precincts affected during the Federal election were far more likely to experience high-salience disruptions—such as 212 cases of late closing and 101 classified as general disruptions—compared to those affected only at the state level. These findings suggest that persistent disengagement is not driven by treatment status, but emerges when administrative failures are both visible and severe enough to undermine voters' perceptions of procedural legitimacy.

Table A2: Treatment Overview

Treatment Federal	Treatment State	Ballot Error	Ballot Missing	Disruption	Unusual Waiting	Open After Official Closing	N
yes	*yes*	32	13	101	90	212	277
no	*yes*	45	61	2	55	1	157
Subsample: Disruption = 0 & Open A.O. Closing = 0							
yes	*yes*	1	2	–	16	–	17
no	*yes*	44	61	–	55	–	154

Notes: The table presents the number of precincts by treatment during the Federal and State elections. The type of irregularity is available only for the State election.

Figure A1: Heterogeneity by Type of Treatment
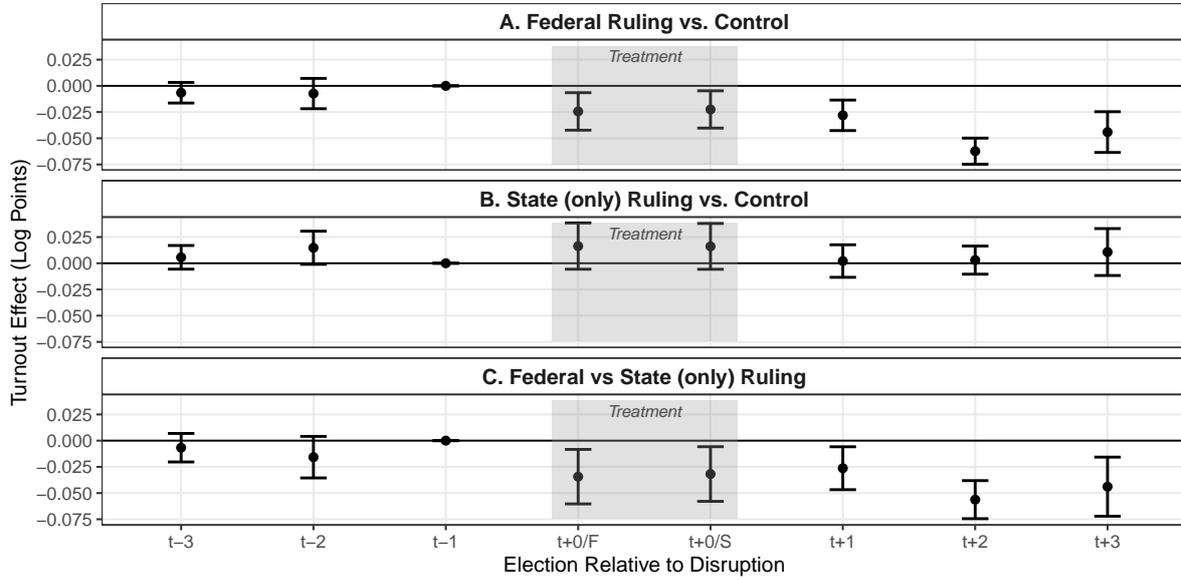
Notes: The figure presents triple-interaction estimates for the natural log of in-person turnout, postal turnout, and total turnout. The reference election $(t-1)$ is the 2019 European Election. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the postal precinct level.

B.3   Controls

In the main specification, I only control for the log number of eligible voters held constant to the 2017 Federal Election, the last federal election before the treatment, and interact them with the election indicators. To test the robustness of the main specification, I add further time-invariant controls, holding them constant for 2019. Controls are the share of the following groups: German citizens with migrant backgrounds, foreigners, persons subject to social security, 6-18 year olds, 18-25 year olds, 65 to 99 year olds, EU citizens, persons living in a civil union, Single households; additionally, I control for the log number of eligible voters. Results are shown in Table A3.

B.4   Clustering

In the baseline analysis, I cluster at the level of postal precincts, which is the level at which the treatment is applied. Two-way clustering—considering precincts, and district × elections results in slightly larger standard errors (Columns 2 and 3 of Table A4) compared to clustering at the postal precinct level.

Table A3: Robustness: Controls

	In-person Turnout (1)	Postal Turnout (2)	Total Turnout (3)
Treatment (t-3)	-0.0043	-0.0040	-0.0051
	(0.0044)	(0.0074)	(0.0040)
Treatment (t-2)	-0.0063	-0.0053	-0.0067
	(0.0052)	(0.0061)	(0.0042)
Treatment (t+0/Federal)	-0.0257***	-0.0196	-0.0228***
	(0.0070)	(0.0128)	(0.0061)
Treatment (t+0/State)	-0.0240***	-0.0215*	-0.0229***
	(0.0071)	(0.0128)	(0.0062)
Treatment (t+1)	-0.0294***	0.0035	-0.0160***
	(0.0076)	(0.0127)	(0.0053)
Treatment (t+2)	-0.0628***	0.0305***	-0.0227***
	(0.0066)	(0.0112)	(0.0051)
Treatment (t+3)	-0.0431***	0.0182	-0.0187***
	(0.0075)	(0.0122)	(0.0059)
R^2	0.92238	0.93415	0.94238
Observations	10,800	10,800	10,800
Precinct FE	✓	✓	✓
Election-District FE	✓	✓	✓

Notes: The table presents event study results based on Equation 1 for in-person turnout, postal turnout, and total turnout controlling for additional variables. The reference election $(t-1)$ is the 2019 European Election. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Robustness: Standard Errors

Panel A: In-person Turnout	(1)	(2)	(3)
Treatment (t-3)	-0.0066	-0.0066	-0.0066
	(0.0050)	(0.0072)	(0.0086)
Treatment (t-2)	-0.0073	-0.0073	-0.0073
	(0.0074)	(0.0074)	(0.0104)
Treatment (t+0/Federal)	-0.0244***	-0.0244***	-0.0244**
	(0.0091)	(0.0076)	(0.0109)
Treatment (t+0/State)	-0.0225**	-0.0225***	-0.0225**
	(0.0091)	(0.0076)	(0.0108)
Treatment (t+1)	-0.0281***	-0.0281***	-0.0281**
	(0.0074)	(0.0085)	(0.0120)
Treatment (t+2)	-0.0623***	-0.0623***	-0.0623***
	(0.0064)	(0.0084)	(0.0095)
Treatment (t+3)	-0.0441***	-0.0441***	-0.0441***
	(0.0099)	(0.0081)	(0.0122)
Standard-Errors	Precinct	Precinct-Election	Precinct & Election-District
R^2	0.88387	0.88387	0.88387

Panel B: Postal Turnout	(1)	(2)	(3)
Treatment (t-3)	-0.0068	-0.0068	-0.0068
	(0.0081)	(0.0116)	(0.0097)
Treatment (t-2)	-0.0040	-0.0040	-0.0040
	(0.0067)	(0.0111)	(0.0056)
Treatment (t+0/Federal)	-0.0215*	-0.0215*	-0.0215*
	(0.0129)	(0.0112)	(0.0125)
Treatment (t+0/State)	-0.0238*	-0.0238**	-0.0238*
	(0.0130)	(0.0113)	(0.0126)
Treatment (t+1)	0.0060	0.0060	0.0060
	(0.0132)	(0.0116)	(0.0145)
Treatment (t+2)	0.0414***	0.0414***	0.0414***
	(0.0112)	(0.0112)	(0.0119)
Treatment (t+3)	0.0327**	0.0327***	0.0327**
	(0.0130)	(0.0118)	(0.0158)
Standard-Errors	Precinct	Precinct-Election	Precinct & Election-District
R^2	0.92498	0.92498	0.92498

Panel C: Total Turnout	(1)	(2)	(3)
Treatment (t-3)	-0.0074	-0.0074	-0.0074
	(0.0049)	(0.0055)	(0.0069)
Treatment (t-2)	-0.0073	-0.0073	-0.0073
	(0.0060)	(0.0062)	(0.0074)
Treatment (t+0/Federal)	-0.0252***	-0.0252***	-0.0252***
	(0.0063)	(0.0055)	(0.0070)
Treatment (t+0/State)	-0.0254***	-0.0254***	-0.0254***
	(0.0064)	(0.0056)	(0.0072)
Treatment (t+1)	-0.0159***	-0.0159**	-0.0159**
	(0.0058)	(0.0064)	(0.0078)
Treatment (t+2)	-0.0189***	-0.0189***	-0.0189***
	(0.0052)	(0.0066)	(0.0068)
Treatment (t+3)	-0.0145*	-0.0145**	-0.0145
	(0.0076)	(0.0065)	(0.0096)
Standard-Errors	Precinct	Precinct-Election	Precinct & Election-District
R^2	0.91729	0.91729	0.91729
Observations	10,800	10,800	10,800
Specification	Precinct	Precinct-Election	Precinct & Election-District
Precinct FE	✓	✓	✓
Election-District FE	✓	✓	✓

Notes: The table presents event study results based on [Equation 1](#) for the natural log of in-person turnout, postal turnout, and total turnout using different standard error calculation methods: Column (1) is the baseline and standard errors are clustered at the precinct level. In Column (2), standard errors are clustered at the state constituency level. Column (3) uses two-way clustered standard errors at the level of postal precinct and District × elections. Standard errors are reported in parentheses. The reference election $(t-1)$ is the 2019 European Election. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.5 Matching on Observables

I conduct various matching procedures to ensure the comparability of the treatment and control group, namely propensity score matching, Entropy Balancing as proposed by Hainmueller (2012), and Mahalanobis distance matching. If applicable, I solely refer to values from the precinct-level structural data measured prior to the 2021 election. I re-evaluate the baseline model (Equation 1) using each matching method and present the findings in Appendix Table A5.

Local Matching First, I use a geographical local matching approach to ensure that the effect is not driven by outliers in the periphery, as postal precincts closer to each other may be more similar. I identify adjacent postal precincts within a district (within 10 meters of the boundaries). I match 295 treated units to 436 untreated units. All treatment effects are close to the baseline estimates and remain statistically significant.

Propensity Score Matching I conduct a propensity score matching procedure to estimate the likelihood of experiencing administrative breakdown during the 2021 Federal elections. This propensity is calculated using a probit regression based on the following pre-treatment socioeconomic characteristics: population measured by the number of eligible voters, share of population aged 6-18, 18-25, 25-35, 35-45, 45-60, 60-70, and over 65, share of foreign population, share of EU citizens, share of females, share of citizens with migrant backgrounds, share of single households, and share of residents subject receiving social security. Additionally, I exact match on the district. All values are precinct-level structural data measured prior to the 2021 election. The matched sample is obtained using 1:1 nearest neighbor matching with replacement. In this process, 294 treated units are matched to 159 control units. The estimates derived from the matched sample confirm my original results for the effect of administrative breakdown on total turnout with the exception of the 2023 State election, which loses significance.

Mahalanobis Matching I check the robustness of my findings by matching treated and untreated units based on the Mahalanobis distance. This method evaluates similarity by measuring the proximity of units' covariates in vector space. I calculate distances using the same pre-treatment covariates as previously mentioned and employ a 1:1 nearest neighbor matching with replacement to create the matched sample. In this process, 294 treated precincts are matched to 162 control units, while 894 units are dropped. The results indicate that the treatment effects remain significant and are consistent with the outcomes of propensity score matching.

Entropy Balancing I implement the entropy balancing approach proposed by Hainmueller (2012). This method offers the advantage of not truncating the sample. Instead, it assigns a set of weights that balance the treatment and control groups across several moments of the covariate distributions. Specifically, I balance the means and variances of the pre-treatment characteristics listed above and use the resulting entropy weights in the event study regressions. The results consistently support my original findings across all specifications and outcomes.

Table A5: Matching

| | In-person Turnout | | | |
	(1)	(2)	(3)	(4)
Treatment (t-3)	-0.0025	-0.0017	0.0024	-0.0036
	(0.0051)	(0.0064)	(0.0062)	(0.0056)
Treatment (t-2)	-0.0057	-0.0007	0.0023	0.0017
	(0.0076)	(0.0091)	(0.0091)	(0.0118)
Treatment (t+0/Federal)	-0.0252***	-0.0150	-0.0228**	-0.0323***
	(0.0096)	(0.0118)	(0.0114)	(0.0123)
Treatment (t+0/State)	-0.0234**	-0.0133	-0.0213*	-0.0303**
	(0.0095)	(0.0117)	(0.0113)	(0.0120)
Treatment (t+1)	-0.0264***	-0.0264***	-0.0314***	-0.0496***
	(0.0077)	(0.0093)	(0.0089)	(0.0114)
Treatment (t+2)	-0.0616***	-0.0583***	-0.0582***	-0.0732***
	(0.0066)	(0.0080)	(0.0078)	(0.0094)
Treatment (t+3)	-0.0437***	-0.0329***	-0.0357***	-0.0513***
	(0.0103)	(0.0126)	(0.0123)	(0.0115)
R^2	0.89019	0.89168	0.89530	0.89627
Observations	5,832	3,632	3,648	10,800
Specification	Local Matching	Propensity Score	Mahalanobis	Entropy Bal.
Precinct FE	✓	✓	✓	✓
Election-District FE	✓	✓	✓	✓

Notes: The table presents event study results based on Equation 1 for the natural log of in-person turnout using different matching approaches. Column (1) is based on a geographical local matching approach. Column (2) uses nearest neighbour matching based on propensity score, and Column (3) based on Mahalanobis distance. Column (4) uses weights from Hainmueller (2012). The reference election ($t-1$) is the 2019 European Election. Standard errors are clustered at the precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.6 Spatial Sample Construction and Weighting

The main panel harmonizes all election outcomes to the 2021 postal precinct geography using population-weighted spatial crosswalks. Because historical precinct boundaries do not always align with the 2021 delineations, some 2021 postal precincts × election observations are constructed from multiple source precincts. This raises the concern that treatment effects could be sensitive to the degree of spatial fragmentation induced by the crosswalk.

To quantify this, I compute election- and margin-specific fragmentation measures based on the population-weighted crosswalk flows. For each election that requires a spatial overlay (2016, 2017, 2019, 2024, and 2025) and separately for in-person and postal allocations, I recover destination shares from crosswalk flows and summarize concentration using (i) the Herfindahl–Hirschman Index (HHI) and (ii) $k90$, the minimum number of source precincts required to cover 90% of the destination mass. High HHI and low $k90$ indicate a clean mapping (including pure merges), whereas low HHI and high $k90$ indicate "messy" mixing across many source precincts. Elections already observed on the 2021 precinct frame (2021 and 2023) have fragmentation measures set to their clean benchmark.

HHI-weighted specification. Column (2) of Table A6 re-estimates the baseline event-study specification weighting each observation by its margin-specific HHI (postal outcomes weighted by $\text{HHI}_{\text{postal}}$, in-person outcomes by $\text{HHI}_{\text{in-person}}$, and total turnout conservatively by the minimum of the two). This smoothly downweights precinct-election observations with fragmented mappings without changing the sample. The estimates are essentially unchanged relative to the baseline (Column 1): the immediate post-breakdown decline in total turnout remains around 2.5 log points, driven by persistent reductions in in-person turnout with partial substitution into postal voting.

$k90$-balanced sample restriction. Column (3) implements a stricter, merge-friendly restriction that removes only highly fragmented mappings. I first select a balanced set of 2021 postal precincts that satisfy $k90 \leq 2$ in *all* crosswalked elections for both voting modes, and then keep these units in all election periods of the panel. This yields a balanced sample of 916 2021 postal precincts per election (7,328 observations), compared to 1,350 units per election in the baseline. The treated share remains very similar (21.3% in the restricted sample vs. 21.8% in the full sample), and pre-treatment covariates are closely aligned (e.g., in EU19: mean eligible voters 1,673 vs. 1,695; migration background share 13.6 vs. 13.8). Estimates in the restricted sample remain qualitatively consistent with the baseline: the post-breakdown decline in in-person turnout persists, postal turnout increases in later elections, and total turnout remains lower in treated precincts, with some attenuation in magnitudes as expected under the more selective mapping requirement.

Table A6: Spatial Sample Construction and Weighting

Panel A: In-person Turnout	(1)	(2)	(3)
Treatment (t-3)	-0.0066	-0.0064	-0.0009
	(0.0050)	(0.0053)	(0.0061)
Treatment (t-2)	-0.0073	-0.0065	-0.0034
	(0.0074)	(0.0079)	(0.0091)
Treatment (t+0/Federal)	-0.0244***	-0.0220**	-0.0207*
	(0.0091)	(0.0094)	(0.0111)
Treatment (t+0/State)	-0.0225**	-0.0201**	-0.0191*
	(0.0091)	(0.0094)	(0.0111)
Treatment (t+1)	-0.0281***	-0.0257***	-0.0179**
	(0.0074)	(0.0080)	(0.0091)
Treatment (t+2)	-0.0623***	-0.0605***	-0.0585***
	(0.0064)	(0.0073)	(0.0079)
Treatment (t+3)	-0.0441***	-0.0394***	-0.0354***
	(0.0099)	(0.0103)	(0.0119)
R^2	0.88387	0.88053	0.87811

Panel B: Postal Turnout	(1)	(2)	(3)
Treatment (t-3)	-0.0068	-0.0060	-0.0069
	(0.0081)	(0.0085)	(0.0095)
Treatment (t-2)	-0.0040	-0.0053	-0.0067
	(0.0067)	(0.0071)	(0.0078)
Treatment (t+0/Federal)	-0.0215*	-0.0232*	-0.0169
	(0.0129)	(0.0130)	(0.0141)
Treatment (t+0/State)	-0.0238*	-0.0255*	-0.0198
	(0.0130)	(0.0130)	(0.0142)
Treatment (t+1)	0.0060	0.0043	0.0099
	(0.0132)	(0.0135)	(0.0157)
Treatment (t+2)	0.0414***	0.0412***	0.0515***
	(0.0112)	(0.0119)	(0.0133)
Treatment (t+3)	0.0327**	0.0328**	0.0346**
	(0.0130)	(0.0137)	(0.0155)
R^2	0.92498	0.92565	0.92613

Panel C: Total Turnout	(1)	(2)	(3)
Treatment (t-3)	-0.0074	-0.0069	-0.0034
	(0.0049)	(0.0050)	(0.0060)
Treatment (t-2)	-0.0073	-0.0079	-0.0059
	(0.0060)	(0.0063)	(0.0072)
Treatment (t+0/Federal)	-0.0252***	-0.0259***	-0.0231***
	(0.0063)	(0.0064)	(0.0071)
Treatment (t+0/State)	-0.0254***	-0.0261***	-0.0237***
	(0.0064)	(0.0065)	(0.0074)
Treatment (t+1)	-0.0159***	-0.0167***	-0.0094
	(0.0058)	(0.0061)	(0.0066)
Treatment (t+2)	-0.0189***	-0.0192***	-0.0132**
	(0.0052)	(0.0059)	(0.0063)
Treatment (t+3)	-0.0145*	-0.0144*	-0.0092
	(0.0076)	(0.0078)	(0.0091)
R^2	0.91729	0.91531	0.91682
Observations	10,800	10,800	7,328
Specification	Baseline	HHI-weighted	k90-balanced(≤ 2)
Precinct FE	✓	✓	✓
Election-District FE	✓	✓	✓

Notes: The table presents event study results based on Equation 1 for the natural log of in-person turnout using two alternative sample constructions. Column (1) shows the baseline. Column (2) re-estimates the baseline specification weighting each precinct-election observation by the population share of the dominant source precinct in the crosswalk to the 2021 postal precinct geography, so that more spatially concentrated mappings receive greater weight. Column (3) restricts the panel to a balanced set of 2021 postal precincts with low crosswalk fragmentation in all crosswalked elections ($k90 \leq 2$ for both the in-person and postal mappings), and retains these units in every election period. The reference election ($t-1$) is the 2019 European Election. Standard errors are clustered at the precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C 2024 Federal Partial Rerun.

In February 2024, a court-ordered partial rerun of the 2021 *Bundestag* election was conducted in the 431 precincts identified by the Federal Constitutional Court (Bundesverfassungsgericht, 2023). This rerun is not included as an outcome period in the panel event-study analysis because voting took place only in affected precincts, implying that no within-election control group exists for that election wave. Consequently, causal inference for the rerun election itself is not possible within the difference-in-differences framework used in the paper.

The partial rerun is also unlikely to explain the main results for subsequent regular elections. First, the turnout gap between affected and unaffected precincts is already visible prior to the partial rerun, most notably in the universal rerun of the state election (which was open to all precincts), where treated precincts exhibit lower in-person and lower total turnout (see Table 1). Second, the partial federal rerun is an institutional response triggered by the 2021 administrative failure and therefore a potential downstream channel through which the initial failure may affect later participation, rather than a separate confounding shock. For these reasons, the long-run turnout effects observed in subsequent regular elections are interpreted as persistent effects of the 2021 disruption.

D Outcome scale: logs vs. levels and implied percentage-point effects

This appendix documents that the substantive conclusions are robust to expressing turnout in levels rather than logs and provides a transparent accounting of in-person declines and postal substitution in percentage-point units.

I re-estimate Equation 1 using turnout measured in levels (percentage points) for in-person, postal, and total turnout. Figure A2 and Table A8 report the corresponding event-study coefficients. The post-treatment pattern mirrors the log specification: treated precincts experience a large and persistent decline in in-person turnout, accompanied by a partial increase in postal turnout and an overall decline in total turnout.

To interpret the main log coefficients in percentage-point units, I compute implied effects using Equation 2, multiplying the proportional effect $(\exp(\hat{\beta}_t) - 1)$ by the control-group mean turnout for election t. Table A7 reports implied effects for in-person, postal, and total turnout by election, alongside an "offset rate" defined as

$$\text{Offset Rate}_t = \Delta\text{Postal}_t^{pp}/-\Delta\text{InPerson}_t^{pp},$$

which captures the share of the in-person decline offset by increased postal voting.[9] Consistent with the main results, the offset rate indicates substantial but incomplete substitution: increases

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[9] The offset rate is primarily meaningful for post-treatment elections. In pre-treatment elections, both in-person and postal turnout may move in the same direction, in which case the ratio does not admit a substitution interpretation.

Figure A2: Levels Specification
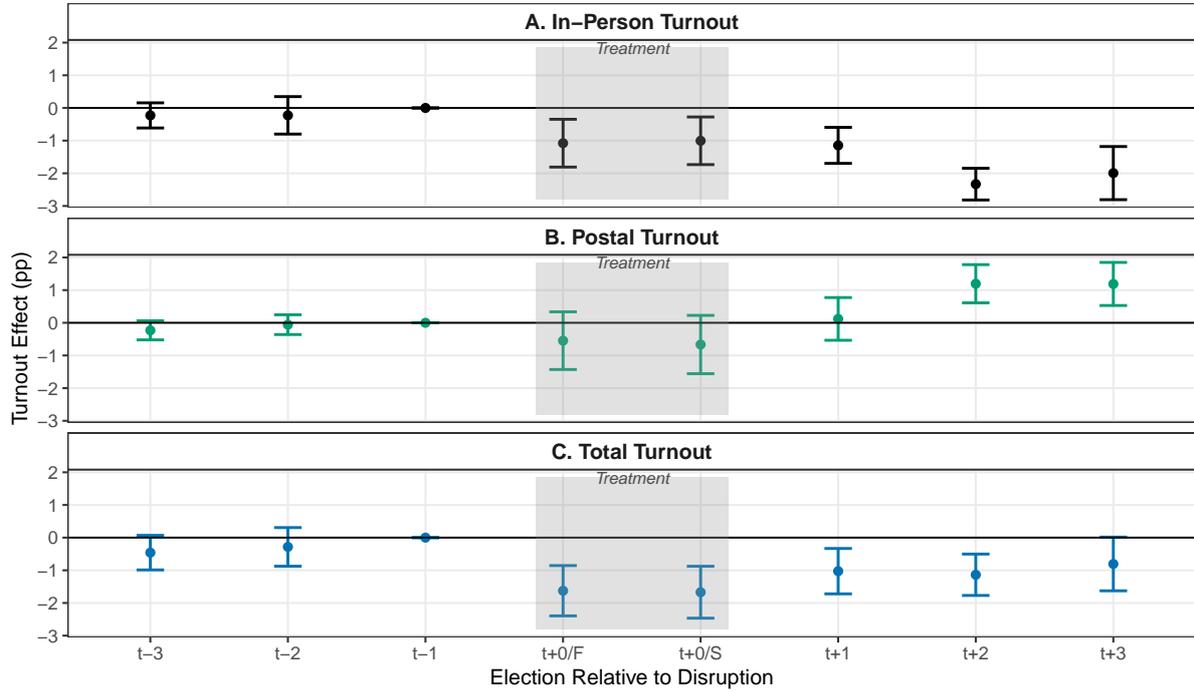
Notes: The figure presents event study results based on Equation 1 for in-person, postal, and total turnout. The reference election $(t-1)$ is the 2019 European election. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the postal precinct level. The point estimates and standard errors underlying the results appear in Column (2) of Table A8.

in postal voting recover part of the lost in-person participation, yet total turnout remains lower in treated precincts.

Table A7: Results Log to Levels

Panel A: In-person Turnout

	Implied pp effect	95% CI	Offset rate
Treatment (t-3)	-0.311 [0.1903]	[-0.772, 0.155]	-0.444
Treatment (t-2)	-0.367 [0.3193]	[-1.081, 0.357]	-0.285
Treatment (t+0/Federal)	-0.957 [0.0075]	[-1.644, -0.258]	-0.814
Treatment (t+0/State)	-0.891 [0.0131]	[-1.580, -0.190]	-0.967
Treatment (t+1)	-0.975 [0.0002]	[-1.469, -0.474]	0.176
Treatment (t+2)	-2.165 [0.0000]	[-2.581, -1.743]	0.526
Treatment (t+3)	-2.101 [0.0000]	[-2.997, -1.187]	0.504

Panel B: Postal Turnout

	Implied pp effect	95% CI	Offset rate
Treatment (t-3)	-0.138 [0.3968]	[-0.454, 0.183]	-0.444
Treatment (t-2)	-0.105 [0.5470]	[-0.442, 0.237]	-0.285
Treatment (t+0/Federal)	-0.779 [0.0970]	[-1.678, 0.142]	-0.814
Treatment (t+0/State)	-0.861 [0.0670]	[-1.759, 0.060]	-0.967
Treatment (t+1)	0.172 [0.6491]	[-0.560, 0.923]	0.176
Treatment (t+2)	1.138 [0.0002]	[0.527, 1.763]	0.526
Treatment (t+3)	1.059 [0.0117]	[0.234, 1.905]	0.504

Panel C: Total Turnout

	Implied pp effect	95% CI	Offset rate
Treatment (t-3)	-0.504 [0.1296]	[-1.149, 0.148]	-0.444
Treatment (t-2)	-0.550 [0.2263]	[-1.432, 0.343]	-0.285
Treatment (t+0/Federal)	-1.899 [0.0001]	[-2.809, -0.978]	-0.814
Treatment (t+0/State)	-1.918 [0.0001]	[-2.849, -0.975]	-0.967
Treatment (t+1)	-1.006 [0.0059]	[-1.711, -0.293]	0.176
Treatment (t+2)	-1.174 [0.0003]	[-1.796, -0.546]	0.526
Treatment (t+3)	-1.158 [0.0561]	[-2.327, 0.029]	0.504

Notes: Implied percentage-point effects translate log coefficients as $(\exp(\hat{\beta}) - 1) \times \bar{y}_t^{ctrl}$.

E   Downstream Electoral Composition

This appendix examines whether Berlin's 2021 election-day administrative breakdown affected the partisan composition of the electorate in addition to reducing participation. Turnout shocks can change representation mechanically if non-participation is uneven across partisan constituencies, even when preferences are unchanged.

E.1   Empirical approach

I estimate treatment effects on party vote shares using the same stacked event-study specification as in the main turnout analysis (see Equation 1), replacing the turnout outcome with party p's vote share in postal precinct i and election t. All specifications include postal precinct fixed effects and election-district-by-election fixed effects, and standard errors are clustered at the postal precinct level. The reference election $(t-1)$ is the 2019 European election.

E.2   Results

Figure A3 summarizes dynamic treatment effects on log party vote shares among votes cast for parties represented in the federal parliament; corresponding point estimates and standard errors are reported in Table A14. Pre-treatment coefficients are generally small, but several parties exhibit statistically significant pre-period estimates, so the vote-share results should be interpreted as suggestive evidence on downstream compositional changes. Post-2021 estimates indicate modest movements relative to the participation effects documented in section 3. The most consistent post-treatment increases appear for LINKE and, in the immediate post-disruption elections, for the Greens and the FDP. By contrast, effects for the SPD and AfD are small and not systematic across post-treatment elections, and the CDU does not display a stable post-treatment pattern in this specification. Overall, any shifts in the partisan distribution of votes cast appear secondary in magnitude to the turnout effects in the main analysis.

E.3   Survey Data: GLES Rolling Cross Sections

To complement the administrative data, I draw on the GLES Rolling Cross-Section (RCS) surveys from 2017, 2021, and 2025. These surveys include repeated nationally representative samples of German citizens during each federal election campaign. I use only respondents aged 18 or older with valid information on residence (`State`), trust in election administration, and nonvoting timing.

The relevant trust item asks whether respondents agree with the statement: *"Die Bundestagswahl wurde von den zuständigen Behörden korrekt und fair durchgeführt"*. I construct three variables: (i) a binary indicator for those who "fully agree", (ii) a relaxed version including "somewhat agree", and (iii) an ordinal scale from -2 ("strongly disagree") to $+2$ ("strongly agree")).

For nonvoters, the timing of abstention is measured via post-election questions. I define two binary indicators: one for individuals who abstained late (on or shortly before election day),

Figure A3: Election Results (Vote Share)
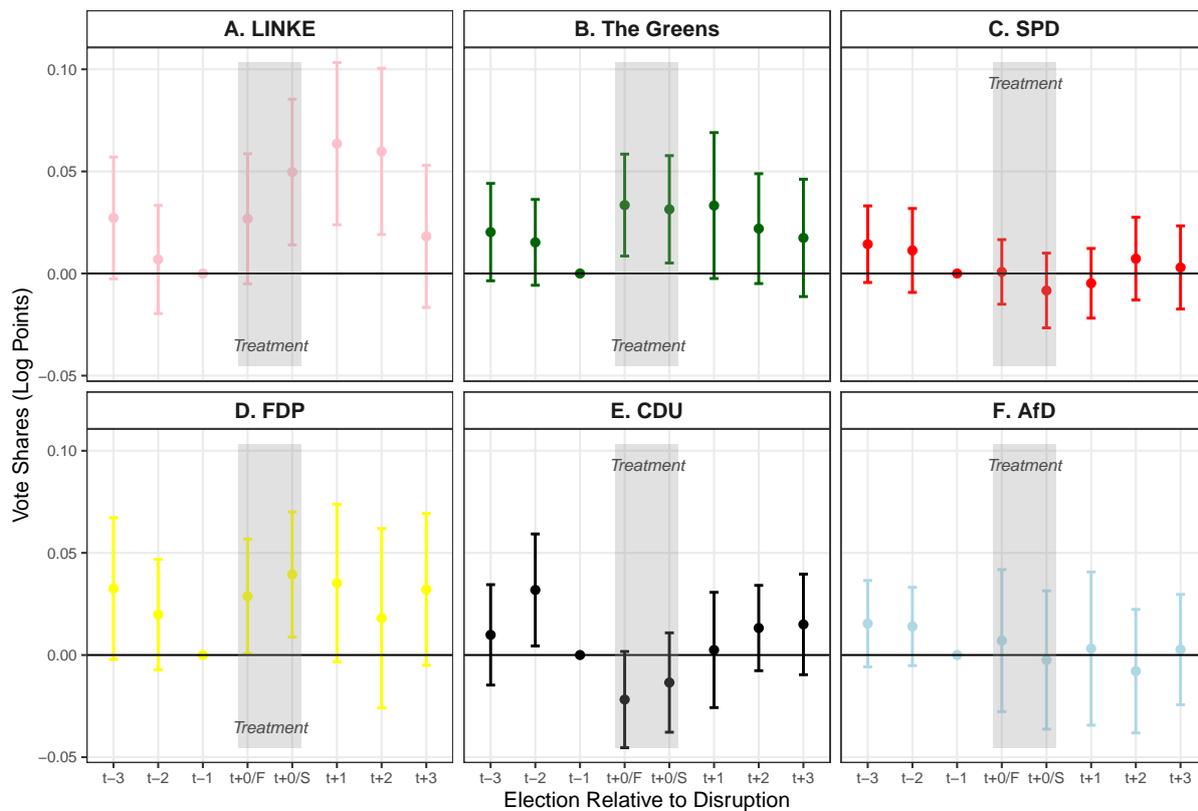
Notes: The figure presents event-study results based on Equation 1 for the vote share of parties represented in the federal parliament: *LINKE* (Left), *The Greens*, *SPD* (Social Democrats), *FDP* (Liberals), *CDU* (Christian Conservatives), and *AfD* (right-wing populist). The reference election $(t-1)$ is the 2019 European election. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the postal precinct level. The underlying estimates appear in Table A14.

and one for those who cited procedural reasons for not voting. These outcomes allow testing for latent cost shocks, as outlined in the behavioral model.

F   Supplementary Figures

Figure A4: Heterogeneity by Socio-Demographics
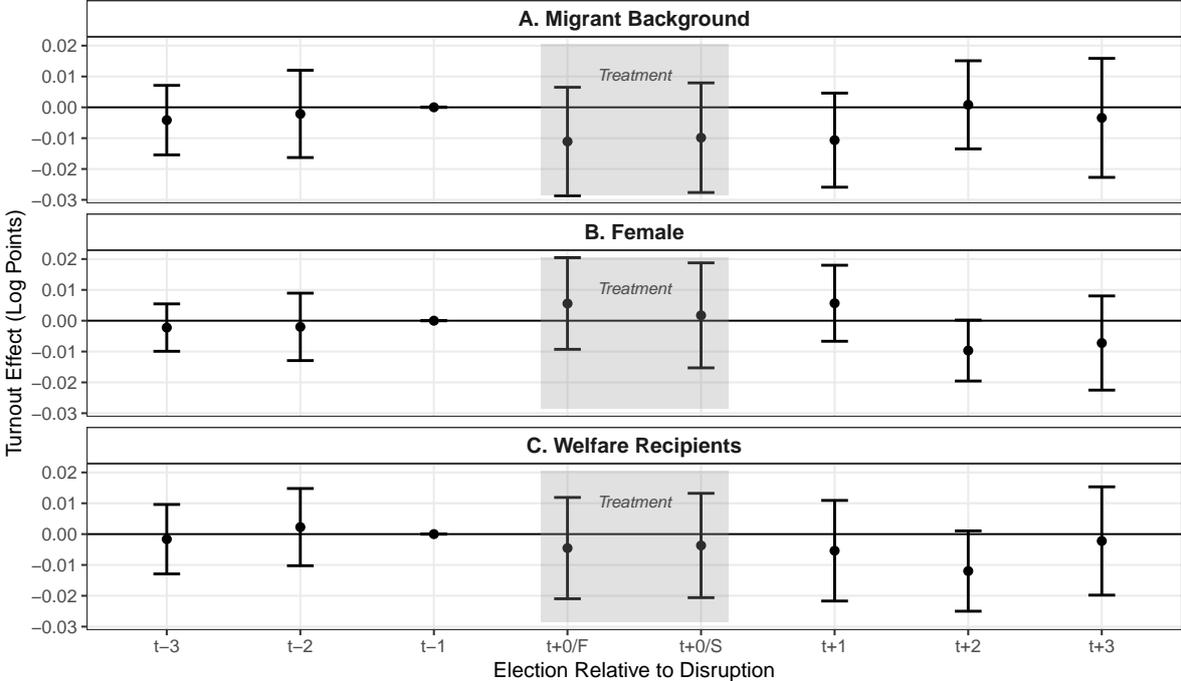
Notes: The figure presents triple-interaction estimates based on Equation 3 for the natural log of in-person turnout. The reference election $(t-1)$ is the 2019 European Election. Confidence intervals are drawn at the 95 percent level using standard errors clustered at the postal precinct level. The point estimates and standard errors underlying the results appear in Appendix Table A11. All first- and second-order interaction terms required for the triple-difference estimator are included or absorbed by the fixed effects.

G   Supplementary Tables

Table A8: Levels Specification

Panel A: In-person Turnout	(1)	(2)
Treatment (t-3)	-0.0066	-0.2283
	(0.0050)	(0.1960)
Treatment (t-2)	-0.0073	-0.2261
	(0.0074)	(0.2922)
Treatment (t+0/Federal)	-0.0244***	-1.078***
	(0.0091)	(0.3729)
Treatment (t+0/State)	-0.0225**	-1.005***
	(0.0091)	(0.3716)
Treatment (t+1)	-0.0281***	-1.145***
	(0.0074)	(0.2809)
Treatment (t+2)	-0.0623***	-2.332***
	(0.0064)	(0.2475)
Treatment (t+3)	-0.0441***	-1.994***
	(0.0099)	(0.4146)
R^2	0.88387	0.89614
Observations	10,800	10,800
Panel B: Postal Turnout	(1)	(2)
Treatment (t-3)	-0.0068	-0.2309
	(0.0081)	(0.1489)
Treatment (t-2)	-0.0040	-0.0576
	(0.0067)	(0.1542)
Treatment (t+0/Federal)	-0.0215*	-0.5472
	(0.0129)	(0.4500)
Treatment (t+0/State)	-0.0238*	-0.6656
	(0.0130)	(0.4547)
Treatment (t+1)	0.0060	0.1184
	(0.0132)	(0.3332)
Treatment (t+2)	0.0414***	1.195***
	(0.0112)	(0.2981)
Treatment (t+3)	0.0327**	1.188***
	(0.0130)	(0.3360)
R^2	0.92498	0.89565
Panel C: Total Turnout	(1)	(2)
Treatment (t-3)	-0.0074	-0.4593*
	(0.0049)	(0.2697)
Treatment (t-2)	-0.0073	-0.2837
	(0.0060)	(0.3014)
Treatment (t+0/Federal)	-0.0252***	-1.625***
	(0.0063)	(0.3926)
Treatment (t+0/State)	-0.0254***	-1.671***
	(0.0064)	(0.4051)
Treatment (t+1)	-0.0159***	-1.027***
	(0.0058)	(0.3541)
Treatment (t+2)	-0.0189***	-1.137***
	(0.0052)	(0.3229)
Treatment (t+3)	-0.0145*	-0.8056*
	(0.0076)	(0.4190)
R^2	0.91729	0.92525
Observations	10,800	10,800
Specification	Log	Levels
Precinct FE	✓	✓
Election-District FE	✓	✓

Notes: The table presents event study results based on Equation 1 for baseline specification (Column 1) and levels in in-person turnout, postal turnout, and total turnout (Column 2). The reference election $(t-1)$ is the 2019 European Election. Standard errors are clustered at the postal precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: Pre-breakdown characteristics of treated and control precincts by election family

Panel A. Federal election baseline (2017)	Treated mean	Control mean	Diff (T - C)	SE	p-value
Turnout 2017	78.55	76.05	2.50	0.50	0.000
Share age 18–25	7.81	7.83	-0.02	0.16	0.877
Share welfare recipients (SGB II)	11.41	13.72	-2.31	0.61	0.000
Share migration background	13.62	13.90	-0.28	0.53	0.601
Panel B. State election baseline (2016)					
Turnout 2016	69.69	67.72	1.96	0.55	0.000
Share age 18–25	7.81	7.83	-0.02	0.16	0.877
Share welfare recipients (SGB II)	11.41	13.72	-2.31	0.61	0.000
Share migration background	13.62	13.90	-0.28	0.53	0.601
Panel C. European election baseline (2019)					
Turnout 2019	65.39	61.17	4.21	0.66	0.000
Share age 18–25	7.81	7.83	-0.02	0.16	0.877
Share welfare recipients (SGB II)	11.41	13.72	-2.31	0.61	0.000
Share migration background	13.62	13.90	-0.28	0.53	0.601

Notes: Each panel compares precincts later annulled by the Federal Constitutional Court ("treated") to other Berlin precincts ("control") in the last pre-breakdown election of the same type. Reported are mean turnout in that baseline election, and demographic composition: *share age 18–25*, *share of SGB II recipients* (welfare), and *share of residents with migration backgrounds.* "Diff (T - C)" is the treated-minus-control difference from an OLS regression of each row variable on the treatment indicator. Standard errors are in parentheses. Lower p-values indicate stronger statistical evidence of a difference. Turnout is in percent of eligible voters.

Table A10: Waiting Time

Panel A: In-person Turnout	(1)	(2)
Treatment (t-3)	-0.0066	-0.0020
	(0.0050)	(0.0053)
Treatment (t-2)	-0.0073	-0.0124
	(0.0074)	(0.0077)
Treatment (t+0/Federal)	-0.0244***	-0.0410***
	(0.0091)	(0.0095)
Treatment (t+0/State)	-0.0225**	-0.0400***
	(0.0091)	(0.0092)
Treatment (t+1)	-0.0281***	-0.0320***
	(0.0074)	(0.0091)
Treatment (t+2)	-0.0623***	-0.0467***
	(0.0064)	(0.0077)
Treatment (t+3)	-0.0441***	-0.0422***
	(0.0099)	(0.0106)
R^2	0.88387	0.88190
Panel B: Postal Turnout	(1)	(2)
Treatment (t-3)	-0.0068	-0.0083
	(0.0081)	(0.0083)
Treatment (t-2)	-0.0040	-0.0046
	(0.0067)	(0.0054)
Treatment (t+0/Federal)	-0.0215*	-0.0143
	(0.0129)	(0.0118)
Treatment (t+0/State)	-0.0238*	-0.0156
	(0.0130)	(0.0118)
Treatment (t+1)	0.0060	0.0099
	(0.0132)	(0.0144)
Treatment (t+2)	0.0414***	0.0131
	(0.0112)	(0.0124)
Treatment (t+3)	0.0327**	-0.0080
	(0.0130)	(0.0130)
R^2	0.92498	0.92438
Panel C: Total Turnout	(1)	(2)
Treatment (t-3)	-0.0074	-0.0044
	(0.0049)	(0.0052)
Treatment (t-2)	-0.0073	-0.0097
	(0.0060)	(0.0059)
Treatment (t+0/Federal)	-0.0252***	-0.0251***
	(0.0063)	(0.0057)
Treatment (t+0/State)	-0.0254***	-0.0256***
	(0.0064)	(0.0058)
Treatment (t+1)	-0.0159***	-0.0100
	(0.0058)	(0.0065)
Treatment (t+2)	-0.0189***	-0.0167***
	(0.0052)	(0.0064)
Treatment (t+3)	-0.0145*	-0.0257***
	(0.0076)	(0.0077)
R^2	0.91729	0.91687
Observations	10,800	9,648
Specification	Baseline	Waiting Time in h
Precinct FE	✓	✓
Election-District FE	✓	✓

Notes: The table presents event study results based on Equation 1 for the natural log of in-person turnout, postal turnout, and total turnout. The reference election $(t-1)$ is the 2019 European Election. Treatment is defined as the maximum waiting time within a precinct as reported by the ruling of the *State Constitutional Court*. Standard errors are clustered at the postal precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A11: Heterogeneity by Socio-demographics

	In-person Turnout		
	(1)	(2)	(3)
Treatment (t-3)	-0.0061	-0.0063	-0.0055
	(0.0050)	(0.0049)	(0.0046)
Treatment (t-2)	-0.0088	-0.0071	-0.0049
	(0.0068)	(0.0074)	(0.0054)
Treatment (t+0/Federal)	-0.0245***	-0.0241***	-0.0211***
	(0.0084)	(0.0092)	(0.0068)
Treatment (t+0/State)	-0.0229***	-0.0223**	-0.0193***
	(0.0082)	(0.0091)	(0.0068)
Treatment (t+1)	-0.0256***	-0.0278***	-0.0277***
	(0.0076)	(0.0074)	(0.0074)
Treatment (t+2)	-0.0624***	-0.0625***	-0.0616***
	(0.0063)	(0.0063)	(0.0064)
Treatment (t+3)	-0.0461***	-0.0439***	-0.0408***
	(0.0092)	(0.0099)	(0.0075)
Treatment (t-3) $\times Z_i$	-0.0042	-0.0022	-0.0017
	(0.0057)	(0.0039)	(0.0057)
Treatment (t-2) $\times Z_i$	-0.0021	-0.0020	0.0023
	(0.0072)	(0.0056)	(0.0064)
Treatment (t+0/Federal) $\times Z_i$	-0.0111	0.0056	-0.0045
	(0.0090)	(0.0076)	(0.0084)
Treatment (t+0/State) $\times Z_i$	-0.0099	0.0017	-0.0037
	(0.0091)	(0.0087)	(0.0086)
Treatment (t+1) $\times Z_i$	-0.0106	0.0057	-0.0054
	(0.0078)	(0.0063)	(0.0083)
Treatment (t+2) $\times Z_i$	0.0008	-0.0097*	-0.0120*
	(0.0073)	(0.0050)	(0.0066)
Treatment (t+3) $\times Z_i$	-0.0034	-0.0072	-0.0022
	(0.0098)	(0.0078)	(0.0090)
R^2	0.90046	0.88515	0.91564
Observations	10,800	10,800	10,800
Covariate Z_i	Migrant Backgrounds	Females	Welfare Recipients
Precinct FE	✓	✓	✓
Election-District FE	✓	✓	✓

Notes: The table presents event study results based on Equation 3 for the natural log of in-person turnout. The reference election $(t-1)$ is the 2019 European Election. The share of citizens with migrant backgrounds is held constant in 2019 and is scaled with a mean of 0 and a standard deviation of 1. Standard errors are clustered at the postal precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A12: Heterogeneity by Age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				In-person Turnout				
Treatment (t-3)	-0.0077	-0.0079	-0.0059	-0.0051	-0.0012	-0.0050	-0.0039	-0.0012
	(0.0050)	(0.0051)	(0.0051)	(0.0051)	(0.0047)	(0.0049)	(0.0049)	(0.0049)
Treatment (t-2)	-0.0091	-0.0086	-0.0079	-0.0062	-0.0016	-0.0029	-0.0041	-0.0022
	(0.0073)	(0.0076)	(0.0072)	(0.0076)	(0.0072)	(0.0068)	(0.0075)	(0.0076)
Treatment (t+0/Federal)	-0.0276***	-0.0268***	-0.0251***	-0.0226**	-0.0178**	-0.0200**	-0.0201**	-0.0196**
	(0.0090)	(0.0091)	(0.0085)	(0.0093)	(0.0090)	(0.0087)	(0.0092)	(0.0094)
Treatment (t+0/State)	-0.0256***	-0.0249***	-0.0233***	-0.0209**	-0.0163*	-0.0183**	-0.0183**	-0.0184**
	(0.0090)	(0.0090)	(0.0084)	(0.0093)	(0.0089)	(0.0087)	(0.0091)	(0.0094)
Treatment (t+1)	-0.0293***	-0.0307***	-0.0263***	-0.0257***	-0.0242***	-0.0294***	-0.0252***	-0.0243***
	(0.0074)	(0.0073)	(0.0074)	(0.0074)	(0.0074)	(0.0074)	(0.0072)	(0.0076)
Treatment (t+2)	-0.0630***	-0.0649***	-0.0614***	-0.0627***	-0.0607***	-0.0625***	-0.0616***	-0.0615***
	(0.0064)	(0.0063)	(0.0063)	(0.0062)	(0.0063)	(0.0064)	(0.0062)	(0.0063)
Treatment (t+3)	-0.0468***	-0.0469***	-0.0451***	-0.0428***	-0.0359***	-0.0388***	-0.0397***	-0.0375***
	(0.0099)	(0.0101)	(0.0095)	(0.0101)	(0.0095)	(0.0093)	(0.0097)	(0.0101)
Treatment (t-3) $\times Z_i$	-0.0107**	-0.0123**	-0.0073	-0.0043	-0.0071*	-0.0124***	0.0127***	0.0077**
	(0.0044)	(0.0049)	(0.0046)	(0.0044)	(0.0038)	(0.0044)	(0.0042)	(0.0039)
Treatment (t-2) $\times Z_i$	-0.0124*	-0.0111	-0.0066	-0.0104	-0.0135***	-0.0138**	0.0196***	0.0146**
	(0.0070)	(0.0077)	(0.0057)	(0.0063)	(0.0052)	(0.0054)	(0.0059)	(0.0057)
Treatment (t+0/Federal) $\times Z_i$	-0.0260**	-0.0214**	-0.0147*	-0.0189**	-0.0215***	-0.0250***	0.0286***	0.0289***
	(0.0101)	(0.0102)	(0.0081)	(0.0078)	(0.0070)	(0.0075)	(0.0080)	(0.0080)
Treatment (t+0/State) $\times Z_i$	-0.0247**	-0.0209**	-0.0139*	-0.0178**	-0.0206***	-0.0217***	0.0280***	0.0263***
	(0.0100)	(0.0100)	(0.0084)	(0.0076)	(0.0069)	(0.0076)	(0.0079)	(0.0080)
Treatment (t+1) $\times Z_i$	-0.0129*	-0.0231***	-0.0136*	-0.0088	-0.0087	-0.0167**	0.0114*	0.0152**
	(0.0071)	(0.0072)	(0.0073)	(0.0066)	(0.0068)	(0.0067)	(0.0064)	(0.0073)
Treatment (t+2) $\times Z_i$	-0.0061	-0.0238***	-0.0076	0.0077	0.0042	-0.0075	-0.0025	-0.0021
	(0.0060)	(0.0055)	(0.0068)	(0.0061)	(0.0058)	(0.0058)	(0.0059)	(0.0060)
Treatment (t+3) $\times Z_i$	-0.0206**	-0.0252**	-0.0100	-0.0092	-0.0143**	-0.0220***	0.0242***	0.0162*
	(0.0102)	(0.0109)	(0.0089)	(0.0089)	(0.0072)	(0.0077)	(0.0084)	(0.0086)
R^2	0.88718	0.88610	0.89261	0.88684	0.88819	0.89148	0.88608	0.88609
Observations	10,800	10,800	10,800	10,800	10,800	10,800	10,800	10,800
Covariate Z_i	Ages 0-6	Ages 6-18	Ages 18-25	Ages 25-35	Ages 35-45	Ages 45-60	Ages 60-70	Ages 70+
Precinct FE	✓	✓	✓	✓	✓	✓	✓	✓
Election-District FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table presents event study results based on Equation 3 for the natural log of in-person turnout. The reference election $(t-1)$ is the 2019 European Election. The share of citizens within the corresponding age group is held constant in 2019 and is scaled with a mean of 0 and a standard deviation of 1. Standard errors are clustered at the postal precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A13: GLES Survey Results

A. "Election Process Correct and Fair"	(1)	(2)
Berlin 2017 (pre)	-0.0058	-0.0123***
	(0.0039)	(0.0032)
Berlin 2021 (pre)	-0.0930***	-0.1275***
	(0.0035)	(0.0040)
Berlin 2025 (pre)	-0.0287***	-0.0075**
	(0.0034)	(0.0026)
Berlin 2025	-0.0175***	-0.0122***
	(0.0050)	(0.0031)
Observations	20,213	31,484
R^2	0.01346	0.01271
Specification	Strongly (Dis)agree	(Strongly) (Dis)agree
B. Timing of Voting Decision: Non-Vote	(1)	(2)
Berlin 2021 (pre)	0.5281***	0.4950***
	(0.0360)	(0.0401)
Berlin 2025	-0.0077	0.1823**
	(0.0527)	(0.0621)
Observations	510	510
R^2	0.04503	0.03429
Specification	Prevented	Same Day | Prevented
Year FE	✓	✓
State FE	✓	✓

Notes: This table reports estimates from Equation 4 using pooled data from the GLES Rolling Cross-Section surveys conducted in 2017, 2021, and 2025. *Panel A* reports respondents' beliefs about electoral integrity, measured via agreement with the statement: *"Die Bundestagswahl wurde von den zuständigen Behörden korrekt und fair durchgeführt"*. The reference election $(2021(pre))$ is the *pre-election survey* of the 2021 Federal Election. Binary dependent variables equal 1 if respondents "fully agree" (Column 1), or "fully" or "somewhat agree" (Column 2); Column (3) codes responses from -2 ("strongly disagree") to $+2$ ("strongly agree"). *Panel B* examines the timing of abstention decisions among nonvoters. The reference election $(2021(pre))$ is the *pre-election survey* of the 2021 Federal Election. Column (1) codes as 1 those who decided not to vote in the final days before the election or reported being prevented from voting. Column (2) isolates respondents who cited inability to vote as the main reason. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A14: Election Results (Vote Share)

	log(LINKE) (1)	log(GREEN) (2)	log(SPD) (3)	log(FDP) (4)	log(CDU) (5)	log(AFD) (6)
Treatment (t-3)	0.0272*	0.0203*	0.0144	0.0325*	0.0099	0.0154
	(0.0152)	(0.0121)	(0.0096)	(0.0177)	(0.0125)	(0.0108)
Treatment (t-2)	0.0069	0.0153	0.0113	0.0198	0.0318**	0.0140
	(0.0135)	(0.0107)	(0.0105)	(0.0138)	(0.0140)	(0.0098)
Treatment (t+0/Federal)	0.0268*	0.0335***	0.0008	0.0288**	-0.0218*	0.0070
	(0.0162)	(0.0127)	(0.0081)	(0.0143)	(0.0120)	(0.0177)
Treatment (t+0/State)	0.0497***	0.0314**	-0.0083	0.0394**	-0.0135	-0.0024
	(0.0182)	(0.0134)	(0.0093)	(0.0156)	(0.0124)	(0.0173)
Treatment (t+1)	0.0636***	0.0333*	-0.0048	0.0352*	0.0025	0.0032
	(0.0202)	(0.0182)	(0.0087)	(0.0196)	(0.0144)	(0.0191)
Treatment (t+2)	0.0598***	0.0220	0.0073	0.0180	0.0132	-0.0079
	(0.0208)	(0.0137)	(0.0103)	(0.0224)	(0.0106)	(0.0154)
Treatment (t+3)	0.0182	0.0174	0.0030	0.0321*	0.0150	0.0027
	(0.0177)	(0.0146)	(0.0104)	(0.0190)	(0.0126)	(0.0138)
R^2	0.96463	0.96961	0.91963	0.93749	0.96707	0.96845
Observations	10,800	10,800	10,800	10,800	10,800	10,800
Precinct FE	✓	✓	✓	✓	✓	✓
Election-District FE	✓	✓	✓	✓	✓	✓

Notes: The table presents event study results based on Equation 1 for the vote share of the political parties represented in the federal parliament *LINKE* (Left), *The Greens*, the *SPD* (Social Democrats), the *FDP* (Liberals), the *CDU* (Christian Conservatives), and the *AfD* (right-wing populist). The reference election $(t-1)$ is the 2019 European Election. Standard errors are clustered at the postal precinct level and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A15: Summary Statistics

Panel A: Full Sample

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Total Turnout	1,350	76.618	9.302	48.053	70.233	77.755	83.153	125.795
In-person Turnout	1,350	39.742	3.604	26.594	37.419	39.783	42.055	56.059
Postal Turnout	1,350	36.876	8.701	13.857	30.586	37.005	42.422	86.934
Eligible Voters	1,350	1,659.061	617.445	258.995	1,160.463	1,430.296	2,146.394	3,848.810
Migrant Backgrounds Share	1,350	13.758	8.069	1.864	7.825	12.104	18.124	63.429
Female Share	1,350	51.879	2.754	40.625	50.138	51.899	53.594	65.376

Panel B: Treatment Group

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Total Turnout	294	77.367	7.929	48.053	73.304	78.883	82.978	100.986
In-person Turnout	294	39.868	3.184	31.101	37.839	39.769	41.933	48.584
Postal Turnout	294	37.499	7.433	14.359	33.195	38.568	42.257	62.366
Eligible Voters	294	1,919.967	720.474	690.971	1,256.041	1,887.964	2,584.046	3,815.900
Migrant Backgrounds Share	294	13.622	7.971	2.725	7.764	11.940	17.776	50.800
Female Share	294	51.821	2.788	40.625	49.966	51.881	53.572	61.141

Panel C: Control Group

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Total Turnout	1,056	76.410	9.643	51.107	69.533	77.041	83.309	125.795
In-person Turnout	1,056	39.707	3.713	26.594	37.363	39.803	42.177	56.059
Postal Turnout	1,056	36.702	9.018	13.857	30.074	36.504	42.458	86.934
Eligible Voters	1,056	1,586.423	564.859	258.995	1,146.354	1,369.789	2,005.608	3,848.810
Migrant Backgrounds Share	1,056	13.796	8.099	1.864	7.838	12.143	18.194	63.429
Female Share	1,056	51.894	2.745	41.689	50.221	51.904	53.597	65.376

Notes: The table presents summary statistics for the 2021 Federal election for the main sample, i.e., excluding those precincts only affected by administrative breakdown in the State election. It is split into a treatment and a control group for selected variables used in the analyses. Turnout exceeds 100% in 17 of 10,800 precincts × elections, one treated and 16 control precincts, because of allocation issues for the district *Tempelhof-Schöneberg* (16), and because precinct boundaries are harmonized using population-weighted overlays, and because external/postal ballots are not perfectly attributable to the eligible-voter denominator in the harmonized precinct frame.