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Research Article

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Posted Date: July 10th, 2026

DOI: <https://doi.org/10.21203/rs.3.rs-10183256/v1>

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Additional Declarations: No competing interests reported.

Natural Disasters and Electoral Consequences: Evidence on Voter Preferences and Participation from U.S. Presidential Elections*

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Abstract

Natural disasters can influence elections through changes in voter preferences or voter participation, yet the relative importance of these channels remains unclear. This study examines how natural disasters and federal disaster relief affect incumbent-party vote share and voter turnout in U.S. presidential elections from 1996 to 2020. Using county-level panel data and a two-way fixed effects framework, we exploit spatial and temporal variation in disaster exposure to identify their electoral consequences. We find that more severe disasters significantly reduce incumbent-party vote share, consistent with *retrospective voting theory*. Presidential Disaster Declarations (PDDs) are associated with a partial mitigation of these electoral losses, supporting the *attentive retrospection hypothesis*. In contrast, disasters exert only weak and inconsistent effects on voter turnout, indicating that their political consequences operate primarily through voter preference shifts rather than aggregate turnout changes. We further show that partisan alignment between the president and governor plays a more consistent role than disaster declarations alone in shaping electoral responses. A broad set of robustness checks confirms the stability of the results.

Keywords: Natural disasters; Presidential elections; Presidential disaster declaration (PDD); Voter preference; Voter participation; U.S. county.

JEL Classification Numbers: D72,N42,O18,P16,R10

*The authors are grateful to Saumitra Jha, Minhae Kim, Neil Malhotra, and David Quigley for helpful comments and discussions.

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1 Introduction

Disasters and the responses to them at all levels of government often carry significant electoral consequences, shaping electoral outcomes in complex and sometimes counterintuitive ways.¹ Despite a substantial and growing body of research, the debate over whether disasters politically help or harm incumbents remains elusive, reflecting both theoretical ambiguities and mixed empirical evidence.

On the theoretical front, the electoral consequences of natural disasters remain ambiguous, as they depend on both voters' assessments of incumbent performance and the particular electoral measure under consideration (see Table 1). A central framework in this literature is *retrospective voting*, which posits that voters evaluate incumbents based on their past performance, including crisis management and disaster response (Healy and Malhotra, 2009, 2013; Achen and Bartels, 2017). Although disasters are often seen as exogenous shocks for which incumbents cannot be directly blamed, voters may still judge them based on their preparedness for the disaster and their response in its aftermath. Consequently, severe disasters or poorly managed ones may erode the support of incumbents, while effective relief efforts may bolster it.

The empirical findings are similarly mixed (see Table 2). Under the *blind retrospection* hypothesis, voters punish incumbents for adverse events regardless of objective responsibility (Achen and Bartels, 2017; Heersink et al., 2017). According to the *attentive retrospection* perspective, however, voters take government relief performance into account, and perceived effective disaster responses can mitigate or even reverse any electoral losses associated with the disaster itself. For instance, Healy and Malhotra (2009) find that successful disaster relief improves incumbent support, and Healy and Malhotra (2010), Gasper and Reeves (2011), and Heersink et al. (2022) show that presidents who issue disaster declarations are rewarded at the polls. Nevertheless, the political effects of Presidential Disaster Declarations (PDDs) still remain underexplored.² This gap is notable given the discretionary nature and political salience of PDD-driven aid (Garrett and Sobel, 2003; Reeves, 2011; Sobel et al., 2007).

¹See, for example, Kaplan et al. (2025); Elliott et al. (2023); Boomhower (2024); Chen (2013); Cerqua et al. (2023).

²Following a major disaster, the governor in the affected state may seek a federal disaster declaration if state resources are insufficient to manage the event. After joint assessment by the Federal Emergency Management Agency (FEMA) and local officials, FEMA records approved declarations, including the declaration date and affected counties. For more information, see FEMA's disaster declaration process: <https://www.fema.gov/disaster-declaration-process>.

The purpose of this study is to bridge this gap by jointly examining how natural disasters affect incumbent vote share and voter turnout. Analyzing these two electoral outcomes simultaneously allows us to distinguish whether disasters primarily influence voter participation or preferences, or both. We further investigate how federal disaster assistance through PDDs and political alignment between the U.S. president and state governor are associated with these electoral channels. Using county-level data covering more than 3,000 U.S. counties across seven presidential elections (1996–2020), we provide a unified analysis of how natural disasters and federal relief efforts affect local support for the incumbent party.

In this regard, we make several key contributions to the literature. First, while most previous studies focus separately on either vote share or voter turnout, we examine both jointly, providing a more comprehensive understanding of how natural shocks shape electoral behavior. Natural disasters may influence elections through two distinct channels: (i) direct effects on incumbent vote share by altering voter preferences, and (ii) indirect effects through changes in voter turnout that modify the composition of the electorate. Analyzing both outcomes simultaneously is important because the competing theoretical hypotheses summarized in Table 1 imply different mechanisms through which disasters affect electoral outcomes.

Most prior studies emphasize vote share, and few systematically analyze turnout — and those that do often treat it as secondary. This narrow focus on vote share, however, risks obscuring important mechanisms such as voter suppression. Disasters can depress participation, and do so disproportionately among certain groups — either directly, by damaging election-facilitating infrastructure and hindering access to the polls, or indirectly, by reducing the motivation or capacity of specific populations to turn out. If disasters appear to increase incumbent vote share but simultaneously suppress turnout, for instance, the apparent gain may result from compositional changes — that is, reduced participation among opposition-leaning voters — rather than from persuading voters to switch their vote (Leeson and Sobel, 2011). Consequently, analyzing vote share in conjunction with turnout enables us to disentangle preference shifts (retrospective evaluations) from participation changes (mobilization or suppression).³

³Given the winner-take-all nature of U.S. elections, voter turnout and voter preference shifts have different electoral consequences. Mobilizing a supporter adds one vote, whereas persuading an opposing voter to switch generates a two-vote advantage. Distinguishing between turnout and vote choice is therefore crucial for understanding the political effects of natural disasters and government responses.

Second, we expand the empirical scope by examining all types of natural disasters across all U.S. counties, rather than focusing on isolated events like Hurricanes Katrina or Sandy. Although case studies provide valuable insights, their findings may lack external validity due to unique local contexts. By employing a nationwide, multi-decade panel encompassing diverse disaster types and election cycles, we capture systematic variation in political responses across regions, party alignments, and levels of disaster exposure.

This broader framework also reduces the risk of bias introduced when media coverage concentrates on highly visible, dramatically televised events. Studies that focus solely on these widely publicized disasters may overstate disasters’ typical electoral consequences⁴, whereas a more comprehensive approach incorporating both major and less-publicized disasters helps capture the full distribution of disaster impacts across contexts.

Third, we employ a two-way fixed effects (TWFE) panel framework that exploits both temporal and spatial variation in disaster exposure (Choi et al., 2025). This approach controls for unobserved time-invariant county characteristics, nationwide electoral shocks, and a rich set of socioeconomic covariates. Identification relies on within-county variation in disaster severity over time, under the assumption that, conditional on county fixed effects and state-by-year fixed effects, fluctuations in natural disaster intensity are plausibly exogenous and effectively random within counties across election cycles (Choi, 2023; Henkel et al., 2024). This framework enables us to isolate the effects of changes in local disaster exposure on voter preferences and electoral participation, net of persistent political, demographic, and economic differences across counties.

To further assess the validity of our empirical design, we conduct placebo randomization tests and event-study diagnostics. The placebo tests exploit the quasi-random timing and geographic distribution of disasters, while the event-study analysis examines whether future disaster exposure predicts current electoral outcomes. Both exercises provide little evidence of spurious correlations or pre-treatment dynamics, lending support to the interpretation that our results reflect a causal effect rather than pre-existing trends. Although recent studies show that TWFE estimators with continuous treatments may recover weighted averages of heterogeneous effects, our goal is to esti-

⁴Hurricanes Sandy (2012) and Michael (2018), for example, have been linked to turnout declines and shifts in incumbent support (Heersink et al., 2022; Lasala-Blanco et al., 2017; Hart, 2014; Velez and Martin, 2013; Zelin and Smith, 2023).

mate the average relationship between disaster severity and electoral outcomes. Because disaster exposure is recurrent, continuous, and lacks a clear treatment adoption date, staggered-adoption estimators are not directly applicable. Moreover, placebo tests, timing analyses, leave-one-election-out exercises, and event-study diagnostics consistently indicate that our results are unlikely to be driven by pre-existing trends.

Finally, we examine how federal disaster assistance through PDDs and partisan alignment between federal and state governments shape the electoral consequences of natural disasters. While earlier studies suggest that voters may reward incumbents for effective disaster management (Neugart and Rode, 2021), the evidence regarding the electoral effects of PDDs themselves remains mixed. Some studies find that federal disaster declarations increase support for incumbents (Healy and Malhotra, 2010; Gasper and Reeves, 2011), whereas others emphasize the conditional role of partisan alignment (Gasper, 2015; Reeves, 2011). In particular, these studies suggest that federal aid tends to benefit the incumbent party more strongly in co-partisan counties, while generating weaker, neutral, or even adverse effects in opposition areas. Consistent with this interpretation, Heersink et al. (2022) show that the increase in President Obama’s approval ratings following Hurricane Sandy was concentrated primarily in Democratic-leaning counties.

Our main findings can be summarized as follows. First, more severe natural disasters significantly reduce the vote share of the incumbent party, consistent with retrospective voting theory. Second, conditional on disaster severity, counties receiving PDDs exhibit smaller declines in incumbent-party support, in support of the attentive retrospection hypothesis. Third, natural disasters have only limited effects on voter turnout, and the role of PDDs is similarly weak. These results suggest that the electoral costs faced by incumbents arise primarily from changes in voter preferences toward the opposition rather than from reduced participation among incumbent supporters. Fourth, our categorical analysis indicates that partisan alignment, rather than the issuance of PDDs alone, plays the dominant role in shaping electoral responses. Alignment between the president and the governor of the disaster-affected state amplifies the political benefits associated with PDDs and related federal aid inflows, implying that intergovernmental political coordination is an important mechanism through which incumbents obtain electoral gains from disaster relief. Finally, our findings remain robust across a wide range of specifications and robustness checks, including alternative measures

of disaster intensity, extensive-margin analyses that incorporate zero-damage counties, controls for potential compositional changes driven by domestic migration flows, and alternative specifications based on the timing of disasters relative to presidential elections.

The remainder of the paper is organized as follows. Section 2 reviews the related literature on natural disasters and electoral outcomes. Section 3 describes the data sources and variable construction. Section 4 outlines the empirical methodology and presents the baseline regression results, including the categorical group analysis. Section 5 reports a range of robustness checks. Section 6 concludes and discusses the broader implications of the findings. Additional discussion of the identification strategy underlying the panel data analysis, along with supplementary data details, is provided in the Appendix.

2 Literature Review

Numerous studies have examined the relationship between natural disasters and electoral outcomes, yet their findings remain inconclusive (e.g., Heersink et al. (2022)). Theory offers competing predictions: disasters may erode incumbent support if voters perceive government failures in preparedness or response, but they may also enhance it when relief efforts are viewed as effective and timely.

A central framework guiding this literature is *retrospective voting*, which posits that voters assess incumbents based on past performance, including their crisis management (e.g., Healy and Malhotra (2009); Achen and Bartels (2017)). Within this framework, two contrasting behavioral patterns emerge. *Blind retrospection* occurs when voters indiscriminately punish incumbents for adverse outcomes, even if those were beyond their control, while *attentive retrospection* reflects a more discerning electorate that rewards effective disaster response. Empirical findings support both interpretations: while some studies document voter punishment following disasters (e.g., Achen and Bartels (2017)), others show that well-executed relief efforts can offset these losses or even reward incumbents (e.g., Healy and Malhotra (2009); Gasper and Reeves (2011)). Complementary micro-level evidence reinforces the latter view — federal disaster aid improves household finances and business continuity, providing a concrete mechanism through which relief efforts can bolster voter evaluations of the incumbent (Gallagher et al., 2023; Duchin and Hackney, 2021; Smiley et al.,

2025).

Beyond retrospective assessments, partisan alignment plays a crucial role in conditioning electoral responses. Voters often reward co-partisan incumbents and penalize those from opposing parties (e.g., Bartels (2002); Heersink et al. (2022)). Following Hurricane Sandy in 2012, for instance, Heersink et al. (2022) find that President Obama gained support in Democratic-leaning counties but lost ground in Republican ones, underscoring how preexisting political loyalties shape disaster-related performance evaluations of the incumbent.

In line with these behavioral observations, a growing body of evidence shows that disaster relief allocation is politically motivated (Deryugina, 2017; Schneider et al., 2022; Schneider and Kunze, 2025). Husted and Nickerson (2014) demonstrate that PDDs are more likely during election years, in swing states, and under co-partisan governors — patterns consistent with the strategic use of federal aid for electoral gain. More recently, Schneider and Kunze (2025) identify a nonlinear relationship between disaster severity and PDD issuance: co-partisan counties are more likely to receive declarations after medium-intensity hurricanes, but partisan bias disappears for weak or extreme events. Despite these insights, few studies directly examine whether politically motivated disaster aid translates into measurable electoral rewards (e.g., Schneider and Kunze (2025)). This study addresses these gaps by linking the literature on retrospective voting and distributive politics.

That being said, most existing studies focus primarily on vote share, potentially conflating two distinct mechanisms through which disasters may affect elections: electoral mobilization (voter turnout) and electoral persuasion (voter preference change). Because disasters can influence both whether citizens vote and whom they support, changes in vote share alone cannot distinguish whether electoral outcomes arise from shifts in political preferences or from participation effects associated with changes in the composition of the electorate. Examining voter turnout alongside vote share therefore provides a clearer framework for identifying the political channels through which natural disasters shape electoral outcomes. Despite its potential importance, however, the mobilization channel remains relatively understudied. As a notable exception, Chen (2013) finds that pre-election disaster aid in Florida increased turnout among Republicans (the incumbent party) while reducing participation among Democrats. This evidence suggests that disasters and relief efforts may influence elections not only through preference shifts but also by altering who

turns out to vote.⁵

Distinguishing between preference shifts and voter turnout effects is particularly important for understanding the roles of government aid and partisan alignment in shaping electoral responses to natural disasters. Theoretically, however, the turnout effects of disasters remain ambiguous. As McCartney (2021) argues, affected citizens may either become politically mobilized out of frustration and anger (the “angry voter hypothesis”) or disengage from politics because of stress, apathy, and the financial or emotional burdens imposed by the disaster. Under the blind retrospection hypothesis, disasters may reduce incumbent vote share even when turnout remains largely unchanged, implying that voters punish incumbents primarily through changes in political preferences rather than through demobilization. In contrast, the attentive retrospection hypothesis suggests that visible government assistance, such as PDDs, may not only increase electoral support for incumbents but also stimulate political participation by strengthening trust in government responsiveness, enhancing political engagement, or facilitating voter mobilization in affected communities. Similarly, partisan alignment between federal and state governments may shape both voters’ evaluations of disaster relief and the extent to which disasters generate political mobilization or demobilization. Jointly examining vote share and turnout therefore allows us to determine whether government aid and political alignment influence electoral outcomes primarily through preference shifts effects, voter turnout effects, or a combination of both.

3 Data and Descriptive Statistics

Our unit of analysis is the county-election year, spanning from the 1996 to the 2020 presidential elections, and covering 3,016 U.S. counties. The data are drawn from multiple sources. County-level presidential election results are obtained from the Congressional Quarterly (CQ) Press, encompassing seven election cycles. Because our focus is on the change in the incumbent party’s vote share relative to the previous election, we calculate the difference in the incumbent party’s county-level

⁵Chen’s evidence, however, is drawn from the specific context of Florida, where disasters are frequent, politically salient, and embedded within a highly competitive two-party environment. Florida’s recurrent hurricane exposure and consistently high levels of federal disaster assistance make it an informative but atypical setting, raising questions about external validity. Consequently, the turnout asymmetries documented in Chen (2013) may not generalize to regions with different disaster risks, demographic compositions, or political institutions.

vote share between consecutive elections (see Choi et al. (2025)).⁶ We focus on presidential elections because only the president has substantial control over disaster relief through FEMA and related agencies.

Data on disaster damage come from the Spatial Hazard Events and Losses Database for the United States (SHELDUS), maintained by Arizona State University and the Department of Homeland Security. The database provides monthly county-level estimates of property and crop losses, injuries, and fatalities for 18 types of natural hazards such as hurricanes, floods, wildfires, and tornadoes. For each county, total losses are aggregated over the incident period. Following the literature (Gasper and Reeves, 2011; Heersink et al., 2022; Choi and Kim, 2025), we measure disaster damage as the logarithm of total losses per 10,000 residents, expressed in billions of constant 2012 dollars.⁷ Although SHELDUS provides the most comprehensive coverage of disaster-related losses, it assigns damages from multi-county events equally across all affected counties because more precise allocation information is unavailable. This equal apportionment may introduce noise into our damage variable, but does not necessarily induce systematic bias. In addition, the timing of disaster episodes in SHELDUS occasionally differs from FEMA’s event classifications (Dlugosz et al., 2022).⁸ Figure 1 illustrates the spatial distribution of average per capita disaster damages (in 2012 dollars) across 3,016 counties from 1994 to 2020, revealing substantial regional variation. Damages are most concentrated in coastal regions, the Great Plains, and the southern United States. This spatial heterogeneity aligns with long-standing regional disparities in exposure and resilience identified by Boustan et al. (2020), who show that geography and historical patterns of development jointly shape the economic persistence of disaster impacts across U.S. counties.

Data on PDDs are drawn from FEMA’s official records for the period 1994–2020. These records

⁶The incumbent party is the party of the sitting President going into the election, Republican for 2004, 2008, 2020; Democrat for 1996, 2000, 2012, 2016. Therefore, the change in incumbent-party vote share is defined as the change in support for the party holding the presidency — Republicans in 2004, 2008, and 2020, and Democrats in 1996, 2000, 2012, and 2016 — allowing for a consistent measure of incumbent performance across administrations with alternating party control.

⁷Our baseline analysis focuses on the intensive margin by excluding observations with zero reported damage, which represent the absence of a disaster event. This restriction ensures that the estimates capture variation in disaster severity among affected counties rather than differences between disaster and non-disaster cases. As shown in Section 5.2, the results are robust to extensive-margin specifications that include zero-damage observations.

⁸In SHELDUS, small-scale events causing less than \$50,000 in property damage or no fatalities are excluded, which may understate cumulative losses from frequent, low-impact incidents (Gall et al., 2009; Roth Tran and Wilson, 2025). However, such minor events are unlikely to generate measurable aggregate electoral effects.

identify both the incident period and the affected counties for all federally declared disasters.⁹ Figure 2 maps the number of PDDs issued across U.S. counties, showing distinct geographic clustering along the Gulf Coast, in Florida, and throughout Tornado Alley (Texas, Oklahoma, Arkansas, and Missouri). Northeastern counties in states such as Maine, New Hampshire, and Vermont also display elevated declaration frequencies, largely due to winter storms and flooding. Notably, disaster declarations are highly localized to counties, rather than issued uniformly across entire states. For the empirical analysis, we measure federal disaster assistance using the number of PDDs issued in each county-year.

To provide a sense of the key variables, Figure 3 presents the median and interquartile ranges (IQR) of the main variables across election cycles. In four of the seven elections, both the median and IQR of the change in incumbent-party vote share lie below zero, indicating a general decline in support for the incumbent party. Although the median change in 2020 appears unrealistically positive at the county level, this result reflects unweighted averages and excluding 2020 does not alter our main conclusions. The top-right panel of Figure 3 shows voter turnout rates, measured as total votes divided by the county’s voting-age population (18+), following Markovich and White (2022).¹⁰ Voter turnout has risen steadily since 2008, accompanied by a narrowing IQR, suggesting increasingly more similar levels of participation nationwide. The bottom-left panel shows logged per capita disaster damages, which have declined on average but exhibit increasing dispersion across counties. The bottom-right panel shows that, on average, roughly half a PDD is issued per county, with the number of PDDs per county peaking in the 2008 election cycle. Excluding 2008 from the analysis does not affect the core results. Across all panels, the data display considerable variation both across counties and over time.

We further include a comprehensive set of county-level control variables to account for economic and socio-demographic conditions that can influence both disaster exposure and electoral outcomes.

Economic indicators include per capita income (from the Bureau of Economic Analysis Regional

⁹We restrict the analysis to natural disasters by excluding PDDs related to man-made events, such as terrorist attacks and mass shootings.

¹⁰Although this measure slightly understates turnout because the denominator includes non-citizens and disenfranchised felons, county-level voting-eligible population data are unavailable for the early years of our sample. Any resulting measurement error is likely more pronounced in high-immigrant, disaster-prone states such as California, Florida, and Texas, potentially correlating with disaster exposure. However, Appendix Table A.4 shows that our main conclusions remain unchanged when these states are excluded.

Accounts), population density, and unemployment rate (from the Bureau of Labor Statistics Local Area Unemployment database). Socio-demographic controls — such as race, educational attainment, veteran status, and age composition — are compiled from the U.S. Census Bureau. Detailed variable definitions and data sources are provided in Appendix Table A.1.

Our balanced panel covers 3,016 counties observed annually from 1994 to 2020, yielding 81,432 county-year observations ($3,016 \times 27$). Restricting the sample to the seven presidential election years after 1996 yields 21,112 county-election observations ($3,016 \times 7$).¹¹ Table A.2 summarizes the key variables. On average, the incumbent party experiences a 1.2 percentage point decline in vote share relative to the previous election, with substantial dispersion ranging from an 8.4 percentage point loss at the 10th percentile to a 5.6 percentage point gain at the 90th percentile. The mean voter turnout rate is 53.7%, varying from 36.1% to 70.5%. The average number of PDDs per county is 0.53, with a median of zero and a pronounced right-skewed distribution. Overall, these patterns highlight significant spatial and temporal variation across counties that underpins the subsequent empirical analysis.

4 Empirical Analysis

In this section, we estimate the impact of natural disasters and disaster relief on electoral outcomes within the framework of TWFE model using a county-level panel of U.S. presidential elections from 1996 through 2020 ($N = 3,016$). By controlling for both within-county and cross-county variations, this approach helps isolate the causal influence of disasters from confounding factors.

4.1 Econometric Framework and Identification Strategy

Our baseline specification is

$$y_{it} = \alpha_i + \delta_{st} + \beta_1 x_{it} + \beta_2 x_{i,t-1} + \mathbf{H}'_{it} \Gamma + \varepsilon_{it}, \quad (1)$$

¹¹The paper considers three different sample periods, each serving a distinct purpose. The election dataset begins in 1992 to compute the change in incumbent vote share for the 1996 presidential election. The annual panel starts in 1994 to construct lagged annual control variables. Finally, the estimation sample comprises the seven presidential elections from 1996 through 2020. Although the United States has more than 3,140 counties, some were created after 1994 or have incomplete data, resulting in their exclusion. In the regression analyses, the effective sample size varies across specifications due to the inclusion of lagged variables and differences in the availability of control variables.

where i indexes counties and t indexes presidential election years. The model exploits the plausibly exogenous timing and intensity of natural disasters as quasi-random shocks within a TWFE design. County fixed effect (α_i) absorb all time-invariant local characteristics, such as baseline political preferences and long-run disaster risk, while state-by-year fixed effects (δ_{st}) control for shocks common to counties within a state in a given election year. Identification therefore comes from within-county changes in disaster exposure over time, net of persistent county characteristics and state-specific election-year shocks.

Our key identification assumption is that, conditional on the fixed effects and covariates, the timing and severity of disasters are exogenous to unobserved determinants of electoral outcomes. Although realized economic losses may partly reflect local asset values rather than the underlying physical intensity of a disaster, constructing a uniform physical-intensity measure, such as rainfall, wind speed, or storm category, is infeasible in our case because our analysis covers multiple disaster types. To mitigate this concern, we scale inflation-adjusted property and crop losses per 10,000 residents by local income, thereby accounting in part for differences in economic exposure across counties. Persistent geographic risk factors are absorbed by county fixed effects, while evolving climatic patterns further enhance the unpredictability of disaster occurrence. Under these conditions, natural disasters provide plausibly exogenous shocks to local economies.¹²

As dependent variables, we consider both the change in incumbent-party vote share (y_{it}) and voter turnout (z_{it}) in county i during election year t . The former is defined as the change in the incumbent party’s county-level vote share relative to the previous presidential election (see Choi et al. (2025)), while turnout is measured as total ballots cast divided by the county’s voting-age population. Joint analysis of these outcomes allows us to distinguish between changes in voter preferences and political participation.

The main explanatory variable, x_{it} , measures disaster exposure as the log of total inflation-adjusted property and crop damages (in 2012 dollars) per 10,000 residents in county i during the election

¹²Disaster-prone regions such as coastal or Tornado Alley counties have higher baseline risks, but these persistent geographic factors are absorbed by α_i . Moreover, recent research documents notable spatial and seasonal shifts in climatic patterns — for instance, the changing geography of tornado activity across the U.S. (e.g., Coleman et al. 2024). These developments enhance the practical unpredictability of when and where specific disasters occur. Conditional on county- and time-fixed effects, which absorb persistent geographic risk and common temporal shocks, the precise timing and location of a disaster can therefore be treated as plausibly exogenous to local political or economic conditions.

year.¹³ We also include its lag, $x_{i,t-1}$, to capture any persistent effects of earlier disasters. The existing literature remains inconclusive regarding whether electoral outcomes respond more strongly to events occurring in the election year itself or to the cumulative effects of events since the previous election. As discussed in Section 5, our analysis indicates that electoral responses are driven primarily by natural disasters that occur within the election year, rather than by cumulative disaster exposure over the intervening years since the previous election. Accordingly, all analyses and control variables are based on election-year disaster exposure. The control vector $\mathbf{H}_{it}$ in Equation (1) includes interaction terms as well as lagged county-level economic and demographic characteristics (in Table 6), such as per-capita income growth, population growth, unemployment changes, and the shares of Black, Hispanic, and college-educated residents. These controls account for underlying economic and social conditions that may influence voting behavior independently of disaster exposure.

This TWFE framework is particularly well suited to our setting because disaster exposure is recurrent, heterogeneous, and lacks a common treatment date (Callaway et al., 2024; de Chaisemartin and D’Haultfoeuille, 2020). Unlike conventional policy interventions, counties may experience multiple disasters of varying severity over the sample period, making standard event-study designs difficult to implement. TWFE exploits both temporal and cross-sectional variation in disaster exposure and estimates the average electoral effect across many disaster episodes and election cycles.

To assess the validity of our identification strategy, we conduct two diagnostic exercises. First, we implement an event-study analysis that tests whether future disaster exposure predicts current electoral outcomes. As illustrated in Figure 5 and further discussed in Appendix A, we find little evidence of statistically significant pre-treatment dynamics, suggesting that the estimated effects are unlikely to be driven by pre-existing electoral trends correlated with future disasters.

Second, we perform a placebo randomization test in which disaster intensity is randomly reassigned across counties within each state-year cell while preserving the underlying distribution of exposure. Specifically, for each placebo sample, we re-estimate the baseline TWFE specification in Equation (1) and record the coefficient on disaster exposure. Repeating this procedure 500 times generates an empirical null distribution of placebo coefficients. As shown in Figure 6, the actual estimate

¹³In Section 5.1, we assess the robustness of our results using a standardized measure of disaster intensity.

(the red vertical line) lies well outside this distribution, indicating that the observed relationship between disaster exposure and electoral outcomes is unlikely to be driven by chance, incidental correlations, or unobserved common shocks.

Throughout the paper, we report heteroskedasticity-robust standard errors clustered at the county level.

4.2 Baseline Regression Results

4.2.1 Impact on Incumbent Vote Share

Table 3 reports the TWFE regression results using changes in the incumbent party vote share as the dependent variable. Rather than relying on a single model specification, we estimate several versions of the TWFE model with increasingly extensive controls. This strategy allows us not only to verify that the estimated effects are robust to alternative specifications and are not driven by omitted variables or modeling choices, but also to shed light on the underlying theoretical mechanisms at work.

We first estimate a reduced-form specification that includes only the key disaster variable in order to capture the overall electoral effect of disaster exposure itself. Accordingly, the baseline TWFE specification is interpreted as identifying the total electoral impact of disasters operating through all relevant channels. We then extend the model by incorporating PDDs and their interactions with disaster severity and political alignment. Because PDD issuance itself could be an endogenous policy response to disaster conditions, these augmented specifications are not intended to recover the total causal effect of disaster exposure.¹⁴ Rather, they are designed to assess whether electoral responses vary systematically across disaster-affected counties depending on the presence of federal disaster declarations and partisan alignment between state and federal governments. This framework permits us to evaluate the attentive retrospection and partisan-response hypotheses while explicitly recognizing that PDDs are part of the broader post-disaster policy-response process.

In all specifications considered (columns 1–4), the results consistently reveal a statistically signifi-

¹⁴A potential limitation is that our measure of election-year disaster exposure may include disasters and PDDs occurring after Election Day in early November. However, because relatively few disasters occur during the brief post-election period and any resulting measurement error is unlikely to be systematically related to electoral outcomes, this issue should have little effect on our estimates or conclusions.

cant and negative relationship between disaster damage and changes in incumbent vote share. In other words, more severe natural disasters are associated with a decline in support for the incumbent party. In the baseline specification (Column 1), a one-log-point increase in per-capita disaster damage is associated with a 0.042 percentage-point decline in incumbent vote share ($p < 0.05$). The estimated effect remains statistically significant and becomes larger as additional controls are introduced, reaching 0.170 percentage points in the most comprehensive specification with re-centering (Column 4). Although economically modest on the continuous margin, the consistency of the estimates across specifications points to a robust electoral penalty associated with disaster exposure. In contrast, the lagged disaster variable is positive but statistically insignificant throughout, suggesting that electoral responses are driven primarily by contemporaneous disasters rather than lingering effects of earlier events. Overall, these findings are broadly consistent with several theoretical perspectives summarized in Table 2.

Conditional on measured disaster severity, we further examine whether counties receiving a PDD exhibit different electoral outcomes from otherwise comparable disaster-affected counties that do not receive such declarations. Because PDD issuance may itself be an endogenous response to disaster conditions, the estimated PDD coefficient should be interpreted as a conditional association rather than a causal effect. Within this framework, the specification tests whether electoral outcomes vary systematically with the receipt of a PDD after accounting for disaster severity.

Table 3 shows that counties receiving a PDD experience significantly greater support for the incumbent party, consistent with the findings of Healy and Malhotra (2009) and Reeves (2011). The estimated PDD coefficients range from 0.25 to 0.36 percentage points ($p < 0.01$). In addition, the interaction between disaster damage and PDDs ($x_{it} \times PDD_{it}$) is positive and statistically significant across specifications, indicating that the electoral benefits associated with federal disaster declarations increase with disaster severity. This pattern is consistent with the attentive retrospection hypothesis (Gasper and Reeves, 2011), whereby voters reward incumbents when visible federal assistance helps alleviate disaster impacts.

The coefficient estimates on partisan alignment enter positively but remain statistically insignificant, as reported in columns 3–4. In contrast, the interaction between disaster damage and partisan alignment ($x_{it} \times \text{Co-partisan}$) is consistently positive and statistically significant, suggesting that

co-partisan alignment between the U.S. president and the state governor increases electoral support for the incumbent party when disasters are more severe.

At the same time, the interaction between PDDs and partisan alignment is negative and statistically insignificant, implying that the electoral benefits of federal disaster aid do not systematically increase under co-partisan alignment. Similarly, the triple interaction among disaster damage, PDDs, and gubernatorial co-partisanship reported in the final column is also negative and insignificant ($p > 0.1$), providing little support for the *double-alignment hypothesis* — the idea that the political returns to federal disaster assistance are amplified when the president and governor belong to the same party. Taken together, these results suggest that although partisan alignment may strengthen incumbent support in the context of severe disasters, it does not necessarily enhance the electoral payoff from federal disaster aid itself. More broadly, the findings provide only limited support for the partisan-effect hypothesis.

Overall, the baseline analysis indicates that natural disasters reduce electoral support for the incumbent party, while federal disaster assistance is positively associated with incumbent electoral support among disaster-affected counties. Although partisan alignment strengthens incumbent support in the context of severe disasters, we find limited evidence that it systematically amplifies the electoral returns to federal disaster aid itself. These findings help reconcile the mixed evidence in the existing literature (Achen and Bartels, 2017; Healy and Malhotra, 2010; Gasper, 2015; Gasper and Reeves, 2011; Heersink et al., 2017; Hirte et al., 2020; Morris and Miller, 2023) by showing that the political consequences of disasters depend critically on institutional responses and prevailing political conditions rather than being uniformly positive or negative. Finally, the within-county R^2 values, which consistently exceed 0.74, suggest that the specifications possess substantial explanatory power.

4.2.2 Impact on Voter Turnout

Table 4 reports the regression results using voter turnout as the dependent variable. Following Markovich and White (2022), turnout is measured as the total number of votes cast divided by the county’s voting-age population in each election year. Although the sign of the disaster coefficient varies across specifications, the estimates remain statistically insignificant throughout, suggesting

that natural disasters exert little systematic effect on voter turnout.

In the baseline specification (column 1), which includes only the key disaster variables along with fixed effects, the coefficients on both current and lagged disaster damages are statistically insignificant. After introducing PDDs and their interactions with disaster damage in column 2, the estimated effects remain insignificant at the 10 percent level. Columns 3 and 4 produce similar results, as the disaster-related coefficients continue to be statistically insignificant across specifications. The absence of a significant turnout response is only broadly consistent with the attentive retrospection and partisan-effect hypotheses, both of which imply theoretically ambiguous effects of natural disasters on voter participation. At the same time, the findings provide little support for a voter-suppression mechanism in which disasters reduce turnout by disrupting polling operations, displacing residents, or weakening civic engagement.

The evidence for a turnout effect of federal disaster assistance is similarly weak. Although the interaction between disaster damage and PDDs is statistically significant in one specification (Model 3), neither the PDD coefficient nor the interaction term is consistently significant across models. These results suggest that federal disaster declarations do not systematically increase turnout or generate stronger mobilization effects in more severely affected counties.

Partisan alignment between the U.S. president and the state governor is positively associated with turnout in Columns 3–4, but its interaction with disaster damage is statistically insignificant. Likewise, the triple interaction involving partisan alignment is insignificant. Together, these findings indicate that partisan alignment may raise turnout on average, but it does not meaningfully alter turnout responses to disasters or federal disaster aid.

In sum, our results indicate that natural disasters exert little systematic effect on voter turnout, while federal disaster declarations generate at most limited and only partially robust mobilization effects. Compared with the much larger and more consistently estimated impacts on incumbent vote share, the turnout responses are economically small and statistically weak across specifications. Taken together, the substantial decline in incumbent electoral support combined with the limited turnout response suggests that natural disasters affect electoral outcomes primarily through changes in voter preferences rather than political participation. In other words, the political consequences

of disasters operate mainly through preference shifts rather than aggregate turnout changes. No single theoretical framework summarized in Table 1 fully explains the observed turnout patterns.

4.3 Categorical Group Analysis

The TWFE analysis thus far highlights a notable contrast between the effects of natural disasters on incumbent vote share and voter turnout. While natural disasters consistently reduce incumbent electoral support, its effects on voter turnout are small, statistically fragile, and highly sensitive to specification. This contrast suggests that the mechanisms shaping voter preferences differ from those influencing political participation. One possible explanation is that aggregate turnout effects mask heterogeneous local responses. By estimating average marginal effects, continuous interaction models may obscure offsetting patterns in which disasters mobilize voters in some counties while demobilizing them in others. Exploring this heterogeneity is therefore crucial for understanding the channels through which disasters influence electoral outcomes.

To uncover these *heterogeneous* and possibly *nonlinear* effects, we complement the continuous specifications with a set of categorical regressions. Specifically, counties are classified by disaster severity (small versus large), presidential disaster declaration (PDD) status (issued versus not issued), and presidential–gubernatorial partisan alignment (aligned versus not aligned). This framework allows us to isolate the marginal role of each factor while holding the others constant and to examine whether electoral responses emerge only under particular combinations of disaster severity, federal aid, and partisan alignment. In addition, categorical analysis helps shed light on the mechanisms through which disasters influence elections. Unlike continuous interaction models, which estimate average marginal effects across the full sample, categorical specifications can reveal subgroup-specific patterns that may otherwise be masked by aggregation. As a result, they provide a useful framework for distinguishing whether the electoral consequences of disasters operate primarily through voter preference shifts, voter turnout, or a combination of both.

Formally, we estimate

$$y_{it} = \beta_2 G_2 + \beta_3 G_3 + \dots + \beta_8 G_8 + \alpha_i + \delta_{st} + \mathbf{H}'_{it} \Gamma + \varepsilon_{it}, \quad (2)$$

where G_h denotes group indicators for all combinations of three binary dimensions: disaster severity (small versus large), PDD status (issued versus not issued), and presidential–gubernatorial partisan alignment (aligned versus not aligned) as shown:

Group	coefficient	Damage	PDD	Political Alignment
Base group	-	Small	No	No
2	β_2	Small	Yes	No
3	β_3	Small	No	Yes
4	β_4	Small	Yes	Yes
5	β_5	Large	No	No
6	β_6	Large	Yes	No
7	β_7	Large	No	Yes
8	β_8	Large	Yes	Yes

The omitted base group consists of counties experiencing small disasters without a PDD and without partisan alignment, while Group 8 represents the opposite extreme: large disasters, PDD issuance, and partisan alignment. Unlike continuous interaction models, this categorical framework allows for nonlinear and non-monotonic relationships without imposing a specific functional form. The threshold separating small and large disasters is defined using either the median (50th percentile) or upper-quartile (75th percentile) of the disaster-damage distribution.

This specification also facilitates direct hypothesis testing. For example, we can assess the role of partisan alignment by comparing aligned and non-aligned counties under identical disaster and PDD conditions, and evaluate the effect of federal disaster declarations by comparing PDD and non-PDD counties holding disaster severity and alignment constant:

Testing political alignment

$$H_0^{(small, no\ PDD)} : \beta_3 = 0$$

$$H_0^{(small, PDD)} : \beta_4 = \beta_2$$

$$H_0^{(large, no\ PDD)} : \beta_7 = \beta_5$$

$$H_0^{(large, PDD)} : \beta_8 = \beta_6$$

Testing PDD occurrence

$$H_0^{(small, no\ alignment)} : \beta_2 = 0$$

$$H_0^{(small, alignment)} : \beta_4 = \beta_3$$

$$H_0^{(large, no\ alignment)} : \beta_6 = \beta_5$$

$$H_0^{(large, alignment)} : \beta_8 = \beta_7$$

The results illustrated in Figure 4 reveal several important patterns. The blue circle (●) denotes the point estimate based on the median threshold separating small and large disasters, while the red diamond (◆) denotes the corresponding estimate based on the 75th percentile threshold of the disaster-damage distribution. In the vote-share regressions (left panel), all categorical coefficients are negative and statistically significant, indicating that incumbent electoral support declines across all disaster categories relative to the reference group. To interpret, regardless of PDD issuance or partisan alignment, large disasters consistently reduce incumbent vote share. Importantly, neither PDD issuance nor partisan alignment is sufficient to generate positive electoral returns from disaster exposure. Instead, federal assistance and political alignment appear to mitigate, but not eliminate, the electoral penalties associated with natural disasters.

The turnout regressions (right panel), however, display a markedly different pattern. Turnout increases following disasters primarily in counties with partisan alignment, consistent with the partisan effect hypothesis that shared partisan affiliations strengthen voters’ confidence in government disaster response and encourage electoral participation. By contrast, turnout effects are generally small and statistically insignificant in non-aligned counties, regardless of disaster severity or the presence of a presidential disaster declaration (PDD). These findings suggest that partisan alignment is a more important determinant of post-disaster voter turnout than either disaster severity or federal disaster assistance. Notably, this pattern is not evident in the baseline continuous-interaction models (Table 4), highlighting the ability of the categorical framework to uncover subgroup-specific dynamics that are averaged out in aggregate estimates. The results are qualitatively unchanged under alternative definitions of large disasters based on the median and 75th-percentile damage thresholds.

The hypothesis tests reported in Table 5 further suggest that partisan alignment plays a more important role than PDD issuance in shaping electoral responses to disasters. The top panel strongly rejects the null hypothesis of no alignment effect at the 1% level across specifications, indicating that presidential–gubernatorial alignment systematically affects both incumbent vote share and voter turnout. Interestingly, alignment is associated with higher turnout but lower incumbent vote share, implying that the additional voters mobilized in aligned counties tend to vote against the incumbent party. This pattern is consistent with the angry-voter hypothesis and

may reflect heightened expectations of government performance and disaster response in politically aligned jurisdictions.

In contrast, the bottom panel provides much weaker evidence for an independent effect of PDD issuance. The estimated PDD contrasts are generally small, and the null hypothesis of no PDD effect cannot be rejected in most specifications, particularly for turnout. Taken together, these results suggest that federal disaster declarations alone do not systematically influence electoral outcomes. Rather, any electoral benefits associated with PDDs appear to depend on the broader political environment, whereas partisan alignment exerts an independent and more robust influence on voter behavior.

Overall, the categorical analysis confirms that the electoral consequences of disasters operate primarily through voter preference shifts rather than broad changes in political participation. At the same time, it suggests that partisan alignment plays a more consequential role than federal disaster aid in shaping electoral responses to disasters. While PDD receipt can mitigate electoral losses, its effects are neither as consistent nor as robust as those associated with presidential–gubernatorial alignment. These results align with Gasper (2015) and Reeves (2011), highlighting the importance of partisan coordination in converting federal disaster assistance into electoral benefits.

5 Robustness Check

Having established the main regression results, we conduct a series of robustness checks to examine their sensitivity to alternative measures of disaster intensity, extensive-margin specifications that include zero-damage counties, migration-induced compositional changes, and the timing of disaster occurrence relative to elections.

5.1 Alternative measure of disaster intensity with control variables

Our baseline measure of disaster intensity is based on realized economic losses. Although widely used in the literature, such losses reflect not only the physical severity of a disaster but also the value of exposed assets and local economic conditions. For example, disasters of similar physical intensity may generate larger measured damages in wealthier counties because housing, infrastructure, and

commercial assets are more valuable there.¹⁵ Consequently, disaster losses may capture differences in both hazard severity and economic exposure.

To address this concern, our baseline analysis uses inflation- and income-adjusted damages. As an additional robustness check, we construct a standardized measure of disaster intensity, $\widetilde{x}_{it} = \frac{(x_{it} - \overline{x_{it}})}{s.d.(x_{it})}$, so that the estimated coefficients represent the effect of a one-standard-deviation increase in disaster intensity. Standardization improves interpretability, reduces the influence of extreme observations, and facilitates comparison across specifications. We also include a comprehensive set of lagged county-level controls—per-capita income growth, population growth, unemployment changes, and the shares of Black, Hispanic, college-educated, and veteran populations—to account for economic and demographic conditions that may affect both disaster exposure and electoral outcomes. Lagged values are used to avoid controlling for variables that may themselves respond to disasters. Detailed variable definitions are provided in Appendix Table A.1.

Tables 6 and 7 report the results using the standardized disaster measure and the full set of controls. The estimates based on the re-centered specification are qualitatively similar to those in Tables 3 and 4. In particular, the signs, statistical significance, and overall patterns of the key coefficients remain largely unchanged, indicating that our main conclusions are robust to the choice of disaster intensity measure.

5.2 External validity and extensive-margin analysis

Our baseline analysis focuses on the *intensive margin* by excluding observations with zero reported disaster damage. As a result, identification comes from comparing counties that experience different levels of disaster losses, and the estimated coefficients capture how variation in disaster severity among affected counties influences electoral outcomes. While this approach strengthens internal validity by restricting attention to genuinely exposed counties, it leaves open the question of whether the findings generalize to counties with no disaster exposure.

To address this concern, we re-estimate the model using the transformation $\ln(1 + \text{damage})$, which retains counties with zero reported losses. This specification incorporates both the *extensive margin*

¹⁵Some studies use more objective measures of disaster intensity, such as rainfall, wind speed, or storm category. However, these measures are less suitable here because our analysis covers multiple types of natural disasters rather than a single hazard.

(disaster versus no disaster) and the intensive margin (variation in severity among affected counties), thereby providing a broader test of the relationship between disasters and electoral outcomes.

Table 8 reports the results from this broader specification. The findings remain qualitatively very similar to those obtained from the intensive-margin analysis. In particular, the estimated signs, statistical significance, and overall patterns of the key coefficients remain largely unchanged across the two approaches. This consistency suggests that our conclusions are not driven by the exclusion of zero-damage observations and generalize beyond disaster-affected counties to the broader population of U.S. counties.

5.3 Accounting for domestic migration flows

Natural disasters may affect elections not only directly but also through population displacement. If disasters induce migration among groups with distinct political preferences, changes in the composition of the electorate could influence electoral outcomes in both origin and destination counties. Given the growing evidence linking domestic migration to electoral change, it is important to assess whether our results are sensitive to this channel.

To do so, Table 9 augments the baseline specification with interactions between disaster damage and county-level migration measures, including in-migration, out-migration, and net migration ratios. Migration data are drawn from the IRS migration database (e.g., Choi et al. (2020)), which tracks county-to-county migration flows using changes in zip codes reported on individual tax returns.¹⁶

The results show that our main findings remain robust. Natural disasters continue to reduce incumbent vote share while having little systematic effect on voter turnout, indicating that the baseline results are not driven by migration-related compositional changes. At the same time, the interaction terms reveal some asymmetries. In-migration does not significantly alter electoral responses to disasters, whereas out-migration is associated with a larger decline in incumbent support, although it remains unrelated to turnout. Similarly, net migration has a positive and significant association with incumbent vote share but little effect on participation.

Overall, these findings suggest that domestic migration influences the magnitude of electoral re-

¹⁶The IRS migration database captures annual migration patterns using changes in zip codes between consecutive tax filings and covers more than 90% of the U.S. population since 1990.

sponses in some cases but does not alter the central conclusion: the political consequences of disasters operate primarily through changes in incumbent support rather than voter participation.

5.4 Robustness of timing of disaster occurrence

We further examine whether our findings are robust to the timing of disaster occurrence and whether the electoral effects of disasters vary systematically over the election cycle. Prior research offers two competing perspectives. One argues that voters respond primarily to recent and highly salient events occurring close to elections (Fair, 1996), while another suggests that voters evaluate incumbent performance cumulatively over the entire inter-election period (Choi et al., 2025). To distinguish between these views, we separately examine disasters occurring during the election year and those occurring in the three preceding off-election years.

To do so, we re-estimate equation (1) after decomposing disaster damages into four distinct periods relative to each election cycle. The results reported in Table 10 show that only disasters occurring during the election year exert a statistically significant negative effect on incumbent vote share, whereas disasters occurring in earlier years have no detectable electoral impact. These findings suggest that voters respond primarily to contemporaneous disaster shocks that are most salient during the election period, rather than to cumulative historical exposure. Our findings underscore the importance of electoral timing in shaping both government responses and voter evaluations following natural disasters.

We next examine whether the results are disproportionately driven by any particular presidential election cycle. Figure 7 presents a leave-one-election-out robustness exercise in which we sequentially re-estimate the TWFE specification while excluding one election cycle at a time. Across all iterations, the estimated disaster coefficients remain negative and statistically significant, and their magnitudes remain relatively stable. This exercise indicates that the main findings are not driven by any single election cycle but instead reflect a systematic relationship between disaster exposure and incumbent electoral performance.

Finally, we investigate whether the electoral effects of disasters differ in open-seat presidential elections, namely the 2000, 2008, and 2016 elections in which no incumbent president sought reelection. Table 11 shows that the negative effect of disaster damage on incumbent party vote share

remains statistically significant and is even larger in open-seat contests. In addition, the positive electoral effect of PDD issuance is also stronger in these elections. One possible interpretation is that, in the absence of an incumbent candidate on the ballot, voters place greater weight on retrospective evaluations of the incumbent party’s disaster management and relief efforts rather than on candidate-specific incumbency advantages. Overall, these findings further reinforce the robustness of our main conclusions across different electoral and timing environments.

6 Concluding Remarks

Natural disasters have long been recognized as events with important political consequences (e.g., Lindell and Prater (2003); Bechtel and Hainmueller (2011); Cole et al. (2012); Beg (2019); Eisessee and Strömberg (2007); Povitkina et al. (2025)), yet their electoral consequences remain the subject of ongoing debate. This study examines how natural disasters and federal disaster relief shape electoral outcomes in U.S. presidential elections between 1996 and 2020. Using county-level panel data within a two-way fixed effects framework, we analyze both incumbent-party vote share and voter turnout in order to distinguish between two competing political channels: voter preference and voter participation.

Three main findings emerge. First, severe natural disasters consistently reduce support for the incumbent party, providing evidence in favor of retrospective voting hypothesis. Second, partisan alignment between the president and the state governor emerges as a more important determinant of electoral responses than the issuance of disaster declarations itself, although federal disaster assistance through PDDs is associated with a partial offset of these electoral losses. Third, and more importantly, the electoral effects of disasters operate primarily through changes in voter preferences rather than through changes in political participation. While disasters exert statistically and economically meaningful effects on incumbent vote share, their effects on voter turnout are generally weak, inconsistent, and substantially smaller in magnitude. This pattern indicates that disasters influence elections mainly by persuading voters to reassess the incumbent party rather than by systematically mobilizing or demobilizing voters. In addition, the electoral consequences of disasters are highly time-sensitive. Only disasters occurring close to elections significantly influence voting outcomes, suggesting that voters place greater weight on recent and salient events than on

cumulative disaster experiences over an entire electoral cycle.

Overall, this paper contributes to the disaster-politics literature by providing a unified framework for understanding how disaster severity, federal disaster assistance, and partisan alignment jointly shape electoral outcomes. Our findings demonstrate that disasters influence elections primarily through changes in voter preferences rather than political participation, and that partisan alignment is a more dominant determinant of electoral responses than disaster aid itself. These findings challenge simple retrospective-voting accounts and highlight the importance of political context in mediating democratic accountability. As climate-related disasters become increasingly frequent and economically consequential, understanding these political mechanisms will be essential for evaluating the electoral incentives facing governments and the resilience of democratic institutions.

Funding declaration:

The authors received no financial support for the research, authorship, and/or publication of this article.

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Tables and Figures

Table 1: Theoretical predictions on various electoral outcomes

Hypothesis	Vote share change	Voter turnout
Angry voter hypothesis	decrease	increase
Attentive retrospection hypothesis		
Federal aid	increase	increase
No federal aid	decrease	decrease
Blind retrospection hypothesis	decrease	-
Distributive spending hypothesis	increase	increase
Financial distress hypothesis	-	decrease
Partisan effect hypothesis		
Co-partisan	increase	increase
Contra-partisan	decrease	decrease

Table 2: Summary of selected studies

Study	Main findings	Data
Achen and Bartels (2017)	Voters punish incumbent presidential candidates after natural disasters	FEMA applications & Florida turnout data
Berlemann et al. (2025)	Positive impact of wildfire disaster declarations on President Obama’s approval ratings during 2009–2017	FEMA disaster aid & MTBS wildfire data & Gallup daily poll
Chen (2013)	Strong asymmetric effect: +5.1% for Republicans, −3.1% for Democrats when aid is delivered just before election	FEMA applications & Florida turnout data
Fukumoto and Kikuta (2024)	Disaster relief increased vote share for incumbent parties’ vote share	Municipalities in Japan (1992-2017)
Gasper and Reeves (2011)	Disaster damage reduces vote share; governors are rewarded but presidents are punished when PDD is denied	County-level vote share data (1970-2006)
Healy and Malhotra (2009)	Voters reward ex-post relief but ignore ex-ante preparedness	State level FEMA data
Heersink et al. (2022)	Partisan alignment affects voters’ response to natural disasters	Hurricane Sandy and historical data
Liao and Junco (2022)	Extreme weather increases campaign contributions to incumbents in vulnerable districts and may reduce challenger performance	County-level election data (1990-2012)
McCartney (2021)	The effects of natural disasters on voter participation are theoretically ambiguous	North Carolina voting data (2010-2012)
Reeves (2011)	More than a one point increase in a statewide contest in return for a single PDD	PDDs in 50 states (1981-2004)
Schneider and Kunze (2025)	Bias in presidential disaster declarations is evident for medium-strength hurricanes, but not for very strong or very weak ones	Meteorological data on wind speed (1965-2018)

Table 3: Impacts of natural disasters on incumbent party vote share change

Explanatory variables	Model 1	Model 2	Model 3	Model 4
Disaster damage (x_{it})	-0.042†[0.017]	-0.131‡[0.023]	-0.160‡[0.025]	-0.170‡[0.028]
Lagged disaster damage ($x_{i,t-1}$)	0.024 [0.017]	0.027 [0.017]	0.027 [0.017]	0.027 [0.017]
PDD_{it}		0.296‡[0.069]	0.254‡[0.070]	0.355‡[0.088]
$x_{it} \times PDD_{it}$		0.039‡[0.007]	0.034‡[0.007]	0.039‡[0.009]
Co-partisan			0.065 [0.714]	0.803 [0.829]
$x_{it} \times$ Co-partisan			0.099‡[0.034]	0.125†[0.049]
$PDD_{it} \times$ Co-partisan				-0.210 [0.141]
$x_{it} \times PDD \times$ Co-partisan				-0.009 [0.014]
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes	Yes	Yes
R-squared (within)	0.745	0.746	0.746	0.746
Observations	13762	13762	13762	13762

Notes: The TWFE regression equation is

$$y_{it} = \alpha_i + \delta_{st} + \beta_1 x_{it} + \beta_2 x_{i,t-1} + \mathbf{H}_{it}' \Gamma + \varepsilon_{it},$$

where $i \in \{1, \dots, N\}$ indexes counties and $t \in \{1, \dots, T\}$ indexes presidential election years. y_{it} denotes the change in incumbent-party vote share, while x_{it} and $x_{i,t-1}$ denote disaster damage occurring in the election year and one year prior to the election, respectively. H_{it} denotes a vector of control variables, including PDD, co-partisan alignment, and their interaction terms. All regressions are estimated with standard errors clustered at the county level. ‡, † and asterisk (*) denote statistical significance at the 1%, 5%, and 10% levels, respectively, with clustered standard errors reported in square brackets. The final sample includes 3,016 counties with complete data for presidential elections.

Table 4: Impacts of natural disasters on voter turnout

Explanatory variables	Model 1	Model 2	Model 3	Model 4
Disaster damage (x_{it})	-0.007 [0.047]	0.038 [0.060]	0.010 [0.053]	-0.036 [0.059]
Lagged disaster damage ($x_{i,t-1}$)	0.168 [0.169]	0.162 [0.161]	0.162 [0.161]	0.161 [0.161]
PDD_{it}		-0.086 [0.157]	-0.128 [0.148]	0.282 [0.396]
$x_{it} \times PDD_{it}$		-0.021 [0.014]	-0.025* [0.015]	-0.005 [0.021]
Co-partisan			16.641‡[1.669]	19.519‡[3.547]
$x_{it} \times$ Co-partisan			0.097 [0.104]	0.216 [0.210]
$PDD_{it} \times$ Co-partisan				-0.853 [0.693]
$x_{it} \times PDD \times$ Co-partisan				-0.042 [0.048]
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes	Yes	Yes
R-squared (within)	0.222	0.222	0.222	0.222
Observations	13762	13762	13762	13762

Notes: The TWFE regression equation is

$$z_{it} = \alpha_i + \delta_{st} + \beta_1 x_{it} + \beta_2 x_{i,t-1} + H'_{it} \Gamma + \varepsilon_{it},$$

where z_{it} denotes the voter turnout rate in the i_{th} county in election year t , which is computed as total votes divided by the 18+ population (voting-age population) in each county (e.g., Markovich and White (2022)). Refer to the footnote accompanying Table 3 for descriptions of all remaining variables.

Table 5: Hypothesis testing for the significance of political alignment and PDD in categorical regression analysis

Null hypothesis (H_0)	Vote share change		Voter turnout	
	Median	75%	Median	75%
Hypothesis testing for the significance of political alignment				
$H_0 : \beta_3 = 0$	-1.841‡[0.426]	-1.873‡[0.422]	15.586‡[1.115]	15.929‡[1.215]
$H_0 : \beta_4 - \beta_2 = 0$	-1.570‡[0.411]	-1.408‡[0.401]	16.786‡[1.332]	16.264‡[0.954]
$H_0 : \beta_7 - \beta_5 = 0$	-1.577‡[0.421]	-1.514‡[0.421]	16.551‡[1.617]	16.620‡[1.654]
$H_0 : \beta_8 - \beta_6 = 0$	-1.162‡[0.395]	-1.235‡[0.396]	15.265‡[0.827]	15.214‡[0.885]
Hypothesis testing for the significance of PDD				
$H_0 : \beta_2 = 0$	-0.455‡[0.132]	-0.410‡[0.110]	-1.009‡[0.474]	-0.088 [0.390]
$H_0 : \beta_4 - \beta_3 = 0$	-0.184 [0.166]	0.054 [0.146]	0.190 [0.376]	0.246 [0.392]
$H_0 : \beta_6 - \beta_5 = 0$	-0.155 [0.097]	-0.076 [0.110]	0.741 [0.819]	0.535 [0.623]
$H_0 : \beta_8 - \beta_7 = 0$	0.260* [0.134]	0.203 [0.144]	-0.545 [0.481]	-0.871 [0.569]

Notes: The TWFE regression equation is

$$y_{it} = \beta_2 G_2 + \beta_3 G_3 + \dots + \beta_8 G_8 + \alpha_i + \delta_{st} + K'_{it} \Gamma + \varepsilon_{it},$$

where G_h denotes a categorical group variable capturing all combinations of three binary dimensions: disaster severity (small vs. large), PDD issuance (yes vs. no), and political alignment between the president and state governor (aligned vs. not aligned). Specifically, G_1 , the base group, represents the group characterized by small disaster damage, with no PDD being issued, and no political alignment between the president and governor, while G_8 corresponds to large disaster damage, with PDD and political alignment. K_{it} denotes a vector of county-level socioeconomic and demographic control variables. The threshold separating small and large disasters is defined either by the median (50th percentile) or the 75th percentile of the disaster damage distribution.

Table 6: Impacts of natural disasters on incumbent party vote share change (using one-standard-deviation increase above the average damage level as x_{it})

Explanatory variables	Model 1	Model 2	Model 3	Model 4
Standardized disaster damage ($\widetilde{x_{it}}$)	-0.099†[0.039]	-0.377‡[0.059]	-0.401‡[0.065]	-0.396‡[0.066]
Lagged disaster damage ($\widetilde{x_{i,t-1}}$)	0.056 [0.039]	0.063 [0.039]	0.063 [0.039]	0.066 [0.041]
PDD_{it}		-0.048* [0.027]	0.015 [0.035]	0.004 [0.035]
$x_{it} \times PDD_{it}$		0.081‡[0.017]	0.091‡[0.022]	0.091‡[0.022]
Co-partisan		-0.804 [0.652]	-0.296 [0.693]	-0.848 [0.688]
$x_{it} \times$ Co-partisan		0.233‡[0.081]	0.294‡[0.116]	0.302‡[0.116]
$PDD_{it} \times$ Co-partisan			-0.131†[0.055]	-0.126†[0.055]
$x_{it} \times PDD \times$ Co-partisan			-0.021 [0.034]	-0.029 [0.034]
Lagged income growth				0.014* [0.008]
Lagged growth in population density				0.051 [0.032]
Lagged unemployment rate				-0.136‡[0.032]
Lagged change in share of Black pop.				-0.108‡[0.026]
Lagged change in share of Hispanic pop.				-0.073‡[0.025]
Lagged change in share of college grads.				0.081‡[0.022]
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes	Yes	Yes
R-squared (within)	0.745	0.746	0.746	0.748
Observations	13762	13762	13762	13762

Notes: The TWFE regression equation is

$$y_{it} = \alpha_i + \delta_{st} + \theta_1 \widetilde{x_{it}} + \theta_2 \widetilde{x_{i,t-1}} + \mathbf{H}'_{it} \Gamma + \varepsilon_{it},$$

where $i \in \{1, \dots, N\}$ indexes counties and $t \in \{1, \dots, T\}$ indexes presidential election years. y_{it} denotes the change in incumbent party vote share and $\widetilde{x_{it}} = \frac{(x_{it} - \overline{x_{it}})}{s.d.(x_{it})}$ represents one-standard-deviation increase above the average damage. Refer to the footnote accompanying Table 3 for descriptions of all remaining variables.

Table 7: Impacts of natural disasters on voter turnout (using one-standard-deviation increase above the average damage level as x_{it})

Explanatory variables	Model 1	Model 2	Model 3	Model 4
Standardized disaster damage ($\widetilde{x_{it}}$)	-0.016 [0.112]	0.024 [0.124]	-0.084 [0.140]	-0.118 [0.140]
Lagged disaster damage ($\widetilde{x_{i,t-1}}$)	0.398 [0.399]	0.382 [0.381]	0.381 [0.381]	0.375 [0.381]
PDD_{it}		0.096 [0.125]	0.324 [0.255]	0.304 [0.255]
$x_{it} \times PDD_{it}$		-0.060 [0.036]	-0.011 [0.051]	0.014 [0.053]
Co-partisan		15.783 $\ddagger$ [1.059]	17.616 $\ddagger$ [1.939]	17.350 $\ddagger$ [2.179]
$x_{it} \times$ Co-partisan		0.230 [0.245]	0.510 [0.495]	0.521 [0.475]
$PDD_{it} \times$ Co-partisan			-0.480 [0.313]	-0.412 [0.297]
$x_{it} \times PDD \times$ Co-partisan			-0.100 [0.114]	-0.116 [0.111]
Lagged income growth				-0.034 [0.055]
Lagged growth in population density				-0.627 $\ddagger$ [0.237]
Lagged unemployment rate				0.391 $\ddagger$ [0.120]
Lagged change in share of Black pop.				0.100 [0.181]
Lagged change in share of Hispanic pop.				-0.313 $\ddagger$ [0.085]
Lagged change in share of college grads.				0.378 $\ddagger$ [0.070]
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes	Yes	Yes
R-squared (within)	0.222	0.222	0.222	0.226
Observations	13762	13762	13762	13762

Notes: The TWFE regression equation is

$$z_{it} = \alpha_i + \delta_{st} + \beta_1 x_{it} + \beta_2 x_{i,t-1} + \mathbf{H}'_{it} \Gamma + \varepsilon_{it},$$

where z_{it} denotes the voter turnout rate in the i_{th} county in election year t , which is computed as total votes divided by the 18+ population (voting-age population) in each county (e.g., Markovich and White (2022)). Refer to the footnote accompanying Tables 4 and 6 for descriptions of all remaining variables.

Table 8: Impacts of natural disasters on vote share change and voter turnout (intensive margin vs. extensive margin)

Explanatory variables	Vote share change (y_{it})		Voter turnout (z_{it})	
	Intensive	Extensive	Intensive	Extensive
x_{it} : Log disaster damage amount				
x_{it}	-0.093† [0.038]	-0.047* [0.028]	0.006 [0.117]	0.104 [0.138]
$x_{i,t-1}$	0.055 [0.039]	-0.034 [0.027]	0.394 [0.396]	0.121 [0.101]
Economic controls	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes	Yes	Yes
R-squared (within)	0.753	0.759	0.229	0.258
Observations	13,762	21,112	13,762	21,112
x_{it} : One s.d. increase in damage amount				
x_{it}	-0.099† [0.039]	-0.058* [0.032]	-0.016 [0.112]	0.172 [0.171]
$x_{i,t-1}$	0.056 [0.039]	0.045 [0.034]	0.398 [0.399]	0.290 [0.315]
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes	Yes	Yes
R-squared (within)	0.745	0.746	0.222	0.227
Observations	13,762	21,112	13,762	21,112

Notes: The ‘intensive margin’ specification excludes zero-damage observations, so identification relies solely on variation among affected counties, comparing those with larger losses to those with smaller but still positive losses. In contrast, the ‘extensive margin’ specification includes zero-damage observations by applying the transformation $\ln(1 + \text{damage})$, thereby capturing variation across both affected and unaffected counties.

Table 9: Robustness check for internal migration flows

Explanatory variables	Vote share changes (y_{it})		Voter turnout (z_{it})	
	Model 1	Model 2	Model 1	Model 2
Disaster damage (x_{it})	-0.148 $\ddagger$ [0.030]	-0.158 $\ddagger$ [0.030]	-0.079 [0.098]	-0.061 [0.073]
Lagged disaster damage ($x_{i,t-1}$)	0.023 [0.018]	0.021 [0.018]	0.224 [0.233]	0.223 [0.232]
$x_{it} \times$ In-migration ratio change	0.016 [0.032]		0.130 [0.290]	
$x_{it} \times$ Out-migration ratio change	-0.152 $\ddagger$ [0.034]		-0.013 [0.085]	
$x_{it} \times$ Net-migration ratio change		0.004 $\dagger$ [0.002]		0.001 [0.003]
PDD_{it}	0.279 $\dagger$ [0.111]	0.288 $\ddagger$ [0.112]	0.420 [0.465]	0.402 [0.454]
$x_{it} \times PDD_{it}$	0.032 $\ddagger$ [0.012]	0.033 $\ddagger$ [0.012]	0.002 [0.028]	0.001 [0.027]
Co-partisan	-1.785 [1.315]	-1.279 [1.258]	17.641 [17.895]	17.119 [16.520]
$x_{it} \times$ Co-partisan	0.086 [0.055]	0.091 [0.055]	0.237 [0.202]	0.235 [0.190]
$PDD_{it} \times$ Co-partisan	-0.175 [0.192]	-0.188 [0.193]	-0.827 [0.712]	-0.826 [0.709]
$x_{it} \times PDD \times$ Co-partisan	0.005 [0.020]	0.004 [0.020]	-0.043 [0.050]	-0.044 [0.050]
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes	Yes	Yes
R-squared (within)	0.755	0.755	0.166	0.166
Observations	12031	11994	12031	11994

Notes: In-migration (out-migration) ratio is constructed by gross in-migration (out-migration) population divided by non-migration population in each year. Net-migration ratio is the difference between in-migration ratio and out-migration ratio. All other variables remain the same as in the baseline regression shown in the notes of Tables 3 and 4.

Table 10: Effect of Disaster Timing on Vote Share Change

Explanatory variables	Model 1	Model 2	Model 3	Model 4	Model 5
Disaster damage (x_{it})	-0.149‡[0.034]	-0.102‡[0.019]			
$x_{i,t-1}$	0.031 [0.023]		0.023 [0.017]		
$x_{i,t-2}$	0.025 [0.023]			0.026 [0.017]	
$x_{i,t-3}$	-0.023 [0.022]				-0.010 [0.019]
PDD_{it}	0.361‡[0.085]	0.249‡[0.080]	0.029 [0.051]	0.037 [0.052]	0.092‡[0.041]
$x_{it} \times PDD_{it}$	0.043‡[0.009]	0.032‡[0.011]	0.010‡[0.005]	0.011‡[0.005]	0.010‡[0.005]
Economic controls	Yes	Yes	Yes	Yes	Yes
Demographic controls	Yes	Yes	Yes	Yes	Yes
County FE (α_i)	Yes	Yes	Yes	Yes	Yes
State-by-Year FE (δ_{st})	Yes	Yes	Yes	Yes	Yes
R-squared (within)	0.701	0.751	0.747	0.751	0.706
Observations	9307	13351	11611	11435	11080

The regression equation is

$$y_{it} = \alpha_i + \delta_{st} + \sum_{h=0}^3 \beta_h x_{i,t-h} + \mathbf{H}'_{it} \Gamma + \varepsilon_{it},$$

where y_{it} denotes the change in incumbent party vote share, and $x_{i,t-h}$ denotes disaster damage occurring h years prior to the election. Refer to the footnote accompanying Table 3 for descriptions of the remaining variables.

Table 11: Impacts of disasters on vote share changes for election cycles without incumbent presidents

Explanatory variables	All sample	w/o incumbents
Disaster damage (x_{it})	-0.098 $\dagger$ [0.017]	-0.171 $\dagger$ [0.039]
PDD	0.667 $\dagger$ [0.078]	0.983 $\dagger$ [0.327]
$x_{it} \times PDD$	0.090 $\dagger$ [0.011]	0.146 $\dagger$ [0.036]
Economic control variables	Yes	Yes
Demographic control variables	Yes	Yes
County fixed effect (α_i)	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes
R-squared	0.745	0.567
Observations	15806	6872

Notes: The regression equation is

$$y_{it} = \alpha_i + \delta_{st} + \beta x_{it} + \mathbf{H}'_{it}\Gamma + \varepsilon_{it},$$

where $t = 2000, 2008, 2016$ when no incumbent presidents ran for reelection.

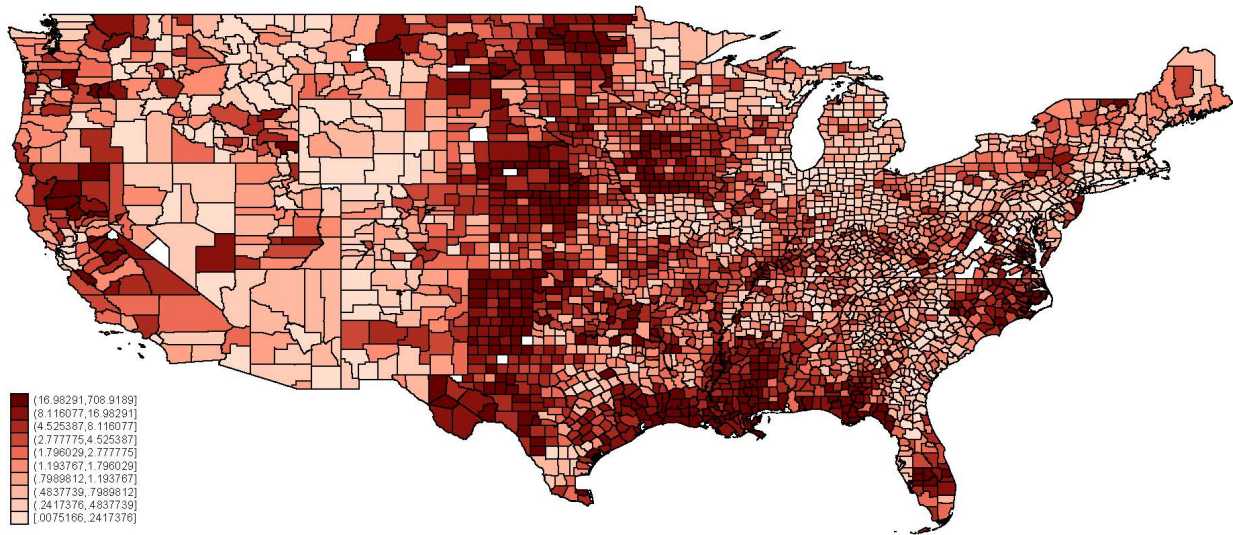


Figure 1: Average total damage per income by county (1994—2020)

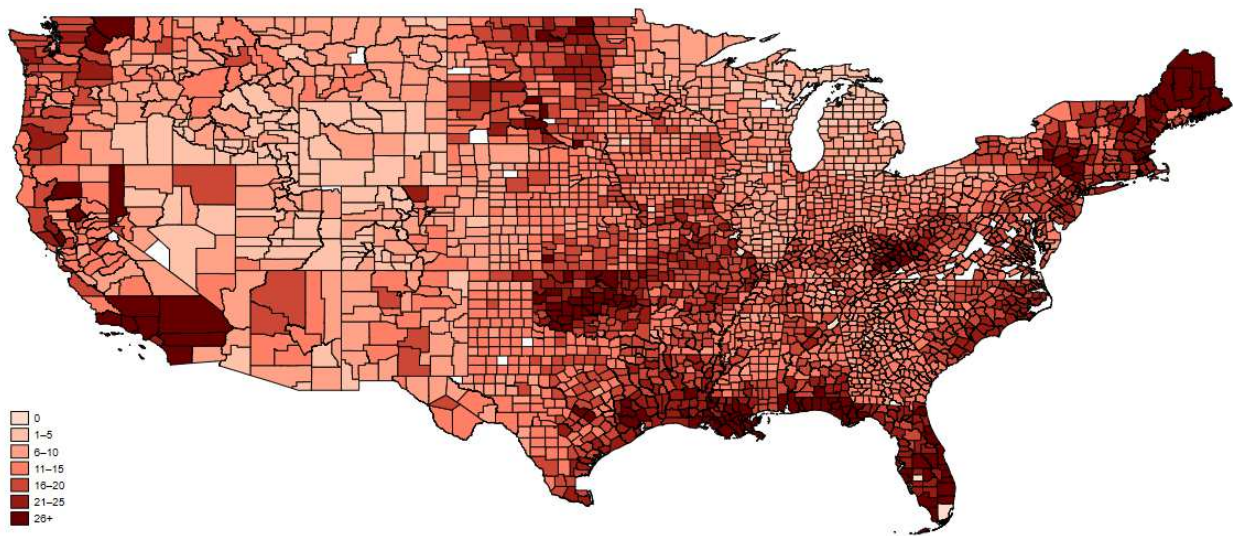


Figure 2: Total issuances of PDD among 3,016 counties

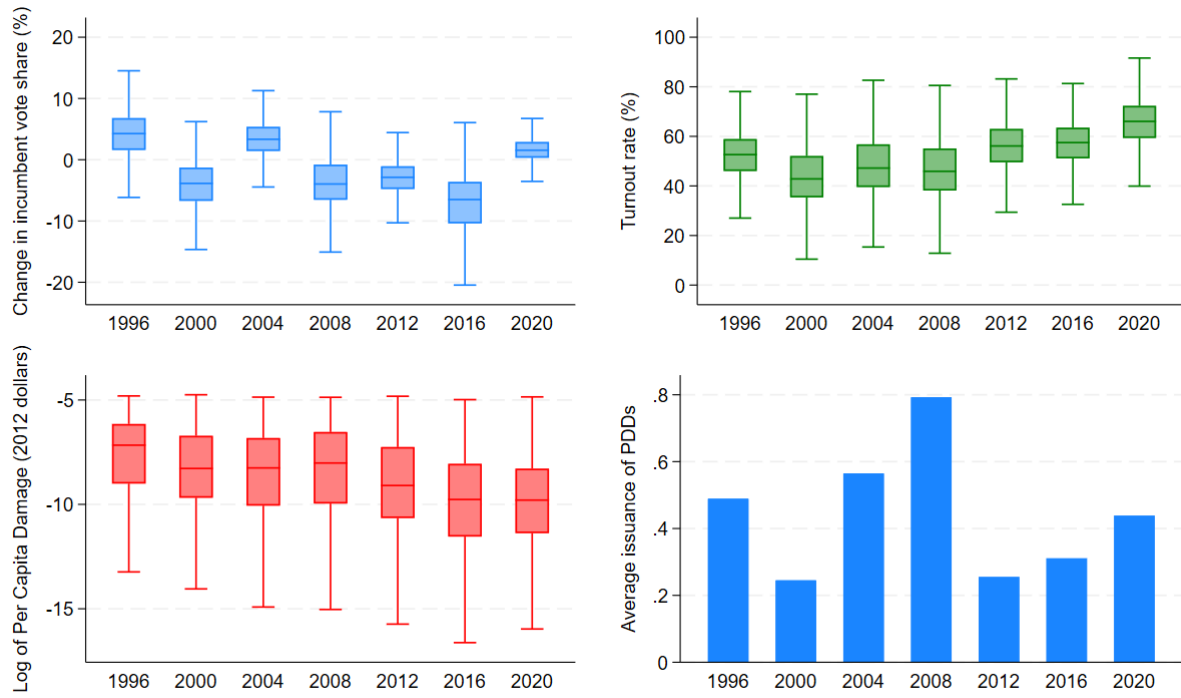


Figure 3: Boxplots of key variables

Notes: Change in incumbent vote share (top left), voter turnout rate (top right), log of per capita damage amount (bottom left), and average issuance of PDDs (bottom right) for each Presidential election cycle during 1996-2020.

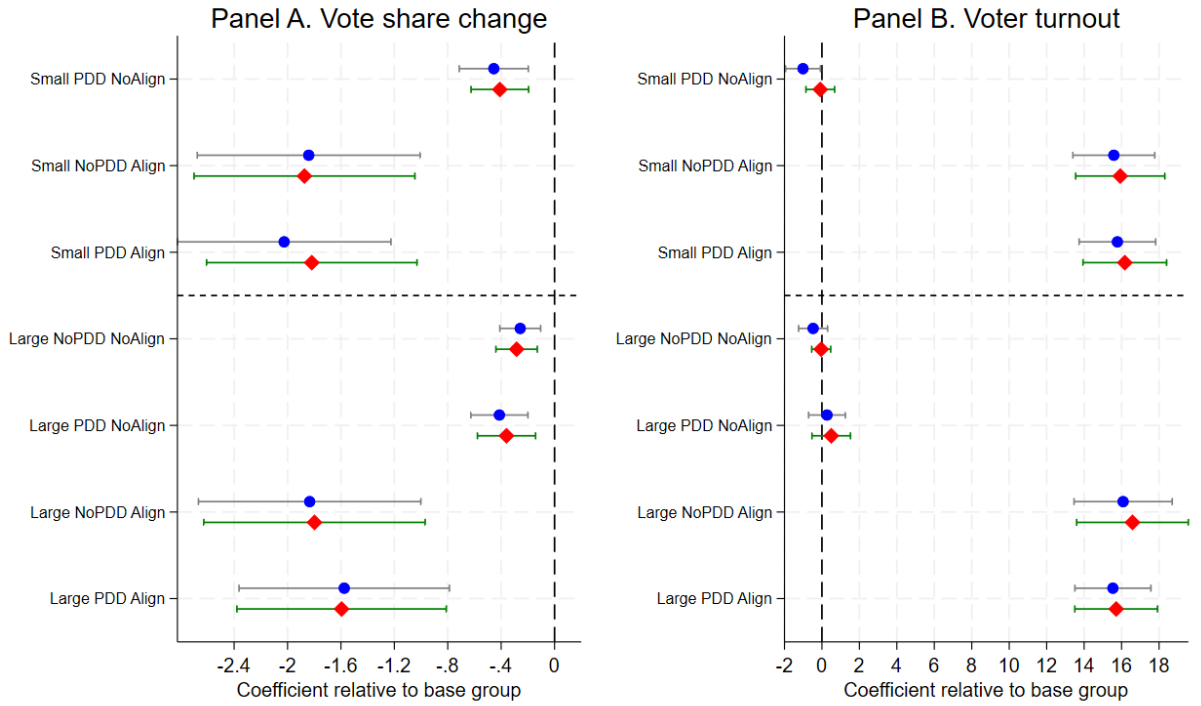


Figure 4: Results of categorical analysis

Notes: Blue circle (●) denotes the point estimate based on the median threshold separating small and large disasters, while the red diamond (◆) represents the corresponding estimate based on the 75th percentile threshold of the disaster-damage distribution.

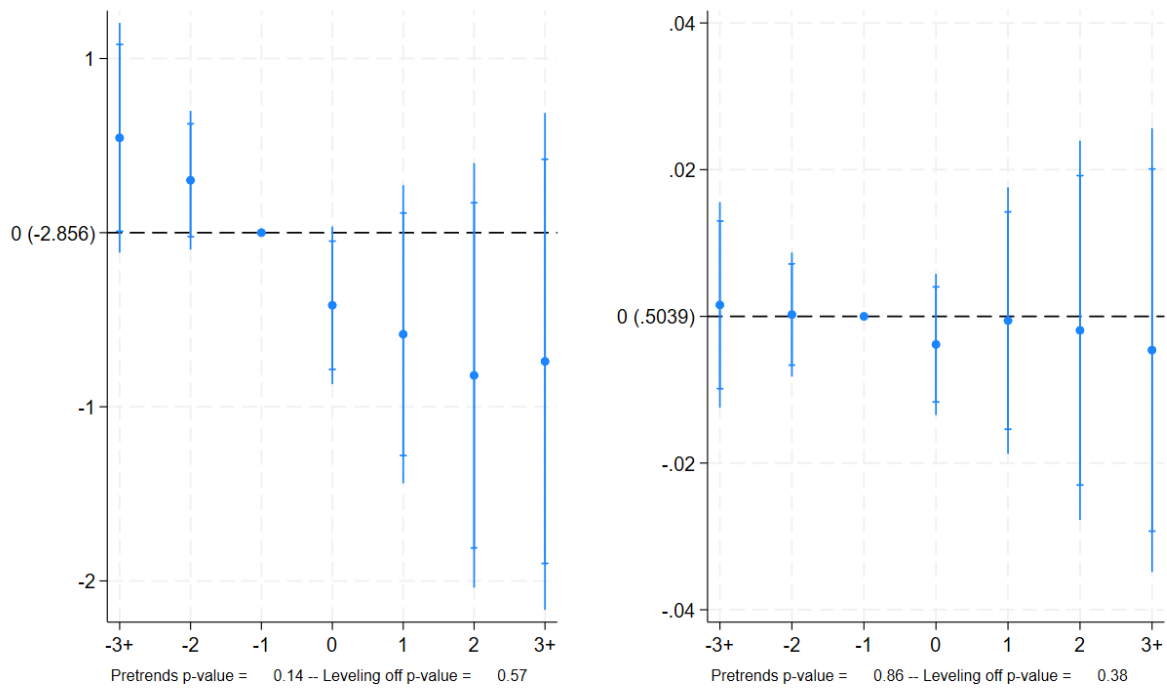


Figure 5: Diagnostic testing of PTA for vote share change (left) and voter turnout (right)

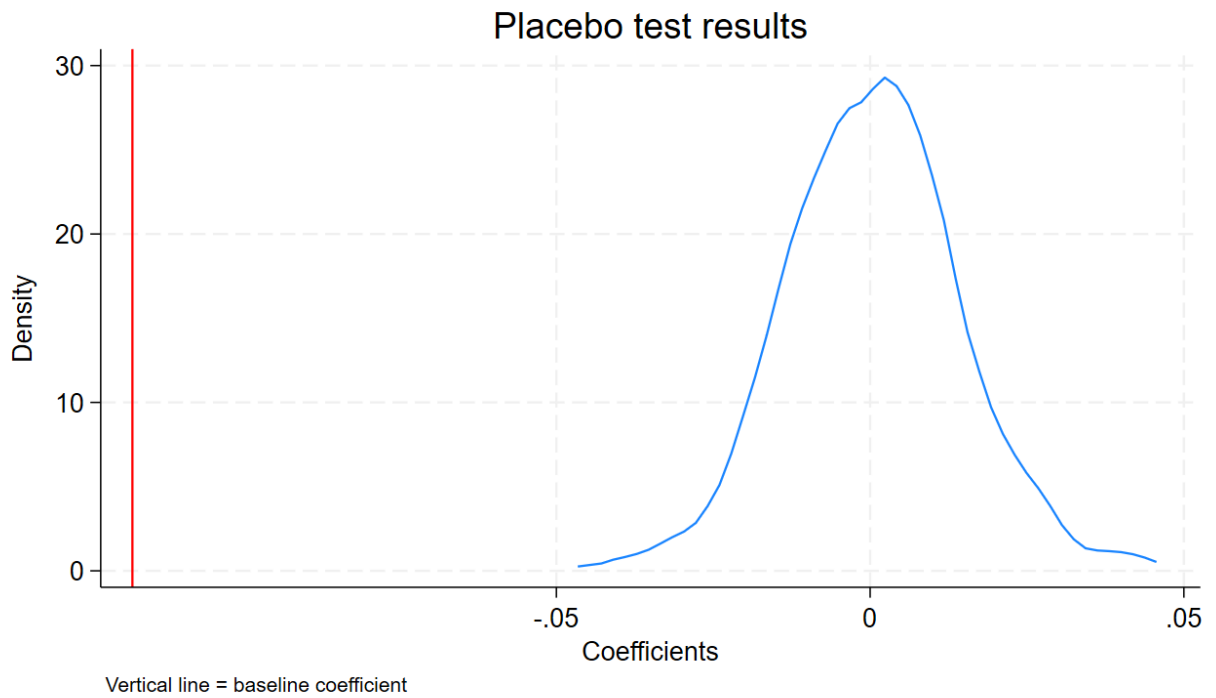


Figure 6: Placebo test results on the impact on vote share

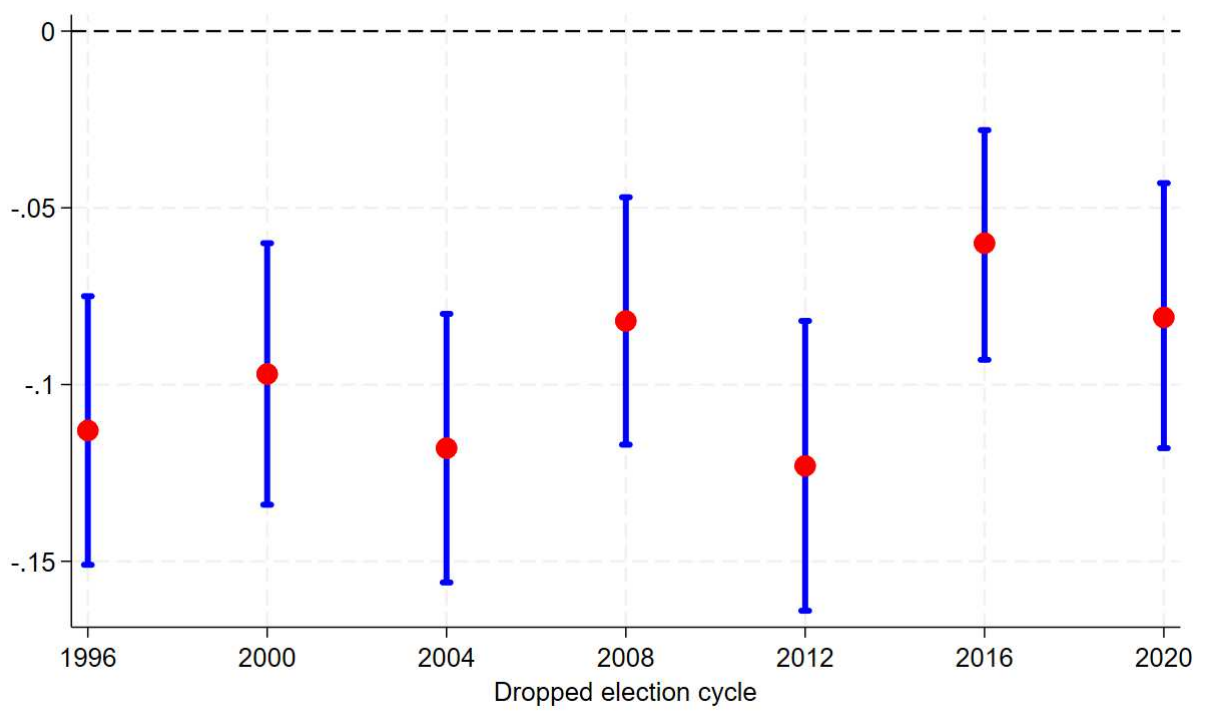


Figure 7: Impacts of disaster without specific election cycles

Appendix

Appendix A: Diagnostic testing for parallel trend assumption (PTA)

To further address concerns regarding the identifying assumptions underlying the TWFE specification, we conduct an additional diagnostic analysis using the ‘xtevent’ event-study procedure in STATA (Roth et al., 2023). Although our empirical framework differs from a canonical staggered-adoption DID design because treatment intensity is continuous, disaster exposure is recurrent, and the outcome variables are observed only during presidential election years, the event-study framework nevertheless provides a useful *diagnostic* exercise. In particular, it allows us to examine whether future disaster exposure systematically predicts current electoral outcomes through the behavior of the lead coefficients prior to treatment. The primary purpose of this exercise is therefore to assess whether the TWFE estimates could be driven by differential pre-treatment trends rather than by contemporaneous electoral responses to disasters.

The event-study results illustrated in Figure 5 provide no strong evidence against the identifying assumption. For incumbent vote share, the pre-treatment coefficients are jointly insignificant (pre-trends p-value = 0.14), implying that we fail to reject the null hypothesis of no pre-existing trends at conventional significance levels, although the test offers only modest support for the parallel trends assumption. Moreover, the lead coefficients do not exhibit a systematic upward or downward pattern prior to disaster exposure, and their confidence intervals largely overlap with zero. Although a few individual pre-treatment estimates are imprecisely estimated near zero, the overall pattern is inconsistent with meaningful differential pre-trends.

The results for voter turnout provide even stronger support for the identifying assumption. The lead coefficients are extremely small in magnitude, their confidence intervals comfortably overlap with zero, and there is no discernible systematic pre-treatment pattern. Consistent with the visual evidence, the joint pretrends test yields a p-value of 0.86, again failing to reject the null hypothesis of no pre-existing trends. Importantly, the pre-treatment estimates are not only statistically insignificant but also economically negligible. In addition, the post-treatment coefficients remain relatively small and imprecisely estimated, which is consistent with the paper’s broader finding that natural disasters exert much weaker and less systematic effects on voter turnout than on incumbent vote share.

Taken together, these event-study diagnostics provide additional support for the validity of the TWFE specification. The absence of meaningful pre-treatment dynamics suggests that the estimated electoral effects are unlikely to reflect underlying county-level electoral trends correlated with future disaster exposure. Instead, the evidence is more consistent with disasters generating contemporaneous electoral responses rather than merely capturing pre-existing differences in electoral trajectories across counties.

Appendix B: Data description

Table A.1: Data sources

Variables	Description	Data source
Election outcome	Presidential election results for 1992-2020	Congressional Quarterly (CQ)
Voter turnout rate	Total votes cast in each election divided by the voting age population (over 18 population)	Census Bureau
Disaster damage	Total damages (in 2012 dollars) relative to income, combining property and crop damages	SHELDUS
Property damage	Total property damage caused by natural disasters, inflation-adjusted (in 2012 dollars)	SHELDUS
Crop damage	Total crop damage caused by natural disasters, inflation-adjusted (in 2012 dollars)	SHELDUS
PDD	Annual number of Presidential Disaster Declarations issued in each county	FEMA
Per capita income	Annual per capita income of a county adjusted for inflation	BEA
Population growth rate	Annual growth rate in county population	BEA
Unemployment rate	County-level unemployment rate	BLS
Population density	County population divided by the county land area in square miles during 1996-2020	Census Bureau
Share of population over 18 years	Share of population based on age groups (over 18 years old)	Census Bureau
Share of population by race	Share of population based on race (Black or Hispanic population)	Census Bureau
Share of college graduate	Share of adults over 25 years old with a bachelor's or higher degree	Census Bureau
In-(out-)migration ratio	Number of in-migrants (out-migrants) divided by the number of non-migrants in a county	Internal Revenue Service (IRS)
Net-migration ratio	Difference between the in-migration and out-migration	Internal Revenue Service (IRS)

Table A.2: Summary statistics

Variables	Mean	SD	10	50	90	N
Incumbent party vote share change (% point)	-1.19	5.57	-8.40	-0.91	5.60	21112
Voter turnout rate (%)	53.73	19.72	36.08	53.25	70.51	21112
Log total damage per 10,000 residents	-8.82	2.36	-12.00	-8.62	-5.88	19505
PDD	0.53	1.22	0.00	0.00	2.00	21112
Per capita income growth (%)	4.02	5.68	-0.95	3.85	8.91	21112
Population density growth (%)	0.42	1.60	-1.15	0.29	2.16	21112
Unemployment rate (%)	5.94	2.75	3.10	5.40	9.60	21112
Average population density (1,000 ppl per sq.mi.)	0.28	1.96	0.00	0.05	0.37	21112
Share of Black population (%)	8.8	14.4	0.1	1.9	30.3	21112
Share of Hispanic population (%)	7.4	12.8	0.7	2.7	19.4	21112
Share of adults with at least bachelor degree (%)	18.2	8.4	10.0	16.1	29.5	21112
Share of veteran (%)	11.9	3.3	8.0	11.7	16.0	21112
Share of population age over 18 (%)	75.7	3.5	71.6	75.8	79.9	21112

Notes: ‘N’ denotes the total number of observations for the entire sample period. The damage variable has 19,505 observations, fewer than the total 21,112, because some counties experienced no disasters in certain election years under study. The unit of total damage is billions of dollars per 10,000 residents.

Table A.3: Disaster declarations by natural hazard type

FEMA Code	Disaster type	Number of issuance	Percent (%)
2	Coastal storm	479	1.32
4	Earthquake	54	0.15
5	Fire	3,050	8.37
6	Flood	3,974	10.91
7	Hurricane	9,651	26.50
8	Mud/Landslide	24	0.07
9	Severe ice storm	1,823	5.01
10	Severe storm	14,615	40.13
11	Snowstorm	2,160	5.93
12	Tornado	488	1.34
14	Freezing	92	0.25
17	Volcanic Eruption	2	0.01
24	Tsunami	8	0.02
Total		36,420	100.00

Table A.4: Robustness check after excluding California, Florida, and Texas

Explanatory variables	Vote share change	Voter turnout
Disaster damage (x_{it})	-0.181 [†] [0.032]	-0.036 [0.080]
Lagged disaster damage ($x_{i,t-1}$)	0.022 [0.020]	0.250 [0.271]
PDD_{it}	0.420 [†] [0.118]	0.374 [0.480]
$x_{it} \times PDD_{it}$	0.053 [†] [0.013]	-0.012 [0.027]
Co-partisan	-2.204 [1.329]	17.177 [17.187]
$x_{it} \times$ Co-partisan	0.025 [0.060]	0.216 [0.239]
$PDD_{it} \times$ Co-partisan	-0.189 [0.202]	-1.105 [0.890]
$x_{it} \times PDD \times$ Co-partisan	-0.016 [0.022]	-0.064 [0.065]
County fixed effect (α_i)	Yes	Yes
State-by-Year fixed effect (δ_{st})	Yes	Yes
R-squared (within)	0.758	0.152
Observations	10554	10554

Notes: See Model 4 in Tables 3 and 4 for the regression specifications.