

Choi, Chi-Young; Noy, Ilan; Sedai, Ashish

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Natural Disasters and Electoral Consequences - Evidence for Voter Participation and Choice in U.S. Presidential Elections*

Chi-Young Choi [†] Ilan Noy [‡] Ashish Sedai [§]

Abstract

This study investigates how natural disasters and federal disaster declarations influence electoral outcomes in U.S. presidential elections from 1996 to 2020. Using county-level panel data and a two-way fixed effects framework, we analyze both incumbent vote share and voter turnout to describe the mechanisms linking disasters to political accountability. We find that severe disasters significantly reduce incumbent-party vote share, consistent with retrospective voting hypothesis. However, Presidential Disaster Declarations (PDDs) substantially mitigate these electoral losses, particularly in politically aligned urban areas, supporting the attentive retrospection hypothesis. Disasters tend to moderately suppress voter turnout, and PDDs only partially offset this effect, suggesting that disasters primarily harm incumbents by prompting voters to switch to the opposition rather than by discouraging their supporters from participating. Categorical analyses further reveal that partisan alignment, rather than PDD issuance alone, consistently drives electoral responses. Overall, our findings highlight how institutional coordination and federal aid shape democratic accountability in the wake of natural disasters.

Keywords: Natural disasters; Presidential elections; Vote share change; Presidential disaster declaration (PDD); U.S. county; Voter turnout.

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

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[†]Corresponding author, Department of Economics, University of Texas at Arlington, Arlington, TX 75063. E-mail: cychoi@uta.edu.

[‡]Department of Economics, Victoria University of Wellington, New Zealand and Gran Sasso Science Institute, Italy.

[§]Department of Economics, University of Texas at Arlington, Arlington, TX 75063.

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 expanding body of research, the debate over whether disasters politically help or harm incumbents remains unsettled, reflecting both theoretical ambiguities and mixed empirical evidence.

Theoretically, the political consequences of disasters are ambiguous and depend on how voters interpret incumbent performance and which electoral measure is considered (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 divided (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, for incumbents, 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

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

²When a major disaster occurs, the governor may request a federal disaster declaration from the U.S. President. This request is contingent on demonstrating that state capacity is insufficient to manage the event. The decision involves joint assessments by the Federal Emergency Management Agency (FEMA) and local officials. FEMA then records each federally declared disaster along with its declaration date and the designated affected counties. For more information, see FEMA's disaster declaration process:

discretionary nature and political salience of PDD-driven aid (Garrett and Sobel, 2003; Reeves, 2011).

The purpose of this study is to bridge this gap by exploring how natural disasters affect electoral outcomes by separating their impacts on incumbent vote share and voter turnout. This decomposition helps clarify whether disasters influence voter preferences, participation, or both. We also investigate how the distribution of federal aid through PDDs is related to these channels. Using county-level data from more than 3,000 U.S. counties across seven presidential elections (1996–2020), we offer a unified analysis of how disasters and federal relief are associated with changes in local electoral support for the incumbent party.

In this regard, we make several key contributions to the literature. First, while most previous studies focus on vote share or voter turnout, we examine them jointly, offering a more comprehensive view of how natural shocks influence electoral behavior. Disasters may affect elections through two channels: (i) direct changes in vote share by shifting voter preferences, and (ii) indirect effects by changing turnout, and thus altering the composition of the electorate. 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. Consequently, analyzing vote share in conjunction with turnout enables us to disentangle preference shifts (retrospective evaluations) from participation changes (mobilization or suppression).

Second, we expand the empirical scope by examining all types of natural disasters across <https://www.fema.gov/disaster-declaration-process>.

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 consequences³, 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 design that utilizes both temporal and spatial variation in disaster exposure (Choi et al., 2025a). This approach controls for unobserved, time-invariant county characteristics, nationwide electoral trends, and a comprehensive set of socioeconomic covariates. Our identification here relies on within-county variation in disaster severity over time, assuming that, after accounting for county- and state-by-year fixed effects, natural disaster intensity varies in a plausibly exogenous manner — effectively random within each county over time (e.g., Henkel et al. (2024)). This framework allows us to isolate how changes in local disaster exposure influence voter preferences and participation, net of persistent political and economic differences.

Finally, we explore how PDDs shape the relationship between natural disasters and electoral outcomes. Although earlier studies suggest that voters may reward incumbents for effective disaster management (Neugart and Rode, 2021), the evidence on whether PDDs themselves generate electoral benefits is mixed. Some work finds that PDDs boost incumbent support (Healy and Malhotra, 2010; Gasper and Reeves, 2011), while others underscore the conditioning role of partisan alignment (Gasper, 2015; Reeves, 2011), showing that federal aid tends to benefit the incumbent party in co-partisan counties and may yield neutral or even adverse effects in opposition areas. In line with this view, Heersink et al. (2022) document

³Hurricanes 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).

that President Obama’s approval gains following Hurricane Sandy were concentrated in Democratic-leaning counties.

Our main findings can be summarized as follows. First, more severe disasters significantly reduce the vote share of incumbent party, consistent with retrospective voting theory. Second, federal disaster declarations mitigate this electoral penalty—the issuance of a PDD substantially offsets the loss in incumbent party support, consistent with the attentive retrospection hypothesis. The mitigating effect is more pronounced in urban areas and when federal aid is delivered under co-partisan alignment across levels of government. Third, disasters tend to modestly reduce voter turnout, and PDDs only partially counteract this decline, suggesting that the electoral costs to incumbents arise primarily from shifts in voter preferences toward the opposition, rather than from reduced participation among their supporters. Fourth, categorical analyses reveal that partisan alignment, rather than PDD issuance alone, consistently drives electoral responses. Alignment between the president and the governor of the disaster-affected state magnifies the political rewards of PDD and its associated aid inflows, implying that coordination across levels of government is key to obtaining electoral gains from federal disaster relief.

Our results are robust to multiple checks, including alternative specifications, varying definitions of disaster severity, event timing, and exclusion of individual election cycles. Placebo tests exploiting the quasi-random timing of natural disasters further confirm that the observed relationships are not driven by confounding trends.

The remainder of the paper proceeds as follows. Section 2 reviews related literature on disasters and electoral outcomes. Section 3 describes the data and variables. Section 4 details the empirical methodology and presents results from the TWFE analyses, including categorical group and robustness checks. Section 5 concludes with key implications. Additional data details appear 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. \(2022\)](#)). Economic theory itself 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 \(2016\)](#)). 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 \(2016\)](#)), 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 measurably 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](#)).⁴

Beyond retrospective assessments, partisan alignment plays a critical 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.

⁴Effective incumbent performance in post-disaster recovery has also been linked to increased political polarization, as re-elected officials become more likely to adopt more extreme ideological positions (e.g., [Dodlova and Zudenkova \(2021\)](#)). Beyond natural disasters, [Mehic \(2023\)](#) shows that non-natural environmental accidents can prompt voters to shift their support toward issue-focused opposition parties.

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 Presidential Disaster Declarations (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.

Most existing studies focus on vote share, potentially conflating two distinct mechanisms: electoral mobilization (voter turnout) and electoral persuasion (vote choice). Yet disasters may shape elections through both channels. Put simply, in addition to altering political preferences, disasters can suppress voter participation by disrupting access to the polls or creating financial or psychological barriers to voting. Despite its potential importance, this mobilization channel remains understudied. A notable exception is Chen (2013), who finds that pre-election disaster aid in Florida increased turnout among Republicans (the incumbent party) while dampening participation among Democrats. This pattern suggests that disasters and relief efforts can shift electoral outcomes by altering the composition of the electorate, not merely by influencing vote choice. Chen’s evidence, however, is drawn from an idiosyncratic Florida setting, 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 assistance make it an informative but atypical case, raising concerns about generalizability. Therefore, the turnout asymmetries documented in Chen (2013) may not extend to regions with different hazard risks, demographic structures, or political institutions. Our analysis addresses this limitation by broadening the scope to the national level, constructing a multi-decade panel that incorpo-

rates heterogeneous disaster types, varying intensities, and diverse partisan configurations. Theoretically, the turnout effect of disasters is ambiguous. As [McCartney \(2021\)](#) observes, affected citizens may either mobilize in anger (“angry voter hypothesis”) or withdraw due to apathy, stress, and financial or emotional hardship generated by the disaster. Equally, voters’ perceptions of the generosity and efficacy of post-disaster assistance can either motivate them to vote or decrease their motivation. Determining which mechanism typically dominates is therefore an open empirical question.

3 Data and Descriptive Statistics

Our unit of analysis is the county-election year, spanning from the 1992 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 vote share between consecutive elections (see [Choi et al. \(2025a\)](#)).⁵ 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 log of total losses per 10,000 residents (in 2012 dollars).⁶ Although SHELDUS provides the most comprehensive cover-

⁵The 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.

⁶In the regression analysis, observations with zero reported damage are excluded. A zero-damage obser-

age 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).⁷ 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 (e.g., Choi et al. (2025b)).

Data on Presidential Disaster Declarations (PDDs) are drawn from FEMA’s official records for the period 1994–2020. These records identify both the incident period and the affected counties for all federally declared disasters.⁸ We construct a binary indicator equal to one if a PDD was issued for a given county-year and zero otherwise. 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.

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

variation indicates the absence of a disaster event, and including such cases would conflate counties or years without disaster exposure with those experiencing actual shocks. Restricting the sample to observations with positive damage values therefore ensures that the estimated effects reflect the variation in disaster severity among genuinely impacted cases rather than differences between disaster and non-disaster contexts.

⁷In 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.

⁸We exclude PDDs following man-made incidents such as terrorist attacks or mass shootings and focus solely on natural disasters.

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).⁹ 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 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 1992 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 observations ($3,016 \times 7$).¹⁰ 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

⁹Although this measure slightly overstates participation because the denominator includes non-citizens and individuals with felony disenfranchisement, we stick to it because county-level data on the voting-eligible population are unavailable for the earlier years of our sample.

¹⁰Because our focus is the change in vote share relative to the previous presidential election, 1996 serves as the first cycle in our sample. Although the United States has more than 3,140 counties, some were created after 1994 or have incomplete data, resulting in their exclusion.

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. We also conduct a two-stage regression to examine the mediating role of voter turnout in shaping the effect of disasters on incumbent vote share, followed by a series of robustness checks to validate our findings.

4.1 Econometric Framework and Identification Strategy

Our baseline specification is given by

$$y_{it} = \alpha_i + \delta_{st} + \beta_1 x_{it} + \beta_2 x_{i,t-1} + \beta_3 y_{i,t-4} + \mathbf{H}'_{it} \Gamma + \varepsilon_{it}, \quad (1)$$

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. Identification comes from within-county variation in disaster exposure over time, net of persistent county characteristics and state-specific shocks in each election cycle.

As the dependent variable, y_{it} , we consider both the change in incumbent-party vote share and voter turnout in county i during election year t . The change in incumbent-party vote share is defined as the difference between the incumbent party's county-level vote share in the current election and its vote share in the previous election (see [Choi et al. \(2025a\)](#)). A negative value therefore reflects a decline in support for the incumbent party. We also

examine voter turnout (z_{it}) using the same empirical specification as in equation (1) where z_{it} is defined as total ballots cast divided by the county’s voting-age population. Together, these outcomes allow us to distinguish between changes in candidate preference (vote share) and political participation (turnout).

The TWFE model includes a county fixed effect (α_i) to capture all time-invariant local attributes, such as baseline political leanings and long-run disaster risk, and state-by-year fixed effects (δ_{st}) to control for shocks common to all counties within a state in a given election year, including state-level political or economic conditions. Identification thus arises from comparisons between counties exposed and unexposed to disasters *within the same state and election year*. Under the standard parallel-trends assumption, treated and untreated counties in the same state-year are assumed to follow similar electoral trajectories absent disaster exposure.

Our key identification assumption is that, conditional on the fixed effects and covariates, the timing and severity of disasters are exogenous to other unobserved political influences. This is plausible given the essentially random nature of most natural hazards.¹¹ While disaster-prone regions such as coastal or Tornado Alley counties have higher baseline risks, these persistent geographic factors are absorbed by α_i .¹² Moreover, recent research documents notable spatial and seasonal shifts in climatic patterns - for example, 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. Under this framework, natural disasters function as exogenous shocks to local economies, providing a quasi-experimental setting for identifying their causal effects.

¹¹[Henkel et al. \(2024\)](#) treat the precise location and timing of hurricane landfalls as exogenous, exploiting variation in storms’ proximity to Election Day while controlling for location and time fixed effects.

¹²Systematic differences in exposure stem primarily from underlying factors such as income, geography, and institutional capacity rather than from contemporaneous political dynamics. This pattern is consistent with cross-country evidence from [Kahn \(2005\)](#), who shows that these structural fundamentals strongly influence both the likelihood and severity of disaster impacts.

Meanwhile, any nationwide political tides, such as wartime rallies or economic crises, or state-level factors, such as a presidential candidate’s statewide popularity, statewide referenda, or regional economic shocks, are absorbed by δ_{st} . Throughout the paper, we report heteroskedasticity-robust standard errors clustered at the county level, allowing for arbitrary serial correlation in ε_{it} within each county over time.

The key 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 year.¹³ We also include its lag, $x_{i,t-1}$, to capture any persistent effects of earlier disasters, and a lagged dependent variable ($y_{i,t-4}$) to account for mean reversion or momentum in local voting patterns. For example, if an incumbent party gained unusual support in the last election (perhaps due to a popular outgoing incumbent president or other one-time factors), a decline in the subsequent election might be expected irrespective of any disaster, and $y_{i,t-4}$ absorbs such dynamics. Additionally, by conditioning on past changes in vote share, we aim to address the concern that our results are driven by continuation of pre-existing partisan swings rather than the disaster shock itself.

Unlike canonical policy interventions, disasters do not occur at a uniform point in time, and many counties experience multiple events of varying severity. Consequently, an event-study design is impractical given overlapping exposures and the limited number of election cycles. The TWFE framework instead exploits all available temporal and cross-sectional variation, averaging the effect of disaster exposure across many episodes and years. This approach, while still reliant on parallel trends, does not require defining a single treatment date per county and yields an interpretable average treatment effect on the treated analogous to a multi-period difference-in-differences design.

The control vector \mathbf{H}_{it} includes time-varying county characteristics such as per-capita income growth, population growth, changes in unemployment, and shifts in socio-demographic

¹³The 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 (e.g., Choi et al. (2025a)). As discussed in Section 4.5, 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 in this paper, including the construction of control variables, are based on election-year disasters.

composition (shares of Black, Hispanic, college-educated, and veteran populations). These covariates account for contemporaneous economic or social changes that could influence voting behavior independently of disasters.

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 in seven presidential election cycles. Rather than relying on a single model specification, we estimate several versions of the TWFE model with increasingly extensive controls. This strategy allows us to verify that the estimated effects are robust to alternative specifications and not driven by omitted variables or model design choices.

In all specifications considered (columns 1–4), the results consistently reveal a statistically significant and economically meaningful 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 model (column 1), a one-unit increase in logged per capita disaster damages, equivalent to a doubling of disaster losses, is associated with a 0.118 percentage-point reduction in incumbent vote share ($p < 0.01$). This negative effect remains stable and even strengthens slightly when lagged vote share and additional controls are included (–0.130 pp in column 2 and –0.129 pp in column 3), although it attenuates slightly to –0.101 pp once the triple interaction terms are added in column 4. The lagged disaster variable ($x_{i,t-1}$) is positive but modest in magnitude and only marginally significant in columns 2–4 ($p < 0.1$), suggesting a limited degree of persistence in the electoral effects of disasters.

To assess whether our results are driven by spurious pre-existing trends rather than a genuine causal effect, we conduct a placebo test following standard practice in the literature. Figure 4 shows the distribution of placebo coefficients from 500 simulated replications for Model 1, with the red vertical line representing the actual estimate reported in Table 3. The actual estimate lies well outside the placebo distribution, suggesting that the observed

relationship between disaster damage and vote share change is unlikely to arise from spurious correlation or underlying unobserved trends.

Another notable result concerns the role of Presidential Disaster Declarations (PDDs). Counties receiving a PDD exhibit a strong, positive association with incumbent electoral gains, consistent with earlier findings by [Healy and Malhotra \(2009\)](#) and [Reeves \(2011\)](#). The estimated coefficients on PDDs range from 0.71 to 0.76 percentage points ($p < 0.01$), indicating that federal disaster declarations are associated with meaningful electoral gains for the incumbent party. In addition, the interaction between disaster damage and PDDs ($x_{it} \times PDD_{it}$) is consistently positive and statistically significant, suggesting that the electoral benefits of federal relief grow with the severity of the disaster. This pattern aligns with the notion of attentive retrospection ([Gasper and Reeves, 2011](#)), whereby voters reward incumbents when visible federal assistance helps offset disaster impacts. Notably, the positive interaction effect remains robust after accounting for partisanship and urbanization ($p < 0.05$).

The triple interaction among disaster damage, PDDs, and gubernatorial co-partisanship in columns 3–4 offers support for the *double-alignment hypothesis* — the notion that the political returns to federal disaster aid are amplified when the governor and the President belong to the same party. The estimated coefficient is positive and statistically significant at the 5 percent level, indicating that electoral gains from federal aid are heightened under partisan alignment across state and federal executives. Likewise, the positive and significant interaction between PDDs and population density suggests that the electoral dividends of disaster aid are larger in more urbanized counties, where federal assistance is likely more visible and salient to voters. The heterogeneity analysis also reveals notable partisan asymmetries. In Democratic-leaning counties, the marginal effect of PDDs on incumbent vote share is negative and statistically significant (-0.124 pp, $p < 0.05$), whereas in Republican-leaning and politically competitive swing counties, the effect is statistically insignificant.¹⁴ This pattern suggests that disaster relief may provide electoral benefits mainly in aligned or more persuadable constituencies, while offering limited advantages - and possibly modest

¹⁴Swing counties are defined as those with a mixed voting history, where the dominant party has shifted at least five times over the past seven presidential elections.

drawbacks - in opposition strongholds.

The coefficients on the control variables largely align with prior expectations. Economic growth, as captured by increases in per capita income, is positively associated with incumbent support, whereas rising unemployment corresponds to electoral losses. Among demographic changes, growth in the shares of college-educated residents and veterans is linked to stronger support toward incumbent party, while increases in the Hispanic population share are negatively related. The sizable negative coefficient on population-density change suggests that compositional shifts in rapidly urbanizing counties may disadvantage the incumbent party. Finally, the within-county R^2 values, which range from 0.72 to 0.75, indicate substantial explanatory power.

Overall, the baseline analysis shows that natural disasters reduce incumbent-party electoral support, but federal disaster aid can attenuate, or partially offset, these losses when delivered under partisan alignment and in more urbanized regions. These findings help reconcile mixed evidence in prior research (Achen and Bartels, 2016; Healy and Malhotra, 2010; Gasper, 2015; Gasper and Reeves, 2011; Heersink et al., 2017; Hirte et al., 2020; Morris and Miller, 2023) by highlighting that the political consequences of disasters depend critically on institutional responses and the prevailing political conditions, rather than being uniformly negative or positive.

4.2.2 Impact on Voter Turnout

Table 4 presents the regression results using voter turnout as the dependent variable. Recall that turnout is measured as the total number of votes cast divided by the county's voting-age population in each election year, following Markovich and White (2022). Although statistical significance varies across model specifications, the results suggest that natural disasters have a modest effect, suppressing electoral participation, while federal disaster declarations - despite its positive sign - does not substantially offset this decline. Relative to the incumbent vote share results in Table 3, the estimated coefficients for turnout are smaller in magnitude, less precisely estimated, and more sensitive to model specification, indicating that the impact of disasters on turnout is weaker and more variable than their

effect on vote share change.

In the baseline specification (column 1), which excludes lagged turnout ($y_{i,t-1}$) and other controls, the coefficient on lagged per capita disaster damages is positive but not statistically significant. After introducing lagged turnout in column 2, the estimated effect becomes larger in magnitude and marginally significant at the 10 percent level. The results imply that doubling per capita disaster damages is associated with roughly a 0.13 p.p. decline in voter turnout, conditional on county and state-by-year fixed effects and prior turnout. Although modest in size, the negative sign is consistent with a voter-suppression mechanism whereby natural disasters may discourage participation by disrupting polling operations, displacing residents, or weakening civic engagement. Models 3 and 4 produce similar point estimates, but the coefficient becomes statistically insignificant in the fully interacted specification (column 4), likely reflecting reduced precision due to collinearity.

The coefficients on Presidential Disaster Declarations (PDDs) are uniformly positive but statistically insignificant in all model specifications considered, indicating that counties receiving a PDD do not experience a meaningful increase in turnout. This pattern is aligned with a modest mobilization mechanism that could stem from heightened visibility of federal assistance, increased administrative activity, or greater local media coverage. However, the evidence for such an effect is weak, and the interaction between disaster damage and PDD is also insignificant. This suggests that the mobilizing influence of PDDs does not vary with disaster severity, nor do declarations themselves generate measurable turnout gains independent of damage levels.

Model 4 examines heterogeneity in turnout responses. Most of the triple interaction terms are statistically insignificant, with one exception: the interaction among disaster damage, PDDs, and swing-county status is positive and marginally significant at the 10 percent level. This indicates that in politically competitive counties, PDDs issued after disasters are associated with an additional 0.28 p.p. increase in turnout relative to non-swing counties. This pattern is consistent with the idea that swing areas, where both parties campaign more intensely and political engagement is generally higher, are particularly responsive to visible government actions. By contrast, the triple interactions involving co-partisan alignment

or urbanization are small and insignificant, suggesting that turnout responses to disaster aid do not systematically vary along partisan or rural–urban dimensions. Likewise, the interactions for Democratic- and Republican-leaning counties are statistically insignificant. Overall, our results show that natural disasters have modest suppressive effects on voter turnout, while federal disaster declarations generate only limited mobilization gains. These turnout effects are small and not consistently robust, especially when compared to the much larger and more consistently estimated impacts on incumbent vote share. Taken together, the substantial negative response of incumbent support, coupled with only moderate turnout reductions, suggests that natural disasters influence electoral outcomes primarily through changes in voter choice rather than through changes in participation. To rephrase, the political consequences of natural disasters appear to arise more from shifts in voter preferences than from changes in participation, suggesting that punishment dynamics operate primarily through persuasion rather than turnout, although turnout remains a contributing factor.

4.3 Indirect Effect of Disaster Through Voter Turnout

Our baseline results so far reveal a substantial negative effect of natural disasters on incumbent vote share but only a modest reduction in voter turnout. In other words, disasters appear to shape electoral outcomes primarily through persuasion - shifting voter preferences - rather than through mobilization. At the same time, PDDs modestly bolster incumbent support by improving voters' evaluations without meaningfully altering participation. These patterns raise a natural question: if turnout declines only slightly, why does incumbent support fall so sharply? Do small turnout reductions disproportionately remove incumbent-leaning voters from the electorate, or is the vote-share decline driven almost entirely by voters switching away from the incumbent? More broadly, how much of the electoral penalty reflects participation changes (voter turnout), and how much reflects preference shifts (vote share)?

To address these questions, we examine the indirect effect of disasters mediated through turnout. Our goal here is to separate the indirect turnout channel from the direct effect

of disasters on vote share (e.g., Heckman and Pinto (2015)). To do so, we implement a two-stage least squares (2SLS) mediation framework that leverages exogenous variation in disaster exposure to isolate the portion of the disaster effect that operates through turnout, after controlling for lagged turnout, county fixed effects, and state-by-year fixed effects.¹⁵ Investigating the turnout-mediated channel is essential for reconciling the large decline in vote share with only a marginal drop in turnout. Even small participation changes could disproportionately reduce incumbent-leaning turnout, so quantifying this indirect channel is informative and imperative. Isolating the mediated effect allows us to assess whether the electoral penalty stems primarily from participation changes or from direct shifts in voter preferences, i.e., whether disasters primarily influence electoral outcome through persuasion rather than turnout.

The intuition behind the two-stage approach is straightforward. In the first stage, we estimate how disaster exposure influences voter turnout. In the second stage, we regress the change in incumbent vote share on the *predicted* turnout component generated by the first stage. This framework clarifies how much of the overall electoral response to natural disasters can be attributed to changes in voter participation versus changes in voter preferences.

The first-stage equation is specified as

$$z_{it} = \alpha_i + \delta_{st} + \pi_1 x_{it} + \pi_2 x_{i,t-1} + \pi_3 z_{i,t-4} + H'_{it} \Pi + \pi_4 PDD_{it} + \pi_5 (x_{it} \times PDD_{it}) + \omega_{it}, \quad (2)$$

where z_{it} denotes voter turnout, and all other variables are defined identically to those in the baseline specification in equation (1).

As shown in the first column of Table 5, the estimated coefficient on current disaster damage (x_{it}) is negative (about -0.13) and statistically significant, indicating that disasters modestly reduce voter turnout in the election year. This finding is consistent with temporary

¹⁵We do not treat disaster exposure as a conventional instrument for turnout, as the exclusion restriction does not hold in the standard IV sense - disasters clearly affect vote share directly. Instead, the 2SLS setup uses disaster exposure to recover the portion of its total effect on vote share that is mediated through turnout and to address potential concerns about turnout endogeneity. If turnout responds strategically or reflects unobserved local conditions, it may mediate part of the relationship between disasters and voting behavior.

disruptions to civic engagement, such as displacement, reduced attention to politics, or logistical obstacles to voting. The absence of significant effects from lagged disasters suggests that these disruptions are short-lived rather than persistent. The coefficients on PDD_{it} and the interaction term ($x_{it} \times PDD_{it}$) are positive but statistically insignificant, indicating that federal disaster declarations do not meaningfully raise turnout or relax constraints enough to stimulate additional participation. Other controls behave largely as expected: lagged turnout ($z_{i,t-4}$) is strongly predictive of current turnout ($\pi_3 \approx -0.88$), indicating voter turnout oscillates around county averages across elections.

The second stage of the mediation framework regresses the change in incumbent vote share (y_{it}) on the *predicted* component of turnout from the first stage (\hat{z}_{it}), along with disaster variables, fixed effects, and other controls:

$$y_{it} = \alpha_i + \delta_{st} + \lambda_1 \hat{\mathbf{z}}_{it} + \lambda_2 x_{it} + \lambda_3 x_{i,t-1} + \lambda_4 y_{i,t-4} + H'_{it} \Lambda + \lambda_5 PDD_{it} + \lambda_6 (x_{it} \times PDD_{it}) + \nu_{it}. \quad (3)$$

The second stage estimates how changes in turnout, as induced by disasters, affect incumbent vote share. Therefore, the coefficient on \hat{z}_{it} captures the *indirect* effect of disasters on vote share transmitted through turnout (participation changes), while the coefficient on the disaster variable (x_{it}) in equation (3) captures the remaining direct effect (preference shifts).

The results, reported in column 2 of Table 5, show that $\hat{\lambda}_1$ is positive and statistically significant. Because \hat{z}_{it} reflects only the portion of turnout that is driven by disaster exposure, this coefficient captures the indirect effect of disasters on incumbent vote share operating through turnout. The positive sign indicates that when disasters reduce turnout, they indirectly weaken incumbent support.¹⁶ Quantitatively, the estimates imply that a one p.p. reduction in turnout caused by disaster exposure leads to roughly a 0.081 p.p. decrease in the vote share of incumbent party. This indirect effect accounts for a nonzero but modest share of the total electoral effect of disasters, separate from their direct impact on voter

¹⁶This is because the indirect effect of disasters on vote share is given by $\frac{\partial y}{\partial z} \frac{\partial z}{\partial x}$. From the first stage, $\pi_1 = \frac{\partial z}{\partial x} < 0$; from the second stage, $\lambda_1 = \frac{\partial y}{\partial z} > 0$. Their product is therefore negative. Intuitively, higher turnout benefits the incumbent, so when disasters reduce turnout, they erode incumbent support by suppressing participation among their likely voters.

preferences.

The direct effect of disaster damage (λ_2) remains negative and statistically significant, with a magnitude (about -0.127) comparable to the reduced-form TWFE estimates. This indicates that, even after accounting for the turnout-mediated pathway, disasters exert a substantial independent negative influence on incumbent party vote share, likely reflecting shifts in voter evaluations. PDDs continue to show a positive and significant effect ($\lambda_5 > 0$), suggesting that federal aid provides a direct electoral benefit, consistent with reward-based mechanisms. The interaction term between disaster damage and PDD is also positive and significant, indicating that the electoral returns from federal disaster declarations increase with the severity of local damage.

These results reveal a dual mechanism through which disasters shape electoral accountability. First, natural disasters directly reduce the incumbent party's vote share, consistent with the "angry voter" hypothesis. Second, federal aid provided through PDDs mitigates, or partially offsets, this penalty, consistent with attentive retrospection, whereby voters reward visible and targeted relief efforts. The mediation analysis further shows that disasters primarily harm incumbents by inducing large-scale shifts in partisan preference toward the opposition rather than by discouraging turnout among their likely supporters

The two-stage framework shows that the turnout-mediated channel exists but is quantitatively modest: the indirect effect of disaster damage on incumbent vote share is roughly -0.01 p.p. ($= -0.131 \times 0.081$) per unit of damage, which is small relative to the substantial direct effect of -0.127 p.p. This comparison reinforces that shifts in voter preferences—not participation changes—constitute the primary pathway through which natural disasters depress incumbent support. The results also underscore that the political benefits of federal disaster relief are unevenly distributed: the positive electoral returns to PDDs are strongest in co-partisan and more urbanized counties, where federal assistance is more visible and residents are more dependent on government intervention. Together, these patterns highlight the strategic nature of federal disaster declarations and their role as instruments of distributive politics.

Taken together, the 2SLS results indicate that natural disasters weaken incumbent support primarily through direct shifts in voter preferences rather than through reductions in turnout. The turnout-mediated effect is present but modest, whereas the direct effect of disaster damage on vote share remains large and statistically significant. In contrast, federal relief efforts, especially when delivered under political alignment, partially offset these losses by improving voter evaluations of government responsiveness. The net electoral impact therefore reflects the balance between these opposing forces: the negative direct and small indirect effects of disaster damage, and the positive effects of federal disaster declarations. The negative coefficient on disaster damage and the positive coefficient on PDDs together support the attentive retrospection hypothesis in which incumbents are penalized when relief is absent or insufficient but rewarded when federal responses are timely, visible, and politically congruent.¹⁷

4.4 Categorical Group Analysis

The existing literature has not yet reached a consensus on whether political alignment shapes the electoral consequences of federal disaster aid. Some studies find that federal assistance benefits incumbents universally, whereas others conclude that such aid generates electoral returns only when federal and state executives are politically aligned. Motivated by this debate, this section examines whether political alignment is essential for PDDs to generate electoral gains or whether PDDs exert an independent electoral effect once alignment is held constant. Put differently, we ask whether political alignment or the declaration of federal aid is the primary driver of electoral benefits.

To address these questions, we implement a set of categorical regressions that compare electoral outcomes across discrete combinations of conditions. This approach isolates the marginal contribution of one factor, such as PDD issuance or partisan alignment, while holding the others constant. By classifying counties into mutually exclusive groups defined by disaster severity (small vs. large), PDD status (issued vs. not issued), and presiden-

¹⁷The identifying assumptions of the mediation framework - exogeneity of disaster shocks and appropriate absorption of fixed effects - are consistent with the institutional environment. Residual diagnostics indicate no remaining serial correlation beyond what is addressed by clustering at the county level.

tial-gubernatorial political alignment (aligned vs. not aligned), we can evaluate how each configuration shapes electoral responses without imposing linearity in disaster severity.¹⁸

Formally, the regression model is expressed as

$$y_{it} = \beta_2 G_2 + \beta_3 G_3 + \dots + \beta_8 G_8 + \alpha_i + \theta_t + \mathbf{H}'_{it} \Gamma + \varepsilon_{it}, \quad (4)$$

where G_h denotes group indicators representing all possible combinations of the three binary dimensions. The reference group is defined by small disaster damage, no PDD, and no political alignment. Each group is then defined by a unique combination of the three binary variables as shown below:

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 base group thus represents counties experiencing small disasters, with no PDD issuance and no partisan alignment, while Group 8 captures the opposite extreme - counties hit by large disasters, receiving a PDD, and governed by a politically aligned governor. The damage threshold separating “small” and “large” events is set at either the median (50th percentile) or the upper quartile (75th percentile) of the disaster damage distribution. Unlike continuous interaction terms (e.g., damage \times PDD \times alignment), this categorical framework allows for nonlinear and non-monotonic interactions without imposing a specific functional form.

¹⁸Significance in continuous interaction models reflects an average effect across the full sample and may obscure localized heterogeneity. In contrast, the categorical specification uncovers distinct subgroup patterns that are otherwise averaged out.

This design enables direct hypothesis testing. For example, to evaluate whether political alignment consistently matters, we compare aligned and non-aligned groups under identical disaster and PDD conditions. Likewise, to assess whether PDD occurrence itself has an independent effect, we compare PDD and non-PDD groups under the same damage and alignment conditions:

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, summarized in Table 6, reveal several key patterns. In the vote-share regressions (Columns 1–2, top panel), all categorical coefficients are negative and statistically significant, indicating that incumbent support declines in all categories relative to the base group. In other words, regardless of PDD issuance or partisan alignment, large disasters consistently reduce the incumbent’s vote share, in line with our intuition. Notably, none of the estimated coefficients for disaster-affected groups is positive, even with federal aid or political alignment, suggesting that the incumbent party never does better than in the no-disaster scenario. This indicates that while PDD and political alignment can soften the electoral blow, they cannot fully eliminate the penalty associated with disaster events.

In contrast, the turnout regressions (Columns 3–4, top panel) show that voter participation increases after disasters only in categories without political alignment, consistent with the angry-voter hypothesis that disasters mobilize voters in ways that penalize incumbent party. In contrast, turnout does not rise significantly in categories with political alignment, regardless of disaster severity or PDD issuance. This pattern suggests that political alignment is a more important determinant of turnout responses to disasters than either disaster intensity or the presence of federal aid. Notably, this role of political alignment was not evident in the baseline continuous-interaction models in Table 4. The discrepancy between the two sets of results likely reflects methodological differences: the categorical specification isolates

distinct subgroup dynamics, whereas the continuous model captures an average effect that may obscure localized heterogeneity. These findings remain robust to alternative definitions of “large” disasters, such as the median or 75th percentile thresholds.

The hypothesis tests in the lower panels of Table 6 provide additional evidence highlighting the importance of political alignment relative to PDDs. The mid-panel, which tests the role of political alignment, shows that all null hypotheses of no alignment effect are rejected at the 1% level. This indicates that presidential–gubernatorial alignment systematically shapes both incumbent vote share and turnout. Notably, alignment raises turnout but lowers incumbent vote share, implying that mobilized voters react against the incumbent, consistent with the angry-voter hypothesis. One possible explanation is that political alignment raises expectations of government performance, and unmet expectations may generate voter backlash.

By contrast, the bottom panel, which tests the independent effect of PDD issuance, reveals a less consistent pattern. The coefficients for PDD-related contrasts are relatively small, and the null of no PDD effect cannot be rejected at the 10% level in most cases, particularly in the turnout regressions. Taken together, these findings suggest that PDDs generate electoral benefits only when political alignment is present, whereas alignment itself exerts influence regardless of whether a declaration is issued. In short, political alignment appears to be a necessary condition for PDDs to translate into electoral rewards.

Overall, the categorical analysis highlights a central conclusion: political alignment—rather than PDD-driven aid alone—is the more consistent determinant of both electoral outcomes and voter participation. Although PDDs can bolster incumbent support under favorable political conditions, it is the alignment between the president and governor that systematically shapes the direction and magnitude of the electoral response. These findings are consistent with prior evidence from [Gasper \(2015\)](#) and [Reeves \(2011\)](#), underscoring that partisan coordination, rather than disaster relief by itself, drives the political returns to federal disaster declarations.

4.5 Robustness Check

In this section, we conduct several robustness checks to validate the key conclusions of this paper presented above. We begin by assessing whether the timing of disaster exposure shapes incumbent electoral support. Prior work offers two competing perspectives: some studies argue that voters respond primarily to events occurring shortly before the election (Fair, 1996), whereas others contend that electoral outcomes reflect a cumulative evaluation of the incumbent party’s performance since the previous election (Choi et al., 2025a). To adjudicate between these views, we distinguish disasters that occur during the election year from those occurring in the three preceding off-election years.

To be specific, we re-estimate equation (1) after decomposing total disaster damages into four periods relative to each election: the election year itself and the three prior non-election years. As shown in Table 7, only disasters occurring during the election year exert a statistically significant influence on incumbent vote share; damages sustained in earlier years have no detectable effects. In short, natural disasters matter electorally when an election is imminent but fade from consideration over longer horizons. This result also parallels Henkel et al. (2024), who find that U.S. counties struck by on-cycle hurricanes - those occurring within a year of election day - see increases in local public goods and services, whereas counties hit by major hurricanes more than a year before an election experience declines.

We also assess whether our main results are sensitive to specific election cycles that may have featured unusually strong voter reactions or atypically high disaster activity. Figure 5 presents a leave-one-out robustness exercise in which we re-estimate the TWFE model while sequentially omitting each election cycle. The horizontal axis indicates the excluded election year. Across all iterations, the estimated disaster coefficients remain negative and statistically significant, with the 95% confidence intervals lying entirely below zero. This stability confirms that our results are not driven by any single election cycle.

Finally, we examine whether disaster effects differ in elections without an incumbent presidential candidate - specifically, the 2000, 2008, and 2016 open-seat contests. As reported in

Table 8, the negative impact of disaster damage on the incumbent party’s vote share is not only statistically significant, but also more pronounced in these elections than in races where an incumbent president was seeking reelection. Notably, the positive effect of PDD issuance is also stronger in open-seat elections, suggesting that when no sitting president appears on the ballot, disaster relief efforts become a more salient basis for voters’ assessments of the incumbent party’s performance.

5 Concluding Remarks

It has long been recognized that natural disasters can exert significant political consequences (e.g., Lindell and Prater (2003); Bechtel and Hainmueller (2011); Cole et al. (2012); Beg (2019); Eisensee and Strömberg (2007); Povitkina et al. (2025)). This study provides novel empirical evidence on how such events influence U.S. presidential elections by examining their effects on both incumbent-party vote share and voter turnout. Drawing on county-level data from more than 3,000 counties across seven presidential elections between 1996 and 2020, we combine insights from retrospective voting and distributive politics to assess how disaster severity, federal disaster declarations and aid, and partisan alignment jointly shape electoral outcomes.

Our findings reveal several key patterns. First, natural disasters weaken the support for the incumbent party, especially in areas that experience greater damage. This pattern aligns with the concept of blind retrospection, in which voters penalize incumbents for adverse events regardless of their direct responsibility. Second, federal disaster relief delivered through Presidential Disaster Declarations (PDDs) softens this electoral penalty, lending support to the attentive retrospection hypothesis. Yet, this moderating effect is uneven - more pronounced in politically aligned and more urbanized counties, highlighting that the electoral returns to federal aid depend critically on partisan context and the local salience of relief efforts.

Third, disasters appear to influence elections more through shifts in voter preferences than through changes in turnout. The evidence points to some movement toward the opposition

rather than large declines in participation, particularly among voters who typically support the incumbent. This pattern suggests that the political consequences of disasters may arise more from incremental preference changes than from widespread demobilization.

Taken together, our findings show that the political consequences of natural disasters are both conditional and asymmetric. Although disasters typically reduce support for the incumbent party, timely and visible federal relief can partially offset these losses, in particular when delivered in contexts of partisan alignment between state and federal governments. These results highlight how environmental shocks, partisan alignment, and governmental responsiveness jointly shape democratic accountability in an era of rising climate risk.

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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
Blind retrospection hypothesis	decrease	-
Attentive retrospection hypothesis	ambiguous	ambiguous
Federal aid	increase	increase
No federal aid	decrease	decrease
Partisan effect hypothesis	ambiguous	ambiguous
Co-partisan	increase	increase
Contra-partisan	decrease	decrease
Financial distress hypothesis	-	decrease
Distributive spending hypothesis	increase	increase

Table 2: Summary of selected studies

Study	Main findings	Data
Achen and Bartels (2016)	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 (Baseline regression results)

Explanatory variables	Model 1	Model 2	Model 3	Model 4
Disaster damage (x_{it})	-0.118‡[0.020]	-0.130‡[0.022]	-0.129‡[0.022]	-0.101‡[0.032]
Lagged disaster damage ($x_{i,t-1}$)	0.024 [0.016]	0.035* [0.019]	0.035* [0.019]	0.036* [0.019]
Previous vote share change ($y_{i,t-4}$)		-0.167‡[0.015]	-0.165‡[0.015]	-0.166‡[0.015]
PDD_{it}	0.760‡[0.118]	0.739‡[0.125]	0.715‡[0.125]	0.713‡[0.125]
$x_{it} \times PDD_{it}$	0.102‡[0.012]	0.094‡[0.013]	0.076‡[0.015]	0.076‡[0.015]
$x_{it} \times PDD \times$ Co-partisan			0.025†[0.012]	0.025†[0.012]
$x_{it} \times PDD \times$ Pop density			0.005* [0.003]	0.005* [0.003]
$x_{it} \times PDD \times D_{Rep}$				-0.032 [0.036]
$x_{it} \times PDD \times D_{Dem}$				-0.124†[0.051]
$x_{it} \times PDD \times D_{Swing}$				0.036 [0.050]
Per capita income growth	0.057‡[0.008]	0.062‡[0.009]	0.062‡[0.009]	0.062‡[0.009]
Population density growth	0.069†[0.031]	0.062 [0.039]	0.060 [0.039]	0.059 [0.039]
Unemployment rate change	-0.146‡[0.035]	-0.245‡[0.047]	-0.234‡[0.048]	-0.235‡[0.048]
Change in share of Black pop.	-0.069‡[0.025]	-0.041 [0.036]	-0.040 [0.036]	-0.040 [0.036]
Change in share of Hispanic pop.	-0.053†[0.025]	-0.083†[0.038]	-0.079†[0.038]	-0.079†[0.038]
Change in share of college grads.	0.121‡[0.022]	0.136‡[0.027]	0.135‡[0.027]	0.135‡[0.027]
Change in veteran population	0.140‡[0.039]	0.198‡[0.043]	0.197‡[0.043]	0.196‡[0.043]
Change in population density	-2.970‡[0.495]	-3.184‡[0.661]	-3.250‡[0.670]	-3.207‡[0.666]
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (θ_t)	Yes	Yes	Yes	Yes
R-squared (within)	0.754	0.721	0.721	0.721

Notes: The TWFE regression equation is

$$y_{it} = \alpha_i + \delta_{st} + \beta_1 x_{it} + \beta_2 x_{i,t-1} + \beta_3 y_{i,t-4} + \mathbf{H}'_{it} \Gamma + \varepsilon_{it},$$

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 h_{it} the change in four-year growth rates of house prices before election. X_{it} denotes the change in a vector of control variables describing the state of the county economy. We arrange them into two sets: (i) fundamental local economic factors, such as per capita income, population density, and unemployment rate; and (ii) socio-demographic characteristics that are important for any model of voting behavior. Regression models are estimated using standard errors clustered at the county level. ‡, † and asterisk (*) respectively indicate statistical significance at the 1%, 5%, and 10% significance levels with the corresponding clustered s.e. in square brackets. The number of counties for which all data fields are populated is 3,016 for presidential election.

Table 4: Impacts of natural disasters on voter turnout (Baseline regression results)

Explanatory variables	Model 1	Model 2	Model 3	Model 4
Disaster damage (x_{it})	0.032 [0.062]	-0.138* [0.083]	-0.131* [0.074]	-0.118 [0.116]
Lagged disaster damage ($x_{i,t-1}$)	0.158 [0.160]	-0.036 [0.052]	-0.036 [0.052]	-0.036 [0.052]
Previous voter turnout ($y_{i,t-4}$)		-0.877‡[0.180]	-0.877‡[0.180]	-0.878‡[0.180]
PDD_{it}	0.007 [0.161]	0.341 [0.262]	0.313 [0.263]	0.302 [0.262]
$x_{it} \times PDD_{it}$	-0.014 [0.014]	0.033 [0.023]	0.023 [0.025]	0.022 [0.025]
$x_{it} \times PDD \times$ Co-partisan			0.010 [0.018]	0.010 [0.018]
$x_{it} \times PDD \times$ Pop density			0.006 [0.007]	0.006 [0.007]
$x_{it} \times PDD \times D_{Rep}$				-0.115 [0.121]
$x_{it} \times PDD \times D_{Dem}$				-0.044 [0.138]
$x_{it} \times PDD \times D_{Swing}$				0.279* [0.153]
Per capita income growth	-0.011 [0.039]	-0.042 [0.034]	-0.042 [0.034]	-0.040 [0.034]
Population density growth	-0.301‡[0.108]	-0.284‡[0.132]	-0.285‡[0.132]	-0.289‡[0.132]
Unemployment rate change	0.606‡[0.135]	0.747‡[0.206]	0.751‡[0.207]	0.756‡[0.207]
Change in share of Black pop.	0.226 [0.235]	0.141 [0.235]	0.140 [0.235]	0.140 [0.235]
Change in share of Hispanic pop.	-0.298‡[0.073]	-0.401 [0.252]	-0.396 [0.251]	-0.396 [0.251]
Change in share of college grads.	0.336‡[0.070]	0.529‡[0.163]	0.530‡[0.163]	0.529‡[0.162]
Change in veteran population	1.003* [0.607]	0.896‡[0.240]	0.895‡[0.240]	0.891‡[0.239]
Change in population density	-7.673‡[1.315]	-11.189‡[2.446]	-11.175‡[2.393]	-11.186‡[2.407]
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (θ_t)	Yes	Yes	Yes	Yes
R-squared (within)	0.229	0.702	0.702	0.702

Notes: The TWFE regression equation is

$$y_{it} = \alpha_i + \theta_t + \beta_1 x_{it} + \beta_2 x_{i,t-1} + \beta_3 y_{i,t-4} + X'_{it} \Gamma + \varepsilon_{it},$$

where y_{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: Two-stage regression results on voter turnout and incumbent vote share change

Explanatory variables	First Stage	Second stage
	Voter turnout (z_{it})	Vote share change (y_{it})
Disaster damage (x_{it})	-0.131* [0.074]	-0.127‡[0.027]
Lagged disaster damage ($x_{i,t-1}$)	-0.036 [0.052]	0.040‡[0.019]
Expected voter turnout (\hat{z}_{it})		0.081‡[0.024]
Previous voter turnout ($z_{i,t-4}$)	-0.877‡[0.180]	0.084‡[0.018]
Previous vote share change ($y_{i,t-4}$)		-0.171‡[0.015]
PDD	0.313 [0.263]	0.231‡[0.073]
$x_{it} \times PDD$	0.023 [0.025]	0.020‡[0.009]
$x_{it} \times PDD \times$ Pop density	0.006 [0.007]	0.020‡[0.009]
$x_{it} \times PDD \times$ Co-partisan	0.010 [0.018]	0.012‡[0.004]
Economic control variables	Yes	Yes
Demographic control variables	Yes	Yes
County fixed effect (α_i)	Yes	Yes
State-by-Year fixed effect (θ_t)	Yes	Yes
Adjusted R-squared	0.702	0.722

Notes: The first state regression equation is

$$z_{it} = \alpha_i + \delta_{st} + \pi_1 x_{it} + \pi_2 x_{i,t-1} + \pi_3 z_{i,t-4} + H'_{it} \Pi + \pi_4 PDD_{it} + \pi_5 (x_{it} \times PDD_{it}) + \omega_{it},$$

and the second stage regression is

$$y_{it} = \alpha_i + \delta_{st} + \lambda_1 \hat{z}_{it} + \lambda_2 x_{it} + \lambda_3 x_{i,t-1} + \lambda_4 y_{i,t-4} + H'_{it} \Lambda + \lambda_5 PDD_{it} + \lambda_6 (x_{it} \times PDD_{it}) + \nu_{it},$$

where \hat{z}_{it} denotes the fitted value (instrumented turnout) from the first stage. Refer to the footnote accompanying Table 3 for descriptions of all remaining variables.

Table 6: Impacts of group classifications on vote share change and voter turnout

Explanatory variables	Vote share change		Voter turnout	
	Median	75%	Median	75%
Group 2 (β_2)	-0.455‡[0.132]	-0.410‡[0.110]	-1.009‡[0.474]	-0.088 [0.390]
Group 3 (β_3)	-1.841‡[0.426]	-1.873‡[0.422]	15.586‡[1.115]	15.929‡[1.215]
Group 4 (β_4)	-2.025‡[0.408]	-1.818‡[0.402]	15.777‡[1.042]	16.175‡[1.138]
Group 5 (β_5)	-0.257‡[0.078]	-0.284‡[0.079]	-0.469 [0.395]	-0.037 [0.258]
Group 6 (β_6)	-0.413‡[0.109]	-0.360‡[0.111]	0.272 [0.502]	0.498 [0.523]
Group 7 (β_7)	-1.834‡[0.425]	-1.798‡[0.423]	16.083‡[1.337]	16.582‡[1.521]
Group 8 (β_8)	-1.575‡[0.402]	-1.595‡[0.400]	15.537‡[1.035]	15.711‡[1.126]
Economic control variables	Yes	Yes	Yes	Yes
Demographic control variables	Yes	Yes	Yes	Yes
County fixed effect (α_i)	Yes	Yes	Yes	Yes
State-by-Year fixed effect (θ_{st})	Yes	Yes	Yes	Yes
R-squared (within)	0.759	0.759	0.258	0.258
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‡[0.001]	-0.410‡[0.110]	-1.009‡[0.474]	-0.088 [0.390]
$H_0 : \beta_4 - \beta_3 = 0$	-0.184 [0.268]	0.054 [0.146]	0.190 [0.376]	0.246 [0.392]
$H_0 : \beta_6 - \beta_5 = 0$	-0.155 [0.109]	-0.076 [0.110]	0.741 [0.819]	0.535 [0.623]
$H_0 : \beta_8 - \beta_7 = 0$	0.260* [0.052]	0.203 [0.144]	-0.545 [0.481]	-0.871 [0.569]

Notes: The TWFE regression equation is

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

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. 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 7: Robustness Check for the Effect of Disaster Timing on Vote Share Change

Explanatory variables	Model 1	Model 2	Model 3	Model 4	Model 5
Disaster damage (x_{it})	-0.121‡[0.026]	-0.100‡[0.019]			
$x_{i,t-1}$	0.032 [0.022]		0.028 [0.019]		
$x_{i,t-2}$	0.000 [0.021]			0.011 [0.019]	
$x_{i,t-3}$	-0.032 [0.021]				-0.024 [0.018]
$y_{i,t-4}$	-0.174‡[0.019]	-0.182‡[0.014]	-0.169‡[0.015]	-0.177‡[0.015]	-0.189‡[0.017]
PDD_{it}	0.810‡[0.141]	0.579‡[0.080]	0.310‡[0.103]	0.375‡[0.101]	0.353‡[0.075]
$x_{it} \times PDD_{it}$	0.095‡[0.014]	0.078‡[0.011]	0.048‡[0.010]	0.055‡[0.011]	0.047‡[0.010]
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 (θ_t)	Yes	Yes	Yes	Yes	Yes
R-squared (within)	0.719	0.726	0.719	0.724	0.724

Table 8: Impacts of disasters on electoral outcomes for election cycles without incumbent presidents

Explanatory variables	All sample	w/o incumbents
Disaster damage (x_{it})	-0.098‡[0.017]	-0.171‡[0.039]
PDD	0.667‡[0.078]	0.983‡[0.327]
$x_{it} \times PDD$	0.090‡[0.011]	0.146‡[0.036]
Economic control variables	Yes	Yes
Demographic control variables	Yes	Yes
County fixed effect (α_i)	Yes	Yes
State-by-Year fixed effect (θ_t)	Yes	Yes
Adjusted R-squared	0.755	0.588

Notes: The regression equation is

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

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

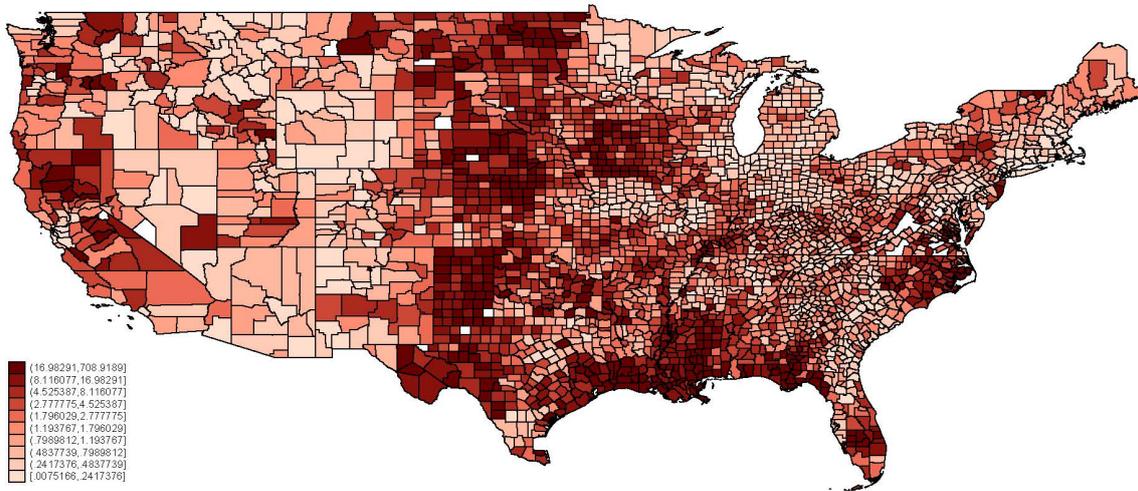


Figure 1: Average total damage per income by county (1994—2020)

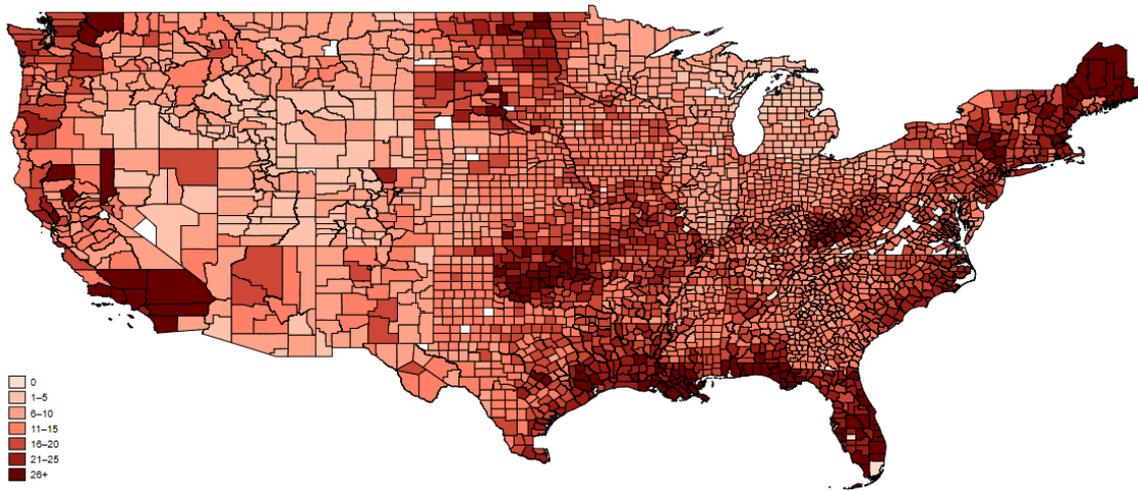


Figure 2: Total issuances of TDD among 3,016 counties

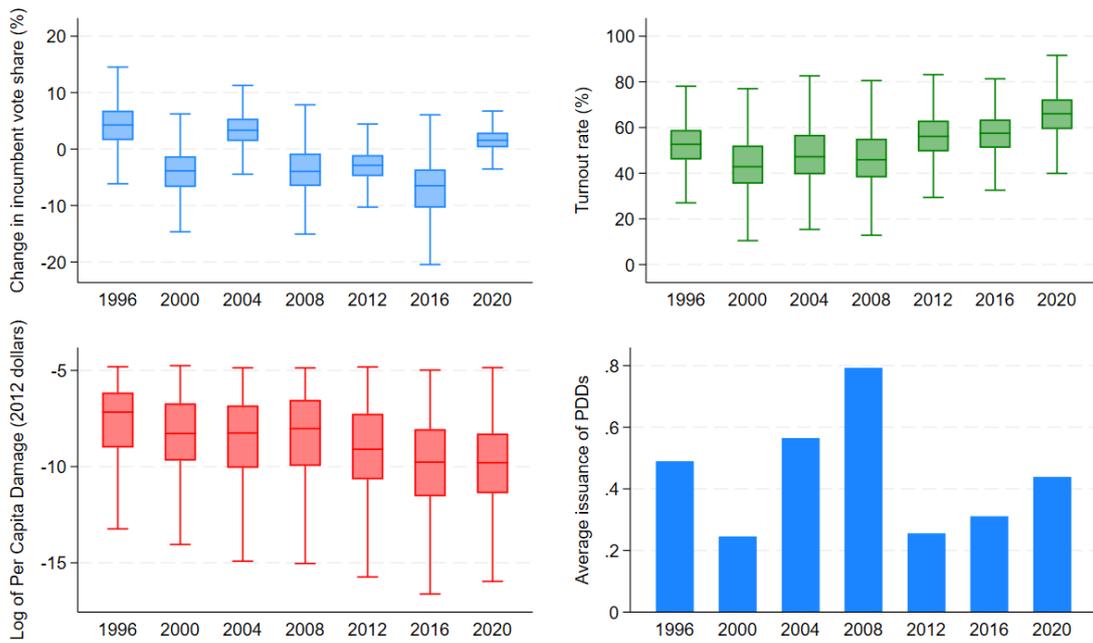


Figure 3: Boxplots of change in incumbent vote share (top left), voter turnout rate (top right), log of per capita damage amount (bottom left), and average issuance of PDDs (bottom right) for each Presidential election cycle

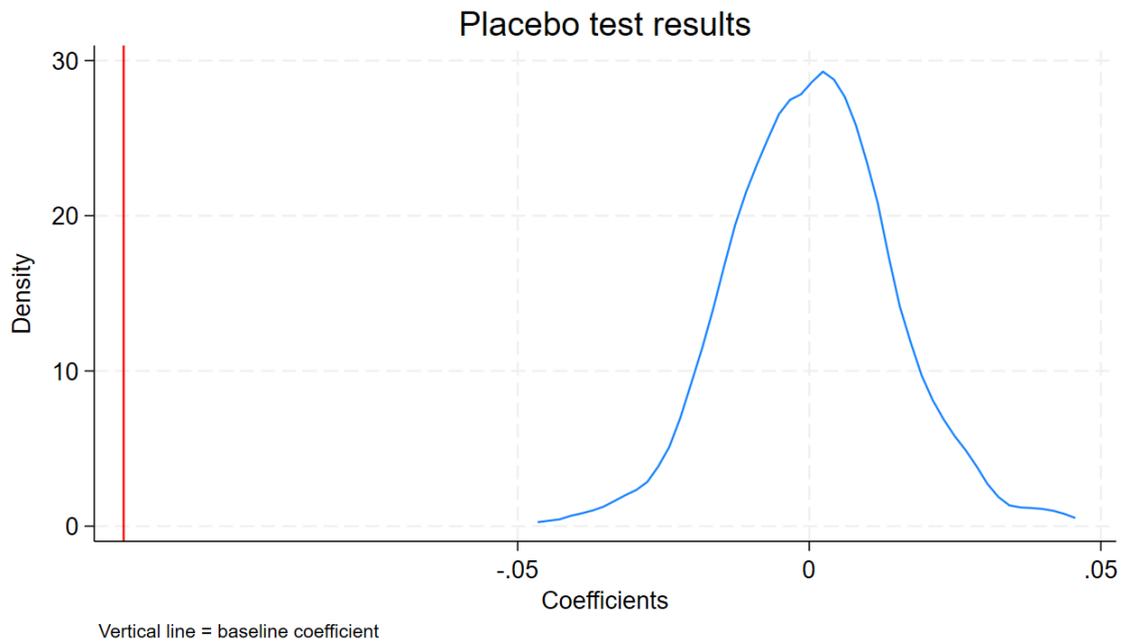


Figure 4: Placebo test results on the impact on vote share

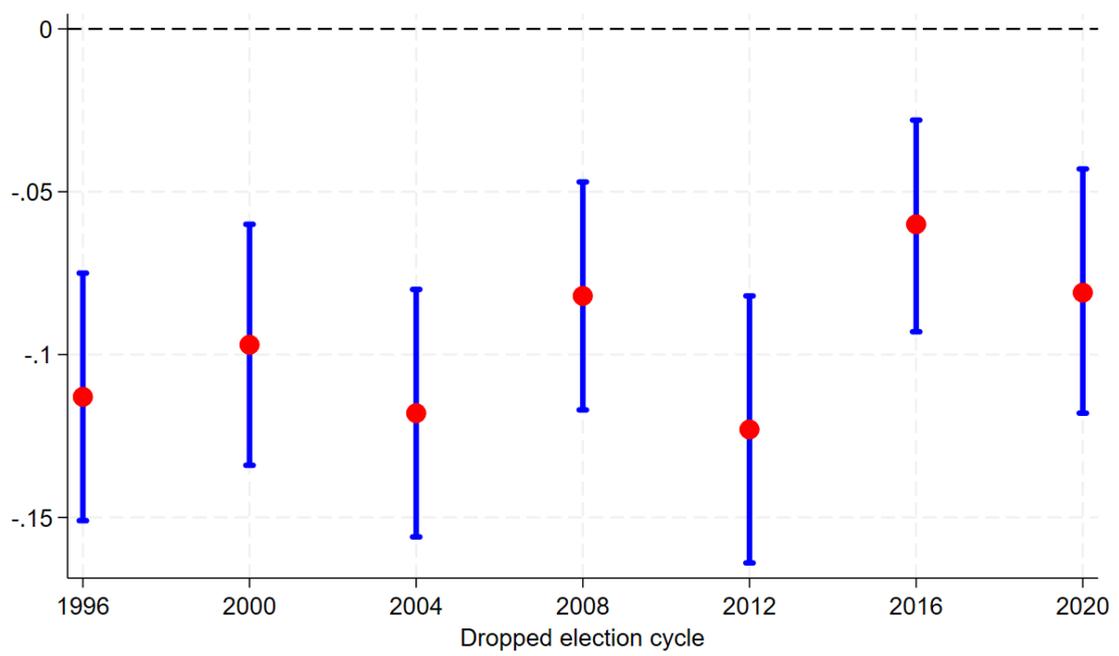


Figure 5: Impacts of disaster without specific election cycles

Appendix

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 (2022 USD)	SHELDUS
Crop damage	Total crop damage caused by natural disasters, inflation-adjusted (2022 USD)	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
Share of veteran	Fraction of population who are veterans	Census Bureau
Digitization rate	Average share of households with broadband Internet subscription for the period 2008-2020	Census Bureau

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.

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