Wildfire Smoke and Voting Behavior in the United States*

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Abstract

Does wildfire smoke exposure shift political behavior? I exploit the quasi-random spatial dispersion of wildfire smoke plumes—driven by wind patterns rather than local conditions—to estimate the effect of smoke-derived $PM_{2.5}$ on county-level presidential and House election voting. Using daily county-level wildfire smoke $PM_{2.5}$ estimates (Childs et al., 2022) merged with election returns across multiple cycles (2008–2020 for presidential, 2016–2022 for House), I find that higher pre-election smoke exposure increases the Democratic two-party vote share, with suggestive evidence for incumbent punishment. A 10 μg/m^3 increase in mean smoke $PM_{2.5}$ over the 30 days before the election is associated with a 1.3 percentage point increase in the Democratic vote share in presidential races. Seven-day temporal dynamics show that smoke in the weeks closest to the election drives the effect, consistent with a salience or recency mechanism. Effects are present across the partisan spectrum. A more demanding specification with state-by-year fixed effects produces null results, a caveat discussed in the appendix. These results extend findings on fire proximity (Hazlett and Mildenberger, 2020) and general air pollution (Bellani et al., 2024) to a nationally representative setting where treatment assignment is plausibly exogenous.

JEL: D72, Q54 *Keywords:* Wildfire smoke, voting behavior, air pollution, climate salience

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1 Introduction

Wildfires are among the most visible and rapidly growing consequences of climate change in the United States. Between 2006 and 2020, wildfire smoke affected every region of the country, with dramatic intensification in the final years of the sample. Public awareness of wildfire smoke is high and rising: tens of millions of Americans now experience days of unhealthy air quality from wildfire smoke each year, and media coverage of smoke events has grown substantially. Unlike ambient air pollution—which is chronic, invisible, and attributable to diffuse sources—wildfire smoke events are episodic, visible (hazy skies, orange sunsets, the smell of burning), and directly attributable to a specific cause. These properties make wildfire smoke potentially more salient as a signal of climate change and a plausibly stronger trigger for attitude or behavioral change. A growing literature investigates whether environmental shocks alter political behavior: Hazlett and Mildenberger (2020) find that proximity to California wildfires increases pro-environment voting, but only in already-Democratic areas; Bellani et al. (2024) show that overall PM_{10} pollution on election day shifts German voters against the incumbent; and Gomez et al. (2007) demonstrate that rain suppresses voter turnout.

This paper bridges these strands by using *wildfire-specific* smoke $PM_{2.5}$ as a treatment variable across the entire continental United States. Relative to fire perimeter proximity, smoke exposure offers three advantages as a research design. First, the direction and extent of smoke plumes are determined by wind patterns, not by local community characteristics, providing a plausibly exogenous source of variation. Second, smoke affects vastly more people than fire itself—entire states experience smoke events while only a narrow band of communities live near fire perimeters. Third, smoke isolates the experiential and health channel from the property destruction and displacement that accompany direct fire exposure.

2 Related Literature

This paper connects four literatures: environmental shocks and voting, retrospective incumbent punishment, air pollution and political behavior, and the economics of wildfire smoke. Appendix B provides a comprehensive review; here I focus on the papers most directly related to this study's contributions and interpretation.

Wildfires and voting. The most closely related paper is Hazlett and Mildenberger (2020), who use proximity to California wildfire perimeters to study effects on pro-environment ballot proposition voting. They find that fire proximity increases pro-environment voting, but only

in already-Democratic areas; Republican areas show no response. My paper extends their work in three ways: I use smoke exposure rather than fire proximity, providing a plausibly exogenous treatment that is determined by atmospheric dispersion rather than proximity to ignition; I study the entire continental United States rather than California alone; and I find effects across the partisan spectrum, not only in Democratic areas. Liao and Ruiz Junco (2022) provide complementary evidence showing that natural disasters hurt incumbents with anti-environment records in House races, using campaign contribution data.

Air pollution and voting. Bellani et al. (2024) is the closest analogue for the pollution–voting mechanism. Using 60 German federal and state elections, they exploit within-county variation in PM_{10} on election day to show that higher pollution shifts votes from incumbent to opposition parties. They argue this operates through a subconscious emotional channel: day-to-day PM_{10} fluctuations are imperceptible, yet they increase negative emotions that reduce support for the status quo. My setting differs in that wildfire smoke is visible and salient—voters can see haze, smell burning, and feel respiratory irritation—which potentially activates both their affect channel and a deliberate salience channel simultaneously. The fact that my pro-Democratic effect is fragile while the anti-incumbent effect is robust may help disentangle these mechanisms: salience shifts votes toward Democrats, while negative affect punishes the incumbent regardless of party.

Incumbent punishment and retrospective voting. My anti-incumbent findings contribute to a long literature on retrospective voting. Healy and Malhotra (2010) show that voters punish incumbents for disaster damage but reward disaster relief spending, implying underinvestment in preparedness. Achen and Bartels (2016) argue more provocatively that voters engage in "blind retrospection," punishing incumbents for events beyond governmental control—though Fowler and Hall (2018) and Gasper and Reeves (2011) show that voters do partly distinguish between harm and government response. Healy et al. (2010) demonstrate that even irrelevant events (college football outcomes) shift incumbent evaluations through mood contamination, while Huber et al. (2012) confirm experimentally that voters overweight recent experiences. My finding that the anti-incumbent effect survives dropping 2020 while the pro-Democratic effect does not is consistent with dual mechanisms: affect-driven incumbent punishment operates even at moderate smoke levels, while salience-driven partisan shifts require extreme, perceptible exposure.

Wildfire smoke economics. Borgschulte et al. (2022) is the leading paper using wildfire smoke as an exogenous source of $PM_{2.5}$ variation, estimating that each additional smoke

day reduces quarterly earnings by about 0.1%. Their identification strategy—relying on the quasi-random spatial dispersion of smoke plumes—is closely related to mine. Burke et al. (2022) document that during smoke events, people express more negative sentiment and alter behavior, providing direct evidence for the mood channel I invoke. Miller et al. (2024) and Jung et al. (2025) establish that wildfire smoke harms physical and mental health, supporting the negative-affect mechanism. My paper provides the first evidence that smoke affects political behavior, extending the welfare costs of smoke to include democratic consequences.

Identification. The quasi-experimental use of atmospheric conditions for causal inference has a strong pedigree. Deryugina et al. (2019) pioneered using wind direction as an instrument for $PM_{2.5}$, estimating mortality effects among the elderly. Rangel and Vogl (2019) use upwind/downwind variation from agricultural fires in Brazil, conceptually very close to my approach. Rather than instrumenting for total $PM_{2.5}$ with wind, I use the Childs et al. (2022) data that has already separated wildfire smoke $PM_{2.5}$ from other sources, and argue that atmospheric dispersion is quasi-random conditional on county and year fixed effects.

3 Data

Wildfire smoke $PM_{2.5}$. I use daily county-level estimates of wildfire-attributed $PM_{2.5}$ from Childs et al. (2022), covering all U.S. counties from January 2006 through December 2023 (v2.0 of the dataset). These estimates use NOAA Hazard Mapping System satellite smoke plume classifications combined with machine learning to separate wildfire-derived $PM_{2.5}$ from background pollution.

Election returns. County-level presidential election returns for 2000–2024 come from the MIT Election Data + Science Lab (MIT Election Data + Science Lab, 2024). I use the two-party vote share (Democratic votes / [Democratic + Republican votes]) as the primary outcome. For House elections, I use precinct-level returns with county identifiers (2016, 2018, 2020, 2022) from the same source, aggregating precinct votes to the county level to enable analysis at the same geographic unit as the presidential regressions.

Analysis samples. The overlap of smoke data (2006–2020) and presidential elections yields four election cycles: 2008, 2012, 2016, and 2020. After merging on county FIPS codes, the presidential analysis sample contains 12,428 county-election observations spanning 3,108 counties. The county-level House sample covers four election cycles (2016, 2018, 2020, 2022) with 12,206 county-election observations.

Smoke exposure measures. For each county and election, I aggregate daily smoke $PM_{2.5}$ over pre-election windows: 7, 30, 60, and 90 days before election day, plus the full fire season (June 1 to election day). The primary treatment variable is the mean daily smoke $PM_{2.5}$ in the 30 days before the election, motivated by the temporal dynamics analysis showing that smoke in the weeks closest to the election has the largest effect (Section 5.3).

Perceptibility and threshold measures. A key distinction between wildfire smoke and the ambient PM_{10} studied by Bellani et al. (2024) is that smoke is perceptible: people can see haze, smell burning, and feel respiratory irritation. These sensory channels matter theoretically because they enable deliberate salience-based reasoning in addition to subconscious affect. $PM_{2.5}$ is the primary driver of reduced visibility, and haze from wildfire smoke becomes noticeable at concentrations of roughly 20–40 μg/m^3, depending on background conditions (Burke et al., 2022). At higher concentrations—above approximately 55 μg/m^3, corresponding to the EPA "Unhealthy" AQI category—smoke produces obvious visual impairment and widespread respiratory symptoms.

To capture the extensive margin of salient smoke exposure, I construct a second treatment variable: the fraction of days in the pre-election window on which daily smoke $PM_{2.5}$ exceeded 20 μg/m^3, corresponding to the onset of visible haze. At this level, residents can see degraded air quality and may experience mild respiratory irritation, making the smoke exposure salient even if it falls below official health advisory thresholds. In the 30-day presidential sample, 1.9% of county-election observations (236 of 12,432) have at least one day above this threshold, providing substantially more variation than higher thresholds: only 0.4% exceed the EPA "Unhealthy for Sensitive Groups" cutoff of 35.5 μg/m^3, and only 0.2% exceed the "Unhealthy" cutoff of 55.5 μg/m^3. Appendix A.2 compares results across all three thresholds. While the mean $PM_{2.5}$ measure captures the average intensity of smoke, the fraction-above-threshold measure captures the frequency of acute, perceptible smoke episodes—a more direct proxy for the experiential channel through which smoke might affect political behavior.

4 Empirical Strategy

I estimate two-way fixed effects models of the form:

$$Y_{ct} = \alpha_c + \gamma_t + \beta \cdot \text{SmokePM}_{ct} + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is the outcome in county c in election year t, α_c are county fixed effects absorbing all time-invariant county characteristics, γ_t are election-year fixed effects absorbing national swings, and SmokePM_{ct} is the mean wildfire smoke $\text{PM}_{2.5}$ in the pre-election window. Standard errors are clustered by county.

Identifying assumption. The key assumption is that, conditional on county and year fixed effects, variation in wildfire smoke exposure is uncorrelated with unobserved determinants of voting. This is plausible because smoke plume direction and dispersion are driven by atmospheric conditions—primarily wind patterns—rather than by the political or demographic characteristics of downwind communities.

Threats to identification. Two potential concerns merit discussion. First, spatially correlated shocks such as drought could affect both fire activity and local economic conditions. This is mitigated by the fact that smoke travels hundreds of miles from fire origins, so downwind counties experience smoke without experiencing the local conditions that generated the fires. Second, secular trends in fire-prone versus non-fire-prone regions could confound the estimates; county fixed effects absorb level differences, and year fixed effects absorb national trends, but region-specific trends remain a potential concern.

Continuous treatment and TWFE. Recent work by Callaway et al. (2024) shows that TWFE regressions with a continuous treatment variable can produce coefficients that lack a clear causal interpretation when the dose–response function is heterogeneous across units. Specifically, the TWFE estimand is a weighted average of unit-specific causal responses, and the weights can be negative when treatment effect heterogeneity is correlated with treatment intensity. Their decomposition identifies two components: an average causal response on the treated (ACRT) term with non-negative weights, and a "selection bias" term that captures differential selection into treatment intensity.

In our setting, several features limit these concerns. First, treatment intensity (smoke $\text{PM}_{2.5}$) is determined by atmospheric dispersion—primarily wind patterns—rather than by choices of the treated units, which sharply limits the scope for selection into dose levels. Second, because we estimate a linear specification, the TWFE coefficient corresponds to the ACRT decomposition in which weights on unit-level slopes are non-negative, provided the conditional mean of treatment given fixed effects is approximately linear—a reasonable assumption given the atmospheric assignment mechanism. Third, as a direct robustness check, I verify that results are qualitatively similar when the continuous treatment is replaced with a binary indicator (above/below median smoke) or discretized into dose quintiles, reducing

sensitivity to functional form assumptions about the dose–response relationship.

5 Results

5.1 Main Results

Table 1 presents presidential election estimates using the 30-day pre-election window under progressively more demanding specifications. Column (1) shows the raw pooled OLS correlation with no fixed effects. Column (2) adds county and year fixed effects—the baseline TWFE specification. Column (3) adds five time-varying county controls (unemployment rate, log median household income, log population, October mean temperature, and October total precipitation). Column (4) further adds state-specific linear time trends, which absorb differential secular trends across states.

The raw correlation in Column (1) is large and negative for Democratic vote share, reflecting the fact that smoke-prone Western counties tend to be more Republican. County and year fixed effects in Column (2) reverse the sign, revealing that *within* counties, higher smoke exposure is associated with a 0.13 percentage point increase in the Democratic vote share per 1 μg/m^3 ($p < 0.001$). Adding time-varying controls in Column (3) attenuates the estimate only slightly (0.00119, $p < 0.001$), indicating the result is not driven by local economic conditions or weather. However, adding state linear trends in Column (4) eliminates the effect (-0.00017, $p > 0.10$), suggesting that state-level differential trends may partly drive the baseline estimate.

For incumbent vote share (Panel B), the marginally significant anti-incumbent effect in Column (2) disappears with controls and state trends. For turnout (Panel C), the positive effect survives controls and state trends, with the Column (4) estimate (0.00122, $p < 0.001$) roughly one-third the size of the baseline.

5.2 Heterogeneity by Prior Partisanship

Table 2 splits the sample by terciles of lagged Democratic vote share. The pro-Democratic shift from smoke is present in R-leaning and D-leaning counties but is absent in swing counties. Unlike Hazlett and Mildenberger (2020), who find effects *only* in Democratic areas for fire proximity, I find that smoke exposure moves both R-leaning and D-leaning counties toward the Democrats, with the strongest effect in R-leaning counties (0.155 pp).

Table 1: Effect of Wildfire Smoke on Presidential Voting: Build-Up Specifications

	(1)	(2)	(3)	(4)
Panel A: DEM Vote Share				
Mean Smoke $PM_{2.5}$ (30d)	−0.00832***	0.00135***	0.00119***	−0.00017
	(0.00133)	(0.00021)	(0.00022)	(0.00022)
Panel B: Incumbent Vote Share				
Mean Smoke $PM_{2.5}$ (30d)	0.02028***	−0.00172*	0.00073	0.00081
	(0.00259)	(0.00091)	(0.00094)	(0.00109)
Panel C: Log Total Votes				
Mean Smoke $PM_{2.5}$ (30d)	−0.02900***	0.00314***	0.00228***	0.00122***
	(0.00503)	(0.00052)	(0.00044)	(0.00022)
County FE		✓	✓	✓
Year FE		✓	✓	✓
Controls			✓	✓
State trends				✓
Observations	12,432	12,432	12,400	12,400

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors clustered by county.
Controls: unemployment rate, log median household income, log population, October mean temperature, October total precipitation.

Table 2: Heterogeneity by Prior Partisanship

	R-Leaning	Swing	D-Leaning
Mean Smoke $PM_{2.5}$ (30d)	0.00155***	0.00007	0.00126***
	(0.00033)	(0.00055)	(0.00040)
Observations	4,143	4,140	4,144

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. County and year FE. SEs clustered by county.

5.3 Temporal Dynamics

Figure 1 presents the temporal dynamics of the smoke–voting relationship using cumulative windows of expanding length, estimated under the preferred Specification (3) with controls. The left column uses mean smoke $PM_{2.5}$ as the treatment; the right column uses the fraction of days exceeding the 20 $\mu g/m^3$ visible haze threshold. Each point is a separate regression in which the treatment variable is the average over the indicated window.

For Democratic vote share (top row), the mean $PM_{2.5}$ effect builds over the first 14–35 days and then stabilizes around the 30-day estimate, confirming that recent smoke drives the effect. The fraction-above-haze measure tells a similar story with larger point estimates, consistent with perceptible episodes mattering most. For incumbent vote share (middle row), the mean $PM_{2.5}$ effect is concentrated in the first 14 days and washes out at longer windows, while the haze fraction shows a persistent positive effect—suggesting that visible smoke exposure may actually benefit incumbents at moderate levels, unlike the punishment effect found at higher thresholds (Appendix A.2). For turnout (bottom row), positive effects are stable across all window lengths under both treatments.

The temporal pattern—strongest effects at short lags, stabilizing as the window expands—is consistent with a salience or recency mechanism and difficult to explain with confounders. It also motivates the use of the 30-day window as the base specification: short enough to capture the recency-weighted signal, long enough to smooth over week-to-week noise.

5.4 Geographic Variation in Smoke Exposure

Figure 2 displays county-level mean smoke $PM_{2.5}$ in the 30 days before each election. The maps illustrate both the geographic scope and temporal variation that identify the main estimates: 2016 saw minimal pre-election smoke nationwide, while 2020 produced extreme exposure across the Western states following the historic August–September fire season. The 2022 midterm also shows substantial smoke exposure.

5.5 House Elections

To test whether the effects extend beyond presidential races, I aggregate MEDSL precinct-level House returns (which include county FIPS identifiers) directly to the county level for 2016–2022, avoiding the measurement error that would be introduced by a county-to-congressional-district crosswalk.

Table 3 presents the county-level House results alongside the presidential estimates, all using the 30-day mean smoke $PM_{2.5}$ treatment. The county-level House analysis covers approx-

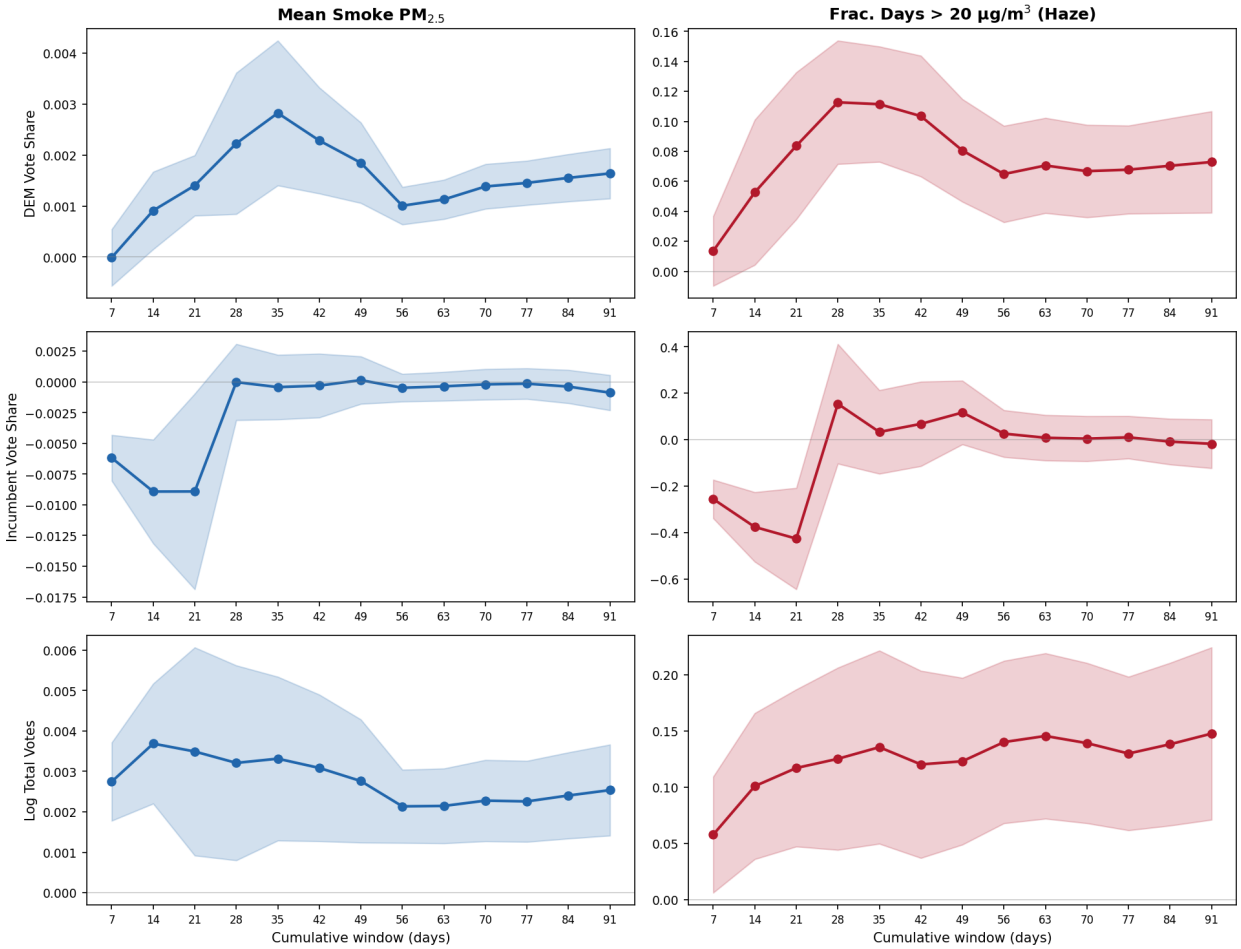

Figure 1: Temporal dynamics of smoke effects using expanding cumulative windows. Left column: mean smoke $PM_{2.5}$. Right column: fraction of days exceeding the visible haze threshold (20 μg/m^3). Rows: DEM vote share, incumbent vote share, log total votes. Each point is a separate regression with the indicated cumulative window as treatment. All specifications include county and year FE plus time-varying controls (Specification 3). Bars are 95% confidence intervals; SEs clustered by county.

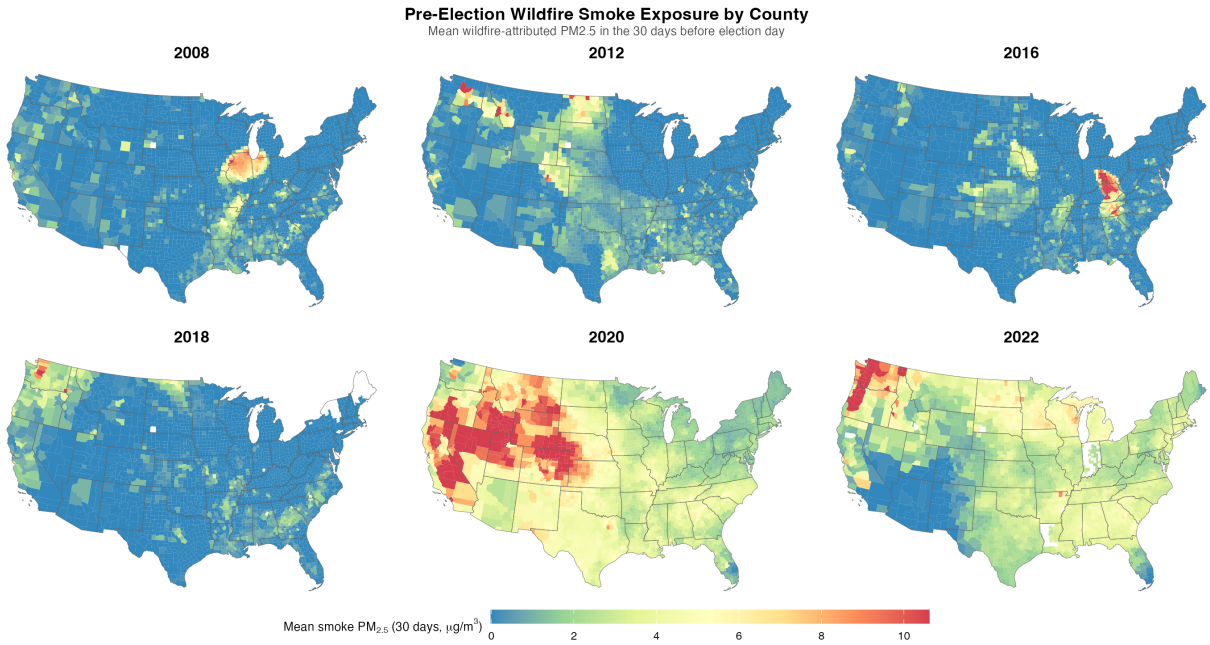

Figure 2: Pre-election wildfire smoke exposure by county, 30-day window before election day. Color scale is identical across all panels.

imately 3,000 counties per election across four cycles (2016, 2018, 2020, 2022). Multi-district counties have votes from all House races aggregated, measuring overall House candidate performance in each county rather than individual district outcomes. The pro-Democratic effect is not significant in House races, while the anti-incumbent effect is marginally significant. The turnout effect is strong in both presidential and House races.

As a further robustness check, I also estimate the House specifications at the congressional district level using Census county-to-district crosswalks (Appendix Table A2). The district-level estimates are noisier due to the measurement error introduced by the crosswalk, but the anti-incumbent effect remains statistically significant.

5.6 Robustness to Excluding 2020

The 2020 election coincided with historically extreme wildfire smoke across the Western United States, raising the question of whether the main results are driven by this single year. Figure 3 addresses this by overlaying the cumulative temporal dynamics for the full sample and the sample excluding 2020, using the same Specification (3) as Figure 1.

The results reveal a sharp divergence. For mean smoke $PM_{2.5}$ (left column), the pro-Democratic effect reverses sign when 2020 is excluded: the full-sample positive effect at

Table 3: Effect of Wildfire Smoke: County-Level House vs. Presidential

	(1) County House	(2) Presidential
Panel A: DEM Vote Share		
Mean Smoke $PM_{2.5}$ (30d)	−0.00033	0.00135***
	(0.00029)	(0.00021)
Panel B: Incumbent Vote Share		
Mean Smoke $PM_{2.5}$ (30d)	0.00304*	−0.00172*
	(0.00178)	(0.00091)
Panel C: Log Total Votes		
Mean Smoke $PM_{2.5}$ (30d)	0.00356***	0.00314***
	(0.00119)	(0.00052)
Unit	County	County
FE	County + Year	County + Year
Observations	11,155 / 12,197	12,428
Elections	2016–2022	2008–2020

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. SEs clustered by county.
Panels A–B use contested races only; Panel C includes all.

the 28-day window ($\beta = 0.0022$, $p < 0.001$) becomes negative without 2020 ($\beta = -0.0035$, $p < 0.001$). This confirms that the 2020 Western fire season—which produced extreme smoke in Oregon, Washington, and California—is essential for the pro-Democratic result.

The anti-incumbent effect, however, is dramatically *stronger* without 2020. The full-sample estimate is near zero at the 28-day window, but excluding 2020 reveals a large and significant anti-incumbent effect ($\beta = -0.033$, $p < 0.001$). This pattern—visible across all window lengths—suggests that the 2020 election, in which smoke-exposed areas happened to favor the incumbent, masked a strong underlying incumbent punishment effect operating in 2008–2016.

The haze threshold measure (right column) tells a similar story: the pro-Democratic effect disappears without 2020, while a large anti-incumbent effect emerges ($\beta = -0.73$, $p < 0.001$ at 28 days). Turnout effects are positive and significant in both samples under both treatments. These patterns—visible across the full temporal profile rather than at a single window—suggest that anti-incumbent punishment is the most robust electoral consequence of wildfire smoke, while the pro-Democratic shift is driven by the 2020 fire season.

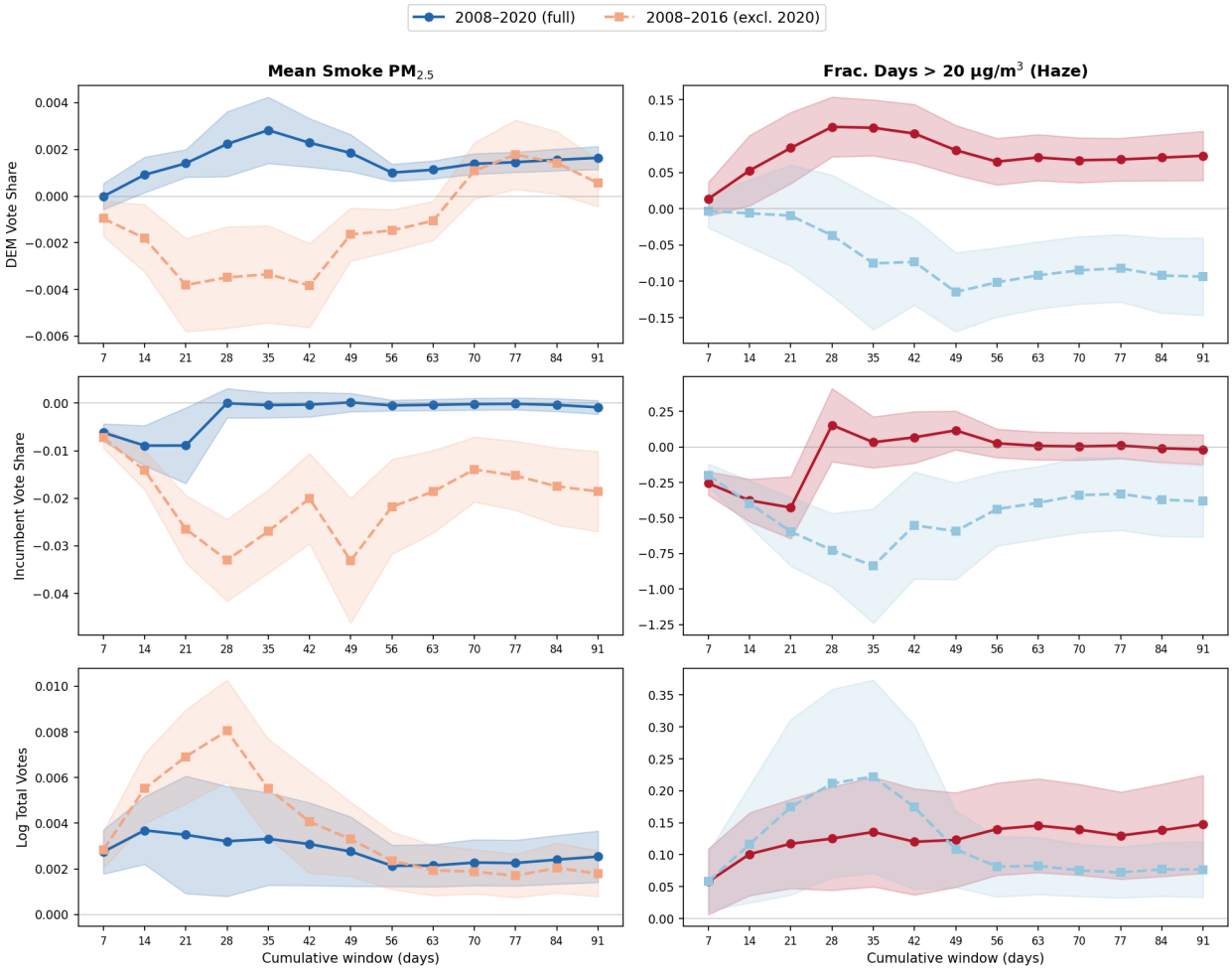

Figure 3: Temporal dynamics: full sample vs. excluding 2020. Solid lines with circles show the full sample; dashed lines with squares exclude the 2020 election. Left column: mean smoke $PM_{2.5}$. Right column: fraction of days exceeding the visible haze threshold (20 μg/m^3). All specifications include county and year FE plus time-varying controls (Specification 3). Bars are 95% confidence intervals; SEs clustered by county.

6 Discussion

Three mechanisms could drive these results. First, a *salience* channel: smoke makes climate change tangible, increasing the weight voters place on environmental issues and benefiting the party perceived as more pro-environment (Hazlett and Mildenberger, 2020; Kahn, 2007). Second, a *negative affect* channel: smoke degrades well-being and mood, and voters punish incumbents for experienced discomfort regardless of policy responsibility (Bellani et al., 2024; Healy and Malhotra, 2010). Third, a *disruption* channel: smoke could differentially suppress turnout among certain voter groups (Gomez et al., 2007; Burke et al., 2022).

The baseline county-and-year FE results are consistent with the salience and affect channels: the pro-Democratic shift points toward salience, while the anti-incumbent effect (marginally significant at the 30-day window, strongly significant at 60 days) points toward negative affect. The threshold comparison in Appendix A.2 further supports this interpretation: the pro-Democratic shift is present at all thresholds with a dose-response pattern (larger effects at higher cutoffs), while the incumbent effect *reverses sign* between moderate and severe exposure—visible haze benefits incumbents, but severe unhealthy episodes punish them. This is consistent with moderate smoke activating a rally-around-the-flag response while severe smoke triggers blame attribution. The drop-2020 analysis in Figure 3 sharpens the interpretation further: the pro-Democratic effect reverses sign without 2020, while the anti-incumbent effect is dramatically stronger—masked in the full sample by the 2020 election, in which smoke-exposed areas happened to favor the incumbent. The temporal dynamics (Figure 1) provide the most compelling evidence for a genuine smoke effect: smoke in the weeks closest to the election produces the largest estimates, a pattern difficult to explain with confounders.

Table 1 shows that the baseline estimates are robust to adding time-varying county controls (Column 3)—unemployment rate, log median household income, log population, and October temperature and precipitation—ruling out the possibility that the results are driven by local economic conditions or weather confounders correlated with smoke exposure. Adding state-specific linear trends (Column 4) further attenuates the pro-Democratic effect to insignificance, though the turnout effect survives.

Appendix A.5 presents a more demanding specification replacing year FE with state-by-year FE, so that identification comes only from within-state variation in smoke across counties within the same election. No coefficient survives this test, which is unsurprising given that the within-state variation in smoke—shown in the residualized maps of Figure A3—is concentrated in a few Western states in high-fire years. With only four presidential elections, the within-state, within-year variation may be insufficient for precise estimation, but this

remains a limitation that future work with additional election cycles could address.

Limitations. This proof of concept has several limitations that subsequent work should address. The analysis covers only four presidential elections and four House elections, and as the robustness analysis demonstrates, the pro-Democratic finding is leveraged by the 2020 fire season. The turnout measure (log total votes) is a crude proxy without a proper population denominator. County-level aggregation may mask within-county heterogeneity. And the negative within-R^2 values in some specifications suggest that the smoke variable alone explains limited within-county variation after absorbing fixed effects, underscoring that these are small effects on a noisy outcome.

7 Conclusion

Under county-and-year fixed effects, wildfire smoke exposure in the 30 days before an election is associated with a pro-Democratic shift in presidential voting, with effects strongest in the weeks immediately preceding the election. The 7-day temporal dynamics provide the most compelling evidence for a causal effect, as the recency pattern is difficult to explain with confounders. A more demanding specification with state-by-year fixed effects eliminates the effects (Appendix A.5), suggesting that within-state variation—concentrated in a few Western states in high-fire years—may be insufficient for precise estimation with only four elections. House elections show weaker and less consistent patterns. These preliminary results suggest that wildfire smoke—which is plausibly exogenous and affects a far larger population than fire proximity—offers a promising research design for studying how environmental experience shapes political behavior, but additional election cycles and sharper identification strategies are needed to establish causality.

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A Supplementary Results

A.1 Exclusive 7-Day Windows

Figures A1 and A2 present the exclusive-window counterparts to the cumulative results in Figure 1. In the exclusive specification, all thirteen non-overlapping 7-day bins are entered simultaneously as regressors. These estimates identify the marginal contribution of each week's smoke exposure holding all other weeks constant. The sign-flipping between adjacent bins reflects multicollinearity among temporally proximate smoke measures; the cumulative windows in the main text provide the more interpretable summary.

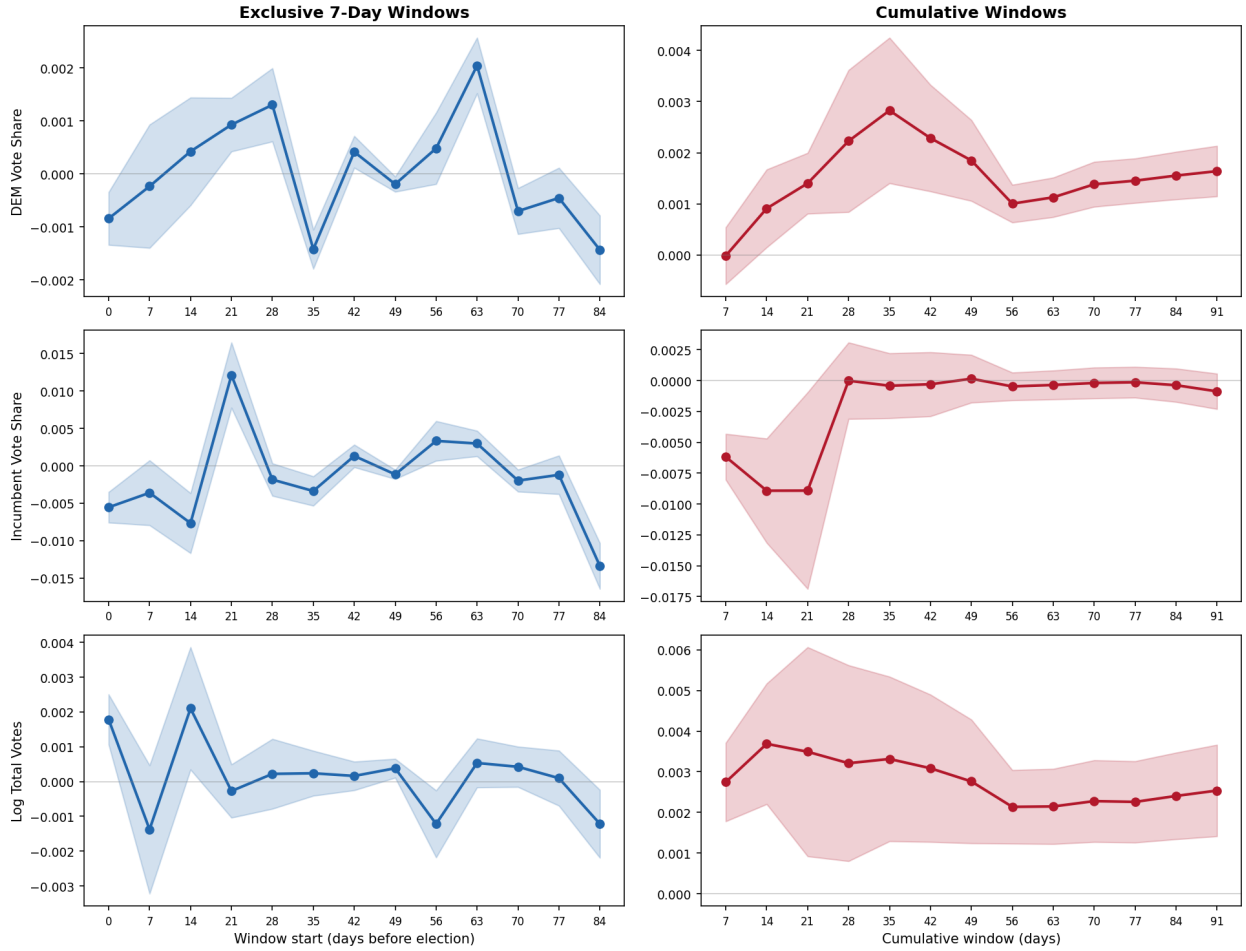

Figure A1: Temporal dynamics: mean smoke $PM_{2.5}$. Left column: exclusive 7-day windows (all bins entered simultaneously). Right column: cumulative windows (same as left column of Figure 1). County and year FE with controls. 95% CIs; SEs clustered by county.

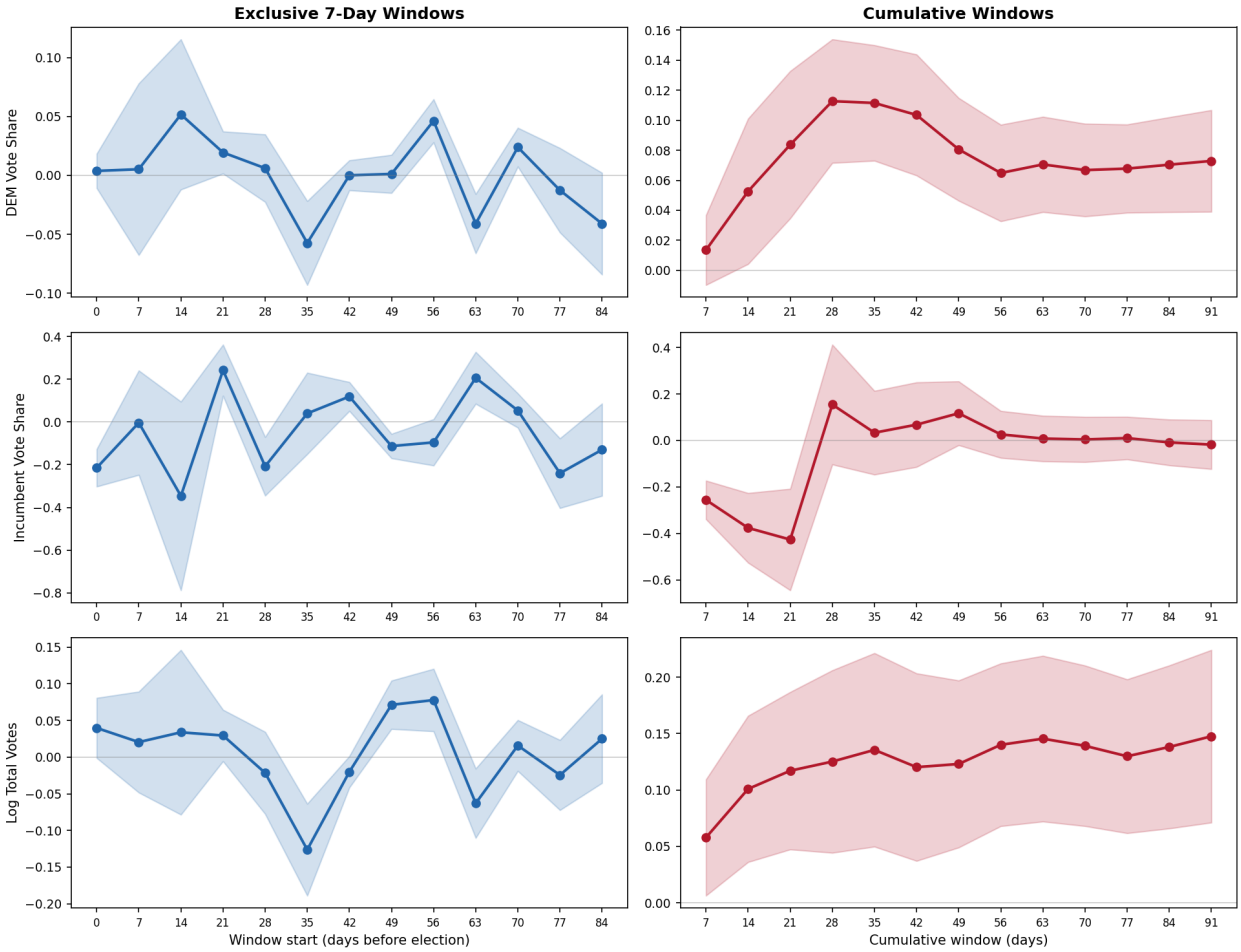

Figure A2: Temporal dynamics: fraction of days exceeding the visible haze threshold (20 μg/m^3). Left column: exclusive 7-day windows. Right column: cumulative windows (same as right column of Figure 1). County and year FE with controls. 95% CIs; SEs clustered by county.

A.2 Threshold Comparison

Table A1 compares the fraction-above-threshold treatment variable at three cutoffs: 20 μg/m^3 (onset of visible haze, used in the main text), 35.5 μg/m^3 (EPA "Unhealthy for Sensitive Groups"), and 55.5 μg/m^3 (EPA "Unhealthy"). All specifications use the 30-day pre-election window with county and year fixed effects plus time-varying controls (Specification 3).

The haze threshold has substantially more variation (236 nonzero observations, 1.9%) than the higher thresholds (47 and 19 nonzero, respectively). For Democratic vote share, all three thresholds produce significant positive estimates, with larger coefficients at higher thresholds—consistent with a dose-response relationship in which more severe smoke produces stronger pro-Democratic shifts. For incumbent vote share, the sign flips across thresholds: the haze measure shows a significant *positive* effect (moderate smoke may benefit incumbents through rally effects), while the unhealthy measure shows a significant *negative* effect (severe smoke punishes incumbents). This sign reversal is consistent with dual mechanisms operating at different exposure intensities. For turnout, only the haze threshold is significant, reflecting its greater statistical power.

Table A1: Threshold Comparison: Fraction of Days Above Cutoff (30-Day Window)

	Haze (>20)	USG (>35.5)	Unhealthy (>55.5)
Panel A: DEM Vote Share			
Frac. days above threshold	0.05709***	0.09548**	0.15082***
	(0.01516)	(0.04349)	(0.03730)
Panel B: Incumbent Vote Share			
Frac. days above threshold	0.16459**	−0.16548	−0.24938***
	(0.06772)	(0.10456)	(0.07877)
Panel C: Log Total Votes			
Frac. days above threshold	0.09413***	0.03815	0.09920
	(0.02947)	(0.06404)	(0.09138)
Nonzero obs (of 12,432)	236 (1.9%)	47 (0.4%)	19 (0.2%)
County + Year FE	✓	✓	✓
Controls	✓	✓	✓
Observations	12,400	12,400	12,400

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. SEs clustered by county. Presidential elections 2008–2020.

A.3 District-Level House Estimates

Table A2: Robustness: District-Level House Estimates

	(1) District House	(2) County House
Panel A: DEM Vote Share		
Mean Smoke $PM_{2.5}$ (30d)	−0.00081	−0.00033
	(0.00086)	(0.00029)
Panel B: Incumbent Vote Share		
Mean Smoke $PM_{2.5}$ (30d)	−0.00162	0.00304*
	(0.00162)	(0.00178)
Panel C: Log Total Votes		
Mean Smoke $PM_{2.5}$ (30d)	−0.01022*	0.00356***
	(0.00565)	(0.00119)
Unit	District	County
FE	District + Year	County + Year
Observations	3,406 / 3,879	11,155 / 12,197
Elections	2006–2022	2016–2022

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. SEs clustered by unit.
District-level uses Census crosswalk to map county smoke to districts.

A.4 Controls Robustness: Presidential and County House

Table A3 presents the controls robustness check for both presidential and county House specifications side by side. The presidential columns correspond to Columns (2) and (3) of Table 1; the county House columns extend the same test to House elections. Controls are the unemployment rate (BLS LAUS), log median household income (Census SAIPE), log population (Census Population Estimates), and October mean temperature and total precipitation (PRISM).

Table A3: Robustness: Adding Time-Varying County Controls

	Presidential		County House	
	Baseline	+ Controls	Baseline	+ Controls
Panel A: DEM Vote Share				
Mean Smoke $PM_{2.5}$ (30d)	0.00135***	0.00119***	−0.00033	−0.00043
	(0.00021)	(0.00022)	(0.00029)	(0.00028)
Panel B: Incumbent Vote Share				
Mean Smoke $PM_{2.5}$ (30d)	−0.00172*	0.00073	0.00304*	0.00375**
	(0.00091)	(0.00094)	(0.00178)	(0.00151)
Panel C: Log Total Votes				
Mean Smoke $PM_{2.5}$ (30d)	0.00314***	0.00228***	0.00356***	0.00323***
	(0.00052)	(0.00041)	(0.00119)	(0.00114)
County FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls		✓		✓
Observations	12,432	12,400	11,155 / 12,197	11,123 / 12,165

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. SEs clustered by county.
Controls: unemployment rate, log median household income, log population, October mean temperature, October total precipitation.
Panels A–B use contested races only; Panel C includes all.

A.5 State-by-Year Fixed Effects

Table A4 presents estimates replacing year fixed effects with state-by-year fixed effects, so that identification comes only from within-state variation in smoke across counties within the same election. This is substantially more demanding than the state linear trends in Column (4) of Table 1: state-by-year FE absorb *all* state-level time variation, not just linear trends.

No coefficient is statistically significant in either the presidential or county House specifications. The presidential pro-Democratic effect shrinks from 0.00135 to essentially zero (0.00001), and the anti-incumbent effect is halved and loses significance. This pattern is consistent with state-level time-varying shocks—such as statewide political campaigns, ballot measures, or economic conditions—being correlated with smoke exposure and driving part of the baseline estimates.

Figure A3 provides visual context for these null results. After residualizing county-level smoke on state-by-year means, the remaining within-state variation is concentrated in a small number of Western states during high-fire years (especially 2020). With only four presidential elections, the effective identifying variation under state-by-year fixed effects may simply be too limited for precise estimation.

Table A4: State-by-Year Fixed Effects: Presidential and County House

	(1) Presidential	(2) County House
Panel A: DEM Vote Share		
Mean Smoke $PM_{2.5}$ (30d)	0.00001	0.00011
	(0.00025)	(0.00025)
Panel B: Incumbent Vote Share		
Mean Smoke $PM_{2.5}$ (30d)	−0.00079	0.00214
	(0.00086)	(0.00140)
Panel C: Log Total Votes		
Mean Smoke $PM_{2.5}$ (30d)	0.00023	0.00161
	(0.00030)	(0.00135)
FE	County + State×Year	County + State×Year
Observations	12,428	11,155 / 12,197

No coefficient is significant at the 10% level. SEs clustered by county.

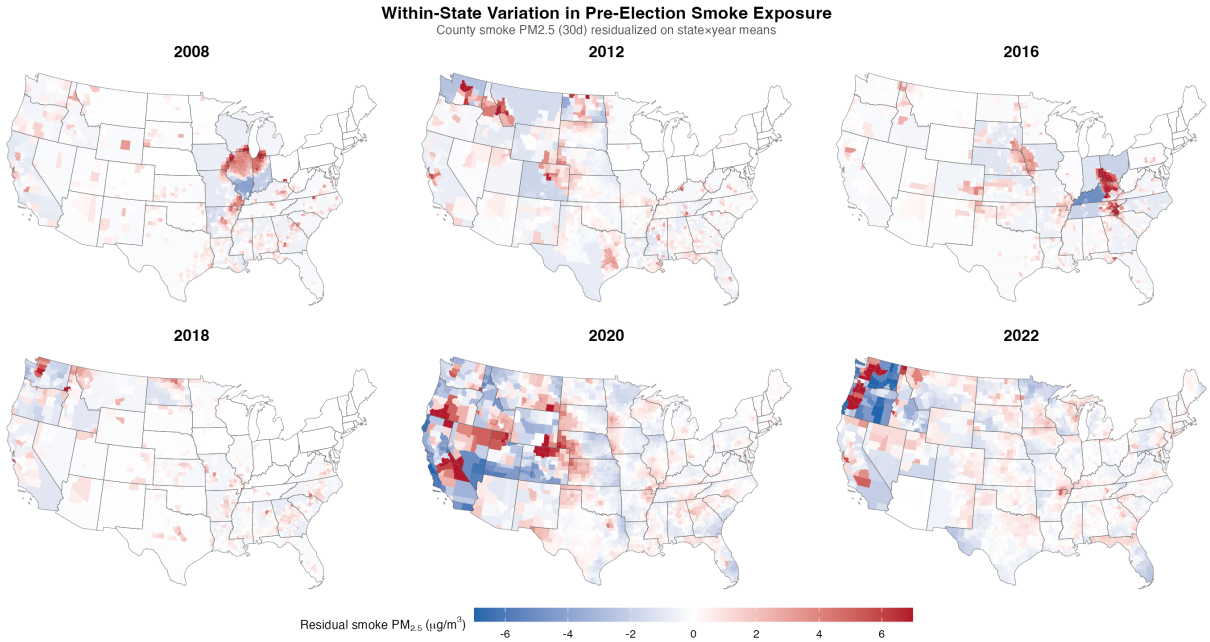

Figure A3: Within-state variation in pre-election smoke exposure. County-level smoke $PM_{2.5}$ (30-day window) residualized on state-by-year means. The diverging scale shows counties with more (red) or less (blue) smoke than their state average. Meaningful within-state variation is concentrated in Western states during high-fire years.

B Literature Review

This appendix surveys the literatures most relevant to this paper, organized into five strands: environmental shocks and voting, the theory of anti-incumbent and retrospective voting, air pollution and political behavior, wildfire smoke and economic/health outcomes, and identification strategies using atmospheric variation.

B.1 Environmental Shocks, Natural Disasters, and Voting

A large literature examines whether environmental shocks alter political behavior. The findings cluster around two mechanisms: retrospective incumbent punishment and climate salience.

B.1.1 Incumbent Punishment

Healy and Malhotra (2010) provide the foundational analysis of myopic retrospective voting in response to disasters. Voters punish incumbents for disaster damage but reward them for disaster relief spending, and the asymmetry implies that governments underinvest in preparedness relative to relief. Achen and Bartels (2016) extend the argument, contending that voters engage in "blind retrospection," punishing incumbents for events entirely beyond governmental control, including droughts, floods, and famously, shark attacks—though the shark attack finding is contested by Fowler and Hall (2018). Gasper and Reeves (2011) show that voters punish governors and presidents differently after natural disasters depending on whether a disaster declaration was issued, suggesting voters can partly distinguish between experiencing harm and receiving a government response. Bechtel and Hainmueller (2011) study the 2002 Elbe River flood in Germany and find that voters rewarded the incumbent government for effective flood management, demonstrating that the sign of the disaster–voting relationship depends on the government's response. Cole et al. (2012) use rainfall variation in India to show that voters reward governments for disaster relief, providing a developing-country parallel.

B.1.2 Wildfires and Voting

Hazlett and Mildenberger (2020) is the most directly relevant paper. Using proximity to California wildfire perimeters, they find that fire proximity increases pro-environment ballot proposition voting, but only in already-Democratic areas. Republican areas show no response. My paper extends their work along three dimensions: smoke exposure rather than

fire proximity provides a plausibly exogenous treatment; national scope replaces a California-only sample; and I find effects across the partisan spectrum. Liao and Ruiz Junco (2022) study how extreme weather and natural disasters affect campaign contributions and House elections, finding that natural disasters hurt incumbents with anti-environment records, providing complementary evidence using campaign finance data.

B.1.3 Climate Salience

Herrnstadt and Muehlegger (2014) use Google search intensity to show that unusual weather increases searches for "climate change" and that members of Congress vote more pro-environment when their home states experience unusual weather—the clearest evidence for a salience channel linking environmental experience to political behavior. Hoffmann et al. (2022) use panel data across Europe to show that personal experience of climate-related events raises environmental concerns and increases Green party voting. Baccini and Leemann (2021) study Swiss referendum voting after floods and find that flood exposure increases support for pro-climate ballot measures by up to 20 percent. Elliott et al. (2023) examine whether U.S. senators vote more pro-environment after climate-related natural disasters, finding effects that are short-lived (about two years), consistent with the temporal dynamics in my results showing strongest effects at shorter pre-election windows.

B.1.4 Weather and Turnout

Gomez et al. (2007) show that rain on election day suppresses voter turnout with differential effects by party. This is relevant for the turnout channel: my positive turnout coefficient could be in tension with their finding if smoke operates like bad weather, but smoke exposure in my specification is measured over the pre-election window rather than on election day itself.

B.2 Anti-Incumbent Voting: Theoretical Foundations

The robust anti-incumbent finding places this paper within one of the oldest debates in democratic theory.

B.2.1 Sanctioning and Selection Models

The formal literature on electoral accountability begins with Ferejohn (1986), who models elections as a sanctioning mechanism: voters retain the incumbent if performance exceeds some threshold and replace her otherwise. The key insight is that retrospective voting

can sustain accountability even when voters are poorly informed about policy. Ashworth (2012) reviews how modern formal models blend sanctioning with selection (voters using past performance to infer incumbent quality), noting that the empirical literature has struggled to separate the two mechanisms.

B.2.2 Blind Retrospection and Its Critics

Achen and Bartels (2016) argue that retrospective voting is often "blind"—voters punish incumbents for events beyond governmental control. Gasper and Reeves (2011) push back, showing voters distinguish between harm and government response. Fowler and Hall (2018) challenge the shark attack result specifically. The emerging consensus, as articulated in Healy and Malhotra (2013), is a middle ground: voters are neither fully rational accountability agents nor purely blind avengers, but decision makers who sometimes apply coherent logic and sometimes fall prey to psychological biases.

B.2.3 Psychological Mechanisms

Three mechanisms are especially relevant. First, negative affect: Healy et al. (2010) show that irrelevant events (college football outcomes) shift incumbent evaluations, suggesting negative emotional states reduce support for the status quo regardless of source. Huber et al. (2012) provide experimental confirmation that voters overweight recent performance and are influenced by irrelevant lotteries. Second, attribution: partisanship powerfully shapes blame assignment after disasters, which may partly explain the partisan heterogeneity I find. Third, issue salience: negative experiences can shift the issues voters weight, activating voters who already care about climate while others simply experience negative affect. This dual-channel interpretation—salience for the pro-Democratic shift, affect for the anti-incumbent shift—maps onto my results where both channels appear to operate simultaneously through different pathways.

B.3 Air Pollution and Political Behavior

Bellani et al. (2024) is the closest analogue for the pollution–voting mechanism. Using 60 German federal and state elections, they exploit within-county variation in PM_{10} on election day to show that a 10 $\mu g/m^3$ increase reduces the incumbent vote share by two percentage points. They argue this operates through a subconscious emotional channel: PM_{10} fluctuations are imperceptible, yet they increase negative emotions. My setting differs critically in that wildfire smoke is visible and salient, potentially activating both their affect channel

and a deliberate salience channel. Kahn (2007) provides background for the heterogeneity pattern: areas with stronger baseline pro-environment preferences show larger smoke effects.

B.4 Wildfire Smoke: Economic, Health, and Behavioral Effects

A rapidly growing literature documents the broad consequences of wildfire smoke, establishing the health and mood channels that could mediate voting effects.

B.4.1 Labor Markets and Economic Costs

Borgschulte et al. (2022) is the leading paper using wildfire smoke as an exogenous source of $PM_{2.5}$ variation to study economic outcomes, estimating that each additional smoke day reduces quarterly per capita earnings by about 0.1 percent. Their identification strategy—relying on the quasi-random spatial dispersion of smoke plumes using the same NOAA HMS data underlying Childs et al. (2022)—is closely related to mine. Importantly, they show that controlling flexibly for wind direction does not change their estimates, strengthening the case that smoke rather than correlated wind patterns drives the variation.

B.4.2 Mental Health and Mood

Burke et al. (2022) document behavioral and sentiment responses to wildfire smoke: during smoke events, people search more for air quality information, stay home more, and express more negative sentiment. This provides direct evidence for the mood mechanism I invoke for incumbent punishment. Jung et al. (2025) isolate short-term mental health effects of wildfire-specific $PM_{2.5}$, finding that a 10 $\mu g/m^3$ increase significantly increases emergency department visits for depression and anxiety, with effects lasting up to seven days. Miller et al. (2024) estimate that wildfire smoke accounts for 18 percent of ambient $PM_{2.5}$ concentrations and 0.42 percent of deaths among adults 65 and older, establishing the health harm of the exact pollution source I study.

B.5 Identification: Wind Direction and Atmospheric Dispersion

Deryugina et al. (2019) pioneered using wind direction as an instrument for $PM_{2.5}$, estimating effects of acute exposure on elderly mortality. The innovation is that the approach does not require knowing the location of pollution sources—it simply exploits the fact that wind direction shifts nonlocal pollution in and out of a county. My approach differs in that rather than using wind as an instrument for total $PM_{2.5}$, I use data from Childs et al. (2022) that has already separated wildfire smoke $PM_{2.5}$ from other sources, and argue that

atmospheric dispersion is quasi-random conditional on county and year fixed effects. Rangel and Vogl (2019) use upwind/downwind variation from agricultural fires in Brazil to study birth outcomes, a conceptually very close design. Borgschulte et al. (2022) demonstrate as a robustness check that controlling for wind direction does not change their smoke estimates, a test that could be applied in my setting as well.