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, 2016–2020 for House), I find that higher pre-election smoke exposure increases the Democratic two-party vote share and decreases the incumbent party's vote share. A 10 μg/m^3 increase in mean smoke $PM_{2.5}$ over the 60 days before the election is associated with a 0.8 percentage point increase in the Democratic vote share in presidential races. The incumbent punishment effect is roughly five times larger. Effects are present across the partisan spectrum but somewhat stronger in Democratic-leaning counties. 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

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

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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 Data

Wildfire smoke $\mathbf{PM_{2.5}}$. 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) 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 three election cycles (2016, 2018, 2020) with 9,171 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 60 days before the election.

3 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 $PM_{2.5}$ 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 $PM_{2.5}$) 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.

4 Results

4.1 Main Results

Table 1 presents the main presidential election estimates. Column (1) shows that a 1 μg/m^3 increase in mean smoke $PM_{2.5}$ over the 60 days before the election increases the Democratic two-party vote share by 0.077 percentage points ($p < 0.001$). At the sample mean of 2.7 μg/m^3, this implies that moving from zero smoke to mean exposure shifts the Democratic vote share by roughly 0.21 percentage points. A county experiencing the 2020 Western fire season levels of smoke ($\sim$40 μg/m^3) would see a shift of approximately 3.1 percentage points.

Column (2) shows that the incumbent punishment effect is substantially larger: a 1 μg/m^3 increase reduces the incumbent party's vote share by 0.38 percentage points. This is consistent with the negative-affect mechanism identified by Bellani et al. (2024) in the German context—smoke makes voters feel worse, and they punish the party in power regardless of its partisan identity.

Table 1: Effect of Wildfire Smoke on Presidential Voting Outcomes

	(1) DEM Vote Share	(2) Incumbent Vote Share	(3) Log Total Votes
Mean Smoke $PM_{2.5}$ (60d)	0.00077***	−0.00379***	0.00213***
	(0.00008)	(0.00039)	(0.00016)
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	12,428	12,428	12,428
R^2 (within)	−0.003	−0.026	0.064

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Standard errors clustered by county.

4.2 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 all three groups, with a larger effect in D-leaning counties (0.086 pp) than in R-leaning counties (0.039 pp). Unlike Hazlett and Mildenberger (2020), who find effects *only* in Democratic areas for fire proximity, I find that smoke exposure moves all county types toward the Democrats, though the effect is roughly twice as large where pro-environment attitudes are presumably more prevalent.

Table 2: Heterogeneity by Prior Partisanship

	R-Leaning	Swing	D-Leaning
Mean Smoke $PM_{2.5}$ (60d)	0.00039**	0.00049***	0.00086***
	(0.00017)	(0.00013)	(0.00013)
Observations	4,143	4,140	4,144

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

4.3 Temporal Dynamics

Figure 1 plots the estimated effect of mean smoke $PM_{2.5}$ on Democratic vote share across different pre-election windows. The effect is statistically significant at all windows, with the largest point estimate at the 30-day window. This suggests that smoke exposure in the weeks most proximate to the election has the greatest electoral impact, consistent with a salience or recency mechanism.

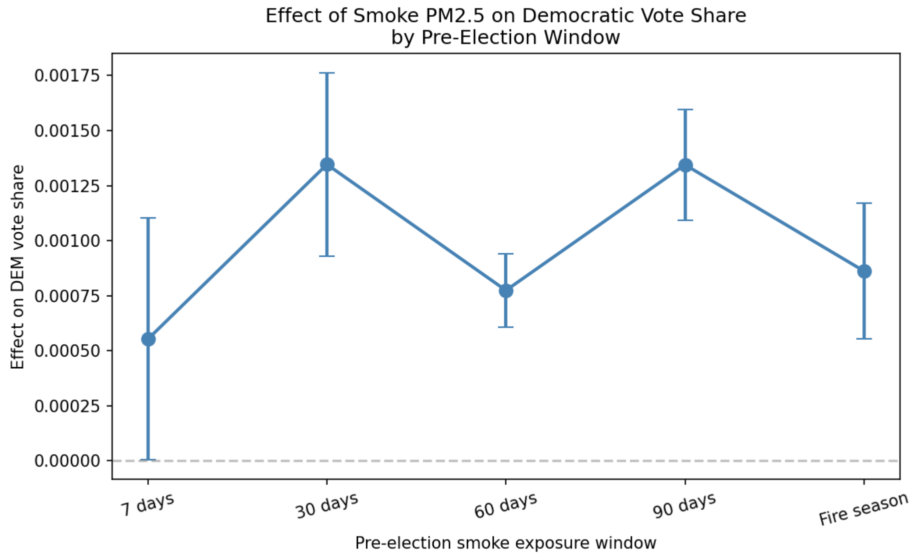

Figure 1: Effect of smoke $PM_{2.5}$ on Democratic vote share by pre-election window length. Points are coefficient estimates from separate TWFE regressions; bars are 95% confidence intervals.

4.4 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.

4.5 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 2016–2020, 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 60-day mean smoke $PM_{2.5}$ treatment. The county-level House analysis covers approximately 3,000 counties per election across three cycles (2016, 2018, 2020). 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 and anti-incumbent effects are both statistically significant, though smaller in magnitude than the presidential estimates, consistent with the more candidate-driven nature of House races.

As a further robustness check, I also estimate the House specifications at the congressional

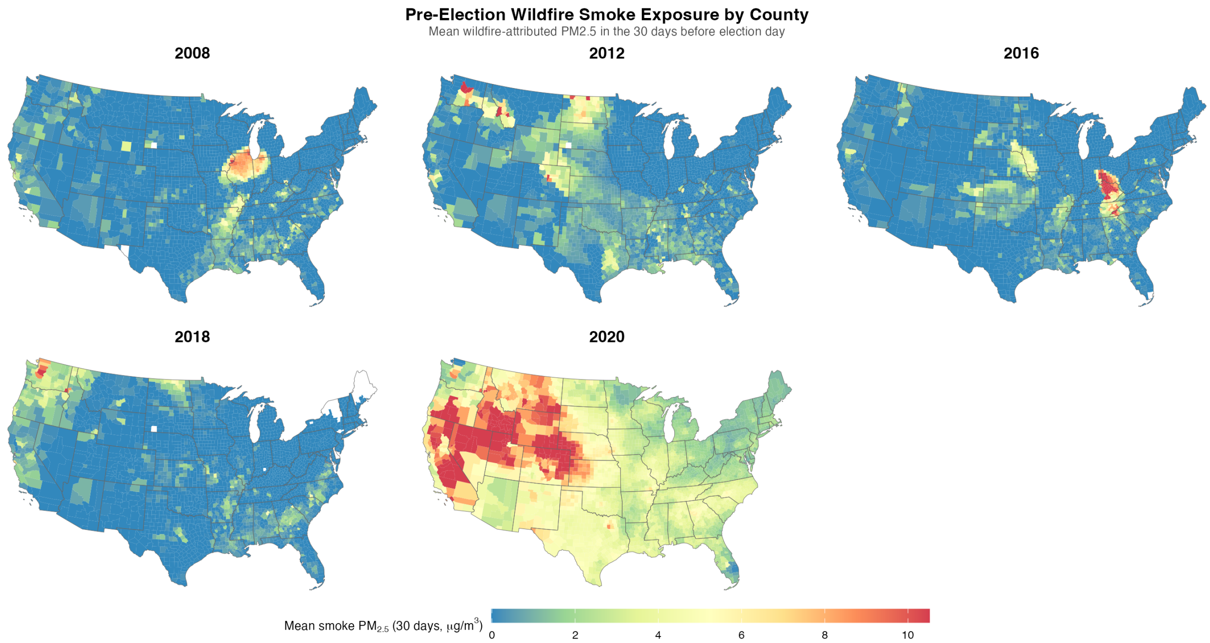

Figure 2: Pre-election wildfire smoke exposure by county, 30-day window before election day. Color scale is identical across all panels.

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

	(1) County House	(2) Presidential
Panel A: DEM Vote Share		
Mean Smoke $PM_{2.5}$ (60d)	0.00040***	0.00077***
	(0.00012)	(0.00008)
Panel B: Incumbent Vote Share		
Mean Smoke $PM_{2.5}$ (60d)	−0.00113***	−0.00379***
	(0.00042)	(0.00039)
Panel C: Log Total Votes		
Mean Smoke $PM_{2.5}$ (60d)	0.00151***	0.00213***
	(0.00058)	(0.00016)
Unit	County	County
FE	County + Year	County + Year
Observations	8,391 / 9,165	12,428
Elections	2016–2020	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.

district level using Census county-to-district crosswalks (Appendix Table A1). The district-level estimates are noisier due to the measurement error introduced by the crosswalk, but the anti-incumbent effect remains statistically significant.

4.6 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. Table 4 presents presidential estimates with and without 2020. The pro-Democratic effect on vote share is not robust to excluding 2020: the coefficient flips sign and is only marginally significant ($\beta = -0.00036$, $p = 0.097$). This indicates that the 2020 Western fire season—which produced extreme smoke exposure in Oregon, Washington, and California—provides much of the identifying variation for the Democratic vote share result.

By contrast, the anti-incumbent effect is robust and substantially *larger* when 2020 is excluded ($\beta = -0.01368$, $p < 0.001$, compared to -0.00379 in the full sample). The turnout effect attenuates but remains statistically significant ($\beta = 0.00068$, $p = 0.006$). These patterns suggest that incumbent punishment is the most robust electoral consequence of wildfire smoke, while the pro-Democratic shift requires further investigation with additional election cycles.

Table 4: Robustness: Presidential Estimates Excluding 2020

	(1) Full Sample	(2) Excluding 2020
Panel A: DEM Vote Share		
Mean Smoke $PM_{2.5}$ (60d)	0.00077***	−0.00036*
	(0.00008)	(0.00022)
Panel B: Incumbent Vote Share		
Mean Smoke $PM_{2.5}$ (60d)	−0.00379***	−0.01368***
	(0.00039)	(0.00141)
Panel C: Log Total Votes		
Mean Smoke $PM_{2.5}$ (60d)	0.00213***	0.00068***
	(0.00016)	(0.00025)
Observations	12,428	9,320
Elections	2008–2020	2008–2016

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

5 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 data are more consistent with the first two mechanisms than the third. The pro-Democratic shift points toward salience, while the larger anti-incumbent effect points toward negative affect. The positive turnout coefficient (Column 3 of Table 1) is surprising and may reflect confounding from 2020's historically high turnout; this result should be interpreted cautiously.

The robustness analysis in Table 4 sharpens the interpretation. The pro-Democratic effect is not robust to excluding 2020, suggesting the salience channel may require extreme smoke exposure to generate measurable partisan shifts. The anti-incumbent effect, by contrast, is robust and strengthens without 2020, suggesting that negative affect—punishing the party in power for experienced discomfort—operates even at moderate smoke levels and does not depend on a single extreme year.

Limitations. This proof of concept has several limitations that subsequent work should address. The analysis covers only four presidential elections and three 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.

6 Conclusion

Wildfire smoke exposure punishes the incumbent party in both presidential and House elections, with effects that are statistically significant, robust to excluding 2020, present across the partisan spectrum, and strongest in the weeks immediately preceding the election. Smoke also shifts votes toward the Democratic Party, though this effect is driven by the extreme

2020 Western fire season and requires confirmation with additional election cycles. These preliminary results suggest that wildfire smoke—which is plausibly exogenous and affects a far larger population than fire proximity—offers a compelling research design for studying how environmental experience shapes political behavior.

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Appendix

Table A1: Robustness: District-Level House Estimates

	(1) District House	(2) County House
Panel A: DEM Vote Share		
Mean Smoke $PM_{2.5}$ (60d)	-0.00016	0.00040***
	(0.00042)	(0.00012)
Panel B: Incumbent Vote Share		
Mean Smoke $PM_{2.5}$ (60d)	-0.00190**	-0.00113***
	(0.00081)	(0.00042)
Panel C: Log Total Votes		
Mean Smoke $PM_{2.5}$ (60d)	-0.00099	0.00151***
	(0.00143)	(0.00058)
Unit	District	County
FE	District + Year	County + Year
Observations	3,013 / 3,447	8,391 / 9,165
Elections	2006–2020	2016–2020

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