Wildfire Exposure Increases Pro-Climate Political Behaviors

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Wildfire exposure increases pro-climate political behaviors

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Abstract

One political barrier to climate reforms is the temporal mismatch between short-term policy costs and long-term policy benefits. Will this barrier weaken when publics experience climate impacts first-hand, making the short-term costs of inaction more salient? Using a natural experiment based on the timing of Californian wildfires, we evaluate, for the first time, how exposure to a climate-related hazard influences political behavior, rather than self-reported attitudes or behavioral intentions. We show that wildfires increased support for costly, pro-climate ballot measures by 4 to 6 percentage points for those living within 15km of a recent wildfire. The effect drops by approximately 1.7 percentage points for every 10km thereafter. This effect is largest in Democratic-voting areas, and nearly zero in Republican-dominated areas. We conclude that experienced climate threats can enhance willingness-to-act, but predominately where the public already holds pro-climate beliefs.

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Despite the severity of the climate threat, global policymaking remains anemic. One political barrier to enacting climate policy has been the temporal mismatch between short-term climate policy costs and long-term climate policy benefits (Jacobs, 2011; Levin et al., 2012). However, as the time horizon for realized climate change moves closer, weather extremes and climate-related hazards could reshape the politics of climate change by making salient the costs of policy *inaction*. Already, climate change has begun to noticeably disrupt economic, social, and environmental conditions across the globe, including in the United States (Diffenbaugh, Swain, and Touma, 2015; Abatzoglou and Williams, 2016).

Yet, scholars remain uncertain whether first-hand climate change experiences are reshaping the public's climate policy preferences or political behaviors. Some scholars find that climate concerns modestly increase with experienced temperature extremes (Brooks et al., 2014; Bergquist and Warshaw, 2019). Others find no such effects (Brulle, Carmichael, and Jenkins, 2012; Mildenberger and Leiserowitz, 2017), only ephemeral effects (Egan and Mullin, 2012; Deryugina, 2013; Konisky, Hughes, and Kaylor, 2016), or that effects are limited to particular political subgroups (Hamilton and Stampone, 2013). Evidence for the relationship between climate-related hazards and reported attitudes is similarly mixed. Some studies find that experiencing hazards increases intention to engage in mitigation and adaptation policies (Spence et al., 2011; Demski et al., 2017) and climate risk perceptions (Lujala, Lein, and Rød, 2015). Others, though, find little or no effect of hazards such as flooding or fire (Whitmarsh, 2008; Brody et al., 2008). Further, the question remains as to whether attitudinal shifts, even if they do occur, provoke change in realized political behaviors (Rudman, McLean, and Bunzl, 2013).

These mixed empirical findings reflect systematic differences in how both climate threats and responses are measured, and in attention to causal identification (Howe, Marlon, et al., 2019). They also reflect different theoretical expectations about political responsiveness to experienced threat. From one perspective, experiencing climate-related hazards may heighten the salience of related social and economic risks, irrespective of an individual's political identity (Slovic and Weber, 2013). Alternatively, an individual's response to experiencing a climate change impact may be conditioned by pre-existing beliefs and identities (Howe and Leiserowitz, 2013; Myers et al., 2013), including party or ideological commitments (Marquart-Pyatt et al., 2014; Hamilton, Wake, et al., 2016) and beliefs in anthropogenic climate change (Brody et al., 2008; Capstick and Pidgeon, 2014). Moreover, climate-related political behaviors may be overshadowed by other factors that influence political preferences during crises, including public evaluation of government performance (Malhotra and Kuo, 2008; Bechtel and Hainmueller, 2011).

In this paper, we evaluate the links between experiencing a climate-related hazard and realized political behavior. Our study offers two major advances over prior work. First, existing research on experienced climate change has exclusively used survey outcomes to measure individual attitudes or behavioral intentions (Howe, Marlon, et al., 2019). By contrast, we estimate the effect of an actual climate-related hazard (wildfires) on a realized political behavior that directly influences policy (ballot initiative support). Specifically, we study how wildfire exposure at the census block group-level shapes voting outcomes on a series of Californian environmental ballot initiatives between 2006 and 2010. Second, we exploit a natural experiment to produce credible causal estimates, together with sensitivity analyses that describe how strong confounding would need to be to alter our results. Specifically, We estimate a causal effect under the assumption of no time-varying confounding, i.e. that the timing of wildfires within any census block group is not driven by time-varying features of those block groups that also relate to support for climate policies. Under this approach, controlling for observed time-varying factors such as population density or (lagged) Democratic vote share are unnecessary, but also have no impact on the estimate. We then relax the assumption of zero time-varying confounding and demonstrate that even very strong confounding, many times stronger than the observed variables considered, can alter our conclusions.

Overall, we find that Californians who experience a wildfire within 5, 10, or 15 km of their census block group are 4-6 percentage points more likely to vote for costly pro-climate policy reforms, relative to those the median distance away (35-40km). This effect decays with distance from a wildfire, weakening by approximately 1.7 percentage points for every additional 10 km of distance. Moreover, this effect is highly heterogeneous depending on partisan identity. The most Democratic tercile of block groups show a strong effect of 7-9 percentage points for those at 5, 10, or 15 km compared to those at the median distance. By contrast, the effects are very small across all distances for block groups dominated by Republican voters. We thus find that responsiveness to climate-related impacts is concentrated in populations that, among other features, are far more likely to believe in anthropogenic climate change (Dunlap, McCright, and Yarosh, 2016). Ultimately our results suggest that as the impacts of climate change become more evident, some parts of the public will respond by increasing their personal and political commitment to climate risk mitigation. However, this shift may remain much smaller in areas where pre-existing climate beliefs are weak, making costly policy change less likely.

Methods

We prepare an original panel of political and wildfire data in California. Electoral outcome and voter registration data available from the California Secretary of State provide precinct-level outcomes for all national elections between 2002 and 2010. The precinct level is the smallest unit with electoral return data in California. However, Californian electoral precinct boundaries and names change over time. We convert all data to 2000 census block group geographies. Official conversion files allow us to compute the overlap between election precincts in each year and the 2000 census block groups. We then aggregate the electoral precinct data to the 2000 census block groups. That is, for any variable expressed as a count or total in each precinct (e.g. the number of votes in support of a ballot initiative), we sum these values across the precincts that contribute to a given block group, weighting each by the fraction of the precinct overlapping with that block group.

Measure of environmental support. Our dependent variable is the proportion of voters supporting four pro-environmental ballot initiatives in each block group, across three unique elections. Californians rarely vote on identical ballot measures across election cycles. However, ballot measures are often substantially similar in their intent and policy implications.

The four ballot measures we consider constitute all the measures that clearly reflect support for costly climate-related policies. We review these briefly. In 2006, Californians voted on Proposition 87, which proposed a new $4 billion dollar program to support clean energy alternatives, funded by a 1.5% to 6% tax on Californian oil producers. It was rejected 55% to 45%. In 2008, Californians voted on Proposition 10, which proposed a support program for research, education and deployment of alternative fuel technologies, and was rejected 59% to 41%. Californians also voted on Proposition 7, which proposed to require increased utility purchases of renewable energy and was rejected by 64% to 34%. We create a single measure of pro-environment voting behavior for 2008 by averaging support for Proposition 10 and Proposition 7. In 2010, Californians voted on Proposition 23, which sought to suspend California's Global Warming Act of 2006 (rejected, 62% to 38%). Critically, we do not assume that support for these four initiatives measure the same thing, i.e. that they would have similar levels of support in the absence of the treatment. In particular, we allow for an arbitrary intercept shifts in the level of support across proposals.

Treatment Measurement. We extract wildfire perimeter data from the Monitoring Trends in Burn Severity (MTBS) dataset, an interagency US government effort tracking large fires via Landsat satellite data. We then spatially merge the wildfire perimeter data to the census block group data to determine each census block group's distance from wildfires that burned at least 5000 acres, over each two-year period preceding a federal election (see Appendix A.1 for details). These wildfires do not occur at randomly with equal probability in all census block groups. They are more common in rural and peri-urban areas that are also, on average, more politically conservative. Naive estimates that merely compare voting behavior in places that did and did not experience wildfires are thus uninformative as to the effect of wildfire, instead only showing how places more or less prone to wildfire tend to differ (see Appendix A.2).

Identification and Estimation. We estimate an effect under the assumption of no unobserved time-varying confounders. Specifically, we expect both that different block groups have different risks of wildfire, and that different years pose different risks of wildfire. However, after accounting for statewide changes in risk over time, we argue that no omitted, time-varying feature of a block group both change the risk of wildfire and systematically influence support for climate policies. Further, in the event that this assumption does not hold precisely, sensitivity analyses reveal how strong any time-varying confounding would have to be in order to alter our conclusions.

Consider a particular block group level voting outcome in a given year, Y_{it}. For each block group at each election, Wildfire2yr_{it} equals 1 if a wildfire occurred within the block group's spatial perimeter in the preceding two year period and equal to 0 otherwise. We estimate the effect of wildfire exposure on voting outcomes using a (two-way, fixed effects) model of the form,

$$\text{Support}_{it} = \gamma_i + \omega_t + \alpha \ \text{Wildfire2yr}_{it} + \beta \ \text{DemVoteShare}_{it} + \eta_{it}, \tag{1}$$

where Support_{it} is environmental ballot measure support i in year t, γ_i are block group fixed effects, ω_t are election-year fixed effects, and η_{it} is the error term. The key parameter of interest is α, the coefficient on Wildfire2yr_{it}. Including Democratic vote share (measured four years prior for Congressional elections), DemVoteShare_{it}, is not necessary under our assumptions, nor does it have an impact on the estimates, but we include it for the benchmarking component of our sensitivity analyses.

Results

We find that block groups exposed to a wildfire larger than 5000 acres have 3.6 percentage point higher support for environmental ballot initiatives (SE=0.004, t=8.92).[1] We then examine how effects vary with distance from the fire with the same two way fixed-effects model, but employing series of indicators that measure the minimum distance between each block group and a wildfire. The indicator variable for block groups near the median wildfire distance (35-40km) are omitted, so that the each coefficient estimate compare the expected level of support at that distance to the expected level of support at the median distance. Figure 1 plots these results. Experiencing a wildfire very near one's block group (0 to 5 km) has the largest estimated effect on pro-environmental voting relative to the median distance (5.8 percentage points, SE=0.002, t=28.4). This estimate decays monotonically down to just 0.76 percentage points (SE=0.001, t=5.4) at 30 to 35 km away, the last group closer than the median distance comparison group. While not a linear decay, as a summary, the effect falls by approximately 1.7 percentage points for every 10 km of additional distance (see Appendix A.3, Table 3 for numerical results). Figure 4 in Appendix A.4 re-expresses these results as the expected *level* of support at each distance, i.e. a dose-response curve, to faciliate any chosen comparison rather than comparing each distance to the median.

A key question is what types of block groups are more or less responsive to this threat. We examine effect heterogeneity, splitting our sample into three terciles based on vote share for Democratic congressional candidates, using prior elections to avoid any influence. Figure 2 shows the effects by distance, again relative to median distance, but now separately for block groups within the highest, middle, or lowest terciles of prior Democratic vote share. We find strong effects in the most Democratic tercile, ranging from 3 to 9 percentage points, always highly significant, and roughly diminishing with distance. By contrast the most

[1] The 5000 acre threshold covers 94% of the state's total burned area over this period. It was chosen after prior examination of separate, satellite-based data to eliminate numerous smaller events too small to be threaten the public. If all wildfires are analyzed regardless of size, the average effect estimate is nearly halved, but remains highly significant (1.99 percentage point higher support, SE=0.004, t=5.67).

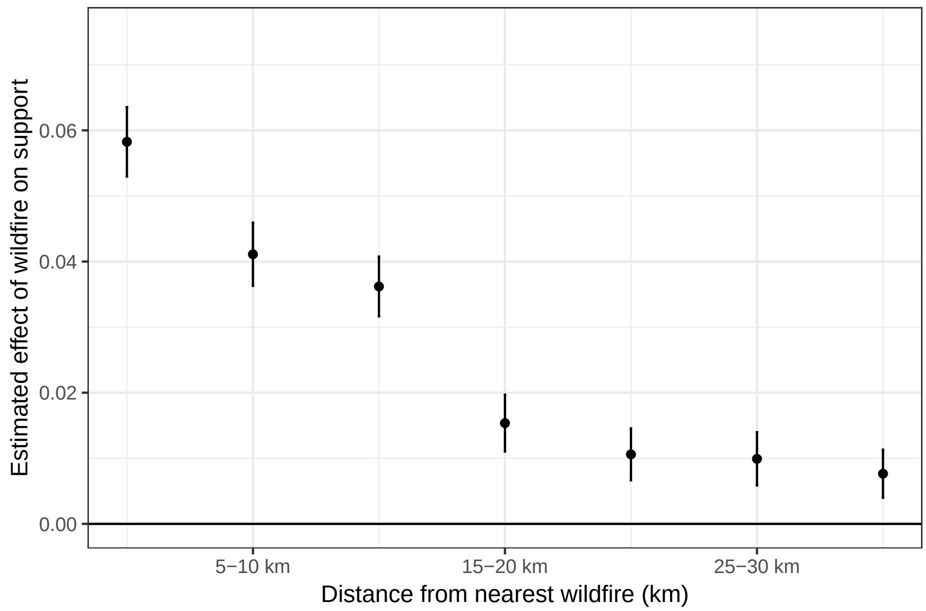

Figure 1: Estimated effect of wildfire exposure on pro-environmental voting, by distance. Estimates compared to response at the median distance (35-40km). All estimates derived from a linear model with block group and year fixed effects and controlling for Democratic vote share in Congressional elections four years prior. Error bars show 99% confidence intervals, using standard errors clustered on block group.

Republican tercile shows small effects at all distances, never reaching even 1 percentage point. The middle tercile shows results that fall in-between these, consistent with the mixed composition of these areas. These tercile models include controls for population density and its square in order to rule out lower population density in Republican areas as the source of this heterogeneity.

Finally, in case the assumption of precisely zero unobserved confounding does not hold, we determine how strong omitted time-varying confounding would need to be to alter our conclusions. Using the bounding approach described in Cinelli and Hazlett, 2018, we return to the simple model for the effect of experiencing wildfire within ones block group as a conservative measure of the effect of wildfire. We find that even an extremely powerful confounder, explaining 10 times the residual variance (in wildfire exposure and in the outcome) as Democratic vote share, would only reduce the implied effect estimate by half. Even if omitted factors are an order of magnitude more powerful than partisanship in predicting

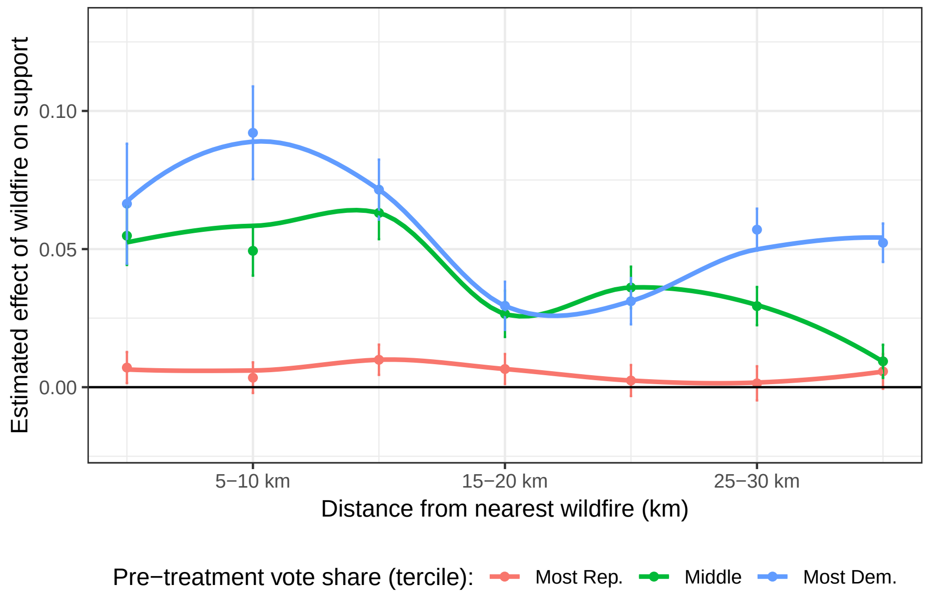

Figure 2: Estimated effect of experiencing a wildfire at various distances, by tercile of Democratic vote share. Error bars show 99% confidence intervals, using standard errors clustered on block group.

both voting behavior and risk of wildfire, the implied effect estimate would remain substantial. Note that our result appear even more robust in the distance-specific models, where effects are larger. We provide additional details and sensitivity analyses in Appendix A.5.

Conclusion

In summary, the haphazard and unpredictable nature of wildfire timing in California provides an empirical opportunity to evaluate the effect of real-world environmental threats on real-world climate-related political behavior. This allows us to assess whether realized climate change could reshape political incentives to act. Our results are the first to justify a causal claim that climate-related experiences can increase realized political behavior. Block groups that experiences a wildfire within their boundaries show higher support for environmental ballot initiatives in subsequent elections by 3.6 percentage points relative to those without one. Block groups that are nearer to wildfires experience larger estimated effects than those farther away: those within 5km, 10km, or 15km of a wildfire boundary show estimated effects of 6.5, 4.8, and 4.3 percentage points respectively, all with $t > 27$ and $p << 0.001$.

By using realized vote share on costly ballot initiatives, these results capture the impact of wildfire exposure on a real world political behavior that can directly influence policy. Fully investigating the mechanism by which this effect occurs requires separate research and a variety of designs. One conclusion we can reach, however, is that the effect is not a simple result of increased (or decreased) turnout alone. Appendix A.6 employs the same models used here, but using voter turnout as the outcome. Wildfires lead to a roughly 3 percentage point, statistically significant increase in turnout in the next election. While substantively large and interesting unto itself, this is too small an effect on turnout for "newly mobilized" voters alone to account for the observed effect without other compositional changes in who is voting.

Moreover, the effect of wildfire strongly varies with the political identities composing these block groups. Voting behavior is most severely impacted by wildfire in the most Democratic census block groups, and largely unaffected in the most Republican census block groups. Experiences with climate change thus enhance willingess-to-act in groups that are more likely to be climate-concerned and to believe in human causes of climate change. The same events did little to mobilize those in highly Republican areas, who are expected to be more skeptical and less climate-concerned. Climate impacts may thus intensify the climate commitments of existing supporters rather than creating new political supporters.

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A Appendix

A.1 Distribution of wildfires in California across electoral precincts

The electoral precinct level is the smallest unit with available electoral return data in California. However, Californian voting geographies and identifiers change on an election-by-election basis constraining our ability to directly contrast voting precinct-level voting outcomes across time. Between 2002 and 2014, the number of electoral precincts in the state varied between a maximum of n=26,985 in 2008 and a minimum of n= 23,185 in 2014.

In the two years preceding each of these elections, between 0.2% and 1.3% of block groups experienced a wildfire that burned at least 5000 acres. Biannual elections occur in early November. A small fraction of units labeled as experiencing wildfires actually did so *after* the November election in even years; however, the number of such cases is small and moreover, this error would bias our result slightly toward zero as it labels some units that were not affected (prior to the election) as if they were.

Election	Block groups without wildfires	Block groups with wildfires
2006	22021	99
2008	21839	285
2010	22040	71

Table 1: Frequency of wildfires burning at least 5000 acres, within boundaries of a census block group, by election cycle.

A.2 Naive relationship between wildfires and political behavior

We begin by descriptively examining the cross-sectional relationship between wildfire and environmental voting, separately in each year and in the pooled data. Results are shown in Table 2 below.

The estimates in columns (1) through (3) all simply show the correlation (as a regression coefficient) between wildfire and voting on the corresponding ballot measure(s) separately for the three relevant elections. Each shows that wildfire is associated with approximately 7

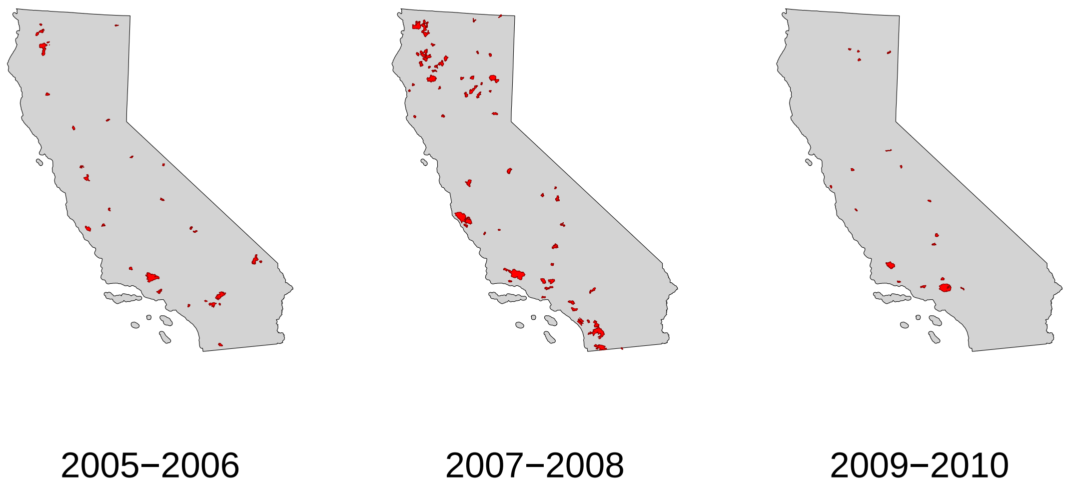


Figure 3: Perimeters of Californian wildfires larger than 5000 acres during each inter-election periods are used for analysis

to 15 percentage points *lower* support for environmental initiatives. The "pooled" version in column (4) includes all the relevant elections/measures, with election fixed effects to allow for different baseline levels of support. It similarly shows a strong negative correlation, with those areas experiencing wildfire having lower support by 12 percentage points. We take these *not* as estimated effects of wildfire on environmental voting, but as an indication that the types of places where wildfires occur are those that tend to be generally less supportive of environmental measures. That this relationship reflects largely "what type of units get treated" rather than an effect of treatment is made evident by replacing the wildfire variable in these models with an indicator for wildfires in the *next* election cycle, which clearly cannot effect (past) support. Column (5) in Table 2 shows that future wildfires also predict 12 percentage point lower support.

These results were expected, as places with wildfires on the whole are likely to be more rural, and more conservative. If true, we also expect to see similar or even larger "imbalances" of this type on a measure of conservatism. The ideal measure for this is Democratic (or Republican) vote share. Unfortunately, a meaningful measure of either is available only

Table 2: Cross-Sectional (Naive) Results for Environmental Outcome

	Dependent variable:				
	envBI				
	2006	2008	2010	pooled	pooled
	(1)	(2)	(3)	(4)	(5)
wildfire2yr	-0.147^{***}	-0.066^{***}	-0.145^{***}	-0.115^{***}	
	(0.004)	(0.002)	(0.004)	(0.002)	
wildfire2yr_f2					-0.121^{***}
					(0.002)
Year=2008				-0.079^{***}	-0.081^{***}
				(0.001)	(0.001)
Year=2010				0.152^{***}	0.150^{***}
				(0.001)	(0.001)
Constant	0.483^{***}	0.401^{***}	0.635^{***}	0.482^{***}	0.483^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	22,091	22,122	22,104	66,317	66,317
R^2	0.047	0.033	0.047	0.440	0.441
Note:					*p<0.1; **p<0.05; ***p<0.01

Cross-sectional description of environmental voting in block groups with and without wildfire in preceding two years. Models (1)-(3) show results separately by year. Model (4) pools cross-sectional comparisons across years, adding year fixed effects so as to allow ballot initiatives in the three years to differ in their baseline levels of support. Model (5) is also pooled but uses a one election (two year) lead of the treatment (Wildfire2yr$_{f2}$). In all cases, the kinds of places that had wildfire in the prior two years (Models 1-4) or in the subsequent two years (Model 5) are places with significantly lower support for environmental measures.

until 2010. From 2012 onwards, California switched to run-off style elections where both candidates running in many congressional districts were Democrats. However, where our analysis requires a measure of Democratic vote share (e.g. as a reassuring but unnecessary control variable, or for examining heterogeneous effects), we wish to use a lagged measure anyway to ensure it is pre-treatment. We thus lag Democratic vote share by two elections both to ensure it is available where needed and is unaffected by the wildfire coded to the same "row" in the data.

A.3 Details of regression for effect by distance

To estimate the distance-varying effects as in Figure 1, we estimate the model

$$\begin{aligned}\text{Support}_{it} = \gamma_i + \omega_t + \alpha_1 \text{ Fire0to5km} + ... + \alpha_7 \text{ Fire30to35km} \\ + \alpha_8 \text{ FireOver40km} + \beta \text{DemVoteShare}_{it} + \eta_{it}, \quad (2)\end{aligned}$$

where Fire0to5km, ..., Fire30to35km are indicators for block groups that experience the nearest wildfire burning at least 5000km within those distances. The indicator for being 35 to 40km from a fire (the median category) is omitted (and the FireOver40km category is included) so that the median group is the omitted one and the coeficient estimates for the distance indicators thus represent the expected change in support at that distance relative to the expected level of support at the median distance. Note that because the coefficient on FireOver40km will reflect the effect of being farther away from a wildfire than the median, it is expected to be (and is) opposite in sign.

A.4 Dose-response estimate

Wildfire is an unusual treatment in that all block groups experience wildfires at *some* distance. In analyzing the effect of wildfire at different distances, we thus do not compare "having a wildfire X kilometers away to having no exposure at all". Rather, distance-based effects are defined as a contrast of the expected level of support at any two distances. While Figure 1 in the main text compares the expected level of support for environmental initiatives at the given distance to the level of support at the median distance, another natural quantity of interest is the "dose-response" curve, i.e. the expected level of support (conditional on or integrating over confounding variables) at each distance. To construct this, we first estimate

Table 3: Regression results for analysis by distance

	Estimate	Std. Error	t value	p-value
fire within 0-5km	0.058	0.002	28.400	0.000
fire within 5-10km	0.041	0.002	22.032	0.000
fire within 10-15km	0.036	0.002	20.513	0.000
fire within 15-20km	0.015	0.002	9.158	0.000
fire within 20-25km	0.011	0.002	6.909	0.000
fire within 25-30km	0.010	0.002	6.304	0.000
fire within 30-35km	0.008	0.001	5.392	0.000
fire over 40km away	-0.007	0.001	-6.046	0.000
Dem. vote share	0.067	0.004	16.914	0.000
Year=2008	-0.092	0.001	-90.408	0.000
Year=2010	0.148	0.000	320.271	0.000

Note Regression results for analysis of effect of wildfire by distance using two-way (block group and year) fixed effects model. Main indicators of interest (and those plotted in Figure 1) correspond to indicators for being various distances to the nearest wildfire burning over 5000 acres. The indicator for the median distance (35-40km) is omitted, so that each coefficient is interpreted as a difference in expected support, relative to the median distance.

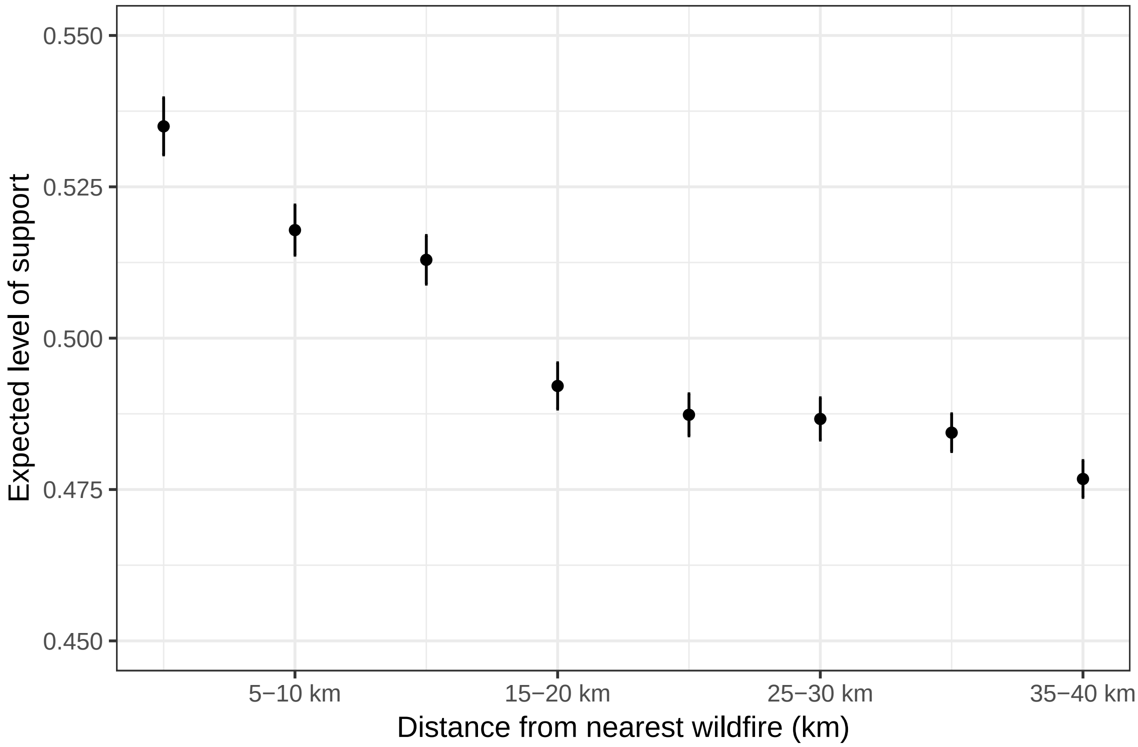
the model,

$$\text{Support}_{it} = \gamma_i + \omega_t + \alpha_1 \text{ Fire0to5km} + ... + \alpha_8 \text{ Fire35to40km} + \beta\text{DemVoteShare}_{it} + \eta_{it}, \tag{3}$$

from which we compute expected levels of support at each distance. Creating actual estimated levels of support requires choosing values of the other covariates – the year, the block group, and the Democratic vote share. The choice matters little, as it results only in a constant shift of all expected levels of support up or down.[2] We use the average DemVoteShare_{it}, and choose the average value of γ_i, thereby averaging the block group intercepts. We leave out ω_t thereby constructing a value that corresponds to the year 2006, the omitted category. Results are shown in Figure 4.

[2]In fact, the dose-response curve is equivalent to Figure 1, but vertically shifted by the response at the median distance (the final category, 35-40km).

Figure 4: Dose-response curve



Note: Dose-response curve showing expected level of support for environmental intiatives as a function of distance to nearest wildfire burning over 5000 acres. To produce these estimates, the year is set to 2006, and the block group intercept shift is given by the average block group fixed effect. Error bars show 99% confidence intervals with standard error estimates clustered on block group.

A.5 Sensitivity statistics and analysis

We provide here details and extensions of the sensitivity analysis in the main text. First, Figure 5 shows the effect estimate implied by any postulated level of confounding (Cinelli and Hazlett, 2018). On such a contour plot, the horizontal coordinate indexs how strongly confounding explains the wildfire (as a partial R^2), and the vertical coordinate describe how strongly confounding explains support. The "height" indicated by the contour then indicates what the corrected effect estimate would be, accounting for such confounding at each position in this space.

Second, towards a norm of transparently communicating the sensitivity of results to unobserved confounding, Cinelli and Hazlett, 2018 suggests routine reporting of a number of sensitivity statistics than can be included in an augmented regression table. Table 4 provides this for the first analysis conducted in the main text, in which we regressed the outcome (support for environmental measures) on the treatment (having a wildfire in one's block group), together with the lagged measure of Democratic vote share.

Outcome: *Support for environmental measure*						
Treatment	Est.	SE	t-value	$R^2_{Y\sim D\|\mathbf{X}}$	RV	df
wildfire2yr	0.036	0.004	8.9	0.30%	5.3%	43943
Bound (Z as strong as *Dem. vote share*): $R^2_{Y\sim Z\|\mathbf{X},D}$=2.5%, $R^2_{D\sim Z\|\mathbf{X}}$=2.2%						

Table 4: Sensitivity statistics for regression of environmental support on having wildfire within the block group, including control for lagged Democratic vote share.

This estimated effect of having a wildfire within the block group boundaries, as reported in the main text 3.6 percentage points. The standard error on this table (*SE*) is the cluster-robust standard error, clustered on block group.[3] The first sensitivity statistic on Table 4 is $R^2_{Y\sim D|\mathbf{X}} = 0.30$. This tells us that "confounding that explains 100% of the residual variation in the outcome, would need to explain only 0.30% of the residual variation in the treatment in order to fully account for the effect?" The next, and more useful value in this case, is the *robustness value*, RV. This tells us that an unobserved confounder explaining any less than 5.3% of the residual variation in wildfire occurrence *and* in support for environmental measures would not be able to fully account for the estimated effect.

More illuminating, we turn to the bounding exercise that was reported in the main text. Information from covariates thought to be important drivers of wildfire and support can aid in reasoning about how "strong" confounding must be to alter our conclusions. Consider as a working assumption the claim that "confounding is not stronger than Democratic vote share." More precisely this means that confounding (one or more confounders acting jointly) may explain as much residual variation in wildfire exposure and in support as does Democratic vote share, but not more. The analysis shows that a confounder of this magnitude

[3] An important technical detail is that the *conventional standard error* (i.e. without clustering) of 0.0031 is used internally in the sensitivity analysis where this quantity is required not to reflect the sampling distribution but because the sensitivity analysis employs expressions that happen to equal the conventional standard error.

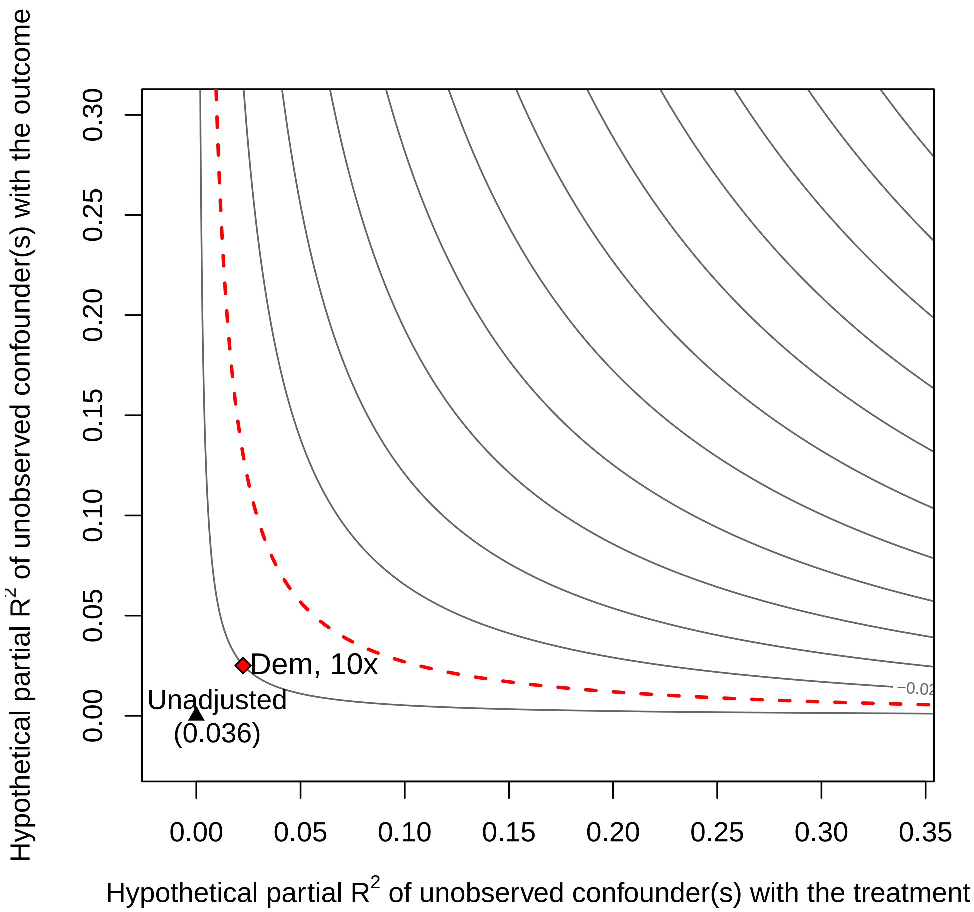


Figure 5: Sensitivity analysis for the estimated effect of wildfire within a census block group on support for pro-environmental voting, using the two-way fixed effect model described. The horizontal axis specifies a hypothesized strength of association between confounding and the treatment (wildfire occurrence in one's census block group), in terms of the partial variance in the wildfire explained by the confounder after accounting for covariates. The vertical axis hypothesizes how strongly confounding is related to the outcome, support for pro-environmental measures, again in terms of partial variance explained. At each hypothesized level of confounding, the adjusted effect implied by that level of confounding is shown by the contours. The conventional estimate assumes zero confounding, and is shown in the bottom left corner ("Unadjusted"). Let us assume that confounding can explain up to 10 times as much residual variance (in both wildfire occurrence and pro-environmental voting) as is explained by Democratic vote share in prior elections, the strongest confounder we were able to think of and include in our data. Even if such a strong confounder exists, it would imply that our adjusted effect is the one marked by *Dem, 10x* on the plot – still approximately half the size of the unadjusted estimate.

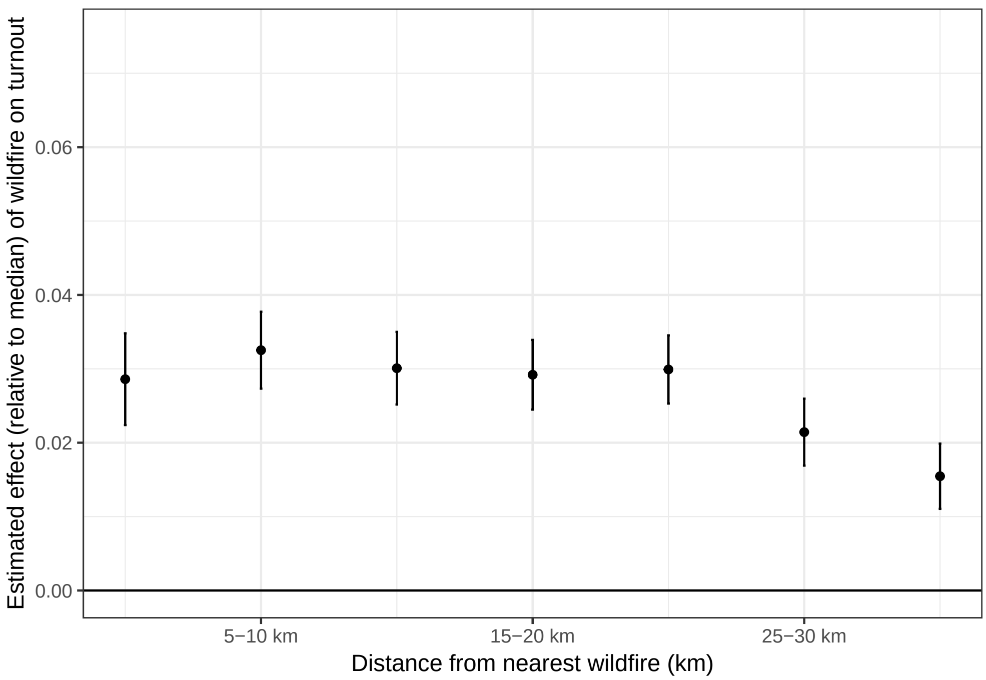
would have virtually no effect on our estimate. Figure 5 shows that even a far more powerful confounder, explaining 10 times the residual variance (in wildfire exposure and in the outcome) as explained by Democratic vote share, could reduce the implied effect estimate only by half. The implication is that whether we have fully eliminated confounding or not, this describes the very high degree of confounding that would be required to change our estimate substantially, much less overturn it. Critiques concerned with whether there exists *any* confounding are uninformative, but critiques that can suggest confounders potentially able to explain far more of the treatment and outcome than a variable such as Democratic vote share would be a contribution.

A.6 Effect of wildfire on turnout

We examine here whether wildfire has an effect on turnout, and whether this is sufficient to explain changes in support simply through the addition (or subtraction) of voters.

Continuing to assume an absence of time-varying confounders, we can estimate the effect of wildfire on turnout by the same approach used to estimate the effect of wildfire on support, changing only the outcome. We thus regress turnout on indicators for distances to wildfire as above, an (optional) control for prior Democratic vote share, and intercepts for each census block group and for each time period. As shown in Figure 6, the results suggest that wildfire has a clear but relatively mild effect on turnout at approximately 3 percentage points for distances up to 20 km, fading thereafter. This is a relatively large and politically relevant effect in substantive terms, making this another finding of interest to political scientists. For present purposes however, it also suggests that the effect of wildfire on support for ballot initiatives cannot be generated solely by newly mobilized voters after wildfires, since the effect of wildfire on support at each distance exceeds the effect on turnout several-fold. Of course, it does remain possible that wildfire's effect occurs at least partially through changes in the composition of voters rather than just "added voters".

Figure 6: Estimated effect of wildfire on turnout



Note: Estimated effect of wildfire on turnout in the following election, relative to median distance. Error bars show 99% confidence intervals with standard error estimates clustered on block group.