Appendix

Wildfire exposure increases pro-environment voting within Democratic but not Republican areas

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A Appendix

A.1 Additional details on California ballot initiatives

In our analysis, we focus on four pro-environment ballot initiatives across three unique elections. Each of these measures involves costly climate-related policies. Here, we summarize the anticipated costs of each, as reported to voters at the time.

The first ballot initiative is California Proposition 87, from the 2006 election. (Official Title: Alternative Energy. Research, Production, Incentives. Tax on California Oil Producers). The proposition involved $4 billion dollars in new program spending on clean energy, funded by a 1.5% to 6% tax on Californian oil producers. The initiative proved highly contentious, with advocates and opponents spending over $150 million on the initiative. The proposition included language to prohibit direct cost pass-throughs to California consumers, opponents vocally claimed that the measure would increase gas prices. The proposition would also have imposed indirect economic costs. The official Fiscal Impact Statement suggested it would lead to state and local revenue reductions in the low tens of millions. Ultimately, Proposition 87 was rejected 55% to 45%.

The second ballot initiative is California Proposition 10, from the 2008 election. (Official title: The California Alternative Fuels Initiative). This proposition proposed a support program for research, education and deployment of alternative fuel technologies, to be paid for using $5 billion dollars in state bonds. The official Fiscal Impact Statement estimated the proposition's total cost as $9.8 billion, including $4.8 billion to service the bonds. Support and opposition focused on whether the state should be prioritizing these funds towards clean energy needs. Ultimately, Proposition 10 was rejected 59% to 41%.

The third ballot initiative is California Proposition 7, also from the 2008 election. (Official title: Standards for Renewable Resource Portfolios). This proposition proposed to require increased utility purchases of renewable energy by 2% annually, up to 40% in 2020 and 50% in 2025. The official Fiscal Impact Statement emphasized broad uncertainty in costs. It suggested that higher power rates would be likely in the short-term and uncertain in the long-term. Opponents claimed the measure would increase consumer electricity costs by 10%, including a $300 increase per household per year. Ultimately, Proposition 7 was rejected by 64% to 34%.

The fourth initiative is California Proposition 23, from the 2010 election. This proposition sought to suspend California's Global Warming Act of 2006, one of the state's primary legislative packages to manage the climate crisis. According to the Fiscal Impact Statement, the proposition (to eliminate climate policy) would have modestly increased state economic activity. Proponents emphasized the measure would save a million jobs, would prevent $3800 in annual household cost increases, and would help protect public services by not imposing economic hardship on the state. Ultimately, this proposition was rejected, 62% to 38%.

Note again, as discussed in text, that we do not assume that support for any of 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.

A.2 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.

Figure 4: Perimeters of Californian wildfires larger than 5000 acres during each inter-election periods are used for analysis

In the two years preceding each of these elections, between 0.3% and 1.3% of block groups experienced a wildfire that burned at least 5000 acres (see Table 1). The perimeters of wildfires in each two-year period are also visualized in Figure 4. 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	19717	88
2008	21353	273
2010	20939	66

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

A.3 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.

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

	Dependent variable:				
			envBI		
	2006	2008	2010	pooled	pooled
	(1)	(2)	(3)	(4)	(5)
wildfire2yr	-0.108^{***}	-0.065^{***}	-0.118^{***}	-0.086^{***}	
	(0.010)	(0.004)	(0.011)	(0.004)	
wildfire2yr_f2					-0.107^{***}
					(0.005)
YEAR=2008				-0.078^{***}	-0.080^{***}
				(0.001)	(0.001)
YEAR=2010				0.155^{***}	0.153^{***}
				(0.001)	(0.001)
Constant	0.476^{***}	0.398^{***}	0.631^{***}	0.476^{***}	0.477^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	19,805	21,626	21,005	62,436	62,427
R^2	0.006	0.012	0.005	0.430	0.431
F Statistic	115^{***}	261^{***}	110^{***}	$15{,}679^{***}$	$15{,}736^{***}$

*p<0.1; **p<0.05; ***p<0.01

Note: 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.

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 to 12 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 9 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 11 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 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.4 Details of regression for effect by distance

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

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

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.

Table 3: Regression results for analysis by distance

	Estimate	Std. Error	t stat.	p-value
fire within 0-5km	0.055	0.002	24.775	0.000
fire within 5-10km	0.031	0.002	16.853	0.000
fire within 10-15km	0.024	0.002	13.397	0.000
fire within 15-20km	0.007	0.002	4.102	0.000
fire within 20-25km	0.004	0.002	2.766	0.006
fire within 25-30km	0.007	0.002	4.021	0.000
fire within 30-35km	0.004	0.002	2.508	0.012
fire over 40km away	-0.012	0.001	-9.942	0.000
Dem. vote share	0.025	0.003	7.164	0.000
precip.2yr	0.000	0.000	42.972	0.000
precip.deviation	-0.194	0.005	-35.873	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.

A.5 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 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.[6] 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 5.

A.6 Effect in areas experiencing prior wildfires

As suggested by a reviewer, it would be desirable to know if the effect varies depending on the degree of prior exposure to wildfire. Because wildfires are statistically rare, the proportion of places with multiple fires is very small; among more than 22000 block groups included in our

6. 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).

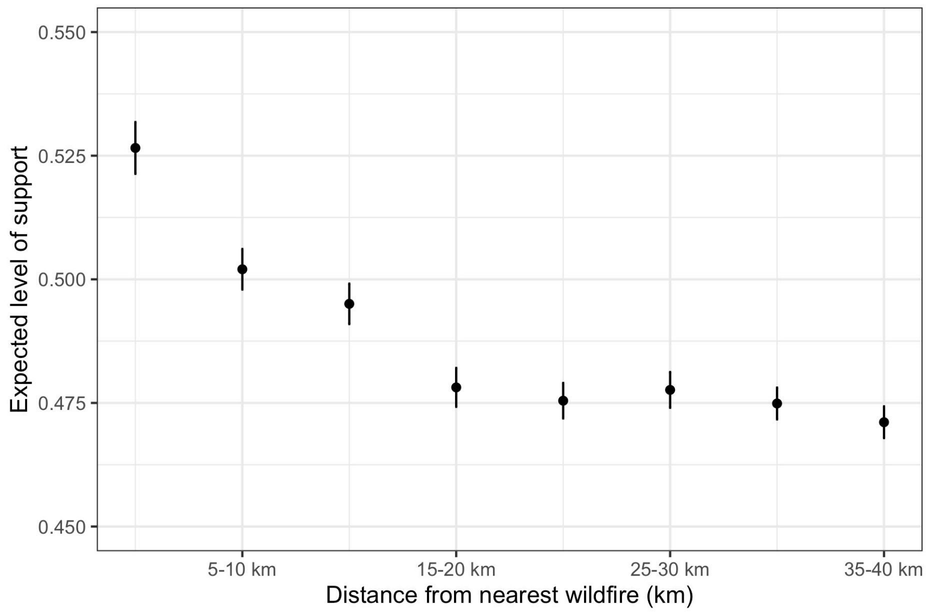


Figure 5: Dose-response curve showing expected level of support for environmental intiatives as a function of distance to nearest wildfire burning over 5000 acres. To produce these estimates, the year is set to 2006, and the block group intercept shift is given by the average block group fixed effect. Error bars show 99% confidence intervals with standard error estimates clustered on block group.

study, only 293 experienced a wildfire prior to the 2005-2006 electoral cycle. Repeating the distance-based analysis in just these unit, Figure 6 shows that units that experienced prior fires may have a weaker response, though as expected the estimates are much less precise in this reduced sample.

A.7 Placebo outcome: Support for housing bonds

Finding a suitable placebo outcome requires constructing an outcome from ballot measures that (i) repeat a similar proposal across multiple years, and (ii) for which we expect little to no effect of wildfire, so that any estimated effect of wildfire we find most likely reveals bias and not a true effect. A particular concern of the latter type is that wildfires, as a source of threat, may make people broadly more conservative in their thinking (see e.g. Jost et al. 2003; Nail et al. 2009). This rules out many measures as useful placebos.

However, in an earlier project we had deemed one set of measures as having the least

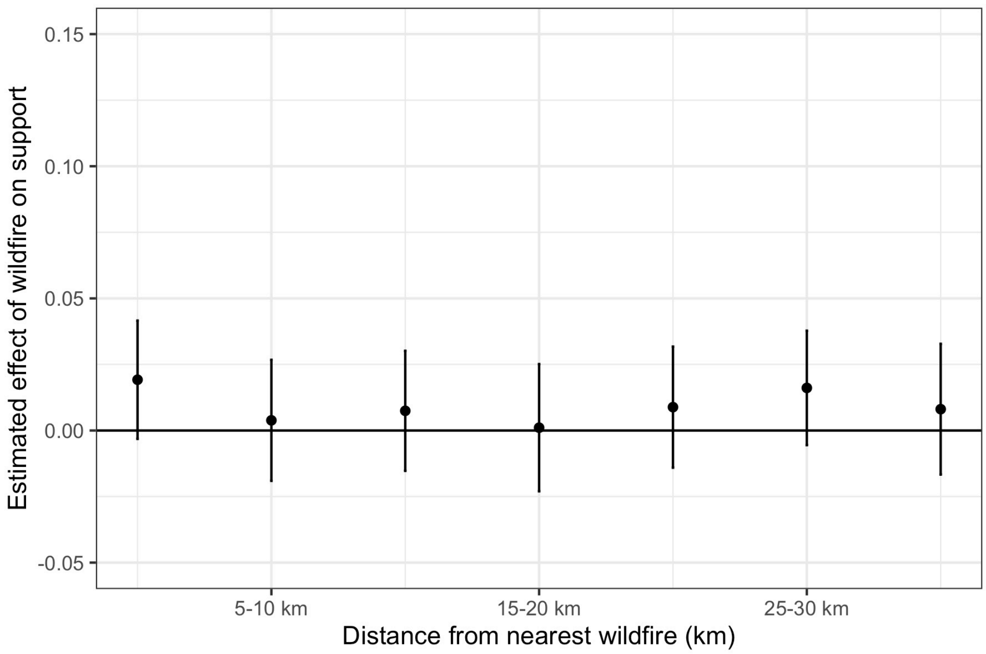


Figure 6: Effect estimate at varying distances for only the 293 units that experienced wildfires prior to the electoral cycles used for form these estimates. Regression model controls for Democratice vote share, precipitation in prior two years, and two year deviation from historical precipitation.

ideological content: support for housing bonds as measured in Proposition 46 in 2002, and Proposition 1C in 2006. Coding the outcome for both so that support is more positive, we replicate the identical model to Equation 1 above but replacing the outcome variable (environmental support) with the outcome for the housing measures. In that model we see no detectable relationship between wildfire and support for housing bonds with a coefficient of only 0.2 percentage points (t-stat=-0.55, p=0.58, 95% CI: [-1.0, 0.58] percentage points).

A.8 Population density

Population density and Democratic vote share are strongly correlated ($r = 0.35$) in the complete data. However conditioning on localities where wildfires occur, for example, breaks this relationship ($r = -0.001$, $p = 0.98$). This may seem surprising but is to be expected. Places with wildfire will have a certain distribution of population densities, strongly skewed towards the lower density areas. This distribution of densities should look the same, however,

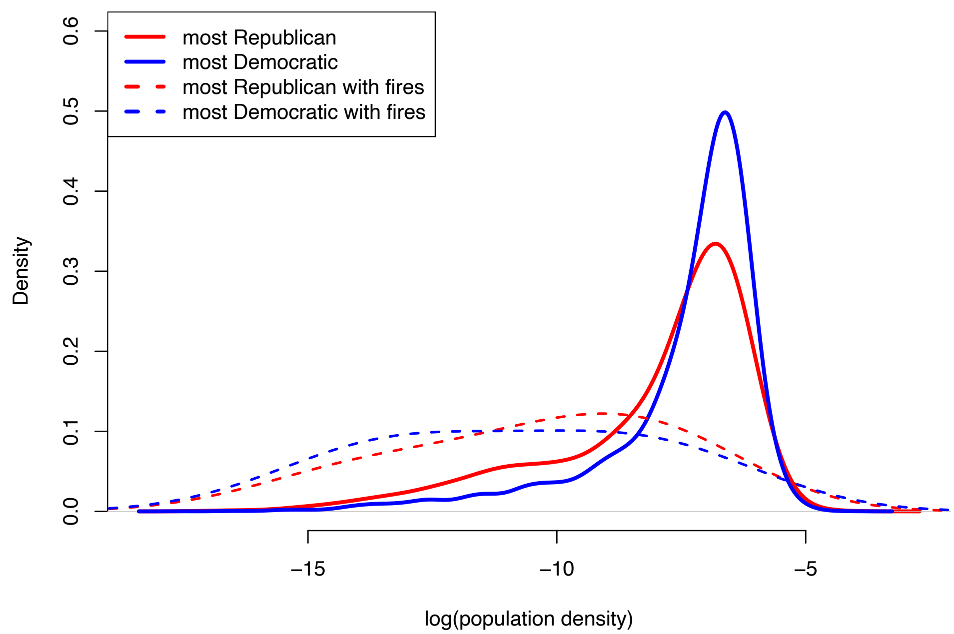


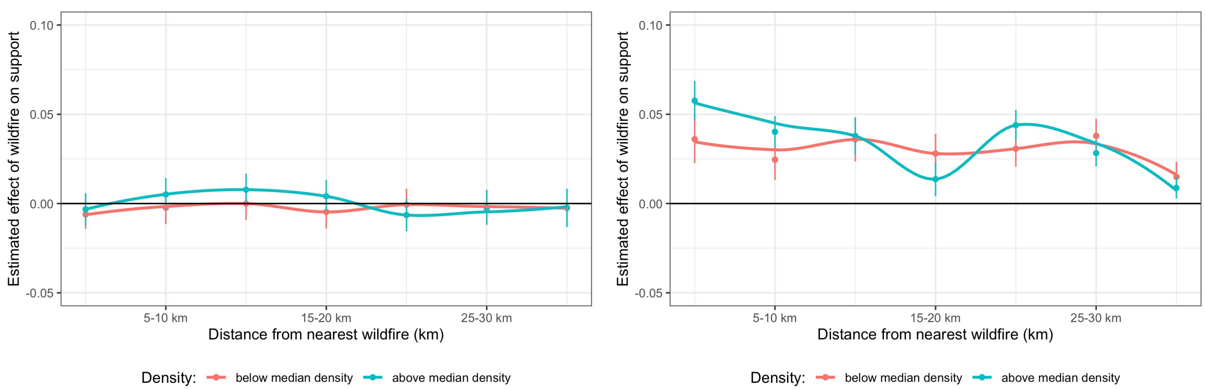
Figure 7: Distribution for the (log) population density, for all areas (solid lines) or for those experiencing a wildfire (dashed line). For all locations, the distribution varies by Democratic votes share (red vs. blue solid lines), with Democratic areas (blue) tending to have higher density. For areas with wildfires however, the distributions of population density are similar for more Republican and more Democratic areas (red vs. blue dashed lines), with the small remaining difference pointing towards Democratic areas having slightly lower population density (not significant as a correlation).

whether we are examining more Democratic or more Republican areas. That is to say once a fire "knows" the population density of an area, it is unconcerned with whether it is more Republican or Democratic.

Figure 7 shows this graphically. Looking at all block groups (whether they have fires or not) the relationship between Democratic vote share and density is strong, as expected. But among block groups with wildfire (dashed lines), the relationship disappears entirely, with the distribution of population densities for places (with wildfire) being nearly the same regardless of party preference. The small remaining differences, which we expect are due to chance, happen to produce slightly lower average density among the Democratic areas, hence the small negative correlation.

Recalling that our estimates are driven by locations with wildfires (for example, the fixed effect estimate depends only on locations that change wildfire status, which are places with at least one wildfire), and that population density is almost unchanging within each location. Hence, by conditioning on locations with wildfire, we are removing the relationship between Democratic vote share and population density. Population density is thus not an explanation for the different effects we find by Democratic vote share in Figure 2.

This does not prohibit variation in population density from having an independent influence on the effect size. Figure 8 expands upon the analysis by taking the most and least Democratic groupings and splitting them into the most and least population dense areas. This finds weak evidence that among the most Republican areas, more dense areas may have stronger effects at some distances; there is very little indication of any such difference within Democratic areas.



(a) Most Republican

(b) Most Democratic

Figure 8: Effect estimate at varying distances for block-groups with 20-40% Democratic vote share (*left*) and 60-80% Democratic vote share (*right*), each now split into the most and least population dense quartiles. Underlying regression model controls for precipitation in prior two years, and two year deviation from historical precipitation as well as the two-way fixed effects.

A.9 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, intercepts for each census block group and for each time period, and (optionally) including the two preciptation variables. As shown in Figure 9, the results suggest that wildfire increases turnout by approximately 3 percentage points for distances of up to 25km. 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".

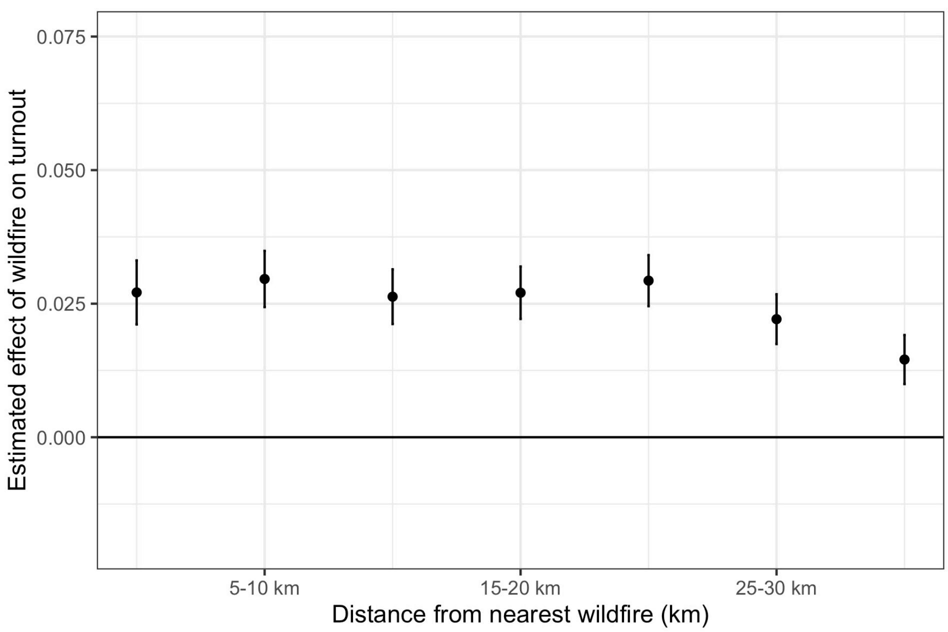


Figure 9: Estimated effect of wildfire on turnout in the following election, at each distance from the nearest wildfire that burned at least 5000 acres in the prior two years, relative to the media distance. Error bars show 99% confidence intervals with standard errors clustered on block group.